Beyond Treatment Exposure: The Timing of Early Interventions and Children's Health*

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Abstract

This paper analyzes the impact of the timing of nurse home visits for newborns and their family on child health, mother health, and parental health investments. To identify the effects of timing, we study the 2008 national nurse strike in Denmark: We exploit exogenous variation in the timing of foregone nurse visits for the population of children born in Copenhagen in the period up to the strike and in control years in a difference in differences framework. Children who missed visits at younger ages have more regular and emergency general practitioner (GP) contacts in their first five years of life compared to children who missed nurse visits later. We find larger impacts for children of parents with no educational background in health and childcare and first-parity children. This finding indicates that universal home visits convey important information and guidance to new parents. While constrained by power issues, our further analyses of mechanisms carefully suggest that the timing of nurse visits impacts parental health investments. In sum, our findings and stylized cost-effectiveness calculations suggest that universal home visiting policies should put special emphasis on nurse visits in the very first months of the child's life.

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1 Introduction

A large literature has documented causal links between childhood experiences—shocks and exposure to policies—and later life outcomes (for an overview see Almond and Currie, 2011; Almond et al., 2018). Studying the causal effects of early-life investment programs (such as nurse home visiting, childcare and early education provisions, or income support), the majority of work has considered the effects of program exposure, i.e. the *extensive* margin. Thus we still lack insights on the causal effects of early-life policies at the *intensive* margin. Moreover, while much of the work on early-life investment policies has been set in a U.S. context and as a consequence has considered *targeted* programs,[1] many countries offer *universal* programs. Thus studies on the impact of universal investment programs are instrumental for policy design in many countries.

This paper makes three contributions to the existing literature on the causal effects of early-life health policies: First, we study the impact of a popular policy, universal nurse home visiting (NHV), on child and maternal health and parental behaviors. In doing so we extend on existing work, which has predominantly considered either contemporary targeted programs[2] or the very long-run impact of the introduction of universal NHV in Scandinavia of the 1930s and 40s (Wüst, 2012; Hjort et al., 2017; Bhalotra et al., 2017; Butikofer et al., 2018).[3] In Denmark, universal NHV by trained nurses has been an integral part of publicly-

[1] Examples include studies on the targeted Perry Preschool Program, the Abecedarian project (Heckman et al., 2013; Conti et al., 2016), and observational studies on the short- and long-run impact of Head Start (Currie and Thomas, 1995; Garces et al., 2002; Masse and Barnett, 2002; Schweinhart et al., 2005; Belfield et al., 2006; Ludwig and Miller, 2007; Anderson, 2008; Deming, 2009; Heckman et al., 2010a,b; Carneiro and Ginja, 2014; Campbell et al., 2014; García et al., 2016; De Haan and Leuven, 2016; García et al., 2017; Thompson, 2017).

[2] Existing evidence suggests that targeted NHV can be effective in improving a large range of short- and long-run child outcomes and point to the role of the qualifications of service providers: Focusing on the targeted Nurse Home Visiting Partnership program in the US, Olds et al. (1986, 1998, 2002) show that visits for at-risk individuals conducted by trained nurses reduced child abuse, decreased children's emergency room visits and their criminal convictions in adolescence. Similarly, Vaithianathan et al. (2016) provide evidence from New Zealand showing that targeted nurse visits reduced infant mortality and increased both vaccination rates and children's participation in early childhood education. Doyle et al. (2015) study a targeted program called Preparing for Life in Ireland and find positive effects on health (such as asthma issues) and accidents.

[3] All existing evidence on the causal short- and long-run effects of NHV in Scandinavia comes from historical data and consider the extensive margin of treatment exposure. These studies have documented positive long-run effects on the health and socio-economic outcomes of exposed cohorts.

funded post-natal and preventive care from 1937 onwards. While the program originally offered an average of 10 visits during the first year of life to all families, today parents are eligible for up to five universal home visits. The focus of the contemporary program is—like in many other settings—on health monitoring and screening of infants, information of new parents (on topics such as infant nutrition, infant behavior and child-parent interactions), and the promotion of adequate parental inputs and interactions.

Second, we study the impact of timing of program exposure rather than the "all or nothing"-margin that has previously been considered. As the "no-program default" is often not a relevant alternative, our paper provides results that are relevant for contemporary policy design.

Third, by examining heterogeneity across families and by studying a set of parental investment decisions, we try to provide new insights on the impact of parental behaviors in response to public policies. While theoretical models of skill formation highlight the importance of parental investments in children (Cunha and Heckman, 2007), we still have limited empirical evidence on the ways in which public policies can modify parental investment behaviors.

To identify the effects of the timing of NHV for child and maternal health and parental investments, we exploit families' exposure to the 2008 nurse strike in Denmark. During the strike period, the vast majority of non-emergency nurse care was canceled. As we show, in our sample, only one-tenth of expectable nurse visits to new families were performed and canceled visits were not rescheduled after the strike. Important for our empirical strategy, while children exposed to the strike on average missed one scheduled nurse visit, depending on their date of birth relative to the strike period, children had a different age at the foregone visit. We exploit this variation together with information on children born in non-strike years in a difference-in-differences design to identify the impact of timing of early-life health policies on outcomes.

Our approach is closely related to Kronborg et al. (2016), who study mothers who gave birth during and shortly prior to the strike in a similar framework. Those mothers received

less pre- and post-natal care (i.e. the strike impacted both their midwife consultations, their hospital admissions at birth and their post-natal nurse visits).[4] Their reduced form estimates show that maternal strike exposure prior to or at birth impacts children's uptake of GP care and maternal breastfeeding decisions. We advance on this study in two ways: First, we only consider children born *before the strike* and thus exploit variation that impacts the timing of foregone nurse visits but—as we will show—holds constant other aspects of pre- and post-natal care. Second, we exploit newly-available municipal nurse records for Copenhagen together with administrative register data from Statistics Denmark. The nurse records contain detailed information on all visits provided from 2007-2010, among other informations the type of visit, the date of the visit and various other observations and notes regarding the family's well-being. These new data allow us to perform a complier analysis, i.e. assess the "coverage" of the treatment in the population of families with respect to a large set of observable characteristics.

A nurse visit may impact child health and parental behaviors through two main channels. First, being a central feature of Danish preventive care, nurse visits may help to identify adverse health conditions in a timely fashion and prompt additional care by other health professionals, such as general practitioners (GP). In absence of the nurse visit, health problems (such as infant feeding problems or maternal post-natal depression) may go unnoticed for a longer period and potentially cause longer-term adverse health effects. Second, in the absence of nurse visits, parents may lack specific guidance and information typically provided by nurses and difficult to replace by other health care providers, such as GPs. This information may impact parents' investment behaviors, such as breastfeeding, parent-child interactions or uptake of other preventive care. Importantly, both channels are likely to be

[4]Mothers, who gave birth during the strike received fewer nurse visits, fewer pre-natal midwife consultations and were more likely to be discharged from hospital on the day of birth. Mothers, who gave birth in the two weeks prior to the strike had higher probability of not receiving the initial nurse visit but were unaffected with respect to the access of pre-natal care. Mothers, who gave birth even earlier (two weeks to two months prior to the strike) were unaffected with respect to the pre-natal care offers, hospital care around birth and initial nurse visits. However, they had an increased probability of a canceled second nurse visit (which would have been scheduled during the strike period).

impacted by the timing of NHV relative to the child's age. Specifically, we hypothesize that earlier visits may have larger impacts on new families than later visits. While we cannot fully disentangle the importance of each potential channel, we hypothesize that the information channel may matter more for some parents. Specifically, if information provision is a relevant channel for effects of NHV, we expect larger effects for parents who are likely to lack information in the absence of a nurse visit.

We show that the 2008 nurse strike impacted families with young children in the expected ways: Depending on the date of birth of their child, families on average missed one nurse visit (at different ages). Importantly, we show that the strike impacted families similarly across observable characteristics observed by nurses. This finding illustrates the broad coverage of the strike in the population and relieves concerns that nurses to a large degree chose the families that would forgo their visit.

Moving on to our reduced form analysis on the impact of strike exposure at specific ages, we show that strike exposure during the initial months of a child's life is relatively more influential for child health. Children, who were born in the three months up to the strike and thus lacked an early nurse visit have both more regular and emergency GP contacts in the first five years of life than children, who were older at strike-exposure. Studying children across different backgrounds, we find that higher parity children and children of parents with an educational background in a health-related field (nurses, midwives, doctors and pedagogues) are less affected by strike exposure than their first-parity and not health-educated counterparts, respectively. This finding suggests that nurses provide relevant information to new parents and that at least part of the beneficial effect of early NHV runs through this information channel. Early NHV also affects maternal health as mothers of children who lost early nurse visits, have more GP contacts five years after childbirth. Studying parents' participation in the vaccination and preventive care programs, we find only weak indication for an impact of the timing of nurse visits. However, these analyses are constrained by power issues.

We perform a stylized cost-effectiveness analysis. During the strike, the municipality of Copenhagen had daily savings of around 36,000 EUR (2015 prices) as they did not pay wages to the employees on strike. Based on this figure, we estimate an average cost of 129 EUR per nurse visit. Using our main estimation results and only considering the positive effects of early NHV relative to later ones on GP fees, we estimate savings that outweigh or at least balance these costs (363 EUR and 101 EUR for the initial and 2-month visit, respectively). Thus early universal nurse visits may be a cost-effective interventions to promote children's health in settings that resemble the Danish health care system.

As a consequence, our findings suggests that policies to restructure existing universal NHV programs or introduce new programs should prioritize the initial period of family formation after the birth of a child. Moreover, the larger impacts for first-borns and children of parents without a health or child-care related educational background may indicate that universal home visits convey important information and guidance that some parents cannot seek on their own.

The paper proceeds as follows: Section 2 provides information on the institutional background, the 2008 nurse strike and the data sources that we use. Section 3 presents our empirical strategy and discusses the identifying assumptions. Section 4 presents descriptive and main results and examines their robustness and heterogeneity. Section 5 performs a simple cost-effectiveness analysis. Finally, section 6 concludes.

2 Background and Data

2.1 Institutional Background: Pre and post-natal care in Denmark

In Denmark, pre and post-natal care are provided in the public health care system. All residents have access to care free of charge. Midwives and general practitioners provide pre-natal care that consists of regular consultations during pregnancy.[5] The majority of

[5]The universal offer consists of 4-7 midwife consultations, 3 GP consultations and 2 ultrasound scans Sundhedsstyrelsen (2007). At-risk pregnancies receive additional care.

uncomplicated births in Denmark are midwife-assisted in public hospitals. Hospital births account for around 98% of all births.

After hospital discharge, the 98 municipalities are responsible for providing post-natal care in the NHV program. While a municipal program with some variation in service levels, the Danish National Board of Health (DNBH) issues guidelines and regulations regarding the number, timing and content of nurse visits. As such, NHV consists of a basic package of services offered to all families with a newborn. Additionally, municipalities can choose to offer supplementary services targeted at specific populations of mothers and children. Those services include additional home visits or other services.[6]

2.2 NHV in Copenhagen

Our study focuses on NHV in Copenhagen, the largest municipality in Denmark with about half a million inhabitants and about 8-10.000 live births per year. Appendix Table A1 shows the main features of NHV in Copenhagen. The suggested number of visits in the basic offer is four: an initial visit shortly after birth, a 2-month visit (scheduled in the month after the child turns two months), a 4-month visit and an 8-month visit (scheduled in the two months after the child turns four and eight months, respectively). Furthermore, the municipality offers different types of optional visits that are offered on the request of parents. Finally, nurses can provide additional visits to children and families with identified needs. The timing of these additional visits is flexible.

Table 1 describes the content of the scheduled universal nurse visits. **tbc**

2.3 The 2008 Nurse Strike

In Denmark, both private and public wages are to a large degree determined by collective bargaining (Ibsen et al., 2011). The health care sector accounts for a large share of public

[6]These services can include offers such as group interventions, interventions targeted at young parents or parents with specific health issues, or interventions specifically directed at fathers.

employment in Denmark and it is heavily unionized. In 2008, the negotiations for all publicly-employed nurses, midwives and a large fraction of other employees in the public health sector broke down and resulted in a conflict. Thus on April 15, 2008 the unionized employees in the health care sector went on a national strike. A total of 45 % of public employees were on strike in the following weeks (Due and Madsen, 2008). The strike lasted 61 days and ended on June 14, 2008.[7]

Although nurses are heavily unionized, managing nurses and a small fraction of regular nurses are employed on specific terms and thus did not participate in the strike. In Copenhagen, the setting for our analysis, these nurses carried out around one tenth of the expectable non-strike default of nurse visits.[8] Moreover, they provided phone services for families that were affected by the strike.

2.4 Sample and Variable Construction

In our analysis, we use data from two sources. First, we use population administrative data from Statistics Denmark for the birth cohorts 2007-2010. The administrative data contains a large set of background characteristics such as parental educational attainment, income, age, family relations, municipality of residence and birth records. The birth records include information on measures such as birth weight, height, gestation age, the five minute APGAR score, birth hospital and levels of pre-natal care. Furthermore, the administrative data contains information on health care usage such as the number of GP contacts and vaccinations.

Second, we merge the administrative data with novel nurse records from the municipality of Copenhagen for the 2007-2009 period. While Scandinavia is well-known for its high-quality administrative data in many domains, individual-level data on exposure to municipal programs (such as NHV, nurseries or preschool) and their features are not readily available at

[7]The unions demanded a 15 % wage growth. The agreement resulted in a 13.3 % wage increase over a three-year duration.

[8]We calculate this share of performed visits by comparing the strike period to the same period in the following year.

Statistics Denmark. Importantly, as often exploited in studies on the roll-out of program, municipal provision introduces considerable variation in program features across municipalities and over time.

The original archived nurse records from Copenhagen include all children born in the municipality from January 1, 2007 to December 31, 2010—a total of 35,213 children.[9] When we analyze data on completed nurse visits, we only consider the years 2008 and 2009 as the data for the 2010 cohort are right-censored, i.e. we do not observe information on all of their visits before the end of the data period. The nurse records contain information on all performed visits made by the municipal nurses and their timing.[10] The nurse records also contain observations noted by the nurses during their visits such as child and maternal health, nutrition problems, breastfeeding behavior and risk factors.

Our main outcome measure of child health is a measure of health care usage, the total number of GP contacts. We measure the accumulated and yearly number of GP contacts at every year of life until the child turns five. This relatively long follow-up period allows us to speak to the role of substitution between nurse visits and GP contacts: While short-term effects on GP contacts may be caused by substitution, the role of substitution in the long-term appears to be less important.

A priori, it is not clear whether a lack of NHV at specific ages should increase or decrease both health-related and prevention-related GP contacts: Any effects on the number of GP contacts may both relate to more precautionary contacts, to decreased child health or to substitution. While we cannot fully disentangle those different mechanisms, we divide the total number of GP contacts into two categories to tease out more information: i) regular GP contacts and ii) emergency GP contacts (i.e., GP contacts on weekends or outside default opening hours). Emergency GPs handle conditions that require medical attention outside GP office hours. While not perfectly independent, emergency GP contacts may be a cleaner

[9]The datasets time frame is due to the use of a specific nurse record system in the given years. The records were transferred to the archive due to a change of the software used by the nurses to register their work.

[10]We use a unique child social security number in the nurse records to merge them with the administrative data.

measure of actual health conditions that require attention.

As supplementary health outcomes, we consider child hospitalizations. While hospitalizations may be a cleaner measure of actual child health, they are relatively rare events and thus require a large effect to be detected in our sample. Moreover, we study the number of GP contacts for mothers. Any effects to mothers' GP contacts may suggest that the timing of nurse visits impacts mothers too and could be a mechanism or a reinforcing factor for longer term child health effects.

To analyze the potential impact of the timing of NHV on parental investments, we study take-up of preventive care and vaccines. The Danish preventive care schedule offers eight health checks for all children: around 5 weeks, 5 month and at 1, 2, 3, 4, 5 and 6 years (Sundhedsstyrelsen, 2007). Participation in the program is voluntary. The Danish vaccination program for children consists of three rounds during the first year of a child's life at 3, 5 and 12 months.[11] Each nurse visit beyond the initial visit is closely spaced around the recommended age for a vaccination. We test the hypothesis that missing a specific nurse visit lowers the probability of having the subsequent vaccination. If this turns out to the case, we may conclude that nurse visits stimulate positive parental behaviour and investments.

3 Empirical Methods

To examine the effects of the timing of NHV, we exploit children's exposure to the nurse strike in a difference-in-differences framework. Specifically, we estimate the following model using regular least squares:

$$\begin{aligned} y_{it} =& \beta_0 + \sum_{j=-7}^{-1} \phi_j 1(bin30_{it} = j) \times 1(Year_t = 2008) \\ &+ \sum_{j=-7}^{-1} \alpha_j 1(bin30_{it} = j) + \gamma' \boldsymbol{X}_{it} + \boldsymbol{\lambda}_t + \epsilon_{it} \end{aligned} \tag{1}$$

[11] Each round consists of two separate vaccinations. First, a combined vaccination to immunize against diphtheria, tetanus, pertussis, polio and hib infection. Second, a pneumococcus bacteria vaccination to prevent infant meningitis.

where y_{it} is a measure of health or parental investments for child i born at time t. In our analyses for administrative outcomes, we consider all children born in the 210 day period prior to April 15 in the years 2008, 2009 and 2010 (14,784 children).[12] We split each period in seven 30 days bins and include indicators that are equal to one if child i's date of birth is within a particular bin. We include a set of fixed effects for the relevant year, $\boldsymbol{\lambda}$.[13] The interactions of the period bins with an indicator for the year 2008 (the year of the strike) identify our estimates of interest: Children born prior to the strike in 2008 are treated while children born at the same dates in 2009 and 2010 are untreated. We omit the bin furthest from April 15 and children in this group constitute the reference group.

In our main specification, we include the following covariates (X_{it}): paternal and maternal income, indicators for the highest level of education (primary school, higher education, university degree), indicators for currently studying and for being employed, an indicator for parental civil status (cohabiting, married) and indicators for missing covariates. All these measures are measured one year prior to birth of the focal child. Additionally, we control for measures drawn from the birth records, including the number of pre-natal midwife visits and indicators for parents being below 21 years old, a Caesarean section, a home birth, low birth weight (below 2500g), a preterm birth (below 37 weeks), child gender, and maternal smoking status at birth.

The coefficients from interacting the age bins and the strike period indicator provide intention-to-treat (ITT) estimates of strike exposure at a certain age relative to the reference group. To show that strike exposure is relevant, we present estimates for the impact of strike exposure on the probability of missing a nurse visit at a specific time in the child's life (the first stage). Furthermore, we present evidence on complier characteristics that substantiates our assessment of the strike as a broad treatment impacting families across many observable

[12] As mothers given birth during the strike also had a larger probability of being discharged on the day of birth and fewer midwife visits (Kronborg et al., 2016), including children born during the strike would confound the impact of NHV with the impact of other aspects of care.

[13] Note that the year indicators cross calendar years: As an example the indicator for the year 2008 (the treated year) is equal to one for all birth in the 210 days prior to April 15, 2008 and thus identifies births in the calendar years 2007/2008.

dimensions.

3.1 Identifying assumptions

For our estimates to identify the causal impact of exposure to the nurse strike, we make two identifying assumptions. First, we assume that, in the absence of the strike, the difference-in-differences between children born in specific periods up to April 15 in the strike and control years should be zero (common trend). Thus our framework allows for the years 2008, 2009 and 2010 to differ. These differences could, for example, include general trends in children's health or macroeconomic shocks that affect care and health of children. Our focus on births from different months of the year also calls for a discussion of the impact of seasonality: We allow children born across seasons to be systematically different from each other (with respect to their average outcomes) as long as this seasonality is the same across years.

One way of empirically assessing the untestable common trend assumption is to study predetermined variables, which should be unrelated to treatment exposure. In other words, we estimate model (1) using parental and birth characteristics as dependent variables. Appendix Tables A2 and A3 show that our treated and control groups are balanced across observable pre-treatment characteristics. Very few coefficients are significant and only at modest levels of significance.[14]

Another informal test is the assessment of pre-trends in outcomes across groups. As we do not observe children's GP visits prior to treatment, Appendix Figure A1 compares the number of GP contacts for mothers of treated and control children a year prior to birth. The figure shows that mothers of both groups had similar trends and levels of GP contacts a year leading up to birth.

Second, we assume that there are no other policies or shocks that covary with the timing of the strike. To provide support for this assumption, we assess whether strike exposure was related to differential health care provision through other channels than the NHV. We

[14]We have also tested the joint significance of the interaction between age bin and strike indicator. None of the tests are significant at the 10 %-level. Results are available on request.

graphically show the relationship between strike exposure and pre-natal midwife visits and GP consultations, the number of days admitted during birth and the C-section rate in Appendix Figure A2. We find no systematic differences to any of these types of care around birth across the groups.

An alternative way of assessing whether other factors confound our interpretation of the strike impact are placebo regressions where we use alternative groups of children from different years. Appendix Table A4 shows placebo tests where we define "treated" children as those born 210 days prior to April 15, 2009 (the year after the strike). We find no significant effects of "strike exposure" in the placebo regressions.[15]

A final concerns that we address is individuals' selection into or out of the sample. First, individuals should not be able to manipulate their treatment status. We do not expect that manipulation is possible since all children in the sample were born prior to the strike or a minimum of four month after the strike ended. Appendix Figure A3 provides graphical evidence of the density of births in a window around strike start where our sample is a subset within the window. The figure show no bunching around the vertical lines. We take this as evidence that parents do not have the ability to avoid the impact of the strike by manipulating the date of birth.

With respect to selection out of the sample, in our main analysis, we omit data for 1,962 children who move from the municipality of Copenhagen during the first year of life. If strike exposed children were more (or less) inclined to move our estimates could be biased.[16] Figure A4 shows the share of individuals that stay in Copenhagen during the first year of life as a function of age at strike and split by treated and control group. The share of stayers are basically the same across periods. A slight concern relates to the apparent difference for children who were born around 150 days prior to the strike. Appendix Figure A4 suggests

[15] Appendix Table A5 shows results from a different placebo test where we use the same periods as in the main regressions but use untreated cohorts. As untreated cohorts, we choose children who had their five year birth days during each period and were too old too have any nurse visits affected by the strike. We find no significant differences between the untreated cohorts.

[16] As the strike was a nation-wide strike, the risk of strike-induced domestic migration seems negligible.

that more children moved in the strike exposed period compared to the control periods. In the strike exposed period 84 % stayed in the sample while the number was 87 % in the control periods for children who were 150 days of age at strike start. As robustness check, we run the regressions in a sample where we include domestic first-year movers such that only death and migration abroad causes exclusion.

4 Results

4.1 Descriptive Statistics

Table 2 presents summary statistics for our main sample of children born in the municipality of Copenhagen in the relevant data period for the years 2008-2010. In the top panel, we present summary statistics for outcomes and covariates from the administrative data. In the bottom panel, we present variables on nurse visits from the nurse records from Copenhagen. In this panel, we further constrain our sample to the data periods in the years 2008 and 2009 as the data—which covers the calendar years 2007-2010—is right-censored for the children born in 2010.

The average child has 4.7 GP contacts during the first year of life. At age 5, that number has increased to 29.4. regular GP contacts constitute around two thirds of the total number of contacts. The infant vaccinations and preventive health checks have high coverage rates at around 90 %. Focusing on the bottom panel of Table 2, we find that the four universal nurse visits are well attended—even considering the strike. The average number of universal visits per child is three, meaning that the average child receives three out of potentially four visits. On an average, children receive one additional visit scheduled due to a specific identified need. This average masks heterogeneity across children.

To assess the representativeness of our sample of families from the capital of Denmark, Appendix Table A6 compares children and parents from Copenhagen to the general Danish population of parents. Children and parents from Copenhagen differ from the general

population on a number of characteristics: they are more likely to cohabit and less likely to be married. Mothers from Copenhagen have a higher educational attainment. Parents from Copenhagen are less likely to be employed and of Danish origin. With respect to children's health and characteristics, children in Copenhagen resemble children from the rest of county: 5 % of children are low birth weight children and 7 % are born prematurely. Children in Copenhagen are also similar to the rest of Denmark with respect to the number of nights at hospital after birth, the number of pre-natal midwife visits, the rate of C-section deliveries, and the share of home births. At the same time, 62 % of children born in Copenhagen are firstborns compared to 43 % outside Copenhagen, their parents are older and less likely to smoke.

Finally, Appendix Table A7 compares children in Copenhagen across the group of treated children (born September 18, 2007 - April 14, 2008) and children in the control group (born September 17, 2008 - April 14, 2009). We present descriptives on a number of covariates, nurse visits and outcomes. Importantly, the two groups are well-balanced across covariates. As expected, the average number of nurse visits they receive is different. The average number of universal (A-D) visits for treated children is 2.7 and 3.3 for control children. The share of children that miss each of the four visits is larger in the treated period. The difference in the number of universal visits across groups is identical to the difference in their total number of visits. This finding indicates that the average number of extra visits was not affected dramatically by the strike.

4.2 First Stage and Compliers

Figure 1 presents graphically the impact of strike exposure for the number of nurse visits for children in the treated and control year (2008 and 2009, respectively). Strike exposure impacted the number of nurse visits that children received. Panel (a) of Figure 1 shows that control children receive an average of 3.3 visits while treated children receive 2.7. Panel (b) shows the total number of visits (universal + extra) divided by treatment status. The

youngest strike exposed children appear to not only loose one but two nurse visits. This finding reflects that children on average receive two visits within the first 14 days of life - one universal visit and one extra visit. In section 4.5, we examine the robustness of our general conclusions to the omission of this group of children (a donut hole-approach).

To further examine the impact of the nurse strike on nurse visits and to confirm that the decrease of the number of visits is driven by visits at specific ages, Figure 2 shows the impact of strike exposure on the probability of missing a specific nurse visit. The graphs plot the probability of missing a nurse visit for children born in the 210 days before the strike for the years 2008 and 2009. In absence of strike, the share of children, who miss a specific nurse visit, is stable as indicated by the grey lines in Figure 2. 60 % of children born immediately before the strike miss the initial visit while all children older than approximately 20 days at strike start miss the initial visit with unaffected probability (20 %). Panels (b) and (c) show that missing the two and four month visits also is correlated with child age at strike. Finally, only the oldest children in our sample had an increased probability of missing the eight month visit while all the younger children were unaffected (because the strike ended by the time their visit was due).

Table 3 presents the described results from regressions based on equation 1. Coefficients are interpreted as the effect of being born in a specific bin on the probability of not receiving nurse visit k (the omitted baseline is the 30 days bin furthest from strike start). The columns show results for the different nurse visits: the initial visit, the two month visit, four month visit and eight month visit, respectively. Regression results mirror the graphical representation: The strike only had an impact on the initial visit for children who were between 30-0 days at strike start. On average children in this bin have 17.1 %-points higher probability of missing the initial visit. Children who were 90 days and below at strike start have an increased probability of a missed two month visit with the 60-31 bin most severely affected (51.1 %-points). Children who were between 61 and 150 days at strike start had their four month visit most severely affected by the strike. Only the oldest children in the strike ex-

posed period have increased probability (around 40 %-points) of a missed eight month visit compared to younger children. Children in the 30-1 day bin lost 0.267 nurse visits more than the reference group (significant at the 10 percent level). This result is driven by children below age two weeks at strike start (results available on request).

Having established that age at strike start has a meaningful impact on timing of missed nurse visits, we are concerned that nurses strategically chose the children they visited, i.e. that only the most well-off children were impacted by the child. This question is important for the interpretation of our findings. In general, the large scale of the strike—with only one tenth of performed nurse visits in Copenhagen during the strike relative to the default—suggests that the strike impacted large parts of the population. Additionally, our unique data allows us to characterize compliers (i.e. children who missed nurse visits due to the strike) in our sample.

Table 4 characterizes the compliers with respect to the probability of missing the inital nurse visit, Appendix Tables A8 through A10 consider the other three visits as outcomes. Following Angrist and Pischke (2008), we characterize the compliers by i) splitting the full sample into relevant subgroups, ii) estimating the model for each subgroup individually and iii) calculating the ratio between the coefficients from each subgroup and the full population. The ratios are the relative likelihood that a complier belongs to that particular subgroup. We look at the first stage estimates across groups of families defined by characteristics that may at least be partly observed by the nurses: child gender, parental education in health[17], initial child health[18] and child parity. In each of the tables, we show coefficients for the 30 day bins that were affected by the strike in the full population. In general, the complier analysis suggests that the strike affected all the considered subgroups relatively similarly. Nurses did not prioritize based on the given characteristics of the families during the strike. This finding is relevant for our interpretation of especially heterogeneous effects (because we can rule out

[17]Having parents with an educational background in health implies that either one of the parents are educated as doctor, midwife, nurse or pedagogue.

[18]We define a children of low initial health as having a birth weight below 2500g and/or being born preterm.

nurses' prioritizing of certain subgroups during the strike as an main driving factor).

It is important to note that all treated children miss on average one nurse visit and thus we cannot fully disentangle the effect of having one less nurse visit from the effects of timing. At the same time, our first stage results provide powerful evidence for the differential timing of the assigned treatment (one less visit). We think it is reasonable to interpret our findings as predominantly being driven by timing given that the different visits cover rather different topics, as outlined in section 2.

4.3 Main Results: Children's Health

To measure children' health we use outcomes measured in the administrative data. Given the sample size in combination with our empirical strategy, we cannot exploit the nurse registrations in this project.[19] Figures 3 presents graphical evidence of the relationship between age at strike start and GP contacts at ages one through five, our main measure of child health: the total number of GP contacts.[20] Black lines are for the treated period and gray lines are for the control periods.

The total number of accumulated GP contacts reveal a clear pattern. Children, who were 100 days or younger at strike start have significantly more GP contacts relative to older age groups and relative to the control group. Furthermore, there is a gradient for children below 100 days at strike start such that the younger the child, the more GP contacts. This finding may indicate that earlier NHV is relatively more important for child health. For children older than 100 days at strike start, the total number of GP contacts is similar to the

[19]During visits, nurses observe and record aspects of child and parental development that are not part of the administrative data. These observations are only observed for children who receive a specific visit. This fact poses a problem for our analysis, as we study the effects of missing a nurse visit at a specific age. To deal with this problem, we constrain our sample to children who received the eight month visit, i.e. were affected by the strike at younger ages. Specifically, we remove children born 210-151 days prior to the strike. Our constrained sample of children, who received the eight month visit allows us to use nurse registrations during the eight month visit as outcomes. These restrictions leave us with less 40 % of the unrestricted sample. Appendix Table A15 shows that our analysis sample is too small to make statements.

[20]WE have also looked at the same figures for regular and emergency contacts. Graphs are available on request. The figures show the raw relationship estimated with kernel weighted local polynomials. We use epanechnikov kernel and 42 (5-day) smoothing points through out.

control children. This suggests that the four month and eight month visits do impact future health care usage. Interestingly, the impact of missing an early nurse visit is persistent as the differences increase as the children ages. Turning to the decomposition of GP contacts in Appendix Figures **??** and **??**, both regular GP contacts and emergency GP contacts follow the same pattern as the total number of GP contacts.

Figure 4 and 5 present results from regressions based on equation (1): Figure 4 plots point estimates for first year child GP contacts along with confidence intervals.[21] In the first year, children that were between 30-1 days at strike start have 1.8 (37.5 %) GP contacts more compared to older age groups. For older age groups the differences are insignificant. The overall increase in all types of GP contacts is driven by both regular and emergency GP contacts. While the first-year effects may be a combination of both health effects and substitution, substitution is less likely to be a factor at age five (where results look very similar and are available on request), especially for emergency contacts.

An alternative way of inspecting our main results is presented in Figure 5: It shows point estimates for accumulated GP contacts for all years between year one and five in one combined graph. The effects on GP contacts increase as the child ages, in particular during the first two years of life. At age five, treated children have 5.4 (18.6 %) more GP contacts in total for the 30-1 bin, 3.0 (10.5 %) for the 60-31 bin and 2.3 (8.0 %) for the 90-61 bin[22]. For regular GP contacts the percentage effect is 15.5 % for the youngest age groups[23]. The percentage effects on emergency contacts are 25.0 %, 17.5 % and 15 % for the 30-1, 60-31 and 90-61 age groups. The relatively large effects on emergency contacts suggest actual health effects from the timing of NHV as these types of contacts are probably less driven by parental precautionary behavior.[24]

To further assess the importance of substitution toward GPs during the first year of life

[21] We have also looked at the equivalent point estimates for contacts accumulated by age five.

[22] The effect is statistically significant for the 90-61 bin

[23] For the 90-61 and 60-31 age bins we see no significant effect on the number of regular GP contacts.

[24] Appendix Tables A11 to A13 show all point estimates, standard errors and significance for the effects of strike exposure on total and decomposed accumulated GP contacts.

for our findings, Table 5 shows the estimates for yearly outcomes. i.e. child GP contacts measured in each year of life of the child. Across periods the estimated effects are significant and the pattern persists in all the considered sub-periods.

Our initial analyses suggest that the timing of NHV matters for child health. Earlier NHV is relatively more important than later visits. The overall treatment effects are averages across all subgroups and may mask important heterogeneity. By studying heterogeneous effects across parity of the child and parental health education[25], we attempt to shed some lights on mechanisms for the estimated health effects. Specifically, we hypothesize that first-time parents and parents without professional knowledge about child health and development may see larger effects due to the existence of an information channel. Research in several contexts has highlighted the potential impact of health knowledge for both the take-up of care and treatments, as well as patient health outcomes.

For brevity, we present results for our measure of accumulated total and emergency GP contacts at age five. We proceed by both splitting the sample into subgroups and by estimating a fully interacted model on the full sample (Appendix Tables A14). Table 6 presents our split sample results.[26]. In column (1)-(4) in Table 6, we show the results by parental health education. There are no significant effects of the timing of NHV for children of parents educated in a health-realted field. For children with parents *not* educated in those fields, effects resemble the effects found in the full population. As shown in Appendix Table A14, the effects are significantly different from each other. Firstborn children see larger effects of strike exposure and a larger gradient than non-firstborn children, as shown in column (5)-(8) in Table 6. The larger effects for firstborn children may also suggest an information channel.[27] Although different in magnitude, the effects by parity are not significantly different

[25]The group with parents educated in health include children who have at least one parent educated as either a medical doctor, midwife, nurse or pedagogue.

[26]Appendix Figure A5 shows the raw relationship between age at strike start and GP contacts divided by parental health education.

[27]That higher parity children still benefit from early nurse visits can be explained by two factors: i) nurses might update and refine the content of their visits in response to new knowledge on proper child development and/or ii) having raised a child once do improve parenting on average but not to a degree that eliminate the benefits from early nurse visits.

(see Appendix Table A14).[28]

Finally, we proceed to analyze additional outcomes: accumulated emergency GP contacts for mothers, maternal mental health (diagnosis of mental health problem within the first year after birth), child hospitalizations, and the probability of children delaying their school start. Table 7 shows that mothers with children who were below 90 days when the strike started (and had elevated risks of losing the initial and the 2-month visits) have 1 (30 %) more emergency GP contacts five years after birth. This health effect may be a mechanism or a reinforcing factor for the health effects found for their children. We find no significant effects on maternal mental health and child hospitalizations (indicator for hospitalization or the number hospital nights). Hospitalizations are a more extreme measure of health than GP contacts and the timing of NHV does not seem to affect that margin by age five. Finally, we study whether children have a higher probability of delaying school start in elementary school. In Denmark, children usually start in school in August the year they turn six years. We find no effects of the timing of strike exposure on the probability of delayed school start.

4.4 Parental Investments: Childhood Vaccinations and Preventive Care

An important mission of NHV is to promote parental investments in their children. Nurses provide information and guidance about issues such as other available health care services, appropriate interactions with children at different ages, and aspects such as sleep and child feeding. To study whether strike exposure impacts parental investment behaviors, we examine their participation in the childhood vaccination program and preventive care program as outcomes.

Almost 80 % of children receive all infant vaccinations and each round of vaccinations are

[28]In Appendix Tables A16-A17, we examine heterogeneity by gender, initial health, socio economic status (SES) and risky parental behavior (proxied by maternal smoking during pregnancy). Boys, children with poor initial health and children of parents with risky parental behavior are relatively more affected by the absence of early NHV. We see no difference in estimates by SES. None of the effects are significantly different as regressions with interactions in Appendix Tables A18-A19 show.

attended by 90 % of children in Copenhagen. According to the The Danish National Board of Health [Sundhedsstyrelsen] (2018) vaccinations remain important due to infection risks of unvaccinated children travelling abroad and foreign and potentially infected individuals visiting Denmark. Participation in the program is voluntary and the decision ultimately rests at the parents. The DNBH specifically mentions nurse visits as a central strategic element to promote the benefits of vaccinations to parents (The Danish National Board of Health [Sundhedsstyrelsen], 2018). The DNBH report highlights the close relationship between the families and their assigned nurse which facilitates dialogue if parents are in doubt or have chosen not to participate. We use the timing of the age at strike and the schedules of nurse visits and vaccinations to draw conclusions.

For the first vaccination round at 3 month and the second round at 5 month there are no significant impact of the age at strike start as shown in Table 8. This finding may be partly driven by the short spacing between the vaccinations and nurse visits. Children born just prior to the strike have an increased probability of missing the initial visits but unaffected two month, four month and eight month visits. Thus the children receive nurse visits both after and before the two first rounds of vaccinations.[29]

Furthermore, the timing of the first vaccines relatively soon after birth may impact the attentiveness of parents positively compared to later vaccinations. In line with this suggestion, children born 91-180 days prior to the strike receive 0.4 %-points fewer vaccinations in the third round. These children had elevated risks of missing the four month visit in particular and to a lesser extent the eight month visit. This finding may indicate that when the spacing between vaccination rounds are large, as is the case between the second and third round, an intermediate nurse visit may have a positive impact on vaccination participation. In Table 9, we use indicators for late vaccination as outcomes. While a missed nurse visit close to a vaccination round might cause a delay in vaccinations, we find no evidence of this

[29] A child born in the second bin 30-60 days prior to the strike would have larger probability of a missed two month visit but with an unaffected initial visits and four month visit and would still have plenty of interactions with nurses close to vaccination rounds. The same applies for older children.

suggestion. Taken together, we do not find strong conclusive evidence that the timing of NHV affects vaccination behavior. We interpret this finding with caution as our analysis suffers from power issues due to our small sample size and modest effect sizes.

Finally, Table 10 shows results for the effects of strike exposure on participation in the preventive care program at the GP. We find no effects on the probability of participation during the first year of life. For participation at age three, we find that children exposed to the strike at younger ages participate more in the preventive care program. Participation in the preventive care program at the GP may both be affected by health concerns and parental investments. Additionally, parents exposed to the strike at a crucial, young age of their child may build stronger ties to their GP (due to more contacts with the GP). Unfortunately, we cannot disentangle the different suggested channels.

4.5 Robustness Tests

Our results are robust to a number of changes to the main specification. For brevity, we only present robustness tests for one main outcome, the total accumulated GP contacts.[30] In Appendix Table A20, we omit control variables. The coefficients are of similar magnitude and significance but slightly less precisely estimated. We experiment with different bin sizes to check the sensitivity of our choice of a bin size of 30 days. The results are robust to changing the bin size to 35 and 21 days as presented in Appendix Tables A21 and A22. In Appendix Table A23, we include children born in one earlier period prior to the treated children as part of the control group. We omit these children in the main analysis because control period children may be treated to some degree as they were born during the strike. All results are robust to this change. As our first stage results are only estimated with data from the years 2008 and 2009, we test the robustness of our reduced form effects to only using those years. Appendix Table A24 shows that our results are robust to this sample restriction.

A further robustness test relates to our main outcome measure of health. The accumulated

[30] Robustness checks with other outcomes are available on request.

number of GP contacts may be a flawed measure of health as it only relates to the number of contacts and not the severity. A single GP contact can be the subject of multiple health issues of varying severity. Thus we run our main regressions with the accumulated GP fees as outcome. GP fees likely increase when the complexity of the GP contact increases. Appendix Table 11 presents the results. The estimates display the same patterns as shown for the number of GP contacts.

Two additional robustness test relate to the movers from Copenhagen and the children born very close to the start of the strike. First, we include movers into the sample. Our results remain robust also when we include movers (Appendix Table A25). Second, we use a donut hole approach where we drop children born within 14 days of strike start. As discussed, those children were likely to loose more than one visit on average. Our main conclusions from this analysis—that earlier strike exposure is relative more important for health—remain intact.

5 Costs and Benefits

In this section, we perform a stylized analysis of the costs and benefits of early NHV (relative to later NHV). Specifically, we estimate the benefits of receiving an initial and a two month visit. We relate health benefits to an estimate of the costs of one visit. Our estimated benefits for the earliest visits are relative to the benefits of the four month and eight month visits. We use the results from the impact of strike exposure on the total GP fees at age 5[31]. We disregard longer-run effects and spill-overs. As a consequence, our measure of benefits is a conservative estimate.

The 30-1 day age group (relative to strike start) missed the initial visit and the two month visit with 17.1 and 32.3 %-points larger probabilities, respectively. The 60-31 day age group missed only the two month visit with 0.511 %-points larger probability. To estimate the benefit in DKK of the two month visit, we divide the increase in GP fees for the 60-31 age group by the increase in the risk of losing the 2-month visit caused by the strike, 373.7/0.511

[31] Appendix Table 11

= 731.3 DKK. To estimate the benefit of the initial visit, we use our estimates and the benefit of the two month visit, (686.0 - 731.3·0.323)/0.171 = 2630.4 DKK[32]. Not receiving an initial visit increases GP fees with 2630.4 DKK at age 5 and not receiving the two month visit increases GP fees with 731.3 DKK at age 5.

As starting point to estimate the cost of a nurse visit, we use the 240,000DKK daily savings for the municipality due to the strike. Approximately, 55 % of striking workers were nurses. The daily savings from the striking nurses were $0.55 \cdot 240,000 = 132,000$ DKK. or 660,000 DKK per (business) week. During the full 7 weeks of the strike, 85 weekly nurse visits were conducted. In the same weeks the following year, the weekly average of visits was 836. We assume the difference $(845 - 85 = 760)$ as the number of canceled visits caused by the strike. Our estimate of the cost of one visit is then (660,000 DKK per week)/(760 visits per week) = 868.4 DKK per visit. We abstract from any other fixed[33] and variable costs beyond salaries to nurses.[34]

Assuming that the four month and eight month visits have zero benefits then at age 5 the two month visit has a return of 731.3 - 868.4 = - 137.3 DKK. In all likelihood the return is bound to become positive when viewed at a larger time horizon and when other benefits are taken into account. Similarly, the initial visit has a return of 2630.4 - 868.4 = 1762 DKK. This represents a substantial return as we only included GP fees savings at age 5 and under the fairly conservative assumption that the four month and eight month visits have zero benefits.

Previous estimates of the cost-effectiveness of NHV programs do not consider the timing but the program in its entirety. Olds et al. (1999) cite an economic evaluation from The RAND Cooperation. They find no net savings to society from NHV to high SES families while NHV to low SES families generate savings from society four times larger than the

[32]We use that the change in GP fees for the 30-1 age group equals the weighted average of the benefits of the initial and the two month visits, with the increased probability of missing those visits as weights.

[33]Examples of fixed costs are the education of nurses, capital (cars, building stock and software).

[34]Variable costs beyond salaries to nurses are management costs, cleaning services, transportation, lunch and coffee among others.

cost of NHV over the lifetime. Paul et al. (2004) estimate the cost-effectiveness of an early nurse home visit[35]. First, they find that a home visit significantly reduces subsequent health care usage. Second, using average reimbursements for health care services, they estimate an incremental cost-effectiveness ratio (ICER) of 181.82 USD within the first 10 days of life.

Evidence on the cost-effectiveness of other types of early-life health policies are mixed. Walker et al. (1984) estimate that the costs for intensive neonatal care for very low birth weight children[36] are higher than the benefits and conclude that targeted intensive neonatal care might not be justifiable.

Our simple analysis indicates that early-life NHV is a cost-effective intervention. We show that the cost of NHV for the early visit is considerably less than the associated health care savings at age 5. Furthermore, taking our regression results into consideration it seems plausible that savings will increase over time. Moreover, early-life health will likely spillover to other dimensions of well-being and future productivity increasing the savings for society more. For the four month and eight month visits, we do not find isolated evidence of positive health effects. This could potentially imply that the cost of these visits are larger than the benefits.

6 Conclusion

Using nurse records and linked administrative data together with exogenous variation in the timing of foregone NHV visits, we add to the literature on the effects of NHV: by studying the intensive margin of a universal program we provide new, policy relevant evidence on the effects of NHV.

We find that the timing of NHV impacts child health (measured as health care usage). Specifically, our results show that nurse visits scheduled within the first months of a child's life have longer term effects and earlier visits are more important for children's health than

[35] Visits in this program was mandated within 48 hours after discharge from hospital for children discharged $<$ 48 hours after birth.

[36] Children with birth weight between 500 and 999 grams.

later visits. We estimate that strike exposure in the first month of life causes a 25% increase of emergency GP contacts at age 5, while exposure during the second and third month of life cause increases of 17.5% and 15%, respectively. The results are not driven by short-term substitution between nurse and GP. Furthermore, we find the largest effects when we use emergency GP contacts as outcome compared to regular GP contacts. This suggests actual health effects and not exclusively increased precautionary behavior or a better relationship with the family GP.

We explore parental investments and heterogeneous effects to detect mechanisms. We cannot detect impacts on participation in the childhood vaccination and preventive care program. We find that health effects are significantly larger for first-borns and for children with parents without an education in a health-related field. Maternal GP contacts display similar effects as for the children, suggesting a further potential mechanism.

Our results show that universal NHV impacts health development and that the timing of treatment matters. This finding points to benefits of early NHV in settings that resemble the Danish one.

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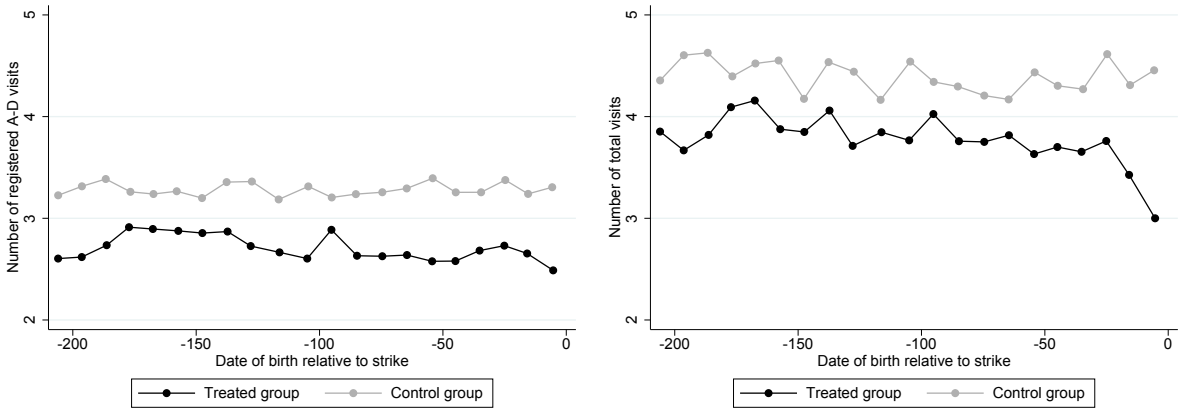
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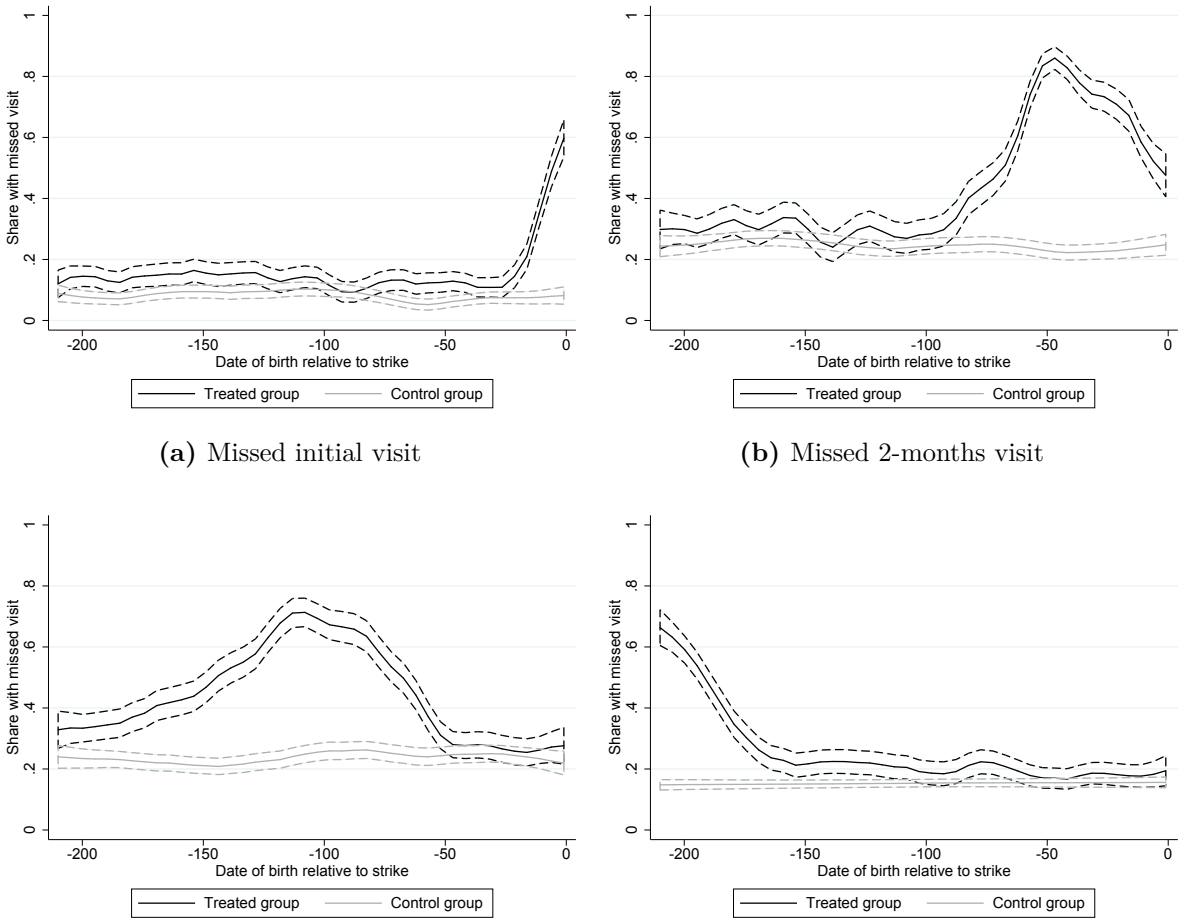
7 Figures and Tables



(a) Universal nurse visits

(b) Total nurse visits (universal + extra)

Fig. 1 Average number of universal and total nurse home visits for children in Copenhagen in the treated (2008) and control periods (2009).



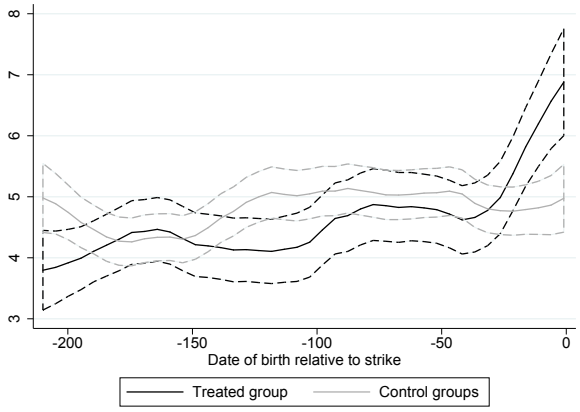
(a) Missed initial visit

(b) Missed 2-months visit

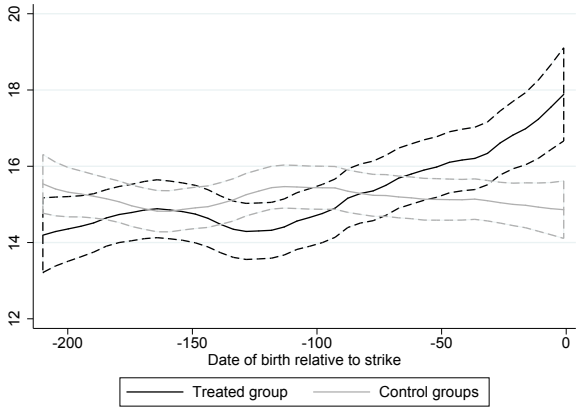
(c) Missed 4-months visit

(d) Missed 8-months visit

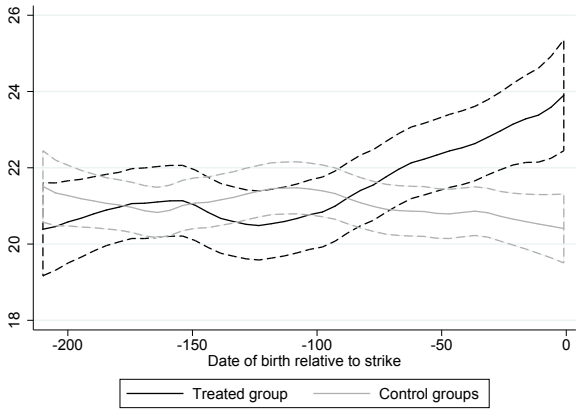
Fig. 2 Share of children with missed nurse visits for children born in the treated and control period. Notes: The vertical lines indicate the groups of children, who had the relevant age for the respective nurse visit during the strike period. Treated period: September 18, 2007 - April 15, 2008. Control periods: September 17, 2008 and 2009 - April 15, 2009 and 2010).



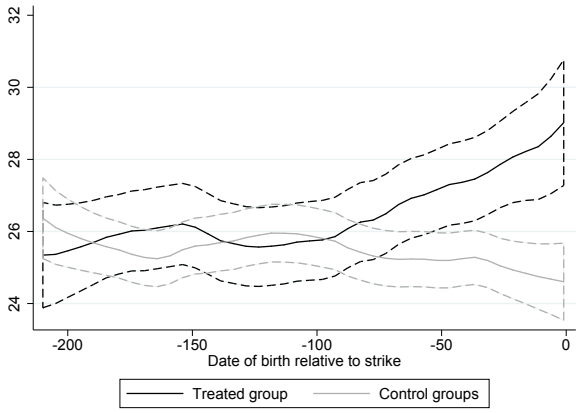
(a) Total GP contacts - one year



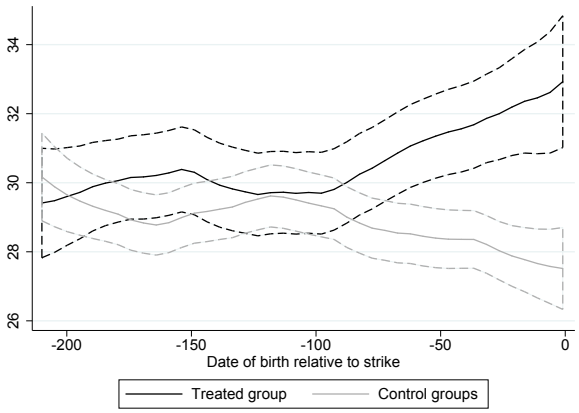
(b) Total GP contacts - two years



(c) Total GP contacts - three years

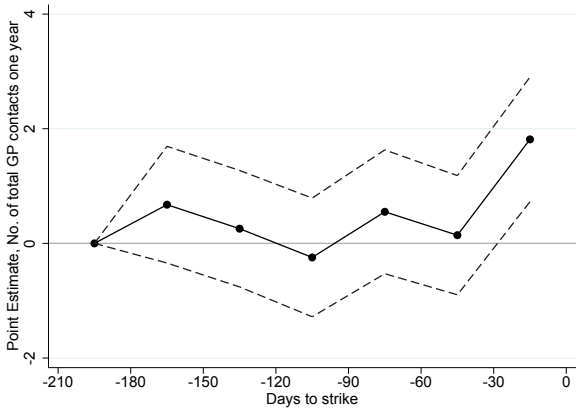


(d) Total GP contacts - four years

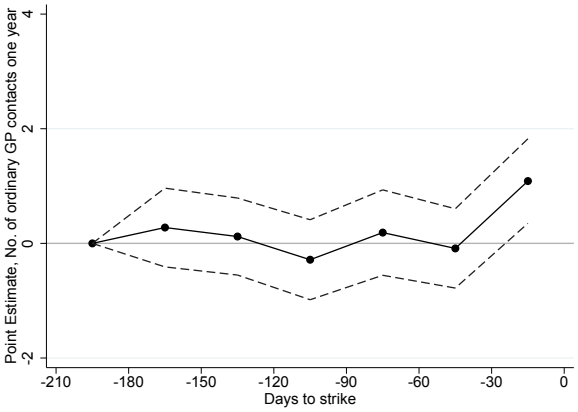


(e) Total GP contacts - five years

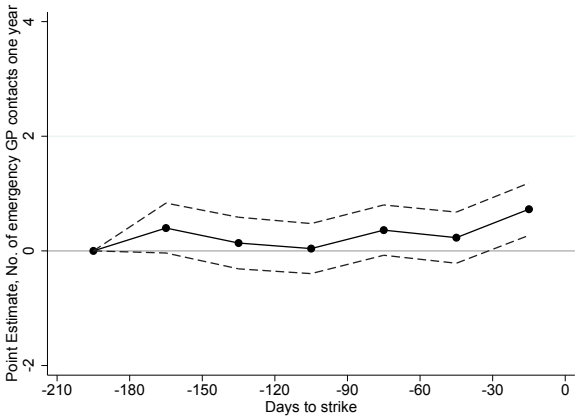
Fig. 3 Accumulated number of GP contacts for children born in the treated (2008) and control periods (2009, 2010).



(a) First year total GP contacts



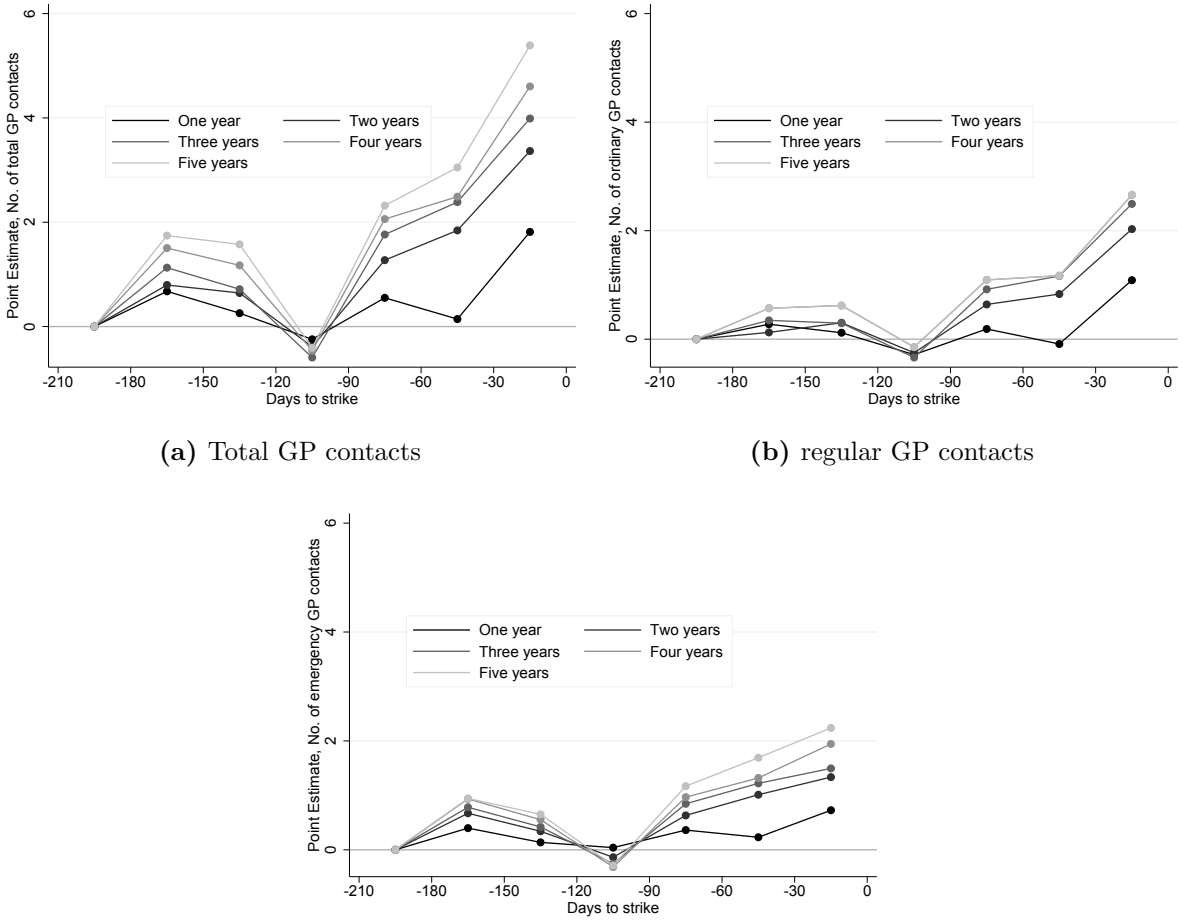
(b) First year regular GP contacts



(c) First year emergency GP contacts

Fig. 4 Coefficients plot: Total number of first-year GP contacts (a) and decomposed in regular (b) and emergency contacts (c).

Notes: Each figure plots the coefficients Ω_τ from estimating equation (1) on the outcomes indicated by the labels in the panels. The coefficients are for the interactions of 30 day bins and a strike indicator. All regressions include period and bin fixed effects, as well as control variables (paternal and maternal income, indicators for primary school as highest level of education, higher education as highest level of education, university degree as highest level of education, currently studying, employed, parents cohabiting, parents married and indicators for missing covariates all measured a year prior to birth and indicators for parents below 21, C-section, home birth, low birth weight (below 2500g), preterm (below 37 weeks), child gender, number of pre-natal midwife visits and whether the mother smokes at the time of birth). The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Dashed lines are confidence intervals for significance at the 5 % level (robust standard errors).



(a) Total GP contacts

(b) regular GP contacts

(c) Emergency GP contacts

Fig. 5 Coefficients plot: Total GP contacts (a) and decomposed in regular (b) and emergency contacts (c).
Notes: See notes to figure 4.

Table 1 NHV in the municipality of Copenhagen, visits and content.

Visit	Main focus
Universal visits	
Initial visits (4-6 days, 2 weeks)	Postpartum health, feeding, health risks, parent-child-interaction
2-month visit	Monitoring, feeding, sleep, info on preventive care offer (GP)
4-month visit	Intro of solid foods, child development
8-month visit	BOEL test, parent-child interactions, childcare
Targeted visits	
Pregnancy visit (week 30)	Contact to at-risk families
Visits based on "needs"	Risk factors, identified issues
1,5- and 3-year visits	Follow-up for at-risk/on demand

Source: Information from the municipality of Copenhagen and guidelines (Danish National Board of Health, 2007).

Table 2 Summary statistics for the analysis sample of children born in Copenhagen, means and standard deviations.

	Mean	Sd.	Obs.
Total GP < 1y	4.740	7.642	12806
Total GP < 5y	29.418	19.809	12345
Ordin. GP < 1y	3.225	5.080	12806
Ordin. GP < 5y	20.248	12.747	12345
Emerg. GP < 1y	1.515	3.334	12806
Emerg. GP < 5y	9.170	9.782	12345
Vacc., 1st round	0.880	0.326	12806
Vacc., 2nd round	0.896	0.305	12806
Vacc., 3rd round	0.899	0.301	12806
Prev. care, 5 weeks	0.906	0.293	12806
Prev. care, 5 months	0.925	0.263	12806
Prev. care, 12 months	0.930	0.254	12806
Midwife visits	4.766	1.528	12477
Smoking status, Mother	0.091	0.288	12601
Child sex	0.481	0.500	12806
Low birth weight	0.052	0.223	12607
Preterm birth	0.062	0.240	12601
C-section	0.213	0.409	12806
Home birth	0.011	0.104	12806
Cohabiting	0.773	0.419	12806
Married	0.387	0.487	12806
Prim. school, mother	0.128	0.334	12806
Uni. degree, mother	0.311	0.463	12806
Student, mother	0.053	0.224	12806
Employed, mother	0.768	0.422	12806
Danish, mother	0.749	0.434	12806
Young mother	0.024	0.152	12806
Young father	0.008	0.091	12565
Income, mother	267.858	203.829	12806
No. of nurse visits	4.089	1.964	8350
Number of registered A-D visits	2.999	1.097	8350
No initial visit	0.119	0.324	8350
No 2-month visit	0.342	0.474	8350
No 4-month visit	0.335	0.472	8350
No 8-month visit	0.205	0.403	8350

Notes: The top panel presents summary statistics for the full sample of children (data periods from the years 2008-2010). The bottom panel presents summary statistics based on the nurse records for the years 2008 and 2009 as the 2010 records are not complete. For at detailed description see 4.

Table 3 First stage: Effects of strike exposure on the probability of a missed nurse visit

	(1) No initial visit	(2) No 2-month visit	(3) No 4-month visit	(4) No 8-month visit	(5) Number of registered A-D visits	(6) No. of nurse visits
Days						
180-151	0.002	-0.040	0.100***	-0.324***	0.261***	0.223
	(0.026)	(0.037)	(0.037)	(0.034)	(0.091)	(0.166)
150-121	0.003	-0.018	0.247***	-0.357***	0.126	0.205
	(0.026)	(0.037)	(0.037)	(0.034)	(0.090)	(0.162)
120-91	-0.027	-0.017	0.364***	-0.363***	0.043	0.181
	(0.026)	(0.037)	(0.037)	(0.033)	(0.088)	(0.164)
90-61	-0.007	0.155***	0.225***	-0.353***	-0.020	0.247
	(0.025)	(0.038)	(0.038)	(0.034)	(0.087)	(0.163)
60-31	-0.005	0.511***	-0.039	-0.423***	-0.044	0.115
	(0.024)	(0.035)	(0.036)	(0.033)	(0.083)	(0.153)
30-1	0.171***	0.323***	-0.079**	-0.395***	-0.019	-0.267*
	(0.028)	(0.037)	(0.036)	(0.033)	(0.085)	(0.158)
Obs.	7874	7874	7874	7874	7874	7874

Notes: Each column shows the estimates from a separate regression. The coefficients are for the interactions of 30 day bins and a strike year indicator. All regressions include period and bin fixed effects, as well as control variables (paternal and maternal income, indicators for primary school as highest level of education, higher education as highest level of education, university degree as highest level of education, currently studying, employed, parents cohabiting, parents married and indicators for missing covariates all measured a year prior to birth and indicators for parents below 21, C-section, home birth, low birth weight (below 2500g), preterm (below 37 weeks), child gender, number of pre-natal midwife visits and whether the mother smokes at the time of birth). The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control period (September 17, 2008 - April 15, 2009). Columns (1)-(4) have each of the four universal nurse visits as outcome, the outcome in column (5) is the number of universal nurse visits and the total number of nurse visits (universal + extra visits) is outcome in column (6). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table 4 Compliers: Effects of strike exposure on the probability of missing the initial visit by subgroup

	Gender		Education		Initial health		Parity	
	Boys	Girls	Not health	Health	Not poor	Poor	>1	=1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
30-1	0.199***	0.130***	0.151***	0.217***	0.157***	0.237**	0.132***	0.194***
	(0.040)	(0.039)	(0.032)	(0.059)	(0.029)	(0.117)	(0.046)	(0.035)
Ratio to full pop.	1.19	0.78	0.90	1.29	0.94	1.41	0.78	1.15
Control group mean	0.09	0.07	0.08	0.09	0.08	0.11	0.10	0.07
Observations	4101	3773	6156	1718	7276	598	3026	4848

Notes: See notes to table 3. In this table, we show estimates for the interactions of 30 day bins and a strike indicator from separate regressions for various sub groups along with the relative coefficients between the full sample estimates and the various sub group estimates. We only show the estimates for the 30-1 day bin because only children in this bin had their initial visit affected by the strike in the full population. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table 5 Effects of strike exposure on child health: Yearly GP contacts by type

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-5 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-5 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-5 years
Days									
180-151	0.675	0.120	0.976	0.277	-0.152	0.661	0.399*	0.272	0.315
	(0.519)	(0.515)	(0.771)	(0.350)	(0.319)	(0.514)	(0.222)	(0.296)	(0.386)
150-121	0.256	0.387	0.895	0.119	0.184	0.591	0.137	0.204	0.303
	(0.518)	(0.508)	(0.775)	(0.343)	(0.319)	(0.510)	(0.230)	(0.290)	(0.388)
120-91	-0.245	-0.169	-0.037	-0.285	0.017	0.151	0.040	-0.187	-0.187
	(0.528)	(0.492)	(0.718)	(0.356)	(0.310)	(0.499)	(0.223)	(0.283)	(0.351)
90-61	0.551	0.693	1.093	0.188	0.441	0.542	0.362	0.252	0.552
	(0.551)	(0.512)	(0.743)	(0.380)	(0.329)	(0.510)	(0.224)	(0.284)	(0.369)
60-31	0.143	1.703***	1.135	-0.088	0.922***	0.496	0.231	0.782***	0.638*
	(0.530)	(0.508)	(0.739)	(0.353)	(0.321)	(0.490)	(0.228)	(0.291)	(0.374)
30-1	1.814***	1.610***	2.135***	1.087***	0.975***	1.199**	0.727***	0.635**	0.937**
	(0.555)	(0.519)	(0.763)	(0.376)	(0.332)	(0.518)	(0.233)	(0.288)	(0.371)
Control group mean	4.80	10.35	13.63	3.26	6.89	9.85	1.54	3.46	3.78
Obs.	12078	11982	11604	12078	11982	11604	12078	11982	11604

Notes: See notes to table 3. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table 6 Effects of strike exposure on child health: Heterogeneity of results for accumulated total and emergency GP contacts at age five

	Not health educ.		Health educ.		Higher parity		First-borns	
	Total GP < 5y	Emerg. GP < 5y	Total GP < 5y	Emerg. GP < 5y	Total GP < 5y	Emerg. GP < 5y	Total GP < 5y	Emerg. GP < 5y
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
180-151	2.610	1.327	-0.621	-0.070	-0.472	-0.122	3.111	1.623*
	(1.615)	(0.816)	(3.161)	(1.510)	(2.138)	(1.109)	(1.918)	(0.943)
150-121	1.902	0.780	0.435	0.025	-0.799	0.120	3.107*	1.033
	(1.573)	(0.783)	(3.335)	(1.680)	(2.224)	(1.178)	(1.875)	(0.921)
120-91	0.308	0.098	-3.626	-1.972	-1.193	-0.688	0.454	0.094
	(1.521)	(0.753)	(3.237)	(1.516)	(2.163)	(1.085)	(1.792)	(0.869)
90-61	3.763**	1.791**	-3.281	-1.355	1.427	0.509	2.471	1.386
	(1.601)	(0.774)	(3.237)	(1.482)	(2.073)	(1.036)	(1.940)	(0.914)
60-31	3.463**	2.111***	0.752	-0.146	2.197	0.832	3.553*	2.333**
	(1.606)	(0.815)	(3.180)	(1.499)	(2.087)	(1.056)	(1.952)	(0.977)
30-1	7.511***	3.295***	-0.995	-1.072	4.253*	1.715	6.139***	2.623***
	(1.710)	(0.830)	(2.952)	(1.350)	(2.182)	(1.080)	(1.974)	(0.945)
Control group mean	29.46	9.03	27.04	8.13	25.09	7.61	31.50	9.64
Obs.	9037	9037	2615	2615	4579	4579	7073	7073

Notes: See notes to table A11. The label of each column indicate which subgroup that we use in the regressions and which outcome. Columns (1)-(4) split the sample by parental health education background. Having parents with an educational background in health implies that either one of the parents are educated as doctor, midwife, nurse or pedagogue. Columns (5)-(8) split the sample by parity. A parity larger than one means that the mother gave birth prior the child in question, while a parity of one means that the child is the first born to the mother. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table 7 Additional outcomes: Effects of strike exposure on hospitalizations, mothers GP contacts and the probability of delayed school start

	(1) Emerg. GP < 1y mothers	(2) Emerg. GP < 5y mothers	(3) Hospital adm. < 1y	(4) Hospital adm. < 5y	(5) Hospital nights < 1y	(6) Hospital nights < 5y	(7) Mental diagnosis < 1y mothers	(8) Delayed school start
Days								
180-151	0.052	0.097	-0.002	-0.007	0.306	1.005	-0.008	0.010
	(0.104)	(0.342)	(0.023)	(0.036)	(0.510)	(0.693)	(0.008)	(0.029)
150-121	0.166	0.256	0.005	-0.016	0.246	0.391	0.001	-0.021
	(0.109)	(0.336)	(0.023)	(0.036)	(0.368)	(0.541)	(0.008)	(0.032)
120-91	-0.062	-0.016	0.009	0.007	0.255	0.689	-0.001	0.037
	(0.102)	(0.340)	(0.023)	(0.036)	(0.445)	(0.647)	(0.009)	(0.030)
90-61	0.122	0.912***	0.024	0.010	0.829	1.511**	0.003	-0.020
	(0.097)	(0.342)	(0.023)	(0.036)	(0.539)	(0.738)	(0.009)	(0.023)
60-31	0.158	1.038***	0.005	0.019	-0.090	1.344	-0.000	-0.010
	(0.124)	(0.376)	(0.023)	(0.035)	(0.453)	(0.831)	(0.008)	(0.024)
30-1	0.132	0.915***	0.013	0.038	-0.285	0.084	0.002	-0.012
	(0.099)	(0.337)	(0.023)	(0.036)	(0.323)	(0.534)	(0.009)	(0.025)
Control group mean	0.57	3.24	0.13	0.43	1.39	2.58	0.01	0.14
Obs.	12078	11652	12078	11652	12078	11652	12078	7874

Notes: See notes to table 3. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table 8 Parental investments: Effects of strike exposure on participation in the infant vaccination program

	(1) Vacc., 1st round	(2) Vacc., 2nd round	(3) Vacc., 3rd round
Days			
180-151	-0.023	-0.015	-0.036
	(0.025)	(0.023)	(0.022)
150-121	-0.004	-0.032	-0.038*
	(0.024)	(0.023)	(0.022)
120-91	0.014	-0.009	-0.044**
	(0.024)	(0.023)	(0.023)
90-61	-0.010	-0.010	-0.020
	(0.025)	(0.024)	(0.023)
60-31	-0.018	-0.026	0.017
	(0.024)	(0.023)	(0.022)
30-1	0.007	0.001	-0.032
	(0.024)	(0.023)	(0.022)
Control group mean	0.89	0.91	0.91
Obs.	12078	12078	12078

Notes: See notes to table 3. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table 9 Parental investments: Effects of strike exposure on the probability of dealyed infant vaccinations

	(1) Vacc. 1st round, 2 month late	(2) Vacc. 2nd round, 2 month late	(3) Vacc. 3rd round, 2 month late
Days			
180-151	0.032	0.006	0.025
	(0.028)	(0.028)	(0.030)
150-121	0.002	-0.005	-0.010
	(0.028)	(0.028)	(0.031)
120-91	0.012	0.009	0.034
	(0.028)	(0.029)	(0.031)
90-61	0.024	-0.002	-0.005
	(0.028)	(0.029)	(0.031)
60-31	0.041	0.016	-0.016
	(0.028)	(0.028)	(0.030)
30-1	0.004	-0.022	-0.011
	(0.028)	(0.028)	(0.030)
Control group mean	0.15	0.15	0.22
Obs.	12078	12078	12078

Notes: See notes to table 3. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table 10 Parental investments: Effects of strike exposure on participation in preventive health checks first year of life

	(1) Prev. care, 5 weeks	(2) Prev. care, 5 months	(3) Prev. care, 12 months	(4) Prev. care, 2 years	(5) Prev. care, 3 years	(6) Prev. care, 4 years
Days						
180-151	0.003	0.005	0.006	0.064*	0.055	0.033
	(0.022)	(0.019)	(0.018)	(0.034)	(0.035)	(0.030)
150-121	0.008	-0.008	0.012	0.042	0.035	0.006
	(0.021)	(0.018)	(0.019)	(0.034)	(0.035)	(0.031)
120-91	-0.009	-0.008	-0.008	0.010	-0.034	-0.014
	(0.022)	(0.019)	(0.019)	(0.035)	(0.036)	(0.031)
90-61	0.015	0.004	0.012	0.106***	0.099***	0.041
	(0.021)	(0.020)	(0.018)	(0.034)	(0.036)	(0.031)
60-31	0.017	-0.014	0.029*	0.034	0.090***	0.018
	(0.021)	(0.019)	(0.018)	(0.033)	(0.034)	(0.030)
30-1	0.012	0.001	0.017	0.056	0.082**	0.035
	(0.020)	(0.019)	(0.018)	(0.034)	(0.035)	(0.030)
Control group mean	0.92	0.93	0.93	0.66	0.58	0.79
Obs.	12078	12078	12078	11982	11832	11729

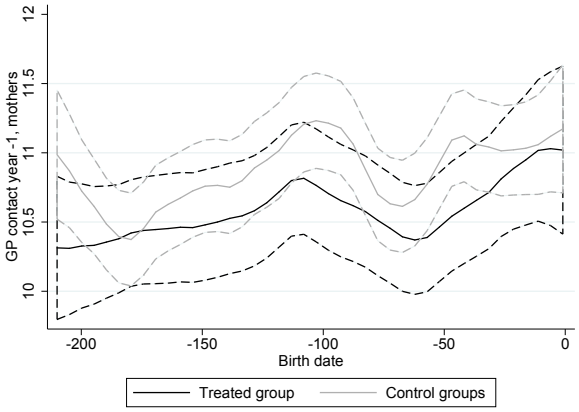
Notes: See notes to table 3. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table 11 Effect of strike exposure on child health measured as accumulated total GP fees, DKK

	(1) GP fees < 1y	(2) GP fees < 2y	(3) GP fees < 3y	(4) GP fees < 4y	(5) GP fees < 5y
Days					
180-151	131.057*	156.086	221.927	272.810*	300.999*
	(75.897)	(109.812)	(137.029)	(158.696)	(176.267)
150-121	25.796	82.347	122.477	162.286	213.582
	(74.594)	(107.394)	(134.278)	(155.112)	(172.749)
120-91	-51.450	-58.314	-62.301	-76.505	-58.729
	(76.370)	(106.809)	(131.586)	(152.691)	(168.052)
90-61	44.158	154.661	239.763*	268.274*	309.139*
	(77.204)	(111.202)	(136.041)	(156.400)	(173.136)
60-31	4.649	250.336**	309.883**	304.765*	373.702**
	(75.183)	(109.950)	(136.398)	(156.661)	(174.195)
30-1	257.400***	454.855***	527.665***	589.648***	686.044***
	(78.428)	(111.679)	(137.556)	(159.102)	(177.279)
Control group mean	723.84	2083.54	2834.09	3414.89	3851.93
Obs.	12078	11982	11832	11729	11652

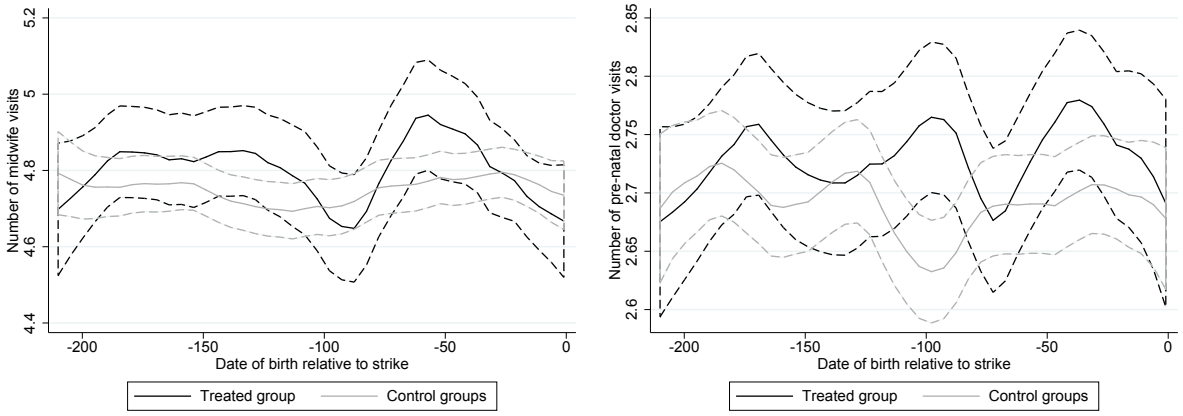
Notes: See notes to table 3. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

A Appendix - For online publication



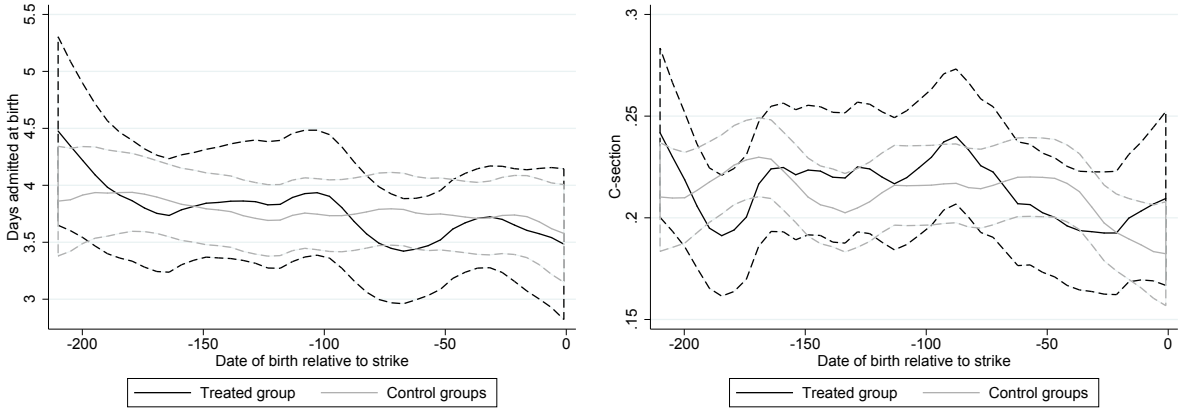
(a) Mothers GP contacts, 365 days prior birth

Fig. A1 Number of mothers' GP contacts in the year prior to birth.



(a) Midwife visits

(b) Pre-natal GP contacts



(c) Days admitted at birth

(d) C-sections

Fig. A2 Care around birth for the treated period and control periods.
Notes: Panel (a) shows the number of pre-natal midwife contacts, panel (b) shows the number of pre-natal GP consultations, panel (c) shows the number of days admitted to hospital at birth and panel (d) shows the C-section rate. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010).

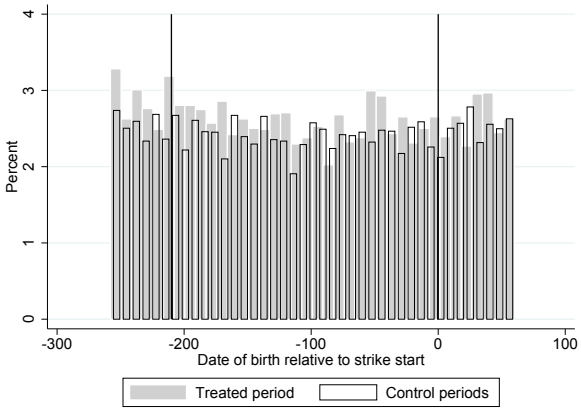


Fig. A3 Density of births

Notes: The figures show the density of births for 20 equally sized bins and a window 258 days prior to the beginning of the strike and 60 after the beginning of the strike. Grey bars are the strike exposed period and bars with black outline are children born on same dates the two following years. The vertical lines indicate the data period of our main analyses (treated period: September 18, 2007 - April 15, 2008 and control periods: September 17, 2008 and 2009 - April 15, 2009 and 2010).

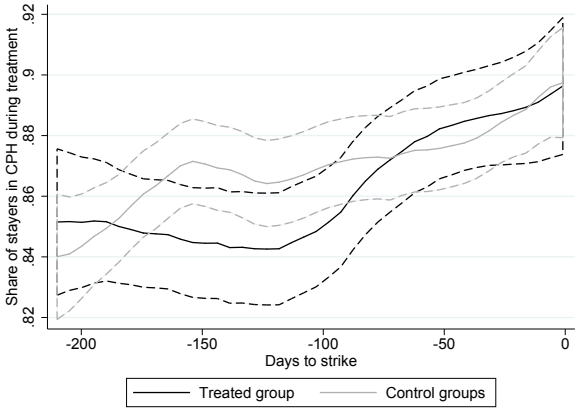
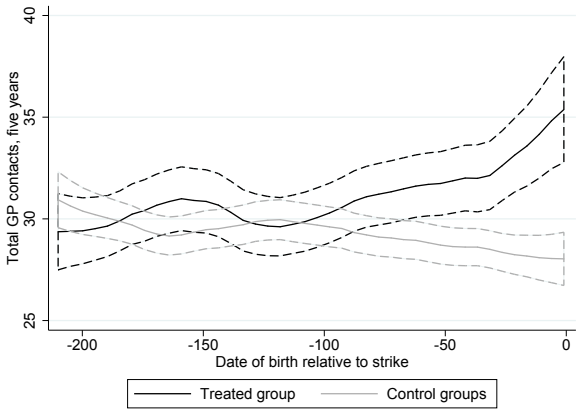
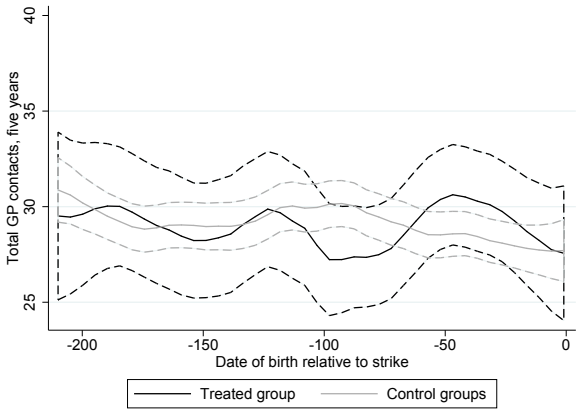


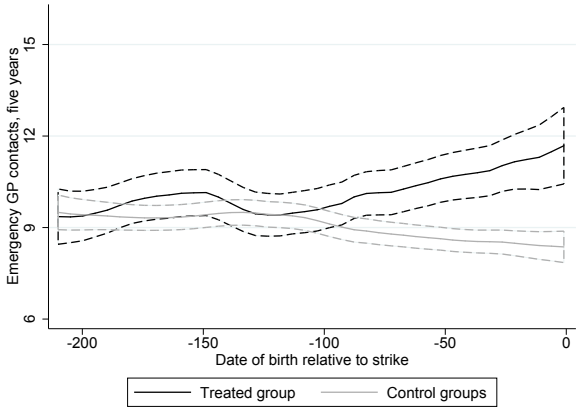
Fig. A4 Share of children observed as Copenhagen residents on January 1 in the treated (2008) and control periods (2009, 2010).



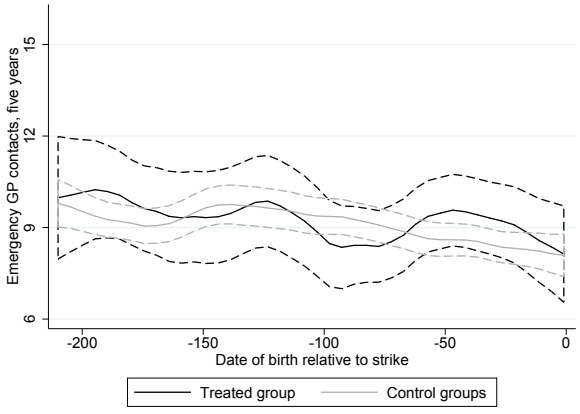
(a) Total GP contacts - Not Health educated



(b) Total GP contacts - Health educated



(c) Emergency GP contacts - Not health educated



(d) Emergency GP contacts - Health educated

Fig. A5 Accumulated GP contacts at age five by treatment status and whether parents are educated in health.

Table A1 Nurse home visiting in the municipality of Copenhagen

Visit (and type)	Details (timing, requirements etc.)
Universal visits	
Initial visit	0-14 days after birth
2-month visit	After two month of life
4-month visit	After four month of life
8-month visit	After eight month of life
Visits on parental demand	
Pregnancy visit	30th week of gestation
Maternity visit	Immediately after birth. Home births and early discharge
1,5-year visit	1,5 years after birth
3-year visit	3 years after birth
Targeted offer (at-risk families)	
Extra home visits	Depending on nurse recommendation

Source: Official guidelines of the municipality of Copenhagen.

Table A2 Balance testing: Parental covariates as outcome

	Prim. school, mother	Prim. school, father	Income, mother	Income, father	Cohabiting	Married	Young mother	Young father
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Days								
180-151	-0.012	-0.023	-8.567	-123.961	0.034	-0.028	0.013	0.008
	(0.024)	(0.024)	(10.197)	(140.780)	(0.031)	(0.032)	(0.011)	(0.007)
150-121	-0.021	-0.001	-1.703	-137.039	-0.008	-0.025	0.004	0.004
	(0.024)	(0.025)	(9.999)	(140.867)	(0.031)	(0.032)	(0.010)	(0.007)
120-91	0.008	-0.039*	10.751	-113.671	0.045	-0.015	-0.012	-0.002
	(0.025)	(0.023)	(10.408)	(141.624)	(0.031)	(0.033)	(0.011)	(0.006)
90-61	0.021	0.007	-1.872	-115.782	0.046	0.017	0.014	0.007
	(0.025)	(0.024)	(10.817)	(140.864)	(0.029)	(0.034)	(0.011)	(0.006)
60-31	-0.034	-0.010	-2.525	-107.583	0.050*	-0.029	0.011	0.008
	(0.024)	(0.024)	(10.205)	(140.496)	(0.029)	(0.032)	(0.010)	(0.006)
30-1	-0.014	-0.034	11.237	-86.723	0.034	-0.015	0.015	-0.003
	(0.024)	(0.023)	(28.824)	(140.922)	(0.029)	(0.033)	(0.011)	(0.006)
Obs.	12568	12568	12568	12568	12568	12568	12568	12332

Notes: Each column shows the estimates from a separate regression. The coefficients are for the interactions of 30 day bins and a strike indicator. All regressions include period and bin fixed effects. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A3 Balance testing: Covariates at birth as outcome

	Hosp. nights at birth (1)	Midwife contacts (2)	C-section (3)	Home birth (4)	Preterm birth (5)	Low birth weight (6)	Head size (7)	Male child (8)
Days								
180-151	-0.912	-0.018	-0.009	0.000	-0.003	-0.019	0.088	0.025
	(0.640)	(0.110)	(0.030)	(0.002)	(0.018)	(0.017)	(0.129)	(0.035)
150-121	-0.308	0.105	-0.005	0.003	-0.034**	-0.019	0.018	0.060*
	(0.636)	(0.108)	(0.029)	(0.004)	(0.017)	(0.016)	(0.137)	(0.035)
120-91	-0.716	0.023	-0.003	-0.002	-0.033*	-0.040**	-0.070	0.038
	(0.716)	(0.116)	(0.030)	(0.002)	(0.018)	(0.016)	(0.129)	(0.036)
90-61	-0.703	-0.004	0.010	-0.000	-0.023	-0.021	-0.039	0.047
	(0.624)	(0.118)	(0.030)	(0.004)	(0.017)	(0.015)	(0.123)	(0.036)
60-31	-0.675	0.090	-0.021	0.001	-0.019	-0.011	0.027	0.071**
	(0.644)	(0.116)	(0.028)	(0.003)	(0.017)	(0.016)	(0.127)	(0.035)
30-1	-0.627	-0.083	-0.001	-0.003	-0.037**	-0.022	0.249*	0.058
	(0.638)	(0.103)	(0.029)	(0.003)	(0.016)	(0.015)	(0.137)	(0.035)
Obs.	12537	12409	12568	12568	12518	12515	12332	12568

Notes: See notes to Table **??**. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A4 Placebo test: The effect of strike exposure on child health measured as accumulated GP contacts by type, data for the two control years 2009 and 2010

	(1) Total GP < 5y	(2) Emerg. GP < 5y	(3) Hospital adm. < 5y
Days			
180-151	0.034	-0.072	-0.005
	(1.602)	(0.779)	(0.042)
150-121	-1.329	0.356	-0.005
	(1.595)	(0.817)	(0.041)
120-91	-1.099	-0.133	-0.005
	(1.616)	(0.795)	(0.042)
90-61	0.313	0.185	-0.010
	(1.615)	(0.782)	(0.042)
60-31	-1.252	-0.629	0.014
	(1.609)	(0.789)	(0.042)
30-1	-2.070	-0.955	-0.000
	(1.575)	(0.738)	(0.041)
Control group mean	28.86	8.81	0.43
Obs.	7908	7908	7908

Notes: Each column shows the estimates from a separate regression. The coefficients are for the interactions of 30 day bins and a strike indicator. All regressions include period and bin fixed effects, as well as control variables (paternal and maternal income, indicators for primary school as highest level of education, higher education as highest level of education, university degree as highest level of education, currently studying, employed, parents cohabiting, parents married and indicators for missing covariates all measured a year prior to birth and indicators for parents below 21, C-section, home birth, low birth weight (below 2500g), preterm (below 37 weeks), child gender, number of pre-natal midwife visits and whether the mother smokes at the time of birth). The sample includes children who were born in Copenhagen in the placebo treated period (September 17, 2008 - April 15, 2009) and in control period (September 17, 2009 - April 15, 2010). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A5 Placebo test: The effect of strike exposure on child health measured as accumulated GP contacts by type, data for untreated (older) cohorts in 2008, 2009, 2010

	(1) Total GP < 5y	(2) Ordin. GP < 5y	(3) Emerg. GP < 5y
Days			
180-151	-0.172	0.148	-0.321
	(1.023)	(0.790)	(0.399)
150-121	0.091	0.057	0.033
	(1.057)	(0.792)	(0.443)
120-91	0.184	0.288	-0.105
	(1.046)	(0.792)	(0.430)
90-61	-0.032	0.144	-0.176
	(1.073)	(0.831)	(0.403)
60-31	0.470	0.320	0.150
	(1.064)	(0.800)	(0.444)
30-1	0.376	0.297	0.079
	(1.052)	(0.805)	(0.417)
Control group mean	19.09	14.51	4.58
Obs.	10061	10061	10061

Notes: See notes to A4. The sample includes children who were born in Copenhagen 5 years prior to the treated period (September 18, 2007 - April 15, 2008) and the control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A6 Variable means, population of children born in Copenhagen and Denmark.

	Denmark Excl. CPH		CPH	
	Mean	Obs.	Mean	Obs.
Cohabitation	0.86	115578	0.78	17949
Married	0.47	115302	0.39	17917
Prim. school, mother	0.18	111553	0.13	17054
Uni. degree, mother	0.13	111553	0.33	17054
Student, mother	0.03	114562	0.05	17927
Employed, mother	0.81	114562	0.79	17927
Prim. school, father	0.19	110697	0.15	16561
Uni. degree, father	0.13	110697	0.33	16561
Student, father	0.01	113425	0.03	17334
Employed, father	0.90	113425	0.86	17334
Danish, mother	0.86	116827	0.76	18302
Danish, father	0.87	115578	0.75	17949
Young mother	0.05	116827	0.02	18302
Young father	0.02	115578	0.01	17949
Income, mother	255.79	114550	267.55	17926
Income, father	367.66	112391	361.10	17179
Length child	51.72	113575	51.66	17849
Low birth weight	0.05	114518	0.05	18021
Preterm birth	0.07	114637	0.06	18020
Head size	34.94	112024	34.79	17746
First time mothers	0.43	112743	0.62	17967
Multiple birth	0.04	116827	0.04	18302
C-section	0.22	116827	0.22	18302
No. of hosptial nights at birth, child	3.83	114819	3.83	18070
Home birth	0.01	116827	0.01	18302
Midwife visits	4.80	111599	4.76	17814
Smoking status, Mother	0.17	114653	0.09	18020
BMI mom	24.46	107368	22.92	17424
Heigth mom	167.98	108542	167.88	17557

Notes: The Copenhagen sample includes all children born in Copenhagen in the period September 18, 2007 - April 15, 2008 and September 17, 2008 and 2009 - April 15, 2009 and 2010. The Denmark samples includes all children born in these periods in Denmark, excluding Copenhagen.

Table A7 Variable means, strike exposed and control period

	Treated group		Control group	
	Mean	Obs.	Mean	Obs.
Total GP < 1y	4.61	4081	4.80	8725
Total GP < 5y	30.60	3934	28.86	8411
Ordin. GP < 1y	3.14	4081	3.26	8725
Ordin. GP < 5y	20.66	3934	20.06	8411
Emerg. GP < 1y	1.47	4081	1.54	8725
Emerg. GP < 5y	9.94	3934	8.81	8411
Vacc., 1st round	0.85	4081	0.89	8725
Vacc., 2nd round	0.87	4081	0.91	8725
Vacc., 3rd round	0.88	4081	0.91	8725
Prev. care, 5 weeks	0.88	4081	0.92	8725
Prev. care, 5 months	0.92	4081	0.93	8725
Prev. care, 12 months	0.93	4081	0.93	8725
Midwife visits	4.80	3970	4.75	8507
Smoking status, Mother	0.10	4014	0.09	8587
Child sex	0.48	4081	0.48	8725
Low birth weight	0.04	4009	0.06	8598
Preterm birth	0.06	4014	0.06	8587
C-section	0.21	4081	0.21	8725
Home birth	0.01	4081	0.01	8725
Cohabiting	0.76	4081	0.78	8725
Married	0.37	4081	0.39	8725
Prim. school, mother	0.15	4081	0.12	8725
Uni. degree, mother	0.30	4081	0.32	8725
Student, mother	0.05	4081	0.05	8725
Employed, mother	0.77	4081	0.77	8725
Danish, mother	0.76	4081	0.74	8725
Young mother	0.02	4081	0.02	8725
Young father	0.01	4014	0.01	8551
Income, mother	262.90	4081	270.18	8725
No. of nurse visits	3.77	4081	4.40	4269
Number of registered A-D visits	2.70	4081	3.28	4269
No initial visit	0.16	4081	0.08	4269
No 2-month visit	0.44	4081	0.25	4269
No 4-month visit	0.44	4081	0.24	4269
No 8-month visit	0.26	4081	0.15	4269

Notes: The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). For the nurse visit statistics the control group only include the period September 17, 2008 - April 15, 2009.

Table A8 Compliers: Effects of strike exposure on the probability of missing the 2-month visit by subgroup

	Gender		Education		Initial health		Parity	
	Boys	Girls	Not health	Health	Not poor	Poor	>1	=1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
90-61	0.175***	0.121**	0.165***	0.109	0.149***	0.238	0.178***	0.157***
	(0.052)	(0.056)	(0.043)	(0.084)	(0.040)	(0.146)	(0.064)	(0.047)
60-31	0.491***	0.533***	0.566***	0.317***	0.514***	0.523***	0.457***	0.563***
	(0.050)	(0.050)	(0.039)	(0.082)	(0.036)	(0.134)	(0.059)	(0.043)
30-1	0.335***	0.308***	0.374***	0.152*	0.320***	0.358**	0.268***	0.364***
	(0.052)	(0.054)	(0.042)	(0.082)	(0.039)	(0.141)	(0.064)	(0.046)
Ratio to full pop. 90-61	1.13	0.78	1.06	0.70	0.96	1.53	1.15	1.01
Ratio to full pop. 60-31	0.96	1.04	1.11	0.62	1.01	1.02	0.89	1.10
Ratio to full pop. 30-1	1.04	0.95	1.16	0.47	0.99	1.11	0.83	1.13
Control group mean	0.25	0.24	0.24	0.26	0.25	0.18	0.32	0.20
Observations	4101	3773	6156	1718	7276	598	3026	4848

Notes: See notes to table A11. In this table, we show estimates for the interactions of 30 day bins and a strike indicator from separate regressions for various sub groups along with the relative coefficients between the full sample estimates and the various sub group estimates. We only show the estimates for the 90-61, 60-31 and 30-1 day bins because only children in these bins had their 2-month visit affected by the strike in the full population. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A9 Compliers: Effects of strike exposure on the probability of missing the 4-month visit by subgroup

	Gender		Education		Initial health		Parity	
	Boys	Girls	Not health	Health	Not poor	Poor	>1	=1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
180-151	0.135***	0.075	0.092**	0.144*	0.095**	0.120	0.102	0.102**
	(0.051)	(0.054)	(0.042)	(0.084)	(0.039)	(0.127)	(0.065)	(0.045)
150-121	0.220***	0.270***	0.269***	0.185**	0.257***	0.135	0.223***	0.260***
	(0.052)	(0.054)	(0.042)	(0.084)	(0.039)	(0.139)	(0.065)	(0.045)
120-91	0.381***	0.355***	0.354***	0.419***	0.378***	0.194	0.291***	0.409***
	(0.052)	(0.053)	(0.042)	(0.085)	(0.039)	(0.138)	(0.066)	(0.045)
90-61	0.150***	0.307***	0.243***	0.167**	0.228***	0.204	0.230***	0.227***
	(0.053)	(0.055)	(0.043)	(0.084)	(0.040)	(0.143)	(0.065)	(0.047)
Ratio to full pop. 180-151	1.35	0.75	0.92	1.44	0.95	1.20	1.02	1.02
Ratio to full pop. 150-121	0.89	1.09	1.09	0.75	1.04	0.55	0.90	1.05
Ratio to full pop. 120-91	1.05	0.98	0.97	1.15	1.04	0.53	0.80	1.12
Ratio to full pop. 90-61	0.66	1.36	1.08	0.74	1.02	0.91	1.02	1.01
Control group mean	0.24	0.23	0.23	0.26	0.24	0.18	0.34	0.17
Observations	4101	3773	6156	1718	7276	598	3026	4848

Notes: See notes to table A11. In this table, we show estimates for the interactions of 30 day bins and a strike indicator from separate regressions for various sub groups along with the relative coefficients between the full sample estimates and the various sub group estimates. We only show the estimates for the 180-151, 150-121, 120-91 and 90-61 day bins because only children in these bins had their 4-month visit affected by the strike in the full population. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A10 Compliers: Effects of strike exposure on the probability of missing the 8-month visit by subgroup

	Gender		Education		Initial health		Parity	
	Boys	Girls	Not health	Health	Not poor	Poor	>1	=1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
30-1	-0.403***	-0.386***	-0.396***	-0.416***	-0.411***	-0.335***	-0.411***	-0.378***
	(0.046)	(0.047)	(0.036)	(0.075)	(0.034)	(0.122)	(0.057)	(0.040)
Ratio to full pop.	1.02	0.98	1.00	1.05	1.04	0.85	1.04	0.96
Control group mean	0.16	0.14	0.15	0.18	0.15	0.15	0.20	0.12
Observations	4101	3773	6156	1718	7276	598	3026	4848

Notes: See notes to table A11. In this table, we show estimates for the interactions of 30 day bins and a strike indicator from separate regressions for various sub groups along with the relative coefficients between the full sample estimates and the various sub group estimates. We only show the estimates for the 30-1 day bin because only children in the reference (210-181) bin had their 8-month visit affected by the strike in the full population. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A11 Effects of strike exposure on child health: Total accumulated GP contacts

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
180-151	0.675	0.797	1.129	1.505	1.744
	(0.519)	(0.846)	(1.097)	(1.292)	(1.440)
150-121	0.256	0.643	0.716	1.173	1.577
	(0.518)	(0.842)	(1.087)	(1.278)	(1.430)
120-91	-0.245	-0.387	-0.594	-0.461	-0.409
	(0.528)	(0.825)	(1.055)	(1.244)	(1.372)
90-61	0.551	1.273	1.764	2.060	2.319
	(0.551)	(0.880)	(1.111)	(1.291)	(1.431)
60-31	0.143	1.843**	2.386**	2.490*	3.049**
	(0.530)	(0.862)	(1.104)	(1.291)	(1.436)
30-1	1.814***	3.365***	3.989***	4.603***	5.391***
	(0.555)	(0.883)	(1.124)	(1.320)	(1.473)
Control group mean	4.80	15.15	21.03	25.46	28.86
Obs.	12078	11982	11832	11729	11652

Notes: Each column shows the estimates from a separate regression. The coefficients are for the interactions of 30 day bins and a strike indicator. All regressions include period and bin fixed effects, as well as control variables (paternal and maternal income, indicators for primary school as highest level of education, higher education as highest level of education, university degree as highest level of education, currently studying, employed, parents cohabiting, parents married and indicators for missing covariates all measured a year prior to birth and indicators for parents below 21, C-section, home birth, low birth weight (below 2500g), preterm (below 37 weeks), child gender, number of pre-natal midwife visits and whether the mother smokes at the time of birth). The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A12 Effects of strike exposure on child health: Accumulated regular GP contacts

	(1) Ordin. GP < 1y	(2) Ordin. GP < 2y	(3) Ordin. GP < 3y	(4) Ordin. GP < 4y	(5) Ordin. GP < 5y
Days					
180-151	0.277	0.125	0.347	0.571	0.800
	(0.350)	(0.539)	(0.694)	(0.814)	(0.920)
150-121	0.119	0.303	0.295	0.620	0.925
	(0.343)	(0.535)	(0.680)	(0.801)	(0.903)
120-91	-0.285	-0.250	-0.338	-0.145	-0.123
	(0.356)	(0.532)	(0.676)	(0.799)	(0.896)
90-61	0.188	0.640	0.919	1.093	1.149
	(0.380)	(0.587)	(0.733)	(0.849)	(0.952)
60-31	-0.088	0.832	1.165*	1.171	1.358
	(0.353)	(0.548)	(0.695)	(0.814)	(0.911)
30-1	1.087***	2.029***	2.493***	2.659***	3.153***
	(0.376)	(0.576)	(0.730)	(0.860)	(0.966)
Control group mean	3.26	10.16	14.16	17.25	20.06
Obs.	12078	11982	11832	11729	11652

Notes: See notes to table A11. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A13 Effect of strike exposure on child health: Accumulated emergency GP contacts

	(1) Emerg. GP < 1y	(2) Emerg. GP < 2y	(3) Emerg. GP < 3y	(4) Emerg. GP < 4y	(5) Emerg. GP < 5y
Days					
180-151	0.399*	0.672	0.782	0.934	0.944
	(0.222)	(0.425)	(0.557)	(0.656)	(0.719)
150-121	0.137	0.340	0.420	0.553	0.652
	(0.230)	(0.428)	(0.562)	(0.653)	(0.720)
120-91	0.040	-0.136	-0.256	-0.316	-0.286
	(0.223)	(0.411)	(0.534)	(0.624)	(0.673)
90-61	0.362	0.632	0.845	0.968	1.170*
	(0.224)	(0.415)	(0.538)	(0.628)	(0.686)
60-31	0.231	1.012**	1.221**	1.319**	1.692**
	(0.228)	(0.432)	(0.561)	(0.655)	(0.721)
30-1	0.727***	1.336***	1.495***	1.944***	2.238***
	(0.233)	(0.429)	(0.550)	(0.644)	(0.709)
Control group mean	1.54	5.00	6.87	8.21	8.81
Obs.	12078	11982	11832	11729	11652

Notes: See notes to table A11. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A14 Heterogeneous effects of strike exposure on child health. Interacted model

	Health education		Parity	
	Total GP < 5y	Emerg. GP < 5y	Total GP < 5y	Emerg. GP < 5y
	(1)	(2)	(3)	(4)
180-151	-4.334	-1.856	3.580	1.691
	(3.613)	(1.747)	(2.870)	(1.458)
150-121	-1.787	-0.841	3.832	0.819
	(3.694)	(1.870)	(2.909)	(1.502)
120-91	-4.123	-2.097	1.595	0.696
	(3.560)	(1.680)	(2.800)	(1.389)
90-61	-7.134**	-3.093*	0.593	0.683
	(3.599)	(1.670)	(2.831)	(1.375)
60-31	-2.685	-2.254	1.363	1.403
	(3.579)	(1.709)	(2.844)	(1.430)
30-1	-9.587***	-4.737***	1.747	0.781
	(3.427)	(1.603)	(2.945)	(1.427)
Observations	11652	11652	11652	11652

Notes: Each column shows the estimates from a separate regression. Column labels indicate the subgroup that we test heterogeneity for. The coefficients are for the interactions of 30 day bins, a strike indicator and subgroup. All regressions include period, bin fixed effects and the interaction between bin indicators and strike exposure and full interactions between those and subgroup indicator. Regressions also include control variables (paternal and maternal income, indicators for primary school as highest level of education, higher education as highest level of education, university degree as highest level of education, currently studying, employed, parents cohabiting, parents married and indicators for missing covariates all measured a year prior to birth and indicators for parents below 21, C-section, home birth, low birth weight (below 2500g), preterm (below 37 weeks), child gender, number of pre-natal midwife visits and whether the mother smokes at the time of birth). The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A15 Effects of strike exposure on child health, mother health and child-parent relationship: Nurse registrations at 8-month visit and constrained sample

	(1) BOEL test	(2) Mother well-being	(3) Child eating behavior	(4) Child-parent contact	(5) Child reactions	(6) Aggr.: Reg. probl.	(7) Aggr.: Referral
120-91	-0.003	0.010	-0.025	0.008	0.004	-0.014	-0.010
	(0.033)	(0.020)	(0.025)	(0.010)	(0.014)	(0.040)	(0.009)
90-61	0.038	0.020	-0.002	0.010	-0.001	0.051	0.008
	(0.032)	(0.020)	(0.026)	(0.010)	(0.015)	(0.041)	(0.010)
60-31	-0.007	0.009	-0.023	0.007	0.012	-0.025	-0.022**
	(0.032)	(0.019)	(0.026)	(0.010)	(0.014)	(0.040)	(0.010)
30-1	0.011	0.011	-0.034	0.002	-0.022	-0.017	0.003
	(0.032)	(0.019)	(0.026)	(0.011)	(0.016)	(0.040)	(0.009)
Control group mean	0.14	0.05	0.08	0.01	0.03	0.25	0.01
Obs.	4612	4612	4612	4612	4612	4612	4612

Notes: See notes to table 3. We constrain the sample to children born 150 days prior to April 15, 2008 and 2009. BOEL test is an indicator equal to one if failed and zero if passed. Outcomes in column (2)-(5) are equal to 1 if nurses register problems in the specific categories and zero otherwise. The outcome in column (6) aggregates outcomes from column (2)-(5). The outcome in column (7) an aggregated measure of referrals form nurses to other part of the health sector. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A16 Heterogeneity by splitting sample: Effect of strike exposure on child health measured as accumulated total GP contacts at age five

	Gender		Initial health		SES		Smoking, mother	
	Boys	Girls	Not poor	poor	High	Low	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
180-151	4.346**	-1.320	1.362	6.772	3.040*	-1.404	1.480	4.821
	(2.025)	(2.048)	(1.528)	(4.230)	(1.601)	(3.037)	(1.482)	(5.755)
150-121	0.685	2.745	1.651	0.894	3.642**	-3.397	1.541	3.274
	(1.941)	(2.119)	(1.492)	(5.270)	(1.617)	(2.942)	(1.487)	(5.155)
120-91	0.293	-1.234	-1.055	10.339**	1.483	-4.544	-0.855	4.312
	(1.951)	(1.939)	(1.444)	(4.652)	(1.538)	(2.872)	(1.420)	(5.410)
90-61	3.946*	0.536	2.152	2.899	3.436**	-0.826	1.858	5.309
	(2.026)	(2.019)	(1.506)	(4.653)	(1.647)	(2.847)	(1.506)	(4.917)
60-31	3.938*	1.950	1.949	15.893**	4.258***	0.294	2.600*	7.658*
	(2.127)	(1.951)	(1.475)	(6.320)	(1.605)	(2.990)	(1.518)	(4.573)
30-1	7.242***	3.314	4.993***	9.164	5.817***	4.576	4.994***	9.631*
	(2.066)	(2.097)	(1.538)	(5.877)	(1.613)	(3.245)	(1.530)	(5.436)
Control group mean	30.03	27.60	28.80	30.81	28.18	30.50	28.69	31.67
Obs.	6047	5605	10732	920	8340	3312	10606	1046

Notes: See notes to table A11. The label of each column indicate which subgroup that we use in the regressions. Columns (1)-(2) split the sample by child gender. Columns (3)-(4) split the sample by initial health. The child has poor initial health if either it has a low birth weight, is born prematurely or if complications were experienced during birth. Columns (5)-(6) split by Socioeconomic status (SES). A low SES background is a child born to parents with either incomes in the bottom decile, below 21 at birth and/or only primary school educated (excl. if parents are students). Columns (7)-(8) split by whether the mother smoked during pregnancy. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A17 Heterogeneity by splitting sample: Effect of strike exposure on child health measured as accumulated emergency GP contacts at age five

	Gender		Initial health		SES		Smoking, mother	
	Boys	Girls	Not poor	poor	High	Low	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
180-151	1.848*	-0.043	0.761	3.447	1.345*	-0.099	0.861	2.087
	(1.024)	(1.005)	(0.760)	(2.204)	(0.724)	(1.701)	(0.719)	(3.347)
150-121	0.058	1.391	0.777	-0.647	1.721**	-1.878	0.910	-1.505
	(1.028)	(1.001)	(0.743)	(2.930)	(0.788)	(1.551)	(0.734)	(2.987)
120-91	-0.230	-0.366	-0.696	5.614**	0.424	-1.709	-0.275	-0.599
	(0.987)	(0.904)	(0.703)	(2.536)	(0.726)	(1.496)	(0.687)	(2.901)
90-61	2.073**	0.194	1.041	2.105	1.345*	0.438	1.152	0.441
	(1.026)	(0.896)	(0.719)	(2.450)	(0.744)	(1.490)	(0.710)	(2.720)
60-31	1.738	1.601*	1.278*	6.664*	2.074***	0.791	1.703**	1.943
	(1.111)	(0.923)	(0.729)	(3.604)	(0.761)	(1.618)	(0.755)	(2.562)
30-1	2.763***	1.693*	2.042***	4.006	2.217***	2.520	2.378***	1.230
	(1.025)	(0.967)	(0.733)	(3.357)	(0.743)	(1.659)	(0.737)	(2.730)
Control group mean	9.39	8.17	8.73	10.03	8.20	10.35	8.57	11.63
Obs.	6047	5605	10732	920	8340	3312	10606	1046

Notes: See notes to table A16. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A18 Test for heterogeneous effects by interactions: Effect of strike exposure on child health measured as accumulated emergency GP contacts at age five

	Gender	Initial health	SES	Smoking, mother
	(1)	(2)	(3)	(4)
180-151	-6.176**	4.289	-4.577	2.381
	(2.881)	(4.296)	(3.408)	(5.961)
150-121	1.670	-0.545	-7.370**	0.510
	(2.873)	(5.079)	(3.321)	(5.245)
120-91	-1.746	10.455**	-6.183*	4.748
	(2.751)	(4.725)	(3.219)	(5.610)
90-61	-3.948	-0.090	-4.274	3.602
	(2.855)	(4.622)	(3.255)	(4.893)
60-31	-2.180	13.649**	-3.751	4.720
	(2.872)	(6.213)	(3.342)	(4.751)
30-1	-4.294	3.592	-1.386	3.725
	(2.942)	(5.708)	(3.596)	(5.565)
Observations	11652	11652	11652	11652

Notes: Each column shows the estimates from a separate regression. Column labels indicate the subgroup that we test heterogeneity for. The coefficients are for the interactions of 30 day bins, a strike indicator and subgroup. All regressions include period, bin fixed effects and the interaction between bin indicators and strike exposure and full interactions between those and subgroup indicator. Regressions also include control variables (paternal and maternal income, indicators for primary school as highest level of education, higher education as highest level of education, university degree as highest level of education, currently studying, employed, parents cohabiting, parents married and indicators for missing covariates all measured a year prior to birth and indicators for parents below 21, C-section, home birth, low birth weight (below 2500g), preterm (below 37 weeks), child gender, number of pre-natal midwife visits and whether the mother smokes at the time of birth). The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A19 Test for heterogeneous effects by interactions: Effect of strike exposure on child health measured as accumulated emergency GP contacts at age five

	Gender	Initial health	SES	Smoking, mother
	(1)	(2)	(3)	(4)
180-151	-2.022	1.892	-1.517	0.791
	(1.441)	(2.242)	(1.845)	(3.522)
150-121	1.162	-1.499	-3.849**	-2.760
	(1.437)	(2.868)	(1.730)	(3.044)
120-91	-0.201	5.698**	-2.224	-0.264
	(1.340)	(2.579)	(1.643)	(3.007)
90-61	-2.009	0.928	-0.985	-0.537
	(1.359)	(2.380)	(1.649)	(2.660)
60-31	-0.116	5.079	-1.376	-0.209
	(1.436)	(3.514)	(1.759)	(2.583)
30-1	-1.164	1.879	0.188	-1.730
	(1.407)	(3.224)	(1.801)	(2.758)
Observations	11652	11652	11652	11652

Notes: See notes to table A18. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A20 Robustness test: Effect of strike exposure on child health measured as accumulated GP contacts without pre-treatment covariates

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
180-151	0.712	0.738	0.976	1.316	1.600
	(0.516)	(0.846)	(1.098)	(1.296)	(1.445)
150-121	0.280	0.586	0.631	1.150	1.613
	(0.516)	(0.840)	(1.089)	(1.283)	(1.439)
120-91	-0.357	-0.661	-1.081	-0.996	-1.049
	(0.519)	(0.816)	(1.050)	(1.239)	(1.371)
90-61	0.614	1.415	2.040*	2.351*	2.696*
	(0.546)	(0.879)	(1.114)	(1.296)	(1.438)
60-31	0.063	1.644*	2.006*	2.012	2.605*
	(0.521)	(0.850)	(1.093)	(1.280)	(1.425)
30-1	1.743***	3.050***	3.476***	4.022***	4.852***
	(0.549)	(0.882)	(1.126)	(1.325)	(1.483)
Control group mean	4.80	15.15	21.03	25.46	28.86
Obs.	12568	12464	12305	12198	12120

Notes: See notes to table A11. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A21 Robustness test: Effect of strike exposure on child health measured as accumulated GP contacts, larger bin size - 35 days

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
175-141	0.485	0.798	0.972	1.198	1.245
	(0.479)	(0.777)	(1.007)	(1.188)	(1.327)
140-106	-0.627	-0.974	-1.178	-0.841	-0.970
	(0.483)	(0.785)	(1.011)	(1.188)	(1.323)
105-71	0.043	0.481	0.397	0.554	0.429
	(0.508)	(0.791)	(0.994)	(1.159)	(1.275)
70-36	0.216	1.683**	2.295**	2.434**	2.939**
	(0.491)	(0.795)	(1.012)	(1.179)	(1.313)
35-1	1.377***	2.905***	3.331***	3.743***	4.400***
	(0.512)	(0.823)	(1.041)	(1.218)	(1.359)
Control group mean	4.80	15.15	21.03	25.46	28.86
Observations	12078	11982	11832	11729	11652

Notes: See notes to table A11. We increase the bin size to 35 days instead of 30 in the main specification. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A22 Robustness test: Effect of strike exposure on child health measured as accumulated GP contacts, smaller bin size - 21 days

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
189-169	0.996	0.989	1.665	2.143	2.641
	(0.606)	(0.978)	(1.261)	(1.479)	(1.644)
168-148	0.999	1.358	1.273	1.625	1.859
	(0.609)	(0.998)	(1.297)	(1.540)	(1.713)
147-127	0.255	0.723	0.650	0.906	1.283
	(0.623)	(1.019)	(1.312)	(1.540)	(1.722)
126-106	-0.193	-0.621	-0.532	0.0156	0.0484
	(0.613)	(0.977)	(1.261)	(1.493)	(1.657)
105-85	0.575	1.347	1.304	1.547	1.846
	(0.652)	(0.988)	(1.249)	(1.463)	(1.606)
84-64	0.496	0.785	0.964	1.284	1.599
	(0.643)	(1.031)	(1.287)	(1.497)	(1.653)
63-43	0.885	2.568**	3.301**	3.504**	4.194**
	(0.629)	(1.011)	(1.290)	(1.497)	(1.660)
42-22	0.358	2.139**	3.008**	3.514**	4.530**
	(0.637)	(1.045)	(1.330)	(1.567)	(1.758)
21-1	2.670***	4.341***	4.873***	5.455***	6.142***
	(0.664)	(1.052)	(1.337)	(1.574)	(1.749)
Control group mean	4.80	15.15	21.03	25.46	28.86
Observations	12078	11982	11832	11729	11652

Notes: See notes to table A11. We reduce the bin size to 21 days instead of 30 in the main specification. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A23 Robustness test: Effect of strike exposure on child health measured as accumulated GP contacts, 210 days prior to April 15, 2008 as strike exposed period and 210 days prior to April 15 2007, 2009, 2010 as control periods

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
180-151	0.652	0.702	0.898	1.093	1.244
	(0.488)	(0.795)	(1.037)	(1.224)	(1.370)
150-121	1.199**	1.459*	1.428	1.839	2.078
	(0.482)	(0.786)	(1.022)	(1.203)	(1.353)
120-91	0.209	0.086	-0.137	-0.070	-0.155
	(0.493)	(0.771)	(0.992)	(1.171)	(1.294)
90-61	0.813	1.381*	1.656	1.806	1.869
	(0.519)	(0.829)	(1.050)	(1.221)	(1.355)
60-31	0.368	2.006**	2.537**	2.699**	3.045**
	(0.495)	(0.808)	(1.038)	(1.216)	(1.355)
30-1	1.758***	3.098***	3.594***	4.141***	4.587***
	(0.526)	(0.838)	(1.069)	(1.257)	(1.409)
Control group mean	4.49	14.83	20.80	25.40	29.08
Obs.	15736	15616	15417	15279	15169

Notes: See notes to table A11. We add a control period (September 17, 2006 - April 15, 2007) to the sample. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A24 Robustness test: Effect of strike exposure on child health measured as accumulated GP contacts, 210 days prior to April 15, 2008 as strike exposed period and 210 days prior to April 15, 2009 as control period (first stage sample)

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
180-151	0.557	0.735	1.126	1.629	1.723
	(0.596)	(0.968)	(1.255)	(1.471)	(1.646)
150-121	0.342	0.876	1.275	1.847	2.308
	(0.596)	(0.974)	(1.266)	(1.485)	(1.662)
120-91	0.324	-0.107	-0.096	0.029	0.166
	(0.607)	(0.947)	(1.216)	(1.435)	(1.591)
90-61	0.708	1.138	1.705	1.853	2.067
	(0.629)	(1.007)	(1.272)	(1.481)	(1.654)
60-31	0.160	1.870*	2.887**	2.777*	3.639**
	(0.613)	(0.992)	(1.268)	(1.488)	(1.656)
30-1	2.461***	4.399***	5.249***	5.565***	6.485***
	(0.627)	(0.998)	(1.271)	(1.499)	(1.675)
Control group mean	4.52	15.35	21.40	26.06	29.93
Obs.	7874	7814	7716	7641	7588

Notes: See notes to table A11. We reduce the sample to the first stage sample. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control period (September 17, 2008 - April 15, 2009). Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A25 Robustness test: Effect of strike exposure on child health measured as accumulated GP contacts, inclusion of movers from Copenhagen - initially omitted from main sample

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
180-151	0.440	0.222	0.354	0.602	0.821
	(0.480)	(0.798)	(1.031)	(1.213)	(1.347)
150-121	0.199	0.397	0.477	0.874	1.288
	(0.483)	(0.793)	(1.019)	(1.193)	(1.332)
120-91	-0.435	-0.998	-1.305	-1.333	-1.217
	(0.484)	(0.774)	(0.988)	(1.163)	(1.281)
90-61	0.230	0.651	1.010	1.154	1.433
	(0.506)	(0.822)	(1.040)	(1.210)	(1.340)
60-31	-0.155	1.219	1.659	1.667	2.184
	(0.491)	(0.811)	(1.036)	(1.212)	(1.345)
30-1	1.418***	2.500***	2.901***	3.222***	3.991***
	(0.518)	(0.835)	(1.060)	(1.243)	(1.386)
Control group mean	4.78	15.25	21.15	25.59	29.00
Obs.	13918	13611	13455	13350	13276

Notes: See notes to table A11. We add children who were born in Copenhagen during the sample period but moved from Copenhagen during the first year of life. We drop these children from the main sample. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A26 Robustness test: Effect of strike exposure on child health measured as accumulated GP contacts - Doughnut approach

	(1) Total GP < 1y	(2) Total GP < 2y	(3) Total GP < 3y	(4) Total GP < 4y	(5) Total GP < 5y
Days					
180-151	0.664	0.777	1.104	1.474	1.712
	(0.519)	(0.846)	(1.097)	(1.292)	(1.441)
150-121	0.263	0.653	0.730	1.190	1.587
	(0.519)	(0.842)	(1.087)	(1.278)	(1.431)
120-91	-0.243	-0.404	-0.616	-0.490	-0.448
	(0.528)	(0.825)	(1.056)	(1.244)	(1.372)
90-61	0.558	1.270	1.761	2.053	2.311
	(0.551)	(0.881)	(1.111)	(1.292)	(1.432)
60-31	0.139	1.826**	2.358**	2.459*	3.012**
	(0.531)	(0.863)	(1.104)	(1.292)	(1.436)
30-15	0.959	1.905*	2.369*	2.683*	3.474**
	(0.671)	(1.045)	(1.301)	(1.525)	(1.718)
Control group mean	4.79	15.19	21.09	25.55	28.98
Obs.	11307	11227	11093	10995	10924

Notes: See notes to table A11. We drop children born within 14 days of strike start. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.