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 visiting (NHV) on infant and maternal health. We study universal NHV in Denmark, where nurses (i) monitor and screen infant and post-partum maternal health, (ii) provide information and counselling to new parents, and (iii) refer families with identified problems to other health care professionals. We exploit exogenous variation in the timing of forgone visits induced by the 2008 national nurse strike. Using data on the population of children born in Copenhagen in the period up to the strike and in control years, we show that children (and mothers) who missed early nurse visits after birth have more general practitioner contacts in their first five years of life compared to those who missed visits later. We speak to mechanism for these effects by showing (i) that nurses in control years perform well in identifying health risks during early home visits, and (ii) that children of parents with no educational background in health and childcare and first-parity children drive the health effects. Taken together, our findings provide evidence for the importance of universal screening and timely provision of information and counselling to new parents. A stylized cost-effectiveness calculation confirms that early universal NHV is worth the while.

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1 Introduction

A large literature documents 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 considers the effects of program exposure, i.e. the *extensive* margin. However, 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. 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,

[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 points 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. Work from developing country contexts also highlights the important role for child development and long-run outcomes that intensive home visiting can play (Attanasio et al., 2014; Gertler et al., 2014).

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

universal NHV by trained nurses has been an integral part of publicly-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, today all new families are eligible for up to five universal home visits. The focus of the contemporary Danish program is—like in many other settings—on health monitoring and screening, information of new parents (on topics such as infant nutrition, infant behavior and child-parent interactions), the promotion of adequate parental inputs and interactions, and the referral of families with specific problems to other health professionals such as general practitioners (GPs) or hospitals.

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, we provide insights on underlying mechanisms for the impact of the timing of NHV (i.e., the importance of different program components) and we explore the impact of NHV on parental behaviors. 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. Importantly, nurse home visiting contains several aspects that are likely to impact parental behaviors: nurses provide new parents with specific information (e.g., on breastfeeding or adequate child-parent interactions), they update parental knowledge about the health and development of their child, and they may contribute to shaping parental beliefs about the relevance and importance of specific investments. While differences in economic resources are arguably important predictors for differences in parental behaviors, recent evidence highlights the importance of systematic differences in parental beliefs (about the timing and interaction of investments in their child, as well as the importance of child endowment) in shaping both parental inputs and child development (see, for example, Cunha et al., 2013; Attanasio et al., 2015; Boneva and Rauh, 2018; Biroli et al., 2018).

To identify the effects of the timing of NHV for child and maternal health, 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 cancelled 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 Danish nurse 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] Reduced form estimates from this study 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 earlier work 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 2007-2009, 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 strike exposure in the population

[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 cancelled second nurse visit (which would have been scheduled during the strike period).

of families with respect to a large set of observable characteristics, and analyze more potential channels for our main results.

Our results show that the 2008 nurse strike impacted NHV in the expected ways: Depending on the date of birth of their child, families on average missed one nurse visit (at different ages). Importantly, exploiting the merged nurse records and administrative data on family background, we show that the strike impacted families similarly across characteristics likely 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 exposure during the initial months of a child's life is relatively more influential for child and maternal health: Children, who were born in the three months up to the strike and thus were likely to lack an early nurse visit have more GP contacts in the first five years of life than children, who were older at strike-exposure. This result holds for both regular and emergency GP contacts and also holds for yearly measures of GP contacts, i.e. it is not driven by a substitution of nurse visits with GP visits only during the strike period.[5] We also find that the mothers, who forego an early nurse visit, have more frequent GP contacts in the first five years after their child's birth. At more extreme margins—child hospital admissions and outpatient contacts, delayed school start, and maternal mental health—we do not detect adverse effects of strike exposure at different ages.

Missing an early nurse visit may impact child and maternal health and parental behaviors through two main channels: screening and information/counseling. First, early nurse visits may help to identify adverse conditions in a timely fashion and prompt additional care by other health professionals, such as GPs. As we show in our data from the non-strike exposed control period, at initial visits, nurses predominantly record issues related to feeding, child physical health and maternal well-being. In our data (from non-strike years) those initial

[5] Our main outcome measures of GP contacts exclude preventive care contacts to the GP, which we study separately.

registrations of in particular problems with feeding and maternal mental health are correlated with both future nurse registrations of health issues and the increased use of health care services among children, as well as the likelihood of future maternal psychiatric contacts. These correlations suggest that early nurse visits act as an important screening device and identify the most vulnerable children and mothers. In absence of early nurse visits, for the marginal child, her own health problem and—potentially more importantly—maternal mental health problems may go unnoticed for a longer period and contribute to longer-term adverse health effects. This channel may be of great importance given documented correlations of maternal postnatal mental health and child-parent interactions, as well as child development (Cooper and Murray, 1998).

Second, in the absence of early nurse visits, parents may lack specific information, which is typically provided by nurses and is difficult to replace by other and less specialized health care providers, such as GPs. Moreover, information and counselling provided by nurses may impact parents' investment behaviors, such as breastfeeding, parent-child interactions or uptake of other preventive care. To examine the relevance of this channel, we study the impact of strike exposure among children across different backgrounds. We find suggestive evidence 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. At the same time, we find no strong and unambiguous evidence for a socio-economic gradient in the effect of early strike exposure. These findings indicate that at least part of the beneficial effect of early NHV runs through a specific information channel. While we study parents' participation in the vaccination and preventive care programs (as our main measures of parental investment behaviors), we do not detect a strong impact of the timing of nurse visits in our design. However, these analyses are constrained by power issues.

Assessing the direct costs and benefits of early nurse visits (relative to later visits), we perform a stylized cost-effectiveness analysis. We estimate that particularly the immediate benefits (in terms of averted GP visits) of initial universal nurse visits (within 14 days after birth) outweigh their costs with an estimated 165 EUR. Thus our findings indicate that early universal visits are a cost-effective intervention to promote children's and mothers' health in settings that resemble the Danish health care system. Moreover, our findings suggest that policies to restructure universal programs or to introduce new (universal) programs should prioritize the initial period of family formation after the birth of a child.

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 is 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.[6] The majority of uncomplicated births in Denmark are midwife-assisted in public hospitals. Hospital births account for around 98percent 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

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

services include additional home visits or other services.[7] Moreover, Danish GP provide the child preventive health program and administer recommended vaccines in the vaccination program. The Danish preventive care schedule offers eight (voluntary) GP health checks for all children: at around five weeks, at around five months and yearly for children aged one through six years (Sundhedsstyrelsen, 2007). Additionally, GPs offer one postpartum health check for mothers. The Danish vaccination program for children consists of three rounds during the first year of a child's life, at three, five and twelve months.[8]

2.2 NHV in Copenhagen

Our study focuses on NHV in Copenhagen, the largest municipality in Denmark with around 500,000 inhabitants and around 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 universal program is four: an initial visit shortly after birth (A visit), a two month visit (B visit), a four month visit and an eight month visit (C and D visit). Children, who are discharged with very short hospital stays can receive two A visits. Moreover, nurses can provide additional targeted visits to children and families with identified needs at their discretion. The timing of these additional visits is flexible. Finally, the municipality offers different types of optional visits that are only offered on the request of parents (visits at 1.5 and three years of the child's life).

Home visits usually last between 30 minutes and one hour. During the visits, nurses provide information and counseling to parents and examine the infant. The visits take their point of departure from a general set of main topics (which are of different importance at different ages of the child) outlined in the national guidelines for NHV provided by the Danish National Board of Health. Those guidelines also state that nurses should focus on the needs

[7]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.

[8]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.

of the specific family and nurses have large discretion to focus their time in the family's home on the topics that matter most in their view. While some topics, typically related to screening (such as tests for certain infant reflexes, monitoring of maternal post-natal well-being and the monitoring of child weight and height), are part of visits to all families, other topics are only covered if the family or the nurse find them relevant. As a result, visits and nurses' registrations from the performed visits vary greatly.

Table 1 illustrates the main topics that structure the universal nurse visits in the child's first year of life (A-D visits) and which registrations nurses can make. Importantly, domains that are covered in each visit such as infant feeding have age-specific items that nurses can make registrations on (such as "issues with establishment of breastfeeding" or "issues with the introduction of solid food").

Although nurses focus on the specific families' needs, we can use data recorded by Copenhagen nurses during or shortly after their home visits to illustrate some of the typical features of the universal home visits: Figure 1 presents data for our control cohort of children and mothers.[9] For each of the four scheduled universal home visits the figure plots the share of families with recorded issues by topic (conditional on having received the visit). As the figure shows, the different visits have different foci: While the share of families with "registrations of an issue in any domain" remains rather stable over the course of the four visits, there are important differences especially when comparing the first two and final two universal visits. During the initial visits, nurses typically record issues related to maternal mental well-being and infant feeding issues. The former is very well-defined, mother-specific and highly correlated for women across visits. The latter is child-related but rather unspecific in its content. While registrations on feeding issues are common during the initial visits, nurse observations and registrations on child developmental problems (a summary measure of various dimensions of child development) are more prevalent in the visits at around four and eight months. Using this information on the focus of the composite nurse treatment, we

[9]As we will detail in section 2.4, we use data on several cohorts of children and mothers, one of them exposed to the nurse strike. In Figure 1, we focus on non-strike exposed children and mothers.

will return to the importance of different aspects of the treatment in section 4.4.

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 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 percent 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.[10]

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.[11] 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

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

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

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 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 records from visits and examinations of all children born in the municipality from January 1, 2007 to December 31, 2010—a total of 35,213 children.[12] Nurses typically complete their registrations on a given visit either at the family home (using a laptop) or at their office directly after the completed visit. The nurse registers the time and type of the visit and her observations regarding factors such as child and maternal health, feeding problems, or relevant risk factors in the family (see Table 1 for examples). When we analyze data on completed nurse visits, we do not consider data from the 2010 cohort as they are right-censored, i.e. we do not observe information on all visits before the end of the data period.[13]

Our main outcome measure of child health is a measure of health care usage, the total number of GP contacts measured in the administrative data from Statistics Denmark. We measure both the yearly and accumulated 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 potential role for substitution in the long-term should be

[12]The datasets time frame is due to the use of a specific nurse record system ("DSI Sund") 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 observations during the visits.

[13]We use a unique social security number in the nurse records to merge children and their parents to the administrative data from Statistics Denmark.

less important. Importantly, we do not exclude GP contacts in the preventive care program but include them in separate analyses. Thus our measures of GP contacts (ordinary consultation and emergency contact) do not measure the participation in the voluntary preventive care program but contacts to GP due to health problems or at least worries related to the child's health.

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 poor 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 GP contacts are typically not at the family doctor's and treat conditions that require medical attention outside GP office hours. While not perfectly independent, emergency GP contacts may be a more direct measure of poor health that requires attention. As additional child health outcomes, we consider child hospitalizations and outpatient contacts. While hospitalizations may be a cleaner measure of actual child health, they are relatively severe events.

Moreover, we consider the impact of strike exposure on maternal health. Specifically, we study the number of GP contacts and maternal contacts with and diagnoses received from psychiatric specialists. Effects on mothers' health and well-being may be an important mechanism or a reinforcing factor for longer term effects on children.

As we exploit information on a sample of children exposed to the nurse strike and thus the lack of a given visit, we are contained in our ability to use nurse registrations on both health and parental inputs at a given visit as outcome measures in our main analyses.[14] To

[14] In supplementary analyses, we constrain our sample to early strike-exposed children and study their outcomes at the nurse visit around eight months (D visit). However, this analysis relies on a small sample relative to the expected effect sizes (and the expected noise in the measurement of outcomes by nurses) and is thus not very informative. Unfortunately, the nurse data on infant feeding (duration of breastfeeding) in the archived data are of very poor quality and we cannot use them at all.

directly analyze the potential impact of the timing of NHV on parental health investments, we focus on the take-up of preventive care and vaccines. Each nurse visit beyond the initial visit is closely spaced around the recommended age for a vaccination. Thus we test the hypothesis that missing a specific nurse visit lowers the probability of having the subsequent vaccination. In that case, we may conclude that nurse visits stimulate positive parental health 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 reduced form relationship:

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

where y_{it} is an outcome measure, such as GP contacts in the first year of life for child i born at time t. In our analyses for outcome measures from the administrative data, we consider all children born in the 210 day period prior to April 15 in the years 2008, 2009 and 2010 (14,784 children).[15] 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 cohort, $\boldsymbol{\lambda}$.[16] The interactions of the period bins with an indicator for the 2008 cohort (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

[15] 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.

[16] 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.

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 total 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, indicators for having had a Caesarean section or a home birth, and indicators for the child having been low birth weight (below 2500g), a preterm birth (below 37 weeks), her gender, as well as 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 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 in levels. These differences could, for example, be due to overall 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 all cohorts.

One way of empirically assessing the untestable common trend assumption is to study pre-determined 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.[17] 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 in the year leading up to their child's 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 graphically show the relationship between strike exposure and pre-natal midwife visits and GP consultations, the number of days admitted to hospital post birth and the C-section rate in Appendix Figure A2. We find no systematic differences in any of these types of care around birth across the groups that we consider.

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

[17] 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 percent-level. Results are available on request.

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.

Second, 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.[18] Appendix Figure A4 shows the share of individuals who stay in Copenhagen during the first year of life as a function of age at strike and split by treated and control cohorts. The share of stayers very similar across periods. As a robustness check, we include domestic first-year movers into our main analyses, 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 Copenhagen across the groups of treated children (born September 18, 2007 - April 14, 2008) and children in the control group (born September 17, 2008 (2009) - April 14, 2009 (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 nurse data is right-censored for the children born in 2010.

Control children have on average 4.8 GP contacts during the first year of life. At age 5, that number has increased to 28.9. 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 percent. The treated and control groups are well-balanced across

[18] As the strike was a nation-wide strike and of relative short duration (which parents were aware of), the risk of strike-induced domestic migration should be small.

covariates.

Focusing on the bottom panel of Table 2, we find that the four universal nurse visits are well attended. The average number of universal visits per child is 3.3 for control children, meaning that the average child receives three out of potentially four visits. On average, children receive one additional visit scheduled due to a specific identified need. This average masks heterogeneity across children. Table 2 also illustrates the impact of strike exposure on the program coverage: For all types of visits, treated children have a higher probability of missing the given visit. 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. In the following, we will in greater detail analyze these patterns.[19]

4.2 First Stage and Compliers

Figure 2 presents graphically the impact of strike exposure on the number of nurse visits for children in the treated and control cohorts (2007/2008 and 2008/2009, respectively). Strike exposure impacted the total number of nurse visits that children received. Panel (a) of Figure 2 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 early hospital-discharged children receive two visits within the first 14 days of life - one universal visit and one extra visit. In section 4.5, we examine the

[19]To assess the representativeness of our sample of families from the capital of Denmark, Appendix Table A4 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 percent of children are low birth weight children and 7 percent 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 percent of children born in Copenhagen are firstborns compared to 43 percent outside Copenhagen, their parents are older and less likely to smoke.

robustness of our general conclusions to the omission of this group of children (a doughnut hole-approach).

To further examine the impact of the strike on nurse visits and to confirm that we have the type of identifying variation that we want (i.e., the overall decrease of the number of visits is driven by a lack of visits at specific ages), Figure 3 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 3. 60 percent 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 percent). 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 have an increased probability of missing the eight month visit while all the younger children are unaffected at that time (because the strike ended by the time their visit was due).

Table 3 presents formal estimates from regressions based on Equation (1). Coefficients reflect the effect of being born in a specific bin on the probability of not receiving each nurse visit (the omitted baseline is the 30 days bin furthest from strike start). The columns show results for the different types of universal nurse visits. The regression results mirror the graphical representation: The strike only has an impact on the initial (A) visit for children who were between 30-0 days at strike start. On average children in this bin have 17.1 percent-points higher probability of missing the initial visit (relative to the reference group). Children who were 90 days and below at strike start have an increased probability of a missed B visit with the 60-31 bin most severely affected (51.1 percent-points). Children who were between 61 and 150 days at strike start have their C visit most severely affected by the strike. Only the oldest children in the strike exposed period have increased probability of a missed eight month visit compared to younger children (around 40 percent-points difference when compared to the children, who were youngest at strike start). As shown in column (5) strike exposure does not differentially impact children's number of completed universal visits (A-D). However, children in the 30-1 day bin loose on average 0.267 nurse visits more than the reference group (significant at the 10 percent level). This result is reflects that children below age two weeks at strike start potentially loose two visits, the universal initial visit and an additional early visit if discharged shortly after birth.

Having established that age at strike start has a meaningful impact on timing (but in general not number) of the missed nurse visit for strike-exposed children, 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 first nurse visit (Appendix Tables A5 through A7 present analyses for the other three universal visits). 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 a health-related field,[20] initial child health,[21] and child parity. In each of the tables, we show coefficients for the 30 day bins that are affected by the strike in the full population. In general, the complier analysis suggests that the strike affected the considered subgroups

[20]Having parents with an educational background in a (child) health-related field implies that either one of the parents are educated as doctor, midwife, nurse or pedagogue.

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

relatively similarly and a stronger first stage does not covary with characteristics that may indicate positive potential outcomes. Thus we think it is reasonable to state that nurses did not prioritize to a great degree based on the given characteristics. This finding is relevant for our interpretation of especially heterogeneous effects (because we can rule out nurses' prioritizing of certain subgroups during the strike as an main driving factor).

Taken together, strike-exposed children missed on average one nurse visit. Thus we cannot fully disentangle the effect of having one less nurse visit from the effects of timing. At the same time, strike exposed children missed this visit at different ages and we compare outcomes of children across years relative to the reference group of children born 180-210 days prior to the strike. Our first stage results provide powerful evidence for the differential timing of the assigned treatment (one less visit). Thus we think it is reasonable to interpret our findings as predominantly being driven by timing given that the different visits coverage-specific topics, as outlined in section 2.

4.3 Main Results: Child and Maternal Health

To measure the impact of strike exposure at different ages on children's and mother's health, we use outcomes from the administrative data. Figure 4 presents graphical evidence of the relationship between age at strike start and accumulated GP contacts at ages one through five. The figure shows the raw relationship between date of birth and GP visits estimated with kernel weighted local polynomials. We use an epanechnikov kernel and 42 (5-day) smoothing points through out. Black lines and confidence intervals are for the treated period, gray lines and confidence intervals are for the control periods.[22] Importantly, for all our measures of GP contacts, the graphs display some seasonality in the control years with children born during the late fall/winter having more GP contacts than spring children. This seasonal pattern holds for all potential control years that we can choose. We argue that it is not problematic for our estimation as long as it is common to all years and we are willing to assume that it

[22] Figures for regular and emergency contacts are available on request.

would be present in the absence of the strike also in the treated year.

The number of accumulated GP contacts reveal a clear pattern: Children, who were youngest at strike start in 2008 have significantly more GP contacts relative to children of older age groups and this pattern looks different in the control group. As Figure 4 further illustrates, there is a gradient inside the early strike-exposed children group such that the youngest child have most GP contacts. This finding indicates that earlier NHV is relatively more important for child health than later NHV. For children older than 100 days at strike start, the total number of GP contacts is similar to the control children. This suggests that the later nurse visits do not impact future health care usage. Interestingly, the impact of missing an early nurse visit is persistent as the differences increase as the children ages.

Figure 5 illustrates results from regressions based on Equation (1): We plot the point estimates for first year child GP contacts along with confidence intervals.[23] In the first year, children that were between 30-1 days at strike start have 1.8 (37.5 percent) GP contacts more compared children who were oldest at strike start. For children of older age groups at strike start 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.

Table 5 shows our main results for the impact of strike exposure on child health. To rule out that substitution toward GP visits during the first year of life drive our findings, we present estimates for *yearly* outcome measures, i.e., child GP contacts measured in each year of life of the child. Across periods the estimated effects are significant and the patterns documented in the graphical analyses persist.[24]

[23]We have also looked at the equivalent point estimates for contacts accumulated by age five, see Figure A5 for an illustration.

[24]Figure A5 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 percent) more GP contacts in total for the 30-1 bin, 3.0 (10.5 percent) for the 60-31 bin and 2.3 (8.0 percent) for the 90-61 bin. For regular GP contacts

To assess the impact of strike exposure at other margins, Appendix Table A8 presents results for more extreme child health outcomes: child hospitalizations, outpatient contacts and the probability of children delaying their school start. We do not see strong indications for consequences of strike exposure for hospitalizations. Hospitalizations are a relatively more extreme measure of health than GP contacts and the timing of NHV does not seem to affect this margin by age five. However, we see some indication for a decrease in (only) first year outpatient contacts. Outpatient hospital referrals to infant care centers constitute one of the main instruments that home visiting nurses can use when referring infants with identified health problems, e.g., with respect to feeding.[25] The negative effect for the youngest strike-exposed children on early outpatient visits is likely driven by two factors: first, in the absence of nurse visits, children did not get referred to outpatient hospital contacts. Second, as also nurses at the hospital were on strike, families who needed extra contacts may have been discouraged from taking up hospital care and see their GP instead.[26] Thus this result may support the idea that early-life increases in GP contacts partly reflect substitution of care. Finally, we study whether children have a higher probability of delaying school start in elementary school. We find no effects of the timing of strike exposure on the probability of delayed school start.[27]

Our main results suggest that early strike-exposure impacts children's number of contacts

the percentage effect is 15.5 percent for the youngest age groups, for the 90-61 and 60-31 age bins we see no significant effect on the number of regular GP contacts. The percentage effects on emergency contacts are 25.0 percent, 17.5 percent and 15 percent for the 30-1, 60-31 and 90-61 age groups.

[25]Unfortunately, we cannot consider specific reasons for outpatient contacts due to data breaks in the diagnoses data. As an example, children can be referred to the hospital as outpatients to receive treatment for jaundice or feeding problems. However, the ICD 10 diagnoses have net been regularly used for those contacts before the years 2009/2010.

[26]Hospitals were obliged to ensure an adequate level of care provision.

[27]We have also considered nurse registrations at age eight months as health outcomes of strike-exposed children: As described in the data section, during visits, nurses observe and record aspects of child and parental development. These registrations 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 than 40 percent of the unrestricted sample. Thus our analysis, presented in Appendix Table A9, does not lead to precise estimates.

to the GP—not only in the short run (which could be due to substitution), but also in the longer run. Importantly, nurses also focus their attention on maternal physical and mental maternal well-being. Table 6 presents results for a set of maternal outcomes: total and emergency GP contacts, maternal mental health diagnoses[28] within the first year after birth, and maternal contacts with psychiatric specialists in the first three years after birth. Table 6 shows that mothers of early strike-exposed children (who had an elevated risks of loosing the initial and the 2-month visits) have three additional GP contacts in years two through five after their birth. One third of this effect is driven by emergency contacts. Apart from showing a health effect for strike-exposed mothers, this health effect may be a mechanism for or reinforce the health effects we find for their children. While we do not find significant effects on maternal mental health outcomes, those outcomes constitute very rare events and are thus difficult to assess in our framework.

4.4 Mechanisms

Our main analyses suggest that early strike-exposure matters for child and maternal health. We interpret this finding as support for the hypothesis that early NHV matters more for the outcomes that we study than later visits. To speak to potential mechanisms for the effects that we estimate, we attempt to shed more light on the importance of different elements of the composite nurse treatment. First, to assess whether information and counselling is a driving force for the health effects that we observe, we study heterogeneous effects across the parity of the child and parental health-related education.[29] Specifically, we hypothesize that first-time parents and parents without professional knowledge about child health and development may see larger effects of early strike exposure due to the existence of an information channel.

For brevity, we present results for our measure of total GP contacts in year one and year two through five of the child's life (Table 7 and Appendix Figure A6).[30] We split our

[28]We use ICD-10 codes between F01-F99.

[29]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.

[30]Results for emergency GP contacts are in Appendix Tables A10 and Appendix Table A12.

sample into subgroups and additionally estimate a fully interacted model on the full sample (Appendix Table A11). Table 7 presents our split sample results. In column (1)-(4), 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-related field. For children with parents *not* educated in those fields, effects resemble the effects found in the full population. Firstborn children also see stronger effects of early strike exposure and a larger gradient than non-firstborn children, as shown in column (5)-(8) in Table 7. While we cannot reject that the effects are the same across subgroups (see Appendix Table A11and A11), taken together, our findings suggest that an information and counselling channel is important for explaining longer-run consequences of early NHV.

Finally, in Appendix Tables A13 and A14, we examine heterogeneity by gender, initial health, socio economic status (SES) and risky parental behaviors (proxied by maternal smoking during pregnancy). We see indication for boys, children with poor initial health and children of parents with risky parental behavior being relatively more affected by the absence of early NHV (however, we cannot reject equality of effects in most cases). We see less systematic differences in estimates across families of high or low socio-economic status, if anything, high SES families appear to respond more to the lack of early NHV. This finding may further underline the importance of specific guidance and information for new parents. Moreover, the finding further points to the importance of another potential beneficial channel for early life NHV, namely universal screening.

Early NHV focuses on screening for potential health problems in infants and mothers: Being designed as a universal program, it represents an early window of opportunity to detect and confront health problems. While we have considered maternal mental health outcomes in Table 6, our sample size does not allow us to study those in great detail. Another way of further examining the importance of screening is to assess the performance of nurses with respect to screening in non-strike years.

Table 8 presents nurse registrations from performed initial visits and administrative data health outcomes for two groups of children and mothers: mothers with and without nurse registrations of maternal mental health problems at the initial visit. Conditional on having those visits, we can observe interesting patterns that point to the importance of nurse screenings very shortly after birth: Around one out of 10 mothers are classified as having a mental health problem at the initial visit in our control period sample. Following those women over time, we see that nurses are more likely to register both mental health problems and other issues in the course of the child's first year. Mothers with early detected mental health problems receive more referrals to other health professionals and, importantly, among early-detected mothers there is a higher prevalence of externally measured health issues. Taking the numbers in Table 8 and relating them to the overall prevalence of maternal mental-health related contacts, the figures here suggest that nurses in our control year identified one third of those mothers who end up having a mental-health related contact with psychiatric specialists in the first three years of their child's life.[31] This illustrative figure suggests large potential health returns from early screening efforts.

A final and important potential pathway for the effect of early NHV are parental investments in response to those. 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. However, given our sample size in combination with our empirical strategy, we are severely constrained in an analysis of those: Appendix Tables A15 through A17 study whether strike exposure impacts participation in the childhood preventive care program and vaccination program participation (as well as timely participation) as outcomes.[32] As the tables illustrate, we cannot draw firm conclusions due to very

[31]Nurses screen around 10percent of mothers in the sample as having a mental health problem. Of those, 11 percent end up having at least one outpatient contact in the first three years of the child's life, which means that nurses capture around one percent, who end up with a contact. In the population, the control mean is around three percent.

[32]Almost 80 percent of children receive all infant vaccinations and each round of vaccinations are attended by 90 percent of children in Copenhagen. Participation in the vaccination 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.

imprecise estimates.

4.5 Robustness Tests

Our main results are robust to a number of changes to our main specification and sample. For brevity, we only present robustness tests for one main outcome, the total number of GP contacts.[33] In Appendix Table A18, we omit control variables. To rule out that our choice of bin size drives our results, we present estimates with bin size of 35 and 21 days in Appendix Tables A19 and A20. Appendix Table A21 includes 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. 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 A22). Across these different estimations our main findings are generally very robust.

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 A23 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.[34]

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 and show that our results are robust to their inclusion (Appendix Table A25). Second, we use a doughnut hole approach (where we drop children born within 14 days of strike start, who were likely to loose more than one visit on average) (Appendix Table A26). Our main

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

[34]Appendix Table A24 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.

conclusions—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 costs and health benefits of early NHV (relative to later NHV). Specifically, we estimate the benefits of receiving an initial and a two-month visit. Our estimated benefits for the earliest visits are relative to the benefits of the four month and eight month visits. Next, we compare those benefits to an estimate of the costs of the early visits.

To begin with, we estimate the benefit of strike exposure on the total GP fees at age five. We focus on GP fees and thus disregard longer-run benefits or spill-overs. As a consequence, our measure of benefits is a conservative estimate. Table 9 presents our results for the yearly and accumulated GP fees as outcome.[35] The estimates display the same patterns as shown for the number of GP contacts.

Children in 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 percentage points larger probabilities, respectively. The 60-31 day age group missed only the two month visit with 51.1 percentage points larger probability. To estimate the benefit in EUR 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 loosing the two month visit caused by the strike, $53.5/0.511 = 104.7$ EUR. To estimate the benefit of the initial visit, we use our estimates and the benefit of the two month visit, $(98.1 - 104.7 \cdot 0.323)/0.171 = 375.9$ EUR[36]. Not receiving an initial visit increases GP fees with 375.9 EUR and not receiving the two month visit increases GP fees with 104.7 EUR at age 5.

Also with respect to costs we constrain our analyses to very immediate costs (for example, we do not factor in the time parents have to invest and be absent from work when their child

[35] GPs are reimbursed for all procedures they provide to patients in a given calendar week. We do not find clear evidence for the treated children having more costly GP visits on average.

[36] 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.

is sick). The municipality of Copenhagen has reported daily savings caused by the striking nurses of 32,000 EUR per workday or 160,000 EUR per (business) week. During the full seven 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 that the difference of those numbers ($845 - 85 = 760$) equals the number of canceled visits caused by the strike. Our estimate of the cost of one visit is then (160,000 EUR per week)/(760 visits per week) = 210.5 EUR per visit. We abstract from any other fixed[37] and variable costs beyond salaries to nurses.[38]

Assuming that the four month and eight month visits have zero benefits then at age 5 the two-month visit has a return of 104.7 - 210.5 = - 105.8 EUR. Thus the cost of a nurse visit is higher than the benefits from fewer GP expenses at age 5 for the two-month visit. Similarly, the initial visit has a return of 375.9 - 210.5 = 165.4 EUR. 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.

When putting our estimates into context, we have to consider studies that focus on the extensive margin of other programs: Olds et al. (1999) refer to an economic evaluation of the nurse family partnership program by "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 cost of NHV over the lifetime. Paul et al. (2004) estimate the cost-effectiveness of early nurse home visits.[39] 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.

Our simple analysis indicates that early-life NHV is a cost-effective intervention. We show that, in our setting, the cost of the initial, early visit is considerably less than the

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

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

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

associated health care savings at age five. Furthermore, taking our regression results into consideration it seems plausible that savings will increase over time. Moreover, early-life health may generate spillovers to other dimensions of well-being and future productivity and thereby increase savings for society. At the same time our estimates indicate the importance of timing: at age five, the increase in GP fees caused by a missed two month visit is lower than the savings from cancelling a visit. Three constrains deserve mentioning: we do not consider potential longer run effects, benefits in other domains (e.g. the visits at older ages are more targeted towards examination of child development of speach, motor functioning and social contacts) and (given our estimation strategy) we calculate all costs and benefits relative to the four month and eight month visits.

6 Conclusion

Using nurse records and linked administrative data together with exogenous variation in the timing of foregone visits, we add to the literature impact of NHV at the intensive margin. Studying a universal program we provide new, policy relevant evidence on the effects of timing of NHV. We find that early NHV (in the very first weeks of the child's life) impacts child and maternal health trajectories (measured as health care usage). Specifically, our results show that nurse visits scheduled within the initial weeks of a child's life have longer term effects and earlier visits are more important for children's and mother's health than later visits. Furthermore, we find relatively larger effects when we use emergency GP contacts as outcome compared to regular working-hours GP contacts. This suggests actual health effects and not exclusively increased precautionary behavior or a better relationship with the family GP.

Importantly, early NHV also plays a role for maternal health outcomes. As a consequence, our results imply that early home visits are likely to impact children through their impact on mothers: Existing research documents strong correlations between maternal post-natal mental health and child outcomes in different domains, and highlights the importance of early detection of maternal mental health problems. Thus early universal home visits can play an important role in securing population maternal and child health through the prevention of undetected and hence untreated mental health problems.

We show that—besides screening—the provision of information and counselling constitute likely mechanisms for our observed effects. While we do not directly observe parental beliefs and only have few measures of parental investment behaviors, both may be contributing to the effects of early home visits that we find.

Finally, while initial visits in the Danish program focus on physical child health and needs and maternal mental well-being, later nurse visits increasingly focus on other domains of adequate child development and parent-child interactions. In our setting, we do not find that those visits impact the health outcomes that we can study. However, future research should explore further the potential role of NHV in shaping parental investments and child development throughout the first year of the child's life. In this paper, we are constrained by our design that relies on strike exposure. Thus we leave this topic as an important alley for future research.

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7 Figures and Tables

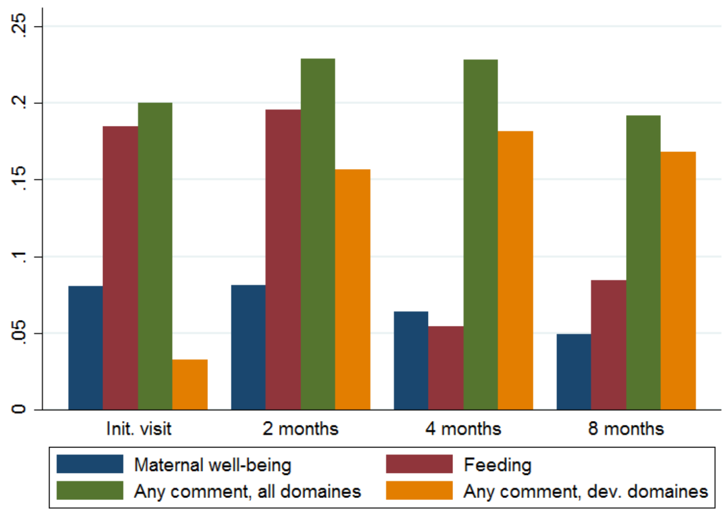
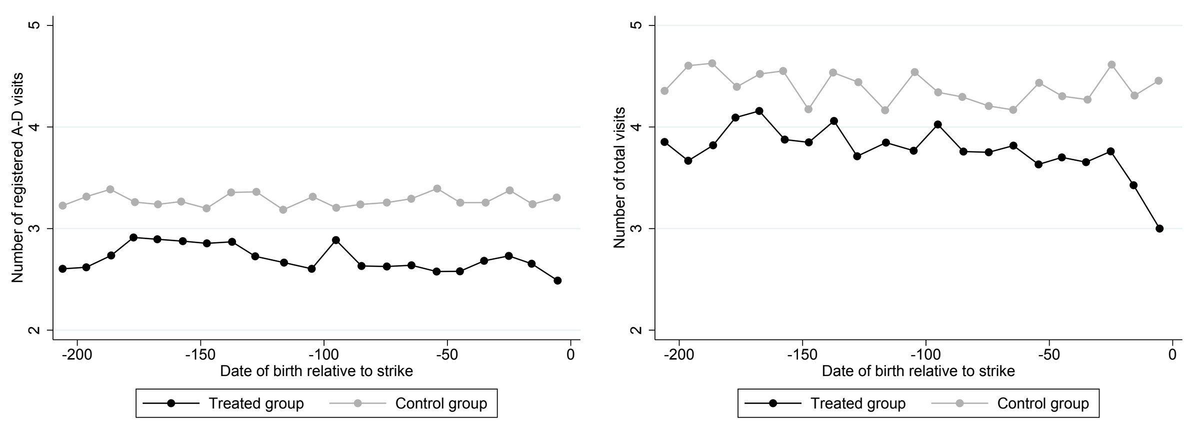


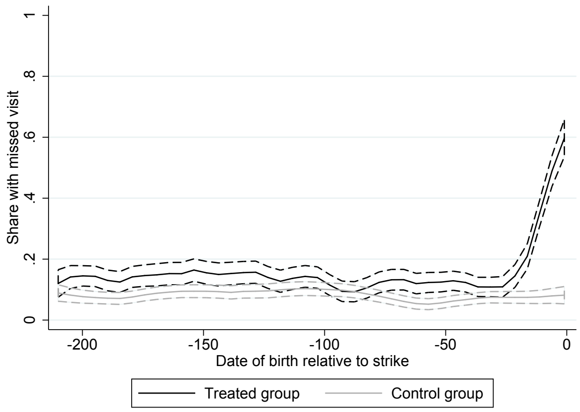
Fig. 1 Share of children with nurse registrations of problems at a given nurse visit (initial visit through eight months visit).

Notes: The share of children with comments in each domain is for all children with a performed visit and born between September 17, 2008 - April 15, 2009 (the control period).

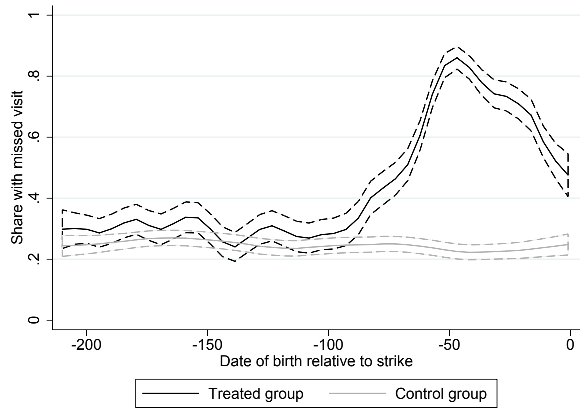


(a) Universal nurse visits **(b)** Total nurse visits (universal + extra)

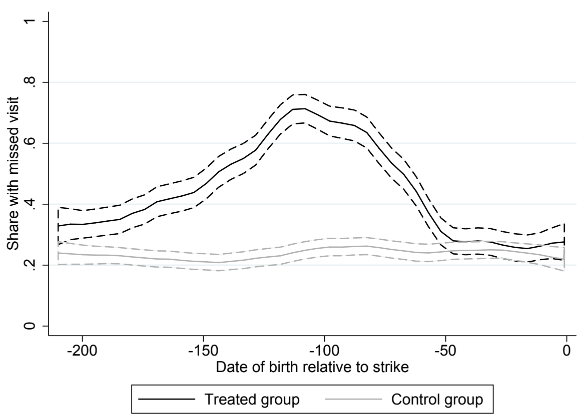
Fig. 2 Average number of universal and total nurse home visits for children in Copenhagen in the treated (2007/2008) and control period (2008/2009).



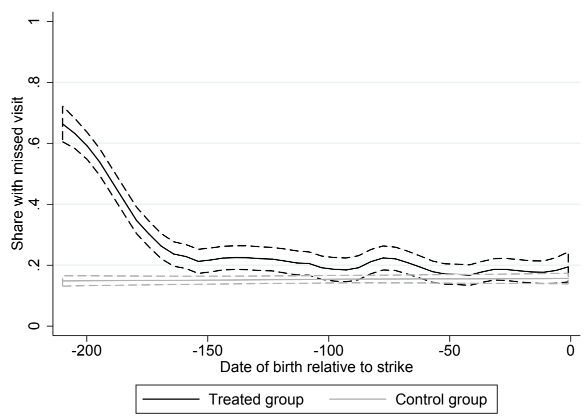
(a) Missed initial visit



(b) Missed two months visit

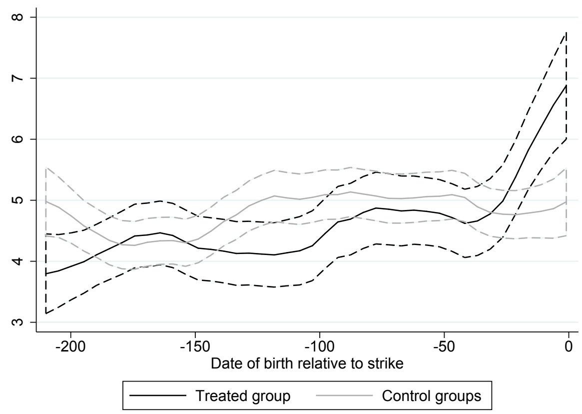


(c) Missed four months visit

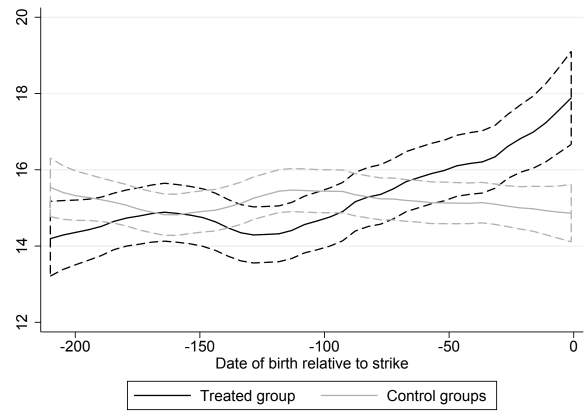


(d) Missed eight months visit

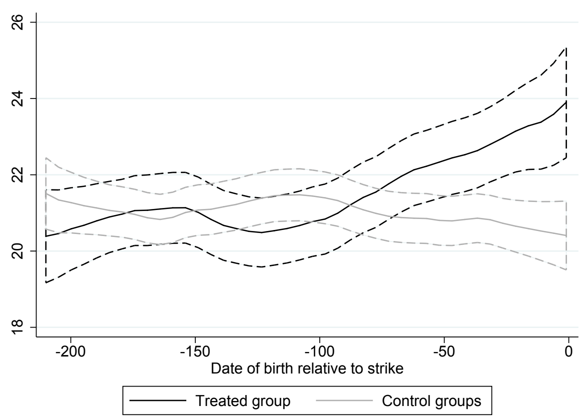
Fig. 3 Share of children with missed nurse visits for children born in the treated and control period. Notes: Treated period: September 18, 2007 - April 15, 2008. Control periods: September 17, 2008 and 2009 - April 15, 2009 and 2010).



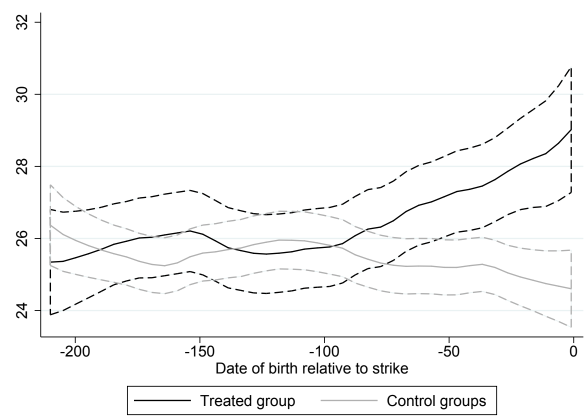
(a) Total GP contacts - one year



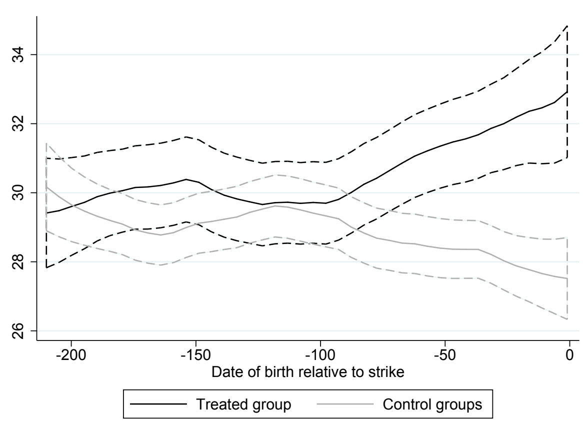
(b) Total GP contacts - two years



(c) Total GP contacts - three years

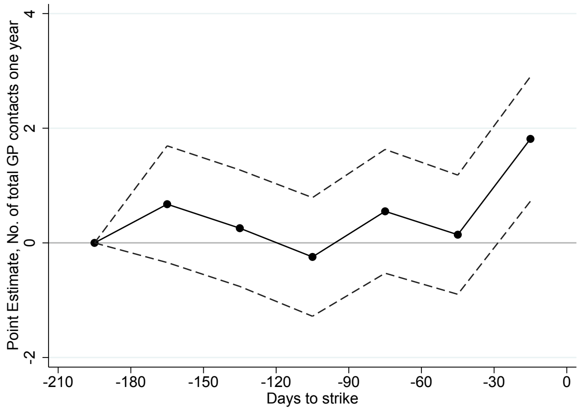


(d) Total GP contacts - four years

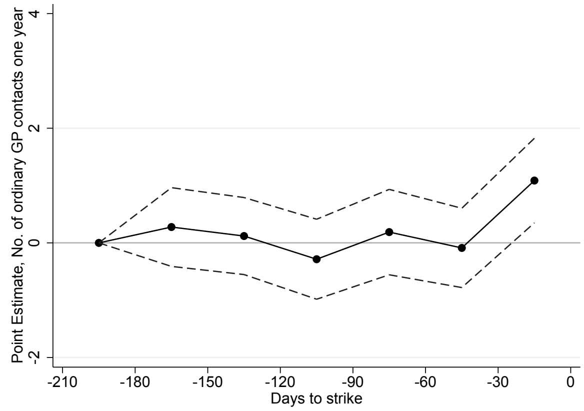


(e) Total GP contacts - five years

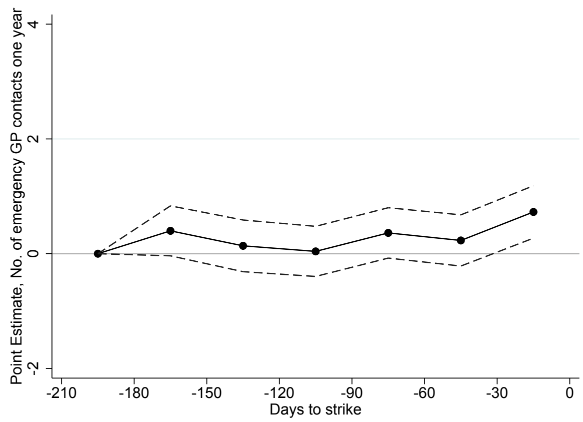
Fig. 4 Accumulated number of GP contacts for children born in the treated (2008) and control periods (2009, 2010).



(a) First year total GP contacts



(b) First year regular GP contacts



(c) First year emergency GP contacts

Fig. 5 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 ϕ_j 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 percent level (robust standard errors).

Table 1 Overview on main topics of nurse visits and nurse registrations in the municipality of Copenhagen.

		Examples for items (some visit-specific)	- - - *Visit* - - -			
	Topic		A 0-14days	B 2months	C 4 months	D 8 months
	Background	Issues related to pregnancy and birth, health risks (parental smoking, alcohol, BMI), family structure, etc	✓			
(1)	Postpartum maternal health	Physical and mental well-being, formal depression screening	✓	✓	✓	✓
(2)	Feeding	Breastfeeding, supplementary feeding, introduction of solid food	✓	✓	✓	✓
(3)	Parent-child interactions	Activities, parental recognition of infant needs/signals	✓	✓	✓	✓
(4)	Child signals and reactions	Sleep patterns, mood, smile/contact, differentiating btw adults	✓	✓	✓	✓
(5)	Child Examinations	Weight and height, jaundice	✓	✓	✓	✓
a.	Physical health	Sucking, crawling, Babinski	✓	✓	✓	✓
b.	Reflexes		✓	✓	✓	✓
c.	Tactile sense		✓	✓	✓	✓
d.	Head	Size, symmetry	✓	✓	✓	✓
e.	Skin and navel	Eczema, color and dryness	✓	✓	✓	✓
f.	Gross motor dev.	Infant: holds head, changes from stomach to back, sits alone, attempts to crawl		✓	✓	✓
g.	Eye-hand coordination	Infant: puts hand in mouth, sees her own hand, pinch grip		✓	✓	✓
h.	Vision	Infant: holds eye contact, follows objects	✓	✓	✓	✓
i.	Communication	Infant: smiles, chatters		✓	✓	✓
j.	Congenital malformations	Ears, hips, genitals, mouth	✓	✓	✓	✓

Notes: The table illustrates the plan for topics at the given home visit and the nurses' options for registrations in "DSI Sund", the registration system used by nurses in Copenhagen in the period that we study. Visits A-D refer to the type of the four scheduled universal visits. Additionally, nurses can offer a targeted pregnancy visit (around week 30 of the pregnancy), visits based on identified needs in the family and a visit at age 1.5 and 3 years, respectively.

Table 2 Variable means, strike exposed and control period

	Treated group		Control group	
	Mean	Obs.	Mean	Obs.
Total GP < 1y	4.61	4081	4.52	4269
Total GP < 5y	30.60	3934	29.93	4105
Ordin. GP < 1y	3.14	4081	3.01	4269
Ordin. GP < 5y	20.66	3934	20.34	4105
Emerg. GP < 1y	1.47	4081	1.50	4269
Emerg. GP < 5y	9.94	3934	9.59	4105
Vacc., 1st round	0.85	4081	0.88	4269
Vacc., 2nd round	0.87	4081	0.90	4269
Vacc., 3rd round	0.88	4081	0.90	4269
Prev. care, 5 weeks	0.88	4081	0.91	4269
Prev. care, 5 months	0.92	4081	0.93	4269
Prev. care, 12 months	0.93	4081	0.93	4269
Mother: Total GP 1st year	6.30	4081	6.48	4269
Mother: Total GP 2-5 years	23.37	4081	23.72	4269
Mother: Emerg. GP 1st year	0.55	4081	0.57	4269
Mother: Emerg. GP 2-5 years	2.51	4081	2.34	4269
Mother: Psych. diag., First year	0.01	4081	0.01	4269
Mother: Psych. hosp. adm., Three years	0.01	4081	0.00	4269
Mother: Psych. outpat. cont., Three years	0.03	4081	0.03	4269
Midwife visits	4.80	3970	4.73	4157
Smoking status, Mother	0.10	4014	0.09	4199
Child sex	0.48	4081	0.47	4269
Low birth weight	0.04	4009	0.06	4209
Preterm birth	0.06	4014	0.06	4199
C-section	0.21	4081	0.21	4269
Home birth	0.01	4081	0.01	4269
Cohabiting	0.76	4081	0.78	4269
Married	0.37	4081	0.39	4269
Prim. school, mother	0.15	4081	0.12	4269
Uni. degree, mother	0.30	4081	0.31	4269
Student, mother	0.05	4081	0.06	4269
Employed, mother	0.77	4081	0.78	4269
Danish, mother	0.76	4081	0.75	4269
Young mother	0.02	4081	0.02	4269
Young father	0.01	4014	0.01	4191
Income, mother	281.78	4081	287.28	4269
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 includes the period September 17, 2008 - April 15, 2009.

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 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. Parental covariates are paternal and maternal income, indicators for the highest level of parental education (primary school, high school, university degree), indicators for the mother currently studying or being employed, parental cohabitation and marital status and separate indicators for missing covariates. All covariates are measured a year prior to birth of the focal child. Child covariates include indicators for parental age below 21 at birth, indicators for a C-section, home birth, low birth weight (below 2500g), a preterm birth (below 37 weeks), child gender, maternal smoking status at birth and the number of pre-natal midwife visits. The sample includes children born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control period (September 17, 2008 - April 15, 2009). The outcomes in columns (1)-(4) are the probability of having missed the respective universal home visit. The outcome in column (5) is the number of universal nurse visits received. Column (6) presents results for the total number of nurse visits (universal and additional visits). 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)
Days								
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 for Table 3. In this table, we exclude covariates and we show estimates for the interactions of 30 day bins and a strike indicator from separate regressions for various subgroups along with the ratio between the full sample estimates and the various subgroup 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 for Table 3. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table 6 Effects of strike exposure on maternal health: GP contacts, post-natal psychiatric diagnoses and contacts with psychiatric specialists

	(1) Total GP 1st year mothers	(2) Total GP 2-5 years mothers	(3) Emerg. GP 1st year mothers	(4) Emerg. GP 2-5 years mothers	(5) Mother Psych. diag. First year	(6) Mother Psych. hosp. adm. Three years	(7) Mother Psych. outpat. cont. Three years
Days							
180-151	-0.365	-1.580	-0.068	-0.099	-0.007	-0.003	-0.010
	(0.342)	(1.292)	(0.085)	(0.257)	(0.008)	(0.005)	(0.012)
150-121	-0.067	0.580	0.011	0.103	0.002	0.002	0.004
	(0.337)	(1.264)	(0.089)	(0.246)	(0.008)	(0.006)	(0.012)
120-91	0.368	0.407	0.029	0.136	-0.001	-0.003	-0.006
	(0.360)	(1.322)	(0.088)	(0.263)	(0.009)	(0.006)	(0.013)
90-61	0.190	2.711**	0.054	0.738***	0.003	-0.001	-0.001
	(0.351)	(1.323)	(0.084)	(0.263)	(0.009)	(0.006)	(0.012)
60-31	0.443	1.482	0.171	0.790***	-0.000	-0.003	-0.001
	(0.364)	(1.334)	(0.105)	(0.274)	(0.008)	(0.005)	(0.012)
30-1	0.516	2.638**	0.100	0.733***	0.002	0.000	0.009
	(0.355)	(1.289)	(0.085)	(0.260)	(0.009)	(0.005)	(0.013)
Control group mean	6.39	23.97	0.54	2.15	0.01	0.01	0.03
Obs.	12078	11593	12078	11593	12078	12078	12078

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

Table 7 Heterogeneity: Effects of strike exposure on total GP contacts by parental education and initial child health

	Not health educ.		Health educ.		Higher parity		First-borns	
	Total GP 1st year (1)	Total GP 2-5 years (2)	Total GP 1st year (3)	Total GP 2-5 years (4)	Total GP 1st year (5)	Total GP 2-5 years (6)	Total GP 1st year (7)	Total GP 2-5 years (8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total GP 1st year	Total GP 2-5 years	Total GP 1st year	Total GP 2-5 years	Total GP 1st year	Total GP 2-5 years	Total GP 1st year	Total GP 2-5 years
Days								
180-151	0.741	0.945	0.638	3.425	0.901	-1.300	0.609	2.730*
	(0.572)	(1.248)	(1.245)	(2.731)	(0.755)	(1.737)	(0.701)	(1.518)
150-121	0.143	1.481	1.146	0.619	0.213	-0.997	0.325	2.867*
	(0.556)	(1.217)	(1.471)	(3.333)	(0.805)	(1.757)	(0.680)	(1.513)
120-91	-0.199	-0.355	-0.839	0.078	-0.448	-0.805	-0.046	0.646
	(0.571)	(1.165)	(1.376)	(2.803)	(0.818)	(1.721)	(0.692)	(1.413)
90-61	0.580	2.077*	0.100	-0.649	0.540	0.864	0.509	2.168
	(0.601)	(1.198)	(1.386)	(2.969)	(0.757)	(1.647)	(0.767)	(1.500)
60-31	0.018	2.862**	0.350	2.663	0.280	1.815	0.068	3.740**
	(0.580)	(1.215)	(1.267)	(2.758)	(0.779)	(1.624)	(0.715)	(1.527)
30-1	1.920***	4.558***	1.257	1.240	2.191***	2.273	1.542**	4.924***
	(0.610)	(1.280)	(1.321)	(2.445)	(0.833)	(1.709)	(0.736)	(1.547)
Control group mean	4.96	24.44	3.78	21.78	4.33	20.81	5.14	26.36
Observations	10445	10015	1633	1578	4750	4557	7328	7036

Notes: See notes for Table 3. Column labels indicate the relevant subgroup and outcome variable studied. Columns (1)-(4) split the sample by parental educational background in a health-related field (either one of the parents are educated as a doctor, midwife, nurse or pedagogue). Columns (5)-(8) split the sample by parity of the child. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table 8 Summary statistics by initial nurse registrations (Issues related to maternal well-being) for children in the control period (2008/2009), means and standard deviations.

	Registration		No registration	
	Mean	Obs.	Mean	Obs.
Initial visit	1.00	316	1.00	3602
Maternal well-being, B visit	0.30	265	0.06	2830
Feeding, B visit	0.35	265	0.18	2830
Child-parent contact, B visit	0.09	265	0.02	2830
Comment: Any area	0.31	265	0.22	2830
Comment: Developmental aspect	0.22	265	0.15	2830
Maternal well-being, C visit	0.21	270	0.05	2848
Feeding, C visit	0.14	270	0.05	2848
Child-parent contact, C visit	0.05	270	0.01	2848
Comment: Any area	0.24	270	0.23	2848
Comment: Developmental aspect	0.20	270	0.18	2848
Maternal well-being, D visit	0.16	284	0.04	3137
Feeding, D visit	0.14	284	0.08	3137
Child-parent contact, D visit	0.04	284	0.01	3137
Comment: Any area	0.20	284	0.19	3137
Comment: Developmental aspect	0.17	284	0.17	3137
Any referral	0.26	284	0.12	3137
Total GP visits (child), 3-5 years	16.32	302	14.33	3461
Mother: Psych. hosp. adm. in first 3 years	0.02	316	0.00	3602
Mother: Psych. outpat. contact in first 3 years	0.11	316	0.02	3602

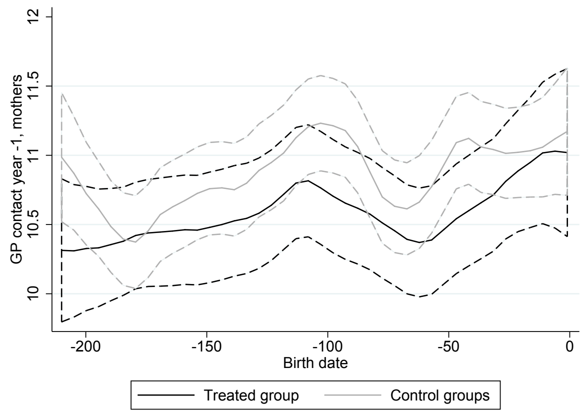
Notes: The sample of children includes children who have received an initial nurse visit (A visit) and who are born between September 17, 2008 and April 15, 2009. For at detailed description see section 4.4.

Table 9 Effect of strike exposure on child health measured as accumulated total GP fees, Euro

	(1) GP fees 1st year	(2) GP fees 2nd year	(3) GP fees 3-5 years	(4) GP fees < 5y
Days				
180-151	18.825*	3.255	21.606*	43.077*
	(10.847)	(8.585)	(12.925)	(25.155)
150-121	3.836	8.452	20.936	31.716
	(10.663)	(8.463)	(12.920)	(24.697)
120-91	-7.211	-2.101	-0.177	-8.178
	(10.919)	(8.205)	(12.112)	(24.017)
90-61	6.339	14.212*	22.229*	44.679*
	(11.034)	(8.501)	(12.498)	(24.753)
60-31	0.656	35.121***	14.656	53.542**
	(10.743)	(8.537)	(12.432)	(24.886)
30-1	36.783***	30.905***	33.726***	98.111***
	(11.207)	(8.507)	(12.625)	(25.352)
Control group mean	103.43	193.03	243.86	550.48
Obs.	12078	12078	12078	11652

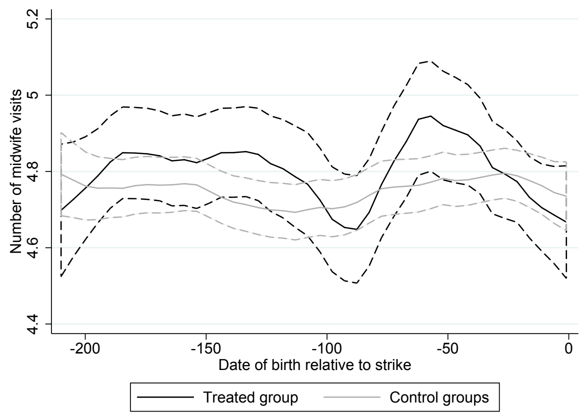
Notes: See notes for Table 3. GP fees are measured in Euro (2015 prices). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

A Appendix - For online publication

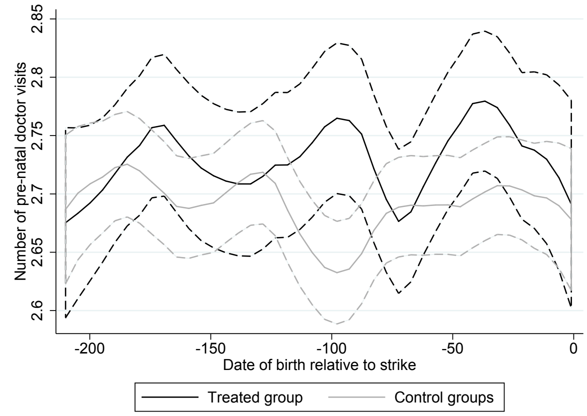


(a) Mothers GP contacts, 365 days prior birth

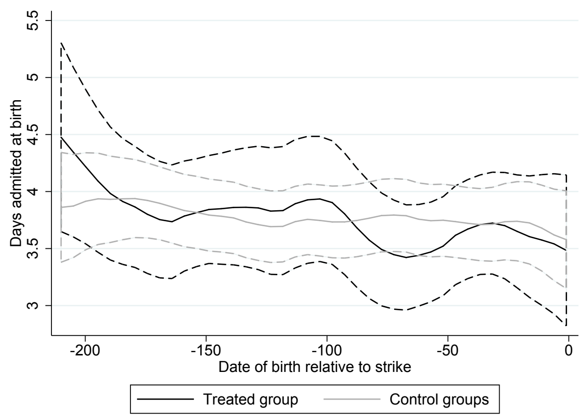
Fig. A1 Number of mothers' GP contacts in the year prior to birth.



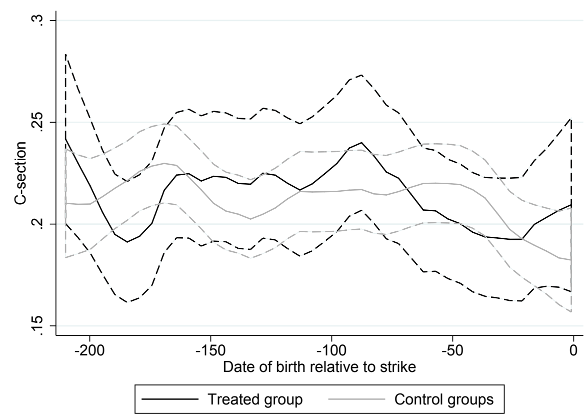
(a) Midwife visits



(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).

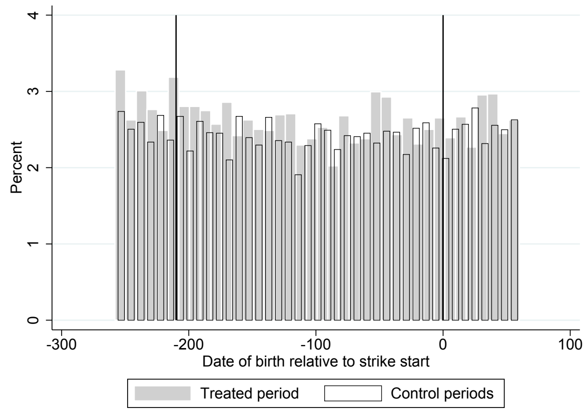


Fig. A3 Density of births

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).

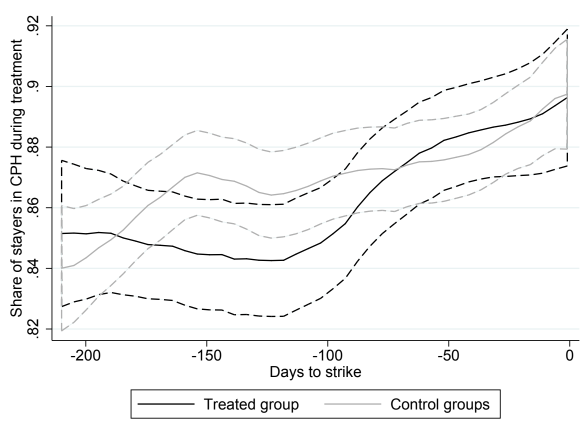
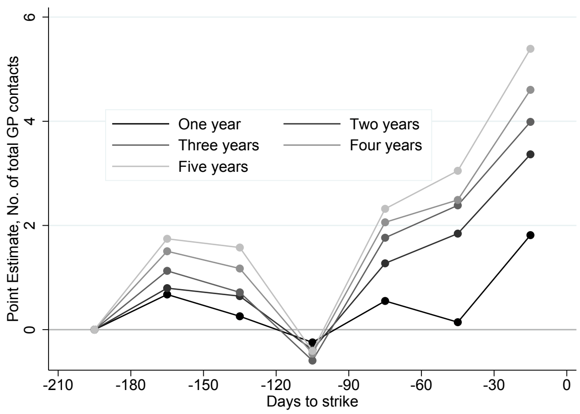
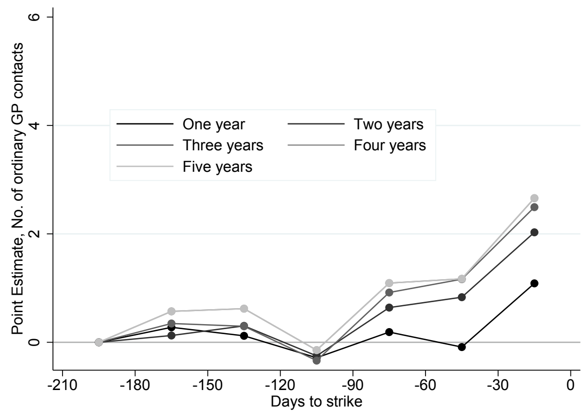


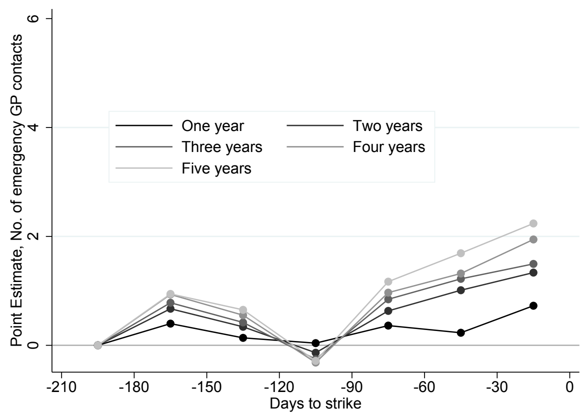
Fig. A4 Share of children observed as Copenhagen residents on January 1 in the treated (2008) and control periods (2009, 2010).



(a) Total GP contacts

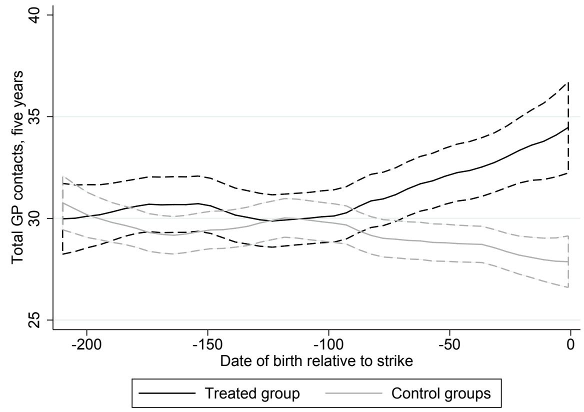


(b) regular GP contacts

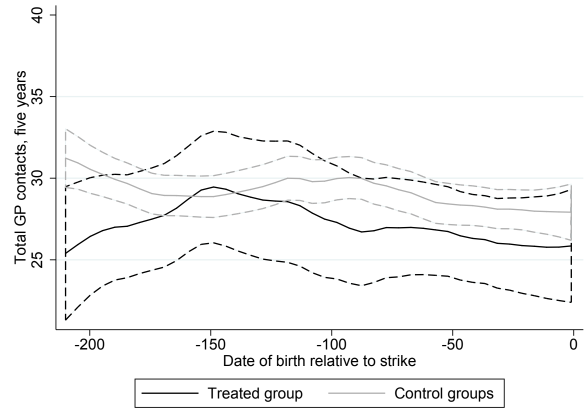


(c) Emergency GP contacts

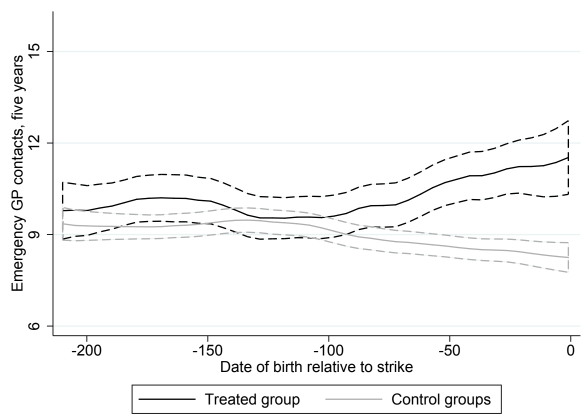
Fig. A5 Coefficients plot: Total GP contacts (a) and decomposed in regular (b) and emergency contacts (c).
Notes: See notes to figure 5.



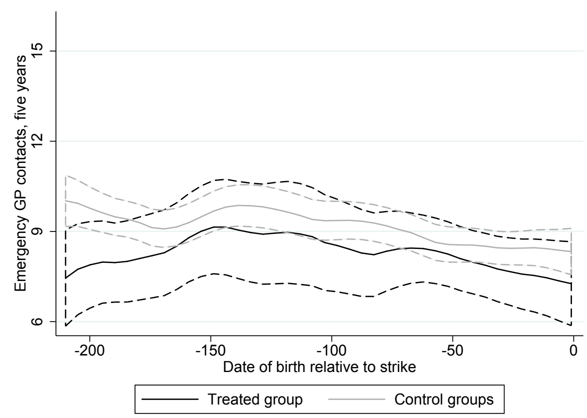
(a) Total GP contacts - Not Health educated



(b) Total GP contacts - Health educated



(c) Emergency GP contacts - Not health educated



(d) Emergency GP contacts - Health educated

Fig. A6 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 eligibility)	Timing
Universal visits	
Initial visit (A)	0-14 days after birth
2-month visit (B)	After two month of life
4-month visit (C)	After four month of life
8-month visit (D)	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 for the Copenhagen NHV program.

Table A2 Balance testing: Parental covariates as outcome

	Prim. school, mother (1)	Prim. school, father (2)	Income, mother (3)	Income, father (4)	Cohabiting (5)	Married (6)	Young mother (7)	Young father (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 for Table A2. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A4 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 A5 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)
Days								
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 for Table 4. 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 A6 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)
Days								
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 4. 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 A7 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)
Days								
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 4. 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 A8 Additional child health outcomes: Effects of strike exposure on child hospitalization and the probability of delayed school start

	(1) Hospital adm. < 1y	(2) Hospital adm. 3-4y	(3) Outpat. cont. < 1y	(4) Outpat. cont. 3-4y	(5) Delayed school start
Days					
180-151	0.008	-0.022	0.009	-0.004	0.011
	(0.030)	(0.018)	(0.035)	(0.030)	(0.029)
150-121	-0.032	-0.009	0.038	-0.023	-0.020
	(0.030)	(0.020)	(0.035)	(0.030)	(0.032)
120-91	0.001	-0.001	-0.012	-0.020	0.037
	(0.031)	(0.020)	(0.035)	(0.030)	(0.030)
90-61	0.037	0.006	-0.003	-0.042	-0.020
	(0.031)	(0.019)	(0.035)	(0.030)	(0.023)
60-31	0.004	-0.007	-0.027	-0.023	-0.010
	(0.030)	(0.019)	(0.035)	(0.030)	(0.024)
30-1	0.036	-0.014	-0.060*	-0.027	-0.012
	(0.030)	(0.019)	(0.034)	(0.030)	(0.025)
Control group mean	0.27	0.08	0.39	0.26	0.14
Obs.	12078	12078	12078	12078	7874

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

Table A9 Effects of strike exposure on child health, mother health and child-parent relationship: Nurse registrations at 8-month visit and constrained sample

	(1) Mother well-being	(2) Feeding	(3) Child-parent contact	(4) Any comment	(5) Comment: Development	(6) Any referral
Days						
120-91	0.010	-0.027	0.012	-0.024	-0.013	0.058**
	(0.018)	(0.025)	(0.010)	(0.036)	(0.034)	(0.029)
90-61	0.015	-0.004	0.013	-0.033	-0.011	0.045
	(0.019)	(0.026)	(0.010)	(0.036)	(0.035)	(0.030)
60-31	0.011	-0.027	0.010	0.009	0.016	0.026
	(0.018)	(0.026)	(0.010)	(0.035)	(0.033)	(0.029)
30-1	0.008	-0.036	0.005	-0.027	-0.022	0.007
	(0.018)	(0.026)	(0.010)	(0.035)	(0.034)	(0.029)
Control group mean	0.05	0.08	0.01	0.17	0.15	0.13
Obs.	4612	4612	4612	4612	4612	4612

Notes: See notes for Table 3. We constrain the sample to children born 150 days prior to April 15, 2008 and 2009. We further constrain our sample to children, who have received a D visit at around 8-10 months. Outcomes in column (1)-(5) are equal to 1 if nurses register problems in the specific categories and zero otherwise. The outcome in column (6) is an indicator for any 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 A10 Heterogeneity: Effects of strike exposure on emergency GP contacts by parental education and initial child health

	Not health educ.		Health educ.		Higher parity		First-borns	
	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years
Days								
180-151	0.439*	0.444	0.381	2.159*	0.479	-0.502	0.406	1.324*
	(0.249)	(0.662)	(0.421)	(1.235)	(0.338)	(0.921)	(0.294)	(0.784)
150-121	0.090	0.414	0.531	1.533	0.313	-0.145	0.072	1.027
	(0.243)	(0.636)	(0.668)	(1.669)	(0.395)	(0.928)	(0.286)	(0.780)
120-91	0.075	-0.463	-0.171	0.443	-0.001	-0.673	0.137	0.026
	(0.246)	(0.607)	(0.472)	(1.271)	(0.396)	(0.867)	(0.276)	(0.727)
90-61	0.406	0.744	0.039	0.920	0.279	0.196	0.399	1.097
	(0.247)	(0.621)	(0.495)	(1.356)	(0.326)	(0.852)	(0.304)	(0.759)
60-31	0.223	1.400**	0.247	1.963	0.243	0.601	0.274	2.133***
	(0.253)	(0.648)	(0.438)	(1.194)	(0.351)	(0.841)	(0.300)	(0.807)
30-1	0.793***	1.899***	0.520	0.685	0.927**	0.853	0.598**	2.153***
	(0.263)	(0.647)	(0.437)	(1.125)	(0.373)	(0.857)	(0.299)	(0.781)
Control group mean	1.61	7.44	1.08	6.22	1.40	6.22	1.64	8.00
Observations	10445	10015	1633	1578	4750	4557	7328	7036

Notes: See notes to table 7. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A11 Heterogeneity: Effects of strike exposure on total GP contacts, interacted model

	Health education		Parity	
	Total GP 1st year	Total GP 2-5 years	Total GP 1st year	Total GP 2-5 years
	(1)	(2)	(3)	(4)
	(1)	(2)	(3)	(4)
	Total GP 1st year	Total GP 2-5 years	Total GP 1st year	Total GP 2-5 years
Days				
180-151	-0.557	1.988	-0.203	3.958*
	(1.341)	(3.115)	(1.027)	(2.308)
150-121	0.816	-0.402	0.097	3.824*
	(1.539)	(3.542)	(1.054)	(2.317)
120-91	-0.492	1.533	0.513	1.300
	(1.481)	(3.071)	(1.070)	(2.217)
90-61	-0.372	-1.619	-0.171	1.023
	(1.498)	(3.170)	(1.076)	(2.221)
60-31	0.274	0.822	-0.150	1.866
	(1.360)	(3.025)	(1.053)	(2.221)
30-1	-0.921	-3.517	-0.601	2.471
	(1.451)	(2.768)	(1.111)	(2.305)
Observations	12078	11593	12078	11593

Notes: Each column shows the estimates from a separate regression. Column labels indicate the relevant subgroup of our sample. 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 all control variables (see notes for Table 3). 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 Heterogeneity: Effects of strike exposure on emergency GP contacts, interacted model

	Health education		Parity	
	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years
	(1)	(2)	(3)	(4)
	(1)	(2)	(3)	(4)
	Emerg. GP 1st year	Emerg. GP 2-5 years	Emerg. GP 1st year	Emerg. GP 2-5 years
Days				
180-151	-0.238	1.544	-0.024	1.730
	(0.483)	(1.423)	(0.449)	(1.211)
150-121	0.377	1.394	-0.221	1.069
	(0.703)	(1.773)	(0.490)	(1.214)
120-91	-0.221	1.306	0.184	0.568
	(0.525)	(1.387)	(0.482)	(1.130)
90-61	-0.332	0.755	0.077	0.762
	(0.543)	(1.468)	(0.444)	(1.137)
60-31	-0.055	0.761	0.074	1.396
	(0.495)	(1.360)	(0.458)	(1.160)
30-1	-0.422	-1.317	-0.303	1.146
	(0.510)	(1.285)	(0.476)	(1.153)
Observations	12078	11593	12078	11593

Notes: Each column shows the estimates from a separate regression. See notes for Table A11. 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 A13 Heterogeneity: Effects of strike exposure on 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)
Days								
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
Observations	6047	5605	10732	920	8340	3312	10606	1046

Notes: See notes to table 3 and 7. Columns (1)-(2) split the sample by child gender. Columns (3)-(4) split the sample by initial health (low birth weight, premature birth or complications during birth). Columns (5)-(6) split the sample by parental socio-economic status (SES). A low SES background is a child born to parents with either incomes in the bottom decile, below age 21 at birth or with only primary schooling. Columns (7)-(8) split the sample 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 A14 Heterogeneity: Effects of strike exposure on total GP contacts, interacted model

	Gender (1)	Initial health (2)	SES (3)	Smoking, mother (4)
Days				
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. See notes for Table A11. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A15 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 for Table 3. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A16 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 for Table 3. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A17 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 for Table 3. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A18 Robustness: Effects of strike exposure on child GP contacts without pre-treatment covariates

	(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	0.712	0.025	0.865	0.323	-0.211	0.615	0.390*	0.236	0.250
	(0.516)	(0.513)	(0.767)	(0.347)	(0.318)	(0.509)	(0.222)	(0.296)	(0.387)
150-121	0.280	0.305	0.938	0.155	0.142	0.711	0.125	0.163	0.227
	(0.516)	(0.505)	(0.781)	(0.340)	(0.317)	(0.509)	(0.229)	(0.289)	(0.392)
120-91	-0.357	-0.334	-0.377	-0.335	-0.053	-0.075	-0.022	-0.281	-0.301
	(0.519)	(0.486)	(0.719)	(0.350)	(0.305)	(0.495)	(0.220)	(0.283)	(0.356)
90-61	0.614	0.763	1.386*	0.234	0.469	0.787	0.381*	0.294	0.599
	(0.546)	(0.510)	(0.742)	(0.375)	(0.327)	(0.506)	(0.223)	(0.284)	(0.373)
60-31	0.063	1.571***	0.907	-0.096	0.896***	0.407	0.158	0.675**	0.500
	(0.521)	(0.501)	(0.735)	(0.347)	(0.317)	(0.486)	(0.223)	(0.288)	(0.373)
30-1	1.743***	1.362***	1.915**	1.064***	0.781**	1.105**	0.679***	0.581**	0.809**
	(0.549)	(0.516)	(0.763)	(0.371)	(0.328)	(0.513)	(0.233)	(0.288)	(0.373)
Control group mean	4.80	10.35	13.63	3.26	6.89	9.85	1.54	3.46	3.78
Obs.	12568	12464	12070	12568	12464	12070	12568	12464	12070

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

Table A19 Robustness: Effects of strike exposure on child GP contacts, larger bin size - 35 days

	(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									
175-141	0.490	0.318	0.631	0.274	0.089	0.440	0.216	0.229	0.191
	(0.479)	(0.472)	(0.721)	(0.321)	(0.294)	(0.478)	(0.210)	(0.271)	(0.360)
140-106	-0.619	-0.334	0.110	-0.453	-0.027	0.207	-0.166	-0.307	-0.097
	(0.483)	(0.472)	(0.700)	(0.324)	(0.296)	(0.473)	(0.207)	(0.270)	(0.347)
105-71	0.046	0.412	0.015	-0.070	0.355	-0.190	0.116	0.057	0.205
	(0.508)	(0.463)	(0.664)	(0.348)	(0.293)	(0.457)	(0.212)	(0.265)	(0.332)
70-36	0.219	1.450***	1.291*	0.008	0.788***	0.573	0.211	0.662**	0.719**
	(0.491)	(0.465)	(0.676)	(0.330)	(0.298)	(0.456)	(0.206)	(0.265)	(0.338)
35-1	1.375***	1.579***	1.683**	0.810**	1.052***	0.916*	0.565***	0.527*	0.767**
	(0.512)	(0.485)	(0.698)	(0.346)	(0.308)	(0.477)	(0.219)	(0.272)	(0.338)
Control group mean	4.80	10.35	13.63	3.26	6.89	9.85	1.54	3.46	3.78
Observations	12078	11982	11604	12078	11982	11604	12078	11982	11604

Notes: See notes to table 3. We increase the bin size to 35 days. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A20 Robustness: Effects of strike exposure on child GP contacts, smaller bin size - 21 days

	(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									
189-169	1.008*	0.00730	1.388	0.536	-0.0659	0.926	0.472*	0.0732	0.462
	(0.606)	(0.599)	(0.882)	(0.410)	(0.381)	(0.594)	(0.258)	(0.340)	(0.441)
168-148	1.011*	0.373	0.759	0.691*	0.0792	0.803	0.321	0.294	-0.0447
	(0.609)	(0.615)	(0.924)	(0.414)	(0.378)	(0.611)	(0.259)	(0.353)	(0.469)
147-127	0.274	0.491	0.610	0.290	0.437	0.729	-0.0155	0.0540	-0.118
	(0.623)	(0.609)	(0.934)	(0.410)	(0.384)	(0.624)	(0.278)	(0.346)	(0.455)
126-106	-0.180	-0.409	0.643	-0.207	-0.0567	0.647	0.0278	-0.353	-0.00403
	(0.614)	(0.600)	(0.888)	(0.420)	(0.381)	(0.605)	(0.250)	(0.337)	(0.453)
105-85	0.584	0.731	0.424	0.305	0.504	0.209	0.278	0.227	0.216
	(0.653)	(0.587)	(0.845)	(0.443)	(0.377)	(0.582)	(0.281)	(0.338)	(0.426)
84-64	0.506	0.260	0.866	0.190	0.259	0.574	0.316	0.00154	0.292
	(0.643)	(0.606)	(0.863)	(0.445)	(0.387)	(0.597)	(0.257)	(0.333)	(0.430)
63-43	0.897	1.740***	1.575*	0.494	1.053***	0.953	0.403	0.687**	0.621
	(0.629)	(0.595)	(0.864)	(0.425)	(0.388)	(0.590)	(0.262)	(0.336)	(0.429)
42-22	0.364	1.759***	2.280**	0.107	1.172***	1.231**	0.257	0.588*	1.049**
	(0.637)	(0.620)	(0.916)	(0.426)	(0.393)	(0.594)	(0.275)	(0.347)	(0.474)
21-1	2.676***	1.749***	1.979**	1.727***	0.942**	1.185*	0.949***	0.807**	0.794*
	(0.664)	(0.623)	(0.904)	(0.452)	(0.396)	(0.627)	(0.277)	(0.350)	(0.431)
Control group mean	4.80	10.35	13.63	3.26	6.89	9.85	1.54	3.46	3.78
Observations	12078	11982	11604	12078	11982	11604	12078	11982	11604

Notes: See notes to table 3. We reduce the bin size to 21 days. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A21 Robustness: Effects of strike exposure on child 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 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	0.658	0.068	0.505	0.269	-0.234	0.375	0.389*	0.301	0.130
	(0.488)	(0.485)	(0.740)	(0.330)	(0.301)	(0.490)	(0.208)	(0.278)	(0.373)
150-121	1.207**	0.283	0.607	0.744**	0.093	0.373	0.463**	0.190	0.234
	(0.482)	(0.478)	(0.744)	(0.320)	(0.302)	(0.487)	(0.211)	(0.272)	(0.372)
120-91	0.215	-0.121	-0.297	0.006	0.041	0.075	0.209	-0.162	-0.372
	(0.493)	(0.462)	(0.684)	(0.336)	(0.293)	(0.474)	(0.204)	(0.264)	(0.336)
90-61	0.817	0.561	0.484	0.357	0.409	0.223	0.460**	0.152	0.262
	(0.519)	(0.482)	(0.711)	(0.361)	(0.312)	(0.486)	(0.208)	(0.266)	(0.356)
60-31	0.371	1.636***	0.965	0.036	0.918***	0.440	0.335	0.718***	0.525
	(0.495)	(0.480)	(0.703)	(0.331)	(0.303)	(0.462)	(0.210)	(0.275)	(0.359)
30-1	1.760***	1.417***	1.567**	1.076***	0.948***	0.907*	0.684***	0.469*	0.660*
	(0.527)	(0.493)	(0.735)	(0.357)	(0.315)	(0.494)	(0.221)	(0.275)	(0.361)
Control group mean	4.49	10.33	14.17	3.08	6.92	10.08	1.41	3.41	4.09
Obs.	15736	15616	15097	15736	15616	15097	15736	15616	15097

Notes: See notes to table 3. We add an additional 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 A22 Robustness: Effects of strike exposure on child 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 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	0.561	0.181	1.070	0.328	-0.209	0.615	0.233	0.391	0.455
	(0.596)	(0.595)	(0.881)	(0.400)	(0.372)	(0.583)	(0.258)	(0.345)	(0.444)
150-121	0.347	0.546	1.381	0.399	0.349	1.019*	-0.051	0.197	0.362
	(0.596)	(0.598)	(0.899)	(0.387)	(0.366)	(0.584)	(0.275)	(0.351)	(0.462)
120-91	0.323	-0.450	0.207	0.267	-0.316	0.332	0.056	-0.134	-0.125
	(0.607)	(0.573)	(0.842)	(0.404)	(0.362)	(0.579)	(0.263)	(0.332)	(0.417)
90-61	0.709	0.418	1.030	0.354	0.267	0.467	0.356	0.151	0.563
	(0.629)	(0.598)	(0.867)	(0.434)	(0.380)	(0.587)	(0.257)	(0.337)	(0.437)
60-31	0.161	1.739***	1.604*	-0.085	0.779**	0.833	0.247	0.960***	0.771*
	(0.613)	(0.588)	(0.867)	(0.410)	(0.374)	(0.569)	(0.264)	(0.336)	(0.443)
30-1	2.461***	2.008***	2.182**	1.623***	0.995***	1.180**	0.839***	1.012***	1.002**
	(0.627)	(0.594)	(0.877)	(0.424)	(0.382)	(0.594)	(0.265)	(0.332)	(0.429)
Control group mean	4.52	10.82	14.47	3.01	7.17	10.07	1.50	3.65	4.40
Obs.	7874	7814	7564	7874	7814	7564	7874	7814	7564

Notes: See notes to table 3. We reduce the sample to only include 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 the control period (September 17, 2008 - April 15, 2009). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A23 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 (see notes for Table 3). 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 A24 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 A23. 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 A25 Robustness: Effects of strike exposure on child GP contacts, including movers from Copenhagen

	(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	0.449	-0.197	0.732	0.202	-0.199	0.724	0.247	0.002	0.008
	(0.480)	(0.484)	(0.716)	(0.325)	(0.301)	(0.480)	(0.204)	(0.279)	(0.360)
150-121	0.209	0.234	0.885	0.105	0.157	0.662	0.104	0.076	0.223
	(0.483)	(0.475)	(0.713)	(0.319)	(0.303)	(0.470)	(0.215)	(0.269)	(0.360)
120-91	-0.428	-0.554	-0.295	-0.327	-0.066	0.150	-0.101	-0.488*	-0.445
	(0.484)	(0.465)	(0.668)	(0.327)	(0.298)	(0.464)	(0.202)	(0.265)	(0.328)
90-61	0.238	0.418	0.812	-0.013	0.340	0.478	0.251	0.078	0.334
	(0.506)	(0.482)	(0.698)	(0.348)	(0.311)	(0.480)	(0.206)	(0.267)	(0.348)
60-31	-0.153	1.384***	0.906	-0.178	0.812***	0.625	0.025	0.572**	0.280
	(0.491)	(0.479)	(0.690)	(0.327)	(0.305)	(0.462)	(0.210)	(0.273)	(0.349)
30-1	1.418***	1.146**	1.557**	0.925***	0.720**	0.908*	0.494**	0.426	0.649*
	(0.518)	(0.490)	(0.713)	(0.352)	(0.316)	(0.483)	(0.217)	(0.271)	(0.349)
Control group mean	4.78	10.41	13.69	3.24	6.94	9.90	1.54	3.47	3.80
Obs.	13918	13611	13197	13918	13611	13197	13918	13611	13197

Notes: See notes to table 3. 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: Effects of strike exposure on child GP contacts, doughnut approach

	(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	0.668	0.118	0.977	0.272	-0.152	0.660	0.396*	0.270	0.317
	(0.519)	(0.515)	(0.771)	(0.351)	(0.320)	(0.515)	(0.222)	(0.296)	(0.386)
150-121	0.269	0.399	0.917	0.127	0.194	0.610	0.142	0.205	0.307
	(0.519)	(0.508)	(0.776)	(0.343)	(0.319)	(0.510)	(0.230)	(0.290)	(0.388)
120-91	-0.241	-0.185	-0.051	-0.284	0.012	0.144	0.044	-0.197	-0.195
	(0.528)	(0.492)	(0.718)	(0.356)	(0.310)	(0.499)	(0.223)	(0.283)	(0.351)
90-61	0.561	0.688	1.098	0.194	0.444	0.549	0.367	0.244	0.549
	(0.551)	(0.512)	(0.744)	(0.380)	(0.329)	(0.511)	(0.224)	(0.284)	(0.369)
60-31	0.141	1.691***	1.127	-0.088	0.914***	0.488	0.228	0.777***	0.639*
	(0.531)	(0.508)	(0.739)	(0.353)	(0.321)	(0.490)	(0.228)	(0.291)	(0.374)
30-1	0.959	0.998*	1.733*	0.478	0.696*	0.807	0.481*	0.303	0.926**
	(0.671)	(0.603)	(0.899)	(0.451)	(0.401)	(0.603)	(0.288)	(0.322)	(0.440)
Control group mean									
Obs.	11307	11227	10877	11307	11227	10877	11307	11227	10877

Notes: See notes to table 3. We drop children born within 14 days of strike start. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.