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Missing a Nurse Visit*

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Abstract

This paper studies the impact of the timing of early-life investment policies on child and maternal health. We exploit variation induced by a 2008 Danish nurse strike that resulted in a large-scale cancellation of home visits for families with infants. Combining unique nurse records with administrative data, we show that missing the first but not later visits increases child and mother contacts to health professionals and the probability of maternal mental health issues. We show that likely mechanisms for these results include nurses' focus on timely maternal mental health screening and information provision to new families.

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1 Introduction

Mounting evidence documents the short- and long-run impact of early-life circumstances for the health and human capital development of children (Almond and Currie, 2011; Almond et al., 2018). In the light of this research, policy decisions in many settings do not evolve around whether or not to provide early-life investment policies, but around the ways in which to design them. This paper contributes causal evidence on the importance of the timing of early investments for child and maternal health. Evidence on this topic is sparse but instrumental for policy: Equivalent to evidence from various disciplines about the importance of the timing of health *shocks*—famously highlighting the critical importance of the period prior to and shortly after birth (Barker, 1990; Gluckman et al., 2008)—evidence on the importance of timing, intensity and content of early-life investment policies can help improve the design of existing interventions, such as public health policies or preschool programs.

We study a popular early-life investment policy: nurse home visiting for newborns and their families. Specifically, we ask: What is the relative importance of early vs. later nurse visits for infants' and mothers' health and well-being? In Denmark, the setting of our work, all new families are eligible for up to five universal home visits by a trained nurse during the first year of a child's life. During each of those visits, nurses provide visit- and thus age-specific health screenings, information and counseling. Furthermore, they refer families to other health care professionals if necessary. To examine the relative importance of the first-year home visits, we exploit the exposure of new families to a nurse strike, that altered the provision of home visits for a cohort of children: In 2008, the central negotiations for employees in the public health care sector (nurses, midwives and other personnel) broke down and resulted in a labor market conflict. Thus on April 15, 2008 the unionized health care employees went on strike. As a result, up to 45 percent of all public employees were on

strike during the next 61 days (Due and Madsen, 2008), leading to large-scale cancellation of non-emergency health services, among them nurse home visits.[1]

We exploit strike-induced variation in the timing of nurse visits for a treated cohort together with information on children born in non-strike years: We compare differences in outcomes within the strike-exposed cohort (who are all exposed to the strike but at different ages) to the differences in outcomes within a non-strike exposed cohort (who are born on the same day of the year in a control period). As a result, our estimates are in the spirit of a difference-in-differences design and assess the importance of forgoing one nurse visit but doing so at different ages. We thereby shed light on the question as to whether the timing (and thus age-specific content) of a missed nurse visit matters for child and maternal outcomes. This question of the relative importance of differently timed visits is policy relevant as every attempt to optimize the number (and content) of visits in an existing program must take into account potential impacts of their timing. Thus the two dimensions are typically inseparable in real-world policy decisions.

To make our study feasible, we link newly-collected individual-level data on program take-up in the largest municipality in Denmark, Copenhagen, to administrative data on family background and health outcomes. While Scandinavia is well-known for high-quality administrative data in many domains, national administrative data sources typically lack individual-level data on municipal programs, such as NHV or preschools. We break new grounds by compiling data on actual program take-up, allowing us to be specific about the intensity of the treatment that we study. The link to administrative data gives us a rich set of health outcomes and allows us to analyze the credibility of our empirical design by assessing compliance with the nurse strike across different groups of families.

In our first set of results, we show that the strike resulted in a mass cancellation of nurse visits in Copenhagen: Comparing the strike period to the same period in control years, we

[1]In Denmark, both private and public wages are to a large degree determined by collective bargaining and the vast majority of public nurses are covered by collective agreements and thus also by labor market conflicts (Ibsen et al., 2011).

show that 90 percent of nurse home visits were canceled. Importantly, due to both capacity constraints and the visits' age-specific content, canceled visits were not rescheduled. We show that children born in the seven months before the strike on average missed one scheduled postnatal nurse visit. Depending on their date of birth relative to the strike, these children had a different age at the forgone visit. Exploiting the merged Copenhagen nurse records and administrative data on family background, we show that the strike affected families similarly across characteristics that nurses are likely to observe. This finding illustrates the broad coverage of the strike in Copenhagen and relieves concerns that nurses strategically decided which families should forgo their visit. Additionally, we show that (given that all children were born before the onset of the strike) other aspects of care around birth (such as prenatal midwife contacts or hospital admissions at birth) were not affected by children's strike exposure.

In our analysis of the impact of strike exposure during infancy, we show that exposure during the initial months of a life is more influential for child and maternal health relative to later exposure. We measure health by the uptake of additional medical care: Children, who were born in the two to three months up to the strike, and thus likely to miss the early nurse visits, have more contacts to general practitioners (GP) in the first four years of life relative to children, who were older at their exposure to the strike. This pattern holds beyond the initial period of the strike and beyond the first year of life. Moreover, it holds for both regular and out-of-office hours contacts (the latter not being performed by the family GP but by other GPs on duty). These findings indicate that our main results are not purely driven by substitution of nurse visits with GP visits during the strike period or a closer relationship of the family with their GP.[2] Further substantiating that our results reflect children's underlying health, we also document that early strike-exposed children have a higher probability of hospital contacts in the second to fourth year after birth.

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

We have two main findings for maternal health. First, mothers, who are likely to forgo an early nurse visit due to the strike, have more GP contacts in the first four years after their child's birth than mothers with older children at strike start. Second, early strike-exposed mothers are also more likely to have at least one contact with a psychologist or psychiatrist in the first four years of the child's life. While missing an early nurse visit initially mechanically may result in fewer mothers being referred to other specialists, this finding suggests that in the longer run early strike exposure leads to an *increased* likelihood of mothers experiencing mental health problems that require specialist attention. This finding is in line with recent studies documenting the importance of different aspects of the early home environment (in our case the early detection and prevention of severe problems) for maternal postpartum mental health (Butikofer et al., 2018; Baranov et al., 2019; Persson and Rossin-Slater, 2019).

Having established the health effects of missing an early nurse visit, we explore potential mechanisms. Forgoing an early nurse visit implies that families miss out on age-specific information and screening for maternal postnatal mental health issues. First, in the absence of early nurse visits, parents may lack specific information, which is provided by nurses and is difficult to replace by other and less specialized health care providers, such as GPs. Moreover, this information and counseling provided by nurses may impact parents' investment behaviors, such as breastfeeding, parent-child interactions or uptake of other preventive care. Finally, a lack of information and counseling may impact parental confidence in parenting decisions. To examine the relevance of the information 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 and childcare-related field (nurses, midwives, doctors and pedagogues) are less affected by early strike exposure than their respective counterparts. Importantly, these subgroup analyses are not simply capturing socio-economic differences, as we do not find evidence for a strong general socio-economic gradient in the impact of early strike exposure. These findings indicate that at least part of the beneficial effect of early NHV runs through a specific in-

formation 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.

Second, we use data from non-strike cohorts to document the focus on maternal mental health screening during early nurse visits: We show that, during the early visits, nurses record issues related to maternal mental health for one in ten mothers. These initial registrations are correlated with future nurse registrations and maternal psychiatric specialist contacts. This descriptive pattern confirms that nurses (i) put a strong focus on maternal mental health during early visits and (ii) detect and refer mothers with mental health issues (i.e., early registrations are more than noise).[3] In the absence of early nurse visits, for the marginal child and mother, health problems thus are likely to remain unnoticed for a longer period and contribute to longer-term adverse health effects. Our results for the impact of early strike exposure on maternal contacts to psychologists or psychiatrists are in line with this reasoning. Moreover, given documented correlations of maternal postnatal mental health and child-parent interactions and child development (Cooper and Murray, 1998; Lovejoy et al., 2000; Paulson et al., 2006; Wachs et al., 2009), screening for postnatal maternal mental health issues may also be a driver for the impact of early NHV on children.

In a stylized analysis of the direct costs and health benefits of early nurse visits, we show that–only considering prevented GP contacts and their costs–the benefits of very early nurse visits (during the first weeks of life) outweigh costs. Thus although our cost-benefit calculations are very conservative they indicate (i) that early universal visits are a cost-effective intervention to promote children's and mothers' health and (ii) that universal early investment programs should have a strong focus on the initial period of family formation after the birth of a child.

[3]Directly identifying the causal effect of screening for maternal mental health issues in the longer run would require us to compare similar mothers who have or have not been screened positively, e.g., in a regressions discontinuity design. We cannot perform this analysis in our research design as all mothers who miss the early home visits also forgo their timely screening.

Our work contributes to a large literature documenting 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). We make three contributions: First, when studying the causal effects of early-life investment and preventive care programs, the majority of work has considered the effects of program *exposure*. We consider the so far largely unexplored causal effects of within-program variation in an early-life health program. Our study extends earlier work by Kronborg et al. (2016), who use administrative data to study the impact of the 2008 nurse strike but focus on mothers giving birth *during* and shortly prior to the strike and only find short-lived effects of strike exposure on the take-up of GP care for children. Similarly, surveying a small sample of mothers giving birth during the strike and a group of non strike-exposed mothers, Kronborg et al. (2012) find that mothers, who gave birth during the strike report shorter breastfeeding durations. In both studies, however, all treated mothers and children forgo the earliest home visits (the ones that we show are influential). Furthermore, families with births during the strike vary in their access to prenatal midwife consultations and to hospital stays after birth. Thus our analysis identifies a different margin of treatment by focusing on the relative importance of forgoing an early vs. later nurse visit only. Moreover, both earlier studies cannot link data on actual take-up of NHV to their data on outcomes and use samples of families residing in a number of Danish municipalities (with likely different approaches to accommodating services during the strike). In contrast, we directly assess the broad coverage of the strike for different groups in Copenhagen, and we more confidently identify the impact of a well-defined treatment (missing a differently-timed nurse visit) for all families. Finally, we contribute new evidence by analyzing a broader set of relevant outcomes, including maternal postnatal mental health issues.

A second contribution of our paper is its focus on a universal early-life program. A large share of the work on early-life investment policies has been set in a U.S. context and as a

consequence has considered *targeted* programs.[4] Existing work on NHV is also primarily focused on targeted programs (Olds et al., 1986, 1998, 2002; Vaithianathan et al., 2016; Doyle et al., 2015; Sandner et al., 2018; Sandner, 2019; Doyle, 2020).[5] However, many countries offer *universal* programs and the results from studies on targeted programs do not easily generalize to settings with universal implementation. Our study is the first to analyze the causal impacts of a contemporary universal program and thus provides instrumental knowledge to policy debates in many settings.[6]

Third, we identify relevant mechanisms for the causal impact of early NHV on child and maternal health: Age-specific screening offers and information.[7] Our unique data allow us to shed some light on the importance of these elements by studying specific nurse registrations and the heterogeneity of effects of NHV across different types of parents.

[4]Examples include RCT studies on the targeted Perry Preschool Program, the Abecedarian project (among others, Masse and Barnett, 2002; Belfield et al., 2006; Heckman et al., 2013; Conti et al., 2016), and observational studies on the short- and long-run impact of Head Start (among others, Currie and Thomas, 1995; Garces et al., 2002; Ludwig and Miller, 2007; Deming, 2009; Carneiro and Ginja, 2014; De Haan and Leuven, 2020). Also in a US context, there are a few examples for studies considering universal provision of preschool (see, for example, Cascio, 2009, 2015).

[5]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 structure of the programs and the qualifications of service providers (for an overview on existing studies and a discussion of the impact of provider quality, target group and program features, see Almond and Currie, 2011): Focusing on the targeted Nurse Home Visiting Partnership program in the US, Olds et al. (1986, 1998, 2002) show that high-frequency pre- and postnatal visits for at-risk mothers 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) and Doyle (2020) study the targeted Preparing for Life-program in Ireland and find positive effects on some aspects of child health (such as asthma issues) and longer-run benefits on cognitive and socioemotional scores. Sandner et al. (2018) and Sandner (2019) document that the German "Pro Kind" program did not impact child health but had impacts on mothers in a RCT: treated mothers reported lower levels of depression. In the longer run, the program increased fertility and decreased maternal labor supply. Work from developing country contexts highlights the important role for child development and long-run outcomes that intensive home visiting can play, potentially through its impact on parental behaviors (Attanasio et al., 2014; Gertler et al., 2014; Attanasio et al., 2020).

[6]Earlier research has documented positive long-run impacts of the historical introduction of universal NHV in Scandinavia (Wüst, 2012; Hjort et al., 2017; Bhalotra et al., 2017; Bütikofer et al., 2019).

[7]Information may matter in its own right or modify parental beliefs. Recent research documents the importance of parental beliefs—their interpretation of rather than their pure awareness of information—for both child health outcomes and parental investment behaviors (see, for example, Cunha et al., 2013; Attanasio et al., 2015; Boneva and Rauh, 2018; Biroli et al., 2020).

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. We examine their robustness, explores underlying mechanisms, and perform a very simple and stylized cost-benefit calculation. Finally, section 5 concludes.

2 Background and Data

2.1 Institutional Background: Pre and postnatal care in Denmark and the 2008 strike

In Denmark, the public health care system provides free pre- and postnatal care for all residents. Midwives and GPs provide prenatal care that consists of regular consultations during pregnancy.[8] The majority of uncomplicated births are midwife-assisted and take place in public (regional) hospitals. Hospital births account for around 98 percent of all births in Denmark.

After hospital discharge, the 98 municipalities provide postnatal care for infants and mothers in the NHV program. While there is variation in municipal service levels, the Danish National Board of Health (DNBH) issues guidelines and regulations regarding the number, approximate timing and specific content of nurse visits. As such, NHV consists of a basic package of services offered to all families with a newborn. Additionally, municipalities offer supplementary services, such as additional (need-based) home visits, open house events or group interventions (targeted at young parents or parents with specific health issues).

GPs provide preventive health checks and administer vaccines in the Danish vaccination program. The preventive care program offers eight (voluntary) GP health checks for all children: at around five weeks, at around five months, and yearly for children aged one

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

through six years (Sundhedsstyrelsen, 2007). Additionally, GPs offer one postpartum health check for mothers. In the first year of the child's life, the Danish vaccination program for children consists of three rounds, at three, five and twelve months, respectively.[9]

The 2008 strike impacted various dimensions of pre- and postnatal care in Denmark: As unionized employees in the regional and municipal health care sector went on strike, both care provided by midwives, hospital nurses and home visiting nurses was impacted. Hospital physicians and GPs are not covered by the same collective agreements and were therefore not on strike. As shown in Kronborg et al. (2016), mothers giving birth during the strike had a larger probability of missing midwife contacts, of being discharged from hospital on the day of birth, and of missing the early visits from home visiting nurses. In this paper, we thus focus on children born prior to the strike, whose strike exposure–as we show in section 4.2—resulted in a differential treatment by home visiting nurses, but left exposure to prenatal care and care at birth unchanged.

During the strike, all 98 municipalities (in charge of NHV) and the five Danish regions (in charge of hospitals) had to adjust their provision of health care to secure the legally required level of emergency care. In Copenhagen, where our study is set, only managing nurses and a small fraction of regular nurses (employed on specific terms and thus not on strike) were on duty. As a result, nurses primarily provided phone services for families during the strike period and performed—as we show in detail in section 4.2—a limited number of home visits. Regional hospitals operated with a minimum of staff and only emergency care was guaranteed. We comment on potential implications of constrained hospital capacity during the strike for our results in section 4.3.

[9]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. While we focus on the vaccinations given in the first year of life, the Danish vaccination program continues with a number of other vaccinations throughout childhood and adolescence.

2.2 NHV in Copenhagen

Our study focuses on NHV in the largest municipality in Denmark, Copenhagen, with around 500,000 inhabitants and around 8-10,000 yearly live births. Appendix Table A1 presents the main features of NHV in Copenhagen. The default number of universally-offered visits in the program is four: an initial visit shortly after birth, a two-month visit, a four-month visit and an eight-month visit. Infants, who are discharged after short hospital stays can receive an additional early visit after hospital discharge.[10] Moreover, nurses can provide additional targeted visits to children and families with identified needs at the nurses' discretion. The timing of these additional visits is flexible. Finally, the municipality offers optional visits that are available on the request of parents (visits at ages 1.5 and three years).

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. At the same time, those guidelines explicitly state that nurses should focus on the needs of the specific family. Thus nurses have large discretion to focus their time in the family home on what they regard as most important. While some topics, typically related to screening (such as tests for certain infant reflexes, monitoring of maternal postnatal 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. Appendix Table A2 illustrates the main topics that structure the universal nurse visits in the child's first year of life 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").

[10] Especially for higher parity births, discharge on the day of birth is not unusual in Denmark: Among uncomplicated births in our sample, 58 percent of mothers are discharged with their infant on the day of birth.

To describe the typical content of nurse visits, Figure 1 presents data from nurse registrations made by Copenhagen nurses during or shortly after their home visits to families in our control group.[11] We aggregate nurse registrations into broader categories and plot for each of those categories the share of families with a recorded issue for each nurse visit (conditional on having received the visit).

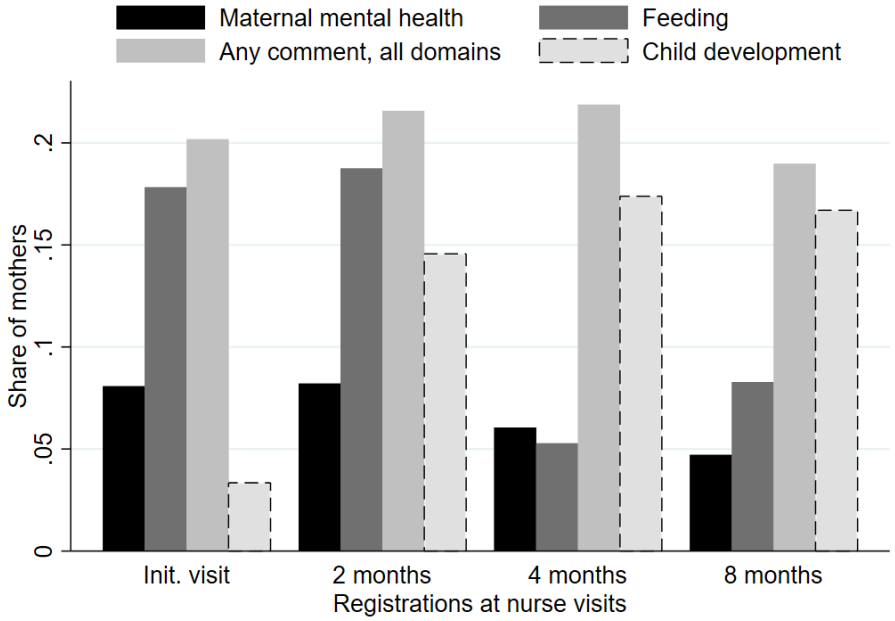


Fig. 1 Share of mothers/children with issues registered by nurses during each universal nurse visit (initial visit through eight-month visit)
Notes: The share of children with registered issues in each domain for all children with a performed visit and born between September 17, 2008 - April 15, 2009 (the control cohort). Each domain aggregates a set of binary measures. Each indicator takes the value one if at least one binary measure is registered as problematic by the visiting nurse.

As the figure illustrates, the visits focus on different domains: While the share of families with a comment on "any domain" remains rather stable over the course of the four visits, during early visits nurses typically record issues related to maternal mental well-being and infant feeding issues. The former is well-defined, mother-specific and highly correlated for women across visits. The latter is child-related but rather unspecific in its content. While

[11] As we will detail in section 2.3, 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 as strike-exposed families naturally lack nurse registrations.

registrations on feeding issues are common during the early visits, nurse observations and registrations on child developmental problems (a summary measure of various dimensions of child development) are more prevalent in the later visits. We return to the importance of different aspects of the program and their timing in section 4.4.

2.3 Data and Variable Construction

In our analysis, we use data from two sources. First, we access archived records on the universe of home visits from the municipality of Copenhagen for the 2007-2009 period.[12] These registrations were either completed at the family home (using a laptop) or at the nurse's office directly after a completed visit. For each visit, nurses register the date and type of visit. Additionally, nurses register their observations regarding factors such as child and maternal health, feeding problems, or relevant risk factors in the family (see Table A2 for examples of focus areas and registration options at different visits).

Second, using children's unique social security number, we merge the nurse records with population administrative data from Statistics Denmark for the birth cohorts 2007-2010.[13] The administrative data contains a large set of parental background characteristics such as educational attainment, income, age, civil status and family links irrespective of co-residence, and municipality of residence. Moreover, the administrative birth records provide information on measures such as children's birth weight and length, gestation age, the five minute APGAR-score, hospital of birth identifiers and the number of prenatal midwife contacts.

Using data for the years 2007-2014, we create three sets of health outcome measures from the administrative data: First, to study child and maternal health, we examine the number

[12]These data come from an archive version of the municipality's administrative system. The full archive of nurse records from Copenhagen includes data on all visits and examinations of children resident in the municipality from January 1, 2007 to December 31, 2010—a total of 35,213 children. These records were transferred to the Copenhagen city archive due to a change of the software used by the Copenhagen nurses. As we are interested in studying the impact of timing of nurse visits in the first year of the child's life, 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]In our analysis of strike exposure on child outcomes, we use an additional cohort of children (2010) in our control group. Our results are not sensitive to the choice of control years, as detailed in section 4.5.

of GP contacts from child age zero to four. GP contacts include both physical meetings, phone contacts during regular consultation hours and e-mail correspondence. Given that we only measure health care usage in our data, we are concerned about as to whether we pick up actual impacts of strike exposure on child health: Parents may act more cautiously and—in the short run—substitute nurse care with GP care. In the longer run, parents may continue to demand more care, for example, because they build a strong relationship with their family GP due to increased initial contacts.

While we cannot fully disentangle true health effects from alternative explanations for changes in health care take-up, we provide additional insights by dividing our measure of GP contacts into two categories: i) regular (scheduled) contacts at the family GP, and ii) out-of-office hours GP contacts (OOH contacts on weekends or outside typical opening hours and thus not performed by the family GP).[14] Out-of-office hours contacts with GPs may be a more direct measure of poor health that requires attention. Moreover, our follow-up period of up to four years (and our analyses of GP contacts after the initial year of the child's life) allows us to speak to the role of substitution between nurse visits and GP contacts: While effects during the period of the strike and first-year effects on GP contacts may be caused by substitution, the scope for substitution in the longer-run is likely small.

Unfortunately, the GP data does not include a direct measure of diagnoses or the reason for a given consultation. As one exception, we directly observe GP preventive care contacts and vaccinations. We do not include preventive care in our main outcome measure, but analyze it separately. Thus our measures of GP contacts do not measure the participation in the voluntary preventive care program but focus on contacts due to health problems or parental concerns about the child's health.

As alternative measures of child health, we also consider two types of hospital contacts: Hospital admissions and outpatient contacts. Around 25 and 39 percent of children are

[14]Due to a restructuring of out-of-office hours GP care there is a data break in 2015 in the administrative data. Therefore, we focus on GP contacts in the first four years of life where both treated and control children are exposed to the same regime of out-of-office hours GP care. Analyses that also include 2015 and later years (and only consider family GP care) lead to very similar results that are available on request.

admitted to the hospital or have an outpatient contact during their first year of life, respectively. While hospital contacts may capture more extreme health problems, these figures illustrate that, in general, hospital contacts are not rare and often related to routine check-ups. One aspect worth noting is that the 2008 strike covered all unionized nurses and thus hospital care for non-emergency patients was restricted. Therefore, GPs may have been more reluctant in referring children to hospitals in the strike period.

Second, we consider the impact of strike exposure on maternal postpartum mental health problems. These potential effects are interesting in their own right and also as mechanisms or reinforcing factors for longer-run effects of strike exposure on children. We create indicators that are equal to one if a mother has at least one contact with a psychologist or psychiatrist in the primary health care sector during the strike, in the first, and the second to fourth year after childbirth. We also consider more extreme margins of maternal mental health issues by creating indicators for any maternal psychiatric hospitalizations (including both in- and outpatient contacts), and indicators for receiving any psychiatric diagnoses during the same time periods (F01-F99 from the International Statistical Classification of Diseases and Related Health Problems (ICD)). While a complementary margin of analysis would be maternal consumption of prescription drugs related to mental health issues, we do not have access to these highly sensitive data.[15]

Third, we attempt to study parental health investment decisions, which may be impacted by NHV and thus contribute to the impact of NHV on children's health. However, as we exploit information on a sample of children exposed to the nurse strike (which resulted in the absence of nurse visits), we are constrained in our ability to use nurse registrations for parental inputs as outcome measures in our main analyses.[16] Relying on administrative data

[15] Access to medical prescription data in Denmark is tied to a set of strict requirements, such as restrictions on the type of other linked information from administrative data (such as dates of birth) and requirements for cell size (to ensure anonymity). We believe that our analyses based on GP and specialist care uptake (which likely results in drug consumption among the most severely impacted mothers) are good proxies for mental health issues after birth.

[16] In supplementary analyses, we have constrained our sample to early strike-exposed children and study their outcomes at the nurse visit around eight months. We have considered indicators for nurse-observed issues concerning maternal mental health, infant feeding, child-parent contact as well as summary measures

only, we therefore consider indicators for participation in the GP preventive care program and the vaccination program.

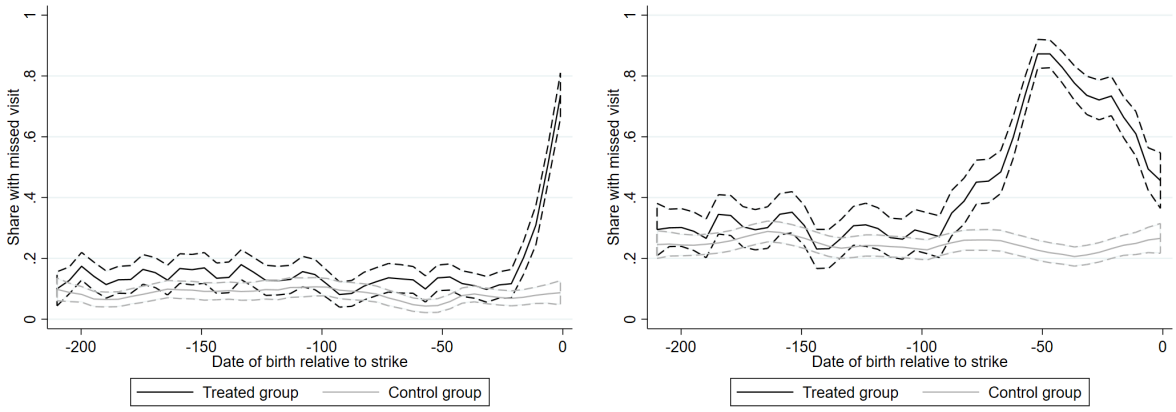
3 Empirical Methods

In our analyses, we exploit that children born our study period prior April, 15 2008 are exposed to the strike at different ages. Thus they all experience a higher probability of forgoing a nurse visit, but do so at different ages. Figure 2 shows this variation: The figure plots the raw relationship between date of birth and missing a nurse visit for children born in the 210 days before the strike for the years 2008 and 2009. Black lines and confidence intervals are for the treated cohort, grey lines and confidence intervals are for the control cohort.

In absence of the strike, the share of children missing a specific nurse visit is stable (depending on the visit at around 10-20 percent) as indicated by the grey lines in Figure 2. 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. Panels (b), (c) and (d) illustrate that—depending on the relative age at strike start—children have an increased probability of missing the two-, four- and eight-month visits.[17] Important for the interpretation of our results, and as we detail in section 4.2, the strike had a broad coverage and impacted families defined along different observable dimensions similarly.

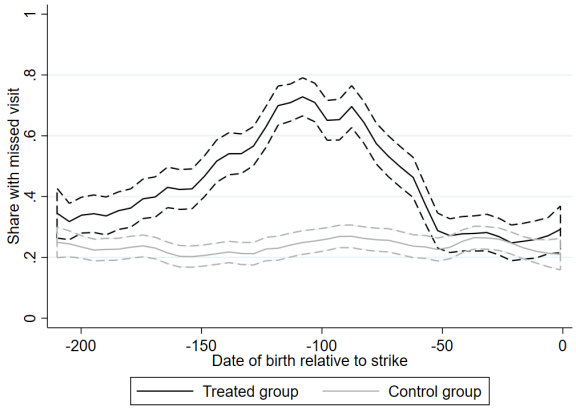
for any nurse comment or referral. However, these analyses rely on a very small sample relative to the expected effect sizes and is thus not very informative. Unfortunately, the nurse data on the duration of breastfeeding in the archived data are of very poor quality and we cannot use them at all.

[17] Appendix Figure A5 further presents graphically the impact of strike exposure on the *number* of nurse visits for children in the treated and control cohorts. Strike exposure impacted the number of universal and total nurse visits that children received: Control children received an average of 3.3 universal visits while treated children received 2.7 visits. Additionally, the youngest strike-exposed children lost more nurse visits. This finding reflects that—during non-strike periods—early hospital-discharged children received two visits within the first two weeks of life (a universal visit and an extra visit). In section 4.5, we examine the robustness of our general conclusions to the omission of this group of children (a doughnut hole-approach).

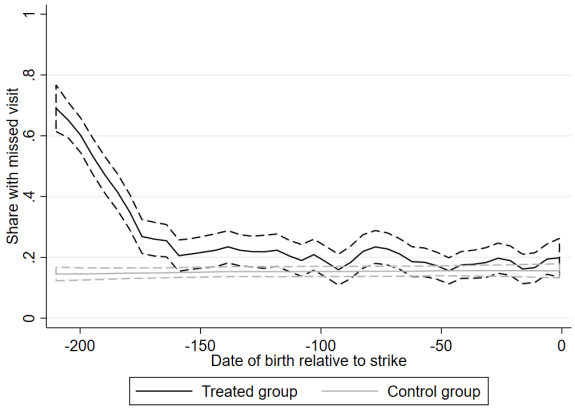


(a) Did not receive initial visit

(b) Did not receive two-month visit



(c) Did not receive four-month visit



(d) Did not receive eight-month visit

Fig. 2 Share of children with missed nurse visits for children in the treated and control cohorts
Notes: The figure shows the raw relationship between date of birth and missing a nurse visit estimated with kernel weighted local polynomials using an epanechnikov kernel, a rule-of-thump bandwidth and 42 (5-day) smoothing points. The black line and dashed black confidence intervals are for the treated cohort, the grey line and dashed grey confidence intervals are for the control cohort. Treated cohort: September 18, 2007 - April 15, 2008. Control cohort: September 17, 2008 - April 15, 2009).

Given that the "time to strike" variation in out setting impacts not one but four endogenous variables (four different nurse visit), we do not pursue an instrumental variable strategy. This strategy would require us to zoom in on children in narrow time frames, such as children born in the two weeks prior to the strike vs children born three-four weeks prior to the strike. However, with too few weekly Copenhagen births, lack power for analyses of that type. Instead we estimate the reduced form relationship between child and maternal

health and timing of birth relative to the strike start in a difference-in-differences analysis. To that end, we divide our exogenous variable (time to strike) into seven 30-day bins and estimate the following relationship using data for the strike year and different sets of control years:

$$\begin{aligned} y_{it} =& \alpha_0 + \sum_{j=-7}^{-1} \phi_j 1(bin30_{it} = j) \times 1(Year_t = 2008) \\ &+ \sum_{j=-7}^{-1} \beta_j 1(bin30_{it} = j) + \gamma' \boldsymbol{X}_{it} + \boldsymbol{\lambda}_t + \epsilon_{it} \end{aligned} \tag{1}$$

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 main 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 (12,078 children). The seven 30-days bin indicators are equal to one if child i's date of birth is within a particular bin. We include cohort fixed effects, $\boldsymbol{\lambda_t}$, which define cohorts across calendar years: The indicator for the 2008 cohort 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. Children born prior to the strike in 2008 are treated while children born on the same dates in 2009 and 2010 are untreated. In our sample of Copenhagen-born children, 99 percent of children were delivered at a hospital. Given that 78 percent of these children were born in the two main hospitals covering the capital region, we do not include hospital fixed effects in our main specification.

In our main specification, we include the following covariates (X_{it}): paternal and maternal total income, indicators for their 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 parental covariates. All the X_{it} are measured one year prior to birth of the focal child. Additionally, we control for measures of pregnancy health and birth characteristics drawn from the birth records: the number of prenatal 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 2,500g) or a preterm birth (below 37 weeks), child gender and maternal smoking status at birth.[18]

The interactions of the period bins with an indicator for the 2008 cohort identify our estimates of interest: They provide estimates for the effect of strike exposure at a certain age relative to the reference group (children born in the 210-181 days bin in the strike cohort). Thus we ask: Faced with a one-visit reduction in the number nurse visits, is it worse to miss an earlier vs. a later visit? Importantly, we face a trade-off when deciding on the bin size in our analyses: As Figure 2 illustrates, narrow bins group children according to their probability of missing one of the four universal nurse visit. However, small bins also raise power issues. Thus we show in our robustness tests that our main conclusions are not sensitive to reasonable variations of bin size.

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 for children born in specific periods up to April 15 in the strike and control cohorts 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 the outcomes of all children in one cohort. 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 predetermined variables, which should be unrelated to treatment exposure. In other words,

[18] We omit children with any missing data from the birth records in the main analyses (around six percent). In our robustness tests, presented in section 4.5, we show that our results are not sensitive to this exclusion.

we estimate model (1) using parental and birth characteristics as dependent variables. Our treated and control groups are balanced across observable pre-treatment characteristics (Appendix Tables A3 and A4) and we find very few significant estimates across the groups. Moreover, this results is confirmed in joint tests of the significance of the age bin-strike interactions in each of the regressions using predetermined characteristics as an outcome.

Another informal test of the common trend assumption is the assessment of pre-trends in outcomes across groups. As we do not observe children's GP visits prior to treatment, we consider maternal pre-birth outcomes: Appendix Figure A1 plots pre-birth averages of maternal GP contacts and the share of mothers receiving a psychiatric diagnosis. The figures show similar trends and levels for both measures of maternal health prior to birth both within and across treated and control cohorts.

Our second assumption, which allows us to interpret our results as reflecting the impact of NHV, is that there are no other co-varying policies or shocks that overlap with the timing of the strike. To provide support for this assumption, we assess whether strike exposure is related to differential health care provision through other channels than NHV. When we plot the average number of prenatal midwife visits and GP consultations, the average number of days admitted to hospital after birth, and the share of mothers having a C-section for mothers in the strike-exposed cohort and control cohorts, the graphs do not indicate systematic differences or trends in any of these types of care around birth across the groups that we consider (Appendix Figure A2). As an example, the average hospital stay after birth is 3.3 days for mothers giving birth in the week leading up the strike compared to 3.5 days for mothers giving birth on the same days in the control cohorts.

Besides the impact of co-varying health policies, the impact of shocks—such as the great recession—may impact our findings. We believe that it is a reasonable assumption that economic conditions impacted all new families equally in the narrow time frame of 210 days prior to the strike (and the same 210 days in the control cohorts). We assess this statement by examining maternal employment in the year of her child's birth for the treated and control

children in the bottom panel of Appendix Figure A2. The graph shows identical levels and trends for the share of time employed during the year of birth for mothers in the treated and control cohorts. The trends within both cohorts are due to our measurement of the share of employment in the calendar year of birth.[19] The parallel development of employment in both treated and control cohorts gives credibility to our assumption that general economic developments and events like the great recession affected our treated and control cohorts similarly.

A final concern that we address is the selection of individuals out of our sample. First, families could not manipulate their treatment status since all children in our analysis sample were born either prior to the strike or a minimum of four months after the strike ended. In Appendix Figure A3, we show that the density of births around the strike does not indicate bunching around the beginning or end of the strike period. Second, families could select out of our analysis sample by moving to a different municipality or out of the country. In our main analysis, to focus on children who were treated with default care in Copenhagen or covered by the strike while residing in Copenhagen, we omit data for 1,962 children, who move out of the municipality during their first year of life. We show that selection out of our sample is not an important concern as the share of children that we observe as Copenhagen residents during their first year of life is not impacted for treated and control cohorts (Appendix Figure A4). This finding makes sense as the strike was large-scale, affected all municipalities and was of a short duration. Thus the risk of strike-induced domestic migration should be small. Finally, including domestic movers into our main analyses (so that only death and migration abroad causes exclusion) does not alter our results.

[19]Given that our cohorts span two calendar years, mothers who give birth at the beginning of the calendar year spend a larger share of the given year on maternity leave than mothers, who give birth at the end of the calendar year.

4 Results

4.1 Descriptive Statistics

Table 1 presents summary statistics for our main sample of children born in Copenhagen across the groups of treated children (born September 18, 2007 - April 14, 2008) and children in the control group (born September 17, 2008 and 2009 - April 14, 2009 and 2010). In the top panel, we present summary statistics for selected outcomes and covariates from the administrative data. In the bottom panel, we present variables on nurse visits from the nurse records. 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 children born in 2010.

Control children have on average 1.5, 9.6 and 20.7 GP contacts during the strike, the first year, and second to fourth year of life, respectively. Out-of-office hours contacts constitute around one third 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 covariates.

Focusing on the bottom panel of Table 1, 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. This figure implies that the average child receives three out of the four universal visits. On average, children additionally receive one home visit scheduled due to a specific need. This average masks heterogeneity across children.

Table 1 Variable means, strike exposed and control cohorts

	Strike cohort		Non-strike cohorts	
	Mean	Obs.	Mean	Obs.
A. Variables based on adminstrative data				
Total GP during strike	1.58	4081	1.54	8725
Total GP 1st year	9.42	4081	9.63	8725
Total GP 2-4 years	21.88	3950	20.65	8445
OOH GP during strike	0.37	4081	0.41	8725
OOH GP 1st year	2.90	4081	2.99	8725
OOH GP 2-4 years	7.28	3950	6.67	8445
Vacc., 1st round	0.85	4081	0.90	8725
Vacc., 2nd round	0.87	4081	0.91	8725
Vacc., 3rd round	0.88	4081	0.91	8725
Prev. care, 5 weeks	0.88	4081	0.92	8725
Prev. care, 5 months	0.92	4081	0.93	8725
Prev. care, 12 months	0.93	4081	0.93	8725
OOH GP during strike mothers	0.12	4081	0.12	8725
OOH GP 1st year mothers	1.02	4081	1.01	8725
OOH GP 2-4 years	2.47	3950	2.28	8445
Psychiatrist/psychologist strike mothers	0.01	4081	0.01	8725
Psychiatrist/psychologist 1st year mothers	0.03	4081	0.03	8725
Psychiatrist/psychologist 2-4 years mothers	0.09	3950	0.09	8445
Midwife visits	4.80	3970	4.75	8507
Child sex	0.48	4081	0.48	8725
Low birth weight	0.04	4009	0.06	8598
Preterm birth	0.06	4014	0.06	8587
C-section	0.21	4081	0.21	8725
Home birth	0.01	4081	0.01	8725
Cohabiting	0.76	4081	0.78	8725
Prim. school, mother	0.15	4081	0.12	8725
Uni. degree, mother	0.29	4081	0.32	8725
Employed, mother	0.77	4081	0.77	8725
Danish, mother	0.76	4081	0.74	8725
Income, mother	281.78	4081	289.58	8725
B. Variables based on nurse records				
No. of nurse visits	3.77	4081	4.40	4269
Number of universal 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 strike period (September 18, 2007 - April 15, 2008) and in non-strike periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). For the data from the nurse records (bottom panel), the control group only includes the period September 17, 2008 - April 15, 2009. OOH GP: out of office hours GP contacts

Table 1 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 analyze these patterns greater details.[20]

4.2 Strike Exposure and Nurse Visits

In Table 2 we present estimates from separate regressions based on Equation (1) for the impact of (binned) age at strike exposure on the probability of missing a nurse visit. Thus the coefficients reflect the effect of being born in a specific bin on the probability of not receiving each nurse visit (relative to the reference bin). The columns show results for the different types of universal nurse visits.

The regression results mirror the graphical presentation from Figure 2: The strike only has a significant impact on the initial visit for children who were aged 30-0 days at strike start. On average, children in this age bin have a 16.9 percentage points higher probability of missing the initial visit (relative to the reference group). Children who were aged 90 days and younger at strike start have an increased probability of missing their two-month visit with children in the 60-31 days bin being most severely affected (51.1 percentage points). Children who were aged 61-150 days at strike start have their four-month visit most severely affected by the strike. Only the oldest children in the strike cohort have an increased probability of a missed eight-month visit compared to younger children (around 40 percentage points

[20]To assess the representativeness of our sample of families from the capital of Denmark, Appendix Table A5 compares children and parents from Copenhagen to the general Danish population. There are a number of differences: Parents in Copenhagen 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 the country: Five percent of children are low birth weight and seven percent are born prematurely. There are no large differences in the number of nights spent at the hospital after birth, the number of prenatal 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 at the time of birth and less likely to smoke.

Table 2 Effects of strike exposure on the probability of a missed nurse visit scheduled for a specific month (m) of the child's life and the number of visits

	(1) No initial visit	(2) No 2-m visit	(3) No 4-m visit	(4) No 8-m visit	(5) Number of universal visits	(6) Number of visits
Days						
180-151	0.001	-0.041	0.099***	-0.318***	0.260***	0.221
	(0.026)	(0.037)	(0.037)	(0.034)	(0.091)	(0.166)
150-121	0.004	-0.017	0.248***	-0.354***	0.120	0.195
	(0.026)	(0.037)	(0.037)	(0.034)	(0.090)	(0.162)
120-91	-0.027	-0.015	0.362***	-0.363***	0.044	0.183
	(0.026)	(0.037)	(0.037)	(0.034)	(0.088)	(0.164)
90-61	-0.007	0.158***	0.227***	-0.349***	-0.029	0.239
	(0.025)	(0.038)	(0.038)	(0.034)	(0.087)	(0.164)
60-31	-0.005	0.512***	-0.039	-0.421***	-0.047	0.107
	(0.024)	(0.035)	(0.036)	(0.033)	(0.083)	(0.154)
30-1	0.169***	0.326***	-0.076**	-0.393***	-0.026	-0.262*
	(0.028)	(0.037)	(0.036)	(0.033)	(0.086)	(0.159)
Obs.	7824	7824	7824	7824	7824	7824

Notes: Each column shows estimates from separate regressions. The coefficients are for the interactions of 30-day bins and a strike cohort indicator. The omitted category is the age bin 210-181. All regressions include cohort 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 parental covariates. All covariates are measured one year prior to birth of the focal child. Child/birth 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 prenatal midwife visits. The sample includes children born in Copenhagen in the treated cohort (September 18, 2007 - April 15, 2008) and in control cohort (September 17, 2008 - April 15, 2009). The outcomes in columns (1)-(4) are indicators for 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.

difference when compared to the children, who were youngest at strike start). As shown in column (5) strike exposure does not differentially impact the number of completed universal visits among treated children. As an exception, children in the 30-1 day bin lose on average 0.262 nurse visits compared to the reference group (significant at the 10 percent level). This result reflects that children below age two weeks at strike start potentially forgo two visits, the universal initial visit and an additional early visit if discharged shortly after birth. We examine the impact of this pattern in a robustness test in section 4.5.

Having established that age at strike start has an important impact on timing of the missed nurse visit for strike-exposed children, one concern is that nurses strategically chose the children they visited, i.e., that only the most well-off children were impacted by the strike. This aspect 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. However, our unique data also allows us to characterize within the full population those children, who missed nurse visits due to the strike. To do so, we estimate separate regressions for the impact of strike exposure on the probability of missing nurse visits across subgroups of the population. Table 3 shows results for the probability of missing the first nurse visit (analyses for the other three universal visits lead to very similar conclusions and are available on request). Following Angrist and Pischke (2008), we present the ratio between the coefficients from each subgroup and the full population alongside with our estimates for the probability of missing the first nurse visit. Similarly to a complier analysis in an instrumental variable setting, we can thus assess whether certain subgroups are more or less likely to miss a nurse visit. Importantly, we study subgroups of families defined by characteristics that may at least be partly observed by the nurses: child gender, parental education in a health-related field, initial child health, and child parity.[21]

[21]Having parents with an educational background in a (child) health-related field implies that at least one of the parents is educated as a doctor, midwife, nurse or pedagogue. We define a child with low initial health as the child having a birth weight below 2500g and/or being born preterm. We focus on the coefficients for the 30-day bin in the table as only initial visits for children born in this bin were impacted by the strike.

Table 3 Effects of strike exposure on the probability of missing the initial visit by subgroup in the population

	Gender		Health educ.		Poor health		Parity	
	Boys	Girls	No	Yes	No	Yes	>1	=1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coef.	0.203***	0.137***	0.165***	0.206***	0.167***	0.235**	0.133***	0.189***
	(0.040)	(0.038)	(0.030)	(0.067)	(0.028)	(0.114)	(0.045)	(0.035)
P-val.	0.232		0.547		0.561		0.326	
Ratio	1.20	0.81	0.97	1.22	0.99	1.39	0.79	1.12
MDV	0.09	0.07	0.08	0.08	0.08	0.11	0.10	0.07
Obs.	4357	3993	7256	1094	7530	645	3142	5059

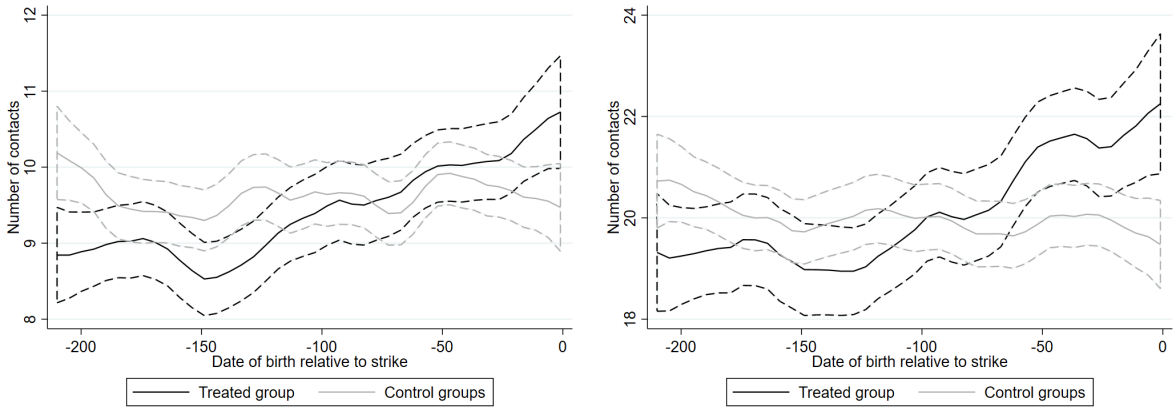
Notes: See notes for Table 2. In this table, we present estimates for the interactions of 30-day bins and a strike indicator from separate regressions for various subgroups along with the ratio between the full-sample estimates and the subgroup-estimates. Both sets of regressions exclude all control variables. 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. The table also shows the ratio of the relevant estimate for the subgroup relative to the estimate in the full population. MDV is the mean of the dependent variable for the control group. The p-values are for a test of equality for the interaction term of the specific subgroup, the 30 day bin indicator and the strike cohort in an interacted model. Robust standard errors are in parentheses.

In general, our analysis suggests that the strike affected the considered subgroups relatively similarly. Moreover, a stronger impact of strike exposure on the probability of missing a nurse visit does not covary with characteristics that may indicate positive potential outcomes. The table also presents p-values for a formal test of the differences of estimates across subgroups (obtained from an interacted model). For all subgroups, we cannot reject equality, i.e., we conclude that the strike affected the probability of forgoing the initial visit similarly across subgroups. Thus 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 our analyses of heterogeneous effects.

Taken together, our graphical and regression results provide powerful evidence for the differential timing of the assigned treatment (forgoing one of the universal nurse visit). Thus we move on to analyzing the health consequences of differently-timed strike exposure.

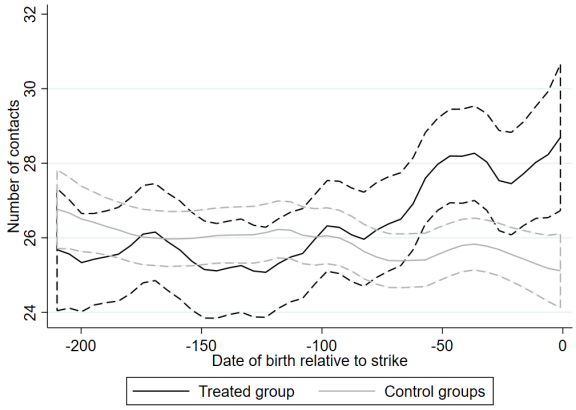
4.3 Strike Exposure and Child and Maternal Health

To estimate the impact of strike exposure on children's and mothers' health, we use outcomes from the administrative data. Figure 3 presents graphical evidence of the raw relationship between age at strike start and accumulated GP contacts at ages one through four.[22]

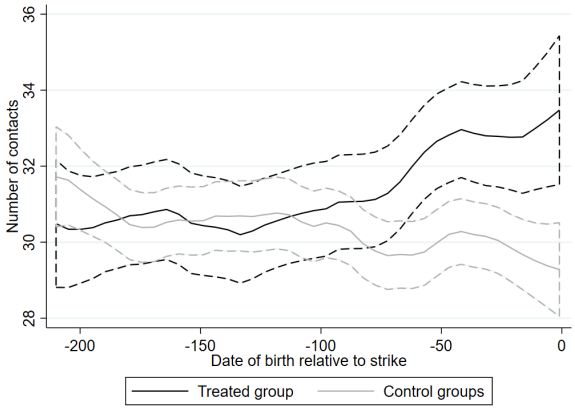


(a) Total GP contacts - one year

(b) Total GP contacts - two years



(c) Total GP contacts - three years



(d) Total GP contacts - four years

Fig. 3 Accumulated number of GP contacts for children born in the treated (September 18, 2007 - April 15, 2008) and control cohorts (September 17, 2008 and 2009 - April 15, 2009 and 2010)
Notes: The figure shows the relationship between date of birth and accumulated total GP contacts. See Figure 2 for further details.

The number of accumulated GP contacts reveal a clear pattern: Strike exposure in 2008 increases the number of GP contacts for the youngest children in our sample. Figure 3

[22]Figures for regular and out-of-office hours contacts are available on request.

illustrates a gradient inside the early strike-exposed group of children: the youngest children 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 average number of GP contacts is similar to the average for control children. Interestingly, the impact of missing an early nurse visit is persistent as the differences increase as the children age.

Table 4 shows our main results for the impact of strike exposure on child health by the type of GP contact. To better distinguish substitution between nurse visits and GP contacts from actual health effects, we present estimates for outcomes measured in three periods of the child's life: during the strike, the first year of life and the second to fourth year of life. Contacts during the strike and in the first year of life may be particularly susceptible to substitution, while contacts from the second year forward are more likely to reflect actual health issues.[23]

During the strike period, strike exposure for children in the 30-1 days age bin increases the number of GP contacts with 0.36 contacts. This difference is driven by both an increase in regular and out-of-office hours GP contacts. However, the increase in percentage terms (evaluated at the average number of GP visits for the control group) is larger for contacts outside typical opening hours (37 percent vs. 18 percent). Thus, we find indications that parents substitute the canceled nurse visit for contacts with their family GP but also increase their contacts to GPs outside typical office hours. This pattern may reflect that parents are more insecure in the absence of an early nurse visit.

In the first year of life, children born in the 30-1 days age bin have 1.93 additional GP contacts in total. We find increases of 12 and 38 percent for regular and out-of-office hours GP contacts, respectively. From the second to fourth year of life children in the 60-31 and 30-1 days age bin have 2.2 and 3.0 additional total GP contacts. Evaluated at the relevant (control group) mean number of contacts, the effect on out-of-office hours contacts

[23]Any effects after the first year of life may still be driven by a better relationship of families with their GP. To indirectly assess this possibility, we have used an indicator for a change of GP after the first year of life as an outcome. We do not find that early strike-exposed parents are more (or less) likely to change their family GP.

(20 percent) is larger than the effect on regular contacts (12 percent). For children in most other age bins the timing of strike exposure has no systematic effects on GP contacts relative to the 210-181 days age bin in the short or longer run.

To assess the impact of strike exposure at other margins, we have also considered alternative measures of child health: child hospitalizations and outpatient contacts (Appendix Table A6). While most point estimates for first year hospitalizations are imprecise, we find evidence that early strike-exposed children are 6-8 percentage points (40 percent) more likely to be hospitalized during the second to fourth year of life. These results support the results for GP care and indicate actual health effects that do not exclusively reflect substitution and precautionary parental behavior. Furthermore, we see some indication for a decrease in outpatient contacts during strike and the first year of life for early strike exposed children relative to the oldest strike exposed children. While nurses in non-strike years can refer families as outpatients to hospitals in case of health or feeding issues, during the strike this option was likely limited (due to nurses in hospitals also being on strike). However, hospitals were obliged to ensure an adequate level of emergency care provision. Given that we do not see longer-run impacts of strike exposure on outpatient contacts, we conclude that our finding for a short-run decrease in outpatient contacts for the early strike-exposed children (relative to the oldest children) supports the idea of some substitution of care during the strike (from hospital care to GP care).[24]

Our main results show that early strike exposure impacts children's number of GP contacts—in the short and longer run. Importantly, nurses also focus their attention on

[24]We have also attempted to analyze child outcomes from nurse registrations at age eight months and longer-run outcomes: Constraining our sample to children who received the eight-month visit, we do not find precise estimates for the impacts of strike exposure on child development at eight months. However, these analyses are based on around 40 percent of our main analysis. Considering longer-run outcomes, we have explored the impact of the timing of strike exposure on the probability of a delayed school start of children. We do not detect any effects. We cannot yet examine longer-run impacts of the 2008 strike on academic test scores (observed for the first time during grade two). Assessing the school entry examination of around 75 percent of the children in our sample, we do not see any impact of timing of strike exposure on child BMI or their probability of being overweight. In our sample we likely lack power to analyze these outcomes (given low level of obesity prevalence at around 7 percent). Furthermore, we miss 25 percent of children in our school entry records that only cover Copenhagen and thus do not include children, who move.

Table 4 Effects of strike exposure on child health: GP contacts by type

	(1) Total GP during strike	(2) Total GP 1st year	(3) Total GP 2-4 years	(4) Regul. GP during strike	(5) Regul. GP 1st year	(6) Regul. GP 2-4 years	(7) OOH GP during strike	(8) OOH GP 1st year	(9) OOH GP 2-4 years
Days									
180-151	-0.101	0.454	0.786	-0.114	-0.178	0.234	0.013	0.632**	0.551
	(0.126)	(0.476)	(0.997)	(0.089)	(0.313)	(0.626)	(0.070)	(0.251)	(0.535)
150-121	-0.113	0.308	0.886	-0.118	-0.028	0.458	0.005	0.335	0.428
	(0.132)	(0.475)	(0.988)	(0.089)	(0.309)	(0.620)	(0.075)	(0.257)	(0.527)
120-91	-0.052	0.628	-0.158	-0.122	0.047	0.162	0.070	0.582**	-0.320
	(0.126)	(0.477)	(0.949)	(0.090)	(0.319)	(0.609)	(0.071)	(0.249)	(0.504)
90-61	0.133	1.122**	1.512	0.107	0.218	0.878	0.026	0.904***	0.635
	(0.127)	(0.496)	(0.974)	(0.092)	(0.334)	(0.631)	(0.069)	(0.257)	(0.511)
60-31	0.157	1.085**	2.225**	0.049	0.266	1.183*	0.108	0.819***	1.041**
	(0.126)	(0.474)	(0.976)	(0.092)	(0.310)	(0.618)	(0.067)	(0.253)	(0.524)
30-1	0.358***	1.930***	3.009***	0.203**	0.787**	1.699***	0.155**	1.142***	1.310**
	(0.136)	(0.492)	(1.001)	(0.101)	(0.328)	(0.651)	(0.070)	(0.252)	(0.515)
MDV	1.54	9.63	20.65	1.13	6.64	13.98	0.41	2.99	6.67
Obs.	11992	11992	11615	11992	11992	11615	11992	11992	11615

Notes: Each column shows estimates from separate regressions. The coefficients are for the interactions of 30-day bins and a strike cohort indicator. The omitted category is the age bin 210-181. All regressions include cohort 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 parental covariates. All covariates are measured one year prior to birth of the focal child. Child/birth covariates include indicators for parental age below 21 at birth, indicators for a C-section, home birth, low birth weight (below 2,500g), a preterm birth (below 37 weeks), child gender, maternal smoking status at birth and the number of prenatal midwife visits. The sample includes children born in Copenhagen in the treated cohort (September 18, 2007 - April 15, 2008) and in control cohorts (September 17, 2008,2009 - April 15, 2009,2010). The outcomes in columns (1)-(3) are the number of total GP contacts in periods defined by the column header. Columns (4)-(6) (regular GP) and (7)-(9) (out-of-office hours GP (OOH GP)) divide the total number of GP contacts by the type of contact. MDV is the mean of the dependent variable for the control group. Robust standard errors in parentheses.

maternal physical and mental well-being. Table 5 presents results for maternal (total, regular and out-of-office hours) GP contacts. Children and their parents typically attend the same (family) GP clinic for regular consultations.

We do not find significant differential effects of age at strike exposure for maternal GP contacts of any type in the short run (first year). However, in the longer run (in the second through fourth year of the child's life), mothers who are strike-exposed shortly after childbirth (90-1 days), have 2.2-3.2 additional GP contacts (9.5-13.6 percent increase at the mean) during the second to fourth year of life. Similar to our findings for children, the relative increase of GP contacts for earlier vs. later strike-exposed mothers are both driven by regular and out-of-office hours GP contacts.

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

	(1) Total GP during strike	(2) Total GP 1st year	(3) Total GP 2-4 years	(4) Regul. GP during strike	(5) Regul. GP 1st year	(6) Regul. GP 2-4 years	(7) OOH GP during strike	(8) OOH GP 1st year	(9) OOH GP 2-4 years
Days									
180-151	0.037	0.274	0.117	0.028	0.267	-0.059	0.009	0.007	0.176
	(0.111)	(0.402)	(1.048)	(0.099)	(0.353)	(0.925)	(0.034)	(0.111)	(0.240)
150-121	-0.094	0.291	1.820*	-0.096	0.200	1.616*	0.003	0.092	0.204
	(0.109)	(0.398)	(1.022)	(0.098)	(0.344)	(0.907)	(0.033)	(0.118)	(0.237)
120-91	-0.055	0.465	0.836	-0.056	0.458	0.631	0.001	0.007	0.205
	(0.112)	(0.407)	(1.059)	(0.103)	(0.358)	(0.945)	(0.032)	(0.110)	(0.239)
90-61	0.035	0.219	2.669**	0.036	0.078	2.041**	-0.001	0.140	0.628**
	(0.117)	(0.405)	(1.056)	(0.106)	(0.361)	(0.943)	(0.034)	(0.107)	(0.248)
60-31	-0.044	0.322	2.164**	-0.077	0.067	1.435	0.033	0.255*	0.729***
	(0.122)	(0.412)	(1.066)	(0.106)	(0.347)	(0.939)	(0.040)	(0.132)	(0.256)
30-1	-0.060	0.647	3.197***	-0.096	0.506	2.488***	0.036	0.141	0.709***
	(0.123)	(0.415)	(1.054)	(0.111)	(0.367)	(0.946)	(0.038)	(0.108)	(0.238)
MDV	1.57	8.38	22.83	1.44	7.57	20.54	0.12	0.81	2.28
Obs.	11992	11992	11615	11992	11992	11615	11992	11992	11615

Notes: See notes for Table 4. Robust standard errors in parentheses.

Finally, Table 6 considers maternal psychologist and psychiatrist contacts, psychiatric hospitalizations and psychiatric diagnoses. Hospitalizations and diagnoses are very rare events limiting our ability to detect impacts given our design and sample size. However, for contacts with psychologists and psychiatrists, which are more common occurrences, our results suggest that strike-exposed mothers of children born in the 30-1 days age bin are more likely to have a contact during the strike, in the first year and the second to fourth year of the child's life (relative to strike-exposed mothers of children born in the 210-181 days age bin). While point estimates are large, our estimates are less precise and thus we are careful in interpreting the size of these estimates.

In sum, our results show that early (relative to later) strike exposure resulting in reduced access to NHV impacts maternal physical and mental health negatively. Moreover, effects on maternal mental health may constitute a mechanism for or reinforce the health effects on children that we have documented.[25]

[25]We have also examined the impact of timing of strike exposure on maternal labor market outcomes. Using measures for yearly taxable income and an indicator for any employment in the year after child birth, we find no effects of the timing of strike exposure. Results are available on request.

Table 6 Effects of strike exposure on maternal mental health: Postnatal contacts with specialists, mental health-related hospital contacts and psychiatric diagnoses

	(1) Psychiatrist psychologist during strike	(2) Psychiatrist psychologist 1st year	(3) Psychiatrist psychologist 2-4 years	(4) Psych. hospitalization during strike	(5) Psych. hospitalization 1st year	(6) Psych. hospitalization 2-4 years	(7) Psych. diag during strike	(8) Mother Psych. diag. First year	(9) Psych. diag 2-4 years
Days									
180-151	0.002	0.013	0.009	0.000	0.002	-0.005	0.002	-0.006	-0.011
	(0.008)	(0.013)	(0.021)	(0.000)	(0.002)	(0.004)	(0.002)	(0.006)	(0.008)
150-121	0.011	0.021*	0.013	-0.000	0.002*	0.003	0.000	-0.004	-0.005
	(0.008)	(0.012)	(0.020)	(0.000)	(0.001)	(0.004)	(0.002)	(0.006)	(0.009)
120-91	0.005	0.005	-0.011	0.000	0.002	-0.003	-0.000	0.001	-0.010
	(0.007)	(0.012)	(0.021)	(0.000)	(0.002)	(0.005)	(0.000)	(0.007)	(0.009)
90-61	0.010	0.013	0.008	-0.001	0.006*	0.005	0.001	0.005	-0.003
	(0.009)	(0.013)	(0.022)	(0.001)	(0.003)	(0.004)	(0.002)	(0.006)	(0.008)
60-31	0.002	0.011	-0.007	0.000	0.004*	0.005	0.002	0.003	-0.006
	(0.007)	(0.012)	(0.020)	(0.000)	(0.002)	(0.005)	(0.002)	(0.006)	(0.008)
30-1	0.014*	0.021*	0.039*	-0.000	0.002	-0.003	-0.001	0.002	-0.000
	(0.008)	(0.013)	(0.022)	(0.000)	(0.002)	(0.004)	(0.002)	(0.007)	(0.008)
MDV	0.01	0.03	0.09	0.00	0.00	0.00	0.00	0.01	0.01
Obs.	11992	11992	11615	11992	11992	11615	11992	11992	11615

Notes: See notes for Table 2. Robust standard errors in parentheses.

4.4 Mechanisms

Our main analyses show that early strike exposure matters more for child and maternal health relative to late exposure. We interpret this finding as support for the hypothesis that early NHV matters more for the considered health outcomes than later visits. To speak to potential mechanisms for the observed effects, we focus on the elements of the nurse home visiting program that are central during the early visits: i) information and counseling and ii) screening and monitoring of infant and maternal health, in particular maternal postnatal mental health.

First, to assess the importance of information and counseling in explaining the negative effects of forgoing an early nurse visit, we study heterogeneous effects across two relevant dimensions: parental education in health-related fields or childcare, and the parity of the child. Specifically, we hypothesize that parents without professional knowledge about child health and development and first-time parents may see larger effects of early strike exposure if information is an important element that strike-exposed parents lack. For brevity, we present results for our measure of total GP contacts during the strike, in year one and year two through four of the child's life (Table 7 for parental health-related education and Appendix Table A7 for child parity). We split our sample and additionally estimate an interacted model on the full sample (Appendix Table A8).[26]

Table 7 shows regression results for samples divided into groups of parents with and without an education in a childcare or a health-related field. While we do not find significant effects of the timing of strike exposure for children of parents educated in a health-related field, for children of parents *not* educated in those fields, our results resemble the main results. Due to power issues, we cannot rule out equality of estimates across groups (as shown by the result for the interacted model in Appendix Table A8). However, the size

[26]We exclude control variables in both the split sample and interacted analyses for consistency. Our conclusions are not sensitive to the omission. Results for regular and out-of-office hours GP contacts are available on request. Moreover, Appendix Figure A6 presents graphical evidence on the raw relationship between the timing of strike exposure and GP contacts accumulated at age four by parental health education status.

Table 7 Heterogeneity: Effects of strike exposure on total GP contacts by parental health-related education

	Total GP contacts					
	Not health educ.			Health educ.		
	During strike	1st year	2-4 years	During strike	1st year	2-4 years
	(1)	(2)	(3)	(4)	(5)	(6)
Days						
180-151	-0.066	0.404	0.424	-0.396	0.754	2.446
	(0.132)	(0.519)	(1.083)	(0.325)	(1.125)	(2.539)
150-121	-0.102	0.215	0.668	-0.239	-0.093	2.908
	(0.135)	(0.505)	(1.054)	(0.378)	(1.346)	(2.913)
120-91	-0.010	0.631	-0.336	-0.498	-0.181	1.942
	(0.130)	(0.506)	(1.015)	(0.341)	(1.266)	(2.547)
90-61	0.195	1.552***	1.806*	-0.287	-0.895	1.104
	(0.133)	(0.536)	(1.044)	(0.323)	(1.192)	(2.611)
60-31	0.232*	0.975*	1.873*	-0.480	0.959	3.501
	(0.132)	(0.509)	(1.049)	(0.329)	(1.121)	(2.450)
30-1	0.401***	2.017***	3.278***	-0.098	0.447	0.523
	(0.145)	(0.532)	(1.103)	(0.327)	(1.172)	(2.206)
MDV	1.55	9.79	20.95	1.46	8.59	18.77
Observations	11065	11065	10708	1741	1741	1687

Notes: See notes for Table 4. Each column shows estimates from separate regressions. Column labels indicate the relevant subgroup and outcome variable studied. All regressions exclude control variables. The table splits the sample by parental educational background in a health and childcare-related field (either one of the parents are educated as a doctor, midwife, nurse or pedagogue). Robust standard errors in parentheses.

of the estimates carefully suggests stronger impacts of early strike exposure for non-health educated parents. Another group of parents that may benefit particularly from early visits are first-time parents. Also at this margin, we see some indication for first-born children having more GP contacts in the longer run if they miss an early home visit but we cannot rule out equality of estimates across groups (see Appendix Table A7).

Considering other margins, as shown by Appendix Table A9, we find less clear differences in estimates across families of high or low socio-economic status, if anything, high SES families appear to see larger effects of early strike exposure.[27] This finding may further underline the importance of specific guidance and information for new parents and, additionally, points to the potential importance of another channel for early life NHV, namely universal screening and health monitoring.

Early NHV puts a strong focus on screening for potential health problems in infants and mothers: Offered as a universal program, it represents an early window of opportunity to detect and confront health problems. Our results for maternal mental health in Table 5 suggest that a lack of early screening impacts maternal mental health negatively. Our design does not allow for a causal analysis of the impact of screening vs. no screening. However, we can describe the performance of nurses with respect to screening in non-strike years. This descriptive analysis can inform us about the potential gains to early screening.

Figure 4 presents nurse registrations, referrals and maternal health care usage for two groups of mothers in our control cohort data: first, mothers with registrations of maternal mental health problems at the initial visit (10 percent of mothers) and mothers without these registrations. Conditional on completing the follow-up visits, we observe interesting patterns that point to the importance of nurse screenings very shortly after birth: Nurses are more likely to register mental health problems in later visits for early-detected mothers. Additionally, mothers with early-detected mental health problems receive more referrals to other

[27] Appendix Table A9 also examines heterogeneity by gender, child initial health, and parental risky 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, also in these analyses, we cannot reject equality of effects in most cases).

health professionals and, importantly, among early-detected mothers there is a higher prevalence of externally-measured mental health issues (contacts with psychologists/psychiatrists registered in the administrative data).

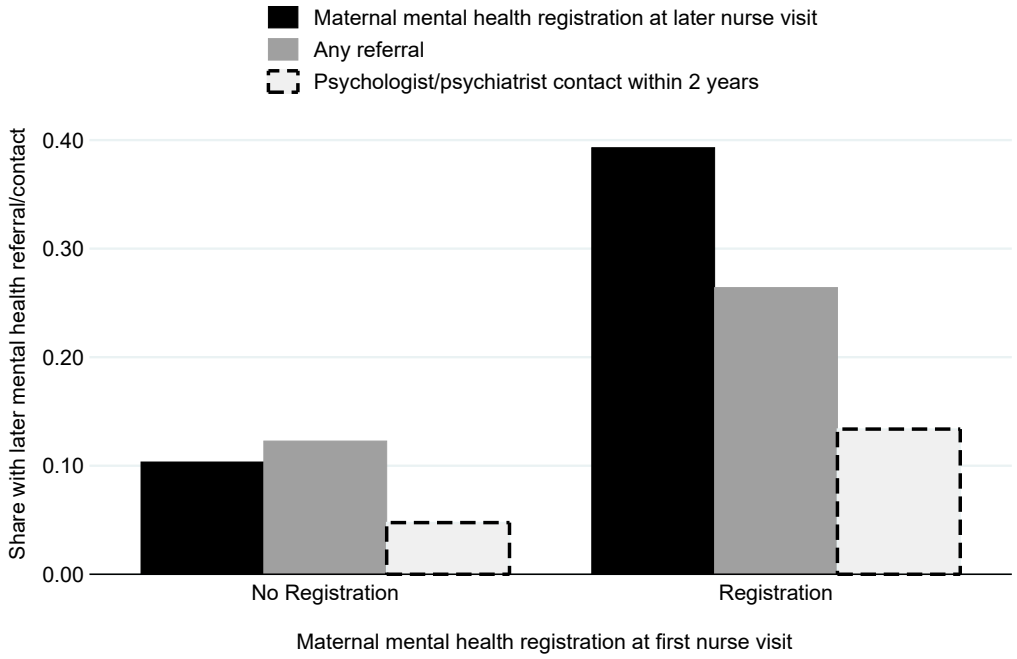


Fig. 4 Share of mothers with nurse registrations related to maternal mental health issues, referrals and contacts to psychologists/psychiatrists by initial registered concern (0/1) at initial nurse visit *Notes:* This figure divides mothers of control cohort children (born in Copenhagen between September 17, 2008 - April 15, 2009) into two groups: The 10 percent of mothers with a mental health concern registered by nurses in their initial visit and the 90 percent of mothers without an initially registered concern. For each group, we plot the share of mothers who receive registrations of maternal mental health issues at later universal visits, the share who are referred to other health care professionals by nurses, and the share for whom we observe any contacts with psychologists/psychiatrists up to two years after their birth.

Relating Figure 4 to the overall prevalence of maternal mental-health related contacts, our calculations suggest that nurses during their first visit identify up to one out of four of the mothers who end up having a mental-health related contact with specialists in the first two years of their child's life.[28] This illustrative figure suggests large potential health returns from early screening efforts.

[28]Nurses screen around 10 percent of mothers in the sample as having a mental health problem. Of those, 13 percent end up having at least one psychologist/psychiatrist contact in the first two years of the child's

A final and important potential pathway for the effect of early nurse visits 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 and empirical strategy, we are constrained in an analysis of those parental behaviors: Appendix Tables A10 and A11 study whether the timing of strike exposure impacts participation in the childhood preventive care and vaccination programs as outcomes.[29] As the tables illustrate, we cannot draw firm conclusions due to very 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 using our measures of total child GP contacts as outcome in the appendix material. We show that our conclusions are not sensitive to the omission of individual-level control variables (Appendix Table A12) and reasonable alternative choices of bin size (Appendix Tables A13 and A14). To rule out that our measure of strike exposure captures other factors, we implement a set of placebo regressions: Appendix Table A15 shows estimates from those regressions 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. Given that children born immediately prior to the beginning of the strike are likely to miss more than one nurse visit, we implement a doughnut hole approach to rule out that those children drive our results. As shown in Appendix Table A16, our main findings are robust to the omission of children born within the 14 days prior to strike start.

life. In the population, the prevalence of those contacts is around 5 percent. These figures suggest that nurses may capture around 20 percent of those mothers, who end up with a contact.

[29] 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. The DNBH specifically mentions nurse visits as a central element to promote the benefits of vaccinations to parents (The Danish National Board of Health [Sundhedsstyrelsen], 2018).

In additional robustness tests, we have ruled out that including movers from Copenhagen alters our conclusions. By using earlier cohorts of children as a control group, by examining the impact of strike exposure on children aged five during the strike, and by constraining our main analysis to using data from the years 2008 and 2009 (where we have full coverage of the nurse records), we confirm that our choices of control and treatment groups do not drive our findings. Our main conclusions—that earlier strike exposure is relative more important for children's and maternal health than later exposure—remain intact across these iterations.

4.6 Costs and Benefits

Given our results for the health effects of strike exposure, in a final part of the analyses we quantify the costs of a nurse visit and relate it to the immediate costs of prevented GP visits for mothers and children. This stylized calculation does not constitute a full cost-benefit analysis but can help quantify the the immediate consequences of the strike.

To quantify the costs of a home visit, we only consider the direct costs related to nurses' salaries and assume that all types of home visits have the same average cost.[30] We estimate the weekly number of canceled visits during the strike to be 760.[31] After the strike, the municipality of Copenhagen reported daily savings during the strike of 35,500 EUR per workday or 177,500 EUR per (business) week (because the municipality did not pay salaries to the unionized nurses on strike).

To calculate the cost of a nurse visit from these figures, we take into account that nurses have several obligations: for example, they provide office hours, monitor the health of school children, and offer support to daycare centers. In our data for the control period, we observe that 155 nurses performed 845 weekly visits, implying 5.5 weekly visits per nurse. Assuming that one visit lasts an hour and that nurses spend an additional 1.5 hours on preparation,

[30] We abstract from any fixed and variable costs beyond salaries to nurses. Examples of fixed costs are the education of nurses, capital (cars, building stock and software). Variable costs beyond salaries to nurses are management costs, cleaning services, transportation, lunch and coffee among others.

[31] In our nurse data we observe that, during the full seven weeks of the strike, 85 weekly nurse visits were performed. In the equivalent weeks of the following year, the weekly average of visits was 845. We assume that the difference in weekly visits equals the number of canceled visits caused by the strike ($845 - 85 = 760$).

transportation and registration, nurses spend 13.75 hours weekly on performing home visits. Assuming that the average nurse works 30 hours per week, we estimate that nurses spend $13.75/30 = 45.8$ percent of their working time on home visits to families with infants. Thus adjusting the the weekly savings reported by the municipality with the actual time spent on home visits and dividing by the number of canceled visits, we estimate the cost of a home visit to be 107 EUR.[32]

As mentioned initially, we do not quantify all benefits of receiving an early nurse visit but focus on the costs related to increased GP visits among early strike-exposed children and mothers. Appendix Table A17 presents results for the impact of strike exposure on accumulated GP fees (for both mother and child) rather than the number of GP visits.[33] As we disregard longer-run benefits, such as prevented child hospital admissions, potential spill-over effects to other domains, such as child cognitive development or maternal mental health, or the time cost associated to additional GP visits, our measure of benefits (prevented GP costs) is likely very conservative. Children born in the 30-1 and 60-31 days age bin and their mothers have significantly higher GP expenses, in line with our finding of increased GP contacts for these groups. Specifically, the given groups have on average 157.8 and 98.1 EUR higher GP expenses accumulated at age four.

These stylized calculations illustrate that even under conservative assumptions about the benefits of visits, the initial nurse visits are likely to have a positive return. Importantly, however, our inability to measure returns to nurse visits in other domains that may be especially relevant for later nurse contacts (such as parent-child interactions and other dimensions of child development) should be kept in mind. In conclusion, our simple analysis highlights that early nurse visits are worth the while: the cost of an early nurse visit is lower than average prevented GP costs in the medium run and thus there is a case for early visits having a high priority.

[32] (177.500 EUR $\times$ 45.8 percent)/760 visits = 107 EUR per visit

[33] 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.

5 Conclusion

Using linked nurse records and administrative data and exploiting exogenous variation induced by a large-scale nurse strike, we provide causal evidence on the impact of NHV beyond the extensive margin of treatment exposure: Studying the Danish universal program, we find that early NHV (during the initial weeks and first two months of the child's life) impacts both child and maternal health trajectories (primarily measured as health care usage in our analyses). Given that access to early NHV impacts out-of-office hours GP contacts and children's hospitalization—also when we omit first year outcomes to avoid picking up substitution—we conclude that earlier visits are more important for children's and mothers' underlying health than later visits.

The suggestive evidence for heterogeneity of effects by parental health knowledge and child parity point to the importance of information and parental confidence as channels for these health effects—supporting both is at the core of early home visits. While we do not directly observe parental beliefs and only have few measures of actual parental investment behaviors, both factors may be contributing to the effects of early home visits that we find.

Importantly, indicating the importance of timely screening for health issues, we find that early NHV also plays a role for maternal postpartum mental health. 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 postnatal 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. In this aspect, our study echoes the finding of other recent work pointing to the importance of supporting the mental health of new parents.

Finally, while initial visits in the Danish program focus on topics such as mother and infant physical health, infant feeding, sleep patterns, and maternal mental well-being, later

nurse visits increasingly focus on other domains of child development and the quality of parent-child interactions. In our setting, we do not find that the later visits impact the health outcomes that we can study. However, these visits and their content may play an important role in further shaping parental investments and child development in other domains. We leave this topic as an important alley for future research.

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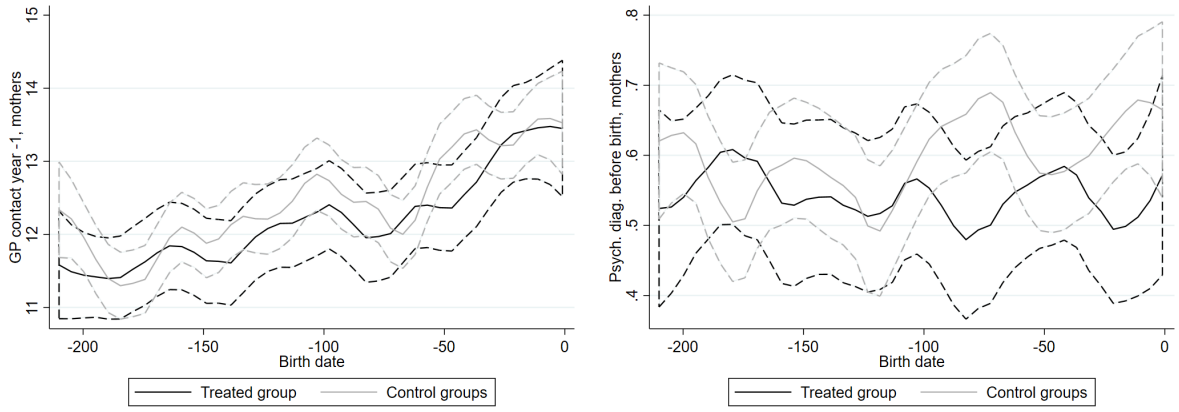
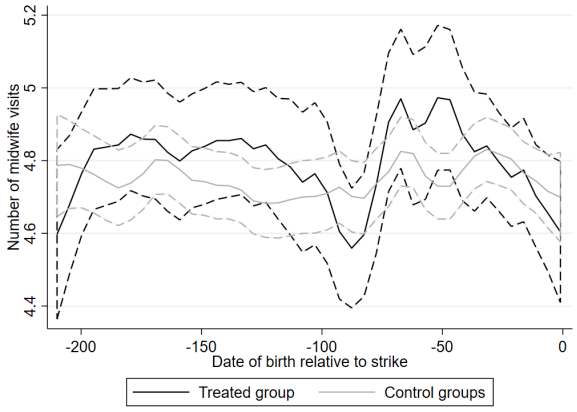
A Appendix - For online publication

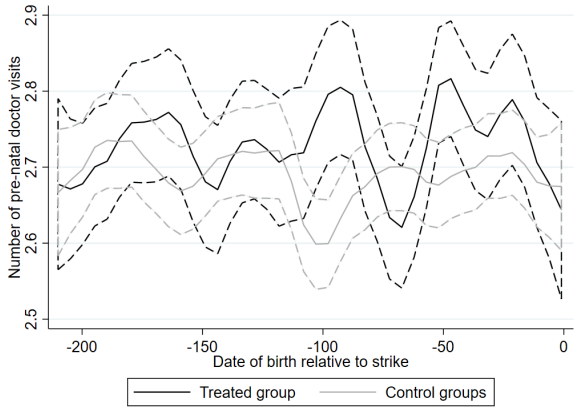


(a) Mothers GP contacts, year prior birth **(b)** Mother psychiatric diagnosis, prior to birth

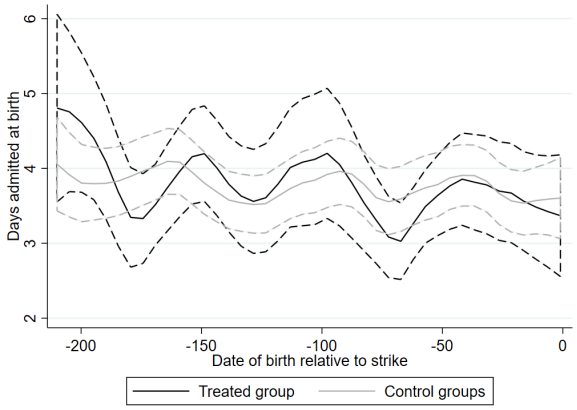
Fig. A1 Common trend in pre-treatment outcomes: (a) Number of maternal GP contacts in the year prior to birth and (b) Indicator for mother receiving a psychiatric diagnosis in the year prior to birth.
Notes: Se notes to Figure 3. Treated cohort: September 18, 2007 - April 15, 2008. Control cohort: September 17, 2008 and 2009 - April 15, 2009 and 2010).



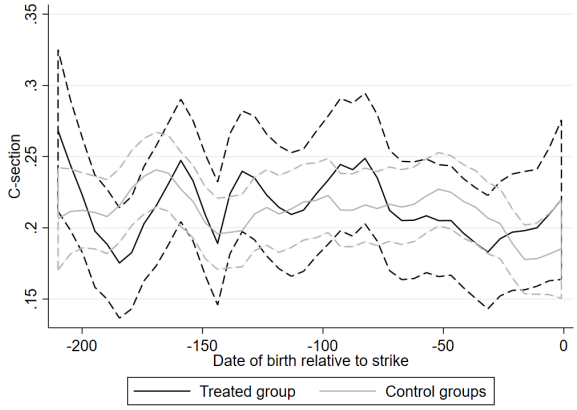
(a) Midwife visits



(b) Prenatal GP contacts



(c) Days admitted at birth



(d) C-sections

(e) Maternal employment, year of birth

Fig. A2 Care around birth and maternal employment for the treated cohort and control cohorts *Notes:* Panel (a) shows the average number of prenatal midwife contacts, panel (b) shows the average number of prenatal GP consultations, panel (c) shows the average number of days admitted to hospital at birth and panel (d) shows the C-section rate. Panel (e) shows the share of time in employment for mothers in the year of birth. See notes to Figure 3. The sample includes children who were born in Copenhagen in the cohort (September 18, 2007 - April 15, 2008) and control cohorts (September 17, 2008 and 2009 - April 15, 2009 and 2010).

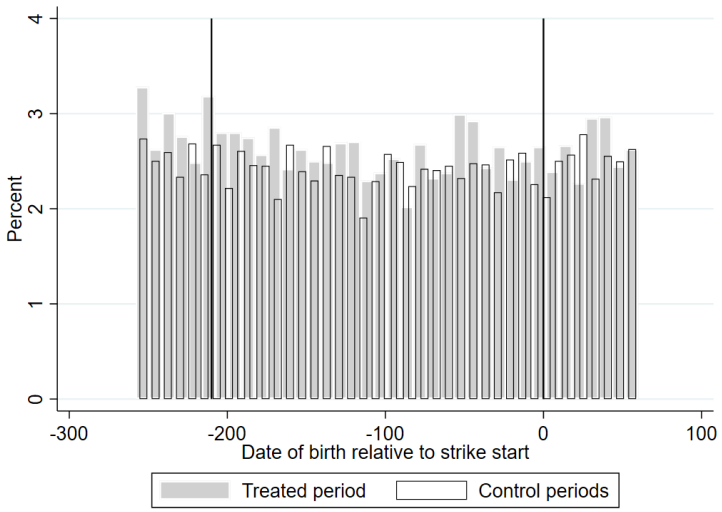


Fig. A3 Density of births

Notes: The figure show the density of births for equally sized bins and a window 258 days prior to the beginning of the strike and 60 days 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 cohort: September 18, 2007 - April 15, 2008 and control cohorts: September 17, 2008 and 2009 - April 15, 2009 and 2010).

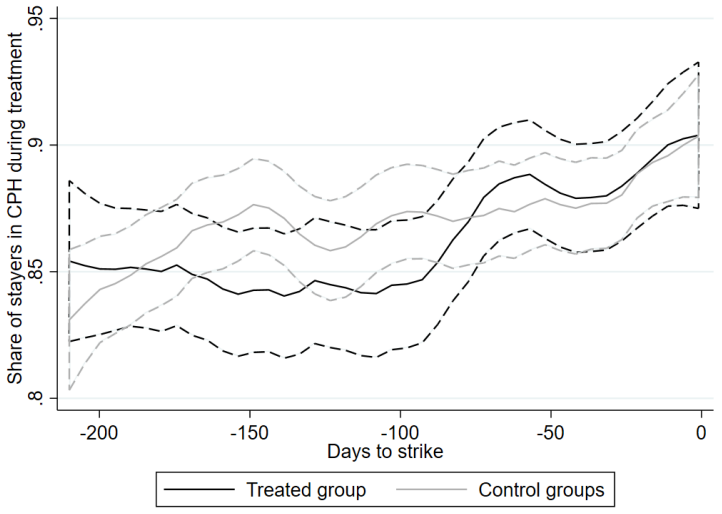
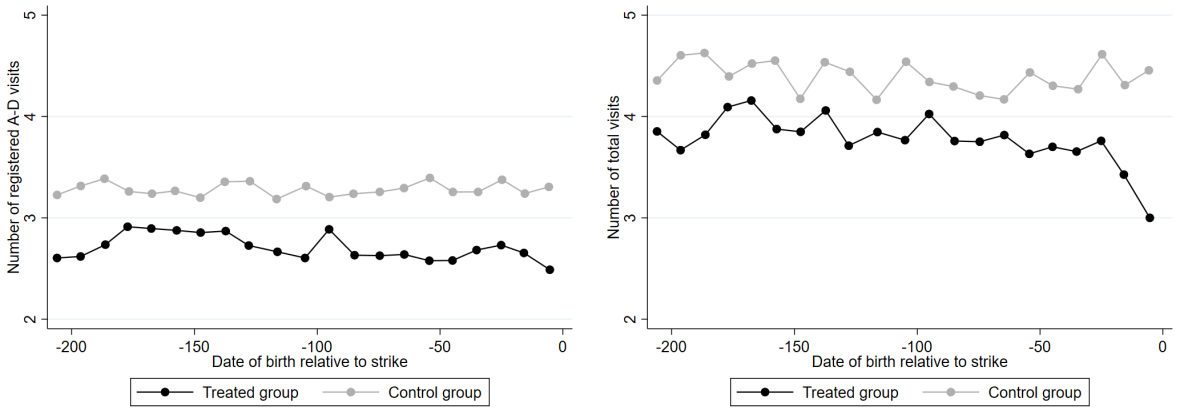


Fig. A4 Share of children observed as Copenhagen residents on January 1 in the treated and control cohorts

Notes: Se notes to Figure 3. The sample includes children who were born in Copenhagen in the treated (September 18, 2007 - April 15, 2008) and control cohorts (September 17, 2008 and 2009 - April 15, 2009 and 2010).

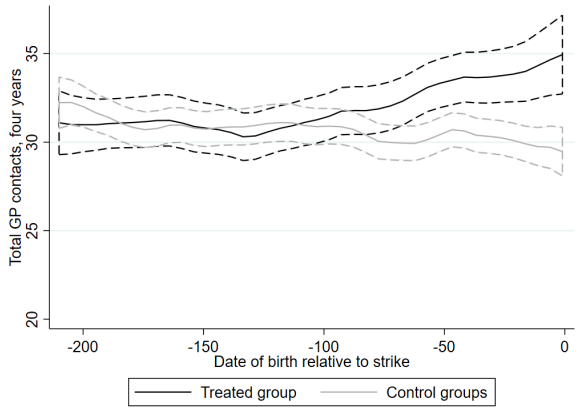


(a) Universal nurse visits

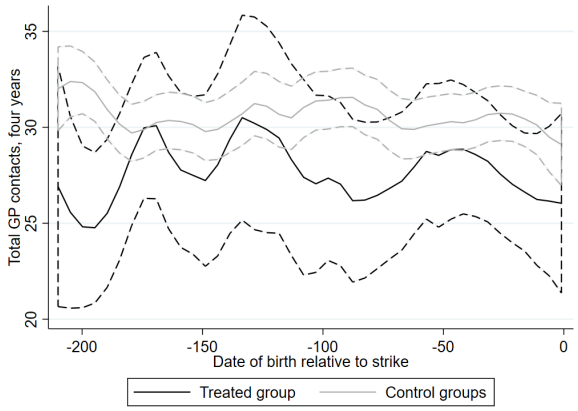
(b) Total nurse visits (universal + extra)

Fig. A5 Average number of universal and total nurse home visits for children in the treated and control cohorts

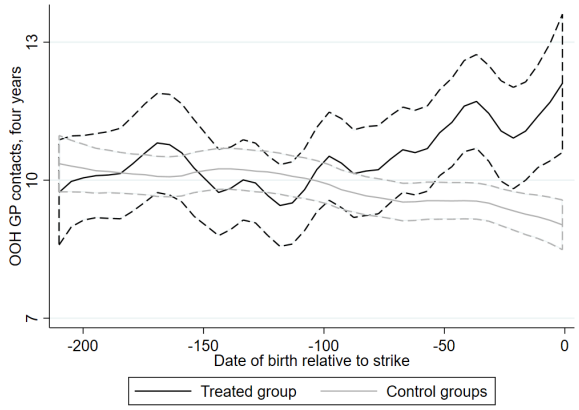
Notes: Average number of visits is calculated for children in the treated (September 18, 2007 - April 15, 2008) and control cphort (September 17, 2008 - April 15, 2009). 21 equally sized 10-days bins.



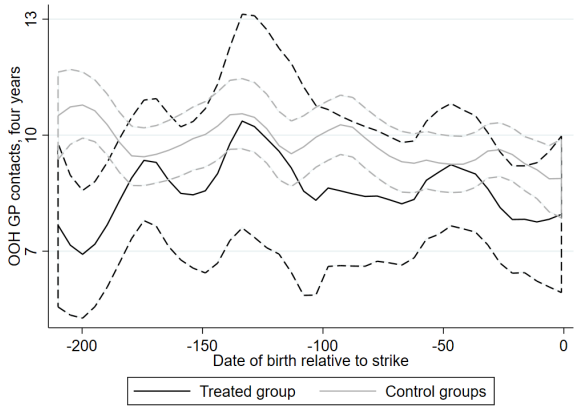
(a) Total GP contacts - Not Health educated



(b) Total GP contacts - Health educated



(c) Out-of-office hours GP contacts - Not health educated



(d) Out-of-office hours GP contacts - Health educated

Fig. A6 Accumulated child GP contacts at age four by parental education status.
Notes: The figure shows the relationship between date of birth and accumulated GP contacts at age four by parental background. See Figure 2 for further details.

Table A1 Nurse home visiting in the municipality of Copenhagen

Visit (and eligibility)	Timing
Universal visits	
Initial visit	0-14 days after birth
2-month visit	After two months of life
4-month visit	After four months of life
8-month visit	After eight months 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	At discretion of nurses

Notes: Source: Official guidelines for the Copenhagen NHV program.

Table A2 Overview on main topics at nurse visits and optional nurse registrations in the municipality of Copenhagen.

	Topic	Examples for items that nurses can register (some visit-specific)
	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, family food habits
(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	
	a. Physical health	Weight and height, jaundice
	b. Reflexes	Sucking, crawling, Babinski
	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 topics covered during home visit. Nurses grant up to four scheduled universal visits (at around 0-14days, 2months, 4 months and 8 months). 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 (on parental demand), respectively.

Table A3 Balancing Test: 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	-0.020	-9.182	-132.863	0.034	-0.028	0.013	0.008
	(0.024)	(0.024)	(10.929)	(150.890)	(0.031)	(0.032)	(0.011)	(0.007)
150-121	-0.021	-0.003	-1.826	-146.880	-0.008	-0.025	0.004	0.004
	(0.024)	(0.024)	(10.717)	(150.983)	(0.031)	(0.032)	(0.010)	(0.007)
120-91	0.008	-0.041*	11.523	-121.833	0.045	-0.015	-0.012	-0.002
	(0.025)	(0.023)	(11.155)	(151.794)	(0.031)	(0.033)	(0.011)	(0.006)
90-61	0.018	0.004	-2.006	-124.096	0.046	0.017	0.014	0.007
	(0.025)	(0.024)	(11.594)	(150.980)	(0.029)	(0.034)	(0.011)	(0.006)
60-31	-0.031	-0.008	-2.706	-115.308	0.050*	-0.029	0.011	0.008
	(0.024)	(0.024)	(10.938)	(150.585)	(0.029)	(0.032)	(0.010)	(0.006)
30-1	-0.017	-0.039*	12.044	-92.950	0.034	-0.015	0.015	-0.003
	(0.024)	(0.023)	(30.893)	(151.042)	(0.029)	(0.033)	(0.011)	(0.006)
Obs.	12568	12568	12568	12568	12568	12568	12568	12332

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

Table A4 Balancing test: 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)	Female 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 A3.

Table A5 Variable means, population of children born in Copenhagen and Denmark.

	Denmark Excl. CPH		CPH	
	Mean	Obs.	Mean	Obs.
Cohabitation	0.85	116827	0.77	18302
Married	0.47	116827	0.38	18302
Prim. school, mother	0.18	116827	0.13	18302
Uni. degree, mother	0.13	116827	0.31	18302
Student, mother	0.03	116827	0.05	18302
Employed, mother	0.80	116827	0.77	18302
Prim. school, father	0.18	116827	0.13	18302
Uni. degree, father	0.13	116827	0.30	18302
Student, father	0.01	116827	0.03	18302
Employed, father	0.88	116827	0.82	18302
Danish, mother	0.86	116827	0.76	18302
Danish, father	0.86	116827	0.74	18302
Young mother	0.05	116827	0.02	18302
Young father	0.02	115578	0.01	17949
Income, mother	274.16	116827	286.50	18302
Income, father	393.88	116827	387.16	18302
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	0.12	112011	0.06	17860
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 periods: September 18, 2007, 2008, 2009 - April 15, 2008, 2009, 2010. The Denmark samples includes all children born in the same periods in Denmark, excluding Copenhagen.

Table A6 Additional child health outcomes: Effects of strike exposure on child hospitalization and outpatient contacts

	(1) Hospital adm. during strike	(2) Hospital adm. 1st year	(3) Hospital adm. 2-4 years	(4) Outpat. cont. during strike	(5) Outpat. cont. 1st year	(6) Outpat. cont. 2-4 years
Days						
180-151	-0.023**	0.005	0.034	-0.000	0.007	0.003
	(0.012)	(0.030)	(0.033)	(0.016)	(0.035)	(0.036)
150-121	-0.017	-0.031	-0.013	0.002	0.037	-0.006
	(0.013)	(0.030)	(0.034)	(0.016)	(0.035)	(0.036)
120-91	-0.023*	0.004	-0.012	0.005	-0.012	-0.049
	(0.013)	(0.031)	(0.034)	(0.017)	(0.035)	(0.037)
90-61	-0.013	0.040	0.015	0.010	-0.005	-0.038
	(0.014)	(0.031)	(0.034)	(0.018)	(0.035)	(0.037)
60-31	-0.021	0.000	0.075**	-0.014	-0.033	-0.018
	(0.015)	(0.030)	(0.034)	(0.018)	(0.035)	(0.036)
30-1	-0.010	0.034	0.064*	-0.032*	-0.059*	-0.002
	(0.017)	(0.031)	(0.034)	(0.019)	(0.034)	(0.036)
MDV	0.05	0.27	0.31	0.07	0.39	0.55
Obs.	11992	11992	11615	11992	11992	11615

Notes: See notes to table 4. Robust standard errors in parentheses.

Table A7 Heterogeneity: Effects of strike exposure on total GP contacts by parity

	Total GP contacts					
	Higher parity			First-borns		
	During strike	1st year	2-4 years	During strike	1st year	2-4 years
	(1)	(2)	(3)	(4)	(5)	(6)
Days						
180-151	-0.116	0.644	-1.086	-0.102	0.321	1.677
	(0.183)	(0.727)	(1.507)	(0.165)	(0.626)	(1.313)
150-121	-0.152	-0.365	-1.390	-0.084	0.495	2.177*
	(0.201)	(0.754)	(1.492)	(0.168)	(0.614)	(1.307)
120-91	0.192	0.992	-1.123	-0.218	0.161	0.003
	(0.199)	(0.751)	(1.491)	(0.159)	(0.612)	(1.224)
90-61	0.240	0.952	0.987	0.069	1.396**	2.304*
	(0.182)	(0.712)	(1.468)	(0.167)	(0.669)	(1.290)
60-31	0.378**	1.460**	0.873	-0.023	0.617	2.730**
	(0.185)	(0.703)	(1.428)	(0.165)	(0.625)	(1.298)
30-1	0.423**	2.227***	0.743	0.232	1.208*	3.448***
	(0.207)	(0.768)	(1.488)	(0.175)	(0.633)	(1.334)
MDV	1.45	8.89	17.88	1.60	10.17	22.59
Observations	4918	4918	4765	7650	7650	7401

Notes: See notes for Table 4. Column labels indicate the relevant subgroup and outcome variable studied. All regressions are estimated without the inclusion of control variables. The table splits the sample by parity of the child. Robust standard errors in parentheses.

Table A8 Heterogeneity: Effects of strike exposure on total GP contacts by parental health-related education and child parity, interacted model

	Health education			Parity		
	Total GP during strike	Total GP 1st year	Total GP 2-4 years	Total GP during strike	Total GP 1st year	Total GP 2-4 years
	(1)	(2)	(3)	(4)	(5)	(6)
Days						
180-151	-0.330	0.351	2.045	0.014	-0.323	2.764
	(0.350)	(1.235)	(2.754)	(0.246)	(0.959)	(1.998)
150-121	-0.137	-0.304	2.310	0.068	0.855	3.573*
	(0.400)	(1.434)	(3.088)	(0.262)	(0.972)	(1.983)
120-91	-0.487	-0.807	2.364	-0.409	-0.837	1.133
	(0.364)	(1.360)	(2.736)	(0.254)	(0.968)	(1.929)
90-61	-0.482	-2.445*	-0.653	-0.171	0.442	1.321
	(0.348)	(1.304)	(2.805)	(0.247)	(0.977)	(1.954)
60-31	-0.712**	-0.012	1.708	-0.400	-0.851	1.870
	(0.354)	(1.229)	(2.657)	(0.248)	(0.940)	(1.929)
30-1	-0.499	-1.567	-2.714	-0.190	-1.030	2.718
	(0.357)	(1.283)	(2.460)	(0.271)	(0.995)	(1.998)
Observations	12806	12806	12395	12568	12568	12166

Notes: Each column shows estimates from a separate regression. Column labels indicate the relevant subgroup of our sample. We do not include additional control variables The coefficients are for the interactions of 30 day bins, a strike indicator and subgroup. All regressions include cohort fixed effects, bin fixed effects and a full set of bin indicator, strike and subgroup interactions. The sample includes children who were born in Copenhagen in the treated cohort (September 18, 2007 - April 15, 2008) and in control cohorts (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses.

Table A9 Heterogeneity: Effects of strike exposure on total GP contacts at age four

	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	2.313	-0.343	1.037	-0.011	1.932	-0.704	0.665	1.544
	(1.861)	(1.929)	(1.431)	(4.166)	(1.498)	(2.805)	(1.383)	(6.591)
150-121	-0.621	3.268*	1.221	1.649	2.812*	-2.741	1.070	-1.330
	(1.820)	(1.948)	(1.392)	(4.987)	(1.528)	(2.664)	(1.387)	(5.629)
120-91	0.677	0.400	-0.092	4.566	2.171	-2.981	-0.006	3.338
	(1.808)	(1.793)	(1.347)	(4.459)	(1.436)	(2.648)	(1.317)	(5.952)
90-61	4.549**	1.039	2.771**	1.681	2.941*	1.703	2.257	5.056
	(1.865)	(1.860)	(1.399)	(4.473)	(1.508)	(2.642)	(1.389)	(5.060)
60-31	3.192*	3.247*	2.282*	12.475**	3.803***	1.428	2.317*	7.409
	(1.926)	(1.761)	(1.356)	(5.399)	(1.473)	(2.671)	(1.373)	(4.947)
30-1	5.781***	2.937	3.765***	7.487	4.662***	4.278	3.590**	7.096
	(1.918)	(1.887)	(1.424)	(4.986)	(1.491)	(2.900)	(1.402)	(6.019)
MDV	31.67	28.92	30.17	33.61	29.82	31.60	30.23	33.93
Observations	6426	5969	11162	994	8709	3530	11316	782

Notes: See notes to Table 4. Each column presents the results from a separate regression excluding all control variables. Columns (1)-(2) split the sample by child gender. Columns (3)-(4) split the sample by initial health (an indicator for 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, parental age below 21 at birth or with only primary schooling completed. Columns (7)-(8) split the sample by maternal smoking status during pregnancy. Robust standard errors in parentheses.

Table A10 Parental investments: Effects of strike exposure on participation in preventive health checks

	(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	-0.002	0.006	0.004	0.062*	0.049	0.030
	(0.022)	(0.019)	(0.018)	(0.034)	(0.035)	(0.030)
150-121	0.008	-0.007	0.013	0.045	0.033	0.004
	(0.021)	(0.018)	(0.019)	(0.034)	(0.035)	(0.031)
120-91	-0.009	-0.008	-0.008	0.012	-0.040	-0.020
	(0.022)	(0.019)	(0.019)	(0.035)	(0.036)	(0.031)
90-61	0.016	0.003	0.013	0.104***	0.093***	0.034
	(0.021)	(0.020)	(0.018)	(0.034)	(0.036)	(0.031)
60-31	0.014	-0.014	0.031*	0.029	0.087**	0.016
	(0.021)	(0.019)	(0.018)	(0.034)	(0.035)	(0.030)
30-1	0.011	0.002	0.016	0.055	0.076**	0.034
	(0.020)	(0.019)	(0.018)	(0.034)	(0.035)	(0.030)
MDV	0.92	0.93	0.93	0.66	0.58	0.79
Obs.	11992	11992	11992	11897	11749	11646

Notes: See notes for Table 4. Outcomes are indicators for participation in each consultation in the preventive health care program. Robust standard errors in parentheses.

Table A11 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	-0.030	-0.014	-0.039*
	(0.025)	(0.023)	(0.022)
150-121	-0.008	-0.030	-0.038*
	(0.024)	(0.023)	(0.022)
120-91	0.012	-0.008	-0.045**
	(0.024)	(0.023)	(0.023)
90-61	-0.013	-0.009	-0.020
	(0.025)	(0.024)	(0.022)
60-31	-0.022	-0.024	0.016
	(0.024)	(0.023)	(0.022)
30-1	0.005	0.002	-0.035
	(0.024)	(0.023)	(0.022)
MDV	0.90	0.91	0.91
Obs.	11992	11992	11992

Notes: See notes for Table 4. Outcomes are indicators for participation in each vaccination round scheduled within the first year of a child's life. Robust standard errors in parentheses.

Table A12 Robustness: Effects of strike exposure on child GP contacts without pre-treatment covariates

	(1) Total GP during strike	(2) Total GP 1st year	(3) Total GP 2-4 years	(4) Regul. GP during strike	(5) Regul. GP 1st year	(6) Regul. GP 2-4 years	(7) OOH GP during strike	(8) OOH GP 1st year	(9) OOH GP 2-4 years
Days									
180-151	-0.108	0.429	0.619	-0.115	-0.201	0.170	0.007	0.630**	0.448
	(0.124)	(0.476)	(0.995)	(0.087)	(0.310)	(0.619)	(0.069)	(0.254)	(0.537)
150-121	-0.112	0.174	0.814	-0.117	-0.102	0.553	0.005	0.276	0.261
	(0.129)	(0.475)	(0.990)	(0.087)	(0.306)	(0.615)	(0.074)	(0.260)	(0.532)
120-91	-0.085	0.397	-0.576	-0.129	-0.051	-0.039	0.044	0.448*	-0.537
	(0.123)	(0.473)	(0.943)	(0.087)	(0.313)	(0.598)	(0.069)	(0.249)	(0.509)
90-61	0.134	1.221**	1.836*	0.112	0.282	1.151*	0.022	0.939***	0.685
	(0.124)	(0.494)	(0.973)	(0.090)	(0.328)	(0.627)	(0.068)	(0.259)	(0.514)
60-31	0.136	0.975**	2.051**	0.058	0.255	1.198**	0.079	0.720***	0.853
	(0.124)	(0.470)	(0.965)	(0.090)	(0.306)	(0.610)	(0.066)	(0.251)	(0.520)
30-1	0.308**	1.638***	2.428**	0.161	0.596*	1.385**	0.147**	1.042***	1.043**
	(0.134)	(0.490)	(0.996)	(0.098)	(0.322)	(0.640)	(0.071)	(0.256)	(0.515)
MDV	1.54	9.63	20.65	1.13	6.64	13.98	0.41	2.99	6.67
Obs.	12568	12568	12166	12568	12568	12166	12568	12568	12166

Notes: See notes to Table 4. We estimate the effects of strike exposure without pre-treatment covariates. Robust standard errors in parentheses.

Table A13 Robustness: Effects of strike exposure on child GP contacts, 35-days bins

	(1) Total GP during strike	(2) Total GP 1st year	(3) Total GP 2-4 years	(4) Regul. GP during strike	(5) Regul. GP 1st year	(6) Regul. GP 2-4 years	(7) OOH GP during strike	(8) OOH GP 1st year	(9) OOH GP 2-4 years
Days									
175-141	-0.061	0.236	0.779	-0.103	-0.058	0.343	0.042	0.295	0.436
	(0.119)	(0.441)	(0.921)	(0.083)	(0.289)	(0.577)	(0.068)	(0.237)	(0.494)
140-106	-0.038	0.023	-0.232	-0.059	-0.093	0.124	0.022	0.116	-0.356
	(0.117)	(0.437)	(0.910)	(0.082)	(0.289)	(0.576)	(0.066)	(0.231)	(0.484)
105-71	0.073	0.873*	0.589	0.037	0.174	0.419	0.036	0.699***	0.170
	(0.118)	(0.459)	(0.880)	(0.084)	(0.306)	(0.566)	(0.066)	(0.242)	(0.468)
70-36	0.223*	1.045**	2.187**	0.144*	0.326	1.122**	0.079	0.719***	1.066**
	(0.116)	(0.437)	(0.884)	(0.085)	(0.288)	(0.567)	(0.062)	(0.231)	(0.473)
35-1	0.404***	1.663***	2.590***	0.215**	0.715**	1.583***	0.189***	0.948***	1.007**
	(0.124)	(0.452)	(0.923)	(0.092)	(0.300)	(0.600)	(0.066)	(0.237)	(0.477)
Control group mean	1.54	9.63	20.65	1.13	6.64	13.98	0.41	2.99	6.67
Observations	11992	11992	11615	11992	11992	11615	11992	11992	11615

Notes: See notes to Table 4. We increase the bin size to 35 days. Robust standard errors in parentheses.

Table A14 Robustness: Effects of strike exposure on child GP contacts, 21-days bins

	(1) Total GP during strike	(2) Total GP 1st year	(3) Total GP 2-4 years	(4) Regul. GP during strike	(5) Regul. GP 1st year	(6) Regul. GP 2-4 years	(7) OOH GP during strike	(8) OOH GP 1st year	(9) OOH GP 2-4 years
Days									
189-169	0.115	0.926	1.067	0.0709	0.423	0.476	0.0442	0.503*	0.591
	(0.150)	(0.565)	(1.145)	(0.104)	(0.370)	(0.735)	(0.0866)	(0.299)	(0.606)
168-148	-0.0699	0.480	0.622	-0.114	0.0381	0.344	0.0438	0.442	0.278
	(0.156)	(0.561)	(1.202)	(0.108)	(0.376)	(0.746)	(0.0876)	(0.293)	(0.649)
147-127	-0.0899	0.255	0.664	-0.0408	0.262	0.708	-0.0491	-0.00720	-0.0439
	(0.157)	(0.568)	(1.184)	(0.108)	(0.372)	(0.755)	(0.0864)	(0.304)	(0.621)
126-106	-0.000275	0.622	0.0621	-0.0870	0.205	0.296	0.0868	0.417	-0.234
	(0.149)	(0.559)	(1.162)	(0.108)	(0.380)	(0.746)	(0.0820)	(0.285)	(0.618)
105-85	0.112	1.499**	1.053	0.0540	0.545	0.681	0.0576	0.954***	0.372
	(0.156)	(0.596)	(1.120)	(0.111)	(0.395)	(0.728)	(0.0875)	(0.319)	(0.599)
84-64	0.182	1.110*	0.760	0.125	0.265	0.562	0.0567	0.846***	0.197
	(0.151)	(0.581)	(1.139)	(0.110)	(0.396)	(0.741)	(0.0815)	(0.293)	(0.592)
63-43	0.256*	1.586***	2.602**	0.180	0.724*	1.548**	0.0755	0.862***	1.053*
	(0.149)	(0.558)	(1.131)	(0.111)	(0.374)	(0.739)	(0.0767)	(0.290)	(0.601)
42-22	0.193	1.274**	3.054**	0.0260	0.518	1.928**	0.167**	0.756**	1.126*
	(0.157)	(0.579)	(1.189)	(0.116)	(0.383)	(0.757)	(0.0820)	(0.305)	(0.629)
21-1	0.541***	2.306***	3.007**	0.372***	1.105***	1.508*	0.169**	1.201***	1.499**
	(0.164)	(0.583)	(1.203)	(0.120)	(0.393)	(0.791)	(0.0860)	(0.296)	(0.615)
Control group mean	1.54	9.63	20.65	1.13	6.64	13.98	0.41	2.99	6.67
Observations	11992	11992	11615	11992	11992	11615	11992	11992	11615

Notes: See notes to Table 4. We reduce the bin size to 21 days. Robust standard errors in parentheses.

Table A15 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 during strike	(2) Total GP 1st year	(3) Total GP 2-4 years	(4) Regul. GP during strike	(5) Regul. GP 1st year	(6) Regul. GP 2-4 years	(7) OOH GP during strike	(8) OOH GP 1st year	(9) OOH GP 2-4 years
Days									
180-151	0.047	0.064	-0.689	-0.019	-0.108	0.014	0.066	0.172	-0.703
	(0.158)	(0.562)	(1.100)	(0.104)	(0.359)	(0.697)	(0.095)	(0.308)	(0.591)
150-121	0.013	-0.081	-1.258	-0.078	-0.146	-1.029	0.091	0.065	-0.229
	(0.159)	(0.566)	(1.087)	(0.103)	(0.358)	(0.672)	(0.098)	(0.315)	(0.600)
120-91	0.069	0.456	0.114	-0.020	0.203	0.441	0.088	0.253	-0.327
	(0.154)	(0.575)	(1.100)	(0.104)	(0.366)	(0.691)	(0.090)	(0.314)	(0.594)
90-61	0.110	0.619	0.544	0.037	0.511	0.565	0.073	0.108	-0.021
	(0.153)	(0.564)	(1.093)	(0.102)	(0.367)	(0.690)	(0.092)	(0.301)	(0.588)
60-31	-0.087	0.312	-0.556	0.047	0.491	0.010	-0.134	-0.179	-0.566
	(0.154)	(0.571)	(1.084)	(0.107)	(0.366)	(0.695)	(0.089)	(0.314)	(0.584)
30-1	0.011	0.074	-0.712	-0.008	0.128	-0.029	0.019	-0.054	-0.683
	(0.158)	(0.563)	(1.072)	(0.113)	(0.369)	(0.701)	(0.087)	(0.296)	(0.554)
MDV	1.54	9.63	20.65	1.13	6.64	13.98	0.41	2.99	6.67
Obs.	8141	8141	7881	8141	8141	7881	8141	8141	7881

Notes: Each column shows the estimates from separate regressions. The coefficients are for the interactions of 30 day bins and a strike indicator. All regressions include cohort and bin fixed effects, as well as control variables (see notes for Table 4). The sample includes children who were born in Copenhagen in the placebo treated cohort (September 17, 2008 - April 15, 2009) and in control cohort (September 17, 2009 - April 15, 2010). Robust standard errors in parentheses.

Table A16 Robustness: Effects of strike exposure on child GP contacts, doughnut hole approach

	(1) Total GP during strike	(2) Total GP 1st year	(3) Total GP 2-4 years	(4) Regul. GP during strike	(5) Regul. GP 1st year	(6) Regul. GP 2-4 years	(7) OOH GP during strike	(8) OOH GP 1st year	(9) OOH GP 2-4 years
Days									
180-151	-0.102	0.446	0.765	-0.115	-0.181	0.224	0.013	0.628**	0.540
	(0.126)	(0.476)	(0.998)	(0.089)	(0.313)	(0.626)	(0.070)	(0.251)	(0.535)
150-121	-0.111	0.306	0.885	-0.118	-0.030	0.467	0.006	0.336	0.418
	(0.132)	(0.475)	(0.989)	(0.089)	(0.309)	(0.621)	(0.075)	(0.257)	(0.527)
120-91	-0.055	0.618	-0.192	-0.124	0.039	0.152	0.070	0.578**	-0.344
	(0.126)	(0.477)	(0.949)	(0.090)	(0.319)	(0.609)	(0.071)	(0.249)	(0.503)
90-61	0.133	1.121**	1.490	0.106	0.219	0.873	0.027	0.903***	0.616
	(0.127)	(0.496)	(0.974)	(0.092)	(0.334)	(0.632)	(0.069)	(0.257)	(0.511)
60-31	0.099	1.256**	2.111**	0.020	0.361	1.171*	0.079	0.895***	0.940*
	(0.137)	(0.513)	(1.036)	(0.102)	(0.339)	(0.670)	(0.070)	(0.274)	(0.555)
30-15	0.259*	1.201**	2.163**	0.095	0.363	1.098*	0.164**	0.838***	1.065*
	(0.140)	(0.518)	(1.046)	(0.103)	(0.339)	(0.666)	(0.075)	(0.271)	(0.548)
Control group mean	1.53	9.64	20.75	1.11	6.64	14.04	0.41	3.00	6.71
Obs.	11224	11224	10884	11224	11224	10884	11224	11224	10884

Notes: See notes to Table 2. We drop children born within 14 days before strike start. Robust standard errors in parentheses.

Table A17 The effect of strike exposure on child and mother health measured as accumulated and yearly total GP fees, Euro

	(1) Total GP fees during strike	(2) Total GP fees 1st year	(3) Total GP fees 2-4 years	(4) Total GP fees < 4y
Days				
180-151	0.173	3.118	24.031	24.215
	(3.642)	(13.981)	(29.975)	(40.086)
150-121	-1.982	5.128	58.449**	60.587
	(3.832)	(13.765)	(29.539)	(39.221)
120-91	-2.672	10.143	16.269	23.219
	(3.671)	(13.650)	(28.770)	(38.226)
90-61	3.373	11.946	76.948***	87.862**
	(3.698)	(13.979)	(29.364)	(39.046)
60-31	5.033	25.977*	77.470***	98.122**
	(4.107)	(13.801)	(29.875)	(39.632)
30-1	9.603**	46.386***	114.357***	157.834***
	(4.220)	(14.077)	(29.503)	(39.834)
MDV	63.34	339.78	736.23	1078.14
Obs.	11992	11992	11615	11615

Notes: See notes for Table 4. GP fees are measured in Euro (2015-prices). Robust standard errors in parentheses.