Essays in the Microeconometric Analysis of Child Health Determinants

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Chapter 1

General Introduction

This dissertation explores multiple determinants of health early in life, motivated by a recently expanding literature that established the long-run consequences of childhood health on labor market outcomes and health in later in life. Spanning the time period from birth until late adolescence, this thesis studies determinants of health at various stages during childhood. The determinants analyzed include in utero economic conditions, guidelines for neonatal care after birth and the change in health care utilization induced by cost-sharing for visits to the health care sector.

A particular focus is on the question whether these determinants affect children differentially depending on their socioeconomic background. Previous research has demonstrated the presence of an income gradient in child health: Children from disadvantaged families have relatively poor health and this divide opens up early in life. This dissertation contributes to explaining the origins of the gradient, which are not yet fully understood.

All three studies contained in this dissertation have in common that they utilize large individual-level datasets from Sweden. These datasets are ideally suited for the questions posed here, as they not only provide rich information on health care use and health outcomes, but also give useful socioeconomic and demographic details about the child and its parents. In all of the studies, the goal is to identify causal relationships on health or medical care use and for this purpose a variety of tools from modern microeconometrics are employed.

In Chapter 2, which is joint work with Anton Nilsson, we exploit a policy change in Sweden to estimate the effect of cost-sharing on the demand for children's and adolescents' use of medical care. To this end, we make use of detailed population-wide information about individuals and their contacts with the health care system. The reform we study was unexpected and came into effect in January 2002. It abolished copayments in outpatient care for children between 7 and 19 years. We estimate a difference-in-differences model using age groups slightly younger and slightly older as controls. When care became free of charge, we find that individuals increased their number of visits to a doctor by 5-10 percent. Effects are similar across age groups but vary substantially by income, with children from low-income families being two-three times as responsive as their more advantaged peers. We check that our findings are not explained by other changes concurrent with the reform and that the variation in the response by family income is not driven by other factors correlated with income. There is suggestive evidence that the reform not only increased visits, but actually improved child health. We also exploit the fact that copayments charged changed discontinuously at age 7 before the reform and at age 20 after the reform. Estimating a regression discontinuity design around these two age thresholds, we obtain very similar estimates to the ones from the difference-in-differences model, thus adding to the overall credibility of our results.

In Chapter 3 (joint with Hans-Martin von Gaudecker), we investigate the determinants of health interventions just after birth, which can have a tremendous impact on babies' health and their chance of survival. We build on previous research which showed that regulatory standards prescribing additional care when birth weight falls below a certain the 1,500-grams-threshold lead to discontinuous improvements in health and long-run schooling outcomes for babies just below the threshold. We estimate a regression discontinuity design to explore whether such birth weight thresholds also guide physicians' treatment behavior in Sweden and find no discontinuous reductions in infant mortality for babies just below 1,500 grams. This finding is insensitive to a range of robustness checks typically conducted in regression discontinuity designs. We demonstrate that changes in treatment intensity – measured by length of hospital stay – or school grades later in life are also absent around the 1,500 grams cutoff. Furthermore, there is no evidence that other birth weight thresholds or thresholds related to variables other than birth weight would be relevant instead of birth weight. To explain our findings, we point to relatively low cost of neonatal care and highly qualified staff in Sweden that make threshold values obsolete as either a regulatory standard or rule of thumb.

In Chapter 4, which is a joint project with Gerard van den Berg and Steffen Reinhold, we revisit the question whether economic downturns are beneficial for health outcomes of newborn infants in developed countries, as speculated in previous literature. We use data from Sweden over the time period 1992-2004. We find that a one-percentage-point 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 by 6-11 percent. We take a rigorous econometric approach that only uses regional variation in unemployment and compares babies born to the same parents so as to address selective fertility based on labor market conditions. Thanks to detailed information about the parents, we are also able to elucidate the channels linking downturns to newborn health. We find that improvements in health cannot be attributed to the father's or mother's employment status. However, we provide evidence that higher unemployment particularly benefits infants of low-status parents and that it reduces the incidence of premature birth. Both findings are consistent with certain channels independent of parents' employment status, such as reductions in stress and air pollution.

All determinants of child health studied here are to some extent subject to political control. Chapter 2 presents evidence that visits to doctors decrease if children are charged a copayment for each visit. Given the far-reaching effects of child health on outcomes in adulthood, cost-sharing for children warrants careful consideration. In Chapter 3, the role of birth weight thresholds, which might be politically prescribed, in guiding treatment decisions is investigated. The finding that these thresholds are irrelevant for newborn health in Sweden – in contrast to studies for other countries – might actually be a signal for efficient allocation of medical care and desirable from policy point of view. Chapter 4 provides evidence that times of high unemployment are beneficial for newborn health. Ideally, policy-makers will bear this result in mind when trading off the costs and benefits of fiscal stimuli.

It is a noteworthy finding that the determinants of child health considered in this thesis particularly affect children with disadvantaged family background. Children of low-income parents respond much more to copayments in Chapter 2, while the health of babies born to low-status parents is more sensitive to the business cycle in Chapter 4. Therefore, our findings can help explain the widely-observed income gradient in child health.

Chapter 2

The Effect of Copayments on Children's and Adolescents' Use of Medical Care[1]

2.1 Introduction

A growing literature shows that health in childhood has strong long-run impacts on both socioeconomic status (SES) and health in adulthood (e.g., Case et al. 2005; Smith 2009; Lundborg et al. 2014a). Forgone health investments during sensitive periods in early stages of life may later be compensated for only at a relatively high cost, if at all (see Currie and Almond 2011). Consistent with this, many government-funded health insurance programs, such as Medicaid and CHIP in the U.S., exempt children from most cost sharing requirements, thus increasing incentives to seek necessary care. However, there is little actual evidence on if, or the extent to which, the price of health care poses a barrier to utilization among young individuals.

If parental SES influences child health outcomes, then health could also be one important channel governing the intergenerational transmission of SES (Currie 2009). A large literature documents that parental SES is positively correlated with child health outcomes, and work including Currie and Moretti (2003), Milligan and Stabile (2011), Lundborg et al. (2014b) and Kuehnle (2014) provides evidence that the relationship is causal. This income gradient in child health is not only driven by higher arrival rates of health shocks for poor children due to, for instance, inappropriate nutrition (the so-called prevalence effect), but also reflects a more adverse response to health shocks once they are present (Case et al. 2002; Currie and Stabile 2003; Reinhold and Jürges 2012). This so-called severity effect might result from

1. This chapter is joint work with Anton Nilsson. We gratefully acknowledge financial support from the European Research Council (Paul, Starting Grant No. 313719) and The Jan Wallander and Tom Hedelius Foundation (Nilsson). We also gratefully acknowledge financial support from the Humboldt Foundation through the Alexander von Humboldt Professur Prize for Gerard van den Berg.

underuse of medical care, perhaps because poor children fail to adhere to therapy, but quite likely also because cost-sharing deters them from seeking care in the first place.

In this paper, we study if cost-sharing that comes in the form of copayments (per-visit fees) influences children's and adolescents' health care utilization. To address the issue of intergenerational transmission of SES, we also investigate whether the response to cost-sharing varies with age or parental SES, such as mother's education and family income. Finally, we tentatively analyze whether increased child and adolescent health care also translates into improved health outcomes.

We exploit a copayment reform in the Swedish county of Skåne. In Skåne, public health insurance is universal, that is, all residents are entitled to tax-financed health care and exposed to the same copayment schedule, irrespective of income and health care provider. The reform we study came into effect in January 2002 and abolished copayments in outpatient care for children between 7 and 19 years of age. Before the reform, children in that age group were subject to essentially the same copayment schedule as adults. A visit to a doctor was charged (the equivalent of) $10-20 and a visit to another caregiver was charged $8. Exemptions for certain types of visits were in place, and there was an out-of-pocket cap that limited the total amount of fees to be paid within a 12-months-period. On average, the impact of the reform was largest for doctor visits, for which the fraction of visits that were charged decreased drastically from almost 80 percent to zero. The abolition of copayments was both unexpected and introduced at short notice, as two conservative members of the county council - actually opposing the reform - unintentionally pressed the wrong button.

We use a large administrative data set that covers the whole population of Skåne. In an unprecedented way, this data set merges socioeconomic variables about individuals, such as income and education, with detailed information about their contacts with the health system, including date and time of a visit, the type of caregiver, and the diagnosis given. Both the large sample size and the fine-grained micro-level information make this data set ideal for the purposes of our study.

Our main analysis employs a difference-in-differences design, where the control group includes either 3-to-6-year-olds (who were always exempt from copayments) or 20-to-24-year-olds (who were never exempt from copayments) and the treatment group includes 7-to-19-year-olds, who were subject to the policy change. The identifying assumption here is that treatment and control groups exhibit similar trends in utilization over time. We test whether this assumption is violated because individuals in each group might be inherently incomparable due to different age or because the treatment group coincides with the group of school students. We also conduct an event study to rule out that intertemporal substitution resulting from anticipation of the reform drives our results. Moreover, noting that copayments charged changed discontinuously at age 7 before the reform and at age 20 after the reform, we supplement our analysis with the estimation of a regression discontinuity design around these two age thresholds. While the regression discontinuity design yields only local treatment effects, it has the advantage that it does not require time trends to evolve in a particular way.

Our results show that making health care free of charge increased doctor visits by 5-10 percent. The estimates from the difference-in-differences approach do not depend on the chosen control group and are very similar to the ones from the regression discontinuity design. The response is also found to be similar across age groups and there is no evidence that our findings are confounded by other factors concurrent with the reform, such as an outbreak of disease among school students or intertemporal substitution. The finding that cost-sharing significantly decreases the health care utilization of children is our first important contribution to the literature. While previous work has suggested that the response of children does not differ from the one of adults, most studies suffered from small sample sizes and from the use of policy changes that affected entire families, so that interactions with parents potentially confounded estimates of children's own-price elasticity.

As a second major contribution of the paper, we show the response to be almost three times as high for low-income as for high-income children. This variation is not driven by family status, maternal education or other factors correlated with income. Very few previous studies have been able to examine heterogeneous responses by income credibly, not even on adults (Baicker and Goldman 2011). We improve on previous studies because, in contrast to them, we are able to observe income at the individual level (rather than approximating it by ZIP code or similar) and we can study the whole population rather than selected subgroups (of employed individuals, for example).

Finally, we also provide suggestive evidence that the reform actually improved child health. Since child health is an important determinant of long-run health and economic success, our findings imply that policymakers should execute great caution when considering cost-sharing for children. Given that the price-sensitivity is particularly large among children from low-income households, cost-sharing provides one potential explanation for the widely observed income gradient in child health, and may thus add to sustained economic inequality across generations.

The rest of the paper is organized as follows. Section 2.2 provides an overview of previous studies estimating the price sensitivity of health care demand. Section 2.3 gives information on the institutional setting, on the data we use, and on our econometric approach. In Section 2.4, we show and discuss our results. Section 2.5 concludes.

2.2 Previous Work

While there are numerous empirical studies that estimate how cost-sharing affects the demand for health care in the adult population,[2] only little evidence exists on how children's use of care responds to prices. One of the most credible estimates comes from the Rand

2. For reviews of this literature, see Chandra et al. (2007), Swartz (2010), and Baicker and Goldman (2011).

Health Insurance Experiment (hereafter Rand HIE; Newhouse and the Insurance Experiment Group 1993). Conducted in the 1970s, the Rand HIE randomly assigned families to different health insurance plans with different levels of cost-sharing. Results were translated into a widely cited overall price elasticity of -0.2, a response that was found to be about the same for adults and children (Keeler and Rolph 1988). Subsequently to the Rand HIE, most studies have used policy changes to estimate how health care demand responds to cost-sharing.[3] Both Cherkin et al. (1989) and Selby et al. (1996) found that patients seek significantly more care when copayments are introduced, and the pattern of the response was found to be similar for children and adults.[4] However, all of these studies, including the Rand HIE, suffered from the problem that changes in cost-sharing affected whole families. If parents face no simultaneous increases in cost-sharing and the family budget constraint is not tightened as much, cost-sharing might have a different effect, if any, on children. Our paper looks at the effect of cost-sharing on children and adolescents in isolation, so that no spill-overs from parents confound the estimates of children's own-price elasticity.[5] A recent paper with a similar approach is Yang et al. (2014), who exploit a discontinuous increase in cost-sharing at age 3 for children in Taiwan and find that visits go down by about 5 percent.[6]

The evidence on whether children's response varies by parental characteristics, such as family income, is even scarcer. In the Rand HIE, poor children appeared to respond more strongly than non-poor children, but this difference could be shown to be significant only for a subgroup of contacts related to trauma and accidents (Lohr et al. 1986). The Rand HIE suffered from small sample sizes at this level of analysis and from the fact that poor families had a lower out-of-pocket-cap that made them more likely to exceed it and to enjoy free care for a considerable part of the year. In our setting, the number of observations is much larger and the out-of-pocket cap, while present, does not depend on family income. Non-experimental work has explored income heterogeneity among adults and findings have been mixed. These studies either had to proxy individual income by regional indicators based on, for instance, ZIP code (Cherkin et al. 1992; Selby et al. 1996; Hsu et al. 2006) or focused on programs targeted at poor people only, so that comparisons with non-poor individuals must rely on estimates from other studies, which are based on different contexts (e.g., Chandra et al. 2014). An exception is Yang et al. (2014), who find similar responses for high and low

3. Two randomized experiments have also been carried out more recently. Michalopoulos et al. (2011) studied the Accelerated Benefits Demonstration, which provided medical benefits to Social Security Disability Insurance beneficiaries immediately rather than after a 24 months waiting time. Finkelstein et al. (2012) studied the Oregon Health Insurance Experiment which allowed a group of uninsured low-income adults to apply for Medicaid. Both studies showed quite substantial effects on health care utilization for the groups participating.

4. Cherkin et al. (1989) found that physician office visits decreased by 11 percent following an introduction of a \$5 copayment and Selby et al. (1996) found that the introduction of a \$25 to \$35 copayment at emergency departments reduced visits by 15 percent. It is not possible to calculate meaningful elasticities based on studies exploiting policy changes where prices were zero either before or after the policy change.

5. There might in principle be spill-overs in the opposite direction, i.e. from affected children on parents or unaffected siblings. The primary interest of this paper is however in children's own-price effect.

6. In the developmental context, Tanaka (2014) shows that abolishing copayments has positive effects on nutritional status in poor Black children under 6 in South Africa.

income households. Surveying the literature, Baicker and Goldman (2011), conclude that "while there is a lot of speculation that the poor have more-elastic demand, there is little evidence..." (p. 58). In our paper, we add a credible estimate of income heterogeneity to the literature for two reasons: First, we use administrative data with precise income information at the individual level. Second, because public health insurance in Sweden covers the whole population, we can make comparisons across all income groups.[7]

2.3 Data and Method

2.3.1 Institutional Setting

Health care in Sweden is provided at the county level. This paper focuses on Skåne, the southernmost county, which has approximately one million inhabitants. The organization of health care is similar in Skåne as in the rest of Sweden, and Sweden is comparable to most European countries.

In Skåne, public health insurance is universal, that is, all residents are entitled to publicly funded health care. Supplemental private health insurance is available, but uncommon.[8] Primary care is to a large extent provided by health care centers that offer all types of ambulatory treatment. Rural communities usually have one health care center, while larger cities have several. Hospitals provide outpatient care by specialists and supply inpatient care; "inpatient care" here refers to all medical contacts that involve at least one overnight stay. Most health care providers are public, that is, they are owned and operated by Region Skåne. In addition, there are privately run providers that work under public contract. Hospitals are almost all public.

Skåne, and Sweden more generally, has rather long waiting times for treatment by international standards. This holds true despite a combination of measures that is aimed at improving access to care: First, there is gate-keeping through a phone triage system, meaning that individuals need to call a nurse at a health care facility (typically a health care center) to schedule a visit and may be denied care if treatment is not deemed necessary. Moreover, in order to access hospital or specialist care, individuals typically need a referral from a GP at a health care center. Second, and important to our paper, Skåne imposes cost-sharing to deter patients from over-using care.

Cost-sharing comes in the form of copayments that are charged for several medical

7. We also add to the literature by considering heterogeneous responses by education. No previous study has examined this, which is surprising given the large literature documenting the positive relationship between education and health (e.g., Cutler and Lleras-Muney (2006)). Indeed, one potential mechanism behind this relationship could be that less educated individuals are more sensitive to cost-sharing. Goldman and Smith (2002) showed that less educated patients are less likely to adhere to the therapy of chronic conditions. However, the issue of whether this behavior is reflected in a larger sensitivity to cost-sharing was not addressed.

8. In the year 2000, only 1.1 percent of the Swedish population had supplemental private health insurance (Finansdepartementet 2008). Private health insurance provides shorter waiting times for treatment and counseling, but only at private health care providers.

services, such as visits to a doctor. Copayment levels are determined by the Skåne Regional Council, whose members are directly elected every four years. Due to the universal coverage of public health care and to the low rate of private insurance, everyone who lives in Skåne is essentially exposed to the same health care copayment structure. While copayments also serve as a source of revenue, their contribution to health care funding is small. Health care is mainly financed through locally levied income taxes and, to a smaller degree, central government subsidies.

Skåne started charging children and adolescents for medical care in July 1999. Only children under 7 continued to receive free care, while those aged between 7 and 19 years became in principle subject to the same copayment schedule as adults. Unlike adults, however, children were exempt from fees for speech therapy and psychiatric care when aged under 16 and 18 years, respectively. This is in line with the observation that there are certain health problems in childhood (such as speech disorders and mental issues) that have long-term consequences if not treated early on, and whose therapy should therefore not be discouraged (Currie and Almond 2011).

Table 2.1 summarizes the outpatient copayment structure as of 2001, which applied to all individuals aged 7 or above (Regionfullmäktige Skåne 2000). The copayment amounted to 100 SEK (approximately $10 in 2001) for seeing a general practitioner at a health care center as well as for seeing a specialist at a hospital after referral. For visits to a specialist without a referral from a GP (for example, a revisit to a specialist), visits during out-of-office hours or visits to the emergency department, individuals were charged 200 SEK. So-called medical services, including X-ray examinations, were free of charge. Visiting a nurse outside primary care or visiting certain other types of health care professionals, such as psychologists, physical therapists or dietitians, was charged 80 SEK. Nurse visits in primary care were free of charge, which meant that most nurse visits were not charged. Several specific services, such as vaccinations and prescriptions without contact with the doctor, were charged with a service-specific fee. Several types of medical treatment were generally exempt from copayments, such as 24-hour-revisits, rehabilitation for disabled individuals and treatment of infectious diseases. Health care at schools (mostly counseling and highly recommended vaccinations) was free of charge.

In the end of October 2001, the left-wing opposition in the Skåne Regional Council put forward the proposal to abolish all copayments for medical visits charged for individuals aged 19 and below. Unexpectedly, the proposal was accepted, as a result of two members of the right-wing majority accidentally pressing the wrong button (Hanson et al. 2001).[9] The reform came into effect in January 2002. As taxes were left unchanged, the policy change was

9. The incident serves as an example of failed tactical voting. The left-wing minority had proposed to abolish fees for individuals up to age 19 whereas a small (populist) right-wing party had proposed to abolish fees for those up to age 12. The larger right-wing parties wanted to keep the status quo. In the first round of votes, the proposal to abolish fees for those up to 19 defeated the proposal to abolish fees for those up to 12 as two members of the larger right-wing parties accidentally voted for the left-wing proposal. The larger right-wing parties were then defeated in the second round as the small right-wing party abstained from voting.

Table 2.1: Outpatient Fee Structure in Skåne in 2001 for Individuals Aged 7 Years and Older

Caregivers: Doctors	SEK	Non-Doctors[d]	SEK
Specialist in general medicine (GP)[a]	100	Nurses	
Other specialists		in primary care	0
with referral	100	in specialist care	80
without referral (typically revisits)	200	Other health care professionals (e.g., psychologists, physical therapists, dietitians)	80
Acute visit during out-of-office hours[b]	200		
Emergency department	200		
Medical service (e.g., x-ray, ultrasound)[c]	0		
Specific services (amongst others):			SEK
Prescription only			50
Vaccination (plus cost of vaccin)			120
Mammography			120
Blood pressure control			200

Exemptions (amongst others):

→ Age-related
- Psychiatric care for individuals under 18 years
- Speech therapy for individuals under 16 years
- Health care at schools (mostly counselling and highly recommended vaccinations)

→ Timing-related
- 24-hour-revisits at the same provider for the same condition
- After waiting for more than 3 months since diagnosis (so-called care guarantee)
- After waiting for more than 30 minutes since the scheduled time of a visit
- Revisits within 7 days of patients with respiratory infection and when the doctor refrained from antibiotic treatment

→ Disease-related
- Rehabilitation for individuals with disabilities
- Dialysis treatment
- Forensic and compulsory psychiatric care

→ Urgency-related
- Treatment of infectious diseases
- Acute treatment of alcohol and drug abuse
- Acute referrals that require immediate medical assessment
- Outpatient visits leading to immediate hospitalization

→ Other
- Birth control
- Contact by telephone/letter
- Trial and adaptation of technical utilities
- Research/drug testing

Notes: [a]Also includes psychiatrists in basic psychiatry. [b]5p.m.-8a.m., weekends and public holidays. [c]Performed by a doctor different from the one treating the patient. [d]Performed independently, i.e. not given directly after and connected with a doctor visit. 100 SEK (=Swedish krona) $\approx$ \$10.

financed by general cutbacks to the health care system (Hanson et al. 2001). In addition to the abolition of copayments for children and adolescents, the decision involved some minor changes to the fee structure, such as free nurse visits in psychiatric care for individuals above age 18, provision of free contraceptives for individuals aged 20 and below, and abolishing the practice of, first, not charging individuals that had to wait more than 30 minutes beyond the appointment time for an acute visit and, second, not charging individuals for outpatient visits that lead to immediate hospitalization (Regionfullmäktige Skåne 2001).

In this paper, we study the time period July 1999 to December 2006. Health care for children remained free of charge from the day the reform took effect until the end of this period. Besides the reform in 2002, other modifications were made to the copayment schedule between 1999 and 2006: increases for doctor visits from 200/100 to 300/150 SEK in July 2003, and for non-doctor visits from 60 to 80 SEK and from 80 to 100 SEK in 2000 and 2004, respectively.[10]

Throughout the time period we study, there was an out-of-pocket cap on fees implying that an individual paying an amount of 900 SEK of fees within a twelve-month period became eligible for a "free card" that granted free outpatient care until the end of that period. In the years they were subject to fees, the out-of-pocket cap applied jointly to all children and adolescents under 18 years who lived in the same household. For inpatient care, children and adolescents paid no fees during the time period we study (individuals above the age of 24 - the age of 20 from July 2003 - were charged a fee of 80 SEK, which is small in light of a hospital's provision of food etc.).

2.3.2 Data and Sample

Our dataset contains the universe of contacts with the medical sector in the Swedish county of Skåne between 1999 and 2008.[11] It combines the two "patient administrative register systems" PASiS and PRIVA that are administered by the Regional Council of Skåne.[12] PASiS contains all publicly provided care, while PRIVA contains all privately provided care. In our empirical analysis, we do not maintain the distinction between public and private providers and treat records from both registers equally.

The dataset includes an extensive range of information about each medical contact. We know whether a visit was classified as acute or non-acute, we observe the fee that the patient was charged for it and all diagnoses that the patient was given. For outpatient care, we can identify the specific caregiver as either doctor or non-doctor. Unless indicated otherwise, we restrict attention to real visits to the medical system and ignore contacts via mail, telephone

10. More minor changes include: exemptions for rehabilitation for adult individuals with disabilities (2000); refunds after waiting for more than 3 months since diagnosis (so-called care guarantee, 2000); no more exemptions for revisits within 7 days of patients with respiratory infection and when the doctor refrained from antibiotic treatment (2004).

11. The only exceptions are visits to the dentist.

12. Kristensson et al. (2007) have used these registers before.

etc. We also exclude preventive visits.[13]

We merge this health data with another Swedish administrative dataset that contains a variety of socio-economic and demographic variables. This dataset covers all persons born between 1940 and 1985 and registered in Sweden as of December 31, 1985, as well as their parents, and all their children. It has been constructed from a number of different registers, most notably the so-called LISA register. The LISA register contains annual information on income by type, as well as data on education, marital status, place of residence, and many other variables for all individuals aged 16 years and above. We use the LISA register for two purposes: First, in the health data we only observe individuals when they have a contact with the medical sector and some individuals might not seek formal health care at all. Therefore, we use the LISA register to define the sample of potential Skåne patients. For children under 16 years who do not have their own LISA entry on residence we utilize information on the parents whom we link to the child via the intergenerational birth register. We apply an elaborate algorithm to identify the parent that the child most likely lives with and impute the child's residence information from that parent.[14] Second, we use the LISA register to analyze whether certain subgroups of the population (defined by maternal characteristics) responded differentially to the reform. Another register merged with the dataset is the Vital Statistics Register, from which we obtain the year and month of birth.

In this paper, we will restrict attention to the time period between July 1999 and December 2006. We exclude 2007 and 2008 because the LISA register is unavailable to us in these years. For the difference-in-differences regressions, we further limit ourselves to one year before and after the policy change (2001 and 2002). This is because the Skåne health care system has been undergoing continuous transformation since its establishment in 1999, and it is important not to confound the effect of the policy change of interest with other administrative changes related to copayments (see the previous section), documentation requirements or reimbursement schemes.[15] Also demographic trends are less likely to violate the common trends assumption - crucial in our identification strategy - when looking at a time window that is close to the policy change.

As the control group in our difference-in-differences framework, we choose the 3-to-6-year-olds, who were exempt from fees during the whole study period, and the 20-to-24-year-olds, who had to pay throughout. We report results separately using these alternative control groups. We only include individuals who lived in Skåne continuously between 2001 and 2002 in order to sidestep potential endogenous immigration in response to the reform.[16] Our

13. We ignore these visits because they all vary highly over time, probably due to changes in documentation requirements.

14. See Section 2.A.1 in the Appendix for details on the algorithm.

15. For instance, we observe a sharp rise in the recorded number of visits to nurses and other non-doctors by almost one third between 2000 and 2001. According to representatives of the Skåne administration, this can be explained by a combination of reimbursement changes and documentation tightening for these visits.

16. Place of residence is only available at a yearly level, determined on December 31. Thus, a person is defined as living in Skåne continuously between 2001 and 2002 when the data indicated residence in Skåne for the years 2000 through 2002.

final sample consists of 280,000 individuals who are observed in 22 months on average.[17] Roughly 15 percent of the individuals in the sample had no contact at all with the health care system in the years 2001 and 2002, although this number varies with age: Only 5 percent of the 3-year-olds made no visits, but up to 20 percent at ages 12 to 14 years.

There emerge two age thresholds that we can exploit to estimate a regression discontinuity design. First, care was only free for children aged 6 years and younger before the reform, creating a potential discontinuity in utilization at the time a child turns 7. Second, after the reform abolished fees for individuals aged between 7 and 19 years, there was another potential discontinuity at the age of 20.[18]

We have 2.5 years of observations for the 6/7 threshold and 5 years for the 19/20 threshold. Since we only observe the month of birth (rather than the exact day), we do not know how many days an individual spends on either side of the threshold in the month he or she crosses it. We therefore drop individuals in months in which they turn 7 or 20 years, respectively. The final sample size used in the regression will depend on the choice of the bandwidth around the threshold. With a bandwidth of 18 months below and above the threshold, we have approximately 1.1 million observations of 70,000 distinct individuals in the case of the 6/7 threshold and 2.0 million observations of 95,000 distinct individuals in the case of the 19/20 threshold.

2.3.3 Descriptive Statistics

2.3.3.1 Fees

Figure 2.1 illustrates that the reform had a significant impact on children's exposure to cost-sharing in Skåne. It shows the distribution of fees actually paid by individuals in the years before and after the reform. In accordance with the rules described above, visits for children aged 3 to 6 years were virtually never charged throughout the study period. In 2001, the distribution of fees for visits by individuals aged 7 to 19 years closely resembles the one for individuals aged 20 years and higher across all types of caregivers. In 2002, the abolition of fees for individuals aged 7 to 19 years manifests itself in a share of zero fees close to 100 percent for this age group, while the older group continues to pay about the same fees as before.[19]

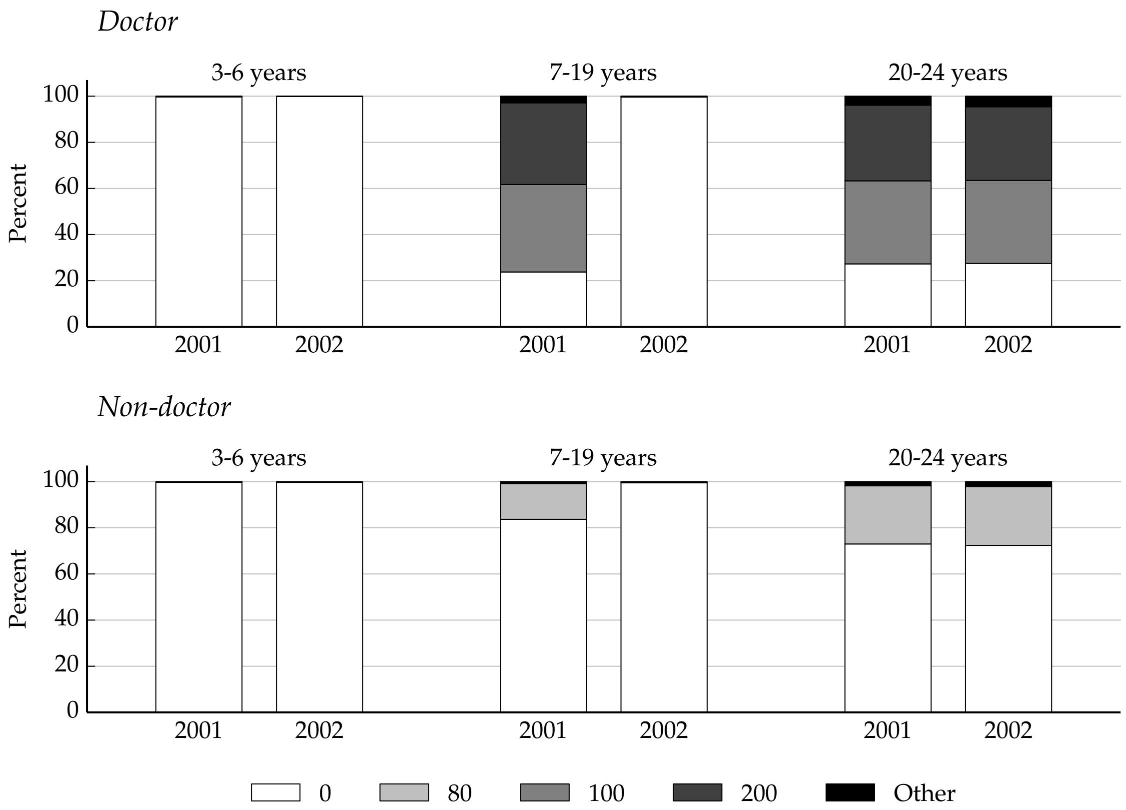
Figure 2.1 also shows that for visits to non-doctors, individuals aged 7 to 19 years were not charged any fee in 80 to 90 percent of the cases already before the reform. This can

17. Note that our sample excludes all individuals who - or whose parents if born after 1985 - were not registered in Sweden as of December 31, 1985. This comprises individuals who - or whose parents if born after 1985 - immigrated to Sweden after 1985. It also excludes individuals born to mothers who were themselves born after 1985. But note that this number must be negligible, since for a child to be 3 years old in 2002, the child's mother must have been pregnant in the year 1999, being at at most 13 years old given that she was born in 1986 or later.

18. Note that the 19/20 threshold also arises in the months before July 1, 1999. But since we only have data starting from January 1, 1999 (= 6 months of observations), we choose to disregard this threshold.

19. For exact percentage changes, see Table 2.9 in the Appendix.

Figure 2.1: Distribution of Actually Paid Fees (in SEK) by Caregiver, Age Group and Year



Notes: Percentages shares of actually paid fees by caregiver in the treatment group (7-19 years) and in the control groups (3-6 and 20-24 years), before (2001) and after the policy change (2002). Only non-preventive in-person visits plus contacts related to prescriptions.

partially be explained by the eligibility for a free card. However, most individuals do not consume enough health care to become eligible for the free card and the prevalence of the free card is rather low among children; only 7 percent of the 7-to-19-year-olds paid with such a card at least once in the year 2001 and the share of visits paid with a free card was only 11 percent. Thus free visits mostly reflect the large number of exemptions mentioned above.[20] In contrast, only one quarter of all doctor visits were free of charge in 2001. Given these figures, we expect the demand for doctor visits to respond more to the reform than the demand for other visits.[21]

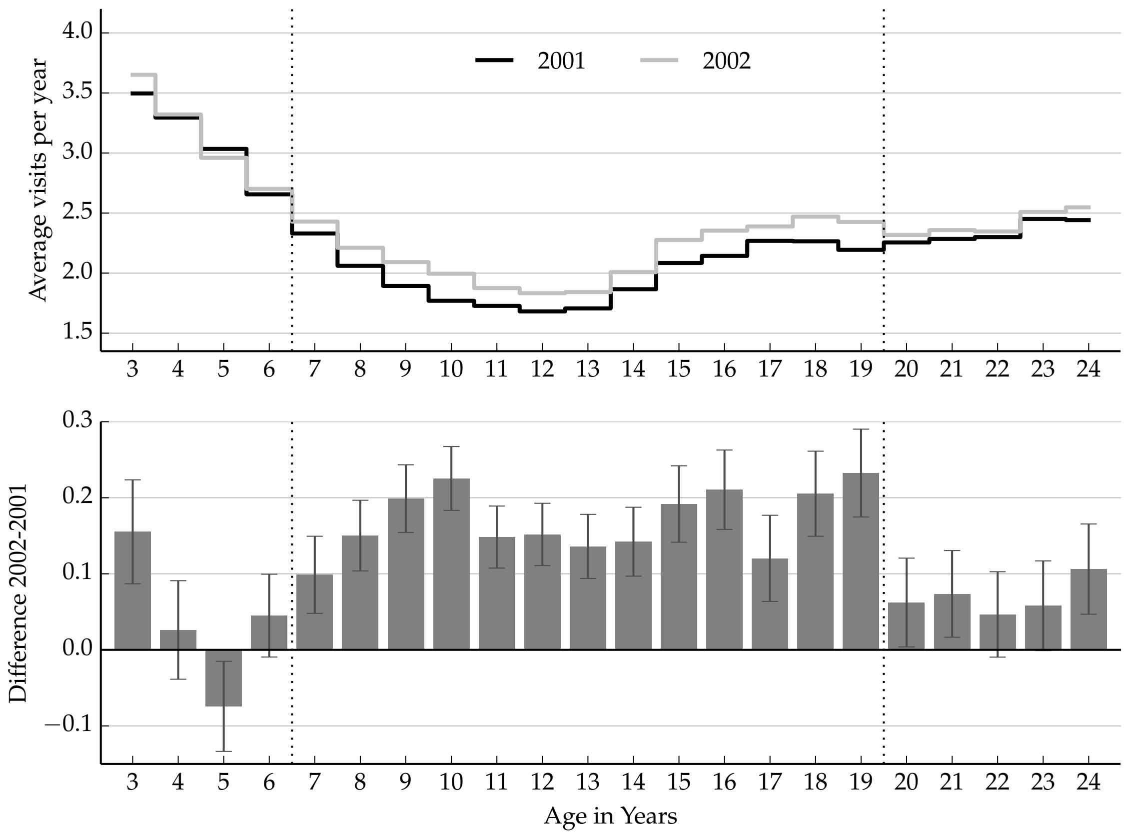
20. In principle, it would be possible to exclude fee-exempt visits from the analysis. We prefer to retain all visits in the sample for two reasons: First, the data do not allow for a clear-cut identification of visits that would have been fee-exempt after the reform. Second, the reform might also affect visits that were free of charge already before the reform. For example, it is conceivable that the reform increases the demand for (previously free) psychiatric care because this care typically follows referrals from a (previously costly, but now free) general practitioner.

21. Regression results reported in Table 2.10 in the Appendix confirm that the reduction in the monthly average fee per visit was highest for doctor visits, amounting to about 115 SEK. The corresponding reduction for non-doctor visits was only around 15 SEK.

2.3.3.2 Visits

In the upper panel of Figure 2.2, we plot the raw number of annual visits of doctor visits in 2001 and 2002. The corresponding graphs for non-doctor and inpatient visits are Figures 2.7 and 2.8 in the Appendix, respectively.[22] We distinguish between these types of visits throughout because of the different fees associated with these visits, where the policy change mostly affected fees for doctor visits and had no effect on fees for inpatient visits at all. The lower panel of each figure plots the *changes* in the number of visits after the reform.

Figure 2.2: Average Doctor Visits Before vs. After the Policy Change by Age.



Notes: The upper graph shows the average number of doctor visits per year before the policy change (2001) and after the policy change (2002) by age. The lower graph shows the difference between the 2002 and the 2001 averages. Numbers are annualized from monthly data. The sample consists of everyone living in Skåne continuously between 2001 and 2002 and being between 3 to 24 years for at least one month during these years.

The number of doctor visits (see Figure 2.2) has a U-shape, with a low of 1.7 at age 12 and a high of about double as much at age 3. As shown in the lower panel, there appears to be no discernible time trend in the average utilization of doctors for the young control group from 2001 to 2002, whereas visits seem to slightly increase for the old control group. For the

22. See table 2.11 for more precise numbers, aggregated by treatment and control group, and also for descriptives on subtypes of visits.

treatment group, in contrast, there is a substantial increase in the number of visits. The shift is very similar across age groups.

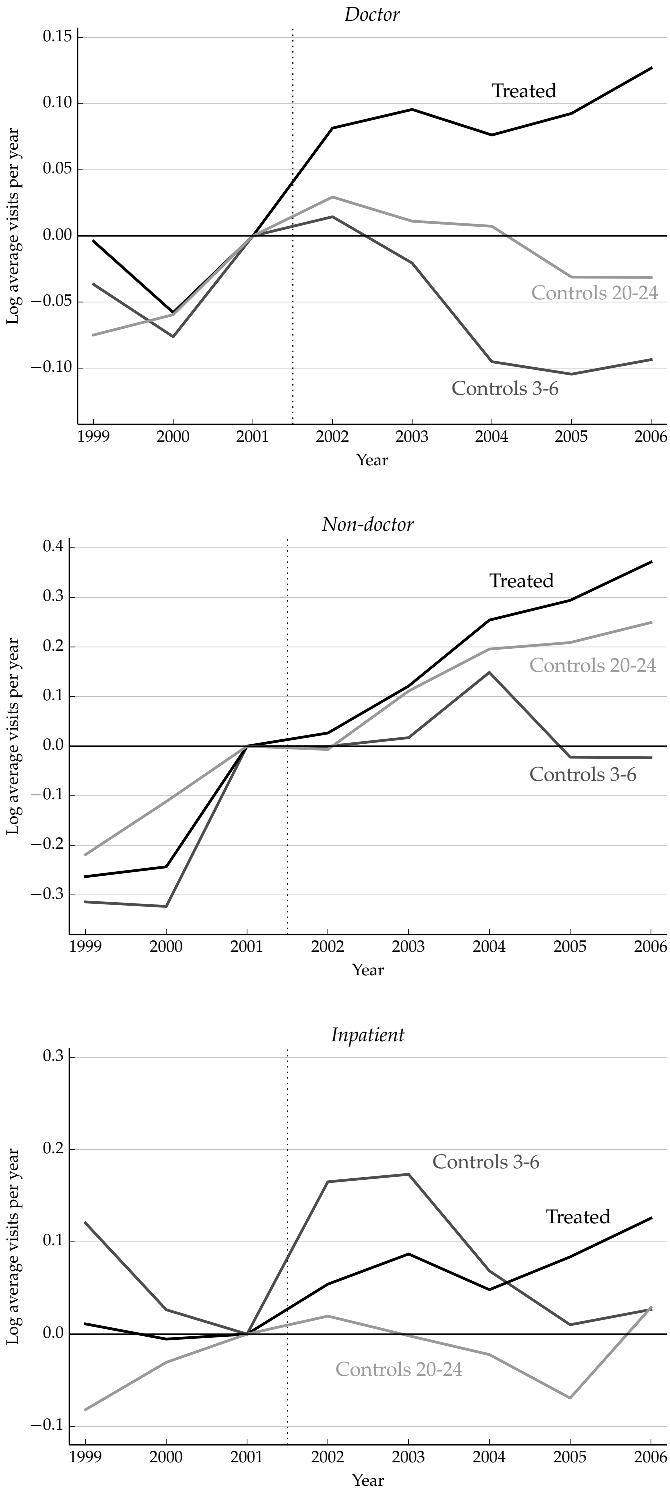
Figures 2.7 and 2.8 show the average numbers of non-doctor visits and inpatient visits. As compared to doctor visits, these numbers are one-third smaller for non-doctor visits and much smaller for inpatient visits, but both are U-shaped as well. For these types of care, fees were generally charged neither before nor after the reform. There are few visible differences between the years, one notable exception being a slight increase in non-doctor visits in the age group 15-18 years.

The difference-in-differences approach is based on the assumption that treatment and control groups would have exhibited similar time trends in the absence of treatment. One way to test the appropriateness of this assumption is to check whether it is fulfilled in years when no reform took place. In Figure 2.3, we show how the number of visits evolved over an extended year range around the reform. As for doctor visits, the 2002 increase in visits for the treated becomes even larger in subsequent years. This might represent a delayed effect of the reform but, as mentioned before, in order to avoid potential confounding with other factors, we prefer not to include these years in the econometric analysis. In the years prior to the reform, doctor visits evolve closely in the different age groups. The picture looks different for non-doctor visits, where patterns of utilization for treatment and control groups move in parallel over the whole period of time (2005 and 2006 are exceptions, but are also years away from the reform.). The same holds true for inpatient visits, although the fluctuations are in general somewhat higher.

In Figure 2.4 we plot the pattern of doctor visits around the thresholds that we exploit for the regression discontinuity estimations. In the left graph of the upper row, we show average visits by age for the 6/7 threshold before the reform (July 1999 - 2001), when turning 7 years implied being charged, potentially giving rise to a discontinuity in utilization. On the right of the upper row, we produce the same graph for the period after the reform (2002-2006), when this threshold was irrelevant for determining fees. As expected, before the reform there is a clear drop in visits for individuals who just became 7 years old, while the number is flat across the threshold for the time after the reform. In the lower row, the graphs look exactly reversed for the 19/20 threshold, consistent with the fact that it had only bite in the years after the reform. Importantly, for neither threshold is there visual evidence of intertemporal substitution in the sense that individuals would increase their utilization just before crossing the threshold and excessively lowering it thereafter.

The corresponding graphs for non-doctor and inpatient visits are shown in Figures 2.9 and 2.10, respectively, in the Appendix. As before, there appears to be no effect on non-doctor visits when using the 6/7 threshold, while turning 20 is accompanied by a visible decrease in visits. Inpatient visits appear unaffected by either threshold.

Figure 2.3: Number of Visits Over Time by Caregiver



Notes: Log average number of visits per year by caregiver in the treatment group (7-19 years) and in the control groups (3-6 and 20-24 years). Year 2001 is normalized to zero. Only non-preventive in-person visits plus contacts related to prescriptions.

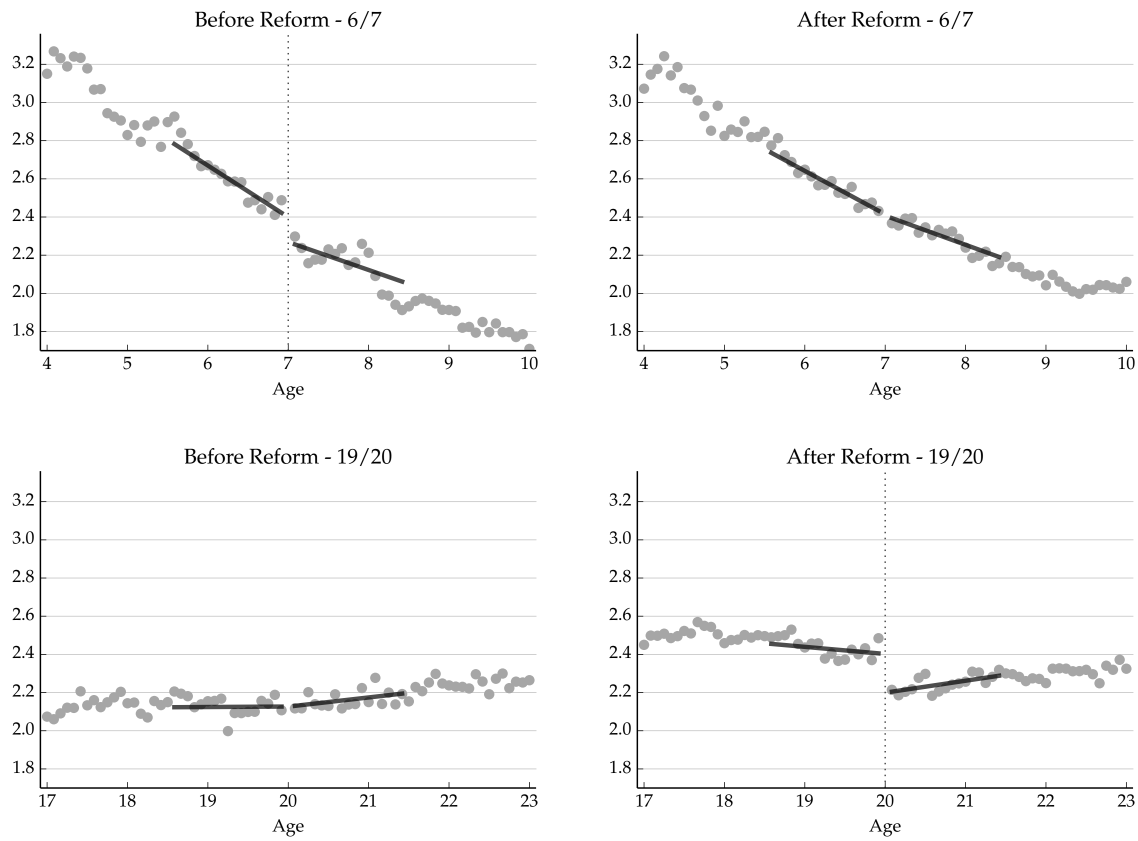
2.3.4 Econometric Method

We aggregate individuals who have the same age measured in months, since treatment depends only on age (besides time) and we only observe the year and month of birth rather than the exact date. We average individual outcomes and take the logarithm, so that the estimated effect has the interpretation of a percentage change. We estimate the following equation in a standard difference-in-differences (DiD) framework:

$$Y_{at} = \alpha + \beta Abolition_{at} + \lambda_{at} + \delta_t + \kappa_{at} + \varepsilon_{at} \tag{2.1}$$

where Y_{at} is the log average outcome Y in the age group a (age in months) in month t. $Abolition_{at}$ is an indicator for whether the age group was treated by the abolition of co-

Figure 2.4: Doctor Visits around RDD Thresholds



Notes: Annualized average number of doctor visits by age. Dots represent months. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. Vertical dotted lines indicate that copayments changed discontinuously at the threshold in the given time period. Dark lines are from fitted RDD models according to the specification described in Section 2.3.4, excluding month fixed effects.

payments in that month; that is, whether the individuals in that group were between 7 and 19 years old and the year was 2002. β is the effect on the treatment group and λ_{at}, δ_t and κ_{at} are age (in months) fixed effects, month fixed effects and treatment-group-specific month-of-the-year fixed effects, respectively.[23] ε_{at} is an error term which captures all other determinants of medical visits. Regressions are weighted by the number of individuals in each age group.

In regressions with yearly outcomes, we focus on those individuals that spend the whole year either in the treatment group or in one of the control groups. As a result, the treatment variable only takes on values 0 and 1, but no fractions. We replace δ_t and κ_{at} by year fixed effects and redefine age as the age in months at the beginning of the year.

In order to ensure valid inference in DiD models, standard errors need to be corrected for serial correlation in the outcome variable (Bertrand et al. 2004). Since sickness today increases the probability of sickness tomorrow, health care utilization is correlated within individuals over time. Moreover, it has been shown that month of birth (Doblhammer and Vaupel 2001) and economic conditions around birth (van den Berg et al. 2006) are correlated with life expectancy, consistent with the notion that these environmental circumstances affect a person's lifetime susceptibility to diseases. As a consequence, health care utilization is likely to be correlated among individuals of the same cohort. To account for both serial correlation within individuals and correlation within cohorts, we cluster standard errors at the birth year $\times$ birth quarter level.

In a DiD framework, the control groups are needed to account for any time trends concurrent with the reform that might confound the estimated treatment effect. An equivalent interpretation is that there are systematic differences between treatment and control group that need to be corrected for using data from a point of time when treatment did not take place. Here, the identifying assumption is that these systematic differences are sufficiently stable over time. We relax this assumption by estimating a regression discontinuity design (RDD), which can be estimated with data from a single point of time. Systematic differences between treatment and control groups are accounted for by focusing on an interval so close around the age threshold that it becomes reasonable to assume that the outcome is a smooth function of age. Any discontinuity in this smooth function can then be considered as the treatment effect. The disadvantage of the RDD is that it only identifies local effects right at the age thresholds, in contrast with the DiD, which identifies effects for the whole treatment group. Two other recent papers that estimate an RDD to exploit changes in cost-sharing at age thresholds are Shigeoka (2014) and Yang et al. (2014).

For the RDD, we estimate a local linear regression that allows for varying slopes below

23. Treatment-group-specific month-of-the-year fixed effects control for the varying degree of seasonal fluctuations across treatment and control groups.

and above the threshold:

$$Y_{at} = \alpha + \beta_1(Age_{at} - Threshold) + \gamma Free\ Care_{at} + \beta_2(Age_{at} - Threshold)Free\ Care_{at} + \delta_t + \varepsilon_{at} \tag{2.2}$$

where Age_{at} is the age in months of individuals in age group a in month t and $Threshold$ is the age threshold of interest (7 or 20 years). $Free\ Care$ is equal to one if $Age_{at} < Threshold$ and zero otherwise. As in Equation 2.1, δ_t are month fixed effects and ε_{at} is an error term which captures all other determinants of medical visits. Since we take deviations of Age_{at} from the $Threshold$, the coefficient γ directly estimates the treatment effect of enjoying free care. We choose a bandwidth of 18 months below and above the threshold. As explained above, observations right at the threshold are disregarded. Weighting is triangular, meaning that observations next to the threshold receive full weight and weights then linearly decrease until reaching zero at a distance of 18 months from the threshold. Age in months is a discrete rather than continuous variable, so we cluster standard errors at the age level (Lee and Card 2008).

2.4 Results

2.4.1 Overall Effect of the Reform

2.4.1.1 Number of Visits

Turning to our econometric analysis, we start with results from the difference-in-differences approach. Columns 1 and 5 in Table 2.2 show how the average number of visits per year of the overall treatment group (ages 7-19) was affected by the abolition of fees. We again distinguish between doctor visits, non-doctor visits and inpatient visits.

Using the control group of 3-6-year-olds, our findings suggest that the fee abolition increased doctor visits by 7.1 percent increase. Changing the control group to individuals between 20 and 24 years of age, the estimated increase in the number of doctor visits is somewhat smaller. It is now 5.7 percent, which is not significantly different from the one with the young controls.

Our identification strategy relies on the assumption that the different age groups associated with treatment and control would have exhibited similar time trends in the absence of the reform. This assumption appears the more questionable the more distant two given age groups are from each other. In particular, children who just turned 7 are potentially incomparable with young adults aged 20-24 and, similarly, individuals in their late adolescence are potentially incomparable with kindergarten children aged 3-6. In order to check if the results based on the whole treatment group are driven by inappropriately comparing age groups far away from each other, we split the treatment into subgroups and run regressions on each

Table 2.2: Effect of the Reform on the Number of Visits

	Young controls (3-6 Years)				Old controls (20-24 Years)			
	Overall (1)	7-10 Years (2)	11-14 Years (3)	15-19 Years (4)	Overall (5)	7-10 Years (6)	11-14 Years (7)	15-19 Years (8)
Doctor								
Abolition	7.13**	7.57**	6.73**	7.13**	5.67**	6.09**	5.29**	5.67**
	(0.89)	(1.07)	(1.08)	(1.15)	(0.76)	(0.91)	(0.96)	(1.06)
2001 Mean	1.98	2.00	1.74	2.19	1.98	2.00	1.74	2.19
N	4,896	2,304	2,304	2,592	5,184	2,592	2,592	2,880
Non-doctor								
Abolition	2.52	0.42	1.56	5.41*	2.27	0.15	1.33	5.16
	(2.02)	(2.69)	(2.22)	(2.41)	(2.27)	(2.75)	(2.48)	(2.68)
2001 Mean	1.06	0.94	0.92	1.30	1.06	0.94	0.92	1.30
N	4,896	2,304	2,304	2,592	5,184	2,592	2,592	2,880
Inpatient								
Abolition	-10.16*	-6.76	-11.47*	-11.88*	2.20	5.56	0.80	0.51
	(4.08)	(4.82)	(5.04)	(4.98)	(3.84)	(4.59)	(4.82)	(4.80)
2001 Mean	0.06	0.04	0.05	0.09	0.06	0.04	0.05	0.09
N	4,712	2,190	2,205	2,523	5,042	2,520	2,535	2,853

Notes: Each panel in each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. The dependent variable is the log monthly number of visits. Means are scaled up to annual figures. In all regressions, we control for age in months, month and treatment-group specific seasonal effects.
* and ** denote significance at the 5 and 1 percent level, respectively.

of them separately. As columns 2-4 and 6-8 show for the young and old control groups, respectively, the estimated treatment effect is very similar across all subgroups, even for the more distant ones in columns 4 and 6. In sum, not only does the estimated treatment effect not depend on the control group used, it is also remarkably constant for different subgroups among the treated.

As for non-doctor visits, we find no effect at all, neither using the young nor the old control group. An exception is column 4, but note treatment and control group are distant from each other. The absence of a response for non-doctor visits probably reflects the fact that these were most often free of charge both before and after the policy change was implemented (see Figure 2.1). Moreover, any potential positive effect may have been mitigated by a substitution with doctor visits, which were more expensive before the reform and thus became relatively more attractive afterwards.

If copayments make patients forgo timely treatment in outpatient care, spill-over effects on the number of visits in inpatient care could arise. The additional spending on inpatient care might then offset any cost-savings from copayments in outpatient care. Such a mechanism has been shown to be at work for populations of elderly people (Chandra et al. 2010; Trivedi et al. 2010), but not by the Rand HIE for the non-elderly population. Given that negative consequences from forgone care take much longer to manifest themselves for children than for the elderly, we do not expect large effects on inpatient care due to the copayment reform. Indeed, as the bottom panel of Table 2.2 shows, we do not find robust significant results on the number of inpatient visits. There is an effect using the young controls, but this seems driven by age groups further away from each other and - looking at Figure 2.8 - an exceptionally large increase in visits for 3-year-old individuals.

The first two columns of Table 2.3 summarize the estimates from the RDD regressions. These are local treatment effects at the thresholds of 6/7 and 19/20 years. Receiving free care is estimated to increase visits by 5.5 and 9.2 percent, respectively. It is important to note that the RDD estimates are of the same order of magnitude as the results above, which were 5.7 and 7.1 percent, depending on the control group. This lends support to our findings. As above, non-doctor visits are unresponsive to copayments for the 6/7 threshold. However, they decrease significantly when individuals turn 20 years. A potential explanation is the larger number of fee exemptions for children under 18 (related to speech therapy and psychiatric care), which might mitigate the burden of cost-sharing. Also, note that children under 18 have a joint out-of-pocket cap with their siblings, making them more likely to exceed it. The coefficients on inpatient visits are insignificant.

We also check that there are no discontinuities in visits at times when the thresholds were not associated with changes in cost-sharing requirements. This was the case in the years 2002 to 2006 for the 6/7 threshold and between July 1999 and 2001 for the 19/20 threshold. As the last two columns of Table 2.3 show, the estimated effects are almost all small and insignificant. This strongly suggests that no other age-dependent factors that might change

Table 2.3: Effect of the Reform on the Number of Visits - RDD

	Effective Thresholds		Control Thresholds	
	Before Reform - 6/7 (1)	After Reform - 19/20 (2)	After Reform - 6/7 (3)	Before Reform - 19/20 (4)
Doctor				
Free care	5.45**	9.22**	-0.14	0.10
	(1.59)	(1.41)	(0.91)	(1.34)
Mean Above	2.27	2.20	2.41	2.13
N	1,020	2,040	2,040	1,020
Non-doctor				
Free care	1.13	13.45**	0.59	-0.12
	(2.60)	(1.19)	(1.35)	(1.96)
Mean Above	0.95	1.47	1.13	1.15
N	1,020	2,040	2,040	1,020
Inpatient				
Free care	6.24	4.25	14.73**	-7.58
	(7.52)	(4.65)	(3.41)	(5.67)
Mean Above	0.05	0.10	0.05	0.11
N	959	2,015	1,886	1,014

Notes: Each panel in each column shows the treatment effect from a separate RDD estimation. Individuals are grouped by age (in months) and regressions are weighted by group size. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. 'Free care' is equal to one if an individual is below the threshold. Standard errors are clustered at the age level and shown in parentheses. The dependent variable is the log monthly number of visits. Means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects) and scaled up to annual figures. Using a bandwidth of 18 months, we estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. * and ** denote significance at the 5 and 1 percent level, respectively.

around the threshold drive our results. An exception is a positive estimate on inpatient visits in column 3, but this is probably accidental as it quickly disappears when we increase the bandwidth of the regressions.

2.4.1.2 Likelihood of Any Visit

In the DiD framework, we also consider effects along the extensive margin, that is, on the likelihood of having at least one medical visit of a certain type during the year. This outcome is of interest because the marginal benefit to medical visits may be higher at the margin of having one visit, and the policy change may therefore be viewed as more successful if it makes individuals willing to visit a medical provider at least at one point during the year and not only affects people's average number of visits.

Table 2.4 reports results from linear probability models that estimate effects along the extensive margin. Since the unit of analysis is now person-years rather than person-months, the number of observations is much smaller.[24] Independently of the control group used, results suggest that the probability of having at least one doctor visit increased by somewhere between 1 and 2 percentage points when fees were abolished. This effect is not large given the baseline probability of around 63 percent and it can only explain a small share of the increase in overall visits; yet it shows that our results in Table 2.2 are not entirely driven by children and adolescents that in any case would have gone to the doctor at some point during the year.

The effects on non-doctor visits are of similar size as the ones on doctor visits when using the control group of individuals aged 3 to 6. For the old control group they are significantly positive only for the more comparable subgroup of 15-to-19-year-olds. There is no robust evidence that the probability of using inpatient care was affected by the reform. The generally small effects along the extensive margin provide an indication that individuals do not in general stay away completely from the medical system due to financial constraints, but instead due to other factors such as health or preferences. In our setting, another interpretation is also possible, however: many individuals might have only learned about the policy change upon visiting a health care provider and for this reason the incidence of a first visit was not affected.

2.4.2 Sensitivity Analysis

The previous section showed that the reform had a significant impact on children's use of health care in Skåne. Before studying how the reform affected specific subgroups of individuals and health effects, we first evaluate the validity of the DiD approach and the robustness of the estimates to alternative specifications. Given that non-doctor visits and inpatient visits exhibited little or no response to the reform, we will focus on doctor visits in the following.

2.4.2.1 Potential outbreak of disease among school students

Our difference-in-differences approach makes the identifying assumption that in the absence of treatment the treatment group would have exhibited the same time trend in the outcome as the control group. Time trends in utilization could, for example, be due to trends in the outbreak of infectious diseases. There is no problem for identification if such outbreaks affected all age groups equally. Note, however, that the age groups affected by the reform (7-19 years) correspond exactly to the group of school students. School students have more interaction with each other than younger and older individuals, which makes the spread of diseases faster and more thorough in this group. Therefore, any finding of a positive effect

24. As explained in subsection 2.3.4, we focus on those that spend the whole year either in the treatment group or in one of the control groups. This sample restriction decreases the sample size further, but has only little impact when applied to the estimations above that are run at the month level and use the number of visits as the outcome (see Table 2.12 in the Appendix).

Table 2.4: Effect of the Reform on the Likelihood of Any Visit

	Young controls (3-6 Years)				Old controls (20-24 Years)			
	Overall	7-10 Years	11-14 Years	15-19 Years	Overall	7-10 Years	11-14 Years	15-19 Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Doctor								
Abolition	0.014**	0.016**	0.015**	0.012*	0.016**	0.018**	0.016**	0.014**
	(0.004)	(0.006)	(0.006)	(0.005)	(0.003)	(0.005)	(0.005)	(0.004)
2001 Mean	0.625	0.631	0.594	0.654	0.625	0.631	0.594	0.654
Non-doctor								
Abolition	0.010*	0.009	0.008	0.013*	0.006	0.005	0.005	0.009*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
2001 Mean	0.252	0.250	0.232	0.278	0.252	0.250	0.232	0.278
Inpatient								
Abolition	-0.000	0.002	-0.002	-0.001	0.002	0.003*	0.000	0.001
	(0.002)	(0.003)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
2001 Mean	0.033	0.026	0.033	0.043	0.033	0.026	0.033	0.043
N	364	170	170	172	388	194	194	196

Notes: Each panel in each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. The dependent variable is an indicator for whether an individual had any visit in the current year. In all regressions, we control for age in months and time (month or year). * and ** denote significance at the 5 and 1 percent level, respectively.

on the treated might simply reflect an outbreak of a disease that particularly affected school students.

There are three reasons why we do not believe that this is the case: First, any health shock that affected school students would also have affected other individuals, albeit to a lesser extent. However, looking at Figure 2.2, the number of visits in the control groups does not exhibit any discernible trend over time, suggesting that no major health shock occurred.

Second, if schools facilitate contagion, then so do families, too. We ran a regression in which we tested for different time trends between those 3-to-6-year-olds who had older siblings affected by the reform and those without. There is no evidence that children with older siblings had more (or less) visits to the health care system after the reform. The effect on the absolute number of doctor visits is -0.06 (standard error: 0.04). Note that this regression also provides evidence against other spill-over effects on younger siblings. For example, one might conjecture that mothers who take their 10-year-old child to the doctor find it convenient to also bring along another, possibly preschool child. Since only individuals with older siblings can be subject to such a mechanism, this result speaks against spill-over effects.

Finally, we counted local newspaper articles using the keywords "flu" and "infections" and found the number of articles to be relatively stable over the whole time period.[25]

2.4.2.2 Congestion

Another potential violation of the identifying assumption arises from congestion effects. As described in Section 2.3.1, Skåne has rather long waiting times in health care. Additional demand triggered by the abolition of fees for 7-to-19-year-olds might increase waiting times for everyone. As a result, individuals in the control groups would be deterred from using health care as often as before, and treatment effects would be overestimated. But note that also the treated would not increase their visits as much in the presence of congestion as in its absence. Moreover, once again, there is no indication that patients in the control groups reduced their utilization after the reform. Also note that while the treatment group (7-19 years) represents 17 percent of the Skåne population in 2001, it only accounts for 8.5 percent of the overall doctor visits in the same year. Our estimates therefore imply that the reform increases overall visits by less than 1 percent, a number lying well within the usual range of year-to-year fluctuations. Finally, we obtained data on the evolution of the total number of doctors in Skåne from the Swedish National Board of Health and Welfare. Between 1996 and 2010, the percentage increase of doctors was actually highest in the year of the reform, 2002 (4.3%), followed by the years 2001 and 2003.

25. Specifically, we searched the database *Mediearkivet* for articles in local newspaper *Sydsvenskan* between 2000 and 2005. See Figure 2.11 in the Appendix.

2.4.2.3 Choice of controls and functional form

In Table 2.5, we evaluate to what extent our estimates vary with the choice of control variables and functional form. The upper panel focuses on the percentage effect on the number of visits, while the lower panel is concerned with the effect along the extensive margin.

Table 2.5: Robustness to Choice of Controls and Functional Form

	Number of Doctor Visits			
	Baseline (1)	Indiv. Level (2)	With Controls (3)	Neg. Bin. (4)
Young controls (3-6 Years)				
% Change	7.13	6.61	6.59	6.80
N	4,896	4,898,199	4,898,128	4,898,199
Old controls (20-24 Years)				
% Change	5.67	5.01	5.03	5.46
N	5,184	5,177,882	5,177,858	5,177,882
	Likelihood of Any Doctor Visit			
	Baseline (1)	With Controls (2)	Probit (3)	Logit (4)
Young controls (3-6 Years)				
Abolition	0.014	0.014	0.013	0.012
N	364	368,411	368,413	368,413
Old controls (20-24 Years)				
Abolition	0.016	0.016	0.016	0.016
N	388	391,636	391,638	391,638

Notes: Each panel in each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year × birth quarter level and shown in parentheses. In the upper part, the dependent variable is the number of doctor visits per month. Column 1 reproduces the estimate from our baseline specification. Column 2 reports results from an individual-level-regression that does not use the logarithm of the outcome. The percentage change is the abolition effect divided by the 2001 mean of those 7-to-19-year-olds that were used in the regression. Column 3 is the same as Column 2, except that we additionally control for municipality of residence and gender. Column 4 gives estimates from a negative binomial count data model that flexibly allows for overdispersion. In the lower part, the dependent variable is an indicator for whether an individual had any doctor visit in the current year. Once again, column 1 reproduces the baseline specification. Column 2 reports results from an individual-level-regression, with additional controls for municipality of residence and gender. Columns 3 and 4 report marginal effects from a probit and logit model, respectively. In all regressions, we control for age in months and time (month or year). In regressions with monthly outcomes, we additionally control for treatment-group-specific seasonal effects.

In our baseline regression reproduced in column 1 of the upper panel, we collapse observations of individuals from the same age group and take the logarithm of their average number of visits. Alternatively, one can also estimate the effect on the absolute number of visits and compute the percentage change through division by the mean utilization. Column

2 shows that the estimated percentage effect is very similar to the one from the baseline specification.

In column 3, we add more controls to the regression. While this gives additional plausibility to the common trends assumption and might help increase efficiency, it is not strictly necessary for the difference-in-differences model to work. Specifically, we control for municipality of residence and gender and find the results to be virtually unaltered.

In column 4, we specify a count data model that takes account of the nonnegative nature of the number of visits. A linear model such as ours might yield negative predictions for the number of visits, which would be at odds with reality. Although there is no reason to expect that inaccuracy of predictions translates into biasedness of the treatment effect of interest, we check how robust our estimates are when using a negative binomial model.[26] Regression coefficients are directly interpretable as percentage changes. Once again, the estimates do not differ substantially from the baseline specification.

In the lower panel of Table 2.5, we also test the sensitivity of the effects on the extensive margin. As above, they are also very similar across all specifications.

2.4.2.4 Dynamics

Next we explore whether our finding of a significant reform effect may spuriously reflect intertemporal substitution. More precisely, once it was announced in late October 2001 that fees would be abolished from January 1, 2002 onwards, individuals might have postponed visits that they originally had planned to make during the rest of the year. Such a delay-catch-up behavior should result in a drop in the number of visits in November/December 2001 and a spike directly afterwards.[27]

To analyze the short-term dynamics of the reform, we run a regression in which we interacted an indicator for belonging to the treatment group with each month between July 2001 and June 2002. We plot the results of this event study in Figure 2.5. Note that in this regression all estimates are relative to the months January-June 2001 and July-December 2002. Because we are interested in effects relative to months just before the announcement of the reform, we reduce the estimates by the average effect between July-October 2001.[28]

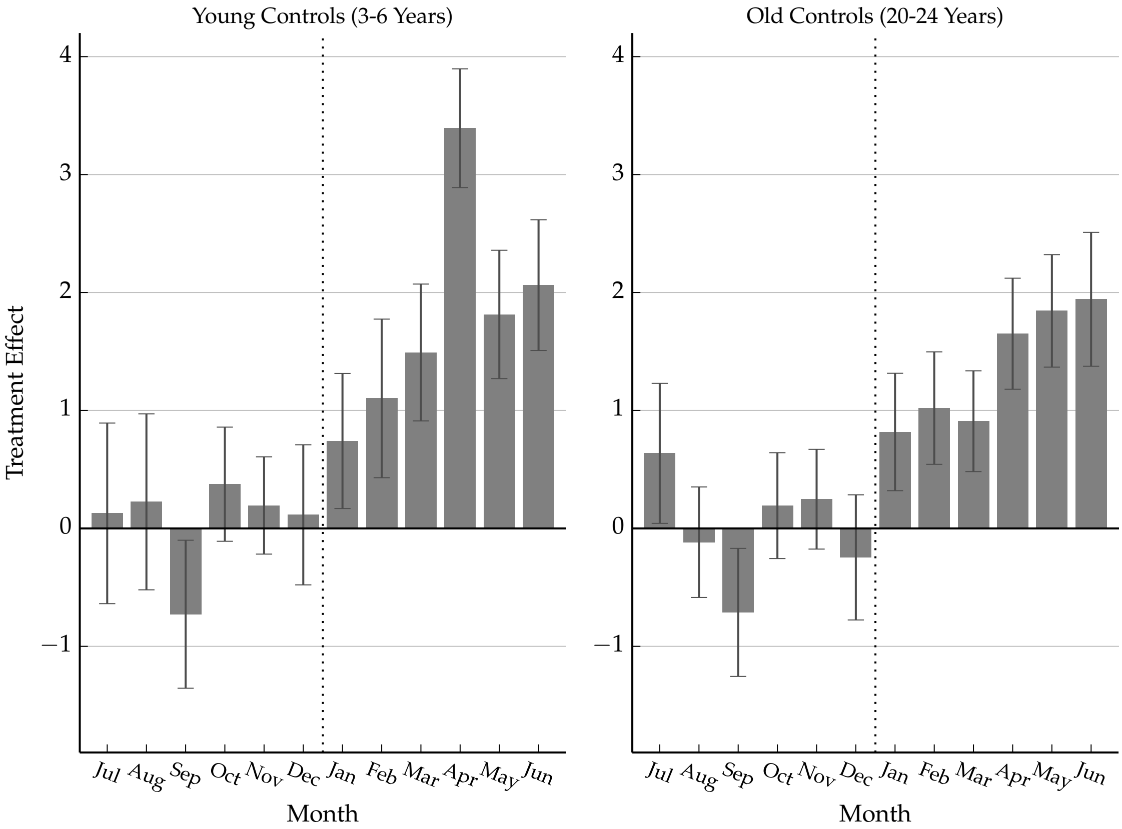
No matter which control group we use, there is no evidence of a deferral of visits in late 2001 and there is no surge in visits right after the reform. It is therefore unlikely that our estimated effects are driven by intertemporal substitution. If anything, it seems that the full effect of the reform takes some time to set in. There are two potential explanations for

26. This model specifies $E[Y] = exp(X'\beta)$ (X being a vector of explanatory variables) and flexibly allows for overdispersion. This is an NB2 model in the notation of Cameron and Trivedi (2013).

27. Chandra et al. (2010) show that such behavioral adjustments are at work in the opposite case of an *introduction* of copayments for prescription drugs.

28. The unadjusted regression estimates can be found in Table 2.13 in the Appendix. We only consider six months before and six months after the policy change since we would otherwise not be able to identify treatment-group-specific month-of-the-year fixed effects.

Figure 2.5: Treatment Effects by Month for Doctor Visits by Control Group



Notes: Effects on doctor visits by month between July 2001 and June 2002 by control group. We only consider six months before and six months after the policy change, since otherwise we would not be able to identify treatment-group-specific month-of-the-year fixed effects. We normalize December 2001 to zero because all estimated effects are relative to the months January-June 2001 and July-December 2002, but we are interested in effects relative to months just before the reform. Numbers are annualized from estimates at the month level. The sample consists of everyone living in Skåne continuously between 2001 and 2002 and being between 3 to 24 years for at least one month during these years. Vertical lines denote 95% confidence bands.

this finding: First, as indicated above, people have to become knowledgeable about the fee abolition. Since the reform was decided on only two months in advance, the word probably had not spread to everyone at the time of implementation. Second, since waiting times may be quite long, some individuals visiting health care in the beginning of 2002 probably sought care already before the reform was announced, and made their decision to seek care on the assumption that fees would remain in place.[29]

In Table 2.14 in the Appendix we report results based on the assumption that the treatment effect is delayed and only sets in after some time. We have redefined the abolition variable to equal zero in the first three months of 2002. As can be seen in the table, this does not

29. Figures 2.12 and 2.13 in the Appendix show the dynamics of the treatment effect for non-doctor and inpatient visits. Once again there is no robust evidence that these visits increased after the reform, not even with some delay.

make very much of a difference from our main results. For doctor visits, the relative change increases from 7.1 to 9.2 percent when using the younger control group, and from 5.7 to 8.2 when using the older control group.

2.4.3 Visits by Characteristics

In this section, we examine if effects differ by characteristics such as socioeconomic background or gender, using detailed individual-level information.[30] We focus on doctor visits because our analysis so far suggested that they were more strongly affected by the reform than other types of medical services. When considering family characteristics, we only use the younger control group, since adult individuals aged 20-24 - many of whom no longer live together with their parents - probably decide about their health care utilization themselves and independently of their parents' characteristics. We also exclude children whom we identify to live separate from the mother. This is because the way that characteristics of the mother determine a child's health care use - which she traditionally takes care of - might differ from that of the father, and a larger fraction of children lives at least with the mother. In this section, we only present results from the DiD analysis. RDD estimations are qualitatively similar and contained in Tables 2.17 to 2.21 in the Appendix.

2.4.3.1 Gender and Mother's Education

First, in Table 2.16 in the Appendix, we split the sample by gender. Girls see a doctor more often than boys and some types of visits are only undertaken by girls (e.g., visits related to birth control). There is no evidence that girls would respond differently to cost sharing than boys, however; irrespectively of the control group chosen and irrespectively of whether we look at the internal or external margin, results are not significantly different for boys and girls. An exception are the RDD results, which indicate that the response is smaller and insignificant for boys across the 6/7 threshold. The reason for this finding is unclear, but it cannot be ruled out that parents treat young boys more favorably than young girls.

Next, in Table 2.6, we stratify by maternal education. Theoretically, education may affect the response to our reform in either direction. On the one hand, higher educated mothers can be assumed to appreciate the value of health care better, especially with respect to children, and therefore react less strongly to financial incentives. Indeed, Goldman and Smith (2002) showed that the more educated patients are more likely to adhere to the therapy of chronic conditions. Similarly, Cutler and Lleras-Muney (2006) argue that more education leads to different thinking and decision-making patterns in general. Of course, higher educated individuals also tend to have higher incomes, which may lead to a smaller sensitivity to copayments since budget restrictions are less important. We return to the income dimension shortly.

30. Table 2.15 in the Appendix shows characteristics both of the child or adolescent and the mother.

Table 2.6: Effect of the Reform by Mother's Education

	Elementary Schooling	Secondary Schooling	University
	3-19 Years	3-19 Years	3-19 Years
	(1)	(2)	(3)
Number of Visits			
Abolition	8.52**	6.70**	6.45**
	(2.89)	(0.98)	(1.71)
p-value	-	0.553	0.539
2001 Mean	2.04	2.03	1.91
N	4,896	4,896	4,896
Any Visit			
Abolition	0.02	0.02**	0.02*
	(0.01)	(0.01)	(0.01)
p-value	-	0.579	0.727
2001 Mean	0.63	0.64	0.62
N	364	364	364

Notes: Each panel in each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. In the upper panel, the dependent variable is the log monthly number of doctor visits. Means are scaled up to annual figures. In the lower panel, the dependent variable is an indicator for whether an individual had any doctor visit in the current year. In all regressions, we control for age in months and time (month or year). In regressions with monthly outcomes, we additionally control for treatment-group-specific seasonal effects. P-values are from t-tests of differences in abolition effects in comparison with "Elementary Schooling". * and ** denote significance at the 5 and 1 percent level, respectively.

On the other hand, higher educated mothers may also be the ones who are better informed about the policy change through a larger use of newspapers and other media. Put another way, individuals unaware of the reform do probably not respond to it. Higher educated mothers may also have healthier children (Lundborg et al. 2014b), which may lead to a different response to copayments.

Table 2.6 provides no clear evidence that effects would differ depending on the mother's level of education. For the number of visits, effects are somewhat larger for lower educated, but differences are not significant. The picture is also mixed when looking at the probability of having at least one visit. There is some indication that the effect is larger if the mother has elementary rather than secondary education.

2.4.3.2 Family Income

In Table 2.7, in the top row, we show results after splitting the sample according to family income. Here, the evidence clearly points in the direction of smaller effects in higher income households. This is in line with the idea that budget constraints are more important when lower-income households decide on their consumption of medical care. In the lowest income quartile, visits increased by 12 percent, but only by 5-7 percent in the higher quartiles. Low-income households thus responded more than double as much to the reform as higher-income households, and the difference is highly significant. As Table 2.7 also shows, this income gradient in the response is present across all ages in the treatment group and becomes larger when children grow older. For the probability of having at least one visit (bottom panel), no clear-cut conclusions can be drawn.

The finding that children from low-income families respond to the reform more strongly might be driven by other factors that correlate with income. For instance, single mothers are disproportionately often low-income. Since single mothers might differ also in other dimensions (such as unobserved preferences and behaviors), we gauge whether the differential effect prevails if we keep family type fixed. Here we juxtapose bottom income (1st quartile) and top income (4th quartile) mothers. The results are presented in Panel A of Table 2.8. We find that the difference in income groups is still present if we condition on mothers who are in a partnership.

Many mothers might have low income because they stay at home rather than go to work, for example because they do not find a job. Staying at home implies low opportunity cost of time. Together with monetary cost in the form of copayments, the cost of time affects the decision on taking a child to a doctor. Assuming there is more less effective care than highly effective care and benefits of care are perceived as similar irrespective of income, a marginal reduction in copayments is more likely to increase utilization for mothers with low opportunity cost of time. When we split the sample by whether a mother works in Panel B (defined to be true if she has nonzero earnings), we find that regardless of whether or not the mother works, the response varies by income in a similar way.

In Panel C of Table 2.8, we ask whether the effects by income are driven by immigrant mothers. We contrast Swedish mothers with those who are originally from Africa, South America, Asia, the Soviet Union or unknown countries. Many factors, such as culture, resources, health, and knowledge might have an impact on the response to the reform. As the results show, the effects by income are very similar for Swedish mothers as for the overall sample and insignificant for immigrant mothers, probably due to the small number of observations.

Finally, in Panel D, we look at the interaction of education and income. Once again, education does not appear to have an independent effect on the response to the reform. Both mothers with and without college education respond more when they are low-income. Summing up Table 2.8, results appear to reflect differential responses by resources rather than other factors correlated with income.

Table 2.7: Effect of the Reform by Family Income

	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
	3-19 Years	3-19 Years	3-19 Years	3-19 Years
	(1)	(2)	(3)	(4)
Number of Visits - All Ages				
Abolition	11.74**	7.07**	4.85**	5.17**
	(1.33)	(1.42)	(1.58)	(1.92)
p-value	-	0.017	0.001	0.005
2001 Mean	2.00	2.04	2.02	1.95
N	4,896	4,896	4,896	4,896
- 7-10 Years				
Abolition	10.21**	7.81**	3.85	6.64**
	(1.57)	(1.72)	(2.11)	(2.12)
p-value	-	0.302	0.015	0.175
2001 Mean	2.05	2.12	2.08	1.94
N	2,304	2,304	2,304	2,304
- 11-14 Years				
Abolition	11.97**	5.63**	0.57	5.59*
	(1.91)	(2.01)	(0.43)	(2.38)
p-value	-	0.022	0.000	0.037
2001 Mean	1.82	1.85	1.82	1.67
N	2,304	2,304	2,304	2,304
- 15-19 Years				
Abolition	13.18**	1.26**	6.08*	3.93
	(1.87)	(0.45)	(2.38)	(2.19)
p-value	-	0.000	0.019	0.001
2001 Mean	2.15	2.15	2.18	2.16
N	2,592	2,592	2,592	2,592
Any Visit				
Abolition	0.02**	0.01	0.02**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
p-value	-	0.351	0.772	0.609
2001 Mean	0.63	0.64	0.64	0.62
N	364	364	364	364

Notes: Each panel in each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. In the upper panels, the dependent variable is the log monthly number of doctor visits. Means are scaled up to annual figures. In the bottom panel, the dependent variable is an indicator for whether an individual had any doctor visit in the current year. In all regressions, we control for age in months and time (month or year). In regressions with monthly outcomes, we additionally control for treatment-group-specific seasonal effects. P-values are from t-tests of differences in abolition effects in comparison with "1st Quartile".

Table 2.8: Family Income and Maternal Characteristics - 1st Quartile vs. 4th Quartile

	In Partnership		Single	
Panel A	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Abolition	12.73**	5.22**	11.38**	-43.53*
	(2.72)	(1.92)	(1.69)	(17.47)
2001 Mean	1.86	1.94	2.06	5.05
N	4,880	4,896	4,896	1,255
	Stays At Home		Works	
Panel B	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Abolition	12.87**	-3.92	11.40**	5.04**
	(3.57)	(7.21)	(1.49)	(1.91)
2001 Mean	2.07	3.01	1.98	1.94
N	4,865	3,328	4,896	4,896
	Swedish		From Other Countries	
Panel C	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Abolition	12.04**	4.67*	9.95	-0.81
	(1.40)	(1.86)	(8.38)	(11.39)
2001 Mean	2.03	1.96	2.61	5.28
N	4,896	4,896	2,797	1,065
	Has No College		Has College	
Panel D	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Abolition	11.32**	6.51*	14.84**	4.07
	(1.64)	(2.61)	(4.05)	(2.36)
2001 Mean	2.02	2.01	1.94	1.86
N	4,896	4,896	4,851	4,896

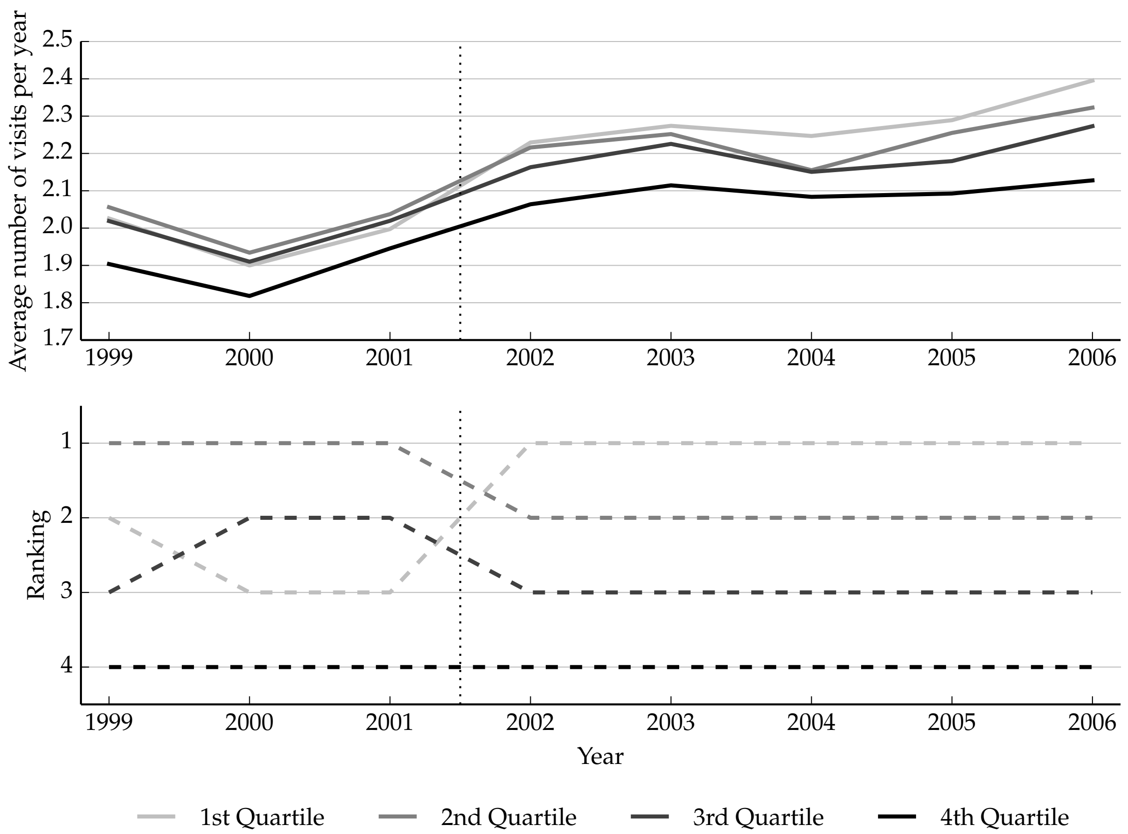
Notes: Each panel in each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. The control group are individuals aged 3-6. Means are scaled up to annual figures. * and ** denote significance at the 5 and 1 percent level, respectively.

2.4.4 Health Effects

So far we have shown that the reform increased the frequency at which individuals see a doctor. A crucial issue is whether the additional visits also brought about positive health effects or whether they only reflect unnecessary visits (or visits with no lasting effects on health). Evaluating the effects on health is a difficult task, however, since our administrative registry data do not contain direct information on health status and the propensity to use health care is affected by many factors other than health.

First, we draw on previous literature which showed that child health improves with parental income in a vast array of countries. This income gradient in child health can be expected to translate into an income gradient in utilization, meaning that low-income children see a doctor less often. Indeed, as Figure 2.6 shows, the number of doctor visits in the group of 7-to-19-year-old individuals is perfectly negatively correlated with parental income in 2002 and subsequent years, when copayments had been abolished for this age group. In contrast, in the years before the reform, children from the bottom quartile rank only second or third in terms of utilization. This is explained by our finding that low-income children are hit harder by copayments, as demonstrated in the previous section. This suggests underuse of care by low-income children with potentially negative effects on health outcomes.

Figure 2.6: Doctor Visits by Income Quartile over Time



Notes: The upper panel shows the average number of doctor visits in the treatment group (7-19 years) by income quartile over time. The lower panel shows the corresponding ranking in the number of doctor visits within a year. Missing income information for 2003 has been imputed from 2002 if possible.

Next, we investigate whether the response differs by type of doctor visit. Traditional economic theory suggests that patients initially reduce least effective visits in response to copayments. This does not imply a welfare loss, since - for least effective visits - the cost of care is higher than the benefits. However, patients are prone to make mistakes: They might accidentally reduce highly effective care before reducing less effective care. Therefore, the idea here is to compare the effects on presumably more effective and presumably less effective care. It would be worrying if the effect on more effective care was as large as the effect on less effective care.

For example, acute visits that are scheduled within 24 hours of the appointment and are probably more effective than non-acute visits. Table 2.22 reports significantly positive effects also on acute visits, which are not always significantly different from the effect on non-acute visits. As another example, revisits are only scheduled when deemed necessary by a doctor, which means they are presumably more effective than new visits. Once again, Table 2.22 shows that effects on revisits are significantly different from zero and in one case (column 4) even significantly exceed the effect on new visits. We also report effect for visits to the emergency room that may be suspected to be more effective (although patients might seek care in the emergency room not only for a broken leg, but also for more minor conditions that tend to fade away over time, such as scrapes and bruises). Overall, we find positive effects also on visits that are relatively more effective, which is suggestive of beneficial health effects.[31]

In Table 2.23, we estimate the treatment effects separately for sickly and non-sickly individuals. Here, we equate "sickly" with having a certain chronic condition. Since the sickly see a doctor more often, copayments impose a larger financial burden on them, possibly leading to a larger response.[32] If the sickly respond more to the reform, effects on health become likely. Note, however, that some of the sickly exceed the out-of-pocket cap before the reform, so that their response is mitigated. If the effect on the sickly is smaller, not much on health effects can thus be learned.

In Table 2.23, we define having a certain condition as being diagnosed with it at least once in 2002 or 2003. We choose these years because there are no copayments that could affect the probability of being diagnosed. We omit them from the regressions to avoid potential biases arising from correlation of having a certain condition with health care use. Because the probability of being diagnosed varies with age and year, we only present RDD results, which do not rest on the common trends assumption. In many cases, the sickly tend to respond less than the non-sickly, although this difference is only statistically significant for asthma, allergic rhinitis and having any condition. We also rank individuals by their visits to a doctor in 2002 and 2003. We define as "sickly" those individuals in the top 20% of the distribution and as "non-sickly" the remainder.[33] Although the use of doctor visits is almost three times as high for the sickly, their percentage response is remarkably similar to the one

31. We also tested whether visits associated with certain diagnoses respond differently. For example, one would be worried if individuals see a doctor more often in connection with cancer after the reform. However, the number of visits with a particular diagnosis is quite low, even if we aggregate diagnoses. As a consequence, effects are only imprecisely estimated and almost always not significantly different from the baseline effects. Results are available on request.

32. An alternative explanation for a larger response would be that the sickly have a higher propensity for moral hazard, perhaps due to a "larger room for adjustment" since the propensity for visits is higher.

33. Since utilization varies with age, each individual is ranked in relation to other individuals with the same birth year.

of the non-sickly. Among low-income individuals, the effect on the sickly is considerably larger in at least one specification, potentially pointing to beneficial health effects for this group.

2.5 Conclusion

In this paper, we exploited a policy change in Sweden to study how copayments for medical visits affect children's and adolescents' usage of health care. The effects of cost-sharing on young patients is particularly interesting because these individuals are typically not yet decision-makers for themselves and forgoing treatment in response to copayments may have larger and longer-lasting health consequences. Yet, there is little previous evidence on if, and how, the health care demand of young individuals responds to cost-sharing.

Obtaining similar estimates from a difference-in-differences approach and a regression discontinuity design, we find that charging children for medical care reduces their use of doctor visits by 5-10 percent. In addition to establishing the overall response to copayments among children and adolescents, our most important contribution is showing that responses vary by family income. It is three times higher for low income children than for high income children. Few studies of health care demand have been able to credibly explore differential responses by income, and in particular not for children. Interestingly, we do not find that education has an independent impact on the size of the response, suggesting that resources rather than knowledge or social class determine health care use.

Previous research for several countries has shown that there is a positive gradient of child health with respect to parental income. Our finding of differential effects of cost-sharing by income provides one explanation for the presence of the gradient. If copayments deter low-income children from receiving necessary medical care, this might have adverse consequences for health not only in the short, but also in the long run.

Besides health care, there are many other inputs in the production of child health that have a price tag, such as healthy food and leisure activities. If parents respond to prices of health care in a manner that depends on their income, it is likely that the price-sensitivity for these inputs is income-dependent as well, thus adding to the spread of the income gradient in child health. While our results suggest that copayments may affect health via their impact on medical care use, future research should look more directly at long-run effects in terms of health, and perhaps school results and economic outcomes. If such effects are found, policymakers should consider charging lower copayments for poor patients, as suggested by Rice and Thorpe (1993).

2.A Appendix

2.A.1 Algorithm for Identifying the Parent that a Child Lives With

If one of the following the conditions is fulfilled, the algorithm stops (the adoptive parents are chosen, if applicable):

1. If both parents have identical family income, identical number of children in household, identical family type and identical parish, they are assumed to live together and the child with them.

2. For a given year in a given age group, we know how many children a mother has and how many children a mother lives with. A mother is assumed to live with a given child if she lives with at least one child and I. the father does not live with any child, or II. the mother is single (i.e. all children she lives with are her own) and the number of children the mother has is equal to the number of children she lives with.

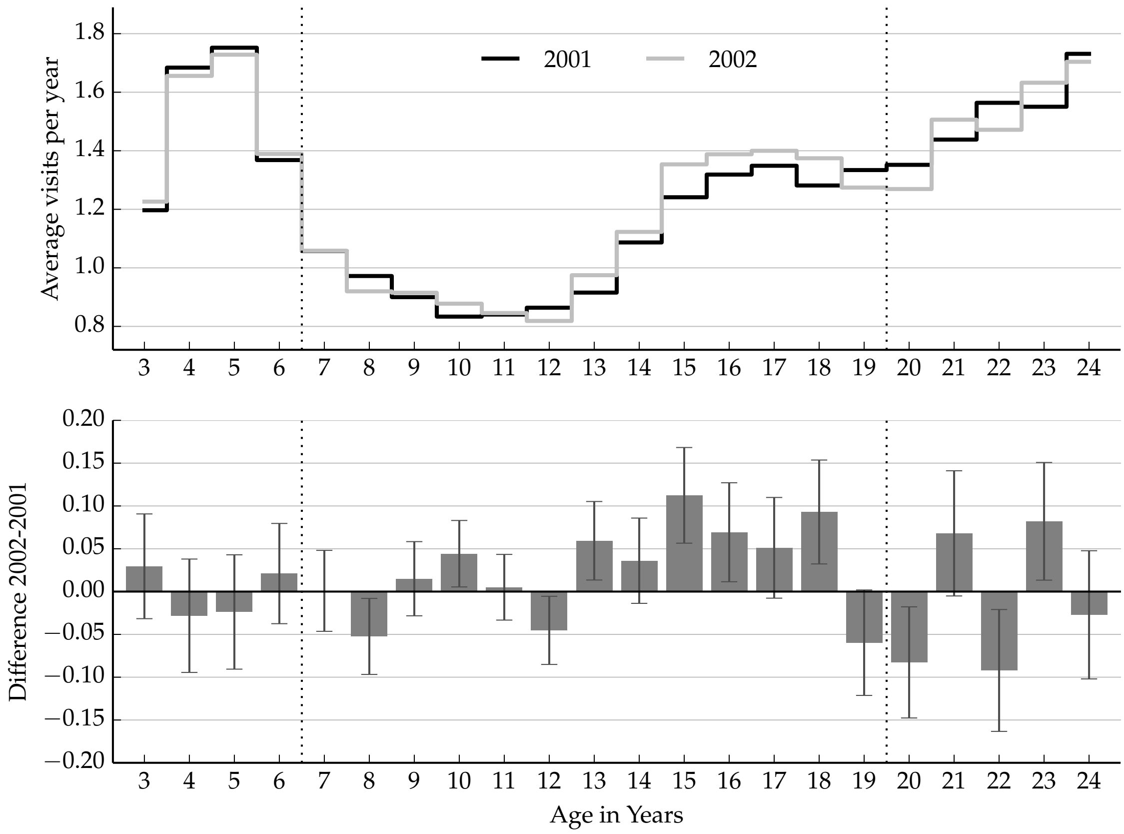
3. The rules in 2. are analogously applied to fathers.

4. If one parent is not present (dead or unknown) in the given year, it is assumed that the child lives with the respective other parent.

5. In any remaining cases, the child is assumed to live with the mother.

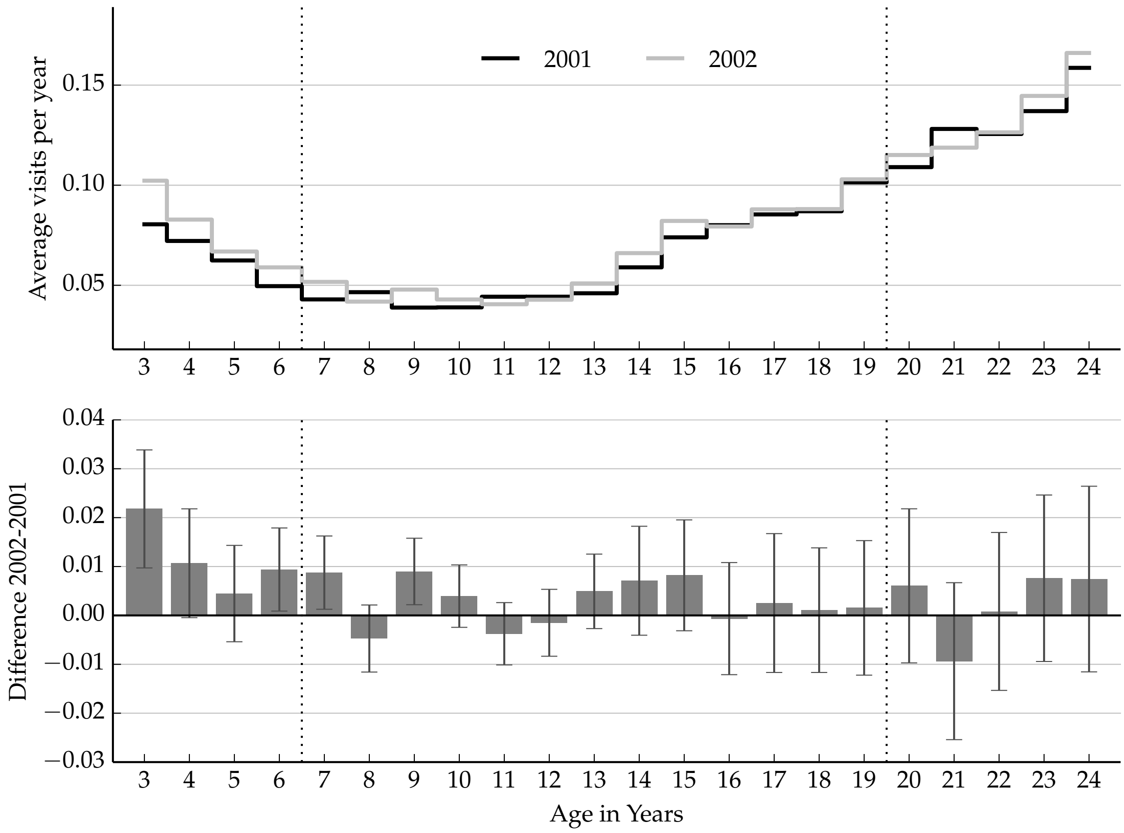
2.A.2 Figures

Figure 2.7: Average Non-doctor Visits Before vs. After the Policy Change by Age.



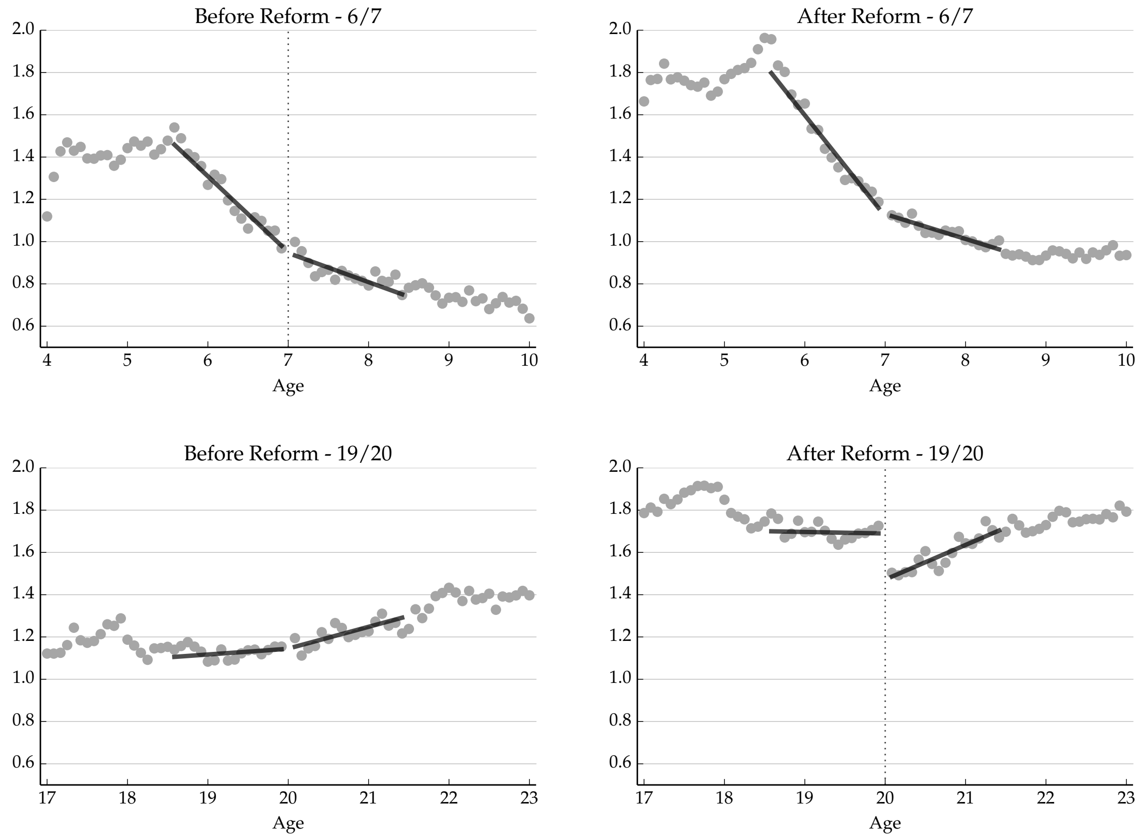
Notes: The upper graph shows the average number of non-doctor visits per year before the policy change (2001) and after the policy change (2002) by age. The lower graph shows the difference between the 2002 and the 2001 averages. Numbers are annualized from monthly data. The sample consists of everyone living in Skåne continuously between 2001 and 2002 and being between 3 to 24 years for at least one month during these years.

Figure 2.8: Average Inpatient Visits Before vs. After the Policy Change by Age.



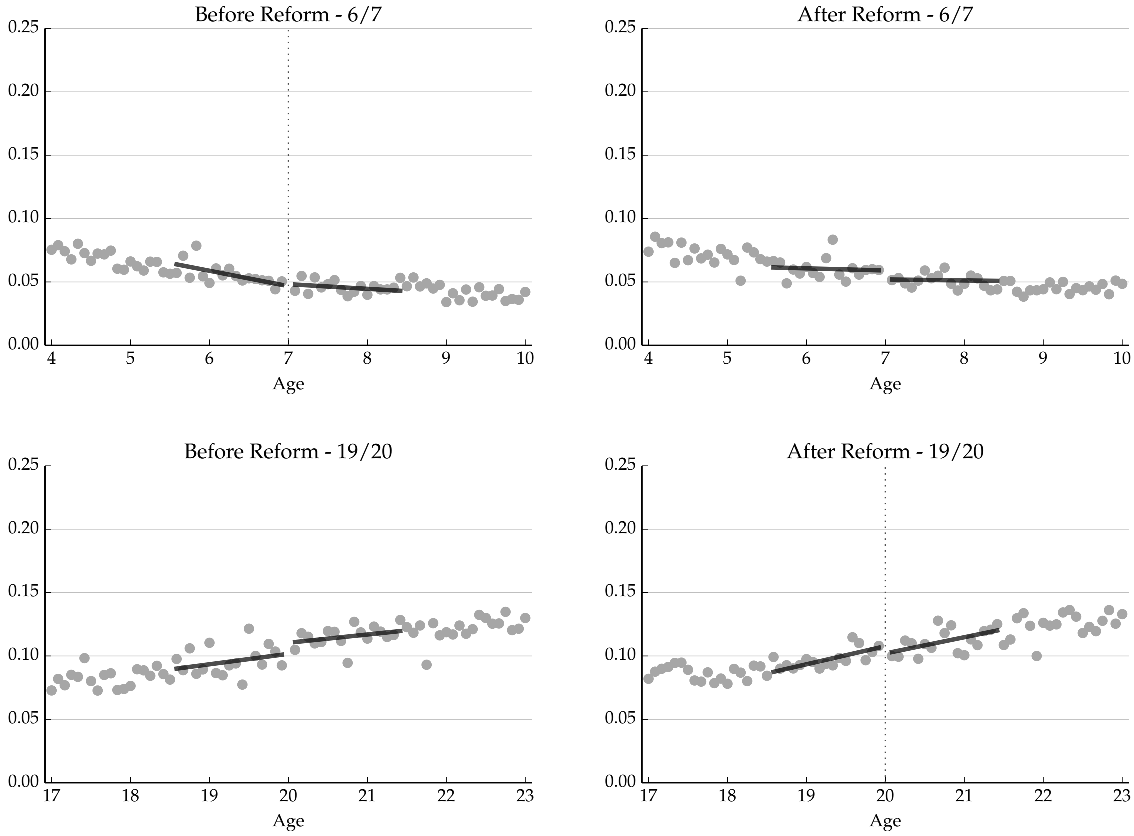
Notes: The upper graph shows the average number of inpatient visits per year before the policy change (2001) and after the policy change (2002) by age. The lower graph shows the difference between the 2002 and the 2001 averages. Numbers are annualized from monthly data. The sample consists of everyone living in Skåne continuously between 2001 and 2002 and being between 3 to 24 years for at least one month during these years.

Figure 2.9: Non-doctor Visits around RDD Thresholds



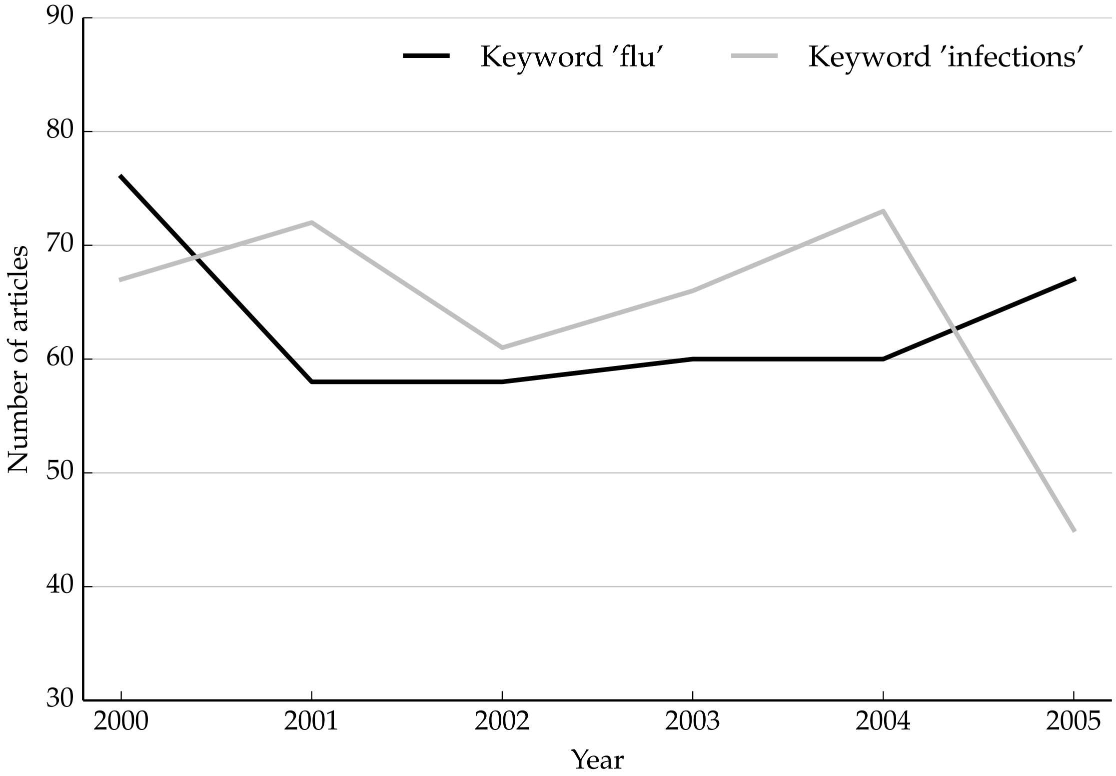
Notes: Annualized average number of non-doctor visits by age. Dots represent months. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. Vertical dotted lines indicate that copayments changed discontinuously at the threshold in the given time period. Dark lines are from fitted RDD models according to the specification described in Section 2.3.4, excluding month fixed effects.

Figure 2.10: Inpatient Visits around RDD Thresholds



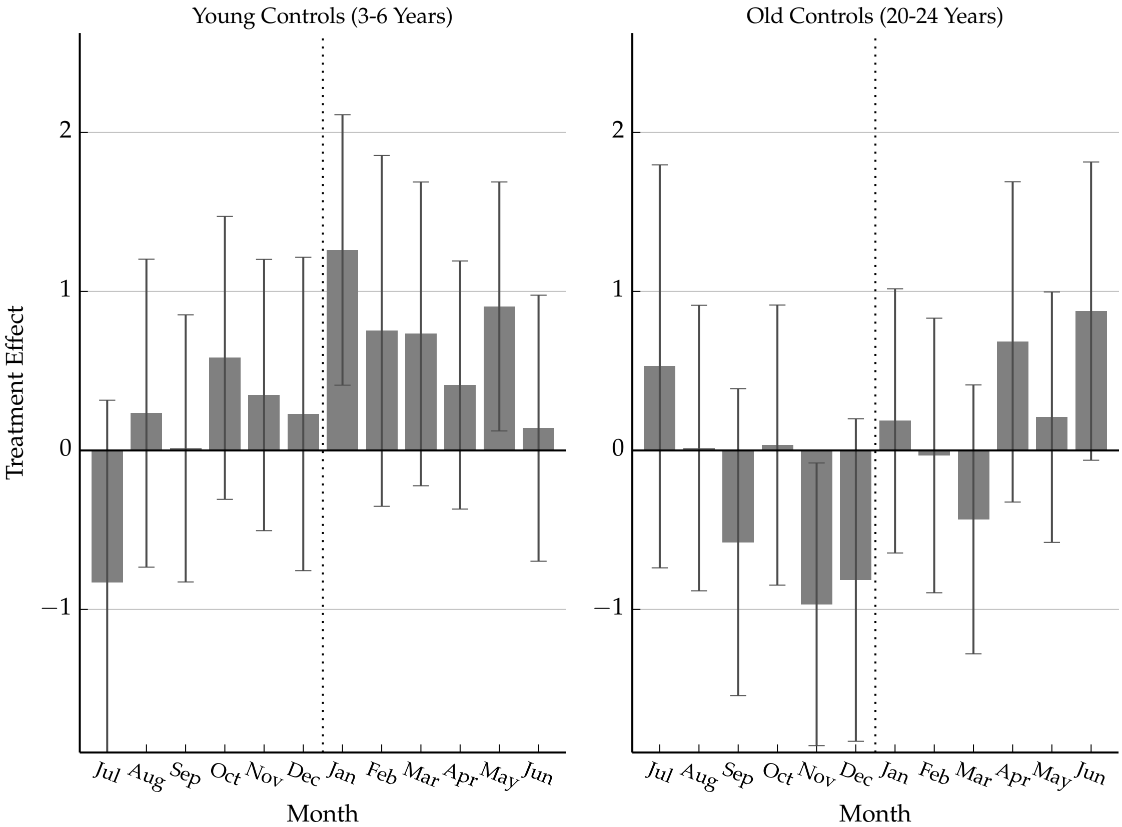
Notes: Annualized average number of inpatient visits by age. Dots represent months. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. Vertical dotted lines indicate that copayments changed discontinuously at the threshold in the given time period. Dark lines are from fitted RDD models according to the specification described in Section 2.3.4, excluding month fixed effects.

Figure 2.11: Number of Articles with Disease-Related Keywords in Local Newspaper *Sydsvenskan* by Year



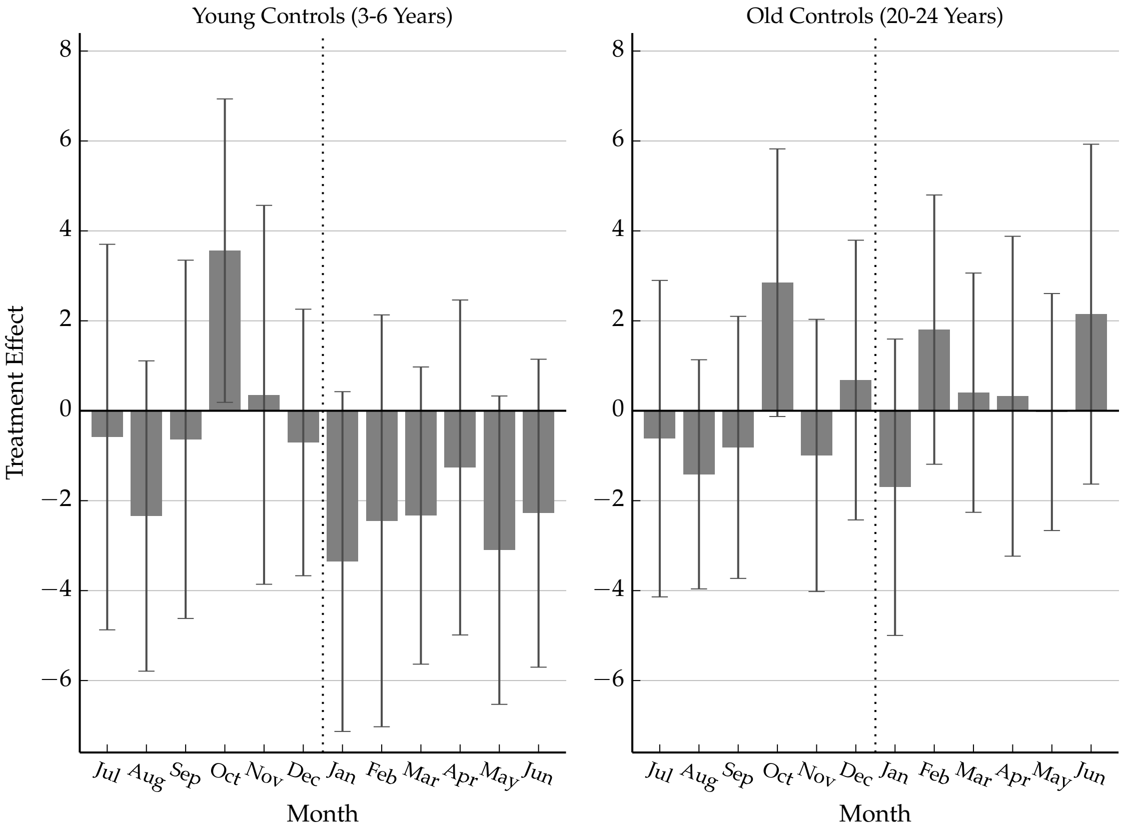
Source: Database Mediearkivet. Data is only available from 2000 onwards.

Figure 2.12: Treatment Effects by Month for Non-doctor Visits by Control Group



Notes: Effects on non-doctor visits by month between July 2001 and June 2002 by control group. We only consider six months before and six months after the policy change, since otherwise we would not be able to identify treatment-group-specific month-of-the-year fixed effects. We normalize estimates by the average estimate in July-October 2001 because all effects are relative to the months January-June 2001 and July-December 2002, but we are interested in effects relative to months just before the announcement of the reform. Numbers are annualized from estimates at the month level. The sample consists of everyone living in Skåne continuously between 2001 and 2002 and being between 3 to 24 years for at least one month during these years. Vertical lines denote 95% confidence bands.

Figure 2.13: Treatment Effects by Month for Inpatient Visits by Control Group



Notes: Effects on inpatient visits by month between July 2001 and June 2002 by control group. We only consider six months before and six months after the policy change, since otherwise we would not be able to identify treatment-group-specific month-of-the-year fixed effects. We normalize estimates by the average estimate in July-October 2001 because all effects are relative to the months January-June 2001 and July-December 2002, but we are interested in effects relative to months just before the announcement of the reform. Numbers are annualized from estimates at the month level. The sample consists of everyone living in Skåne continuously between 2001 and 2002 and being between 3 to 24 years for at least one month during these years. Vertical lines denote 95% confidence bands.

2.A.3 Tables

Table 2.9: Distribution of Actually Paid Fees (in SEK) by Caregiver, Age Group and Year

		3-6 Years		7-19 Years		20-24 Years	
	Fee	2001	2002	2001	2002	2001	2002
Doctors	200	0.1	0.0	35.4	0.2	32.8	31.9
	100	0.1	0.0	37.9	0.1	36.0	36.0
	50	0.0	0.0	1.8	0.0	2.6	2.9
	0	99.7	99.9	23.8	99.6	27.3	27.5
	Other	0.1	0.0	0.9	0.1	0.9	1.4
Non-Doctors	80	0.1	0.0	15.4	0.1	25.2	25.4
	0	99.7	99.7	83.7	99.5	73.0	72.4
	Other	0.2	0.2	0.7	0.3	0.9	1.2

Notes: Percentages shares of actually paid fees by caregiver in the treatment group (7-19 years) and in the control groups (3-6 and 20-24 years), before (2001) and after the policy change (2002). Only non-preventive in-person visits plus contacts related to prescriptions.

Table 2.10: Fees

	Doctor		Non-Doctors	
	3-6 Years	20-24 Years	3-6 Years	20-24 Years
	(1)	(2)	(3)	(4)
Abolition	-116.26**	-114.12**	-14.37**	-15.34**
	(0.59)	(0.74)	(0.36)	(0.48)
2001 Mean	117.06	117.06	15.46	15.46
N	4,896	5,184	4,896	5,184

Notes: Each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. The dependent variable is the average fee per visit, which is only defined if an individual had at least one visit in a month. In all regressions, we control for age in months, month and treatment-group specific seasonal effects. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.11: Average Visits per Year (by Type, Caregiver, Age Group and Year)

	3-6 Years		7-19 Years		20-24 Years	
	2001	2002	2001	2002	2001	2002
Total Visits						
Doctor	3.10	3.14	1.98	2.15	2.35	2.42
Non-Doctors	1.50	1.50	1.06	1.08	1.53	1.52
Inpatient	0.07	0.08	0.06	0.06	0.13	0.13
Outpatient	4.60	4.65	3.04	3.23	3.88	3.94
Acute Visits						
Doctor	1.40	1.40	0.81	0.84	0.91	0.91
Non-Doctors	0.13	0.13	0.09	0.09	0.13	0.11
Inpatient	0.04	0.05	0.04	0.04	0.10	0.10
Non-Acute Visits						
Doctor	1.70	1.74	1.17	1.31	1.44	1.50
Non-Doctors	1.37	1.37	0.97	1.00	1.40	1.41
Inpatient	0.02	0.03	0.02	0.02	0.03	0.03
New Visits						
Doctor	1.28	1.28	0.78	0.82	0.95	0.94
Non-Doctors	0.27	0.26	0.16	0.16	0.23	0.21
Revisits						
Doctor	0.99	0.98	0.72	0.75	0.79	0.81
Non-Doctors	1.20	1.18	0.75	0.74	0.86	0.86
Emergency Department						
Doctor	0.16	0.09	0.13	0.11	0.18	0.15
Non-Doctors	0.00	0.00	0.00	0.00	0.00	0.00
Psychiatric child and youth care						
Doctor	0.01	0.01	0.05	0.05	0.00	0.00
Non-Doctors	0.06	0.06	0.18	0.18	0.01	0.01
Telephone						
Doctor	0.10	0.16	0.07	0.13	0.08	0.16
Non-Doctors	0.26	0.45	0.14	0.25	0.16	0.28
Out-of-office hours						
Doctor	0.60	0.61	0.31	0.34	0.40	0.41
Non-Doctors	0.12	0.22	0.08	0.15	0.11	0.19
Referral						
Doctor	0.17	0.17	0.13	0.13	0.15	0.14
Non-Doctors	0.04	0.05	0.02	0.02	0.02	0.03
Preventive Visits						
Doctor	0.17	0.18	0.02	0.02	0.09	0.09
Non-Doctors	0.59	0.59	0.15	0.16	0.54	0.58
Observations	510,064	485,356	1,942,363	1,960,416	642,100	633,003

Notes: Average number of visits per year in the treatment group (7-19 years) and in the control groups (3-6 and 20-24 years), before (2001) and after (2002) the policy change. Numbers are annualized from monthly data. The number of observations is the number of months in the given year at the given age of everyone living in Skåne continuously between 2001 and 2002.

Table 2.12: Effect of Reform on the Number of Visits - With Restricted Extensive Margin Sample

	Doctor		Non-Doctors		Inpatient	
	3-6 Years	20-24 Years	3-6 Years	20-24 Years	3-6 Years	20-24 Years
	(1)	(2)	(3)	(4)	(5)	(6)
Abolition	6.03**	5.48**	1.00	1.08	-12.19**	1.26
	(1.05)	(0.87)	(2.54)	(2.53)	(4.20)	(4.38)
2001 Mean	1.96	1.96	1.05	1.05	0.06	0.06
N	4,368	4,656	4,368	4,656	4,204	4,521

Notes: Each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. Means are scaled up to annual figures. In all regressions, we control for age in months, month and treatment-group specific seasonal effects. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.13: Effect of the Reform by Month (Event Study)

	Doctor		Non-Doctors	
	3-6 Years	20-24 Years	3-6 Years	20-24 Years
	(1)	(2)	(3)	(4)
Month × Abolition				
2001M7	-7.11*	-4.24	-11.01*	4.32
	(3.26)	(2.52)	(4.87)	(5.39)
2001M8	-6.29*	-10.51**	-2.15	0.04
	(3.17)	(1.99)	(4.12)	(3.82)
2001M9	-14.24**	-15.47**	-3.99	-4.89
	(2.67)	(2.31)	(3.57)	(4.10)
2001M10	-5.05*	-7.93**	0.75	0.20
	(2.06)	(1.91)	(3.78)	(3.75)
2001M11	-6.55**	-7.48**	-1.20	-8.15*
	(1.75)	(1.80)	(3.63)	(3.78)
2001M12	-7.21**	-11.59**	-2.19	-6.88
	(2.53)	(2.26)	(4.19)	(4.31)
2002M1	-2.00	-2.73	6.41	1.46
	(2.44)	(2.12)	(3.62)	(3.53)
2002M2	1.01	-1.04	2.17	-0.35
	(2.86)	(2.03)	(4.69)	(3.67)
2002M3	4.26	-1.96	2.01	-3.70
	(2.47)	(1.82)	(4.06)	(3.60)
2002M4	20.10**	4.21*	-0.67	5.60

Continued on next page

	(2.14)	(2.00)	(3.32)	(4.28)
2002M5	6.94**	5.83**	3.45	1.66
	(2.31)	(2.03)	(3.33)	(3.35)
2002M6	9.02**	6.65**	-2.94	7.22
	(2.36)	(2.41)	(3.56)	(3.99)
N	4,896	5,184	4,896	5,184

Notes: Each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. Means are scaled up to annual figures. In all regressions, we control for age in months and time (month or year). In regressions with monthly outcomes, we additionally control for treatment-group-specific seasonal effects. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.14: Effect of Reform on the Number of Visits - Ignoring First 3 Months After Reform

	Doctor		Non-Doctors		Inpatient	
	3-19 years (1)	7-24 years (2)	3-19 years (3)	7-24 years (4)	3-19 years (5)	7-24 years (6)
Abolition	9.19**	8.21**	2.18	3.31	-9.41*	4.65
	(0.95)	(0.89)	(2.23)	(2.63)	(4.70)	(4.40)
2001 Mean	1.98	1.98	1.06	1.06	0.06	0.06
N	4,896	5,184	4,896	5,184	4,712	5,042

Notes: Each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. The dependent variable is the log monthly number of visits of the type given in the column heading. Means are scaled up to annual figures. In all regressions, we control for age in months, month and treatment-group specific seasonal effects. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.15: Descriptives of Socioeconomic Data (by Age Group)

	3-6 Years			7-19 Years			20-24 Years		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Female	0.49	0.50	995,373	0.49	0.50	3,902,755	0.49	0.50	1,275,103
Age	4.56	1.12	995,420	12.73	3.63	3,902,779	22.03	1.41	1,275,103
Mother's Age	34.70	4.95	941,900	41.88	6.09	3,808,996	50.16	5.09	1,271,116
Mother's Education									
Elementary Schooling	0.10	0.30	939,249	0.14	0.35	3,773,448	0.21	0.41	1,235,272
Secondary Schooling	0.60	0.49	939,249	0.59	0.49	3,773,448	0.54	0.50	1,235,272
University	0.30	0.46	939,249	0.27	0.44	3,773,448	0.25	0.43	1,235,272
Mother's Country of Birth									
Sweden	0.95	0.23	941,900	0.91	0.28	3,804,449	0.88	0.32	1,261,535
EU-15, North America	0.02	0.15	941,900	0.04	0.18	3,804,449	0.05	0.21	1,261,535
Other Europe	0.02	0.12	941,900	0.03	0.18	3,804,449	0.05	0.22	1,261,535
Other Countries	0.02	0.13	941,900	0.02	0.14	3,804,449	0.02	0.14	1,261,535
Mother's Marital Status									
Married	0.57	0.49	940,423	0.63	0.48	3,777,975	0.67	0.47	1,237,398
Single, Married Before	0.08	0.27	940,423	0.19	0.39	3,777,975	0.25	0.44	1,237,398
Single, Never Married	0.34	0.48	940,423	0.18	0.38	3,777,975	0.08	0.27	1,237,398

Notes: Descriptive information about socioeconomic variable in the treatment group (7-19 years) and in the control groups (3-6 and 20-24 years). The sample consists of everyone living in Skåne continuously between 2001 and 2002 and being between 3 to 24 years for at least one month during these years. "EU-15, North America" also includes Norway and Oceania. "Other countries" include Africa, South America, Asia, the Soviet Union and unknown countries.

Table 2.16: Effect of the Reform by Whether Female

	No		Yes	
	3-6 Years (1)	20-24 Years (2)	3-6 Years (3)	20-24 Years (4)
Number of Visits				
Abolition	7.05**	6.90**	7.07**	5.02**
	(1.11)	(1.51)	(1.32)	(0.92)
p-value	-	-	0.992	0.288
2001 Mean	1.86		2.11	
N	4,896	5,184	4,896	5,184
Any Visit				
Abolition	0.01*	0.02**	0.02**	0.01**
	(0.01)	(0.00)	(0.01)	(0.00)
p-value	-	-	0.723	0.733
2001 Mean	0.61		0.64	
N	364	388	364	388

Notes: Each panel in each column shows the treatment effect from a separate difference-in-differences regression. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. In the upper panel, the dependent variable is the log monthly number of doctor visits. Means are scaled up to annual figures. In the lower panel, the dependent variable is an indicator for whether an individual had any doctor visit in the current year. In all regressions, we control for age in months and time (month or year). In regressions with monthly outcomes, we additionally control for treatment-group-specific seasonal effects. P-values are from t-tests of differences in abolition effects in comparison with "No". * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.17: Effect of the Reform by Whether Female - RDD

	Before Reform - 6/7	
	No	Yes
Free care	2.67	8.89**
	(2.03)	(2.10)
p-value	-	0.03
Mean Above	2.39	2.14
N	1,020	1,020
	After Reform - 19/20	
	No	Yes
Free care	7.66**	10.36**
	(1.68)	(1.49)
p-value	-	0.23
Mean Above	1.71	2.72
N	2,040	2,040

Notes: Each panel in each column shows the treatment effect from a separate RDD estimation. Individuals are grouped by age (in months) and regressions are weighted by group size. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. 'Free care' is equal to one if an individual is below the threshold. Standard errors are clustered at the age level and shown in parentheses. The dependent variable is the log monthly number of doctor visits. Means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects) and scaled up to annual figures. Using a bandwidth of 18 months, we estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.18: Effect of the Reform by Mother's Education - RDD

	After Reform - 19/20		
	Elementary Schooling	Secondary Schooling	University
Free care	14.00**	6.86**	11.01**
	(1.76)	(2.03)	(3.01)
p-value	-	0.01	0.39
Mean Above	2.19	2.06	1.89
N	1,631	1,632	1,632

Notes: Each panel in each column shows the treatment effect from a separate RDD estimation. Individuals are grouped by age (in months) and regressions are weighted by group size. 'After Reform' includes the years 2002-2006. '19/20' indicates that the threshold is the month in which individuals become 20 years old. Since education is only available starting from 2001, we exclude the 6/7 threshold here. 'Free care' is equal to one if an individual is below the threshold. Standard errors are clustered at the age level and shown in parentheses. The dependent variable is the log monthly number of doctor visits. Means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects) and scaled up to annual figures. Using a bandwidth of 18 months, we estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.19: Effect of the Reform by Family Income

	Before Reform - 6/7			
	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Free care	6.46*	8.33**	5.58**	3.64
	(2.59)	(2.42)	(1.53)	(3.02)
p-value	-	0.60	0.77	0.48
Mean Above	2.37	2.33	2.29	2.23
N	1,020	1,020	1,020	1,020
	After Reform - 19/20			
	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Free care	14.93**	10.03**	10.85*	3.21
	(3.89)	(2.45)	(4.35)	(3.13)
p-value	-	0.29	0.48	0.02
Mean Above	1.98	2.04	2.07	2.02
N	1,632	1,632	1,632	1,632

Notes: Each panel in each column shows the treatment effect from a separate RDD estimation. Individuals are grouped by age (in months) and regressions are weighted by group size. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. 'Free care' is equal to one if an individual is below the threshold. Standard errors are clustered at the age level and shown in parentheses. The dependent variable is the log monthly number of doctor visits. Means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects) and scaled up to annual figures. Using a bandwidth of 18 months, we estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.20: Effect of the Reform by Family Income - RDD - 6/7

	In Partnership		Single	
Panel A	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Free care	8.57	3.61	5.89*	7.44
	(4.77)	(2.91)	(2.44)	(25.78)
Mean Above	2.25	2.23	2.44	1.51
N	1,020	1,020	1,020	154
	Stays At Home		Works	
Panel B	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Free care	16.49*	-6.44	3.58	3.82
	(8.11)	(9.99)	(3.55)	(2.83)
Mean Above	2.15	2.25	2.42	2.22
N	1,017	807	1,020	1,020
	Sweden		From Other Countries	
Panel C	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Free care	6.44*	3.28	5.87	-28.27
	(2.73)	(3.23)	(12.15)	(21.18)
Mean Above	2.41	2.24	1.74	1.70
N	1,020	1,020	595	233

Notes: Each panel in each column shows the treatment effect from a separate RDD estimation. Individuals are grouped by age (in months) and regressions are weighted by group size. 'Free care' is equal to one if an individual is below the threshold. Standard errors are clustered at the age level and shown in parentheses. The dependent variable is the log monthly number of doctor visits. Means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects) and scaled up to annual figures. Using a bandwidth of 18 months, we estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.21: Effect of the Reform by Family Income - RDD - 19/20

	In Partnership		Single	
Panel A	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Free care	15.90**	2.79	14.73**	25.44*
	(4.12)	(3.25)	(3.64)	(10.23)
Mean Above	1.86	2.02	2.03	1.82
N	1,584	1,632	1,631	555
	Stays At Home		Works	
Panel B	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Free care	11.03	9.34	15.45**	3.65
	(5.92)	(9.20)	(4.41)	(3.22)
Mean Above	2.42	2.18	1.86	2.02
N	1,585	857	1,632	1,632
	Sweden		From Other Countries	
Panel C	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Free care	14.63**	3.28	-9.06	-16.35
	(3.47)	(3.10)	(9.47)	(14.40)
Mean Above	2.03	2.02	1.27	2.02
N	1,632	1,632	818	296
	Has No College		Has College	
Panel D	1st Quartile	4th Quartile	1st Quartile	4th Quartile
Free care	14.64**	-2.48	17.60**	10.31*
	(3.67)	(4.84)	(6.48)	(4.70)
Mean Above	2.03	2.11	1.82	1.90
N	1,632	1,632	1,540	1,630

Notes: Each panel in each column shows the treatment effect from a separate RDD estimation. Individuals are grouped by age (in months) and regressions are weighted by group size. 'Free care' is equal to one if an individual is below the threshold. Standard errors are clustered at the age level and shown in parentheses. The dependent variable is the log monthly number of doctor visits. Means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects) and scaled up to annual figures. Using a bandwidth of 18 months, we estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.22: Doctor Visits by Type

	Difference-in-differences		Regression discontinuity	
	3-19 Years	7-24 Years	Before Reform - 6/7	After Reform - 19/20
	(1)	(2)	(3)	(4)
Panel A. Non-Acute Visits				
	8.98**	6.74**	5.02*	10.20**
	(1.13)	(0.96)	(2.55)	(1.58)
Mean	1.17	1.17	1.33	1.35
Panel B. Acute Visits				
	4.29**	4.32**	6.15**	7.85**
	(1.01)	(0.97)	(1.50)	(1.48)
Mean	0.81	0.81	0.94	0.85
Panel C. New Visits				
	5.03**	5.75**	4.12**	2.02
	(0.96)	(1.02)	(1.58)	(1.31)
Mean	0.78	0.78	0.88	0.87
Panel D. Revisits				
	6.11**	3.36*	2.47	11.60**
	(1.49)	(1.60)	(3.29)	(2.70)
Mean	0.72	0.72	0.85	0.75
Panel E. Out-of-office hours				
	9.29**	8.23**	6.50**	8.77**
	(1.58)	(1.48)	(1.64)	(1.51)
Mean	0.31	0.31	0.37	0.39
Panel F. Emergency Department				
	37.60**	1.34	8.19	4.05
	(3.54)	(2.28)	(5.22)	(2.70)
Mean	0.13	0.13	0.12	0.17

Notes: Each panel shows the treatment effect from a separate difference-in-differences regression in columns 1 and 2 and from a separate RDD estimation in columns 3 and 4. Individuals are grouped by age (in months) and regressions are weighted by group size. Standard errors are clustered at the birth year × birth quarter level and shown in parentheses. The dependent variable is the log monthly number of doctor visits of the given type. In all columns 1 and 2, we control for age in months, month and treatment-group specific seasonal effects. In columns 3 and 4, we use a bandwidth of 18 months to estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. Means are scaled up to annual figures. In columns 1 and 2, means from 2001 are reported. In columns 3 and 4, means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects). 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. 'Free care' is equal to one if an individual is below the threshold. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 2.23: Effects by Health Status

	Before Reform - 6/7		After Reform - 19/20	
	Non-Sickly	Sickly	Non-Sickly	Sickly
Diabetes	5.6***	-6.2	9.0***	12.9
	(1.6)	(8.9)	(1.8)	(7.9)
Mean Above	2.26	5.79	2.17	4.75
p-value	-	-	-	0.32
Cystic Fibrosis	5.5***	-2.3	9.2***	0.9
	(1.6)	(9.5)	(1.8)	(10.2)
Mean Above	2.25	5.24	2.17	4.85
p-value	-	-	-	-
Mental Disorders	5.3***	10.6	8.9***	18.0*
	(1.5)	(7.6)	(1.9)	(10.0)
Mean Above	2.24	3.70	2.17	4.22
p-value	-	0.25	-	0.18
Epilepsy	5.4***	7.9	9.0***	12.9*
	(1.5)	(6.7)	(2.0)	(7.5)
Mean Above	2.24	4.64	2.15	4.57
p-value	-	0.36	-	0.31
Cerebral Palsy	5.1***	6.2	9.1***	5.1
	(1.6)	(7.9)	(1.9)	(13.8)
Mean Above	2.26	6.78	2.17	6.37
p-value	-	0.45	-	-
Allergic Rhinitis	5.3***	5.5	9.8***	2.7
	(1.7)	(4.4)	(1.9)	(2.9)
Mean Above	2.18	4.11	2.10	3.85
p-value	-	0.48	-	-
Asthma	6.0***	-1.6	9.7***	0.4
	(1.9)	(2.7)	(1.7)	(5.2)
Mean Above	2.15	4.98	2.11	4.30
p-value	-	-	-	-
Juvenile Arthritis	5.7***	-2.2	9.2***	3.6
	(1.5)	(12.0)	(1.9)	(13.5)
Mean Above	2.25	6.60	2.17	7.02
p-value	-	-	-	-

Continued on next page

Any Chronic Condition	5.8***	4.0*	10.2***	4.8*
	(1.8)	(2.1)	(1.9)	(2.8)
Mean Above	2.02	4.27	1.99	4.16
p-value	-	-	-	-
Use in 2002/2003	6.1***	6.2***	9.5***	9.0***
	(1.9)	(1.8)	(2.2)	(2.6)
Mean Above	1.74	4.64	1.69	4.57
p-value	-	0.48	-	-
- Low Income	5.7**	7.0**	9.6	23.8***
	(2.8)	(3.5)	(6.7)	(3.8)
Mean Above	1.77	4.89	1.43	4.46
p-value	-	0.38	-	0.03
- High Income	6.2*	3.0	-2.1	10.0*
	(3.3)	(5.2)	(3.6)	(5.5)
Mean Above	1.73	4.65	1.58	4.56
p-value	-	-	-	0.03

Notes: Each panel in each column shows the treatment effect from a separate RDD estimation. Individuals are grouped by age (in months) and regressions are weighted by group size. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. The reported coefficient belongs to an indicator for being below the threshold. Standard errors are clustered at the age level and shown in parentheses. The dependent variable is the log monthly number of doctor visits. Means are estimated just above the threshold (the coefficient of the constant from a regression without month fixed effects) and scaled up to annual figures. Using a bandwidth of 18 months, we estimate local linear regressions with triangular weighting that allow for varying slopes on either side of the threshold. Individuals are defined as suffering from a chronic condition if they were diagnosed with it at least once during 2002 and 2003. 'Sickly' indicates having the condtion. Use is measured as the average number of monthly doctor visits in 2002 and 2003 and ranked among individuals of the same birth year. 'Sickly' indicates being in the top 20% of the doctor visits distribution. Years 2002 and 2003 are not used in the regressions. P-values are from a one-sided t-test that the effect on the 'Sickly' is smaller than or equal to the that on the 'Non-Sickly'. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Chapter 3

The Role of Low Birth Weight Thresholds in the Absence of Regulatory Standards: Evidence from Sweden[1]

3.1 Introduction

Across the world, infant mortality has decreased to historically low values over recent decades. In 2013, mortality had fallen to only two deaths per 1,000 live births in several high-income countries including Sweden, Norway and Japan among others. However, chances of survival are still relatively low in most developing countries and in general for babies that are born premature or with low birth weight. Therefore, measures effective at reducing infant mortality remain of high interest to public policy. In this context, the role of neonatal care - medical interventions right after birth - is at the center of attention. This is because spending on neonatal care is large and progress in medical technology has been rapid. In addition to reducing mortality, neonatal care might also improve cognitive and physical functioning of those babies that survive.

Juxtaposing the long-run downward trend in infant mortality with the continuous increase in medical spending on babies strongly suggests that neonatal care has had a large impact on the likelihood of newborn survival (Cutler and Meara 2000). However, such an analysis reveals little about a marginal extension of neonatal care at a given point in time, which is of particular policy interest (Almond et al. 2010). Estimating marginal returns

1. This chapter is joint work with Hans-Martin von Gaudecker. It is based on, but substantially extends, my master thesis submitted at the University of Mannheim in 2011, which was at the same time a dissertation proposal (Paul 2011). We gratefully acknowledge financial support from the European Research Council through Starting Grant No. 313719. We also gratefully acknowledge financial support from the Humboldt Foundation through the Alexander von Humboldt Professur Prize for Gerard van den Berg.

to neonatal care is complicated by the endogeneity of treatment intensity to health status. Newborns that receive more care are most often those that have a higher propensity to die, so that estimated effects of neonatal care are biased downwards.

Recently, several studies identified and exploited changes in treatment intensity that are discontinuous - and therefore exogenous - with respect to health status. Almond and Doyle (2011), for instance, use discontinuous variation in the length of hospital stays for babies born shortly before and shortly after midnight. They do not find that additional days in hospital - which facilitate additional interventions - would be associated with improvements in infant survival or other major health outcomes.

Another approach is based on the internationally well-known WHO classifications of low birth weight. According to the WHO, newborns weighing less than 2,500 grams are considered to be low birth weight (LBW). Sub-categories are 1,500 grams (very low birth weight or VLBW) and 1,000 grams (extremely low birth weight or ELBW). In the medical literature, particularly the VLBW threshold of 1,500 grams has been used as a basis for recommendations on neonatal care. The threshold has been referred to in regulatory standards implemented at the hospital or higher organizational levels, but might also be used by decision-making physicians as a simple rule of thumb. Since newborns slightly below and slightly above 1,500 grams vary only little with respect to their underlying mortality risk, this threshold generates exogenous variation in the intensity of neonatal care.

Exploiting the VLBW threshold, Almond et al. (2010, 2011) estimate a regression discontinuity design and find a sizable reduction in infant mortality for newborns below 1,500 grams by about 20 percent. The effects are concentrated in low-income hospitals. In combination with information on associated increases in medical care spending, they conclude that neonatal care is cost-effective at the VLBW margin. Bharadwaj et al. (2013) and Breining et al. (2015) confirm that VLBW designation reduces infant mortality using data from Chile/Norway and Denmark, respectively. In addition, these papers document positive effects on long-run schooling outcomes of surviving infants. Breining et al. (2015) also report that VLBW designation has positive spill-over effects on the schooling outcomes of siblings.

Bharadwaj et al. (2013) and Breining et al. (2015) show that the VLBW threshold only plays a role for babies born after 32 weeks of gestation, since babies arriving earlier receive care regardless of weight. This is in accordance with another WHO classification that defines babies with fewer than 32 completed weeks of pregnancy as "very preterm". Preterm birth more generally refers to infants born before 37 weeks of gestation. Babies with less than 28 weeks are considered extremely preterm. Daysal et al. (2013) use the 37-weeks-threshold to identify the impact of obstetrician supervision of births in the Netherlands. However, since the timing of birth is more easily manipulable than newborn weight, the exogeneity assumption is more likely to fail around thresholds of weeks than around thresholds of weight.

In this paper, we add evidence on the role of the VLBW threshold in a different country:

Sweden. In contrast to all previous studies of other countries, we find that the VBLW threshold does not trigger a discontinuous decrease in infant mortality for infants below 1,500 grams in Sweden. This result is robust to a number of checks commonly performed in regressions discontinuity designs. We also show that neither treatment intensity - as measured by hospital length of stay - nor school grades later in life change around the VLBW cutoff. We conclude that the VLBW cutoff was irrelevant in governing treatment decision in neonatal care in Sweden. As a consequence, we explore whether birth weight values other than 1,500 grams or values of other variables such as gestational age and small for gestational age were used instead of VLBW. There is no evidence for the primacy of any treatment threshold different from VLBW.

We provide a tentative interpretation of our findings. The combination of these findings with our reading of the medical literature leads us to conjecture that regulatory standards in the form of threshold values were absent in Sweden. The main rationale underlying the use of threshold values is to control cost and prevent overuse. We argue that certain cost of neonatal care is relatively low in Sweden. Moreover, Sweden has historically had large expertise in treating premature babies reflected in low infant mortality rates. High expertise of well-trained medical staff may explain why the use of VLBW as a regulatory standard or as a rule of thumb was obsolete in Sweden.

The paper is structured as follows. Section 3.2 describes the dataset and the definition of the sample. In Section 3.3, we explain the details of our econometric approach. Section 3.4 presents baseline results on the effect of VLBW designation on infant mortality, followed by a range of sensitivity checks. It also explores threshold values other than VLBW. Section 3.5 discusses and interprets our results. Section 3.6 concludes.

3.2 Data and Sample

Our main data source is the Swedish Medical Birth Register, which is maintained by Statistics Sweden and includes the universe of births in Sweden since 1973. For each birth, it provides information on the weight of the newborn, the gestational age and the hospital where the birth took place. It also documents if the newborn died within 28 days after birth (neonatal mortality). For infant mortality, i.e. deaths within 1 year of birth, we add data from the Cause of Death Register, which informs about deaths up until 2005, so that we observe infant mortality up until 2004. The linkage is enabled by the presence of a unique personal identifier that is used across all registers. The Medical Birth Register also contains information on the mother's county of residence and on whether she is cohabiting with the child's father. Where county of residence is missing, we add it from the mother's demographic and socioeconomic data records - the so-called LISA register. From this register, we also obtain maternal earnings and income. In addition, we identify the Inpatient Register entry associated with the birth (and recorded with the mother's identifier) to obtain the duration of the hospital spell.

Finally, in order to study long-run educational outcomes, we merge school grades from the 9th grade, which is the last year of compulsory school in Sweden.

Our main sample restricts attention to the years 1980 to 1993, but we also report results for the years up until 2004. The focus on the years 1980 to 1993 is for several reasons: First, infant mortality was relatively high in these years (24 percent for VLBW babies), leaving much scope for reductions through neonatal care. Second, this choice facilitates the comparison of our results with those by Bharadwaj et al. (2013), who study a similar and geographically adjacent country (Norway) over the same time period. Third, Sweden introduced diagnosis-related groups (DRG) for hospital care in 1995 (Serdén and Heurgren 2011). Sweden uses the NordDRG system, which is shared by several Nordic countries and was augmented with DRG codes for neonatal care based on birth weight in 2001. Specifically, the reimbursement rules change when birth weight falls below each of the LBW, VLBW and ELBW thresholds (Socialstyrelsen 2001). As a result, financial incentives to manipulate birth weight emerge, potentially invalidating the regression discontinuity design (Jürges and Köberlein 2013). Finally, school grades are not yet available for more recent birth cohorts.

There occurred 1.47 million births between 1980 and 1993, of which 63,444 (= 4.3 percent) were low (< 2,500 grams) and 9,704 (= 0.66 percent) were very low (< 1,500 grams) birth weight.[2] In the baseline specification of our econometric analysis, we choose a bandwidth of 200 grams below and above the VLBW threshold. The resulting window of births between 1,300 and 1,700 grams of birth weight contains 7,214 observations. We estimate regressions for different values of gestational age, which forces us to omit birth records where this information is missing, so that we are left with 7,164 observations.[3]

When outcomes other than infant mortality are studied, the sample size is further reduced. This is because the mother can be matched to the newborn in only 96 percent of the cases, which implies fewer observations when looking at e.g. hospital spells and maternal income.[4] Similarly, school grades are only available for birth cohorts 1982-1991 (and for a few individuals born in 1981 and 1992) and even during these years only for 78 percent of the cases because some children died early in life or simply missed the exams.

Table 3.1 provides descriptive statistics of our main sample compared with all births over the time period 1980-1993. We only include observations with non-missing values of both birth weight and gestational age. It is reassuring that both samples are very similar in

2. We exclude stillbirths, i.e. babies that die before or during delivery, because we are interested in how the VBLW threshold affects newborn health through medical interventions after birth.

3. Note that in the regressions we use triangular weighting which attaches zero weight to observations at the borders of the 400-gram-birth-weight window. In effect, weights 1,300 grams and 1,700 grams are ignored, so that 6,789 observations are used in the estimation.

4. The hospital spell pertaining to the birth is identified using those spells with ICD-9 codes 650-669 or ICD-10 codes O80-O84 that were recorded with the mother in the year of birth (= year of admission) with known duration of spell. In the case of duplicates (meaning that a given mother had several babies within the same year), a random spell is picked. This approach misses mothers who were admitted to hospital in the year prior to the baby's birth, which should however be a relatively rare event. The fraction of births whose spell can be identified using this strategy is roughly two thirds given the mother is known.

Table 3.1: Summary Statistics

	Births with Weight 1,300-1,700 Grams			All Births		
	Mean	SD	N	Mean	SD	N
Birth Year	1,987.06	3.95	7,164	1,986.92	4.03	1,462,368
Weight is multiple of 100	0.12	0.33	7,164	0.12	0.33	1,462,368
Female	0.49	0.50	7,164	0.49	0.50	1,462,364
Multiple Birth	0.21	0.41	7,164	0.02	0.14	1,462,368
Weight	1,516.41	117.33	7,164	3,496.23	574.81	1,462,368
Gestational Age (in days)	226.73	18.64	7,164	278.31	13.34	1,462,368
Infant Mortality	0.09	0.28	7,164	0.01	0.07	1,462,368
Neonatal Mortality	0.07	0.25	7,164	0.00	0.06	1,462,368
Apgar (5 min)	8.55	1.86	6,645	9.62	0.85	1,406,088
Small for Gestational Age	0.48	0.50	5,658	0.03	0.17	1,430,936
Mother Lives With Child's Father	0.92	0.27	5,501	0.95	0.23	1,154,128
Length of Hospital Stay (in days)	11.36	8.07	4,623	5.47	3.49	1,094,664
Length of Hospital Stay (in days, constructed)	6.75	27.57	6,991	3.48	154.20	1,448,747
Hospital with NICU	0.46	0.50	7,137	0.33	0.47	1,447,747
Overall Grade	194.69	65.41	4,084	204.58	63.61	930,335
Mother's Age	28.34	5.60	6,867	28.02	5.08	1,405,517
Birth Order	1.89	1.06	6,867	1.92	0.97	1,405,518
Number of Children Born Later	0.91	1.01	6,867	0.86	0.97	1,405,518
Log Mother Income in Year of Test	12.76	0.51	3,806	12.83	0.49	920,623
Mother Employed in Year of Test	0.87	0.34	3,809	0.91	0.29	921,340

terms socioeconomic and demographic characteristics. An exception are multiple births (e.g. twins), which constitute 21 percent of all newborns in the low weight sample, but only 2 percent in the full sample. As expected, health outcomes tend to be worse for light babies along all dimensions. Their overall grade is also lower on average.

3.3 Method

We estimate the regression discontinuity design with a local linear regression that allows for flexible slopes below and above the threshold:

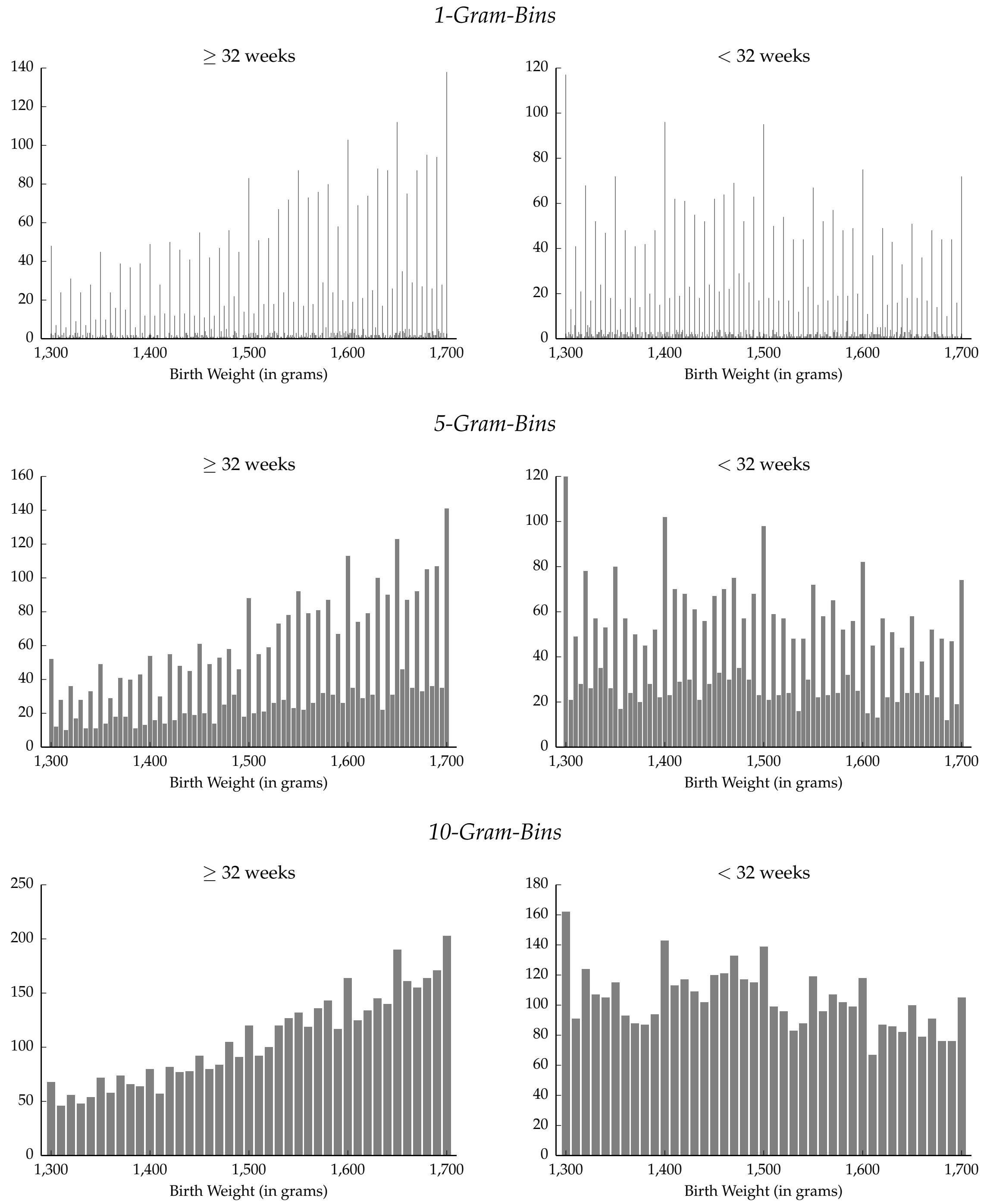
$$\begin{aligned} Y &= \alpha + \beta_1(Weight - 1{,}500) + \\ &\quad \gamma(Weight < 1{,}500) + \beta_2(Weight - 1{,}500)(Weight < 1{,}500) + X'\theta + \varepsilon \end{aligned} \tag{3.1}$$

where Y is a measure of mortality or some other outcome variable. In the case of infant mortality, it is a dummy variable that takes on the value 1 if the child died within 1 year of birth and 0 otherwise. Since we take deviations of *Weight* from 1,500 grams, the coefficient γ directly estimates the treatment effect of additional neonatal care due to falling just below the cutoff. X is a vector of controls including dummies for sex, birth year, multiplicity of birth as well as county fixed effects. In principle, identification in the regression discontinuity design does not rely on the inclusion of controls, but they might increase the precision of the estimation. Here we only use controls that are present for all observations, but demonstrate the robustness of our results to the inclusion of fewer or more controls in Section 3.4.1.3.

We also include a dummy for babies whose birth weight is exactly 1,500 grams. This addresses the fact that birth weight is subject to substantial rounding to multiples of 5, 10, 50 and 100 grams, as visible in the form of heaping in the frequency distribution of birth weight (see Figure 3.1). The propensity for weight to be rounded is likely correlated with other factors that might affect mortality, such as hospital quality. In Table 3.12 in the Appendix, we present evidence that heaping is correlated with birth year and mother's age, both of which in turn correlate with mortality. Rounding to multiples of 100 grams is particularly frequent and one such multiple (1,500 grams) is right above the cutoff. If babies weighing 1,500 grams are fundamentally different from those close by because of rounding, then the regression discontinuity estimates will become invalid (Barreca et al. 2015). Besides addressing this problem with a dummy for 1,500 grams, in our sensitivity analysis we also run so-called donut regressions where we drop observations at 1,500 grams and include a dummy for all multiples of 100 grams.

Since estimation occurs at the boundary, the preferred choice with regards to the asymptotic variance is the triangular kernel that assigns linearly decreasing weight to observations further away from the threshold. Observations at the cutoff receive full weight and those at the borders of the birth weight window receive zero weight.

Figure 3.1: Histograms of Birth Weight by Gestational Age



Notes: Histograms showing the absolute frequency of birth weight between 1,300 and 1,700 grams for different bin sizes. Only years 1980-1993.

We choose a bandwidth of 200 grams below and above the threshold, but also check the sensitivity of our estimates to alternative bandwidth values.[5] We also experiment with fitting infant mortality as a quadratic rather than a linear polynomial. Birth weight is a discrete rather than continuous variable and therefore we cluster standard errors at the age level (Lee and Card 2008).

3.4 Results

3.4.1 Newborn Mortality

3.4.1.1 Baseline

We start our empirical analysis with plotting infant mortality against birth weight around the VLBW cutoff in 1980-1993 in the upper panel of Figure 3.2. Since babies with gestational age smaller than 32 weeks are considered at-risk irrespective of birth weight and therefore might receive additional treatment anyway, we create separate graphs for babies born both before and after 32 weeks of gestation. In both graphs, infant mortality is decreasing with weight but exhibits no visible discontinuous jump at 1,500 grams. We also add dark lines fitted according to the specification in Section 3.3 to the graphs. If anything, there is an increase in infant mortality for babies with weight just the below 1,500 grams. We will report precise estimates and standard errors shortly. The graphs look very similar for the years 1994-2004, except that the level of infant mortality is generally lower in this period.

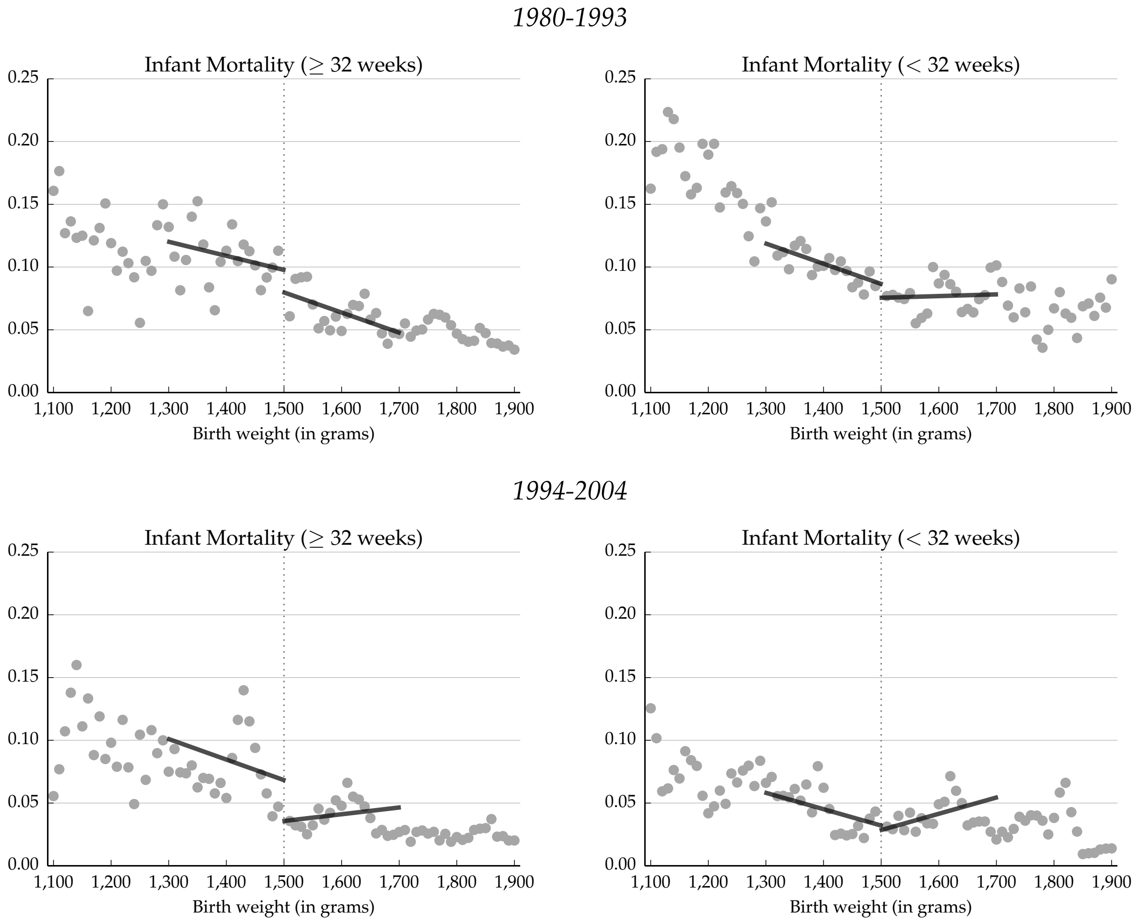
In Table 3.2, we present corresponding regression results. VLBW designation increases infant mortality by 2.2 percentage points in the 1980-1993 period for babies born after 32 weeks of gestation. The mean right above the cutoff - corresponding to α in equation 3.1 - is 0.08 and implies a percentage increase in mortality by 28 percent. This increase is at odds with the conjecture that falling below the VLBW cutoff triggers interventions that are beneficial for newborn health. However, the estimate is not significantly different from zero. The estimates are similar for babies born before 32 weeks of gestation and in the overall sample. Repeating the analysis for the period 1994-2004 and for neonatal mortality as an alternative outcome measure yields qualitatively comparable results.

3.4.1.2 Alternative Bandwidths and Functional Form

In the regression discontinuity design, the bandwidth choice must trade-off bias due to misspecification of functional form with estimation precision. A larger bandwidth increases precision, while a smaller bandwidth reduces bias. Bias is also reduced by allowing for functions of higher polynomial order, but estimation of additional coefficients once again comes at the cost of decreased precision. Figure 3.3 shows how our estimate changes

5. The choice of 200 grams is also made by Bharadwaj et al. (2013), which eases comparison of our results with theirs.

Figure 3.2: Infant Mortality by Time Period and Gestational Age



Notes: Each point gives the average infant mortality in 30 grams bins of birth weight centered at 10 gram intervals. The 1,500 gram point is dropped. Dark lines are from fitted RDD models according to the specification described in Section 3.3, but excluding the vector of controls to ensure consistency with the bin points.

when we vary the bandwidth between 50 and 300 grams and when we use a quadratic rather than a linear polynomial (see also Table 3.13 in the Appendix for precise numbers). For the years 1980-1993, the size of the estimate is very similar and insignificant across all bandwidths and for both polynomial orders. An exception are very small bandwidths, where we have significantly positive effects. These might however be due to overfitting. The effects become almost significant in the linear specification at large bandwidths. Misspecification of functional form might drive these results, as significance vanishes when using a quadratic polynomial. Overall, our estimates appear independent of bandwidth and functional form.

Table 3.2: Mortality around 1,500 Grams of Birth Weight by Time Period and Gestational Age

	1980-1993			1994-2004		
Gestational Age	All	$\geq$ 32 weeks	$<$ 32 weeks	All	$\geq$ 32 weeks	$<$ 32 weeks
Infant Mortality						
Birth weight < 1,500	0.0161	0.0218	0.0114	0.0145	0.0315	0.0058
	(0.0115)	(0.0174)	(0.0185)	(0.0118)	(0.0211)	(0.0135)
Mean	0.078	0.080	0.076	0.032	0.036	0.028
N	6,789	3,474	3,315	4,837	2,239	2,598
Neonatal Mortality						
Birth weight < 1,500	0.0154	0.0123	0.0163	0.0088	0.0130	0.0066
	(0.0101)	(0.0156)	(0.0165)	(0.0104)	(0.0164)	(0.0132)
Mean	0.062	0.062	0.063	0.027	0.029	0.025
N	6,789	3,474	3,315	4,837	2,239	2,598

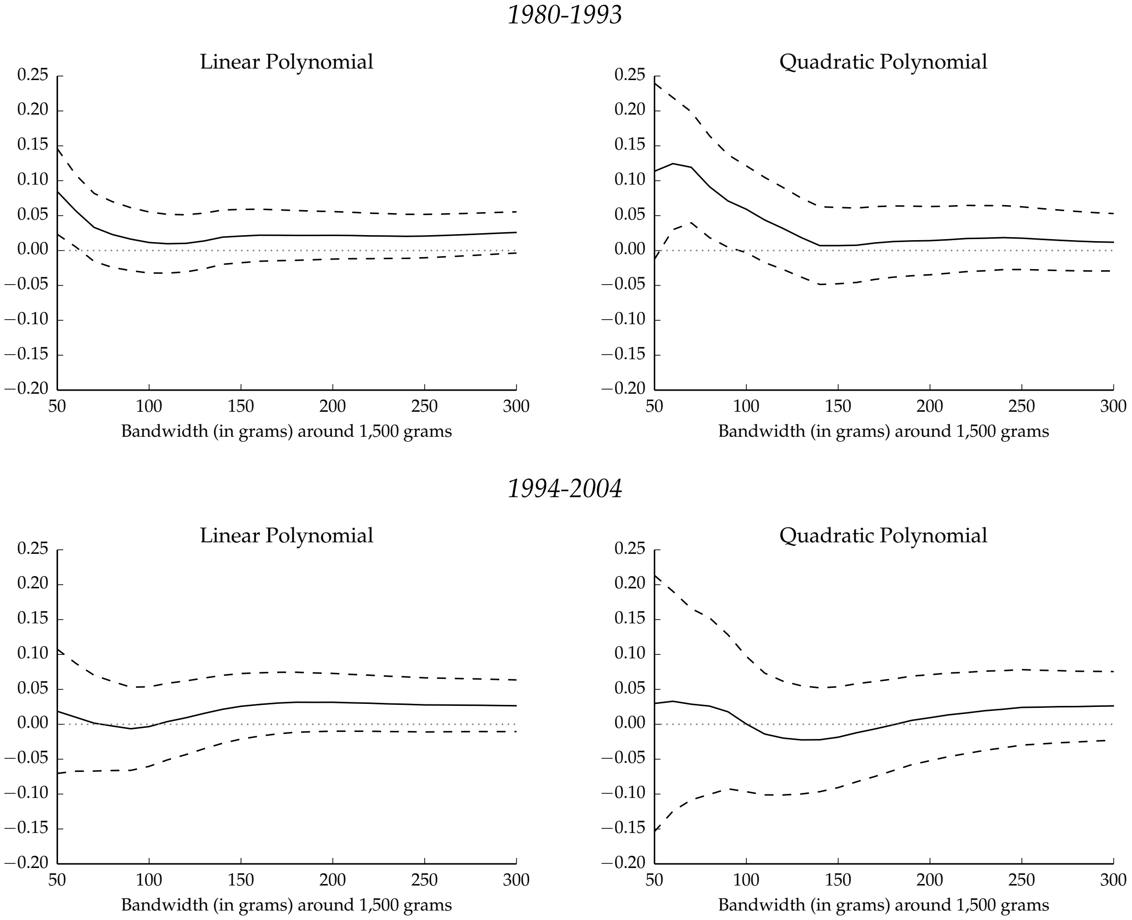
Notes: Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

3.4.1.3 Balancedness of Covariates around Cutoff

One identifying assumption of the regression discontinuity design is that there are no discontinuities in other covariates at the cutoff that are are also correlated with the outcome. If covariates are unbalanced across the cutoff, they might bias the estimation of the treatment effect. In our setting, the absence of an effect might be due a change in some covariate at the cutoff that counteracts the conjectured decrease in infant mortality below 1,500 grams.

One source of unbalanced covariates is the manipulation of birth weight. While it is impossible to time delivery such that weight falls just below the cutoff, the manipulation of birth weight after delivery is well-conceivable. Indeed, Jürges and Köberlein (2013) document that DRG reimbursement of neonatal care based on the VLBW cutoff leads to an unusual surge in birth weights just below 1,500 grams. This points to the downward manipulation of birth weight in response to monetary incentives. They also show that babies with manipulated weight have poorer health on average. Under these conditions, a regression discontinuity design becomes invalid. Our main sample focuses on the time period 1980-1993, when DRG reimbursement was not yet in place. Manipulation due to monetary incentives is therefore limited. However, manipulation might also occur for other reasons, e.g. because parents are aware of treatment guidelines based on VLBW and pressure physician into reporting a lower birth weight such that additional interventions can be performed. Such type of parents might also be different in other dimensions that are

Figure 3.3: Infant Mortality around 1,500 Grams of Birth Weight by Time Period, Polynomial Order and Bandwidth



Notes: Solid lines indicate estimated treatment effects on infant mortality around the 1,500 gram cutoff when we vary the bandwidth between 50 and 300 grams (baseline: 200 grams). Dashed lines indicate 95%-confidence-bands of these estimates. "Quadratic polynomial" means that we allow infant mortality to be a quadratic function of birth weight separately above and below the cutoff. Only 32 and more weeks of gestational age.

potentially correlated with infant mortality.[6]

One way to check for manipulation is to test for a discontinuity in the density of the birth weight distribution at the cutoff, as suggested by McCrary (2008). To perform this test, we collapse observations by birth weight and count the frequency of each value in the window between 1,300 and 1,700 grams. We then estimate the same regression as above, but with frequencies rather than infant mortality as the outcome variable. In line with a visual inspection of Figure 3.1, we find no statistically significant discontinuity at the cutoff.[7]

6. For evidence on the correlation of parental socioeconomic status with newborn mortality in Sweden, see Arntzen et al. (2008) on maternal education and Leon et al. (1992) on social class.

7. For births with gestational age above 32 weeks, the coefficient is -2.68 (S.E. 4.01). For births with gestational age below 32 weeks, the coefficient is -1.18 (S.E. 4.28).

Table 3.3: Other Covariates around 1,500 Grams by Time Period

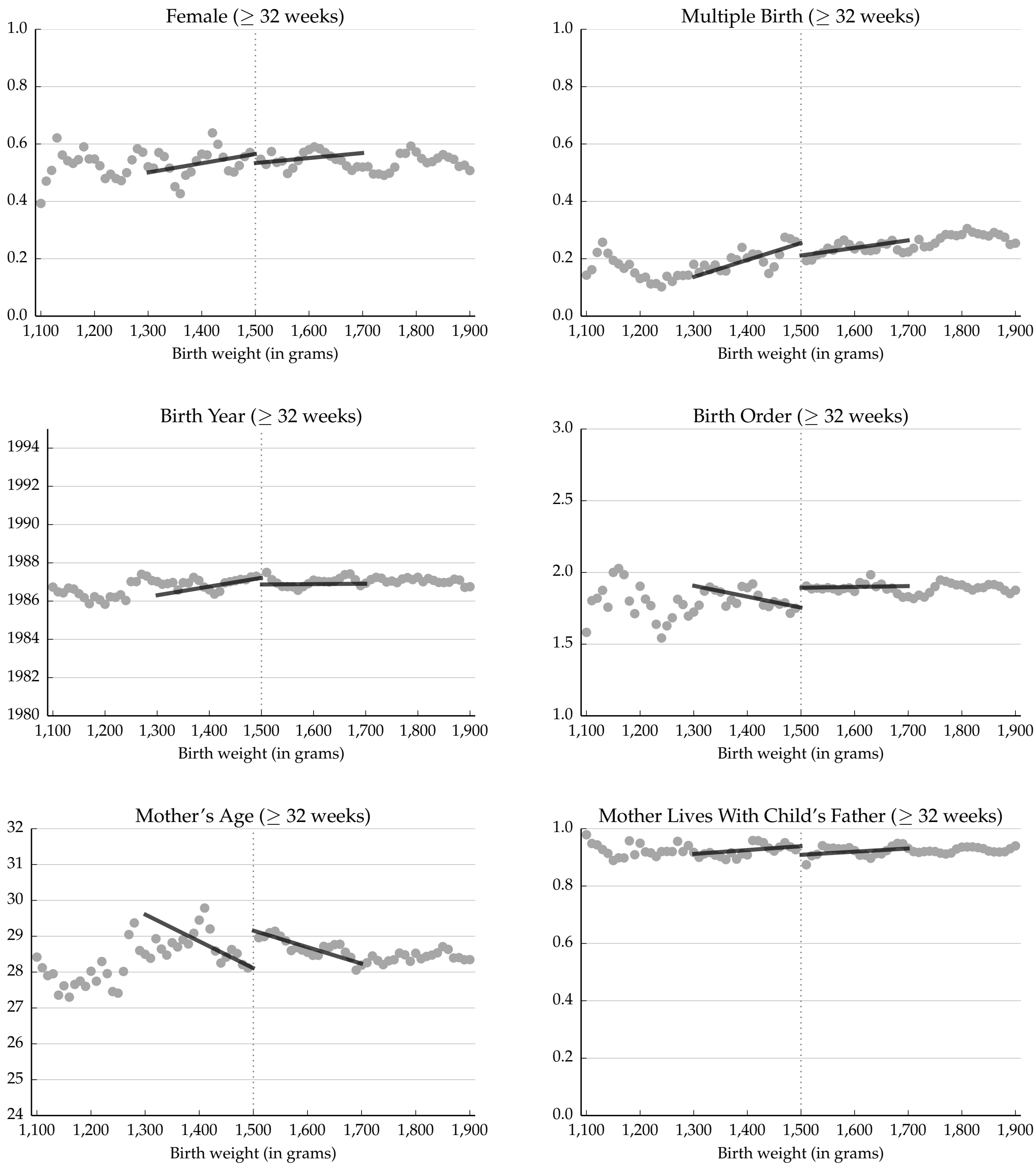
	1980-1993	1994-2004
Female	0.0317	-0.0996
	(0.0371)	(0.0546)
Mean	0.533	0.602
N	3,474	2,239
Multiple Birth	0.0435	-0.0380
	(0.0267)	(0.0454)
Mean	0.211	0.316
N	3,474	2,239
Birth Year	0.3470	0.4578
	(0.5667)	(0.3159)
Mean	1,986.868	1,998.393
N	3,474	2,239
Birth Order	-0.1383*	-0.0734
	(0.0626)	(0.0924)
Mean	1.893	1.768
N	3,328	1,941
Mother's Age	-1.0470*	0.2501
	(0.4614)	(0.5475)
Mean	29.156	29.450
N	3,328	1,941
Mother Lives With Child's Father	0.0302	-0.0140
	(0.0235)	(0.0269)
Mean	0.909	0.917
N	2,695	2,058

Notes: Bandwidth of 200 grams on either side of 1,500 grams is used. We include no controls other than a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Only 32 and more weeks of gestational age. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

This is evidence against downward manipulation of birth weight. However, downward manipulation might also have been counteracted by a simultaneously happening upward manipulation, so that an additional analysis of covariates remains warranted.

Another source of unbalanced covariates besides manipulation is data heaping. As described in Section 3.3, birth weight values that are multiples of 5, 10, 50 or 100 grams are particularly frequent in the data because of rounding. Moreover, this heaping is correlated with several observable characteristics that are also associated with infant mortality. As a consequence, correlated heaping may generate unbalanced covariates and bias in the treatment effect, especially if strong heaping occurs close to the cutoff (Barreca et al. 2015). In our setting, strong heaping can be found at multiples of 100 grams, one of which is exactly the cutoff.

Figure 3.4: Other Covariates around 1,500 Grams



Notes: Each point gives the average value of the respective covariate in 30 grams bins of birth weight centered at 10 gram intervals. The 1,500 gram point is dropped. Dark lines are from fitted RDD models according to the specification described in Section 3.3, but excluding the vector of controls to ensure consistency with the bin points. Only years 1980-1993 and only 32 and more weeks of gestational age.

For covariates that are observable, we can test their unbalancedness directly. Table 3.3 reports results from regressions where we use some covariate as the dependent variable and exclude all other covariates from the controls. Figure 3.4 contains the corresponding plots. There are significant decreases in birth order and mother's age once birth weight falls below 1,500 grams. These findings might only be a statistical artifact, but since both variables are correlated with infant mortality, this poses a threat to our identification strategy.

To address this threat, we run regressions where we include additional variables among the controls. Table 3.4 gives the results. Starting from the baseline regression in column 5, we one-by-one control additionally for hospital fixed effects, mother's age, birth order and whether the mother lives with the child's father. Throughout columns 1 to 8, our estimate remains remarkably robust. An exception is including a dummy for whether the mother lives with the child's father. But this variable is only available from 1983 onwards so that the sample size becomes much smaller. On the whole, our results do not appear to be driven by discontinuities in observable covariates. However, we cannot rule out such discontinuities in other, unobserved characteristics.

Additional checks that do not rely on the observability of covariates can be performed to address correlated heaping. In our baseline specification, we use a dummy to control for observations at the 1,500 grams threshold. Alternatively, one might just drop these observations from the regression. This so-called donut regression produces very similar results as before, as shown in the top panel of Table 3.14. The bottom panel shows regressions in which we do not only control for 1,500 grams, but *all* birth weights that are multiples of 100 grams. Once again, the estimates are very similar.

3.4.1.4 Other Sensitivity Checks

So far, we conducted estimations over relatively long time periods (1980-1993 and 1994-2004). There are reasons to believe that effects are not constant within these time periods and hence we look at shorter intervals in the following. At the end of the 1980s, surfactant therapy was introduced. This lung treatment is effective at reducing newborn deaths due to the infant respiratory distress syndrome. Swedish researchers were heavily involved in the development of surfactant therapy and the first Swedish child was treated with surfactant as early as in 1983 (Bohlin et al. 2009). Bharadwaj et al. (2013) find that their effects on school outcomes are driven by years when surfactant therapy was in use. The same holds true for infant mortality, but only in Chile and not in Norway. The authors attribute this finding to the overall lower level in mortality in Norway in later years. To study if surfactant therapy increase the relevance of the VLBW cutoff in Sweden, we divide the 1980-1993 period into years before and after 1987.

We also divide the 1994-2004 period into years before and after 2001. This is because - as described in Section 3.2 - DRG codes based on the VLBW threshold were introduced in 2001, generating monetary incentives to manipulate birth weight. These incentives should

Table 3.4: Sequentially Adding Controls

Control added	-	Female	Multiple Birth	County of Residence	Birth Year	Hospital	Birth Order	Mother's Age	Mother Lives With Child's Father
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1980-1993	0.0179	0.0184	0.0202	0.0210	0.0218	0.0280	0.0272	0.0259	0.0115
	(0.0180)	(0.0179)	(0.0182)	(0.0184)	(0.0174)	(0.0172)	(0.0171)	(0.0169)	(0.0186)
Mean	0.0798	0.0798	0.0798	0.0798	0.0798	0.0798	0.0798	0.0798	0.0798
N	3,474	3,474	3,474	3,474	3,474	3,465	3,319	3,319	2,570
1994-2004	0.0326	0.0313	0.0307	0.0285	0.0315	0.0256	0.0391	0.0391	0.0320
	(0.0206)	(0.0205)	(0.0205)	(0.0211)	(0.0211)	(0.0215)	(0.0235)	(0.0235)	(0.0227)
Mean	0.0357	0.0357	0.0357	0.0357	0.0357	0.0357	0.0357	0.0357	0.0357
N	2,239	2,239	2,239	2,239	2,239	2,207	1,912	1,912	1,761

Notes: Bandwidth of 200 grams on either side of 1,500 grams is used. The set of controls is extended in each column with the variable given in the column heading. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Only years 1980-1993. Only 32 and more weeks of gestational age. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

be absent in the years before 2001.

Table 3.5 shows the estimation results for each of the resulting time intervals. First note that the mean of infant mortality decreases by two thirds over time - from 10 percent in 1980-1986 to 3 percent in 2001-2004. This large drop points to the potential of neonatal care - including surfactant therapy - for reductions in mortality. If neonatal care changes discontinuously across the cutoff, then this should be reflected in mortality outcomes. Nevertheless, our discontinuity estimate remains statistically insignificant across all intervals. The point estimate is somewhat smaller in the 1987-1993 period when surfactant came into use, but still positive. On the whole, there is no indication that the VLBW threshold would play a major role in determining care in Sweden at any point in time.[8]

Table 3.5: Mortality around 1,500 Grams of Birth Weight by Detailed Time Period

$\geq$ *32 weeks*	1980-1986	1987-1993	1994-2000	2001-2004
Infant Mortality				
Birth weight < 1,500	0.0578	0.0003	0.0436	0.0056
	(0.0385)	(0.0228)	(0.0284)	(0.0346)
Mean	0.098	0.064	0.033	0.042
N	1,588	1,886	1,481	758
Neonatal Mortality				
Birth weight < 1,500	0.0273	0.0044	0.0195	-0.0015
	(0.0324)	(0.0181)	(0.0203)	(0.0333)
Mean	0.085	0.043	0.023	0.040
N	1,588	1,886	1,481	758

Notes: Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Only 32 and more weeks of gestational age. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Finally, we also check whether our results hinge on the separation of the sample into births above and below 32 weeks of gestation. While this choice is motivated by the WHO classification of "very preterm" births, which likely receive extra care anyway, we also experiment with alternative values for the division of the sample in Table 3.15 in the Appendix. Qualitatively, the results do not change at all.

8. In principle, it is possible to shrink time intervals further, but then we would run into power problems due to insufficient sample size.

3.4.2 First Stage and Long-run Outcomes

3.4.2.1 First Stage

Our failure to detect any changes in mortality across the VLBW cutoff might simply reflect the irrelevance of the cutoff for neonatal care in Sweden. Unfortunately, due to lacking data, we cannot study to what extent specific interventions, such as continuous positive airway pressure or surfactant therapy, change around 1,500 grams. However, we do observe and investigate one indirect measure of treatment intensity.

Specifically, we look at the number of days spent in hospital after birth. Both Almond et al. (2010) and Bharadwaj et al. (2013) show that reductions in infant mortality can be explained by corresponding increases in length of hospital stay.[9]. In Panel A of Table 3.6, we estimate the above specified regression discontinuity design with the length of hospital stay as the outcome variable. At 1,500 grams, babies spent about 13 days in hospital on average, as opposed to 5.5 days in the general population of births. For the period 1980-1993, we estimate an effect of VLBW on length of stay that is virtually equal to zero. The confidence interval is quite tight, ranging from about -2.0 to +2.0. As a whole, the estimates in Table 3.6 suggest that length of stay does not discontinuously change around the cutoff.

Table 3.6: First Stage around 1,500 Grams of Birth Weight by Time Period and Gestational Age

	1980-1993		1994-2004	
Gestational Age	$\geq$ 32 weeks	$<$ 32 weeks	$\geq$ 32 weeks	$<$ 32 weeks
A. Length of Hospital Stay (in days)				
Birth weight < 1,500	0.1751	-0.0817	-1.9589	-0.2074
	(1.1159)	(0.8644)	(1.1585)	(1.1722)
Mean	12.817	11.306	10.309	10.440
N	2,235	2,152	1,334	1,541
B. Length of Hospital Stay (in days, constructed)				
Birth weight < 1,500	0.4445	1.3277	0.1564	-1.9931
	(1.0373)	(1.5434)	(2.3143)	(1.3233)
Mean	6.621	4.794	7.935	5.870
N	3,384	3,237	2,148	2,454

Notes: Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Because the exact number of days spent in hospital is missing for about one third of the

9. By contrast, Almond and Doyle (2011) find that additional days in hospital do not lead to improvements in infant survival.

birth records, we also construct an alternative length-of-stay measure that is available for more observations. We generate this measure from information on birth date and discharge date. Since birth date is only observed at the month level, we assume the individual was born on the 15th day of the respective month. This magnifies the standard deviation of the variable considerably. As shown in Panel B of Table 3.6, the average number of days is about 7 at 1,500 grams of birth weight and 3.5 days in the general population. Once again, there are no discontinuous changes of length of stay across the cutoff.

All in all, we do not find any evidence that hospital stays become longer for infants whose birth weight is just below 1,500 grams. In Table 3.16 in the Appendix, we divide the sample into shorter time intervals and confirm this finding.

Another indirect measure of treatment intensity are referrals to neonatal intensive care units (NICUs). A NICU is a hospital unit with specialized staff and equipment that is - in particular - able to provide continuous mechanical ventilation to newborn infants. Unfortunately, we do not observe postnatal referrals to a NICU, but only the hospital where the delivery took place.[10] However, referrals should be rare for hospitals that have a NICU and more frequent for those without. We can therefore investigate whether effects on infant mortality are concentrated in hospitals without a NICU, since here the VLBW cutoff might be used to decide about referrals.

To identify hospitals with and without a NICU, we draw on Finnström, Olausson, et al. (1997), who classify Swedish hospitals into internationally used levels of neonatal care.[11] We define hospitals at levels III and IIa as those with a NICU and hospitals at levels IIb, I and others as those without a NICU. As Finnström, Olausson, et al. (1997) point out, level IIa hospitals seldom refer infants to level III hospitals, so that most referrals originate from hospitals that we define as not having a NICU.

Table 3.7 shows the results when we separately estimate the effect of the VLBW cutoff on infant mortality in hospitals with and without a NICU. For an gestational age of more than 32 weeks, we find a negative effect of VLBW in hospitals without a NICU in the period 1980-1993. However, this estimate is not statistically significant, as are all others in the table.

Overall, there is no evidence that treatment indicators such as length of stay or referrals change around the cutoff. However, we cannot rule out that certain interventions not captured by these indicators are more frequently performed below the cutoff.

3.4.2.2 Long-run Outcomes

Even if neonatal care does not reduce infant mortality below 1,500 grams, it might still lead to cognitive and mental improvements in those infants that survive. As an example,

10. Referrals can and do take place also before birth, but since birth weight is not known before delivery with sufficient precision, referrals are unlikely to be determined by the VLBW threshold.

11. Finnström, Olausson, et al. (1997) do not provide the names of the hospitals that belong to each level. For this information, we additionally refer to Finnström, Ewald, et al. (1997).

Table 3.7: Infant Mortality around 1,500 Grams of Birth Weight by NICU Availability, Time Period and Gestational Age

	1980-1993		1994-2004	
	$\geq$ 32 weeks	$<$ 32 weeks	$\geq$ 32 weeks	$<$ 32 weeks
With NICU				
Birth weight < 1,500	0.0486	0.0109	0.0428	0.0197
	(0.0279)	(0.0314)	(0.0333)	(0.0191)
Mean	0.083	0.090	0.038	0.017
N	1,530	1,619	898	1,246
Without NICU				
Birth weight < 1,500	-0.0091	0.0126	0.0189	-0.0223
	(0.0278)	(0.0284)	(0.0247)	(0.0191)
Mean	0.079	0.062	0.035	0.038
N	1,935	1,679	1,309	1,310

Notes: Regressions separately run for hospitals with and without NICU. Based on levels of neonatal care provided by Finnström, Olausson, et al. (1997), we define hospitals at levels III and IIa as those with a NICU and hospitals at levels IIb, I and others as those without a NICU. Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

surfactant therapy helps prevent insufficient oxygen supply that causes persistent brain damage. Bharadwaj et al. (2013) investigate how children born below 1,500 grams perform at school later on. They find that test scores and grades are 0.15-0.22 standard deviations higher as compared to children just above the cutoff. Breining et al. (2015) report increases of 0.3-0.4 standard deviations in test scores for both VLBW children and their siblings.

This section explores the long-run impact of VLBW designation on grades in Sweden. Specifically, we look at school grades from the 9th grade, which is the last year of compulsory school in Sweden. These grades are important to students, since they are used to apply for high school. We focus on the overall grade in our analysis, which is the sum of 16-17 individual grades for different subjects that must include English, Swedish and mathematics. Grades take on the values 10, 15 and 20 points and fails give zero points. As shown in Table 3.1, the average overall grade is 205 points, implying an average of about 12 points per subject. The numbers are a slightly lower for individuals with birth weight between 1,300 and 1,700 grams, in line with the assertion that prematurity causes long-lasting cognitive impairments.

We estimate equation 3.1 with the overall grade as the dependent variable. Grades are only available for the years 1998-2007, corresponding to birth cohorts 1982-1991 (and a few individuals born in 1981 and 1992). We lose additional observations because some children die early in life or simply miss the exams. We also exclude students who have several overall grades due to retaking exams. Table 3.8, Panel A presents the results. There is no significant effect of VLBW on the overall grade.

Table 3.8: Long-run Outcomes around 1,500 Grams of Birth Weight by Gestational Age

	1980-1993	
Gestational Age	$\geq$ 32 weeks	$<$ 32 weeks
A. Overall Grade		
Birth weight < 1,500	-5.3020	6.7975
	(5.8360)	(4.1456)
Mean	198.475	184.588
N	2,023	1,871
B. Log Mother Income in Year of Test		
Birth weight < 1,500	0.0006	0.0546
	(0.0687)	(0.0556)
Mean	12.798	12.708
N	1,909	1,698
C. Mother Employed in Year of Test		
Birth weight < 1,500	-0.0235	-0.0291
	(0.0393)	(0.0375)
Mean	0.903	0.868
N	1,911	1,699
D. Number of Children Born Later		
Birth weight < 1,500	0.0777	-0.1124
	(0.0759)	(0.0759)
Mean	0.869	0.944
N	3,328	3,168

Notes: Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Any effect on school grades will be biased if parental investments in children also change differentially around the cutoff. For example, parents whose children are just above the cutoff and do not receive additional care might decide to compensate for this deficit with own investments. This would mitigate the estimated effect. In Panels B-D of Table 3.8 we examine indicators of such parental investments. If mothers decide to stay at home to help children with homework, we would see changes in income and employment status across the cutoff. Even changes in subsequent fertility are conceivable, if mothers choose to have fewer kids in order to focus on the premature one.

None of the variables in Panels B-D changes significantly across the cutoff. This suggests that differential parental investments do not drive the absence of an effect on school grades. Instead, it seems that in line with previous sections, VLBW designation does not affect grades in the first place.

3.4.3 Other Birth Weight Cutoffs and Alternative Variables

Previous sections have suggested that the VLBW cutoff does not guide treatment decisions in Swedish neonatology. However, treatment decisions might instead rely on birth weight values other than 1,500 grams or even on values of variables other than birth weight, such as gestational age or small for gestational age. We explore these alternatives in the following.

3.4.3.1 Other Birth Weight Cutoffs

Table 3.9 reports results from regressions in which we replace the below-1,500-grams dummy in equation 3.1 with a dummy for birth weight falling below each of the 100-grams multiples between 1,000 grams and 2,500 grams. 1,000 grams correspond to the extremely low birth weight (ELBW) cutoff in the WHO classification and 2,500 grams to the low birth weight (LBW) cutoff. With more effective neonatal care measures becoming available for very light babies over time, the ELBW cutoff might have gained relevance in more recent years.[12]

Most infants around the ELBW cutoff are born before week 32 and most infants around the LBW cutoff are born after week 32. We therefore stop conditioning on gestational age above 32 weeks and pool all infants in the respective birth weight window regardless of age. As shown in Table 3.9, none of the ELBW or LBW or any other pseudo-cutoff generates discontinuities in infant mortality. There are two exceptions (1,400 grams in 1980-1993 and 1,600 grams in 1994-2004), but given the large number of tests, these might just as well be random. On the whole, we find no evidence that birth weight cutoffs other than 1,500 grams would guide neonatal care in Sweden.

3.4.3.2 Gestational Age

Since cutoffs in birth weight do not appear to play a role for treatment decisions in Sweden, we test whether there is instead any response to commonly used cutoffs in gestational age. Specifically, we follow the WHO classification and study infants younger than 37 weeks ("preterm"), 32 weeks ("very preterm") and 28 weeks ("extremely preterm") of age. We present estimates for different bandwidths and both linear and quadratic trends.

The validity of the regression discontinuity design is more questionable with gestational age as the running variable than with birth weight. Pregnant women know the current week of their pregnancy and can use this information to time birth (for example, via demanding a

12. Finnström, Olausson, et al. (1997), for instance, predict that care of ELBW babies will become more centralized in higher quality hospitals in Sweden.

Table 3.9: Infant Mortality around Other Birth Weight Cutoffs by Time Period

	1980-1993							
Cutoff (in grams)	1,000	1,100	1,200	1,300	1,400	1,500	1,600	1,700
Birth weight < Cutoff	0.0062	-0.0361	0.0163	-0.0082	-0.0251*	0.0161	-0.0154	-0.0068
	(0.0355)	(0.0319)	(0.0253)	(0.0182)	(0.0127)	(0.0115)	(0.0150)	(0.0108)
Mean	0.2017	0.1870	0.1432	0.1087	0.1081	0.0785	0.0706	0.0595
N	3,751	4,237	4,692	5,343	6,071	6,789	7,949	9,271
	1,800	1,900	2,000	2,100	2,200	2,300	2,400	2,500
Birth weight < Cutoff	0.0051	-0.0020	-0.0001	0.0098	0.0036	0.0017	0.0005	0.0026
	(0.0111)	(0.0071)	(0.0058)	(0.0073)	(0.0054)	(0.0036)	(0.0027)	(0.0017)
Mean	0.0451	0.0430	0.0379	0.0222	0.0195	0.0187	0.0153	0.0096
N	11,054	13,767	16,955	21,306	27,090	35,081	46,229	61,353
	1994-2004							
Cutoff (in grams)	1,000	1,100	1,200	1,300	1,400	1,500	1,600	1,700
Birth weight < Cutoff	-0.0051	0.0013	-0.0014	0.0047	-0.0027	0.0145	-0.0269**	0.0083
	(0.0301)	(0.0229)	(0.0187)	(0.0182)	(0.0163)	(0.0118)	(0.0096)	(0.0095)
Mean	0.1036	0.0771	0.0713	0.0597	0.0624	0.0320	0.0555	0.0214
N	2,820	3,098	3,359	3,726	4,264	4,837	5,656	6,487
	1,800	1,900	2,000	2,100	2,200	2,300	2,400	2,500
Birth weight < Cutoff	-0.0088	0.0051	-0.0038	0.0002	0.0048	0.0026	-0.0006	0.0002
	(0.0072)	(0.0071)	(0.0048)	(0.0064)	(0.0043)	(0.0026)	(0.0019)	(0.0017)
Mean	0.0316	0.0186	0.0199	0.0197	0.0136	0.0102	0.0097	0.0062
N	7,593	9,279	11,397	14,317	18,244	23,278	30,255	40,047

Notes: Bandwidth of 200 grams on either side of cutoff is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for observations at the cutoff. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. All gestational ages. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Caesarian section). Manipulation of gestational age can also occur after birth, as it cannot be objectively measured and verified so long after conception. Daysal et al. (2013) use the 37-weeks-threshold that triggers discontinuous changes in the probability of obstetrician supervision of births in the Netherlands, but do not find any effects on health outcomes. Almond et al. (2010) also study the 37-weeks-threshold and do find reductions in infant mortality.

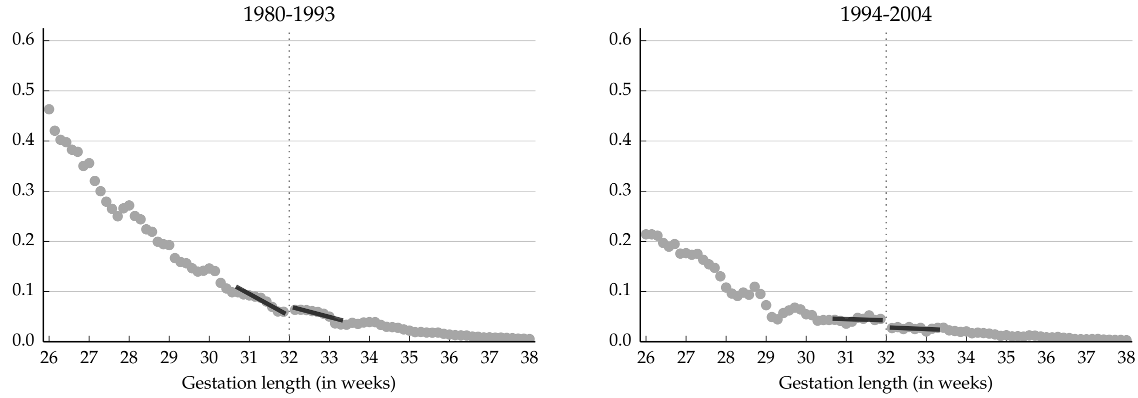
In the 1980-1993 period, Table 3.10 shows that only the 32-weeks-cutoff leads to decreases in mortality in some specifications. However, the effects disappear when we allow for quadratic trends, indicating that the linear specification is inadequate. In the 1994-2004 period, there is robust evidence for an *increase* in mortality across the 28-week-threshold, a finding that appears implausible. We also find effects around the 37-week cutoff which are

Table 3.10: Infant Mortality around Gestational Age Cutoffs by Time Period and Bandwidth

	1980-1993											
Cutoff (in weeks)	28				32				37			
Bandwidth (in days)	4	7	10	14	4	7	10	14	4	7	10	14
Linear	0.0281	-0.0215	-0.0410	-0.0407	0.0025	-0.0131*	-0.0167**	-0.0158**	0.0008	-0.0011	-0.0012	-0.0011
	(0.0370)	(0.0334)	(0.0308)	(0.0249)	(0.0030)	(0.0064)	(0.0063)	(0.0053)	(0.0016)	(0.0015)	(0.0014)	(0.0011)
Mean	0.2324	0.2532	0.2563	0.2558	0.0588	0.0639	0.0703	0.0714	0.0083	0.0092	0.0092	0.0090
Quadratic	-0.0226	0.0181	-0.0133	-0.0408	0.0385**	0.0017	-0.0059	-0.0159	0.0103**	0.0012	-0.0002	-0.0012
	(0.0409)	(0.0388)	(0.0360)	(0.0377)	(0.0061)	(0.0083)	(0.0079)	(0.0083)	(0.0001)	(0.0018)	(0.0015)	(0.0016)
Mean	0.4204	0.2508	0.2508	0.2564	0.0424	0.0589	0.0563	0.0683	0.0094	0.0085	0.0090	0.0095
N	1,278	2,345	3,331	4,999	3,706	6,959	10,213	15,953	56,500	112,415	182,998	328,383
	1994-2004											
Cutoff (in weeks)	28				32				37			
Bandwidth (in days)	4	7	10	14	4	7	10	14	4	7	10	14
Linear	0.0364*	0.0474**	0.0306**	0.0285**	-0.0035	0.0079	0.0138	0.0148	-0.0028**	-0.0032**	-0.0022*	-0.0013
	(0.0158)	(0.0115)	(0.0105)	(0.0087)	(0.0156)	(0.0161)	(0.0143)	(0.0115)	(0.0011)	(0.0008)	(0.0009)	(0.0010)
Mean	0.1023	0.0900	0.1066	0.1083	0.0243	0.0276	0.0285	0.0282	0.0064	0.0065	0.0062	0.0057
Quadratic	0.0310	0.0321	0.0554**	0.0331*	-0.1041**	-0.0157	0.0007	0.0116	-0.0106**	-0.0047**	-0.0038**	-0.0036**
	(0.0345)	(0.0213)	(0.0153)	(0.0146)	(0.0084)	(0.0163)	(0.0183)	(0.0176)	(0.0003)	(0.0017)	(0.0010)	(0.0009)
Mean	0.0595	0.1040	0.0761	0.1033	0.0304	0.0264	0.0281	0.0278	0.0067	0.0073	0.0067	0.0067
N	970	1,752	2,616	3,819	2,746	5,154	7,794	11,983	37,301	75,222	128,375	227,460

Notes: Regressions with gestational age as running variable and cutoff values, 28, 32 and 37 weeks.Bandwidths of 4, 7, 10 and 14 days on either side of the cutoff are used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for observations at the cutoff. We report estimates for both linear and quadratic trends that might be different below and above the cutoff and use local polynomial regressions with triangular weighting. Standard errors clustered at the days level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Figure 3.5: Infant Mortality around 32 Weeks of Gestational Age by Time Period



Notes: Each point gives the average infant mortality in 3 days bins of gestational age centered at 1 day intervals. The 32 weeks (=224 days) point is dropped. Dark lines are from fitted RDD models adapted from the specification described in Section 3.3, with a bandwidth of 10 days, a dummy for 32 weeks and excluding the vector of controls to ensure consistency with the bin points.

of the expected sign. This indicates that this cutoff may govern actual treatment decisions. However, we prefer to treat this finding with caution given the identification problems mentioned above, the generally low level of infant mortality at this gestational age in these years, and the lack of any visible discontinuity at 37 weeks in Figure 3.5.

3.4.3.3 Small for Gestational Age

Low birth weights are more critical for infants with higher gestational age. This idea is conceptualized in the definition of Small for Gestational Age (SGA). For any given gestational age, it provides upper bounds of birth weight below which an infant is considered "small" or "light" for gestational age. The typical cutoffs are defined as the tenth percentile of the conditional birth weight distribution or, alternatively, two standard deviations below the conditional mean of birth weight. This section explores whether the SGA definition is used for treatment decisions in Sweden.

The Swedish Medical Birth Register provides us with information on whether an infant was classified as SGA. However, none of the official Swedish reference standards described in Sterky (1970) and Niklasson et al. (1991) appears to coincide with the SGA classification that we observe in the data. Many babies labeled SGA in the register were not actually SGA according to either of the definitions.[13] Because the actual definition is probably more relevant for medical interventions than theoretical definitions, we decided to reconstruct the actual definition from the register using the following procedure: First, we identify all

13. Sterky (1970) provides definitions based on births that took place between July 1st, 1956 and June 30, 1957. Niklasson et al. (1991) update this definition with data from 1977-1981.

births that were classified as small for gestational age in the data. We restrict attention to gestational ages between 26 weeks (= 182 days) and 37 weeks (= 259 days) of gestation because for lower ages there are only few observations and for higher ages the indicator is unlikely to be used for treatment decisions. For each gestational age at the day level, we identify the upper bound of birth weight below which an infant is characterized as SGA in the data.[14] We conduct the whole procedure separately for girls and boys.[15] This gives us an age- and sex-specific cutoff value of birth weight below which we assume a newborn was classified as small for gestational age. We then define deviations in weight from this cutoff as the running variable in our regression.

Table 3.11 provides the results. Since the relevance of SGA may vary with age, we separately investigate infants in different intervals of age as shown in the table. We also also allow for different bandwidths. Overall, we do not find significant effects across the SGA cutoff beyond those to be expected from statistical chance. To conclude, SGA cutoff does not appear to guide treatment decisions for newborn infants in Sweden.

3.5 Discussion

The previous sections demonstrated that the VLBW cutoff did not play a role in guiding treatment decisions in Swedish neonatal care since 1980. We failed to find the expected drop in infant mortality just below the 1,500 grams cutoff. This stands in contrast to evidence from other countries, most notably Norway, which is comparable to Sweden in many dimensions. Our results are unlikely to be driven by idiosyncratic assumptions, as we closely match the Norwegian study by Bharadwaj et al. (2013) in terms of time period and econometric specification. Although the standard errors of our main estimate are too large to rule out that there actually is an effect of VLBW designation, they are of the same order of magnitude as in Norway.[16] Moreover, in contrast to Norway, there is no visible discontinuity in a plot of infant mortality against birth weight around 1,500 grams. In addition to infant mortality, we also show that the VLBW cutoff is associated neither with changes in length of hospital stay nor with long-run schooling outcomes. This adds to our conclusion that the VLBW cutoff is irrelevant in Sweden.

We next explore potential reasons for this finding. Bharadwaj et al. (2013) provide evidence that regulatory standards for neonatal care are in place at the national level in Chile, some of which make VLBW an explicit requirement for treatment. Similarly, there

14. Due to coding error, this value might sometimes be too high. To address this problem, we compute the difference to the second-largest value. If this difference is too large, we define the second-largest value rather than the first-largest value to be cutoff value of birth weight. Since not only one but several observations might be miscoded, we repeat this step for additional lower-ranked values up until the sixth-largest value.

15. We also allow for different SGA definitions before and after 1992 to account for the possibility that the definitions did change after the update proposed by Niklasson et al. (1991).

16. For 1980-1993, our estimations suggest a standard error of 0.017 for infants with at least 32 weeks of gestation and of 0.012 in the whole sample. The respective values for Norway are 0.013 and 0.015.

Table 3.11: Infant Mortality around Small for Gestational Age (SGA) Cutoff by Time Period, Weeks of Gestational Age and Bandwidth

	1980-1993											
Gestational Age	$26 \leq$ Week < 30				$30 \leq$ Week < 34				$34 \leq$ Week < 37			
Bandwidth (in grams)	50	100	150	200	50	100	150	200	50	100	150	200
Linear	-0.0051	-0.0273	-0.0391	-0.0437	-0.0047	0.0072	0.0024	0.0055	-0.0125	-0.0022	-0.0029	-0.0049
	(0.0534)	(0.0426)	(0.0385)	(0.0352)	(0.0253)	(0.0202)	(0.0162)	(0.0141)	(0.0127)	(0.0090)	(0.0074)	(0.0070)
Mean	0.2549	0.2690	0.2688	0.2668	0.0662	0.0493	0.0475	0.0458	0.0279	0.0278	0.0278	0.0268
Quadratic	0.0095	-0.0286	-0.0024	-0.0237	-0.0446	-0.0062	0.0054	0.0005	-0.0363**	-0.0140	-0.0032	-0.0008
	(0.0984)	(0.0541)	(0.0477)	(0.0463)	(0.0331)	(0.0273)	(0.0236)	(0.0203)	(0.0112)	(0.0129)	(0.0110)	(0.0094)
Mean	0.2568	0.2534	0.2667	0.2689	0.0700	0.0627	0.0533	0.0502	0.0263	0.0301	0.0281	0.0281
N	352	706	1,086	1,485	862	1,764	2,632	3,632	1,768	3,552	5,478	7,515
	1994-2004											
Gestational Age	$26 \leq$ Week < 30				$30 \leq$ Week < 34				$34 \leq$ Week < 37			
Bandwidth (in grams)	50	100	150	200	50	100	150	200	50	100	150	200
Linear	-0.0495	-0.0404	-0.0337	-0.0189	-0.0155	-0.0086	-0.0088	-0.0062	0.0061	0.0015	0.0008	0.0011
	(0.0499)	(0.0383)	(0.0317)	(0.0293)	(0.0173)	(0.0135)	(0.0128)	(0.0116)	(0.0167)	(0.0109)	(0.0082)	(0.0069)
Mean	0.1596	0.1493	0.1288	0.1159	0.0180	0.0269	0.0312	0.0302	0.0171	0.0125	0.0129	0.0124
Quadratic	-0.0505	-0.0415	-0.0608	-0.0568	-0.0175	-0.0146	-0.0133	-0.0118	0.0158	0.0088	0.0040	0.0023
	(0.0656)	(0.0532)	(0.0460)	(0.0406)	(0.0213)	(0.0181)	(0.0148)	(0.0147)	(0.0257)	(0.0171)	(0.0135)	(0.0112)
Mean	0.1577	0.1607	0.1628	0.1469	0.0253	0.0167	0.0244	0.0298	0.0238	0.0152	0.0129	0.0134
N	422	849	1,322	1,796	788	1,664	2,502	3,449	1,338	2,683	4,205	5,820

Notes: Regressions with small for gestational age as running variable at different gestational age intervals.Bandwidths of 50, 100, 150 and 200 grams on either side of the cutoff are used. Controls are female, birth year, multiple birth and county of residence. We report estimates for both linear and quadratic trends that might be different below and above the cutoff and use local polynomial regressions with triangular weighting. Standard errors clustered at the grams level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

are nation-wide recommendations in Norway and evidence according to which all neonatal wards in Norway listed the VLBW as a determinant of care. Despite a lengthy search of the literature, to the best of our knowledge there are no nation-wide regulatory standards or recommendations that prescribe additional care in Sweden below 1,500 grams. We do find the VLBW and ELBW thresholds to be mentioned in some documents, but only very recently and only at the regional level. In addition, in line with our previous analyses, there is no evidence that other cutoff values of birth weight or alternative variables would be used instead of the VLBW cutoff in Sweden.

We provide some speculative explanations for this absence of regulatory standards. The deeper rationale for implementing a cutoff such as 1,500 grams is that many neonatal interventions do not pay off above the cutoff, since infants have a high chance of surviving anyway and benefit only little from these costly measures. Setting a guideline is therefore effective at controlling cost and preventing overuse. One cost component of newborn care are transfers from small hospital to larger ones with specialized neonatal intensive care units. As noted by Daltveit et al. (1999), Norway is much more sparsely populated than Sweden and has a larger number of maternity wards than Sweden despite its smaller population. As a consequence, costly transfers of infants to NICUs are required relatively frequently, potentially generating more need for neonatal care standards than in Sweden. So relatively low cost could be one explanation for the absence of standards in Sweden.

Another explanation could be the wide-spread expertise in neonatal care in Swedish hospitals. Sweden has always been among those countries with the lowest infant mortality rate worldwide and its health care system has often served as a role model to which other countries were compared to (e.g. Frechette and Russo 1982). As an example for their leading position, Swedish neonatologists were involved in the development of surfactant therapy and were among the first to use it (Bohlin et al. 2009). This historically grown expertise in neonatal care might help physicians make appropriate treatment decisions as regards cost-effectiveness so that no standards are needed.

For a similar reason, the VBLW cutoff might not have been used as a rule of thumb either. Frank and Zeckhauser (2007) point to cognition, coordination and communication cost as motives for the use of rule of thumbs in medical care. In the context of neonatal care, cognition cost refers to the mental effort required for making the optimal treatment decision for an individual infant. Coordination cost is increased when there is division of labor between several physicians and nurses within a maternity ward. Communication cost is the extent to which time and persuasiveness are needed to justify denial of care to the infant's parents. While all three types of cost motivate the use of rule of thumbs, they are substantially reduced if personnel is highly qualified, as is the case in Sweden. This could explain why the VLBW is not used as a rule of thumb in Sweden. This hypothesis is consistent with studies from the United States, where Almond et al. (2010, 2011) provide only anecdotal evidence for hospital protocols using the VLBW cutoff, suggesting it is merely used as rule of thumb. Here, the effects are concentrated in low-quality hospitals, where coordination is worse and less-trained physicians face higher cognition cost.[17]

3.6 Conclusion

This paper shows that in contrast with other countries, birth weight just below 1,500 grams (very low birth weight - VLBW) has not been associated with a discontinuous drop in infant mortality in Sweden since 1980. This finding is insensitive to various alternative econometric specifications. We also show that treatment intensity - as measured by hospital length of stay - and school grades later in life do not change around the VLBW cutoff. This indicates that the VLBW cutoff did not play a role in guiding treatment decision in neonatal care in Sweden. Finally, we find no evidence that birth weight values other than 1,500 grams or values of other variables such as gestational age would be used as a substitute for VLBW.

Based on these findings and an evaluation of the medical literature, we conjecture that regulatory standards in form of threshold values were absent in Sweden. Potential reasons for this absence are relatively low treatment cost and the high level of expertise in neonatal care. After all, while the absence of a threshold value might be regrettable from an econometric point of view, it perhaps simply signals the presence of a well-functioning health care system.

Future work should shed light on the role that cost of neonatal care and qualification level of medical staff play for the imposition of regulatory standards and the adherence to rule of thumbs. In particular, the nature of the hurdles that lead maternity wards to make oversimplified treatment decisions should be investigated.

17. An alternative explanation is that the VLBW cutoff is only used as a decision rule for referrals and only low-quality hospitals actually make referrals.

3.A Appendix

3.A.1 Tables

Table 3.12: Correlation of Frequent Birth Weight Values with Socioeconomic and Demographic Characteristics

	Baseline				With county fixed effects				With hospital fixed effects			
	1980-1993											
Dep. Var. Multiple of (in grams)	5	10	50	100	5	10	50	100	5	10	50	100
Female	0.0108	0.0240*	-0.0095	0.0056	0.0063	0.0202*	-0.0109	0.0049	0.0037	0.0190	-0.0115	0.0054
	(0.0078)	(0.0103)	(0.0086)	(0.0070)	(0.0068)	(0.0098)	(0.0086)	(0.0069)	(0.0065)	(0.0097)	(0.0086)	(0.0070)
Birth Order	0.0003	0.0053	-0.0034	-0.0042	-0.0061	0.0005	-0.0041	-0.0048	-0.0035	0.0018	-0.0040	-0.0055
	(0.0043)	(0.0056)	(0.0044)	(0.0036)	(0.0038)	(0.0053)	(0.0044)	(0.0036)	(0.0036)	(0.0052)	(0.0044)	(0.0036)
Birth Year	-0.0167**	-0.0369**	-0.0098**	-0.0060**	-0.0149**	-0.0357**	-0.0096**	-0.0057**	-0.0143**	-0.0353**	-0.0094**	-0.0057**
	(0.0011)	(0.0015)	(0.0014)	(0.0011)	(0.0010)	(0.0015)	(0.0014)	(0.0011)	(0.0010)	(0.0015)	(0.0014)	(0.0011)
Multiple Birth	-0.0130	-0.0205	-0.0107	-0.0137	-0.0198*	-0.0277*	-0.0132	-0.0150	-0.0144	-0.0222	-0.0107	-0.0139
	(0.0099)	(0.0129)	(0.0103)	(0.0082)	(0.0085)	(0.0123)	(0.0103)	(0.0082)	(0.0080)	(0.0122)	(0.0103)	(0.0082)
Mother's Age	-0.0021**	-0.0019	-0.0028**	-0.0012	-0.0004	-0.0008	-0.0026**	-0.0010	-0.0005	-0.0009	-0.0025**	-0.0009
	(0.0008)	(0.0010)	(0.0008)	(0.0007)	(0.0007)	(0.0010)	(0.0008)	(0.0007)	(0.0007)	(0.0010)	(0.0008)	(0.0007)
Mother Lives With Child's Father	0.0097	0.0249	0.0149	0.0074	0.0139	0.0297	0.0163	0.0090	0.0137	0.0271	0.0153	0.0082
	(0.0145)	(0.0190)	(0.0155)	(0.0126)	(0.0126)	(0.0177)	(0.0155)	(0.0126)	(0.0122)	(0.0175)	(0.0155)	(0.0125)
N	8,091	8,091	8,091	8,091	8,091	8,091	8,091	8,091	8,071	8,071	8,071	8,071
	1994-2004											
Dep. Var. Multiple of (in grams)	5	10	50	100	5	10	50	100	5	10	50	100
Female	0.0082	0.0054	-0.0028	-0.0025	0.0164	0.0099	-0.0010	-0.0014	0.0073	0.0075	-0.0019	-0.0022
	(0.0106)	(0.0128)	(0.0082)	(0.0065)	(0.0087)	(0.0124)	(0.0082)	(0.0065)	(0.0078)	(0.0123)	(0.0083)	(0.0066)
Birth Order	-0.0107	-0.0124	-0.0065	-0.0038	-0.0051	-0.0101	-0.0054	-0.0029	-0.0032	-0.0101	-0.0058	-0.0034
	(0.0058)	(0.0068)	(0.0041)	(0.0032)	(0.0048)	(0.0066)	(0.0041)	(0.0032)	(0.0043)	(0.0065)	(0.0042)	(0.0033)
Birth Year	-0.0016	0.0007	-0.0011	-0.0005	-0.0008	0.0015	-0.0011	-0.0005	-0.0020	0.0020	-0.0011	-0.0004
	(0.0016)	(0.0020)	(0.0013)	(0.0010)	(0.0014)	(0.0019)	(0.0013)	(0.0010)	(0.0013)	(0.0020)	(0.0013)	(0.0011)
Multiple Birth	-0.0192	-0.0046	-0.0116	-0.0119	-0.0252**	-0.0060	-0.0124	-0.0121	-0.0199*	-0.0005	-0.0113	-0.0112
	(0.0116)	(0.0138)	(0.0086)	(0.0067)	(0.0096)	(0.0134)	(0.0086)	(0.0067)	(0.0085)	(0.0133)	(0.0088)	(0.0068)
Mother's Age	0.0001	-0.0026*	-0.0005	-0.0002	-0.0003	-0.0026*	-0.0005	-0.0002	0.0006	-0.0017	-0.0004	0.0000
	(0.0011)	(0.0013)	(0.0008)	(0.0006)	(0.0009)	(0.0013)	(0.0008)	(0.0006)	(0.0008)	(0.0013)	(0.0008)	(0.0007)
Mother Lives With Child's Father	-0.0558**	0.0367	0.0233	0.0152	-0.0537**	0.0410	0.0216	0.0144	-0.0196	0.0439	0.0257	0.0157
	(0.0196)	(0.0261)	(0.0155)	(0.0122)	(0.0159)	(0.0251)	(0.0155)	(0.0122)	(0.0143)	(0.0252)	(0.0158)	(0.0122)
N	6,046	6,046	6,046	6,046	6,046	6,046	6,046	6,046	5,977	5,977	5,977	5,977

Notes: Only birth weights between 1,200 and 1,800 grams. OLS regressions of dummies for multiples of 5, 10, 50 and 100 grams on various covariates. Heteroskedasticity-robust standard errors in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 3.13: Infant Mortality around 1,500 Grams of Birth Weight by Time Period, Bandwidth and Polynomial Order

1980-1993	50	100	150	200 (=Baseline)	250	300
Linear	0.0845**	0.0115	0.0207	0.0218	0.0207	0.0259
	(0.0313)	(0.0223)	(0.0195)	(0.0174)	(0.0159)	(0.0151)
Quadratic	0.1136	0.0591	0.0070	0.0141	0.0177	0.0119
	(0.0643)	(0.0317)	(0.0279)	(0.0249)	(0.0229)	(0.0210)
N	790	1,659	2,546	3,474	4,565	5,714
1994-2004	50	100	150	200 (=Baseline)	250	300
Linear	0.0186	-0.0033	0.0258	0.0315	0.0278	0.0265
	(0.0454)	(0.0291)	(0.0240)	(0.0211)	(0.0198)	(0.0189)
Quadratic	0.0298	0.0004	-0.0185	0.0094	0.0242	0.0263
	(0.0936)	(0.0495)	(0.0368)	(0.0314)	(0.0276)	(0.0251)
N	489	1,043	1,588	2,239	2,979	3,750

Notes: Regressions for alternative bandwidths in grams on either side of 1,500 grams. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We report estimates for both linear and quadratic trends that might be different below and above the cutoff and use local polynomial regressions with triangular weighting. Standard errors clustered at the grams level are given in parentheses. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 3.14: Alternative Controls for Heaping in Birth Weight

	1980-1993		1994-2004	
Gestational Age (in weeks)	$\geq$ 32 weeks	$<$ 32 weeks	$\geq$ 32 weeks	$<$ 32 weeks
Donut: Dropping Observations at 1,500 Grams				
Birth weight < 1,500	0.0218	0.0111	0.0308	0.0044
	(0.0175)	(0.0185)	(0.0211)	(0.0134)
Mean	0.080	0.076	0.036	0.028
N	3,391	3,220	2,215	2,550
Controlling for Multiples of 100 Grams				
Birth weight < 1,500	0.0255	0.0162	0.0295	0.0071
	(0.0169)	(0.0176)	(0.0208)	(0.0132)
Mean	0.077	0.071	0.038	0.028
N	3,474	3,315	2,239	2,598

Notes: Rather than a dummy for 1,500 grams, this table reports results from regressions where we either drop observations at 1,500 grams or control for *all* multiples of 100 grams. Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 3.15: Infant Mortality around Alternative Gestational Age Cutoffs by Time Period

	1980-1993							
Gestational Age	$\geq$ 30 weeks	$<$ 30 weeks	$\geq$ 31 weeks	$<$ 31 weeks	$\geq$ 33 weeks	$<$ 33 weeks	$\geq$ 34 weeks	$<$ 34 weeks
Birth weight < 1,500	0.0073	0.0666	0.0017	0.0419	0.0251	0.0131	0.0423	0.0128
	(0.0129)	(0.0441)	(0.0152)	(0.0263)	(0.0193)	(0.0166)	(0.0263)	(0.0139)
Mean	0.0745	0.0973	0.0761	0.0798	0.0781	0.0782	0.1013	0.0701
N	5,632	1,157	4,548	2,241	2,547	4,242	1,767	5,022
	1994-2004							
Gestational Age	$\geq$ 30 weeks	$<$ 30 weeks	$\geq$ 31 weeks	$<$ 31 weeks	$\geq$ 33 weeks	$<$ 33 weeks	$\geq$ 34 weeks	$<$ 34 weeks
Birth weight < 1,500	0.0096	0.0295	0.0145	0.0188	0.0197	0.0156	-0.0040	0.0196
	(0.0125)	(0.0345)	(0.0162)	(0.0163)	(0.0266)	(0.0121)	(0.0384)	(0.0119)
Mean	0.0308	0.0374	0.0392	0.0165	0.0514	0.0239	0.0615	0.0258
N	4,020	817	3,125	1,712	1,488	3,349	899	3,938

Notes: This tables shows regressions for alternative splits of the sample according to gestational age. Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Table 3.16: First Stage around 1,500 Grams of Birth Weight by Detailed Time Period

$\geq$ 32 weeks	1980-1986	1987-1993	1994-2000	2001-2004
Length of Hospital Stay (in days)				
Birth weight < 1,500	0.1272	-0.0119	-2.6661	-0.2263
	(2.5483)	(1.0294)	(1.5604)	(1.5380)
Mean	13.685	12.273	10.891	8.905
N	802	1,433	894	440
Length of Hospital Stay (in days, constructed)				
Birth weight < 1,500	-2.2165	2.4487	-2.0940	4.3910
	(1.9477)	(1.2789)	(1.4128)	(6.3010)
Mean	8.032	5.519	7.815	8.202
N	1,560	1,824	1,418	730

Notes: Bandwidth of 200 grams on either side of 1,500 grams is used. Controls are female, birth year, multiple birth and county of residence. We also include a dummy for 1,500 grams. We estimate local linear regressions with triangular weighting that allow for different trends on either side of the cutoff. Standard errors clustered at the grams level are given in parentheses. Only 32 and more weeks of gestational age. Mean refers to the estimated mean of the dependent variable just above the cutoff. * and ** denote significance at the 5 and 1 percent level, respectively.

Chapter 4

Economic Conditions, Parental Employment and Newborn Health[1]

4.1 Introduction

An expanding literature studies how up- and downturns of the economy affect the health of newborn children. For developing countries, there is now overwhelming evidence that recessions tend to increase infant mortality, while booms tend to lower it.[2] 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. Dehejia and Lleras-Muney (2004) use U.S. state-level data and estimate 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 why recessions might not be detrimental for babies' health in developed countries (see also the discussion in Ferreira and Schady (2009)): First, credit markets are more widespread, allowing mothers to smooth income and thus consumption on health care spending. Second, recessions are shorter and less deep and given the higher level of health spending, marginal reductions are less severe. Third, 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).

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2. 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, leading to higher income but fewer time-intensive health investments in child health due to increased labor supply, raise infant mortality in Colombia in coffee-growing regions.

Support for the health-enhancing effects of recessions on newborn babies comes from studies about economic fluctuations and health of the *general* population. Pioneering work by Ruhm (2000) and many subsequent studies provide strong evidence for the procyclicality of the total mortality rate.[3] Several of the channels linking the business cycle to adult health also apply to babies, both while still in utero and after birth. First, mothers might lose their job or reduce working hours during recessions. As a consequence, their opportunity cost of time decreases. They become more likely to engage in time-intensive activities that benefit babies' health, such as prenatal care, physical exercise or breast-feeding (Miller and Urdinola 2010). Their exposure to hazardous working conditions and job-related stress also decreases. There is plenty of evidence that stress affects birth outcomes, particular during the first trimester of pregnancy (Camacho 2008; Torche 2011; Mansour and Rees 2012; Bozzoli and Quintana-Domeque 2014; Foureaux Koppensteiner and Manacorda 2015). Second, job loss also lowers the available income that can be spent on tobacco and alcohol. Smoking and drinking are highly detrimental for newborn health if mothers do not abstain from them during pregnancy. It has been shown that these behaviors decrease during downturns (Ruhm 2000; Ruhm and Black 2002). Third, recessions are associated with less traffic and less air pollution. Air pollution has been shown to be an important determinant of infant mortality (Chay and Greenstone 2003; Currie and Walker 2011). Fourth, economic upturns are characterized by a shortage of medical staff, resulting in lower quality of (neonatal) care (Stevens et al. 2013).

While both theoretical considerations and empirical evidence suggest that recessions improve newborn health in developed countries, there is only little evidence. One complication in estimating the effect of the cycle is that women who decide to give birth in a recession might systematically differ from those who decide to give birth in a boom. Dehejia and Lleras-Muney (2004) argue that low-educated women – who do not suffer from skill depreciation – prefer to become pregnant 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 mother would also prefer to give birth in recessions, but cannot afford to do so since credit constraints prevent them from smoothing income over time. Also Salvanes (2013) finds that low-educated mothers are overrepresented in recessions, while Bhalotra (2010) and Aparicio and González (2014) detect the opposite pattern. Selection into pregnancy is likely to also occur along other, potentially unobserved dimensions. Since maternal characteristics such as education and marital status are known to be correlated with newborn health, it is essential to control for changes in the composition of mothers when estimating the effect of the business cycle.

One way to address compositional changes is to compare babies born to the same

3. Gerdtham and Ruhm (2006) show that this relationship also holds in a panel of 23 OECD countries. Ruhm (2013) however reports that it has become instable in the United States in recent years.

mother at different stages of the business cycle. Econometrically, this may be achieved by including mother fixed effects in the regression equation, which requires individual-level data. However, when Dehejia and Lleras-Muney (2004) use a Californian subsample of mothers who had at least two births and control for mother fixed effects, the effects on newborn health practically disappear. Other studies of developed countries employing fixed-effects identification strategies fail to establish a significant relationship with the cycle as well (Salvanes (2013) for Norway and Aparicio and González (2014) for Spain).[4] In contrast, most studies of developing countries find their results unaltered when accounting for selection bias (Paxson and Schady 2005; Bhalotra 2010; Baird et al. 2011), probably because in these countries the income shocks associated with recessions are so severe that any compositional changes become negligible. In sum, whether recessions actually improve newborn health in developed countries remains an open question.

This paper makes two contributions to the existing literature. First, it utilizes data from Sweden from 1992 to 2004 to address the question whether downturns improve newborn health in developed countries. For this purpose, we match micro-level information about newborn health with local-labor-market-level unemployment rates, which will serve as our indicator of the business cycle. Exploiting geographical variation in unemployment within Sweden, we can control for national-level trends in other variables that might generate a spurious correlation between unemployment and newborn health. Moreover, we use knowledge about the mother and father to compare health outcomes of babies born to the same parents. In that way, we control for the possibility that parents select into pregnancy depending on the state of the business cycle. We find that an increase in the unemployment rate by one percentage point reduces the incidence of neonatal mortality and very low birth weight by about 6–11 percent. The effect is entirely driven by the unemployment rate of men. We also find evidence for selective fertility based on the cycle, which underlines the importance of controlling for parents fixed effects.

Second, having demonstrated the effect of the cycle on newborn health, we illuminate the mechanisms underlying this relationship. Thanks to rich socioeconomic and demographic information about the parents, we can address this issue from several angles. First of all, we show that father's or mother's unemployment, which are more prevalent in times of recessions, cannot account for positive effects on newborn health. This means that more health-enhancing activities – due to lower opportunity cost of time – or reduced smoking and drinking – due to lower available income – are unlikely drivers of the relationship.

As a next step, we investigate whether infants are affected differently depending on the social status of the parents. We provide suggestive evidence that health is more responsive to recessions in infants of low-educated and low-income parents. This finding is consistent with reduced stress and air pollution improving newborn health in recessions if these variables

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

disproportionately affect low-status families. Supporting this explanation, we also find that recessions decrease the occurrence of premature birth, which has been shown to be affected by air pollution and stress in earlier studies.

Our findings are important since they show that economic conditions may harm newborn health and put infant survival at risk. Even for those infants that survive, newborn health has long-run impacts on both socioeconomic status and health in adulthood (see Currie 2009, for an overview). If particularly infants of low-status parents are affected by economic conditions, then the business cycle can help explain the widely observed income gradient in child health (e.g. Case et al. 2002).

This paper is structured as follows: Section 4.2 provides background information about the interaction of the business cycle with health in Sweden. Sections 4.3 and 4.4 explain the data and econometric method, respectively. Section 4.5 presents the results, starting with an analysis of selective fertility. We then report baseline effects on newborn health, followed by an investigation of different types of unemployment and potential mechanisms. Section 4.6 concludes.

4.2 The Business Cycle and Health in Sweden

There are several features that distinguish Sweden from other developed countries and that tend to make recessions health-improving. First, Sweden has a large welfare state. Everybody has access to the tax-funded public health care sector, with private health insurance and patient copayments only playing a minor role. Moreover, income inequality is fairly low and consumer credit widely available. Overall, Swedish mothers are unlikely to cut back on medical care use and other healthy goods in economically depressing times. Second, female labor force participation is relatively high in Sweden. Recessions are more likely to reduce the opportunity cost of time for women in Sweden than in other countries. As a consequence, Swedish mothers' time spent on healthy activities might respond more to economic fluctuations. Third, 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 2015). We therefore suspect that fluctuations in the quality of prenatal care over the cycle are rather limited.

However, there are also factors suggesting that recessions are not particularly welfare-enhancing in Sweden. First, some parts of Sweden are relatively sparsely populated. In recessions, traffic congestion and resulting pollution might not change dramatically in comparison with booms. Second, since overtime work is regulated through collective bargaining agreements, the stress caused by overtime hours in booms is limited. Third, while recessions are moderate in most developed countries, Sweden experienced a severe downturn in the early 1990s, with GDP per capita dropping by one quarter in 1993. We control for national-level fluctuations with our econometric approach. However, some regions in Sweden might have experienced economic slumps so severe that some mothers could not buffer their baby's health against the associated income loss. In sum, while there are good reasons to believe that recessions are beneficial for newborn health in Sweden, only an empirical analysis can provide a definite answer.

This paper is not the first to explore the relationship between unemployment and health in Sweden. The two studies most closely related to ours are Svensson (2007, 2010). They use county-level variation in unemployment and estimate the effect on total and cause-specific mortality rates in the *general* population. No robust association is found for total mortality. But mortality due to some types of accidents is found to be procyclical and mortality due to heart disease is found to be countercyclical. Other papers employ only national-level indicators of the business cycle, which are potentially subject to omitted variable bias: Gerdtham and Johannesson (2005) find countercyclical variation in mortality for males. In contrast, Tapia Granados and Ionides (2008, 2011) and Svensson and Krüger (2012) all detect procyclical patterns of mortality in the second half of the 20th century. These three papers also find evidence for the procyclicality of infant mortality, but in addition to using only national-level indicators of the cycle, they fail to control for selection into pregnancy. One paper concerned with birth weight is van den Berg and Modin (2013). They study the Swedish city of Uppsala and find no relationship between the business cycle and birth weight. However, they only look at cohorts born between 1915 and 1929, an era in which Sweden was not yet a developed economy in today's sense of the word.

4.3 Data

For the purposes of our study, we construct a dataset from two sources: First, monthly unemployment data at the municipality level and second, population-wide administrative data on newborn infants and parental characteristics at the individual level.

4.3.1 Unemployment Data from the HÄNDEL Register

The unemployment data come from the so-called HÄNDEL register created by Swedish public employment offices.[5] 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 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 labor force surveys are also registered as unemployed.

5. We are grateful to Linus Lindqvist of the IFAU (Institute for Evaluation of Labour Market and Education Policy) in Uppsala for his invaluable recurrent help with accessing and understanding the data. These data have been previously used by Richardson and van den Berg (2013) and Carling et al. (2001).

The HÄNDEL register is available to us from January 1992 onwards and contains the number of unemployed individuals by month and municipality and additionally 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. We divide by the corresponding number of individuals in the population to obtain the unemployment-to-population ratio, to which we simply refer as "unemployment rate" in the following. Unfortunately, we do not observe the size of the labor force, which would enable us to compute the actual unemployment rate.

If the labor 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 is free and might find it optimal for job search to register with an employment office in a different municipality. To capture spill-overs from surrounding areas, we aggregate municipality-level unemployment rates to the local labor market level. This approach also alleviates concerns about measurement error in municipality-level unemployment.[6]

We use the definition of local labor 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).[7] 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).

The benefits of aggregation to local labor markets must be weighed against the reduced power due to ignoring the idiosyncratic variation of unemployment at the municipality level. We therefore explore the sensitivity of our results to various degrees of aggregation in our 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 months following conception. We also study the impact of lags and leads of unemployment, which we define as the nine-months-periods before and after pregnancy, respectively.

The upper part of Figure 4.1 illustrates the evolution of the pregnancy-averaged unemployment rate for six randomly selected local labor markets between 1992 and 2004. Reflecting 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 up to 30 percent. Unem-

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

7. 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).

ployment then sinks to a low around 2001/02 and subsequently rises again. In the empirical analysis, we will use a detrended version of the unemployment rate that takes out permanent differences across local labor markets, year-specific national shocks and seasonal variation. The detrended time series is shown in the lower part of Figure 4.1. Since we are interested in the effects of unemployment as an indicator for fluctuations, it is noteworthy that the residual variation in unemployment after detrending is still fairly large. For some local labor 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-labor-market-specific time trends in the results section.

4.3.2 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 labor 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.[8]

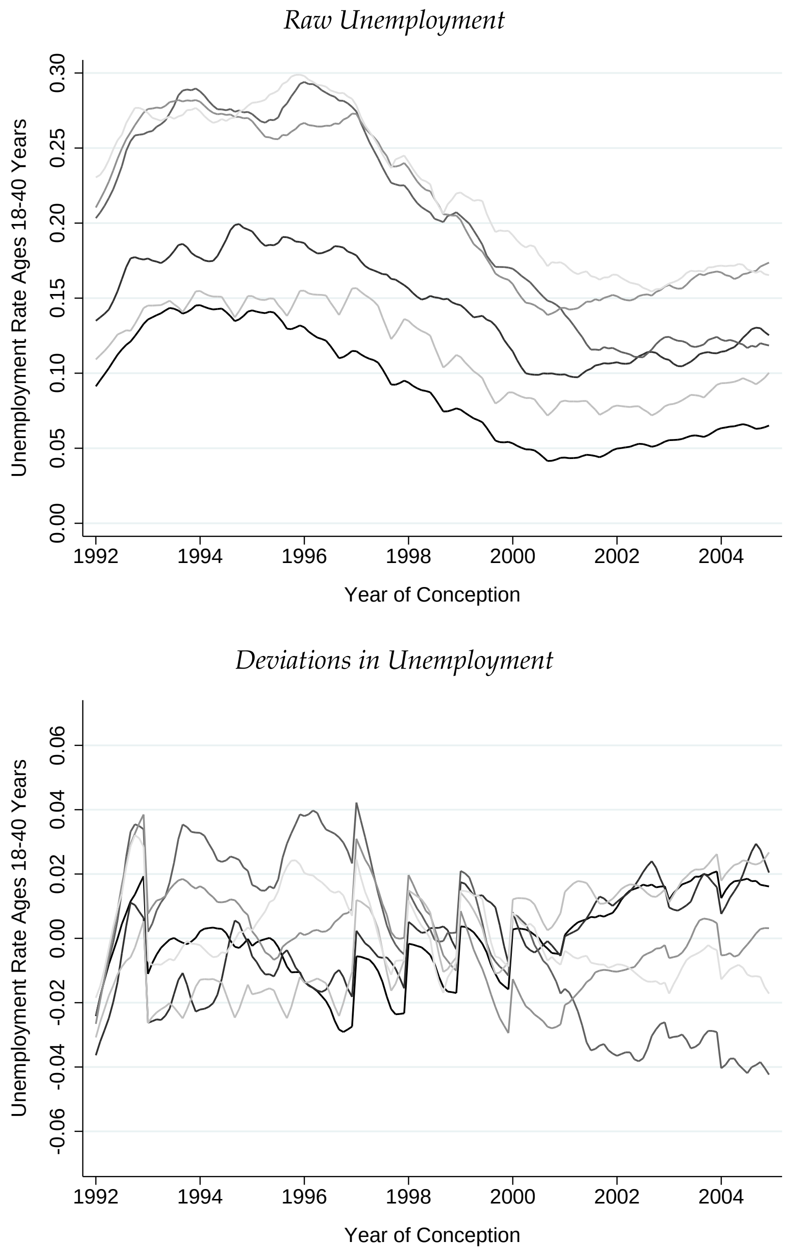
The Medical Birth register also contains data on birth weight, Apgar scores [9] and neonatal mortality, i.e. whether a newborn infant died within 28 days after birth. For infant mortality, i.e. deaths within a year of birth, we add information from the Cause of Death register, which includes deaths up 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 labor 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 socioeconomic 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

8. 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.

9. The Apgar score is a summary measure for the health of newborn infants. It ranges between 0 and 10, with higher values indicating better health. It is taken 1, 5 and 10 minutes after birth.

Figure 4.1: Unemployment Rate (18–40 Years) by Year for Subset of Local Labor Markets



Notes: Monthly unemployment rates (18–40 years) for six randomly selected local labor markets. Deviations in unemployment are after detrending the unemployment rate by taking out permanent differences across local labor markets, year-specific national shocks and seasonal variation.

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.[10] Finally, to determine the birth order of a newborn infant, we count the number of children

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

that the mother has given birth to in the past.

4.3.3 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 2005 or earlier, as dictated by the availability of unemployment data and father information (see previous section). We apply a number of restrictions to obtain the final sample: First, we disregard all parents from 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.[11] 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 exclude extremely light newborn babies weighing less than 500 grams who have very a low chance of survival. Third, 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 labor market conditions during pregnancy are identical for multiples, within-multiples comparisons are not informative for the relationship between unemployment and newborn health outcomes. Finally, we limit attention to mothers who were aged between 18 and 49 at the time of conception because the drivers of pregnancy are likely different for mothers outside this age interval.

After excluding infants whose father is still unknown (to us), which applies to about 6 percent of births, we are left with 874,503 babies conceived between 1992 and early 2004. They are born to 590,503 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 percent have them with two or more different partners. The corresponding number for men is a little smaller (12.3 percent), 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 245,008 parent pairs in the sample that fulfill this criterion (529,008 births). In the empirical analysis, we will cluster standard errors at the level of the local labor market that parents reside in at the time of birth. We therefore focus on parent pairs that have several births in exactly one local labor market (235,554 parent pairs). There are parent pairs that have several babies in multiple local labor markets, but rather than selecting a random local labor market, we choose to disregard these parent pairs. The parent pairs that we keep might have additional isolated births in a different local labor market, but then we exclude these observations from the analysis. Our final regression sample consists of 506,501 birth records. Table 4.1 provides descriptive statistics for both the whole sample

11. 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).

Table 4.1: Summary Statistics by Sample

	Whole Sample			Regression Sample		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Neonatal Mortality	0.0016	0.0405	874,503	0.0022	0.0468	506,501
Infant Mortality	0.0027	0.0516	874,503	0.0035	0.0594	506,501
Weight (in Grams)	3,584.0170	563.4740	871,460	3,605.6292	551.3155	504,682
Weight < 1,500 Grams	0.0051	0.0710	871,460	0.0042	0.0647	504,682
Gestational Age (in Days)	278.9491	12.6360	874,381	279.0924	12.1686	506,437
Gestational Age < 37 Weeks	0.0494	0.2166	874,381	0.0451	0.2075	506,437
Gestational Age < 32 Weeks	0.0064	0.0794	874,381	0.0054	0.0730	506,437
Small for Gestational Age	0.0210	0.1435	870,289	0.0180	0.1329	504,001
Unemployment 18-40 Years - Month	0.1380	0.0534	874,503	0.1388	0.0529	506,501
Unemployment 18-40 Years - Pregnancy	0.1377	0.0528	874,503	0.1371	0.0521	506,501
Unemployment 18-64 Years - Pregnancy	0.1087	0.0346	874,503	0.1093	0.0345	506,501
Unemployment Men 18-40 Years - Pregnancy	0.1444	0.0586	874,503	0.1424	0.0573	506,501
Unemployment Women 18-40 Years - Pregnancy	0.1306	0.0486	874,503	0.1315	0.0486	506,501
Birth Order						
1	0.4194	0.4935	874,503	0.3795	0.4853	506,501
2	0.3739	0.4838	874,503	0.4349	0.4957	506,501
3	0.1467	0.3538	874,503	0.1311	0.3375	506,501
4	0.0412	0.1987	874,503	0.0359	0.1861	506,501
Mother's Age						
Below 25 Years	0.1981	0.3985	874,503	0.2018	0.4013	506,501
25-35 Years	0.7120	0.4528	874,503	0.7308	0.4436	506,501
Above 35 Years	0.0900	0.2861	874,503	0.0675	0.2508	506,501
Mother's Marital Status						
Single	0.5763	0.4941	873,761	0.5685	0.4953	506,307
Married	0.3875	0.4872	873,761	0.4097	0.4918	506,307
Divorced	0.0362	0.1869	873,761	0.0218	0.1461	506,307

Continued on next page

Mother's Education						
Primary and Lower Secondary	0.0684	0.2524	850,572	0.0559	0.2297	500,113
Secondary Education and Vocational	0.5673	0.4955	850,572	0.5682	0.4953	500,113
Graduate and Postgraduate	0.3643	0.4812	850,572	0.3759	0.4844	500,113
Mother's Country of Birth						
Sweden	0.9599	0.1962	874,496	0.9666	0.1798	506,499
Developed Countries	0.0211	0.1436	874,496	0.0173	0.1303	506,499
Developing Countries	0.0190	0.1366	874,496	0.0162	0.1261	506,499
Father's Age						
Below 25 Years	0.1017	0.3022	874,503	0.0979	0.2972	506,501
25-35 Years	0.6952	0.4603	874,503	0.7274	0.4453	506,501
Above 35 Years	0.2031	0.4023	874,503	0.1747	0.3797	506,501
Father's Marital Status						
Single	0.5721	0.4948	872,630	0.5612	0.4962	505,718
Married	0.3893	0.4876	872,630	0.4108	0.4920	505,718
Divorced	0.0385	0.1925	872,630	0.0280	0.1648	505,718
Father's Education						
Primary and Lower Secondary	0.1165	0.3209	860,740	0.1024	0.3032	503,044
Secondary Education and Vocational	0.5698	0.4951	860,740	0.5720	0.4948	503,044
Graduate and Postgraduate	0.3136	0.4640	860,740	0.3256	0.4686	503,044
Father's Country of Birth						
Sweden	0.9572	0.2025	874,442	0.9626	0.1898	506,487
Developed Countries	0.0241	0.1535	874,442	0.0214	0.1448	506,487
Developing Countries	0.0187	0.1355	874,442	0.0160	0.1254	506,487
Father's Unemployment: No Wage	0.0818	0.2740	873,097	0.0710	0.2568	505,895
Mother's Unemployment: No Wage	0.1041	0.3054	874,485	0.1014	0.3018	506,498
Father's Unemployment: No Reimbursements	0.0540	0.2259	801,967	0.0431	0.2031	475,021
Mother's Unemployment: No Reimbursements	0.0953	0.2937	803,264	0.0912	0.2879	475,590

Notes: Summary statistics for selected variables by sample. The regression sample focuses on parents that have several births in exactly one local labor market, see Section 4.3.3. 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 Denmark, Finland, Norway, other EU-15, North America and Oceania. Developing countries include other 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.

and the final regression sample.

4.4 Econometric Specification

To estimate aggregate changes in birth rate and demographic composition of parents over the cycle, we follow the previous literature in specifying the following equation:

$$Y_{lt} = \alpha + \beta(\mathit{Unemployment\ Rate})_{lt} + \delta_t + \kappa_t + \lambda_l + \theta_l(\lambda_l \times t) + \varepsilon \tag{4.1}$$

where Y_{lt} is an outcome relating to all births conceived in month t by parents living in local labor 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. β captures the effect of unemployment on the outcome. δ_t are year fixed effects that capture countrywide fluctuations in unemployment in the year of conception. These are included to control for third factors that affect unemployment (such as labor market policies or long-run increases in educational attainment) and also correlate with newborn health outcomes. As a result, the identifying variation in unemployment stems from regionally divergent economic conditions. κ_t are month-of-year fixed effects that address concerns about seasonal variation in newborn health by controlling for monthly variation in unemployment within a year. λ_l are local-labor-market fixed effects that account for constant and persistent differences in unemployment across local labor markets, as seen in Figure 4.1. In some specifications, we also allow for local-labor-market-specific linear time trends. These may help reduce omitted variable bias even further, but come at the cost of increasing estimation uncertainty.

Given that local labor markets vary considerably with respect to population size and a few small ones do not encounter a single birth in some months, we use the number of 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 labor market.

The main purpose of this paper is to study the impact of unemployment on newborn health, while adjusting for potential shifts in the demographic composition of parents. We therefore slightly modify equation 4.1 to include parents fixed effects:

$$Y_{it} = \alpha + \beta(\mathit{Unemployment\ Rate})_{lt} + \delta_t + \kappa_t + \rho_i + \theta_l(\lambda_l \times t) + X_i'\gamma + \varepsilon \tag{4.2}$$

Here, i refers to a pair of parents consisting of mother and father. Y_{it} is some health outcome such as a dummy for very low birth weight ($<$ 1,500 grams) or neonatal mortality (death within 28 days of birth.).[12] By including parents fixed effects ρ_i, we essentially compare babies born to the same parents and thus account for selective fertility over the cycle. Note that ρ_i also absorb local-labor-market fixed effects since – by construction of the sample – all births belonging to the same parents were conceived in the same local labor market (see Section 4.3.1). In the sensitivity analysis, we also experiment with controlling for additional parent-level characteristics, such as marital status and birth order (X_i). We once again cluster standard errors at the level of the local labor market.

4.5 Results

4.5.1 Selection

Before studying how economic conditions impact newborn health outcomes, we first investigate how the demographic composition of parents changes with the cycle. This exercise yields useful insights about variables that potentially confound our estimates of health outcomes. By showing how different demographic groups shift pregnancy based on the cycle, it also sheds light on the determinants of fertility decisions, which are of independent interest.[13]

Table 4.2: Effect of Unemployment in Month of Conception on Birth Rate

	Mother		Father	
	Baseline	With Trends	Baseline	With Trends
Overall	-1.1840	-1.3058**		
	(0.8038)	(0.6183)		
% Change	-0.32%	-0.35%		
Birth Order 1	0.1142	0.0268		
	(0.5979)	(0.5109)		
% Change	0.07%	0.02%		
Birth Order 2	-0.5273*	-0.4558*		
	(0.3013)	(0.2573)		
% Change	-0.36%	-0.31%		
Birth Order 3	-0.4096**	-0.5216***		
	(0.1591)	(0.1711)		
% Change	-0.56%	-0.72%		
Birth Order 4	-0.5459***	-0.5156***		

Continued on next page

12. Note that we follow the previous literature by specifying linear probability models rather than binary choice models such as logit or probit.

13. 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). Note that the the former is probably more prevalent in developing countries.

	(0.1313)	(0.1269)		
% Change	-1.8%	-1.7%		
Age - Below 25 Years	-0.6429**	-0.8813***	-0.4988**	-0.4770***
	(0.2556)	(0.2459)	(0.2135)	(0.1760)
% Change	-0.64%	-0.88%	-0.89%	-0.86%
Age - 25-35 Years	-0.7516	-0.4658	-1.0096*	-1.0543**
	(0.6232)	(0.5366)	(0.5661)	(0.4747)
% Change	-0.29%	-0.18%	-0.39%	-0.4%
Age - Above 35 Years	0.2268	0.2249	0.1713	0.1845
	(0.2511)	(0.1967)	(0.3184)	(0.2919)
% Change	0.54%	0.54%	0.2%	0.22%
Marital Status - Single	-0.5310	-0.3514	-0.5000	-0.4092
	(0.4779)	(0.5008)	(0.4884)	(0.4980)
% Change	-0.22%	-0.15%	-0.21%	-0.17%
Marital Status - Married	-0.9286**	-1.2516***	-0.9029**	-1.1794***
	(0.3839)	(0.2681)	(0.3822)	(0.2728)
% Change	-0.69%	-0.93%	-0.67%	-0.87%
Marital Status - Divorced	-0.0349	0.0595	-0.0197	0.0786
	(0.1138)	(0.1214)	(0.0957)	(0.1000)
% Change	-0.15%	0.25%	-0.09%	0.35%
Education - Primary and Lower Secondary	-0.3833***	-0.4158***	-0.2105	-0.3511
	(0.1358)	(0.1553)	(0.2426)	(0.2137)
% Change	-1.07%	-1.16%	-0.37%	-0.61%
Education - Secondary Education and Vocational	-1.0643**	-1.0719***	-1.5819***	-1.4804***
	(0.4757)	(0.3710)	(0.4599)	(0.3111)
% Change	-0.45%	-0.46%	-0.63%	-0.59%
Education - Graduate and Postgraduate	0.6045	0.5685*	0.6641	0.5720
	(0.6127)	(0.3411)	(0.6177)	(0.3717)
% Change	0.51%	0.48%	0.73%	0.63%
Country of Birth - Sweden	-1.2615	-1.4216**	-1.3251	-1.3902**
	(0.8400)	(0.5891)	(0.8463)	(0.5601)
% Change	-0.35%	-0.39%	-0.36%	-0.38%
Country of Birth - Developing Countries	-0.0829	0.0190	-0.1165	-0.0677
	(0.0957)	(0.0821)	(0.0973)	(0.0472)
% Change	-0.79%	0.18%	-1.24%	-0.72%
Country of Birth - Developed Countries	-0.0255	-0.0949	0.0021	-0.0102
	(0.1809)	(0.1524)	(0.1409)	(0.1174)
% Change	-0.14%	-0.5%	0.01%	-0.06%

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Notes: OLS regressions of the birth rate on the unemployment rate in the age group 18-40 years in the month of conception. Birth rates are defined as the number of births with same month of conception in the given subgroup per 1,000 women aged 18-49 years in the population. Controls are year fixed effects, month-of-year fixed effects, local-labor-market fixed effects and local-labor-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

We start with estimating the effect of unemployment on the birth rate – defined as the number of births per 1,000 women aged 18–49 years in the population. Here we use the overall unemployment rate among individuals aged between 18 and 40 years in the month of conception.[14] Table 4.2 shows that higher unemployment tends to decrease the overall birth rate, although the coefficient is only significant in the specification with regional trends. A 10 percentage point increase in the unemployment rate implies a 3 percent fall in the birth rate. When we stratify the analysis by parental characteristics, we find that the negative impact on fertility is concentrated in second- or later-born children and parents that are young, married, low-educated and Swedish.

Effects on birth rate pose a threat to our identification strategy only if they vary so much by demographic characteristics that the demographic composition of parents changes significantly with the cycle. We investigate the effect on the composition of births more directly by regressing shares of demographic groups on unemployment. As Table 4.3 shows, not all of the changes in birth rates eventually result in changes in the composition. There are significant decreases in the share of children with birth order 3 and 4, the share of married parents and the share of low-educated mothers.

Table 4.3: Effect of Unemployment in Month of Conception on Composition of Birth Cohorts

	Mother		Father	
	Baseline	With Trends	Baseline	With Trends
Birth Order 1	0.0668	0.0542		
	(0.0632)	(0.0629)		
% Change	0.17%	0.14%		
Birth Order 2	-0.0012	0.0250		
	(0.0578)	(0.0589)		
% Change	-0.0%	0.07%		
Birth Order 3	-0.0318	-0.0575*		
	(0.0288)	(0.0314)		
% Change	-0.19%	-0.35%		
Birth Order 4	-0.0463***	-0.0447***		

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14. Tables 4.12 for the birth rate and 4.13 for compositional changes show that results are similar when using the average unemployment during pregnancy.

	(0.0172)	(0.0145)		
% Change	-0.86%	-0.83%		
Age - Below 25 Years	0.0129	-0.0284	0.0049	0.0009
	(0.0405)	(0.0425)	(0.0374)	(0.0335)
% Change	0.05%	-0.12%	0.04%	0.01%
Age - 25-35 Years	-0.0521	0.0015	-0.0486	-0.0466
	(0.0398)	(0.0396)	(0.0410)	(0.0437)
% Change	-0.08%	0.0%	-0.07%	-0.07%
Age - Above 35 Years	0.0391	0.0269	0.0437	0.0457
	(0.0303)	(0.0273)	(0.0347)	(0.0398)
% Change	0.46%	0.32%	0.22%	0.23%
Marital Status - Single	0.1505***	0.2117***	0.1584***	0.2075***
	(0.0432)	(0.0603)	(0.0468)	(0.0640)
% Change	0.24%	0.33%	0.25%	0.33%
Marital Status - Married	-0.1896***	-0.2526***	-0.1856***	-0.2370***
	(0.0465)	(0.0625)	(0.0464)	(0.0584)
% Change	-0.58%	-0.77%	-0.56%	-0.72%
Marital Status - Divorced	0.0391***	0.0409**	0.0272*	0.0295*
	(0.0141)	(0.0171)	(0.0143)	(0.0157)
% Change	1.0%	1.05%	0.74%	0.8%
Education - Primary and Lower Secondary	-0.0724**	-0.0816***	-0.0192	-0.0400
	(0.0326)	(0.0297)	(0.0369)	(0.0306)
% Change	-1.03%	-1.16%	-0.16%	-0.33%
Education - Secondary Education and Vocational	-0.0064	0.0600	-0.0287	0.0564*
	(0.0489)	(0.0454)	(0.0582)	(0.0306)
% Change	-0.01%	0.09%	-0.04%	0.08%
Education - Graduate and Postgraduate	0.0787*	0.0215	0.0479	-0.0165
	(0.0466)	(0.0428)	(0.0531)	(0.0330)
% Change	0.27%	0.07%	0.23%	-0.08%
Country of Birth - Sweden	0.0210	0.0139	-0.0013	0.0111
	(0.0278)	(0.0173)	(0.0306)	(0.0142)
% Change	0.02%	0.01%	-0.0%	0.01%
Country of Birth - Developing Countries	0.0016	0.0046	-0.0089	-0.0078
	(0.0153)	(0.0108)	(0.0175)	(0.0080)
% Change	0.18%	0.5%	-1.22%	-1.07%
Country of Birth - Developed Countries	-0.0227	-0.0185	0.0103	-0.0032
	(0.0176)	(0.0145)	(0.0171)	(0.0126)
% Change	-1.01%	-0.83%	0.45%	-0.14%

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Notes: OLS regressions of the share of infants with the same month of conception in a given subgroup on the unemployment rate in the age group 18-40 years in the month of conception. Controls are year fixed effects, month-of-year fixed effects, local-labor-market fixed effects and local-labor-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Our findings are consistent with previous studies on the compositional impact of the cycle, which yielded mixed results. In line with the work by Aparicio and González (2014) for Spain, we find low-educated mothers to be underrepresented in recessions, but Dehejia and Lleras-Muney (2004) and Salvanes (2013) detect the opposite pattern for the United States and Norway, respectively. Similar to Salvanes (2013), we observe that the share of married mothers decreases with higher unemployment, a finding contrary to the one by Aparicio and González (2014). Finally, in agreement with Salvanes (2013) and Aparicio and González (2014), we fail to detect a clear pattern in parental age, whereas Dehejia and Lleras-Muney (2004) find less young- and more medium-aged mothers in recessions.

If parental characteristics are correlated with newborn health, then compositional changes in birth cohorts caused by the business cycle entail changes in average newborn health outcomes. We explore the implications of selective fertility for average health using Table 4.14. Recalling from Table 4.1 that the average share of VLBW infants is 0.0051 and that of infants dying with 28 days within birth (neonatal mortality) is 0.0016, Table 4.14 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. A smaller fraction of low-educated mothers in recessions therefore – ceteris paribus – leads to better average health in the population. Similarly, babies born to married mothers tend to suffer from neonatal mortality significantly more often. As a consequence, a reduction in married mothers has positive implications for average health. Regarding birth order, second-born children are healthiest in terms of both measures, with health deteriorating for higher birth orders. A reduction in fourth-order children therefore involves an improvement in average health, especially for neonatal mortality. Overall, demographic groups selecting out of fertility in recessions are those with inferior health, potentially resulting in increases in average health.

As a result of selective fertility, our estimate of the effect of unemployment on newborn health might be biased. By including parents fixed effects in the regression, we therefore control for time-invariant parental characteristics, both observed – such as education – and unobserved. Parents fixed effects also partially absorb variables that are in principle time-varying. For example, if parents have their children while either married or unmarried and do not change marital status between births, then marital status is essentially time-invariant. Parents fixed effects therefore likely account for much of the compositional changes due to both time-invariant and time-varying characteristics.

Finally, note that the health effects induced by compositional changes are of negligible size. As an example, consider the shift from married mothers to single mothers by about 0.005 for a 1 percentage point increase in the unemployment rate. From Table 4.14, the excess neonatal mortality of married mothers relative to single mothers is 0.0006. Given an average neonatal mortality of 0.0014, this implies that a change in the unemployment rate of 1 percentage point will decrease neonatal mortality by only about 0.2 percent. Note that compositional shifts and differentials in health are even smaller for other variables.

4.5.2 Baseline Effects on Newborn Health

We next turn to the micro-level analysis of how unemployment affects newborn health. We estimate versions of equation 4.2, which controls for parents fixed effects to address selective fertility. The baseline results are presented in column 1 of Table 4.4.

Note that in addition to unemployment during pregnancy, defined as the average unemployment during the 9 months following conception, we also report results for values of unemployment in the 9-months-periods before and after pregnancy. The rationale for looking at lagged unemployment is that economic conditions might have a delayed effect on health. Stress, for example, which is a likely link between economic conditions and health, might need to accumulate before becoming harmful for health. The rationale for studying lead unemployment is that adjustments in employment often take time so that unemployment data follow data on economic activity with some delay.

There is a negative and in most cases significant effect of unemployment on both very low birth weight and neonatal mortality across all three periods of unemployment. While for very low birth weight the coefficient on unemployment *during* pregnancy is largest and most significant, unemployment *before* pregnancy is more relevant for neonatal mortality, suggesting that economic conditions have a delayed effect here.

The size of the effect is quite large. A one-percentage point (= 0.01) increase in the unemployment rate is associated with a 6–7 percent decrease in very low birth weight and a 6–11 percent decrease in 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 Section 4.5.1. Hence, our results cannot be driven by fluctuations in these variables.

We test the robustness of this estimate by allowing for labor-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 columns 4–6, we sequentially control for birth order, a third-order polynomial in

Table 4.4: Effects of Unemployment on Health with Sequentially Added Covariates

		LLM-specific Time Trends		Maternal Controls		
	Baseline	Linear Trends	Quadratic Trends	Birth Order	Age	Marital Status
Weight < 1,500 Grams	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment Before Pregnancy	-0.0281*	-0.0261	-0.0277	-0.0256*	-0.0252*	-0.0253*
	(0.0144)	(0.0160)	(0.0173)	(0.0145)	(0.0145)	(0.0145)
% Change	-6.88%	-6.38%	-6.77%	-6.26%	-6.16%	-6.19%
N	474,738	474,738	474,738	474,738	474,738	474,560
Unemployment During Pregnancy	-0.0303**	-0.0297**	-0.0265	-0.0270**	-0.0265**	-0.0266**
	(0.0132)	(0.0147)	(0.0161)	(0.0133)	(0.0134)	(0.0133)
% Change	-7.24%	-7.09%	-6.33%	-6.46%	-6.34%	-6.34%
N	503,275	503,275	503,275	503,275	503,275	503,081
Unemployment After Pregnancy	-0.0241*	-0.0204	-0.0190	-0.0205	-0.0193	-0.0193
	(0.0125)	(0.0150)	(0.0181)	(0.0125)	(0.0126)	(0.0126)
% Change	-5.76%	-4.86%	-4.54%	-4.89%	-4.61%	-4.6%
N	503,275	503,275	503,275	503,275	503,275	503,081
Neonatal Mortality	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment Before Pregnancy	-0.0229***	-0.0287***	-0.0252*	-0.0192**	-0.0185**	-0.0185**
	(0.0080)	(0.0100)	(0.0134)	(0.0084)	(0.0084)	(0.0084)
% Change	-10.99%	-13.76%	-12.08%	-9.21%	-8.85%	-8.86%
N	477,873	477,873	477,873	477,873	477,873	477,695
Unemployment During Pregnancy	-0.0131*	-0.0159*	-0.0109	-0.0078	-0.0069	-0.0064
	(0.0073)	(0.0085)	(0.0102)	(0.0073)	(0.0073)	(0.0073)
% Change	-5.94%	-7.22%	-4.95%	-3.55%	-3.15%	-2.92%
N	506,501	506,501	506,501	506,501	506,501	506,307
Unemployment After Pregnancy	-0.0024	-0.0031	0.0060	0.0015	0.0032	0.0038
	(0.0095)	(0.0109)	(0.0123)	(0.0096)	(0.0097)	(0.0097)
% Change	-1.1%	-1.4%	2.73%	0.68%	1.47%	1.72%
N	506,501	506,501	506,501	506,501	506,501	506,307

Notes: Unemployment refers to the unemployment rate among individuals in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before and after pregnancy are the average unemployment rates during the 9-months-period before and the period 10-18 months after conception, respectively. Percentage changes divide the unemployment effect by the mean level of the outcome in the regression sample. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

mother's age and mother's marital status. These variables might help reduce bias in the estimation, but are only imprecisely identified if simultaneously controlling for parents and year fixed effects eliminates most of their variation. Once again, coefficients change negligibly with the inclusion of these variables. We therefore do not include them in our preferred specification.

4.5.3 Health Effects by Type of Unemployment

4.5.3.1 Additional Leads and Lags

The previous section showed that not only unemployment during pregnancy, but also unemployment shortly before and after pregnancy are associated with enhanced health outcomes. A plausible explanation are delayed effects or simply serial correlation in the unemployment variable. At the same time, it would be worrying if newborn health outcomes were correlated also with unemployment in periods even further in the past or in the future. In column 1 of Table 4.5, we extend the analysis to unemployment 10–18 months before and after pregnancy. Recall that unemployment data are only available to us from 1992 onwards so that the number of observations decreases when we go back in time. It is encouraging to see that the estimates become smaller and insignificant as we move away from pregnancy, suggesting that our estimate actually captures the effect of the cycle.

4.5.3.2 Men and Women Unemployment

Table 4.5 also investigates whether men and women unemployment affect health outcomes differently. Columns 2 and 3 of Table 4.5 show that the effect of unemployment is entirely driven by men unemployment, for which coefficients are of similar size, but more precisely estimated. Men unemployment is typically more highly correlated with the business cycle than women 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 men unemployment is more strongly linked to GDP than women unemployment. We will return to this point in Section 4.5.5.1 and focus on men unemployment in the following.

4.5.3.3 Age Groups

In Table 4.6 we explore the effects for unemployment rates of different age groups. Including older individuals reduces measurement error in the unemployment rate because it is based on more observations. In addition, if the cycle causes fluctuations in unemployment that are idiosyncratic to older individuals, these will also be captured. However, variation in the unemployment rate is typically lower for the old and much of it due to factors unrelated

Table 4.5: Effect of Additional Leads and Lags of Unemployment by Gender

	Unemployment		
Weight < 1,500 Grams	Overall	Men	Women
Before Pregnancy: 10-18 Months	-0.0172	-0.0230*	-0.0037
	(0.0159)	(0.0140)	(0.0141)
N	448,148	448,148	448,148
Before Pregnancy: 9 Months	-0.0281*	-0.0285**	-0.0171
	(0.0144)	(0.0126)	(0.0131)
N	474,738	474,738	474,738
During Pregancy	-0.0303**	-0.0322***	-0.0178
	(0.0132)	(0.0123)	(0.0115)
N	503,275	503,275	503,275
After Pregnancy: 9 Months	-0.0241*	-0.0224**	-0.0170
	(0.0125)	(0.0113)	(0.0120)
N	503,275	503,275	503,275
After Pregnancy: 10-18 Months	-0.0243	-0.0160	-0.0239
	(0.0159)	(0.0131)	(0.0153)
N	503,275	503,275	503,275
Neonatal Mortality	Overall	Men	Women
Before Pregnancy: 10-18 Months	0.0001	-0.0004	0.0002
	(0.0111)	(0.0101)	(0.0094)
N	451,182	451,182	451,182
Before Pregnancy: 9 Months	-0.0229***	-0.0221***	-0.0154**
	(0.0080)	(0.0072)	(0.0073)
N	477,873	477,873	477,873
During Pregancy	-0.0131*	-0.0130**	-0.0085
	(0.0073)	(0.0065)	(0.0074)
N	506,501	506,501	506,501
After Pregnancy: 9 Months	-0.0024	-0.0050	0.0014
	(0.0095)	(0.0086)	(0.0091)
N	506,501	506,501	506,501
After Pregnancy: 10-18 Months	-0.0016	-0.0045	0.0021
	(0.0109)	(0.0094)	(0.0103)
N	506,501	506,501	506,501

Notes: Unemployment refers to the unemployment rate among the indicated gender in the age group 18-40 years. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

to economic conditions, such as policy changes in early retirement or disability benefits. Measurement error might therefore actually increase.

The size of the estimate rises as we include older individuals. While this might reflect

Table 4.6: Effect of Men Unemployment by Age Group

Weight < 1,500 Grams	18-24 Years	18-30 Years	18-40 Years	18-64 Years
Unemployment Before Pregnancy	-0.0170**	-0.0220**	-0.0285**	-0.0308
	(0.0081)	(0.0096)	(0.0126)	(0.0194)
N	474,738	474,738	474,738	474,738
Unemployment During Pregnancy	-0.0159**	-0.0254***	-0.0322***	-0.0383**
	(0.0081)	(0.0097)	(0.0123)	(0.0193)
N	503,275	503,275	503,275	503,275
Neonatal Mortality	18-24 Years	18-30 Years	18-40 Years	18-64 Years
Unemployment Before Pregnancy	-0.0132***	-0.0169***	-0.0221***	-0.0411***
	(0.0045)	(0.0057)	(0.0072)	(0.0109)
N	477,873	477,873	477,873	477,873
Unemployment During Pregnancy	-0.0079**	-0.0094*	-0.0130**	-0.0258***
	(0.0039)	(0.0050)	(0.0065)	(0.0090)
N	506,501	506,501	506,501	506,501

Notes: Unemployment refers to the unemployment rate among men in the indicated age group. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before pregnancy is the average unemployment rates during the 9-months-period before conception. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

smaller measurement error, it is very likely 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. The 18–64-years-unemployment measure produces the largest estimates, but also has very large standard errors for very low birth weight. It remains unclear from Table 4.6 which definition of unemployment is suited best for our purposes, but unemployment in the age group 18–40 years appears to be an appropriate choice.[15]

4.5.3.4 Regions

As discussed earlier, there is a trade-off when choosing the optimal degree of geographic aggregation of the unemployment rate. We have chosen to compute unemployment rates at the level of the local labor market, but alternative regional units are conceivable. In Table 4.7, we report results for the unemployment rate aggregated to the municipality and county level. Each of the 283 municipalities belongs to only one local labor market. In contrast, one local labor market might extend to several counties, although in total the number of local labor markets (72) is larger than the number of counties (21).[16]

15. See Table 4.15 for corresponding regressions for women unemployment.
16. More precisely, 9 local labor market extend to 2 counties and one local labor market to 3 counties.

Table 4.7: Effect of Men Unemployment by Region

Weight < 1,500 Grams	County	Local Labor Market	Municipality
Unemployment Before Pregnancy	-0.0164	-0.0285**	-0.0190**
	(0.0142)	(0.0126)	(0.0093)
N	476,342	474,738	436,111
Unemployment During Pregnancy	-0.0168	-0.0322***	-0.0223**
	(0.0124)	(0.0123)	(0.0091)
N	504,976	503,275	462,489
Neonatal Mortality	County	Local Labor Market	Municipality
Unemployment Before Pregnancy	-0.0269***	-0.0221***	-0.0131**
	(0.0100)	(0.0072)	(0.0056)
N	479,504	477,873	438,985
Unemployment During Pregnancy	-0.0178*	-0.0130**	-0.0100
	(0.0096)	(0.0065)	(0.0063)
N	508,232	506,501	465,450

Notes: Unemployment refers to the unemployment rate among men in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before pregnancy is the average unemployment rates during the 9-months-period before conception. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the level of the indicated region are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Table 4.7 shows that estimates at the municipality level are generally smaller than those at the local-labor-market level. This is in line with spill-over effects from surrounding areas that are ignored at the municipality level. Probably for the same reason, estimates are also larger at the county level, but only for neonatal mortality. They are smaller and insignificant for very low birth weight, possibly because countervailing variation in unemployment cancels out at more aggregated levels. Overall, the local labor market level appears to balance the up- and downsides of aggregation adequately.[17]

4.5.4 Effect on Other Health Outcomes

The previous section showed that recessions reduce the incidence of neonatal mortality, i.e. deaths within 28 days of birth, by 0.0221 (using the effect on men unemployment before pregnancy in Table 4.5). In Table 4.8, 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 (-0.0262) is about the same size as the coefficient on neonatal mortality. This demonstrates two points: First, recessions have no effect on deaths later than 28 days after birth, also shown by the insignificant estimate for postneonatal mortality. Second, and more importantly, deaths not happening within 28 days

17. See Table 4.16 for corresponding regressions for women unemployment.

Table 4.8: Effect of Men Unemployment on Other Health Outcomes

	Infant Mortality	Postneonatal Mortality	Weight (in Grams)	Log Weight
Unemployment Before Pregnancy	-0.0262***	-0.0041	-38.2635	0.0114
	(0.0100)	(0.0067)	(85.8226)	(0.0299)
% Change	-7.81%	-3.24%	-0.01%	0.0%
N	477,873	477,873	474,738	474,738
Unemployment During Pregnancy	-0.0123	0.0007	17.1919	0.0286
	(0.0099)	(0.0064)	(68.5005)	(0.0224)
% Change	-3.47%	0.55%	0.0%	0.0%
N	506,501	506,501	503,275	503,275
	Apgar Score (5 min) < 5	Small for Gestational Age	Gestational Age < 32 Weeks	Gestational Age < 37 Weeks
Unemployment Before Pregnancy	-0.0105	-0.0301	-0.0289**	0.0262
	(0.0127)	(0.0228)	(0.0147)	(0.0410)
% Change	-1.93%	-1.72%	-5.46%	0.59%
N	471,374	473,563	477,766	477,766
Unemployment During Pregnancy	0.0123	-0.0356	-0.0266*	0.0065
	(0.0105)	(0.0256)	(0.0138)	(0.0318)
% Change	2.28%	-1.98%	-4.96%	0.15%
N	499,556	502,055	506,390	506,390

Notes: Unemployment refers to the unemployment rate among men in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before pregnancy is the average unemployment rates during the 9-months-period before conception. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Percentage changes divide the unemployment effect by the mean level of the outcome in the regression sample. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

of birth are not deferred to a later point in time within the first year. As a result, lives are actually saved. To give precise numbers, note that about 100 infants die within 28 days after birth in Sweden each year. Thus, a one-percentage point increase in unemployment, which leads to a 10 percent decrease in neonatal mortality, will save about 10 infants per year.

Table 4.8 also explores the effects of unemployment on absolute birth weight, log 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”). 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 in the next section, which examines mechanisms.

4.5.5 Mechanisms

The previous sections established a positive relationship between economic downturns and newborn health. Although the channels linking downturns to improvements in newborn health are manifold, one can distinguish two main categories. The first category includes channels that are related to parental employment status, which is affected by downturns through job loss and lower chances of re-employment. The second category includes all channels unrelated to parental employment status.

Among channels in the first category is an increase in health-enhancing time-consuming activities, such as prenatal care, due to the mother's job loss. Another channel are income reductions due to parental job loss – no matter whether the mother or father lost the job – that lead to lower consumption of tobacco and alcohol during pregnancy. The second category includes all other channels such as reduced stress thanks to lower workload, less traffic and air pollution as well as higher availability of prenatal care. For each of the two categories, we now evaluate whether it can rationalize the above findings, starting with the first category, which includes channels related to parental employment status.

4.5.5.1 Parental Unemployment

Recall from Section 4.5.3.2 that the effect of the cycle on newborn health was entirely driven by then men unemployment rate, with the women unemployment rate being virtually uncorrelated with newborn health. At the same time, while uncorrelated with newborn health, women unemployment is a strong indicator of the mother's employment status. Table 4.17 in the appendix presents regressions of two binary unemployment indicators on men and women unemployment separately. The first indicator ("No Wage") takes on the value one if a gross wage of zero is reported in the statement of income submitted to the tax agency. 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. However, it is not available to us in the year 2003.

Irrespective of the indicator used, women unemployment is a much better predictor of mother's unemployment than men unemployment. With women unemployment strongly correlated with 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. The last two columns of Table 4.17 also show that women unemployment decreases log family earnings more than men unemployment. It follows that income reductions – and associated decreases in the consumption of detrimental goods – do not qualify as a likely channel either. The combination of women unemployment affecting health only little and men unemployment being more tightly linked to the business cycle (see Section 4.5.3.2) suggests that the cycle

Table 4.9: Effect of Parental Unemployment ("No Wage")

	Baseline	Mother		Father	
Weight < 1,500 Grams	(1)	(2)	(3)	(4)	(5)
Unemployment During Pregnancy	-0.0322***	-0.0321***	-0.0318***	-0.0315**	-0.0312**
	(0.0123)	(0.0124)	(0.0121)	(0.0122)	(0.0123)
No Wage		-0.0006	-0.0001	-0.0017***	-0.0013
		(0.0005)	(0.0013)	(0.0006)	(0.0011)
No Wage × Unemployment			-0.0030		-0.0028
			(0.0087)		(0.0071)
N	503,275	503,272	503,272	502,675	502,675
	Baseline	Mother		Father	
Neonatal Mortality	(1)	(2)	(3)	(4)	(5)
Unemployment During Pregnancy	-0.0130**	-0.0127*	-0.0135**	-0.0126*	-0.0129**
	(0.0065)	(0.0065)	(0.0064)	(0.0065)	(0.0066)
No Wage		0.0002	-0.0008	-0.0005	-0.0010
		(0.0005)	(0.0009)	(0.0005)	(0.0013)
No Wage × Unemployment			0.0066		0.0032
			(0.0053)		(0.0084)
N	506,501	506,498	506,498	505,895	505,895

Notes: Unemployment refers to the unemployment rate among men in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate 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. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

operates through channels more general than individual unemployment.

Table 4.9 presents 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. Even though mother's unemployment is highly endogenous to pregnancy and its coefficient must be treated with caution, we can gain useful insights from studying whether its inclusion changes the effect of the unemployment rate. 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 unemployment and yield comparable results. Table 4.18 in the appendix reports the same set of regressions for the "No Reimbursements" indicator of parental unemployment, with results being essentially unaltered.

Table 4.10: Effect of First Differences of Unemployment

Weight < 1,500 Grams	Overall	Men	Women
Unemployment Before Pregnancy	-0.0146	-0.0044	-0.0217
	(0.0199)	(0.0187)	(0.0192)
N	448,148	448,148	448,148
Unemployment During Pregnancy	-0.0074	-0.0027	-0.0100
	(0.0137)	(0.0125)	(0.0134)
N	448,148	448,148	448,148
Unemployment After Pregnancy	0.0250	0.0289	0.0091
	(0.0203)	(0.0177)	(0.0186)
N	503,275	503,275	503,275
Neonatal Mortality	Overall	Men	Women
Unemployment Before Pregnancy	-0.0188	-0.0165	-0.0124
	(0.0178)	(0.0158)	(0.0163)
N	451,182	451,182	451,182
Unemployment During Pregnancy	-0.0051	-0.0044	-0.0032
	(0.0118)	(0.0108)	(0.0104)
N	451,182	451,182	451,182
Unemployment After Pregnancy	0.0232*	0.0176*	0.0215
	(0.0122)	(0.0105)	(0.0131)
N	506,501	506,501	506,501

Notes: Unemployment refers to the unemployment rate among the indicated gender in the age group 18-40 years. Unemployment during pregnancy is the first-differenced average unemployment rate in the nine months following conception. Unemployment before and after pregnancy are the first-differenced average unemployment rates during the 9-months-period before and the period 10-18 months after conception, respectively. In each case, first-differencing means subtracting the average unemployment rate from the previous nine-months-period. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Finally, 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 4.10 shows the corresponding estimates for first differences in overall, men and women unemployment. There is no significant effect of first differences in unemployment on newborn health. In line with Lindo (2011), who finds that father's job loss actually reduces birth weight, we conclude that parental job loss captured by first differences does not explain the positive effects of unemployment on newborn health.

Overall, we find that parental employment status cannot account for the beneficial health effects of recessions. Channels unrelated to parental employment are more likely to be relevant and will be explored next.

4.5.5.2 Heterogeneity

Among the most likely alternative mechanisms linking downturns to newborn health are reduced stress, less traffic and air pollution as well as higher availability of prenatal care. If reduced stress and less air pollution improve newborn health in recessions, then we might see stronger effects for low-status parents. For example, if there is job-related stress due to fluctuations in the workload, this will particularly affect low-educated individuals who are disproportionately employed in sectors sensitive to the business cycle, such as manufacturing or simple services. In addition, low-status individuals tend to live in neighborhoods with higher levels of pollution. In contrast, the quality of prenatal care presumably varies only little over the cycle due to Sweden's public health care system (see also Section 4.2). But even if it did vary, note that financial barriers to prenatal care are virtually absent, so that there is no reason to expect low-status families to be affected more strongly by the cycle.

The upper part of Table 4.11 explores whether the effect of recessions on very low birth weight varies by socioeconomic status of the parents, by marital status or by the gender of the child. Regarding mortality in the general population, Haaland and Telle (2015) find no evidence that the effect of the cycle would depend on socioeconomic status. The first column of Table 4.11 allows for differential effects of unemployment for fathers with different levels of educational attainment. The coefficient in the first row gives the effect on fathers who only have primary or secondary education, which is the reference category in this regression. The estimate of -0.0447 is much larger than our baseline estimate of -0.0322 from Table 4.9. The other coefficient in the same column (just below) belongs 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 very similar for mother's education, although the interaction term lacks significance here. Note that differences in absolute effects are not driven by differences in levels, as becomes clear when comparing the corresponding percentage changes. In sum, the effect of unemployment on very low birth weight seems entirely driven by low-educated parents.

In column 3, we also study effects by family income, which is another indicator for socioeconomic status. This indicator ranks given parents in the distribution of family income of all parents with a baby conceived in the same year.[18] 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

18. 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 4.11: Heterogeneity of Unemployment Effect by Subgroup

	Education				
	Father	Mother	Family Income	Marital Status	Gender
Weight < 1,500 Grams	(1)	(2)	(3)	(4)	(5)
Unemployment During Pregnancy	-0.0447***	-0.0416***	-0.0334	-0.0302**	-0.0320***
	(0.0145)	(0.0153)	(0.0214)	(0.0123)	(0.0122)
Graduate and Postgraduate	0.0489**				
	(0.0239)				
Graduate and Postgraduate		0.0297			
		(0.0243)			
Top 25%			0.0081		
			(0.0174)		
Married				-0.0084	
				(0.0097)	
Girl					-0.0000
					(0.0053)
Mean - Reference	0.0043	0.0045	0.0043	0.0041	0.0042
Mean - Interaction	0.0038	0.0037	0.0041	0.0041	0.0041
% - Reference	-10.3%	-9.3%	-7.76%	-7.34%	-7.54%
% - Interaction	1.09%	-3.24%	-6.23%	-9.46%	-7.75%
N	503,275	503,275	216,424	492,121	503,275
Neonatal Mortality	(1)	(2)	(3)	(4)	(5)
Unemployment Before Pregnancy	-0.0232**	-0.0205**	-0.0234	-0.0170**	-0.0252***
	(0.0096)	(0.0093)	(0.0211)	(0.0080)	(0.0078)
Graduate and Postgraduate	0.0019				
	(0.0265)				
Graduate and Postgraduate		-0.0054			
		(0.0233)			
Top 25%			0.0263*		
			(0.0150)		
Married				-0.0154*	
				(0.0088)	
Girl					0.0059
					(0.0048)
Mean - Reference	0.0021	0.0022	0.0021	0.0018	0.0024
Mean - Interaction	0.0020	0.0020	0.0022	0.0024	0.0018
% - Reference	-10.99%	-9.5%	-11.01%	-9.42%	-10.6%
% - Interaction	-10.56%	-13.25%	1.33%	-13.74%	-10.82%
N	477,873	477,873	204,501	467,198	477,873

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 men in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before pregnancy is the average unemployment rates during the 9-months-period before conception. Controls are parents fixed effects as well as subgroup-specific year fixed effects and month-of-year fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

married mothers and boys with girls, the effects of unemployment do not differ either.

In the bottom part of Table 4.11, we repeat the above analysis for neonatal mortality. We focus on unemployment in the 9 months before pregnancy, which was shown to have the highest effect on neonatal mortality in Section 4.5.2. Here, effects do not vary by educational attainment. However, the effect of unemployment is significantly smaller for parents in the top quarter of the income distribution compared to the bottom quarter. It is significantly larger for married than for single mothers. Overall, Table 4.11 provides suggestive evidence that the positive effects of recessions on newborn health are stronger for low-status parents. This is consistent with the channels related to stress and air pollution, to which low-status parents are more likely to be exposed.

4.5.5.3 Effects on Gestational Age

We can gain additional insights by exploiting knowledge about the production function of birth weight. According to Kramer (1987), birth weight is mechanically the product of two determinants: gestation length and intrauterine growth. Even if intrauterine growth occurs at a normal pace, a baby will have low birth weight if it is born premature. Similarly, a baby born at full term will have low birth weight if it suffered from insufficient intrauterine growth. Intrauterine growth is affected by cigarette smoking and nutrition, while gestation length – besides being affected by smoking – strongly responds to stress (Torche 2011; Foureaux Koppensteiner and Manacorda 2015). Gestation length is also sensitive to air pollution (Currie and Walker 2011). Recall from Section 4.5.4 that unemployment reduces the incidence being born with less than 32 completed weeks of gestation ("very preterm"). The size of the decrease has about the same size as the decrease in very low birth weight from column 2 of Table 4.5, suggesting that short gestation length 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. This is consistent with stress and air pollution playing a major in linking recessions with improvements in newborn health.

4.6 Conclusion

This paper addresses the question whether economic downturns improve newborn health in developed countries using a dataset from Sweden. We exploit fluctuations in labor market conditions at the local level and control for changes in the composition of births by comparing infants born to the same parents. Our results suggest that downturns improve newborn health outcomes. A one-percentage-point increase in the unemployment rate is associated with a 6–11 percent reduction in the incidence of having a birth weight below 1,500 grams and of dying within 28 days after birth. The increase in infant survival is permanent and not offset by delayed death later in the first year of life. In the Swedish context, our results imply that a one-percentage-point increase in the unemployment rate saves about 10 infant lives per year.

Thanks to detailed micro-level information about the parents, we also shed light on the mechanisms underlying the positive relationship of downturns and newborn health. We find that parental unemployment is unlikely to be a mediating factor. However, we find evidence that the reduction in mortality can fully be accounted for by an equally large reduction in premature birth. Premature birth has been attributed to maternal stress in earlier literature, as well as to air pollution. We also show that downturns disproportionately affect low-status parents. Low-status parents are more likely exposed to stress due to their business-cycle-sensitive type of occupation and to pollution due to residence in poor neighborhoods.

In downturns, air pollution decreases due to lower traffic volume. Stress plausibly decreases as a result of reduced working hours and more available time. Reduced working hours might also lead to higher demand for prenatal care, but such a channel would be difficult to reconcile with our finding that the effects on unemployed mothers are similarly large. For them, available time does not change in recessions. However, they might benefit from lower stress of the spouse or a general slowdown of hectic daily life.

To distinguish further between air pollution and stress, note that Sweden has rural parts where air pollution is permanently at negligible levels. In additional analyses (not reported), we find that the effect of recession is even larger in sparsely populated areas, arguing against air pollution as a channel, but other factors might drive this result. In sum, this paper provides a first step toward a better understanding of the mechanisms linking downturns to enhanced newborn health. More research on this question will be needed.

4.A Appendix

4.A.1 Tables

Table 4.12: Effect of Unemployment During Pregnancy on Birth Rate

	Mother		Father	
	Baseline	With Trends	Baseline	With Trends
Overall	0.0935	-0.1707		
	(1.1617)	(0.8235)		
% Change	0.02%	-0.05%		
Birth Order 1	0.5091	0.2776		
	(0.7968)	(0.6298)		
% Change	0.33%	0.18%		
Birth Order 2	0.0328	0.1717		
	(0.3892)	(0.3173)		
% Change	0.02%	0.12%		
Birth Order 3	-0.1871	-0.3215		
	(0.1774)	(0.2182)		
% Change	-0.26%	-0.44%		
Birth Order 4	-0.6063***	-0.5532***		
	(0.1459)	(0.1322)		
% Change	-2.0%	-1.82%		
Age - Below 25 Years	-0.0492	-0.3427	-0.2811	-0.2008
	(0.2980)	(0.3246)	(0.2313)	(0.1970)
% Change	-0.05%	-0.34%	-0.5%	-0.36%
Age - 25-35 Years	-0.1417	0.2602	-0.3142	-0.5157
	(0.8450)	(0.7156)	(0.8002)	(0.6181)
% Change	-0.06%	0.1%	-0.12%	-0.2%
Age - Above 35 Years	0.3168	0.2254	0.4902	0.5310
	(0.3844)	(0.2697)	(0.4646)	(0.3950)
% Change	0.76%	0.54%	0.58%	0.63%
Marital Status - Single	0.3794	0.9094	0.3967	0.7539
	(0.5451)	(0.6054)	(0.5711)	(0.6137)
% Change	0.16%	0.38%	0.16%	0.31%
Marital Status - Married	-0.5848	-1.3774***	-0.5731	-1.2629***
	(0.6606)	(0.3506)	(0.6507)	(0.3668)
% Change	-0.43%	-1.02%	-0.42%	-0.94%
Marital Status - Divorced	-0.0987	0.0589	-0.0926	0.0544

Continued on next page

	(0.1672)	(0.1525)	(0.1049)	(0.1029)
% Change	-0.42%	0.25%	-0.41%	0.24%
Education - Primary and Lower Secondary	-0.4702***	-0.4950***	0.1648	0.0994
	(0.1559)	(0.1839)	(0.3128)	(0.2885)
% Change	-1.31%	-1.38%	0.29%	0.17%
Education - Secondary Education and Vocational	-0.0753	-0.1073	-0.8546	-0.7042
	(0.6155)	(0.4660)	(0.6160)	(0.4283)
% Change	-0.03%	-0.05%	-0.34%	-0.28%
Education - Graduate and Postgraduate	1.3658	1.0554*	1.3490	0.9958
	(1.0621)	(0.5796)	(1.0644)	(0.6505)
% Change	1.16%	0.89%	1.49%	1.1%
Country of Birth - Sweden	0.0934	-0.3007	-0.1301	-0.4510
	(1.2258)	(0.7554)	(1.2646)	(0.7338)
% Change	0.03%	-0.08%	-0.04%	-0.12%
Country of Birth - Developing Countries	-0.0710	0.1234	-0.1733	-0.0126
	(0.0957)	(0.0855)	(0.1344)	(0.0570)
% Change	-0.68%	1.18%	-1.85%	-0.13%
Country of Birth - Developed Countries	0.0933	0.0576	0.0278	0.0692
	(0.2145)	(0.1838)	(0.1339)	(0.1054)
% Change	0.5%	0.31%	0.15%	0.38%

Notes: OLS regressions of the birth rate on the average unemployment rate in the age group 18-40 years in nine months during pregnancy. Birth rates are defined as the number of births with same month of conception in the given subgroup per 1,000 women aged 18-49 years in the population. Controls are year fixed effects, month-of-year fixed effects, local-labor-market fixed effects and local-labor-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Table 4.13: Effect of Unemployment During Pregnancy on Composition of Birth Cohorts

	Mother		Father	
	Baseline	With Trends	Baseline	With Trends
Birth Order 1	0.0973	0.0643		
	(0.0717)	(0.0721)		
% Change	0.25%	0.17%		
Birth Order 2	-0.0118	0.0374		
	(0.0668)	(0.0674)		
% Change	-0.03%	0.1%		
Birth Order 3	-0.0205	-0.0552		
	(0.0319)	(0.0346)		
% Change	-0.12%	-0.33%		
Birth Order 4	-0.0718***	-0.0725***		
	(0.0229)	(0.0205)		
% Change	-1.33%	-1.34%		
Age - Below 25 Years	0.0454	-0.0067	0.0067	0.0120
	(0.0470)	(0.0481)	(0.0427)	(0.0397)
% Change	0.19%	-0.03%	0.06%	0.1%
Age - 25-35 Years	-0.0744	0.0188	-0.0560	-0.0610
	(0.0597)	(0.0493)	(0.0499)	(0.0527)
% Change	-0.11%	0.03%	-0.08%	-0.09%
Age - Above 35 Years	0.0290	-0.0121	0.0493	0.0491
	(0.0484)	(0.0342)	(0.0449)	(0.0455)
% Change	0.34%	-0.14%	0.25%	0.25%
Marital Status - Single	0.1870***	0.3584***	0.1999***	0.3465***
	(0.0684)	(0.0713)	(0.0628)	(0.0765)
% Change	0.3%	0.57%	0.31%	0.55%
Marital Status - Married	-0.2147***	-0.3929***	-0.2180***	-0.3717***
	(0.0739)	(0.0740)	(0.0714)	(0.0676)
% Change	-0.65%	-1.2%	-0.66%	-1.13%
Marital Status - Divorced	0.0277	0.0346	0.0182	0.0252
	(0.0198)	(0.0252)	(0.0202)	(0.0216)
% Change	0.71%	0.89%	0.49%	0.68%
Education - Primary and Lower Secondary	-0.0999***	-0.1064***	-0.0341	-0.0509
	(0.0386)	(0.0295)	(0.0536)	(0.0445)
% Change	-1.42%	-1.51%	-0.28%	-0.42%
Education - Secondary Education and Vocational	-0.0713	0.0583	-0.1166	0.0407
	(0.0683)	(0.0546)	(0.0925)	(0.0447)

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% Change	-0.11%	0.09%	-0.17%	0.06%
Education - Graduate and Postgraduate	0.1712**	0.0481	0.1507*	0.0102
	(0.0713)	(0.0498)	(0.0881)	(0.0452)
% Change	0.58%	0.16%	0.73%	0.05%
Country of Birth - Sweden	0.0465	0.0184	-0.0045	-0.0192
	(0.0387)	(0.0181)	(0.0498)	(0.0197)
% Change	0.05%	0.02%	-0.0%	-0.02%
Country of Birth - Developing Countries	-0.0047	0.0113	-0.0208	-0.0004
	(0.0199)	(0.0118)	(0.0279)	(0.0111)
% Change	-0.51%	1.22%	-2.84%	-0.05%
Country of Birth - Developed Countries	-0.0418*	-0.0298*	0.0253	0.0196
	(0.0245)	(0.0171)	(0.0256)	(0.0159)
% Change	-1.87%	-1.33%	1.11%	0.86%

Notes: OLS regressions of the share of infants with the same month of conception in a given subgroup on the average unemployment rate in the age group 18-40 years in nine months during pregnancy. Controls are year fixed effects, month-of-year fixed effects, local-labor-market fixed effects and local-labor-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Table 4.14: Average Health by Subgroup

	Weight < 1,500 Grams				Neonatal Mortality			
	Mean	SD	N	t-value	Mean	SD	N	t-value
Birth Order								
1	0.0067	0.0817	365,312	-	0.0018	0.0423	366,809	-
2	0.0036	0.0597	325,935	-18.44	0.0013	0.0367	326,935	-4.72
3	0.0038	0.0619	127,922	-13.10	0.0017	0.0410	128,301	-0.84
4	0.0051	0.0712	35,916	-4.08	0.0020	0.0447	36,017	0.82
Mother's Age								
Below 25 Years	0.0048	0.0694	172,634	-	0.0016	0.0403	173,214	-
25-35 Years	0.0047	0.0685	620,435	-0.64	0.0016	0.0396	622,613	-0.55
Above 35 Years	0.0084	0.0914	78,391	9.77	0.0023	0.0476	78,676	3.31
Mother's Marital Status								
Single	0.0051	0.0715	501,748	-	0.0014	0.0374	503,518	-
Married	0.0048	0.0689	337,420	-2.40	0.0020	0.0445	338,585	6.35
Divorced	0.0074	0.0854	31,551	4.50	0.0018	0.0428	31,658	1.76
Mother's Country of Birth								
Sweden	0.0050	0.0708	836,537	-	0.0016	0.0403	839,424	-
Developing Countries	0.0065	0.0801	16,565	2.26	0.0020	0.0452	16,640	1.19
Developed Countries	0.0051	0.0714	18,351	0.15	0.0021	0.0460	18,432	1.44
Mother's Education								
Primary and Lower Secondary	0.0069	0.0825	57,932	-	0.0016	0.0404	58,161	-
Secondary Education and Vocational	0.0053	0.0725	480,907	-4.40	0.0017	0.0409	482,514	0.26
Graduate and Postgraduate	0.0043	0.0658	308,773	-6.92	0.0016	0.0400	309,897	-0.16
Father's Age								
Below 25 Years	0.0053	0.0723	88,596	-	0.0015	0.0385	88,905	-
25-35 Years	0.0047	0.0682	605,824	-2.28	0.0016	0.0395	607,952	0.56

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Above 35 Years	0.0064	0.0795	177,040	3.57	0.0020	0.0447	177,646	3.07
Father's Marital Status								
Single	0.0051	0.0715	497,517	-	0.0014	0.0379	499,260	-
Married	0.0048	0.0690	338,585	-2.31	0.0020	0.0443	339,749	5.65
Divorced	0.0070	0.0833	33,493	3.95	0.0014	0.0374	33,621	-0.20
Father's Country of Birth								
Sweden	0.0050	0.0705	834,081	-	0.0016	0.0404	836,979	-
Developing Countries	0.0070	0.0834	16,284	3.05	0.0018	0.0421	16,359	0.42
Developed Countries	0.0068	0.0819	21,035	3.08	0.0019	0.0440	21,104	1.01
Father's Education								
Primary and Lower Secondary	0.0062	0.0783	99,919	-	0.0020	0.0442	100,298	-
Secondary Education and Vocational	0.0051	0.0712	488,915	-4.05	0.0016	0.0397	490,483	-2.51
Graduate and Postgraduate	0.0045	0.0671	268,923	-5.91	0.0017	0.0407	269,959	-1.82

Notes: t-values are from tests of equal means compared with the first subgroup in each category.

Table 4.15: Effect of Women Unemployment by Age Group

Weight < 1,500 Grams	18-24 Years	18-30 Years	18-40 Years	18-64 Years
Unemployment Before Pregnancy	-0.0125*	-0.0175*	-0.0171	-0.0150
	(0.0071)	(0.0095)	(0.0131)	(0.0200)
N	474,738	474,738	474,738	474,738
Unemployment During Pregnancy	-0.0125*	-0.0168*	-0.0178	-0.0197
	(0.0069)	(0.0087)	(0.0115)	(0.0173)
N	503,275	503,275	503,275	503,275
Neonatal Mortality	18-24 Years	18-30 Years	18-40 Years	18-64 Years
Unemployment Before Pregnancy	-0.0056	-0.0103*	-0.0154**	-0.0248**
	(0.0046)	(0.0058)	(0.0073)	(0.0105)
N	477,873	477,873	477,873	477,873
Unemployment During Pregnancy	-0.0046	-0.0057	-0.0085	-0.0165
	(0.0042)	(0.0055)	(0.0074)	(0.0105)
N	506,501	506,501	506,501	506,501

Notes: Unemployment refers to the unemployment rate among women in the indicated age group. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before pregnancy is the average unemployment rates during the 9-months-period before conception. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Table 4.16: Effect of Women Unemployment by Region

Weight < 1,500 Grams	County	Local Labor Market	Municipality
Unemployment Before Pregnancy	-0.0153	-0.0171	-0.0152
	(0.0148)	(0.0131)	(0.0094)
N	476,342	474,738	436,111
Unemployment During Pregnancy	-0.0152	-0.0178	-0.0119
	(0.0094)	(0.0115)	(0.0096)
N	504,976	503,275	462,489
Neonatal Mortality	County	Local Labor Market	Municipality
Unemployment Before Pregnancy	-0.0179**	-0.0154**	-0.0070
	(0.0089)	(0.0073)	(0.0065)
N	479,504	477,873	438,985
Unemployment During Pregnancy	-0.0120	-0.0085	-0.0081
	(0.0082)	(0.0074)	(0.0065)
N	508,232	506,501	465,450

Notes: Unemployment refers to the unemployment rate among women in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before pregnancy is the average unemployment rates during the 9-months-period before conception. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the level of the indicated region are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Table 4.17: Effect on Labor Market Outcomes

	No Wage				No Reimbursements					
	Mother		Father		Mother		Father		Log Family Income	
	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
Unemployment Before Pregnancy	0.2196***	0.3168***	0.0938	0.0273	0.1668**	0.2856***	0.1073**	0.0419	-0.1058	-0.3078***
	(0.0791)	(0.0746)	(0.0621)	(0.0510)	(0.0686)	(0.0676)	(0.0527)	(0.0413)	(0.0997)	(0.0868)
N	477,870	477,870	476,946	476,946	422,266	422,266	421,464	421,464	385,405	385,405
Unemployment During Pregnancy	0.1922***	0.2811***	0.1192**	0.0539	0.1713**	0.2560***	0.1333***	0.0427	0.0390	-0.1707**
	(0.0698)	(0.0630)	(0.0504)	(0.0451)	(0.0679)	(0.0589)	(0.0425)	(0.0347)	(0.0725)	(0.0738)
N	506,497	506,497	505,528	505,528	450,786	450,786	449,938	449,938	411,500	411,500

Notes: Unemployment refers to the unemployment rate among the indicated gender in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. Unemployment before pregnancy is the average unemployment rates during the 9-months-period before 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. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

Table 4.18: Effect of Parental Unemployment ("No Reimbursements")

	Baseline	Mother		Father	
Weight < 1,500 Grams	(1)	(2)	(3)	(4)	(5)
Unemployment During Pregnancy	-0.0322***	-0.0371***	-0.0364***	-0.0363***	-0.0358***
	(0.0123)	(0.0120)	(0.0117)	(0.0119)	(0.0119)
No Reimbursements		-0.0006	0.0003	-0.0008	0.0006
		(0.0005)	(0.0015)	(0.0008)	(0.0016)
No Reimbursements × Unemployment			-0.0058		-0.0087
			(0.0104)		(0.0101)
N	503,275	472,543	472,543	471,980	471,980
	Baseline	Mother		Father	
Neonatal Mortality	(1)	(2)	(3)	(4)	(5)
Unemployment During Pregnancy	-0.0130**	-0.0108	-0.0115*	-0.0106	-0.0115*
	(0.0065)	(0.0068)	(0.0067)	(0.0068)	(0.0068)
No Reimbursements		0.0002	-0.0008	-0.0008	-0.0032
		(0.0005)	(0.0010)	(0.0005)	(0.0020)
No Reimbursements × Unemployment			0.0067		0.0138
			(0.0058)		(0.0125)
N	506,501	475,590	475,590	475,021	475,021

Notes: Unemployment refers to the unemployment rate among men in the age group 18-40 years. Unemployment during pregnancy is the average unemployment rate in the nine months following conception. "No Reimbursements" takes on the value 1 if no work-related reimbursements are received. Controls are year fixed effects, month-of-year fixed effects and parents fixed effects. Standard errors clustered at the local labor market level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

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Affidavit

I hereby affirm that this dissertation was written and completed independently and without outside assistance. All auxiliary means were listed clearly and comprehensively.

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