Beyond Treatment Exposure–The Impact of the Timing of Early Interventions on Child and Maternal Health*

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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 nurse strike in Denmark, 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 early but not later visits increases subsequent child and mother contacts to health professionals. 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 (NHV) for newborns and their families. Specifically, we ask: What is the relative importance of early vs. later nurse visits for infants' and mothers' health? 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 those visits, nurses provide 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 collective agreement 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 forgone 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 a 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. 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 find that 90 percent of home visits were canceled. Importantly, due to both capacity

[1]In Denmark, public wages are largely determined by centralized collective bargaining and the vast majority of public nurses are covered by collective agreements (Ibsen et al., 2011).

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 nurse visit, and that children born in the two weeks up to the strike on average missed two visits (given the closer spacing of early relative to later nurse visits). Depending on their date of birth relative to the strike, children had a different age at the forgone visits. We show that the strike affected families similarly across characteristics likely observed by nurses. 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 midwife contacts or hospital care at birth, were not affected by children's strike exposure.

We show that strike exposure during the initial two to three months of 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 approximately two months up to the strike, and thus likely to miss their early nurse visits, have more non-preventive care 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. 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.

Turning to maternal health, we have two main findings: 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, we find

suggestive evidence for early strike-exposed mothers being more likely to have at least one mental health-related inpatient hospital admission (but less likely to have outpatient contacts) during the first year after birth. While missing early nurse visits mechanically may result in fewer mothers being referred to check-up outpatient contacts with specialists, the finding for admissions suggests that early strike exposure leads to an increased likelihood of mothers experiencing mental health problems that require specialist attention, potentially due to forgone referrals. 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 health impacts of missing an early nurse visit, we explore two potential mechanisms: the role of information provision and mental health screenings. First, in the absence of early nurse visits, parents may lack information that is difficult to replace by less specialized health care providers, such as GPs. Information and counseling provided by nurses may impact parents' investment behaviors, such as breastfeeding, parent-child interactions or uptake of other preventive care, as well as 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 that low SES families, families with unfavorable health behaviors and children of parents without an educational background in a health or childcare-related field (nurses, midwives, doctors and childcare teachers) are more affected by early strike exposure than their respective counterparts. Thus our findings suggest that at least part of the beneficial effect of early nurse visits for children runs through an information channel. Moreover, our results for children from low socioeconomic status families suggest a role for universal nurse visits in addressing early life health inequalities. This finding is in line with other recent work from Denmark documenting the important role of nurses' information provision especially for the health of children from disadvantaged families (Altindag et al., 2021).

Second, and more descriptively, we show that nurses (i) put a strong focus on maternal mental health during early visits and (ii) routinely refer mothers with identified mental health issues to other health care professionals.[2] In the absence of early nurse visits, for the marginal mother, mental health problems thus are likely to remain unnoticed for a longer period and may contribute to longer-term adverse effects. Our results for the impact of early strike exposure on maternal mental health-related inpatient contacts are consistent 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.

Finally, analyzing the direct costs and health benefits of early nurse visits, we show that, only considering prevented GP contacts and their costs, the benefits of 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 put special emphasis on the initial period after the birth of a child.

Our work contributes to a large literature documenting causal links between childhood experiences—shocks and exposure to policies—and later life outcomes. 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 previously 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

[2]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 setting as all mothers who miss the early home visits also forgo their screening.

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. 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 evidence for a broader set of relevant outcomes, including maternal postnatal mental health.

A second contribution of our paper is its focus on a universal 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. Examples include RCT studies on the 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).[3] 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). This evidence suggests that targeted NHV can be very 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.[4] However,

[3]Also in a US context, there are a few examples for studies considering universal provision of preschool (see, for example, Cascio, 2009, 2015).

[4]Olds et al. (1986, 1998, 2002) show that high-frequency pre- and postnatal visits for at-risk mothers in the Nurse Home Visiting Partnership reduced child abuse, decreased children's emergency room visits and their

many countries offer *universal* programs and the results from studies on targeted programs do not easily generalize to settings with universal implementation. Our study provides instrumental knowledge to policy debates in many settings by analyzing the causal impacts of a *contemporary* universal program (while earlier work has mainly focused on the historical introduction of universal Scandinavian programs (Wüst, 2012; Hjort et al., 2017; Bhalotra et al., 2017; Bütikofer et al., 2019)).

Third, we explore relevant mechanisms for the causal impact of early NHV on child and maternal health: Age-specific screening offers and information. Information may matter in its own right or modify parental beliefs. Recent research documents the importance of those beliefs for child health and parental investments (see, for example, Cunha et al., 2013; Attanasio et al., 2015; Boneva and Rauh, 2018; Biroli et al., 2020). 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.

2 Background and Data

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

The Danish public health care system offers free pre- and postnatal care for all residents. Midwives and GPs provide regular prenatal care consultations.[5] Uncomplicated births are typically midwife-assisted and take place in public hospitals. Hospital births account for

criminal convictions in adolescence. Vaithianathan et al. (2016) show that targeted nurse visits reduce infant mortality and increasing vaccination rates and children's participation in early childhood education in New Zealand. Doyle et al. (2015) and Doyle (2020) study the Irish Preparing for Life-program and find positive effects on some aspects of child health (such as asthma issues) and longer-run cognitive and socio-emotional scores. Sandner et al. (2018) and Sandner (2019) document that the German "Pro Kind" program did not impact child health but reduced maternal depression. Finally, work from developing countries 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).

[5]The universal offer consists of four to seven midwife consultations, three GP consultations and two ultrasound scans (Sundhedsstyrelsen, 2007). At-risk pregnancies receive additional care.

around 98 percent of all births. After hospital discharge, the 98 municipalities provide postnatal care for infants and mothers in the NHV program. While there is variation in municipal services, the Danish National Board of Health issues guidelines and regulations regarding the number, approximate timing and specific content of nurse visits. NHV consists of a basic package of services offered to all families with a newborn and supplementary services, such as additional (need-based) home visits or group interventions (targeted at young parents or parents with specific health issues). GPs additionally provide preventive health checks and administer vaccines in the Danish vaccination program.[6]

In our analyses, we exploit variation introduced by a national strike in the Danish public health care sector in 2008. In the Danish public sector, unions and employers centrally and regularly negotiate wages. When the 2008 collective agreement negotiations remained unsuccessful for the health sector, on April 15 around 75,000 public employees went on strike. The strike lasted around eight weeks until June 14, 2008.[7]

The strike impacted various dimensions of pre- and postnatal care, including care provided by midwives, hospital nurses and home visiting nurses. 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 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 and the five Danish regions (in charge of hospitals) adjusted their provision of health care to secure the legally required level of emergency care.

[6]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 through six years (Sundhedsstyrelsen, 2007). Additionally, GPs offer one postpartum health check for mothers. The Danish vaccination program for children consists of three first-year rounds, at three, five and twelve months, respectively, and continues throughout childhood and adolescence.

[7]The strike was unprecedented large scale but did not result in spectacular results for the involved unions: It resulted in a new collective agreement, granting—from the perspective of the unions—moderate wage increases to the relevant employees, increases that were very close to pre-strike offers by the employer side (Andersen and Frederiksen, 2010).

In Copenhagen, 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 during the strike period and performed 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.

2.2 NHV in Copenhagen

Our study focuses on the municipality Copenhagen, which has around 500,000 inhabitants and sees around 8-10,000 yearly live births. There are four universally-offered visits in the Copenhagen program (see Appendix Table A1 for an overview): 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.[8] As the timing of the suggested universal visits indicates, health authorities devote more focus to the initial months of the child's life and the postpartum period. This focus reflects the preventive angle of the program: The underlying purpose is to detect potential health and developmental problems early, screen for postnatal mental health issues and to focus the program on the (frequent) transitions in these early months (with respect to issues such as infant feeding, sleep patterns, or infant development). At any time, 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 around one hour. The visits take their point of departure from a general set of main topics outlined in national guidelines. At the same time, those guidelines explicitly give nurses 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

[8] Among uncomplicated births in our sample, 58 percent of mothers are discharged with their infant on the day of birth.

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 covered in the universal nurse visits in the child's first year of life and which age-specific registrations nurses can make.

Figure 1 presents data from registrations made by Copenhagen nurses, based on a non-strike exposed sample, during or shortly after their home visits in our control group. These records are either completed at the family home (using a laptop) or at the nurse's office directly after a completed visit. We aggregate registrations into broader categories and plot for each of those domains the share of families with a recorded issue for each nurse visit, conditional on having received the visit.

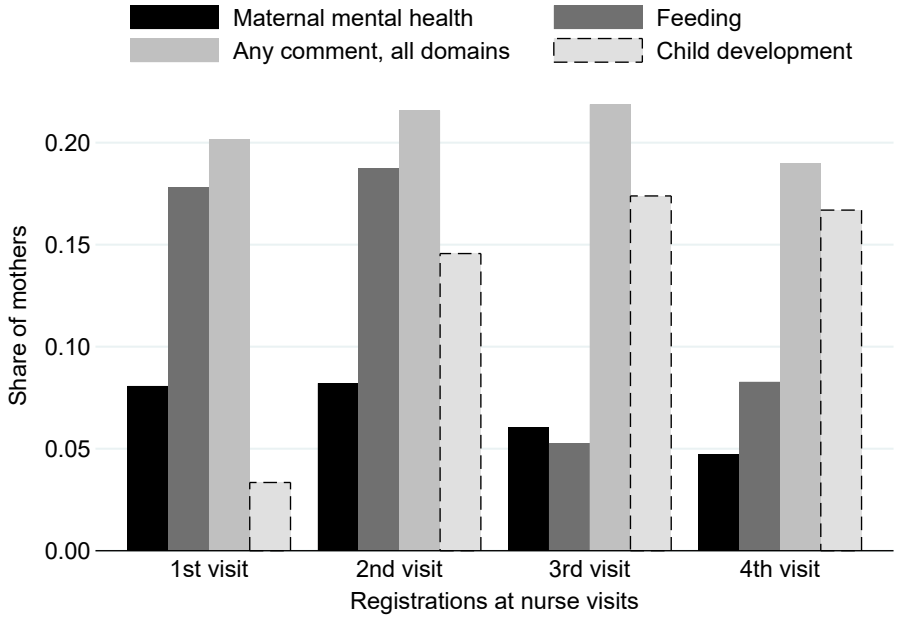

Fig. 1 Share of newborns/mothers with nurse registrations

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 14, 2009 (the control cohort). Each domain aggregates a set of binary measures. Each indicator takes the value one if the nurse registers a concern in the given domain. "Any comment" is equal to one if the nurse registers any concern across all domains.

As Figure 1 illustrates, while the first and second visits are more likely to note issues about infant feeding, the later visits are more likely to note child development issues. Issues related to mothers' mental health are often already noted in the first or second visit. Since nurses' focus on maternal mental health is especially high in the initial visits, mothers who lost access to these early visits may see negative mental health consequences due to the forgoing of screening and the lack of timely referral to a range of additional treatments.

Based on our the description of age-specific visit content and the overview on nurse registrations at those visits in non-strike years, we expect the timing of strike exposure to matter: Exposure at different ages impacts the monitoring of children's health status across different domains and advice and counseling given to parents. This latter point may both impact parents' confidence and the underlying health of children. Early (rather than later) exposed mothers forgo timely mental health screenings. In the following, we detail which measures of health we use to assess the importance of the timing of forgone nurse visits.

2.3 Data and Variable Construction

We use data from two sources. First, we access archived records on the universe of home visits, their timing and type, from the municipality of Copenhagen for the 2007-2009 period.[9] 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.[10] The administrative data contain a large set of parental background characteristics including 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, and the number of prenatal midwife contacts.

[9]The full archive of nurse records in the Copenhagen archive includes data from all visits for resident children from January 1, 2007 to December 31, 2010 (N=35,213). As we study the impact of timing of visits in the first year of the child's life, we do not consider data from the 2010 cohort, which are right-censored.

[10]In our analysis of strike exposure on child outcomes, we use an additional cohort of children in the control group (2010). Our results are not sensitive to the choice of control years, as detailed in section 4.5.

Using administrative data for the years 2007-2014, we create a set of health outcome measures: First, we examine the number of GP contacts for mother and child, respectively, from child age zero to four. We identify contacts from reimbursement data and include both in-person visits, phone contacts during regular consultation hours, and e-mail correspondence. Unfortunately, the GP data do not include a direct measure of diagnoses and for the vast majority of contacts no reason for the given consultation.

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 with the family GP, and (ii) out-of-office hours GP contacts.[11] 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.

Second, as alternative measures of child health, we consider two types of hospital contacts: Hospital inpatient admissions and outpatient contacts. Around 27 and 39 percent of children are 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, (brief) hospital contacts are not rare and, in the case of outpatient care, often related to routine check-ups. One aspect worth noting is that the 2008 strike restricted non-emergency hospital care. Therefore, GPs may have been more reluctant to refer children to hospitals during the strike period.

Third, we consider the impact of strike exposure on maternal postpartum mental health problems. These problems are interesting in their own right and as mechanisms or reinforc-

[11] 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 regular GP care) lead to very similar conclusions (Appendix Table A3).

ing 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 in the first or the second to fourth year after childbirth. To capture more severe problems, we create indicators for any maternal psychiatric hospital contact due to a set of psychiatric diagnoses during the same time periods (F01-F99 from the International Statistical Classification of Diseases and Related Health Problems (ICD)). We separately consider indicators for any inpatient and outpatient contacts. 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.

3 Empirical Methods

In our analysis, we exploit that all infants born in a period prior to April, 15 2008 experience the risk of forgoing a nurse visit, but do so at different ages. Figure 2 shows the the share of newborns not receiving a given nurse visit by date of birth relative to the strike start for the strike exposed group and the control group born in the same time period in the year after the strike. The circles show the raw daily shares and the black lines a local linear regression using a 15 day bandwidth.

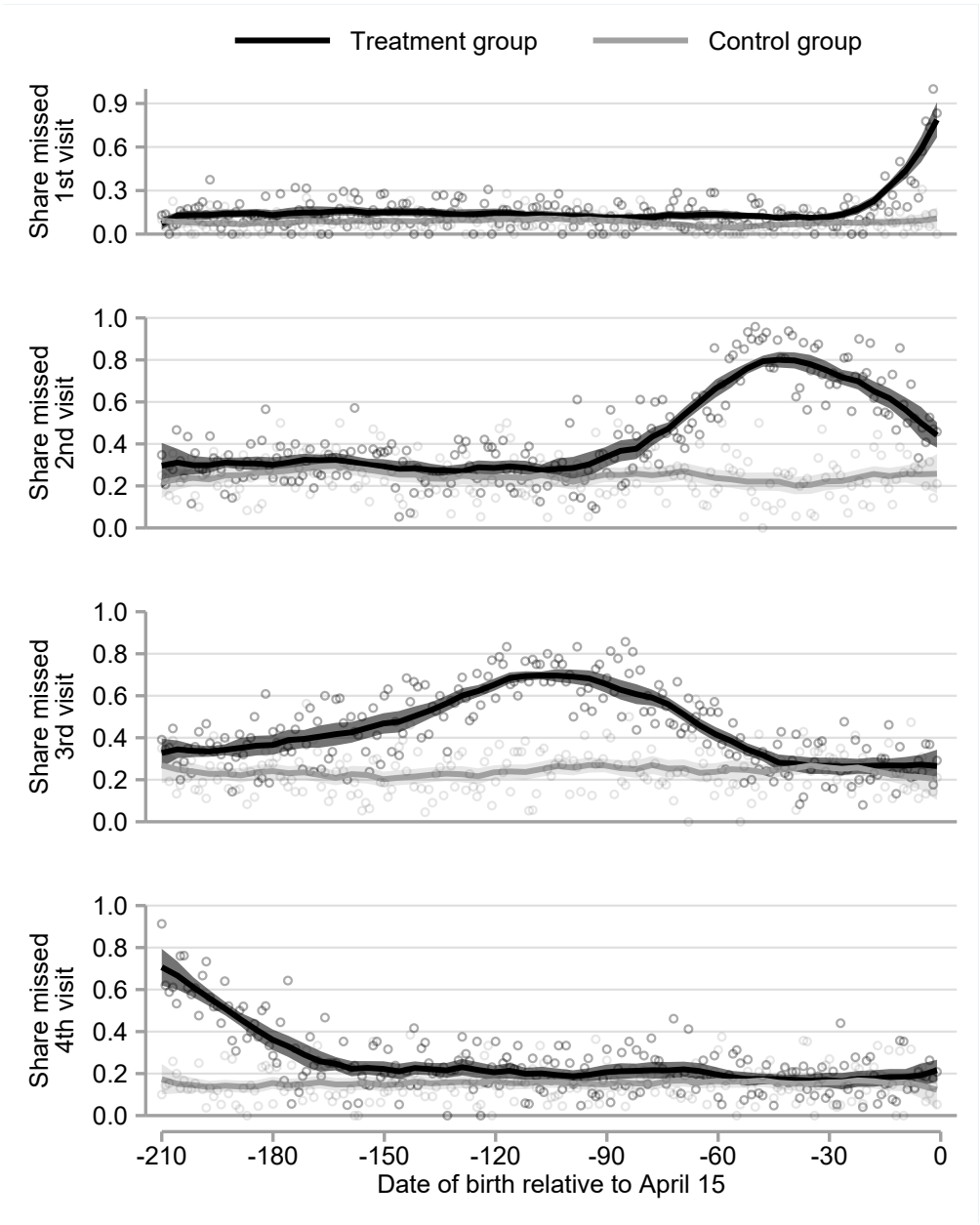

Fig. 2 Date of birth and missed nurse visits

Notes: The figure shows the share of newborns missing a nurse visit, by day of birth relative to the beginning of the strike on April 16 for the treatment and control group. The treated cohort is born between September 18, 2007 and April 14, 2008, and the control cohort is born between September 17, 2008, and April 14, 2009. The circles show the raw shares for a given day. The solid lines show local linear regressions of the raw shares using a 15 day bandwidth. The shaded area shows the 95 percent confidence interval.

In absence of the strike, the share of children missing a specific nurse visit is stable (around 10-20 percent). The first panel of Figure 2 shows that for children born more than

30 days before the strike, there is no difference in the probability of not receiving the first nurse visit across treated and control cohorts. However, in the 30 days up to the strike this difference increases sharply up to almost 90 percent. The second panel shows that the probability of missing the second visit peaks at 80 percent for those born around 45 days before strike start. The third panel shows that the risk of missing the third visit peaks at 75 percent for those born around 110 days before strike start. Finally, the last panel shows that the risk of missing the fourth visit is up to 50 percentage points higher for children born around 210 before strike start, but not notably different for children born around 170 days or closer to the strike start. 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.

Given that the "time to strike" variation in our setting impacts not one but four endogenous variables (four different nurse visits), 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, we lack power for analyses of that type. Instead we estimate the reduced form relationship between child or maternal health and the timing of birth relative to the strike start in a difference-in-differences analysis. To that end, we divide our exogenous variable—days to strike start—into a set of time-to-strike bins and use those to assess the impact of strike exposure on children and mother's outcomes. Specifically, we pursue two strategies: First, we create bins based on the empirical patterns of missed nurse visits for children born in the period up to the strike. Second, to demonstrate the robustness of our conclusions, we create somewhat more arbitrary and equally sized 30-day bins.

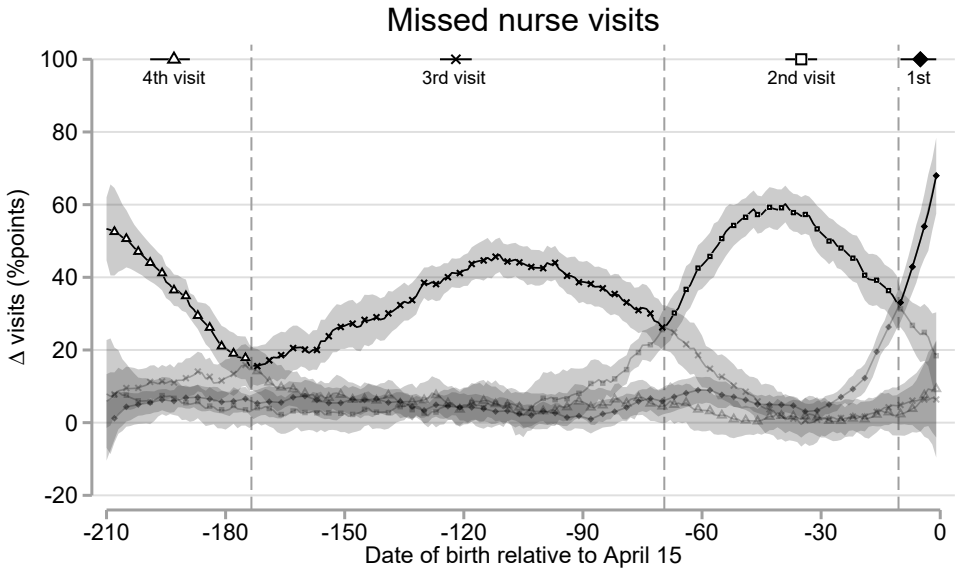

Fig. 3 Date of birth and changes in probability of missing a specific nurse visit

Notes: The figure shows the percentage point changes for the share of newborns missing a nurse visit, by day of birth relative to the beginning of the strike on April 16 comparing the treatment cohort to our control group. The treated cohort is born between September 18, 2007 and April 14, 2008, and the control cohort is born between September 17, 2008, and April 14, 2009. The relationship between date of birth and the difference in the share missing a nurse visit is estimated using local linear regressions using a 15 day bandwidth. The vertical lines show the empirically defined bins that we use in the regressions. The four bins (one for each universal visit) are defined using the crossing points of the four local linear regressions lines. The shaded area shows the 95 percent confidence interval.

Figure 3 summarizes our preferred approach: The figure plots the probability of missing the four universal nurse visits over the days to strike variable. In our regression framework, we define four bins using the crossing points of the four local linear regressions lines: The local linear regression line for missing the first visit crosses the corresponding line for missing the second visit at 10 days before strike start. Thus we define the first bin as including births between 1 and 10 days before strike start. Children born in this bin are most likely to miss the first visit and–as illustrated in Appendix Figure A1–are more likely to miss more than one nurse visit. The second bin includes children born 11 to 69 days before strike start, where the local linear regression lines for the second and third visit cross each other. The

third bin includes children born 70 days to 173 before strike start. The fourth bin (174 to 210 days before strike start) serves as our reference category.

Thus in our main empirical analyses, we estimate the following equation:

$$y_i = \alpha + \sum_{j=3}^{1} \phi_j \mathbf{1}[bin_i^j = 1] \times \mathbf{1}[Strike_i = 1] + \sum_{j=3}^{1} \beta_j \mathbf{1}[bin_i^j = 1] + \gamma' \boldsymbol{X}_i + \boldsymbol{\lambda}' \boldsymbol{C}_i + \epsilon_i \qquad (1)$$

where y_i is the outcome of interest for child i, such as GP contacts in the first year of life. 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 variables bin_i^1, bin_i^2, and bin_i^3 are indicators that equal one for children born between one and 10 days, 11 and 69 days, and 70 and 173 days prior to the strike, respectively.

In both cases, the variable $Strike_i$ equals one for children born in the 210 days up to strike start on April 16, 2008, and zero for the cohorts born in the 210 days before April 16 in the control years. The vector C_i includes separate indicators for cohorts: The 2008 cohort denotes all births from September 18, 2007 through April 14, 2008. The control cohorts are defined similarly to the 2008 cohort, namely covering children born September 17, 2008-April 14, 2009 and born September 17, 2009-April 14, 2010. Thus children born in the cohort defined prior to the strike in 2008 are treated, while children born on the same dates in 2008/2009 and 2009/2010 are untreated.[12]

We include the following covariates (X_i): paternal and maternal total income, indicators for mother and fathers highest level of education (primary school, higher education, university degree), indicators for currently studying and for being employed for both parents, an indicator for parental civil status (cohabiting, married) and indicators for missing parental covariates. All the X_i are measured one year prior to birth of the focal child. Additionally,

[12]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.

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. 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 or the exclusion of control variables from our main specification.

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. In our robustness tests, we show that our conclusions are robust to using somewhat arbitrarily defined 30-day bins instead.

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 outcomes across 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. We also 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, we estimate model (1) using parental and birth characteristics as dependent variables. Our treated and control groups are balanced across observable pre-treatment characteristics (Ap-

pendix Tables A5 and A6) and we find very few significant estimates across the groups. Moreover, this result 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 A2 plots pre-birth averages of maternal GP contacts and the share of mothers receiving a psychiatric diagnosis in any year prior to the year of their child's birth. The figures show similar trends and levels for both measures of maternal health prior to the focal 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 (see Appendix Figure A3). 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). This statement is supported by Appendix Figure A4, examining maternal employment in the year of her child's birth for treated and control children. The graph shows identical levels and identical seasonality for

the share of employed mothers in the treated and control cohorts. The seasonal pattern in maternal employment is due to the measurement of employment on a yearly basis in our data and is present for both treatment and control cohorts.[13] 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 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 A5, 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. 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 differentially for treated and control cohorts. This pattern 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 cause exclusion) does not alter our results.

[13]Given that our cohorts span two calendar years, mothers who give birth at the very beginning of the calendar year spend a larger share of the year of birth on maternity leave than mothers, who give birth late during the (previous) calendar year.

4 Results

4.1 Descriptive Statistics

Table 1 presents summary statistics for all 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 are right-censored for children born in 2010. Control children have on average 1.5, 9.7 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 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 additional home visit scheduled due to a specific need.[14] Panel B of 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 in greater detail.[15]

[14] 36 percent of children in our sample have no additional visits while 40 percent have one additional visit.

[15] To assess the representativeness of our sample of families from the capital of Denmark, Appendix Table A4 compares children and parents from Copenhagen to the general Danish population. There are expectable differences with respect to cohabitation, educational attainment and employment of parents. Children in Copenhagen resemble children from the rest of the country at birth: Five percent of children are low birth weight and seven percent are born prematurely. There are no significant differences in nights spent at the hospital after birth, prenatal midwife visits, or the rate of C-sections. At the same time, 62 percent of

Table 1 Variable means, strike exposed cohorts and control cohorts

	Strike cohort		Non-strike cohorts	
	Mean	Obs.	Mean	Obs.
A. Variables based on administrative data				
Total GP during strike	1.58	4081	1.54	8725
Total GP 1st year	9.46	4081	9.68	8725
Total GP 2-4 years	21.90	3950	20.66	8445
OOH GP during strike	0.37	4081	0.41	8725
OOH GP 1st year	2.91	4081	3.00	8725
OOH GP 2-4 years	7.29	3950	6.66	8445
Mother: OOH GP during strike	0.12	4081	0.12	8725
Mother: OOH GP 1st year	0.84	4081	0.81	8725
Mother: OOH GP 2-4 years	2.48	3950	2.29	8445
Mother: Psychiatrist psychologist during strike	0.01	4081	0.01	8725
Mother: Psychiatrist psychologist 1st year	0.03	4081	0.03	8725
Mother: Psychiatrist psychologist 2-4 years	0.09	3950	0.09	8445
Midwife visits	4.80	3970	4.75	8507
Child female	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				
Number of nurse visits	3.77	4081	4.40	4269
Number of universal visits	2.70	4081	3.28	4269
Missed 1st visit	0.19	4081	0.12	4269
Missed 2nd visit	0.44	4081	0.25	4269
Missed 3rd visit	0.44	4081	0.25	4269
Missed 4th visit	0.26	4081	0.15	4269

Notes: The sample includes all children who were born in Copenhagen in the strike period (September 18, 2007 - April 14, 2008) and in non-strike periods (September 17, 2008 and 2009 - April 14, 2009 and 2010). For the data from the nurse records (bottom panel), the control group only includes the period September 17, 2008 - April 14, 2009. OOH GP: out of office hours GP contacts

children in Copenhagen are firstborns compared to 43 percent outside Copenhagen, their parents are older and less likely to smoke.

4.2 Strike Exposure and Nurse Visits

In Table 2 we present estimates from separate regressions based on Equation (1).[16] The coefficients reflect the effect of being born in a specific bin on the probability of not receiving a nurse visit (relative to the reference bin). The columns show results for the different types of universal nurse visits, the number of universal and number of total visits (i.e., combining universal and additional nurse visits).

The regression results mirror the graphical presentation from Figure 2, indicating that the strike only has a significant impact on the initial visit for children who were born in first visit bin. On average, children in this bin are 42 percentage points more likely to miss the initial visit relative to the reference group. Children born in the first and second visit bins have an increased risk of missing the second visit. This risk is, however, much higher for children born in the second bin (between 69 and 11 days before the strike start) as they are 45 percentage points more likely to miss the second nurse visit. Children born in the third visit bin are 23 percentage point more likely to miss this specific nurse visit. As the final two columns show, children born very close to the strike start (the first visit bin) have a lower number of universal and total number of nurse visits, i.e. they are both impacted in terms of timing and number of visits. Thus for those children impacts on outcomes may both be driven by the timing and number of forgone visits. We discuss this pattern in our description of the main results for health outcomes.

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 typical patterns—suggests that the strike impacted large parts of the population. However, our unique data also allow us to characterize the children who missed

[16] Appendix Table A7 presents equivalent results using 30-day bins.

Table 2 Effects of strike exposure on the probability of missing a specific nurse visit and the number of visits (universal visits and total visits)

	Dependent variable: Missed visit					
	— Specific universal visits —				— Total visits —	
	1st	2nd	3rd	4th	Universal	Univ.+Addit.
	(1)	(2)	(3)	(4)	(5)	(6)
1st visit bin	0.418***	0.175***	-0.068	-0.315***	-0.211*	-0.760***
	(0.044)	(0.054)	(0.050)	(0.044)	(0.124)	(0.215)
2nd visit bin	0.012	0.447***	-0.046	-0.354***	-0.058	0.011
	(0.020)	(0.029)	(0.030)	(0.027)	(0.069)	(0.128)
3rd visit bin	-0.008	0.009	0.232***	-0.298***	0.064	0.100
	(0.019)	(0.027)	(0.028)	(0.026)	(0.066)	(0.121)
Mean dep. var.	0.08	0.25	0.24	0.15	3.28	4.40
Obs.	7824	7824	7824	7824	7824	7824

Notes: Each column shows estimates from separate regressions. In columns (1) to (4) the dependent variable is an indicator for missing the specific universal visit list in the column header. In column (5) the dependent variable is the number of universal visits (i.e. the sum of (1) to (4)), and in column (6) the dependent variable is the sum of universal and additional visits. The omitted category is being born between 174 & 210 days before strike start. 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 of gestation), 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 14, 2008) and in the control cohort (September 17, 2008 - April 14, 2009). The 1st visit bin covers births one to 10 days before the strike start, the 2nd visit bin covers births 11 to 69 days before the strike start, the 3rd visit bin covers births 70 to 173 days before the strike start, and births between 174 and 210 before the strike start constitute the reference category. Appendix Table A7 presents equivalent results using 30-day bins. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

nurse visits due to the strike. To do so, we have estimated separate regressions for the impact of strike exposure on the probability of missing nurse visits across subgroups of the population. In general, we find that the strike affected the considered subgroups relatively similarly–with the exception of health-educated parents, who are somewhat more likely to forgo an early nurse visit (see Appendix Table A8 for the probability of missing the first nurse visit.). Across subgroups in the population of families the mean of the dependent variable is similar as well. Moreover, a stronger impact of strike exposure on the probability of missing a nurse visit does not unambiguously covary with characteristics that may indicate positive

potential outcomes. For most 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 fully accommodate and 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 4 presents graphical evidence of the raw relationship between age at strike start and GP contacts. The vertical lines in the Figures indicate the bins used in our regression analyses. To better distinguish substitution between nurse visits and GP contacts from actual health effects, we present evidence 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.[17]

The figure reveal a clear pattern: Strike exposure in 2008 increases the number of GP contacts for the earliest strike-exposed children in our sample in each considered time period. Figure 4 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,

[17]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.

the average number of GP contacts is similar to the average for control children. The impact of missing an early nurse visit is persistent as the pattern repeats as the child ages.

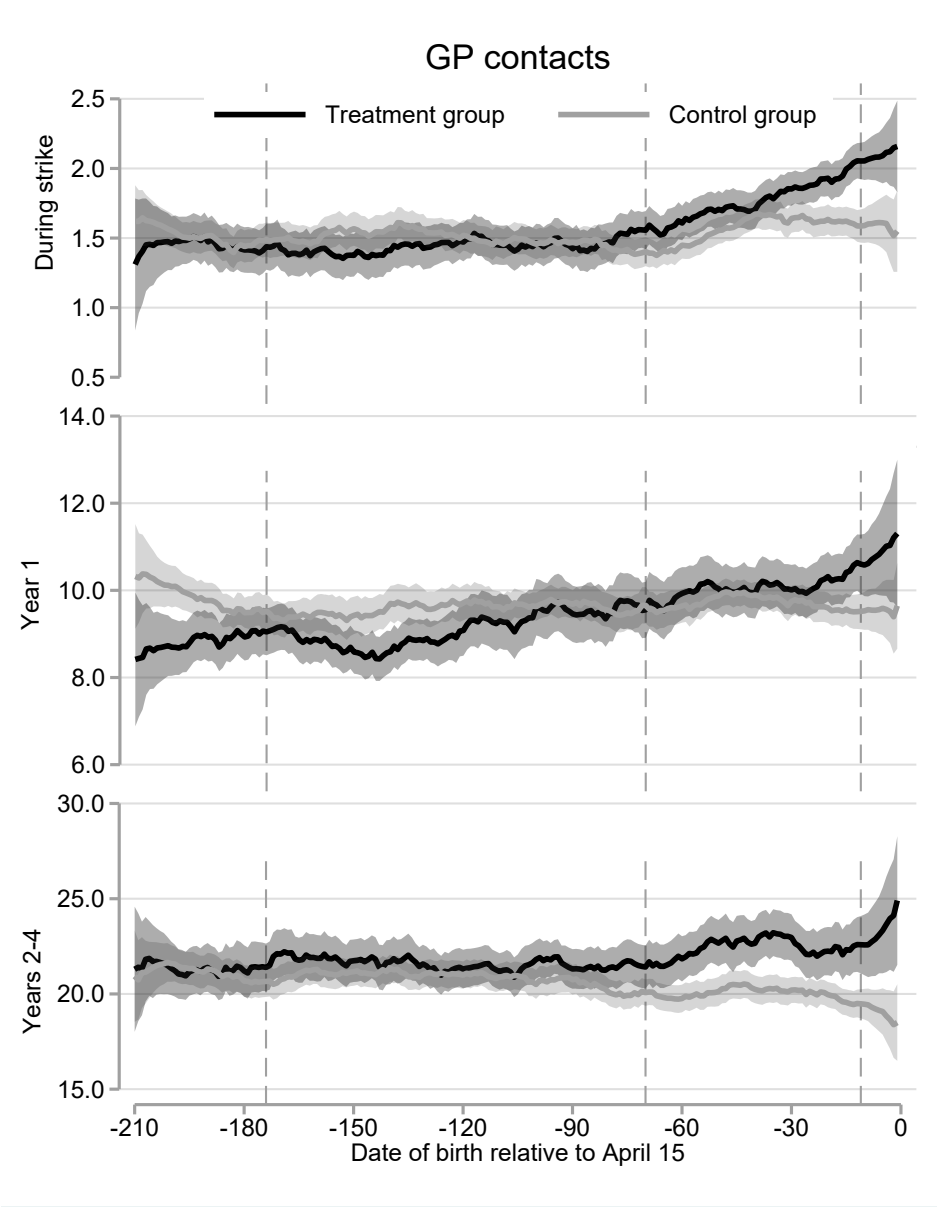

Fig. 4 Date of birth and GP contacts for children born in the treated and control cohorts

Notes: The figure shows the the average number of GP contacts by day of birth (relative to the beginning of the strike on April 16) for the treatment and control group. The treated cohort was born between September 18, 2007 and April 14, 2008, and the control cohort was born between September 17, 2008/2009, and April 14, 2009/2010. The solid lines shows the local linear regression using a 15 day bandwidth. The shaded area shows the 95 percent confidence interval.

Table 3 shows our main results for the impact of strike exposure on child health by type of GP contact.[18] During the strike period, strike exposure for children in the first visit bin increases the number of GP contacts by 0.57 contacts. This difference is driven by both an increase in regular and out-of-office hours GP contacts (not shown in the table). Thus, we find indication 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. Also children in the second visit bin have more GP contacts during the strike (0.28 visits). Recall that children in the first visit bin on average miss more nurse visits (which typically focus on similar topics given their close spacing in the initial months of the child's life). Part of the largest effect of strike exposure may be attributable to this fact.

In the first year of life, children in the first visit bin have 2.6 additional GP contacts in total corresponding to 20 and 40 percent increases for regular and out-of-office hours GP contacts, respectively. From the second to the fourth year of life children in the initial visit and the second visit bin have 4.8 (23 percent) and 2.0 (10 percent) additional total GP contacts, respectively. This increase in GP contacts from the second to the fourth year after childbirth is driven by both regular and out-of-office contacts.

To assess the impact of strike exposure at other margins, we have also considered alternative measures of child health: child hospitalizations and outpatient hospital contacts (Appendix Table A10). While most point estimates for first year hospitalizations are imprecise, we find evidence that early strike-exposed children are 10 percentage points (32 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 the probability of especially outpatient hospital contacts during strike and the first year of life for strike exposed children. However, while nurses can refer families to hospi-

[18] Appendix Table A9 presents results for our specification with equally sized 30-day bins. Results are very much in line across both specifications.

Table 3 Effects of strike exposure on child health: GP contacts by type

	Dependent variable: GP contacts						
		All		Regular		Out of hours	
	During	Year	Years	Year	Years	Year	Years
	strike	1	2-4	1	2-4	1	2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st visit bin	0.567***	2.589***	4.817***	1.354***	2.776***	1.235***	2.041***
	(0.189)	(0.685)	(1.474)	(0.463)	(0.946)	(0.353)	(0.774)
2nd visit bin	0.280***	1.260***	1.984**	0.473*	1.089**	0.786***	0.895**
	(0.104)	(0.389)	(0.781)	(0.256)	(0.506)	(0.205)	(0.412)
3rd visit bin	-0.009	0.380	0.233	0.050	0.240	0.329*	-0.006
	(0.094)	(0.353)	(0.718)	(0.233)	(0.459)	(0.186)	(0.382)
Mean dep. var.	1.54	9.68	20.66	6.68	13.99	3.00	6.66
Obs.	11992	11992	11615	11992	11615	11992	11615

Notes: Each column shows estimates from separate regressions. The omitted category is being born in the fourth visit bin. 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 of gestation), 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 14, 2008) and in the control cohorts (September 17, 2008 - April 14, 2009/2010). The 1st visit bin covers births 1 to 10 days before the strike start, the 2nd visit bin covers births 11 to 69 days before the strike start, the 3rd visit bin covers births 70 to 173 days before the strike start, and births between 174 and 210 before the strike start constitute the reference category. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

tals in case of health or feeding issues and while hospitals were obliged to ensure an adequate level of emergency care provision, during the strike this option was likely more limited (due to nurses in hospitals also being on strike). 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 strike-exposed children supports the idea of some substitution of care during the strike, from hospital care to GP care. We will return to this point when discussing the maternal mental health results.

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 maternal physical and mental well-being. Table 4 presents results for maternal total, regular

and out-of-office GP contacts. Children and their parents typically attend the same (family) GP clinic for regular consultations. We find significant differential effects of age at strike exposure for maternal out-of-hours GP contacts in the short run in the first year of life. More importantly, in the longer run (second through fourth year of the child's life), mothers who are strike-exposed shortly after childbirth have 3.9 and 2.4 additional GP contacts (17 and 10 percent increase at the relevant mean). 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 4 Effects of strike exposure on maternal health: GP contacts by type

	Dependent variable: Maternal GP contacts						
		All		Regular		Out of hours	
	During strike	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st visit bin	0.032	0.724	3.867***	0.383	3.214**	0.342**	0.652*
	(0.190)	(0.590)	(1.483)	(0.540)	(1.331)	(0.144)	(0.338)
2nd visit bin	-0.081	0.444	2.394***	0.250	1.738**	0.194**	0.655***
	(0.097)	(0.334)	(0.859)	(0.289)	(0.767)	(0.096)	(0.196)
3rd visit bin	-0.061	0.147	0.728	0.095	0.569	0.052	0.159
	(0.082)	(0.300)	(0.770)	(0.263)	(0.686)	(0.081)	(0.175)
Mean dep. var.	1.57	8.40	22.88	7.59	20.59	0.81	2.29
Obs.	11992	11992	11615	11992	11615	11992	11615

Notes: Each column shows estimates from separate regressions. For additional details, see Notes for Table 3. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Finally, Table 5 considers our available measures of maternal postpartum mental health, measured as maternal psychologist and psychiatrist contacts, psychiatric inpatient hospital contacts and psychiatric outpatient hospital contacts for our set of relevant diagnoses. Table 5 shows sizable but imprecise estimates for increased contacts with psychologists or psychiatrists for mothers who are exposed to the strike early especially in the second to fourth year after birth. Unlike our results for child and maternal GP contacts, we find that for these infrequent outcomes, bin construction impacts statistical significance across specifications. Results for equally-sized 30-day bins as presented in Appendix Table A11 suggest that es-

pecially strike exposure in the first month of the child's life impacts maternal mental health measured as contacts to psychologists and psychiatrists negatively and significantly in the first year and the second to fourth year of the child's life (higher probability of any contact in those periods). While hospitalizations are even rarer events, we find some evidence that strike-exposed mothers with a high risk of losing the first nurse visit are more likely to have an inpatient contact in the first year of the child's life. However, we find no significant impacts in our analysis with equally sized 30-day bins in Appendix Table A11. Finally, as for child outpatient contacts, we find that early strike-exposed mothers are less likely to have a first-year outpatient hospital contact due to mental health issues. This finding may point to fewer referrals by nurses to outpatient care for these mothers but as for inpatient contacts, it is not robust to alternative bin construction.

It is important to highlight that we interpret these findings as reflecting the longer-run consequences for mental health of both forgoing screening and, for the marginal mother, forgoing a timely referral to additional treatments. One factor contributing to our findings may be that forgoing the early screening and referral may lead to delay in both non-medical and medical treatment for women with postpartum mental health issues–and as a result in a higher probability of mental health issues that require treatments. A point of caution is that while point estimates for maternal contacts to mental health specialists are very large relative to the mean of the dependent variables, so are most confidence intervals leaving room for small to medium sized impacts.

In sum, our results show that early (relative to later) strike exposure has negative impacts on child physical health, maternal physical health and potentially maternal mental health. Those effects on maternal mental health may constitute a mechanism for or reinforce the health effects on children that we have documented. How do our findings and their magnitude compare to existing work? We study a universal preventive care program for an average and healthy population of families. Thus our estimated impacts are smaller than results from studies on targeted, high-intensity programs such as the Nurse Family Partnership in the U.S.

Table 5 The effect of strike exposure on maternal health: Maternal mental health-related contacts

	Dependent variable:					
	Any Psychiatrist/ Psychologist visit		Any Contact as inpatient		Any Contact as Outpatient	
	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)
1st visit bin	0.0101	0.0397	0.0022*	-0.0009	-0.0225***	-0.0159
	(0.0171)	(0.0293)	(0.0013)	(0.0049)	(0.0087)	(0.0130)
2nd visit bin	0.0098	0.0071	-0.0005	-0.0013	0.0061	-0.0046
	(0.0103)	(0.0172)	(0.0020)	(0.0045)	(0.0069)	(0.0090)
3rd visit bin	0.0049	-0.0012	0.0015	-0.0023	-0.0021	-0.0107
	(0.0093)	(0.0154)	(0.0017)	(0.0043)	(0.0059)	(0.0085)
Mean dep. var.	0.0303	0.0902	0.0022	0.0056	0.0112	0.0239
Obs.	11992	11615	11992	11615	11992	11615

Notes: Each column shows estimates from separate regressions. The measures for any inpatient and outpatient contacts refer to any contacts with the set of mental health issues specified in section 2. For additional details, see Notes for Table 3. * significance at the10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Those studies have documented large benefits across multiple domains of child and maternal health and well-being (for example Olds et al., 1999). We focus on child and maternal health and our findings of five additional child GP visits (a 23 percent increase) and four additional mother GP visits in the second to fourth year of life for the earliest strike-exposed families constitute traceable impacts. Those are well in line with other related work from Denmark on changes in universal postnatal care programs: Sievertsen and Wüst (2017) show that the introduction of hospital discharge on the day of birth for healthy infants and their mothers in the 1990-2000 period had negative impacts. It increased first month GP care (0.2 visits) and decreased ninth grade test scores for treated children (-0.08 of a SD). Infants and mothers from disadvantaged backgrounds drove these impacts. Mothers with an outpatient birth also had more GP contacts in the first three years (four additional contacts) and reported that their children were in poorer health than comparable peers at age seven. Taken together

these results show that early universal care after birth can have long-run health impacts. In the next section we explore potential mechanisms for these impacts.

4.4 Mechanisms

To explore potential mechanisms for the observed effects of early strike exposure, 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.

To assess the importance of information and counseling, we study heterogeneous effects across a number of relevant dimensions, among them 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. We split our sample and additionally estimate an interacted model on the full sample to test for the equality between the subgroups. We exclude control variables in both analyses for consistency. Our conclusions are not sensitive to the omission.

Table 6 shows our regression results for different subsamples defined by parental SES, family health behaviors (maternal smoking during pregnancy), parental health education status, and child parity. For brevity, the outcome in these regressions is the accumulated number of GP contacts for children in the first four years of life.[19] For all subgroups, early strike exposure increases the number of accumulated child GP contacts. 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 estimate is larger than in the main results.[20] Due to power issues, we cannot rule out equality of estimates

[19]Results for regular and out-of-office hours GP contacts are available in online Appendix Tables A12 and A13.

[20]Notice that all families in the sample have at least one parent on parental leave during the strike. Thus differential impact of the strike on time spent at work for health care workers should not be relevant for this analysis.

across groups, as confirmed by the presented p-value for a test of equality of coefficients across samples from a fully interacted model. However, the size 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.

Table 6 Heterogeneity: Effects of strike exposure on accumulated total child GP contacts by parental socioeconomic status, mother smoking behavior, parental health and childcare education, and parity

	Low SES		Mother smokes		Health Education		First parity	
	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st visit bin	5.481***	13.915***	5.128**	24.053***	8.078***	0.327	3.747	7.934***
	(2.051)	(4.949)	(2.022)	(8.881)	(2.160)	(4.697)	(3.205)	(2.541)
	[0.04]		[0.01]		[0.13]		[0.26]	
2nd visit bin	3.970***	1.284	2.573**	5.062	3.435***	1.294	3.314**	2.545*
	(1.197)	(2.130)	(1.108)	(4.087)	(1.139)	(2.739)	(1.556)	(1.407)
	[0.24]		[0.55]		[0.49]		[0.72]	
3rd visit bin	1.654	-1.120	0.370	2.674	0.759	0.426	-0.833	1.123
	(1.098)	(1.970)	(1.011)	(4.106)	(1.036)	(2.698)	(1.464)	(1.273)
	[0.19]		[0.56]		[0.91]		[0.32]	
Mean dep. var	29.88	31.65	30.28	33.97	30.87	27.43	26.86	32.90
Obs.	8709	3530	11316	782	10708	1687	4765	7401

Notes: See notes for Table 3. In this table, we present estimates for the interactions of the bins and a strike indicator from separate regressions for various subgroups. The outcome is total GP contacts for the child at age four. We exclude all control variables. The p-values are for a test of equality of the coefficients across groups from a fully interacted model. Robust standard errors are in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Importantly, families of low socio-economic status and families with unfavorable health behaviors as proxied by maternal smoking status during pregnancy benefit significantly more than their respective counterparts from early nurse visits.[21] This finding may indicate that nurses' early information provision and counselling can prevent later health care needs in

[21]We have also considered heterogeneity across child gender and initial health status with no significant differences across groups.

those families. These findings also speak to the importance of early universal nurse visits in addressing inequalities in child health, which are a policy concern not only in Denmark.

Another important component of early home visits is the screening for potential health problems in infants and mothers. Our results for maternal mental health in Table 4 suggest that a lack of early screening may impact 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 to explore the potential benefits of early screening. When we compare the trajectories of mothers with and without nurse registrations of mental health problems at the initial nurse visit (10 percent and 90 percent of mothers), we find that (i) nurses are more likely to register mental health problems in later visits for mothers with early detected mental health issues and (ii) mothers with early registrations of issues are more likely to be referred to other health professionals, among them psychologists/psychiatrists (see Appendix Figure A6). These descriptive patterns confirm that nurses have a strong focus on mental health screenings already in early visits, which may contribute to the prevention of more severe issues through timely referral of at-risk mothers to additional treatments.

A final and likely important potential pathway for our main results is parental investments in response to information and guidance from nurses. Unfortunately, we do not consistently observe measures of parental investments such as breastfeeding in our data. We have explored impacts of the strike on first year GP preventive care and vaccination take-up. However, high compliance of around 90 percent and our modest sample size result in imprecise and inconclusive results for these measures.

4.5 Robustness Tests

We have shown that our main conclusions are robust to our bin-construction choices. In the following, we illustrate that they are also 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. First, we show that our conclusions are not sensitive to the omission of individual-level control variables (Appendix Table A14). Second, 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 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.

In additional robustness tests, we have ruled out that including movers from Copenhagen alters our conclusions (see Appendix Table A16). By using earlier cohorts of children as an additional control group, by examining the impact of strike exposure on children already aged five during the strike/control periods, 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 (see Appendix Tables A17-A19). 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 analysis we quantify the costs of a nurse visit and relate it to the immediate costs of prevented GP visits for mothers and children. This 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 abstract from any fixed and variable costs beyond nurse salaries and assume that all types of home visits have the same average cost. We estimate the weekly number of canceled visits during the strike to be 760.[22] After the strike, the municipality of Copenhagen reported daily savings during the strike of 35,500

[22]In our nurse data we observe 85 weekly nurse visits during the strike. In the same weeks of the following year, the average number of visits was 845 ($845 - 85 = 760$).

EUR per workday or 177,500 EUR per 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, 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.[23]

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. In our calculations, we discount benefits measured in the first four years of the child's life using a 3 percent discount rate.[24] Appendix Table A20 presents results for the impact of strike exposure on accumulated GP fees (for both mother and child) rather than the number of GP visits.[25] 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 in the first and second visit bins have significantly higher GP expenses, in line with our finding of

[23](177.500 EUR $\times$ 45.8 percent)/760 visits = 107 EUR per visit

[24]Specifically, we discount benefits beyond the first year of life by a factor of $1/R^x$ for benefits x years from birth.

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

increased GP contacts. Specifically, the given groups have on average 197 (190 discounted) and 107 (104 discounted) EUR higher GP expenses accumulated at age four.

These calculations illustrate that even under conservative assumptions about the benefits of visits, the early 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 analysis highlights that early nurse visits are worth the while: our estimated 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.

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 impacts both child and maternal health trajectories. 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. These results are well in line with earlier work on reform-based changes in universal postnatal care in Denmark having negative impacts on children and mothers (Sievertsen and Wüst, 2017). While our results emphasize the importance of timing, they cannot fully disentangle timing and intensity of nurse visits, as very early strike exposed families are more likely to forgo more than one nurse visit. Given the closer spacing of visits in the initial period of the child's life, we think it is still reasonable to conclude that our results are informative about the importance of timing of early-life investment policies such as home visiting.

Our subsample of analyses by parental SES, parental health behaviors and knowledge, as well as child parity suggest that timely and early health information and counseling are important drivers for our findings. They may especially be important for parental confidence—supporting parents in making decisions on health inputs is at the core of early home visits. While we do not directly observe parental beliefs and lack measures of actual parental investment behaviors that we can use in our design, both factors may be contributing to the effects of early home visits that we find.

Indicating the importance of timely screening and referrals to treatment for mental 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 at least partly 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 and treatment 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.

One interpretation of our results is that only the earliest universal nurse visits matter for child and maternal health. Following this reasoning, future revisions of the nurse program should focus on these early home visits. Our results are too narrow to support this conclusion. 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 important topic for future research.

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A Appendix - For online publication

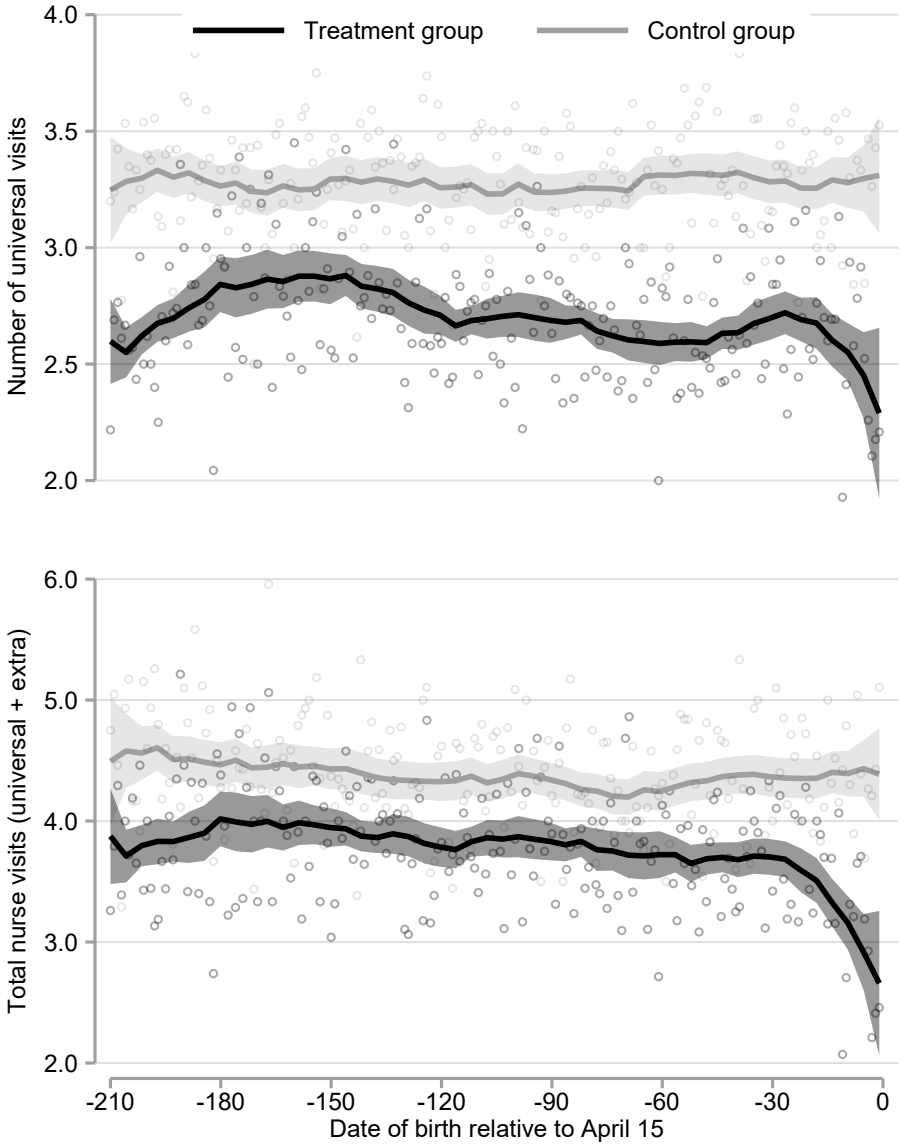

Fig. A1 Average number of universal (top panel) and total nurse visits (bottom panel) for children in the treated and control cohorts

Notes: Se notes to Figure 4. Average number of visits is calculated for children in the treated (September 18, 2007- April 14, 2008) and control cohort (September 17, 2008 - April 14, 2009).

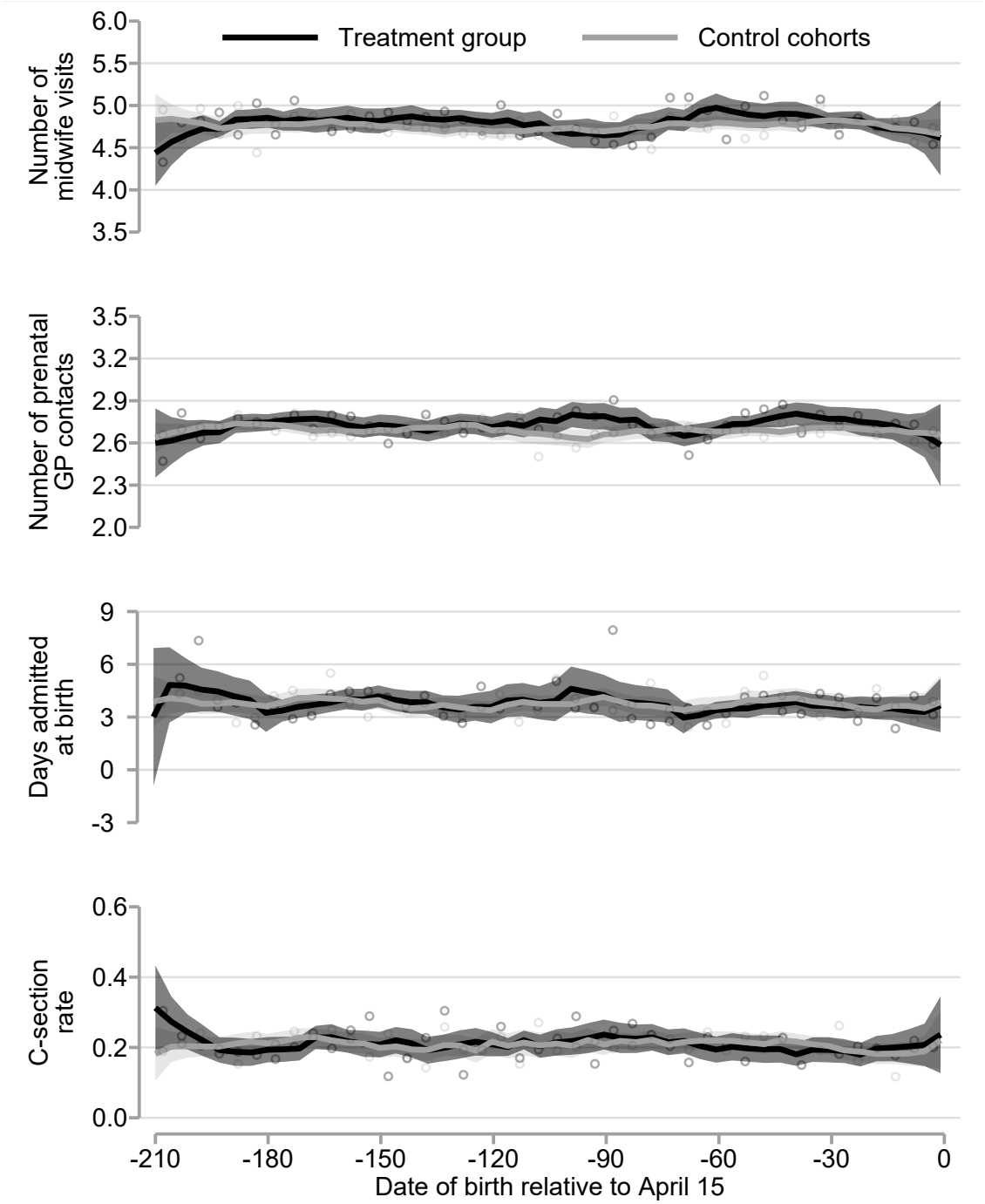

Fig. A3 Care around birth for the treated cohort and control cohorts

Notes: See notes to Figure 2. Treated cohort: September 18, 2007 - April 14, 2008. Control cohorts: September 17, 2008/2009 - April 14, 2009/2010). Due to data confidentiality reasons, we present five day averages in the scatter plots.

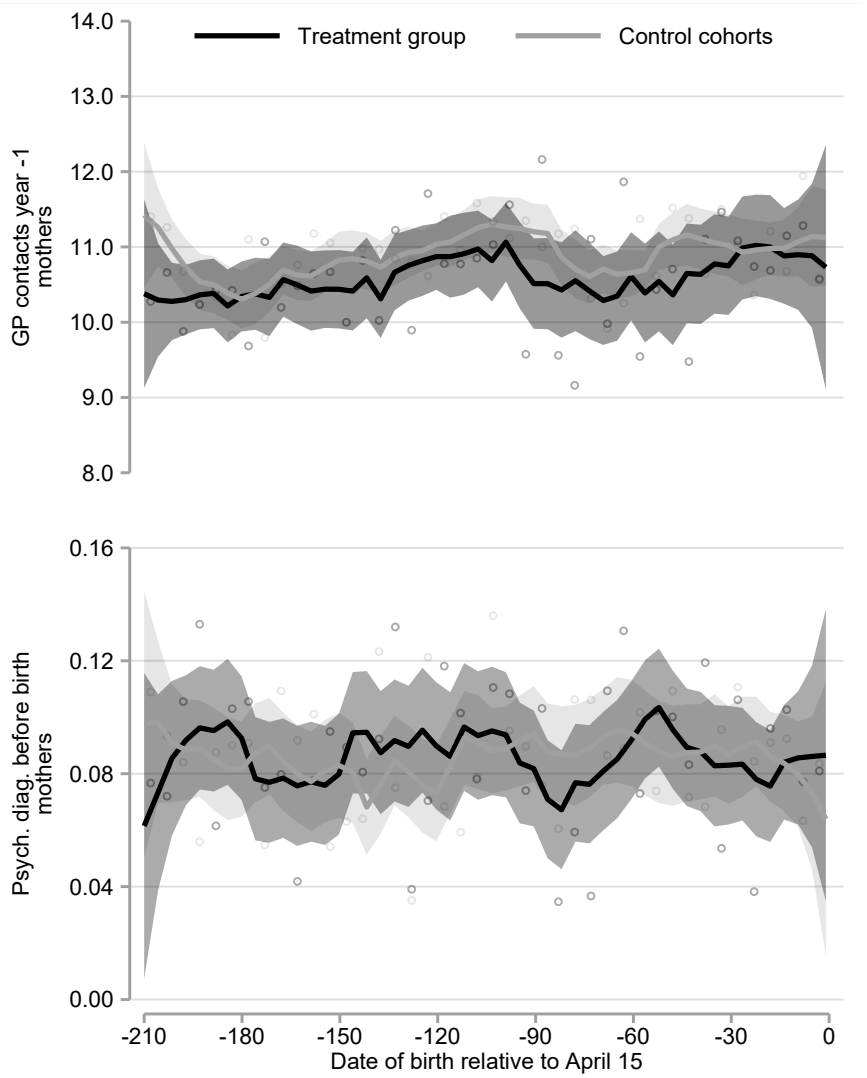

Fig. A2 Common trend in pre-treatment outcomes: (top) Number of maternal GP contacts in the year prior to birth and (bottom) Indicator for mother being diagnosed with a psychiatric diagnosis prior to the focal birth (accumulated for all pre-birth years)

Notes: Se notes to Figure 2. Treated cohorts: September 18, 2007 - April 14, 2008. Control cohort: September 17, 2008/2009 - April 14, 2009/2010). Due to data confidentiality reasons, we present five day averages in the scatter plot.

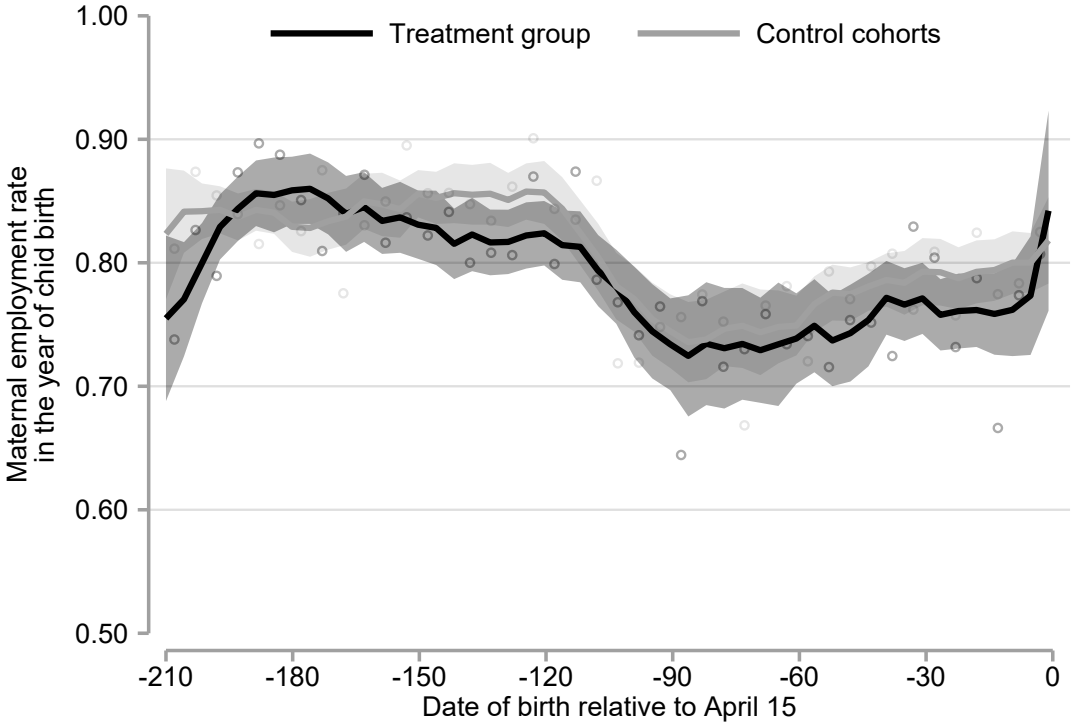

Fig. A4 Common trend in maternal employment in the year of birth

Notes: Se notes to Figure 2. The scatters show means across five day bins. Treated cohort: September 18, 2007 - April 14, 2008. Control cohorts: September 17, 2008/2009 - April 14, 2009/2010).

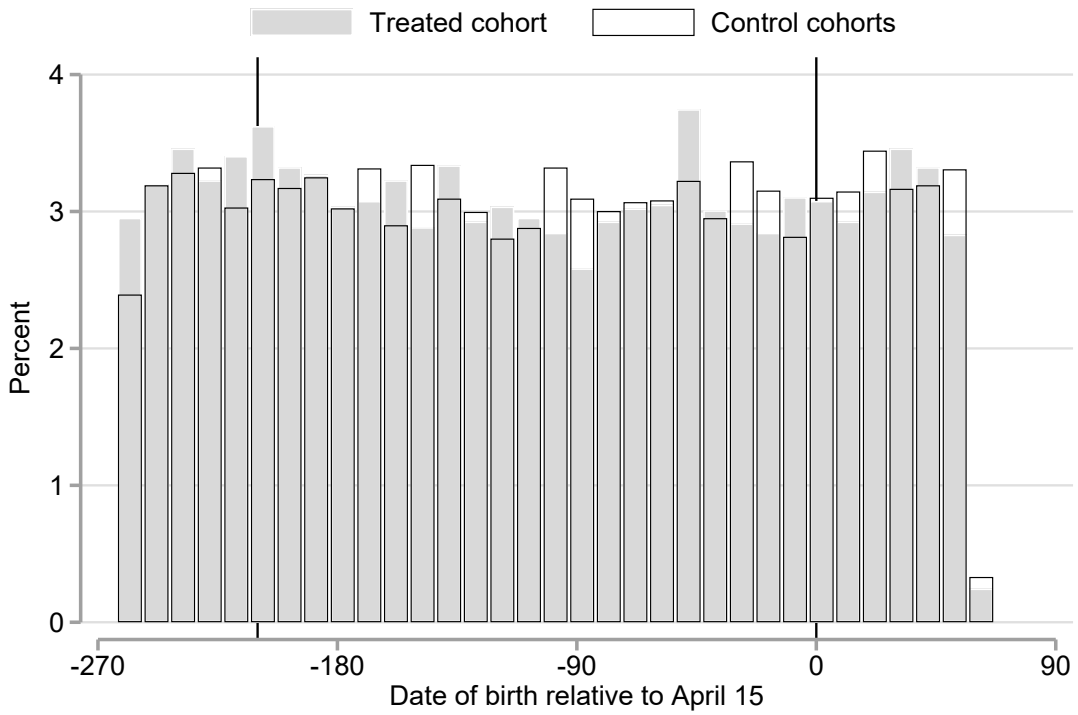

Fig. A5 Density of births

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 14, 2008 and control cohorts: September 17, 2008/2009 - April 14, 2009/2010).

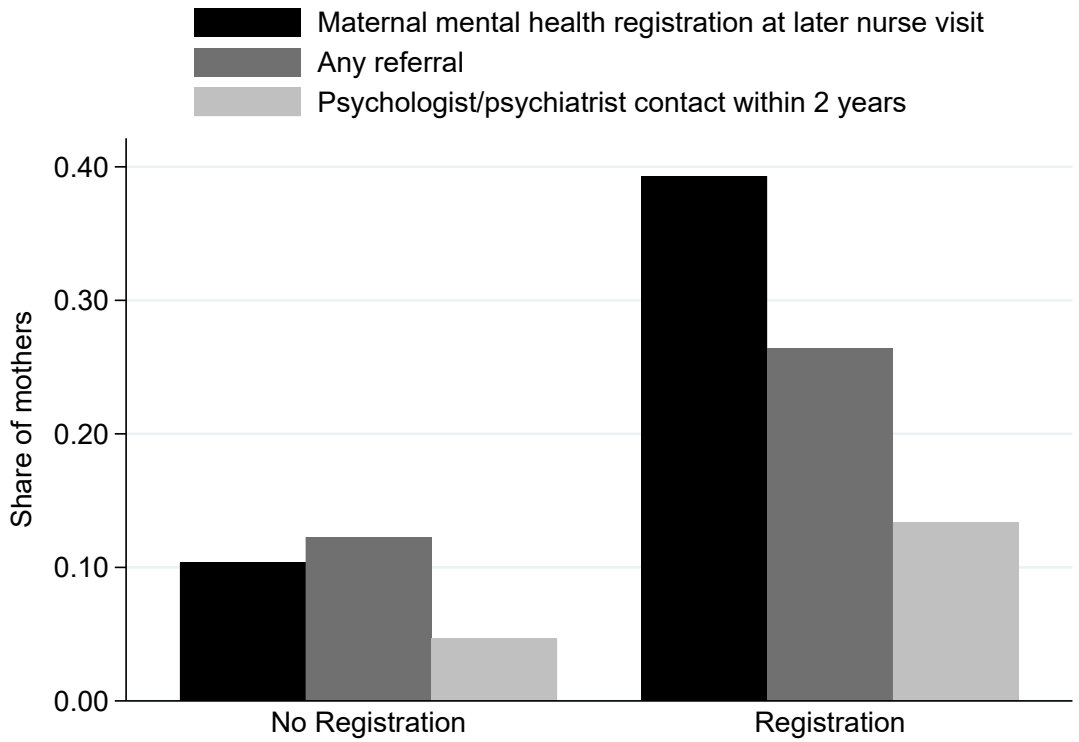

Fig. A6 Share of mothers with nurse registrations related to maternal mental health issues, referrals and contacts to psychologists/psychiatrists by registered concern (0/1) at initial nurse visit

Notes: This figure divides mothers of control cohort children (born in Copenhagen between September 17, 2008 - April 14, 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.

Table A1 Nurse home visiting in the municipality of Copenhagen

Visit (and eligibility)	Timing
Universal visits	
1st visit	0-14 days after birth
2nd visit	After two months of life
3rd visit	After four months of life
4th 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, 2 months, 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 Robustness: Effects of strike exposure on child regular GP contacts during year 2-5

	Child Regular GP contacts year 2-5
1st visit bin	3.071***
	(1.100)
2nd visit bin	1.247**
	(0.586)
3rd visit bin	0.319
	(0.531)
Mean dep. var.	16.52
Obs.	11897

Notes: See notes for Table 3. In this table, we present estimates for the interactions of the bin indicators and a strike indicator. The outcome is the number of regular GP contacts for the child during years two through five. Due to a data break in the data on out of office GP care in 2015, we constrain our main analyses to earlier years. Robust standard errors are in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A4 Variable means, population of children born in Copenhagen and Denmark.

	Denmark Excl. CPH		CPH	
	Mean	Obs.	Mean	Obs.
	Denmark Excl. CPH		CPH	
	Mean	Obs.	Mean	Obs.
Cohabitation	0.86	93824	0.77	14768
Married	0.47	93824	0.38	14768
Prim. school, mother	0.18	93824	0.13	14768
Uni. degree, mother	0.13	93824	0.31	14768
Student, mother	0.03	93824	0.05	14768
Employed, mother	0.79	93824	0.77	14768
Prim. school, father	0.18	93824	0.13	14768
Uni. degree, father	0.13	93824	0.30	14768
Student, father	0.01	93824	0.03	14768
Employed, father	0.88	93824	0.82	14768
Danish, mother	0.86	93824	0.75	14768
Danish, father	0.86	93824	0.74	14768
Young mother	0.05	93824	0.02	14768
Young father	0.02	92807	0.01	14479
Income, mother	274.82	93824	287.61	14768
Income, father	394.03	93824	391.02	14768
Length child	51.71	91201	51.65	14403
Low birth weight	0.05	91952	0.05	14543
Preterm birth	0.07	92045	0.06	14534
Head size	34.94	89985	34.77	14315
First time mothers	0.43	90560	0.62	14493
Multiple birth	0.04	93824	0.04	14768
C-section	0.22	93824	0.22	14768
No. of hosptial nights at birth, child	3.83	92191	3.86	14584
Home birth	0.01	93824	0.01	14768
Midwife visits	4.81	89571	4.76	14383
Smoking status, Mother	0.12	90016	0.07	14418
BMI mom	24.48	86360	22.91	14068
Heigth mom	167.95	87313	167.85	14176

Notes: The Copenhagen sample includes all children born in Copenhagen in the periods: September 18, 2007, 2008, 2009 - April 14, 2008, 2009, 2010. The Denmark samples includes all children born in the same periods in Denmark, excluding Copenhagen.

Table A5 Balancing Test: Parental covariates as outcome

	Prim. school, mother	Prim. school, father	Income, mother	Income, father	Cohabiting	Married	Young Mother	Young Father
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st visit bin	-0.021	-0.035	83.955	-45.102	0.018	-0.033	0.002	-0.002
	(0.033)	(0.032)	(82.565)	(126.800)	(0.039)	(0.046)	(0.014)	(0.005)
2nd visit bin	-0.010	-0.006	-5.951	-82.790	0.024	-0.034	0.014	0.004
	(0.020)	(0.019)	(9.117)	(123.840)	(0.024)	(0.026)	(0.009)	(0.005)
3rd visit bin	0.001	-0.008	3.272	-101.427	0.004	-0.016	0.003	0.002
	(0.018)	(0.018)	(8.189)	(124.314)	(0.023)	(0.024)	(0.008)	(0.005)
Obs.	12568	12568	12568	12568	12568	12568	12568	12332

Notes: Each column shows estimates from separate regressions. The omitted category is being born between 174 & 210 days before strike start. All regressions include cohort and bin fixed effects. The sample includes children born in Copenhagen in the treated cohort and in control cohorts. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A6 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)
1st visit bin	-0.133	-0.060	0.022	0.000	-0.043**	-0.020	0.130	0.019
	(0.762)	(0.137)	(0.040)	(0.002)	(0.021)	(0.020)	(0.198)	(0.049)
2nd visit bin	-0.339	0.066	0.003	-0.002	-0.025*	-0.010	0.084	0.072**
	(0.515)	(0.091)	(0.023)	(0.002)	(0.014)	(0.013)	(0.105)	(0.028)
3rd visit bin	-0.315	0.027	0.023	0.001	-0.027**	-0.021*	-0.020	0.036
	(0.497)	(0.082)	(0.022)	(0.002)	(0.013)	(0.012)	(0.096)	(0.026)
Mean dep. var.								
Obs.	12537	12409	12568	12568	12518	12515	12332	12568

Notes: See notes for Table A5.

Table A7 Robustness: Effects of strike exposure on the probability of a missed nurse visit scheduled for a specific month of the child's life and the number of visits, 30 day bins.

	— Specific universal visits —				— Total visits —	
	1st	2nd	3rd	4th	Universal	Univ.+Addit.
	(1)	(2)	(3)	(4)	(5)	(6)
Bin						
180-151	0.001	-0.041	0.099***	-0.316***	0.258***	0.220
	(0.026)	(0.037)	(0.037)	(0.034)	(0.091)	(0.166)
150-121	0.004	-0.017	0.248***	-0.353***	0.118	0.195
	(0.026)	(0.037)	(0.037)	(0.034)	(0.090)	(0.162)
120-91	-0.027	-0.015	0.362***	-0.362***	0.042	0.179
	(0.026)	(0.037)	(0.037)	(0.034)	(0.088)	(0.164)
90-61	-0.007	0.156***	0.227***	-0.347***	-0.029	0.237
	(0.025)	(0.038)	(0.038)	(0.034)	(0.088)	(0.164)
60-31	-0.005	0.512***	-0.039	-0.419***	-0.048	0.107
	(0.024)	(0.035)	(0.036)	(0.033)	(0.084)	(0.154)
30-1	0.169***	0.326***	-0.076**	-0.392***	-0.027	-0.263*
	(0.028)	(0.037)	(0.036)	(0.033)	(0.086)	(0.159)
Mean dep. var.	0.08	0.25	0.24	0.15	3.28	4.40
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 of gestation), 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 14, 2008) and in control cohort (September 17, 2008 - April 14, 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. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A8 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)
1st visit bin	0.407***	0.417***	0.389***	0.599***	0.401***	0.547***	0.356***	0.453***
	(0.062)	(0.064)	(0.048)	(0.109)	(0.046)	(0.169)	(0.075)	(0.055)
	[0.91]		[0.06]		[0.32]		[0.20]	
Ratio	0.97	1.00	0.93	1.43	0.96	1.31	0.85	1.08
Mean of dep. var.	0.09	0.07	0.08	0.08	0.08	0.11	0.10	0.07
Obs.	4071	3753	6808	1016	7233	591	3004	4820

Notes: See notes for Table 2. In this table, we present estimates for the interactions of the initial visit bin and a strike indicator from separate regressions for various subgroups along with the ratio between the full-sample estimate and the subgroup-estimates. Both sets of regressions exclude all control variables. 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 from a fully interacted model (in square brackets). The 1st visit bin covers births 1 to 10 days before the strike start. Robust standard errors are in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A9 Robustness: Effects of strike exposure on child GP contacts with equally sized 30-day bins

	Dependent variable: GP contacts						
		All		Regular		Out of hours	
	During	Year	Years	Year	Years	Year	Years
	strike	1	2-4	1	2-4	1	2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bin							
180-151	-0.101	0.472	0.752	-0.162	0.203	0.635**	0.549
	(0.126)	(0.477)	(0.998)	(0.314)	(0.627)	(0.252)	(0.535)
150-121	-0.113	0.341	0.863	0.00461	0.423	0.336	0.440
	(0.132)	(0.477)	(0.990)	(0.310)	(0.621)	(0.258)	(0.528)
120-91	-0.0520	0.643	-0.209	0.0693	0.105	0.574**	-0.314
	(0.126)	(0.479)	(0.949)	(0.320)	(0.609)	(0.249)	(0.503)
90-61	0.133	1.150**	1.471	0.252	0.824	0.899***	0.647
	(0.127)	(0.500)	(0.974)	(0.336)	(0.632)	(0.259)	(0.511)
60-31	0.157	1.110**	2.168**	0.283	1.119*	0.827***	1.049**
	(0.126)	(0.476)	(0.977)	(0.312)	(0.618)	(0.253)	(0.524)
30-1	0.358***	1.978***	2.945***	0.822**	1.641**	1.156***	1.304**
	(0.136)	(0.494)	(1.001)	(0.329)	(0.652)	(0.253)	(0.515)
Mean dep. var.	1.54	9.68	20.66	6.68	13.99	3.00	6.66
Obs.	11992	11992	11615	11992	11615	11992	11615

Notes: See notes for Appendix Table A7 and Table 3. The coefficients are for the interactions of 30-day bins and a strike cohort indicator. The omitted category is the age bin 210-181. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A10 Additional child health outcomes: Effects of strike exposure on child hospitalization and outpatient contacts

	Hospital adm. during strike (1)	Hospital adm. year 1. (2)	Hospital adm. years 2-4. (3)	Outpat. cont. during strike. (4)	Outpat. cont. year 1 (5)	Outpat. cont. years 2-4 (6)
1st visit bin	0.0036	0.0160	0.0996**	-0.0195	-0.0774	-0.0079
	(0.0296)	(0.0434)	(0.0468)	(0.0316)	(0.0473)	(0.0504)
2nd visit bin	-0.0142	0.0031	0.0654**	-0.0243*	-0.0604**	-0.0295
	(0.0119)	(0.0247)	(0.0271)	(0.0144)	(0.0282)	(0.0291)
3rd visit bin	-0.0241***	-0.0255	0.0011	-0.0067	-0.0273	-0.0356
	(0.0093)	(0.0226)	(0.0246)	(0.0124)	(0.0258)	(0.0266)
Mean dep. var.	0.0520	0.2669	0.3067	0.0730	0.3903	0.5506
Obs.	11992	11992	11615	11992	11992	11615

Notes: See notes for Table 3. Robust standard errors in parentheses. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A11 Robustness: Effects of strike exposure on maternal mental health (Probability of postnatal contacts with primary care mental health specialists, probability of mental health-related inpatient and outpatient hospital contacts related to mental health diagnoses), 30 day bins.

	Psych./Psychiatr. Contact (0/1)		Inpatient Contact (0/1)		Outpatient Contact (0/1)	
	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)
Bin						
180-151	0.0148	0.0115	0.0026	-0.0069	-0.0092	-0.0082
	(0.0127)	(0.0209)	(0.0027)	(0.0057)	(0.0080)	(0.0117)
150-121	0.0216*	0.0160	-0.0002	-0.0025	-0.0020	-0.0083
	(0.0123)	(0.0203)	(0.0021)	(0.0064)	(0.0082)	(0.0116)
120-91	0.0056	-0.0100	0.0015	-0.0059	-0.0043	-0.0224*
	(0.0116)	(0.0207)	(0.0018)	(0.0062)	(0.0084)	(0.0117)
90-61	0.0169	0.0117	0.0043	-0.0065	0.0059	-0.0116
	(0.0131)	(0.0217)	(0.0034)	(0.0053)	(0.0090)	(0.0114)
60-31	0.0128	-0.0028	-0.0007	-0.0063	-0.0017	-0.0077
	(0.0119)	(0.0205)	(0.0022)	(0.0052)	(0.0081)	(0.0110)
30-1	0.0248*	0.0411*	0.0011	-0.0011	0.0002	-0.0075
	(0.0128)	(0.0216)	(0.0029)	(0.0053)	(0.0090)	(0.0114)
Mean dep. var.	0.0303	0.0902	0.0022	0.0056	0.0112	0.0239
Obs.	11992	11615	11992	11615	11992	11615

Notes: See notes for Appendix Table A7 and Table 5. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A12 Heterogeneity: Effects of strike exposure on accumulated child regular GP contacts by parental socioeconomic status, mother smoking behavior, parental health and childcare education, and parity

	Low SES		Mother smokes		Health Education		First parity	
	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st visit bin	3.446***	6.003**	2.658**	18.016***	4.373***	0.821	2.120	4.483***
	(1.305)	(3.039)	(1.264)	(5.444)	(1.344)	(3.000)	(2.031)	(1.570)
	[0.31]		[0.00]		[0.27]		[0.30]	
2nd visit bin	2.127***	0.115	1.116	4.631*	1.655**	0.735	2.038**	1.061
	(0.771)	(1.271)	(0.693)	(2.564)	(0.708)	(1.825)	(1.001)	(0.870)
	[0.16]		[0.18]		[0.63]		[0.46]	
3rd visit bin	1.251*	-0.838	0.333	2.200	0.690	-0.065	-0.772	1.110
	(0.700)	(1.163)	(0.628)	(2.393)	(0.638)	(1.747)	(0.915)	(0.782)
	[0.11]		[0.45]		[0.67]		[0.13]	
Mean dep. var.	20.63	20.09	20.57	20.77	20.71	19.11	18.12	22.16
Obs.	8891	3648	11603	793	10969	1729	4884	7580

Notes: See notes for Table 6. In this table, we present estimates for the interactions of the bins and a strike indicator from separate regressions for various subgroups. The outcome is total GP contacts for the child at age four. We exclude all control variables. The p-values (in square brackets) are for a test of equality of the coefficients across groups from a fully interacted model. Robust standard errors are in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A13 Heterogeneity: Effects of strike exposure on accumulated child out-of-hours GP contacts by parental socioeconomic status, mother smoking behavior, parental health and childcare education, and parity

	Low SES		Mother smokes		Health Education		First parity	
	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st visit bin	2.386**	6.082***	2.450**	6.369	3.532***	0.278	1.423	3.667***
	(1.056)	(2.332)	(1.037)	(4.336)	(1.107)	(2.149)	(1.551)	(1.333)
	[0.08]		[0.28]		[0.23]		[0.25]	
2nd visit bin	1.965***	1.256	1.591***	-0.613	1.853***	0.669	1.258	1.645**
	(0.581)	(1.173)	(0.564)	(2.275)	(0.596)	(1.180)	(0.808)	(0.727)
	[0.55]		[0.31]		[0.46]		[0.73]	
3rd visit bin	0.683	0.197	0.406	-0.053	0.488	0.400	0.034	0.482
	(0.533)	(1.104)	(0.517)	(2.392)	(0.546)	(1.194)	(0.780)	(0.661)
	[0.65]		[0.82]		[0.95]		[0.67]	
Mean dep. var.	8.99	11.06	9.38	12.95	9.81	8.13	8.48	10.37
Obs.	8891	3648	11603	793	10969	1729	4884	7580

Notes: See notes for Table 6. In this table, we present estimates for the interactions of the bins and a strike indicator from separate regressions for various subgroups. The outcome is total GP contacts for the child at age four. We exclude all control variables. The p-values (in square brackets) are for a test of equality of the coefficients across groups from a fully interacted model. Robust standard errors are in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A14 Robustness: Effects of strike exposure on child GP contacts without pre-treatment covariates

	Dependent variable: GP contacts						
		All		Regular		Out of hours	
	During strike	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st visit bin	0.535***	2.301***	4.207***	1.158**	2.448***	1.144***	1.759**
	(0.189)	(0.689)	(1.476)	(0.456)	(0.930)	(0.360)	(0.778)
2nd visit bin	0.249**	1.173***	1.857**	0.427*	1.091**	0.746***	0.766*
	(0.103)	(0.386)	(0.776)	(0.252)	(0.500)	(0.205)	(0.410)
3rd visit bin	-0.014	0.293	0.148	0.001	0.242	0.291	-0.094
	(0.093)	(0.353)	(0.717)	(0.230)	(0.453)	(0.188)	(0.384)
Mean dep. var.	1.54	9.68	20.66	6.68	13.99	3.00	6.66
Obs.	12568	12568	12166	12568	12166	12568	12166

Notes: See notes for Table 3. We exclude pre-treatment covariates in this Table. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A15 Placebo test: The effect of strike exposure on child health measured as accumulated GP contacts by type, data only including the two control years 2009 and 2010

	Dependent variable: GP contacts						
		All		Regular		Out of hours	
	During strike	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st visit bin	-0.005	-0.210	-0.426	0.103	0.386	-0.313	-0.812
	(0.214)	(0.778)	(1.468)	(0.521)	(0.963)	(0.398)	(0.757)
2nd visit bin	-0.064	0.196	-0.691	0.326	-0.083	-0.130	-0.608
	(0.125)	(0.457)	(0.866)	(0.297)	(0.562)	(0.245)	(0.456)
3rd visit bin	0.036	0.306	-0.459	0.161	-0.073	0.144	-0.386
	(0.117)	(0.422)	(0.803)	(0.271)	(0.509)	(0.229)	(0.433)
Mean dep. var.	1.53	9.62	19.81	6.71	13.48	2.90	6.33
Obs.	8141	8141	7881	8141	7881	8141	7881

Notes: See notes for Table 3. The sample includes children born in Copenhagen in the control cohort (September 17, 2008 - April 14, 2009,2010). The placebo treated cohort is born between September 17, 2008 and April 14, 2009. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A16 Robustness: Effects of strike exposure on child GP contacts; including movers from CPH

	Dependent variable: GP contacts						
		All		Regular		Out of hours	
	During strike	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st visit bin	0.528***	1.977***	3.925***	1.054**	2.350***	0.923***	1.575**
	(0.180)	(0.652)	(1.391)	(0.439)	(0.891)	(0.331)	(0.732)
2nd visit bin	0.240**	0.849**	1.590**	0.373	1.060**	0.476**	0.530
	(0.096)	(0.361)	(0.737)	(0.239)	(0.476)	(0.189)	(0.390)
3rd visit bin	-0.039	0.124	-0.005	-0.004	0.325	0.127	-0.331
	(0.086)	(0.326)	(0.673)	(0.216)	(0.429)	(0.170)	(0.360)
MDV	1.52	9.61	20.76	6.64	14.08	2.98	6.68
Obs.	13820	13820	13199	13820	13199	13820	13199

Notes: See notes for Table 3. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A17 Robustness: Effects of strike exposure on child GP contacts; control cohorts including 2006

	Dependent variable: GP contacts						
		All		Regular		Out of hours	
	During strike	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st visit bin	0.557***	2.195***	4.272***	1.091**	2.418***	1.104***	1.854**
	(0.178)	(0.650)	(1.421)	(0.440)	(0.913)	(0.335)	(0.745)
2nd visit bin	0.276***	1.123***	1.667**	0.353	0.993**	0.770***	0.674*
	(0.097)	(0.366)	(0.743)	(0.242)	(0.480)	(0.191)	(0.391)
3rd visit bin	-0.019	0.373	0.025	0.015	0.076	0.358**	-0.052
	(0.088)	(0.331)	(0.681)	(0.220)	(0.436)	(0.173)	(0.360)
MDV	1.53	9.65	20.90	6.72	14.16	2.93	6.74
Obs.	15624	15624	15125	15624	15125	15624	15125

Notes: See notes for Table 3. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A18 Robustness: Effects of strike exposure on child GP contacts; results for cohorts of older children (2003-2005)

	Dependent variable: GP contacts					
	All		Regular		Out of hours	
	Year 5	Years 6-8	Year 5	Years 6-8	Year 5	Years 6-8
	(1)	(2)	(3)	(4)	(5)	(6)
1st visit bin	0.014	0.431	0.041	0.668	-0.027	-0.237
	(0.364)	(0.805)	(0.303)	(0.655)	(0.149)	(0.306)
2nd visit bin	-0.519***	0.059	-0.386***	-0.058	-0.132	0.117
	(0.190)	(0.435)	(0.148)	(0.333)	(0.086)	(0.182)
3rd visit bin	-0.046	-0.409	-0.073	-0.229	0.026	-0.180
	(0.177)	(0.393)	(0.138)	(0.301)	(0.079)	(0.165)
MDV	3.81	9.34	2.82	7.20	0.99	2.15
Obs.	12887	12612	12887	12612	12887	12612

Notes: See notes for Table 3. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A19 Robustness: Effects of strike exposure on child GP contacts; results for first stage sample (cohorts 2008/2009)

	Dependent variable: GP contacts						
		All		Regular		Out of hours	
	During strike	Year 1	Years 2-4	Year 1	Years 2-4	Year 1	Years 2-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st visit bin	0.575***	2.726***	5.019***	1.312**	2.582**	1.414***	2.437***
	(0.221)	(0.791)	(1.683)	(0.526)	(1.083)	(0.415)	(0.889)
2nd visit bin	0.314***	1.158***	2.314**	0.304	1.119*	0.855***	1.196**
	(0.118)	(0.445)	(0.905)	(0.293)	(0.585)	(0.240)	(0.477)
3rd visit bin	-0.027	0.231	0.450	-0.028	0.282	0.259	0.168
	(0.108)	(0.404)	(0.832)	(0.264)	(0.530)	(0.220)	(0.446)
MDV	1.54	9.74	21.54	6.64	14.52	3.10	7.02
Obs.	7824	7824	7579	7824	7579	7824	7579

Notes: See notes for Table 3. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level

Table A20 The effect of strike exposure on subsequent combined mother and child GP expenses (Euro)

	Dependent variable: GP expenses				
	During strike	Year 1	Years 2-4	Years 0-4 No discounting	Years 0-4 Discounting
	(1)	(2)	(3)	(4)	(5)
1st visit bin	11.6715*	49.4244***	146.9380***	196.9282***	190.0815***
	(6.3135)	(18.8399)	(39.8701)	(53.6126)	(51.6289)
2nd visit bin	7.0340**	32.8376***	79.8212***	107.3265***	103.9109***
	(3.2361)	(11.2760)	(23.9681)	(32.0741)	(30.8490)
3rd visit bin	-0.8013	3.0347	27.7118	27.3815	26.1035
	(2.6803)	(10.1911)	(21.7072)	(28.8833)	(27.7706)
Mean dep. var.	63.3417	341.4027	736.9238	1080.4793	1041.5153
Obs.	11992	11992	11615	11615	11615

Notes: See notes for Table 3. GP fees (combined for mother and child) are measured in Euro (2015-prices). The final column presents results for GP fees over the first four years of the child's life where yearly fees are discounted with a discount rate at 3 percent. Robust standard errors in parentheses. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level