Social Science & Medicine 150 (2016) 201–211

Contents lists available at ScienceDirect

Social Science & Medicine

journal homepage: www.elsevier.com/locate/socscimed

Care around birth, infant and mother health and maternal health investments – Evidence from a nurse strike

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ARTICLE INFO

Article history:
Received 30 January 2015
Received in revised form
8 December 2015
Accepted 23 December 2015
Available online 29 December 2015

Keywords:
Denmark
Care around birth
Health policy
Child health
Parental investments
Breastfeeding duration
Nurse strike
Difference-in-differences

ABSTRACT

Care around birth may impact child and mother health and parental health investments. We exploit the 2008 national strike among Danish nurses to identify the effects of care around birth on infant and mother health (proxied by health care usage) and maternal investments in the health of their newborns. We use administrative data from the population register on 39,810 Danish births in the years 2007–2010 and complementary survey and municipal administrative data on 8288 births in the years 2007–2009 in a differences-in-differences framework. We show that the strike reduced the number of mothers' prenatal midwife consultations, their length of hospital stay at birth, and the number of home visits by trained nurses after hospital discharge. We find that this reduction in care around birth increased the number of child and mother general practitioner (GP) contacts in the first month. As we do not find strong effects of strike exposure on infant and mother GP contacts in the longer run, this result suggests that parents substitute one type of care for another. While we lack power to identify the effects of care around birth on hospital readmissions and diagnoses, our results for maternal health investments indicate that strike-exposed mothers—especially those who lacked postnatal early home visits—are less likely to exclusively breastfeed their child at four months. Thus reduced care around birth may have persistent effects on treated children through its impact on parental investments.

1. Introduction

Evidence on the effect of care around birth on mothers' and children's health is important for at least two reasons. First, universally accessible care around birth may play an instrumental role for promoting population health and preventing downstream costs to the health system. These costs can arise from untreated conditions that result in serious health issues and, consequently, the uptake of care from more specialized health care providers. Second, a central objective of care around birth is to promote parental health investments, such as breastfeeding. Given a recent emphasis on the importance of early investments for short- and longer-run child outcomes (for an overview, for example, Almond and Currie, 2011), knowledge on the impact of care around birth on parental investment decisions is important for policy.

This paper exploits a national strike among all Danish nurses in spring 2008 to identify the effect of care around birth for a low-risk population of mothers and infants on mother and child health and maternal health investments. Care around birth consists of midwife consultations, a postpartum hospital stay, and home visits by trained nurses. The strike impacted all three types of preventive and non-emergency nurse services, which were available on a lower level during the strike period. We exploit this supply-side shock to circumvent the endogeneity problems that flaw analyses of the effect of care around birth, i.e., we account for selection of mothers and children into more intensive care based on characteristics that are unobserved to the researcher.

Using population data on Danish births from 2007 to 2010, we show that the 2008 strike caused clear departures from overall trends in care provision. By focusing on mothers locally around the 2008 strike period, we furthermore illustrate that the *timing of birth relative to the strike* resulted in different degrees of treatment intensity (see Fig. 1 for an illustration): As an example, mothers who gave birth during the first days of the strike received all midwife consultations but were discharged from hospital early and did not receive early home visits. Similarly, mothers who gave birth towards the end of the two-months strike did not receive all regular

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http://dx.doi.org/10.1016/j.socscimed.2015.12.034
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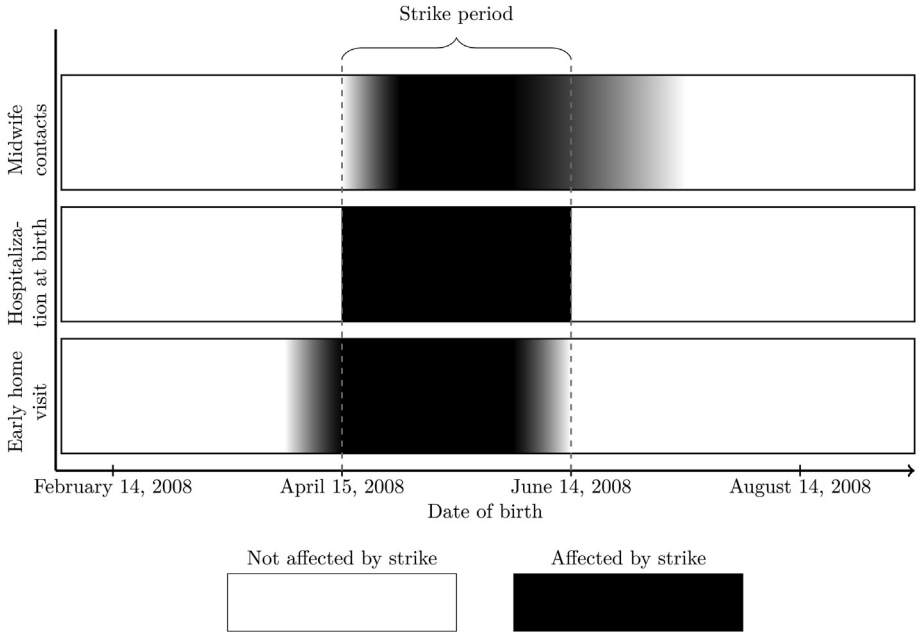


Fig. 1. Treatment exposure by date of birth.

midwife contacts, were discharged early, but received early home visits after discharge (as the strike was finished by the time they were discharged). These observations justify our identification strategy: We compare the differences in outcomes for children and mothers in a set of periods defined relative to the 2008 strike, to the same differences in outcomes of mothers and children in the same periods in 2009 (difference-in-differences framework).

Children and mothers who were impacted by the strike have more general practitioner (GP) contacts in the first month of the child's life. This increase in health care usage may indicate both, underlying health problems of treated mothers and children and substitution of one health care service with another in the setting of universal health insurance in Denmark (i.e., a setting were GP visits are free of charge.) Exploiting the variation in treatment intensity, we show that especially for children, this increase of GP visits appears to be driven by a lack of early home visits by trained nurses, who usually monitor children's health and advise parents on infant care. We find no strong persistent effects on longer-run health care usage (GP contacts). Moreover, we find no effects on hospital readmissions or a set of relevant diagnoses (child nutritional problems, postpartum maternal complications). Unfortunately, these results are imprecise, likely due to power issues. Additionally, nurses managed to keep up a minimum level of care, and we do not observe which women were exposed to lower levels of care because of the strike, i.e., we present intent-to-treat effects of strike exposure.

Taken together, our findings suggest that the initial GP effect may mainly reflect substitution of one type of care with another in the general population of mothers and children without health risks and this substitution may have been instrumental in preventing longer-run health problems for strike-exposed mothers and children. However, turning to an analysis of the impact of the strike on maternal investment decisions, we find indication for the strike impacting mothers' probability of breastfeeding exclusively for at least four months. This finding gives further credibility to earlier studies—based on smaller samples and less comprehensive data—on the impact of the 2008 strike (Kronborg et al., 2012). Furthermore, because we find effects on breastfeeding, an issue GPs typically do not provide counseling on, our results point to the importance of the content of postnatal care visits. Thus reduced care and topic-specific guidance around birth may have potential longer-run consequences on children through their effect on parental investments.

Our findings contribute to the relatively small literature on the short-run effects of care around birth. A number of studies has examined the benefits of highly specialized care, such as prenatal care for at risk-populations (Joyce, 1999; Abrevaya and Dahl, 2008), neonatal medical interventions (Almond et al., 2010; Bharadwaj et al., 2013; Freedman, 2012) or targeted home visits by health professionals (Olds et al., 1997, 1998; Eckenrode et al., 2010; Gertler et al., 2013). Most papers suggest important positive effects of these targeted interventions.

However, our understanding of the returns to care that is provided to low-risk mothers and infants, is still limited. While some studies on the impact of care around birth for a general population of mothers find very limited or no health effects (Fiscella, 1995; Evans and Lien, 2005; Currie and MacLeod, 2008; Almond and Doyle, 2011), two recent studies from Denmark and the Netherlands suggest that there may be important short- and longer run effects in these universal health care settings: Daysal et al. (2015) show that hospital (vs home births) for complying, low-risk mothers and their children has important infant mortality benefits in the Netherlands. Additionally, recent evidence from Denmark demonstrates negative longer-run consequences of mandated shorter postpartum hospital stay (for all multiparous mothers) on health and schooling outcomes of children (Sievertsen and Wüst, 2014). Importantly, Sievertsen and Wüst (2014) show that both a direct health channel and a parental response channel account for longer-run negative impacts of early discharge on child outcomes: At risk-mothers who are discharged early are less likely to breastfeed exclusively for four months and their children are less likely to receive all scheduled vaccines. Our finding of shorter breastfeeding durations for strike-exposed mothers confirms this earlier finding that early postnatal care impacts parental investments.

Moreover, while studies exploiting historical records on universal home visiting in Denmark have demonstrated positive short- and long-run health benefits of the program (Hjort et al., 2014; Wüst, 2012), we know little about the causal effects of contemporary, universal home visiting programs. Guldager (1992) has

compared families across areas in Copenhagen with ordinary services by home visiting nurses and with nurse shortages in 1980. The study finds a positive impact of universal home visiting on parent-reported measures of self-confidence and investments. While the design of this study is promising, concerns about the validity of the comparison across areas remain. Furthermore, the outcomes studied were mainly collected with a retrospective survey among a small sample of parents.

Given its large impact on health care spending in many countries, analyses of the health effects of care for a general population of mothers and infants are highly relevant. Our study extends existing work by focusing on changes in the pre- and postnatal care package that may be of particular importance for parental investment behaviors. The article is organized as follows. Section 2 contains relevant background information on the Danish health care system and the 2008 strike. Section 3 describes our data and Section 4 presents our empirical strategy. Section 5 presents results and robustness checks. Section 6 concludes.

2. Background: care around birth in Denmark and the 2008 strike

In Denmark, the timing and content of prenatal care, and all matters regarding hospital and home births are centrally regulated by the Danish National Board of Health (DNBH). The DNBH also issues regulations for the home visiting program, which is implemented by the Danish municipalities. All pregnant women have access to free public pre- and postnatal care, including birth at a public hospital. There are no private birth clinics and only very few private suppliers of pre- and postnatal care services, such as private midwife clinics. Those clinics account for a negligible share of services provided to pregnant women and new mothers.

Prenatal care consists of services provided by midwives and GPs, who monitor pregnant women's health and the progress of the pregnancy. The timing of midwife consultations during pregnancy puts special emphasis on the second and third trimester (The Danish National Board of Health [Sundhedsstyrelsen], 1998). In 2008, all pregnant women were offered three scheduled GP visits (in weeks 6–10, 24, and 35), seven midwife consultations (around weeks 12, 16, 20, 28, 32, 38, and 40), and two ultrasound examinations (around weeks 12 and 20) (The Danish National Board of Health [Sundhedsstyrelsen], 1998, 2009). At-risk groups of mothers received additional interventions. No changes to the recommended prenatal care occurred in the period 2007–2009.

All hospital births are performed under the supervision of midwives (98 percent of children are born in hospitals). Physicians only intervene in the case of complications. After birth and discharge from hospital, trained municipal home visiting nurses monitor infants' health and development, promote child–parent interactions, and encourage and support breastfeeding. GPs monitor infants' and mothers' health, conduct vaccinations, and refer infants and mothers to other health professionals, if necessary. In 2008, women and infants were entitled to around seven home visits (less strictly enforced for higher parity mothers) and three scheduled child GP (and one to two scheduled mother GP) visits in the first year of the infant's life (The Danish National Board of Health [Sundhedsstyrelsen], 1995, 2007).

During the collective agreement negotiations in 2008, the health professionals' union had asked for a 15% wage increase and—in the light of a segregated Danish labor market—the establishment of a commission on the gender wage gap. When employers and trade unions did not reach an agreement, on April 15, 2008, approximately 75.000 publicly-employed midwives, nurses and home visiting nurses went on strike. The strike lasted until June 14, 2008 and cost the health trade unions 650 million DKK (about 90 million Euro) (Stamhus et al., 2009). It resulted in a collective agreement granting a 13.3% wage increase over three years to nurses and other health professionals.

As only a minimum number of nursing staff securing emergency care was on duty, during the two-month strike period 372,516 operations were canceled (Andersen and Frederiksen, 2010). Moreover, routine procedures such as pre- and postnatal care as well as hospitalizations in general were impacted, exposing most women and infants in the low-risk pregnancy-group to lower levels of care. During the period until June 14, 2008 the strike additionally led to major shortages in the municipal home visiting programs.

3. Data and graphical analysis

We use data from three sources. Our study and all its data sources were registered at the Danish Data Protection Agency (Datatilsynet). In Denmark, no ethical review for observational studies using administrative register data or survey data is required or offered. First, we use population administrative data that covers all births in Denmark 2007–2010. The administrative data contain information on the date of birth, length of hospitalization at birth, contacts to health professionals (GPs, midwives), medical diagnoses given at hospitals, and an encompassing set of mother and infant background characteristics. These characteristics are observed by medical professionals and most likely impact the allocation of care both during the strike and under the default care regime. We use an indicator for child sex, the mother's and father's years of education, indicators for mother's and father's unemployment status, mother's and father's taxable income, and indicators for missing covariates for mother or father (see also Appendix Table A.1 in the Online Appendix). We do not control for other child birth outcomes, that we observe, because these outcomes are potentially impacted by the strike and its impact on prenatal care.

As the national administrative data does not contain information on the timing and intensity of the municipal home visiting programs, we use complementary data from two sources. The Child Health Database (CHD) for the period 2007–2009 is municipal administrative data. It contains data collected by nurses on all infants from 13 municipalities on Zealand and one municipality in Jutland. Home visiting nurses register information on the municipality of residence, the child's date of birth, frequency and timing of visits, child development, and mothers' breastfeeding behavior. Unfortunately, we cannot link the CHD data to the national register data and thus we do not use controls from the administrative data in the analyses based on CHD data.

Finally, we add data from a survey on home visiting and breastfeeding behavior from the region of Central Jutland. The survey was performed among women who had given birth during the strike period and a control period in fall 2008 (for details see Kronborg et al., 2012). The survey contains information on home visiting nurses' visits and mother-reported breastfeeding measures.

To compare the characteristics of parents and children in our main data source and the two complementary data sources, Appendix Table A.2 in the Online Appendix shows means for relevant variables drawn from the administrative register data for the full population, all families in the areas covered by the CHD, and all families in the areas covered by the survey data (for families who have a child 56 days before and during the strike in 2008). As the table illustrates, there are some differences between the full population data, the urban areas around Copenhagen (covered in the CHD) and the rural area in Jutland covered in the survey. While parents in the area around Copenhagen are somewhat older and of higher income as the country average, the opposite holds for parents in the survey region. However, with respect to other factors,

such as maternal education and both parents' unemployment, the table does not suggest large differences in the samples. Also measures for the health at birth of the children born in the different areas do not indicate major differences. Moreover, given that the two data sources represent rather different areas of Denmark (with respect to urbanity, geographic location and a subset of parental characteristics), we believe that they are a meaningful addition to our population data used in the main analyses.

3.1. Outcome measures

Our primary outcome measures come from the administrative data: First, we measure the number of GP contacts for mothers and infants in both the first 28 days and the first 1–3 years of the child's life. We also compute the total amount of fee-for-service payments to GPs for each child/mother in the given periods. Second, we examine mother and infant readmissions to hospitals (in the first month and in the first year of the child's life). Third, we examine a set of diagnoses given to children that are in contact with a hospital during the first month of life (admitted or outpatient) and that related to nutritional problems, such as jaundice. Fourth, we construct a measure of relatively frequent mother post-birth complications and operations within three months after birth (Danish National Board of Health, 2005). For details on the diagnoses and complications measures consult Appendix A.1 in the Online Appendix. To further test whether the strike led physicians and nurses to manipulate the timing of births and change the type of care they provided during labor, we examine whether the weeks around the strike impacted the rates of Caesarean sections (CS), among them the unplanned ones. While planned CS are usually scheduled due to complications during the pregnancy, unplanned CS signal problems during labor. An increase in unplanned CS may also signal that the quality of hospital births themselves (and not only pre- and postnatal care) was impacted during the strike. In Denmark, CS solely due to maternal request (and no medical indication) are not common practice (Danish National Board of Health, 2005).

To examine the impact of the strike on parental investments, we exploit the complementary data and study breastfeeding duration. We study breastfeeding because (1) the postnatal (and prenatal) care program explicitly aims at promoting breastfeeding and (2) breastfeeding duration is reported in our data with few missing values and nurses focus on the correct report of this measure. One important limitation of our study is that we lack other interesting measures of parental investments such as parent–child interactions, which are only reported in selected years in the CHD and therefore unfortunately cannot be studied here. However, we believe that while the size and economic significance of longer-run breastfeeding effects are still debated in international expert and practitioner arenas (Fletcher, 2011; Colen and Ramey, 2014; American Academy of Pediatrics, 2012; Kramer et al., 2001, 2008; Der et al., 2006; Del Bono and Rabe, 2012; Sievertsen and Wüst, 2014), breastfeeding duration is a reliable and relevant measure for early parental investments. Given high and stable breastfeeding rates in Denmark and the resulting sample size requirements (or effect size requirements) for detecting an effect of strike-exposure on breastfeeding rates, we pool the two available data sources (CHD and survey data) and create an outcome variable that takes the value one if a mother breastfed exclusively for four months.

Table A.1 presents summary statistics for a sample of birth 56 days before and the 58 days during the 2008 strike. The table shows that there are no significant differences in maternal, paternal and child characteristics across periods. In the following analyses we focus in greater detail on the impact of the timing of strike exposure.

3.2. Graphical analysis

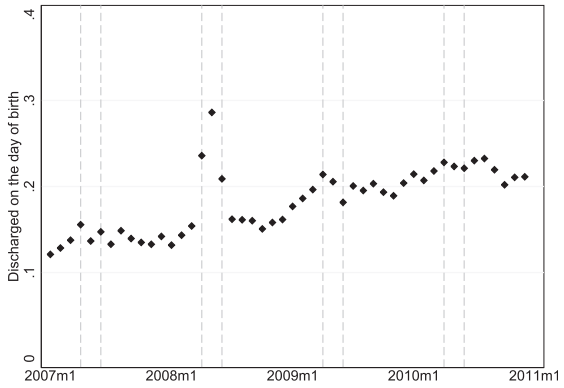
Fig. 2 shows the changes in care levels in and around the strike and the clear departure from the longer-run trends in the years 2007–2010. All figures plot monthly averages. The vertical lines mark the strike months and the respective months in 2007, 2008, 2009 and 2010. The top panel in Fig. 2 is based on administrative data and plots averages for the number of midwife consultations and the percentage of mothers discharged from hospital on the day of birth. Given the higher intensity (more consultations) of midwife care during the third trimester, the decrease in the number of midwife consultations persists for women giving birth during the first months after the strike (as these women were pregnant during the strike and cannot catch up with respect to the total number of midwife visits). For early discharge rates, we see a clear upwards jump on the day the strike started and a clear downwards jump the on day the strike ended. The bottom panel of Fig. 2 uses data from the Child Health Database (CHD) and shows the percentage of mothers with an initial home visit by a home visiting nurse in 13 Danish municipalities. The figure shows a clear trend break in the probability of receiving an early home visit for mothers who give birth during the strike. Appendix Figure A.1 in the Online Appendix, also based on the CHD, shows that the average age of children at the first home visit for children born shortly before and during the strike was higher. At the same time, at one year, there were only small differences in the average number of home visits in the first year of the child's life across periods.

Although the CHD does not cover all Danish municipalities, we show that our findings for the reduced availability of home visiting can be generalized to all Danish municipalities. Besides anecdotal evidence, we rely on data from a 2008 survey among mothers who gave birth in the region of central Jutland during the strike (April 15–June 14) and a control period (August 1–October 31) (Kronborg et al., 2012). In these data we show that the same pattern (lower home visiting availability) was present also in other municipalities, not covered by the CHD (Appendix Figure A.8 in the Online Appendix). In sum, we illustrate clear trend breaks in all three aspects of care around birth. These trend breaks give rise to our identification strategy, described in the next section.

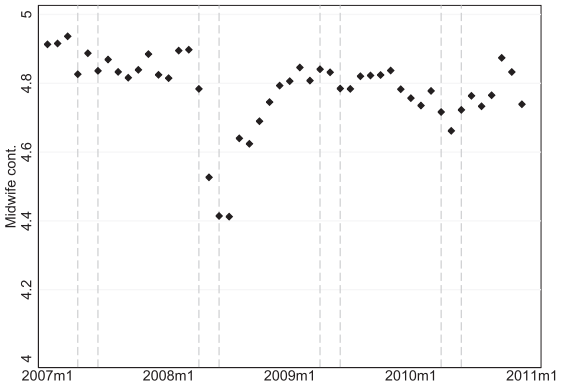
4. Empirical strategy

Fig. 3 focuses on the data used in our analyses and shows the 2008 and 2009 average levels of care around birth for four-day bins in the 80 days before the strike and the strike period (April 15–June 14, 2008). The figure plots averages for the number of midwife contacts, the percentage of women and infants discharged in the day of birth and the percentage of women with an early home visit. The figures show no jumps in the respective treatments in 2009. Furthermore, the figures illustrate (1) the similarity of trends in care in 2009 and 2008 in non-treated weeks and (2) the differential exposure of women at different points in time relative to the strike period. As an example, trends in early discharge rates are flat in the pre-strike period for both 2009 and 2008 (although longer-run increasing trends in the use of early discharge show up as a higher level of early discharge in 2009). Moreover, the impact of the strike on midwife contacts emerges for births that occur during the strike, while the impact of the strike on home visiting nurses is already present for pre-strike births. Respective figures that plot these first stage relationships by parity of the child look very similar and are available in the online Appendix (Figure A.6). Finally, Appendix Figure A.9 confirms that the strike was temporary, i.e., that care levels went "back to normal" after the strike.

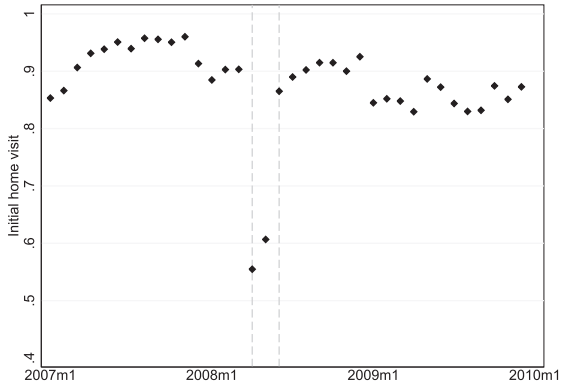
Because the strike impacted mothers and children differently depending on the date of birth, we focus on the effect of being born



(a) Percentage discharged on the day of birth (all children)



(b) Number of midwife contacts (all children)



(c) Percentage with initial nurse home visit (CHD)

Fig. 2. Discharged on the day of birth, number of total midwife contacts, percentage with the initial nurse visit; administrative data (discharge and midwife), CHD (nurse home visits); 2007–2010.

in a set of periods around the strike. That is, we compare the differences in outcomes of children and mothers with birth dates in eight two-week periods around April 15, 2008 (and thus exposed to different features of the strike) to the outcomes of children and mothers in the same periods in a control year. We perform our analyses using either 2009 or 2007 as comparison years. On the one hand, children born in 2007 are potentially impacted by the strike when they are one year old and thus our measures for longer-run health may be impacted. On the other hand, children born in 2009 are potentially impacted by longer-run consequences of the nurse strike. For brevity (and as anecdotal evidence suggests that the impact of the strike on 2009 care provision was at most minimal), we focus on the 2008/2009 comparison in the main analyses of this paper but analyses using the 2007/2008 comparison are available on request.

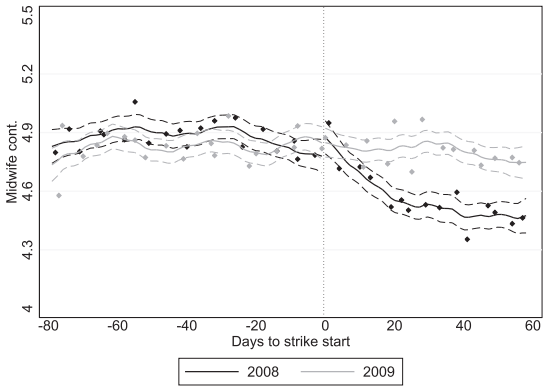
As the strike impacted several aspects of care around birth, we focus on the reduced form relationship of strike exposure and outcomes. Thus we estimate:

$$\begin{aligned} Y_{ithm} = \beta_0 + \sum_{j=-3}^{3} \phi_j 1(bin14_{ihm} \\ = j) \times Year2008_t + \sum_{j=-3}^{3} \alpha_j 1(bin14_{ihm} \\ = j) + \beta_1 Year2008_t + \gamma' \boldsymbol{X}_{ithm} + \boldsymbol{\lambda}_m + \boldsymbol{\mu}_h + \boldsymbol{\theta}_{weekday} + \varepsilon_{ithm} \end{aligned} \quad (1)$$

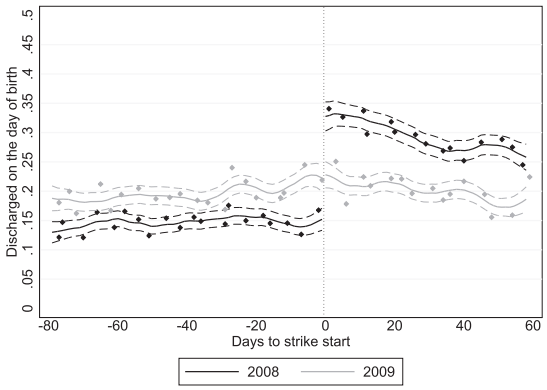
where Y is the outcome of interest, such as infant GP contacts, for infant i born in year t in hospital h and residing in municipality m. For the analysis based on national administrative data, X_{ithm} is a set of mother and child control variables. $Bin14_{ihm}$ is a set of fortnight-indicators centered around the date of the start of the strike (e.g., $Bin14_{ihm} = -1$ if the child is born between April 1 and April 14). We omit the indicator for the two-week period 8–6 weeks before the strike. The main effects for the fortnight bins and $Year2008_t$ account for the effect of being born in a given period (in any year) and birth year-specific shocks. To further rule out that differential health status or care availability by day of the week confound our analyses, we include $\theta_{weekday}$, which captures weekday fixed effects. Finally, λ_m and μ_h are fixed effects for municipality of residence and hospital of birth (again, the latter we can only include in analysis of national register data). We estimate equation (1) both on our full sample and separately for primi- and multiparous mothers.

The vector of coefficients ϕ_j in equation (1) represents the reduced form estimates for the effect of exposure to specific weeks before and during the 2008 strike on outcomes. Importantly, we estimate the average intent-to-treat effect of strike exposure on all women and on women of different parity/other characteristics. Because of the differential treatment by birth date, we find a simpler difference-in-differences estimate—that compares the differences in strike and non-strike births from 2008 and the same periods in 2009—less attractive.

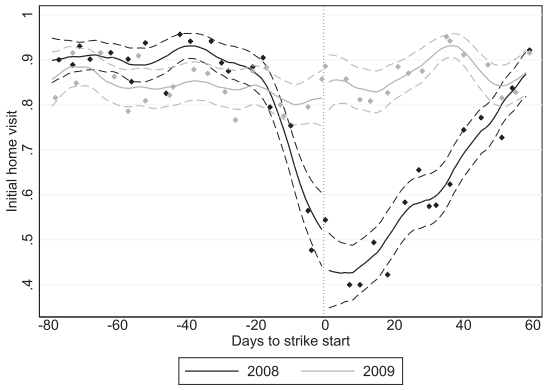
For our identification strategy to be valid, we rely on the assumption that the difference in outcomes of mothers/infants across the weeks that we compare in each year would have been the same in 2008 and 2009 in the absence of the strike (common



(a) Number of midwife contacts



(b) Percentage discharged on the day of birth



(c) Percentage with initial nurse home visit (CHD)

Fig. 3. Discharged on the day of birth, midwife contacts and percentage with home visits, 80 days before the strike and during the strike, local polynomial and four day bins, comparison 2008/2009.

trend assumption). We show in graphs for our central outcomes (see, e.g., Appendix Figure A.4 in the Online Appendix) that trends in outcomes over the 2007–2010 period are very smooth. Conveniently, our strategy (that is based on comparisons in a relatively short timespan during spring in a set of years) should take care of any season of birth-effects. As a robustness check, Appendix Table A.4 in the Online Appendix shows our main analysis with additional pre-periods. While this table shows, as expected, some significant differences in pre-periods, the estimates for these differences are very small and mostly imprecise. This finding is also in line with Fig. 3 and thus gives further credibility to our main specification.

Finally, we assume that women did not time their birth according to knowledge about the strike. As the strike came up rather surprisingly and the length of the strike was relatively short and hard to predict, we find this assumption reasonable. Moreover, Appendix Figures A.3a and A.3b in the Online Appendix show that the density of births in 2007–2009 displays seasonality but no indication for manipulation during the 2008 strike period.

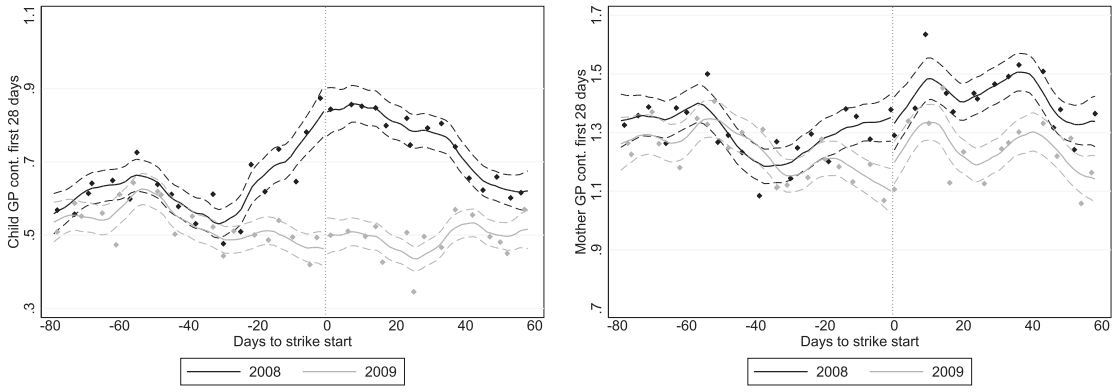
5. Results

5.1. Main health results

Fig. 4 is analog to Fig. 3 and presents graphical analyses of our main outcome, mother and child GP contacts in the first month of the child's life. The Figure suggests that for children and mothers first-month GP contacts increase during the strike (Appendix Figure A.9 in the Online Appendix confirms that this increase only persisted during the strike). However, for children the number of GP contacts increases already in the weeks up to the strike, a finding that may hint at the importance of home visiting.

Table 1 presents the results for our main outcomes based on equation (1) for all mothers and their children. In each row, we present the coefficients on the interaction term of the year 2008-indicator with two-weekly indicators. The reference group are children born between 56 and 43 days before that day. We center our data around the first day of the strike. In the analyses based on administrative data, we allow for municipality, hospital and day of the week fixed effects and we control for a set of parent and child characteristics (an indicator for child sex, the mother's and father's years of education, indicators for mother's and father's unemployment status, mother's and father's taxable income, and indicators for missing covariates for mother or father). In analyses based on the CHD and survey data we only can include day of the week and municipality fixed effects.

Panel A in Table 1 presents the regression-equivalents of the graphs for the effect of the strike on care around birth (the first stage). The strike decreases the total number of midwife contacts, most so for women later during the strike. Discharge rates increase sharply in the first weeks of the strike and stay high during all strike weeks. During the strike, mothers are between 12 and 17 percentage points more likely to be discharged on the day of birth, at the relevant mean this figure translates to a 100 percent increase of



(a) Child (b) Mother

Fig. 4. Child and Mother GP contacts in the first 28 days, 80 days before the strike and during the strike, local polynomial and four day bins, comparison 2008/2009.

Table 1
The effect of strike exposure on care exposure and maternal and child health. All births, data for the strike and control periods from 2008 to 2009.

Distance to strike (days)	−42−29	−28−15	−14−1	0−13	14−27	28−41	42−58	No. of obs.
A. First stage								
Midwife cont.	−0.01	−0.06	−0.16***	−0.17***	−0.41***	−0.45***	−0.41***	38,112
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	
Discharged on the day of birth	0.02	0.00	−0.02	0.17***	0.14***	0.12***	0.15***	39,715
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	
Initial home visit	0.01	−0.01	−0.24***	−0.45***	−0.37***	−0.36***	−0.11***	4322
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	
Child's age initial visit (weeks)	0.44	0.47	0.36	1.44***	1.72***	1.46***	1.50***	3162
	(0.44)	(0.43)	(0.41)	(0.48)	(0.50)	(0.49)	(0.54)	
B. Reduced form								
Child GP cont. ≤ 28 days	−0.01	0.07*	0.24***	0.30***	0.31***	0.23***	0.07*	39,715
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	
Child GP fee ≤ 28 days	−2.70	11.83**	30.81***	56.69***	54.49***	52.56***	26.98***	39,715
	(5.94)	(6.00)	(6.04)	(6.19)	(5.97)	(6.35)	(5.75)	
Child GP cont. 1. year	−0.12	−0.04	0.22	0.16	−0.22	−0.36	−0.55**	39,715
	(0.26)	(0.27)	(0.26)	(0.26)	(0.27)	(0.26)	(0.25)	
Child GP cont. 2. year	0.56**	0.35	0.72***	0.48*	0.26	0.19	0.28	39,715
	(0.25)	(0.26)	(0.26)	(0.25)	(0.26)	(0.25)	(0.24)	
Child GP cont. 3. year	−0.17	−0.05	0.37**	0.05	0.00	−0.18	−0.19	39,715
	(0.17)	(0.18)	(0.17)	(0.17)	(0.18)	(0.17)	(0.17)	
Mother GP cont. ≤ 28 days	−0.06	0.02	0.19***	0.12*	0.18***	0.19***	0.13**	39,712
	(0.06)	(0.07)	(0.06)	(0.07)	(0.06)	(0.07)	(0.06)	
Mother GP fee ≤ 28 days	−6.84	3.56	20.94**	21.18**	20.78**	20.20**	16.58**	39,712
	(8.06)	(8.67)	(8.21)	(8.70)	(8.36)	(8.58)	(8.04)	
Mother GP cont. 1. year	−0.03	0.25	0.48*	0.41	0.51*	0.03	0.17	39,712
	(0.29)	(0.29)	(0.28)	(0.29)	(0.29)	(0.29)	(0.27)	
Mother GP cont. 2. year	−0.31	0.08	−0.09	−0.24	−0.36	−0.55*	−0.13	39,712
	(0.32)	(0.31)	(0.31)	(0.31)	(0.32)	(0.32)	(0.30)	
Mother GP cont. 3. year	−0.50*	0.39	0.60**	0.22	0.34	0.12	0.12	39,712
	(0.30)	(0.32)	(0.31)	(0.30)	(0.31)	(0.30)	(0.29)	
Caesarean section	−0.01	−0.01	0.01	−0.02	−0.01	0.01	−0.01	39,715
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Unplanned caesarean section	−0.01	−0.02	−0.01	−0.00	−0.01	−0.01	−0.00	39,715
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	

Notes: All models based on administrative data include municipality, hospital and day of the week fixed effects. All models based on administrative data also include mother and child controls(child sex, indicators for mother's and father's education and unemployment status, mother's and father's taxable income, indicators for missing covariates for mother or father). We omit the indicator for children born between −56 and −43 days before strike. The regressions for *initial home visit* and *child's age at visit (conditional on having a visit)* are for the full CHD sample of children. These models include a day of the week indicator and a municipality of residence fixed effect and no controls. Robust standard errors in parenthesis. $p^{***} < 0.01$, $p^{**} < 0.05$, $p^{*} < 0.1$.

discharge on the day of birth rates. Finally, and as discussed above, the impact of the strike on the initial home visits emerges already for mothers giving birth in the two-week period up to the strike and is strongest for mothers giving birth in the beginning of the strike period. Children born in these weeks who received the early home visit are also older at the first visit.

Turning to our outcome measures, we find that mothers and children affected by the strike have more GP visits in the first month of the child's life. Again the effect on GP contacts emerges already in the pre-strike periods, with an effect size that is largest for the earliest strike weeks and thus the weeks with the lowest share of mothers who have received the initial home visit. We also find that GP fees are higher in the given weeks (on average 11–57 DKK or 1.5–7.6 EURO). This increase may reflect both the number of

visits and the intensity of treatment during those visits. The finding of a response with a higher frequency of GP visits may point to the importance of early home visits for children's (and mothers') health care usage (recall that mothers who give birth before the strike are treated, i.e., receive fewer and later home visits, due to the timing of their birth). Health professionals monitor children's health and advise parents on issues such as breastfeeding. In their absence parents may turn to other sources of support. Thus we may view this immediate health care demand as a substitution effect, i.e., mothers and children who lack one type of care during the strike demand care from another (free of charge) provider.

In line with this suggestion, we only see limited evidence for persistent health effects for children and mothers in our full sample (measured as GP contacts in the first three years of the child's life). Also considering mother and child readmissions to hospital and a set of diagnoses and complications after birth, we do not find clear patterns, partly due to power issues (see Appendix Table A.3 and Appendix Figure A.5 in the Online Appendix). We are constraint by the number of treated births in the relatively short strike period.

Finally, Table 1 shows no indication for manipulation through change of birth mode or a change in the quality of birth services, e.g., for children born just before the strike starts (prevalence of Ceasarean sections). These results also suggest that the emergency staff at work managed to maintain an adequate service level at birth wards.

5.2. Parental investment results: breastfeeding duration

To examine the impact of care around birth on parental investments, we use the two complementary data sources described in Section 3 and study maternal breastfeeding behavior. While we attempt to construct comparable measures in the two data sources, we have to keep the following limitations in mind: First, the data sources do not cover entire Denmark. Second, while the CHD contains nurses' registrations of breastfeeding behavior, the survey from Central Jutland contains retrospective maternal reports. However, in both data sources, around 60 percent of mothers breastfeed exclusively at least until month four of the child's life and the rates are similar for mother reports and nurse registrations. Third, the survey data from Central Jutland only contributes with births during the strike (April 15–June 14, 2008) to our analysis sample, from the CHD we include birth from the weeks around the strike in 2007–2009 (as in our main analysis). Having these drawbacks in mind, we pool the data sources to increase power.

Table 2 presents the results of our analyses based on the pooled CHD and survey data. As the first panel shows (for the pooled data on all parity births) the impact of the strike on the initial home visits by nurses after birth emerges for the weeks up to the strike. The next three panels show estimation results for three alternative samples that differ with respect to the control periods that we include: The second and third panel only compare births in 2007/2008 and 2009/2008, respectively. The last panel uses all observations from the "control years". Thus we compare the strike-year periods to both 2007 and 2009, i.e. we compare the week $\times$ 2008-indicators to the baseline of this pooled data (2007 and 2009 births).

While the estimates in Table 2 are less precise than our results for early GP visits, the table cautiously suggests that strike exposure impacts the probability of breastfeeding exclusively for four months negatively for mothers who give birth in the weeks up to and early in the strike in 2008 relative to their 2007/2009 counterparts. Moreover, as Fig. 5 illustrates, the estimates for the impact of the strike on breastfeeding of children born relatively "far away" from the start of the strike are close to zero. The only significant and negative estimates—that suggest a negative impact of strike exposure (and especially lack of early home visiting) on breastfeeding duration—are in the weeks around the start of the strike. Children born in these weeks (days 0–13 of the strike) are also the ones who are least likely to having received an initial home visit. These estimates very cautiously suggest that exposed mothers (who are the ones who had to wait longest for an early home visit or never received one) are around 11 percentage points—or 15 percent at the relevant sample mean—less likely to exclusively breastfeed at four months.

5.3. Robustness and heterogeneity

We have performed an extensive set of robustness tests. First, we use 2007 as our control year in our analysis of health outcomes and find very similar results (as also illustrated in Appendix Figures A.6 and A.7 in the Online Appendix). Furthermore, we have run a "simpler" difference-in-differences estimation that only compares two periods instead of a set of periods. Here we have performed (1) a donut-hole approach that omits births in the period of transition into the strike and measures full strike exposure (with one strike and one control period in 2008 and 2009) and (2) compared spring and fall births in 2008 and 2009, respectively. The most robust finding in both approaches is again the impact of the strike on short-run GP contacts.

Appendix Table A.5 in the Online Appendix shows that our results are robust to constraining our main analysis to the areas covered by the CHD and the survey. This finding also suggests that the two complementary data sources are informative additions to our national administrative data. Online Appendix Table A.6 shows results that are based on a sample of mothers and children, who are likely to be compliers (i.e. children in families with non-first time mothers of at least 18 years of age, at least one parent with more than 14 years of schooling, and family income in the top 33 percent). These results confirm our main analysis and suggest that the strike impacted low-risk children most. Finally, Online Appendix Table A.7 shows a placebo test. We show that we find no consistent effects of the strike periods (on care provision and outcomes) in two non-strike years (2009 and 2010). We only use the administrative register data in this table. Estimates are mostly very imprecise and much smaller in magnitude. A graphical analysis presented in Online Appendix Figure A.10 confirms that the 2009/2010 comparison of births around the strike does not indicate similar systematic jumps in either care provision or child and mother outcomes.

Our analyses on the full sample of children and mothers may conceal important differences across children of different parity. Experienced mothers may not require the same intensity of care as first time mothers. The data indicates that health care providers take mothers' experience into account when assigning levels care to them, both during the strike and under the default care regime (see Appendix Figure A.6 in the Online Appendix). Thus the strike hit mothers of different parity at different levels of care (which in turn may affect the size of any effect).

Tables 3 and 4 show that overall patterns for the impact of the 2008 strike are similar across parity (we omit longer-run GP outcomes and readmissions and diagnoses in these tables for brevity). Online Appendix Table A.8 shows the p-values for a test of the point-estimate differences between the two sets of mothers being different from zero. The differences in estimates across parity suggest some heterogeneity of the effect of the strike on home visiting. However, although the graphical inspection (see Appendix Figure A.7) indicates a larger effect on GP contacts for primiparous mothers, across all outcome measures we cannot reject the null hypothesis of the point estimates being equal across parity (partly due to power issues).

Table 2
The effect of strike exposure on care exposure breastfeeding probability at four months. All births in the CHD and strike births in the Region of Central Jutland, data for the strike and control periods from 2007 to 2009.

Distance to strike (days)	−42–29	−28–15	−14–1	0–13	14–27	28–41	42–58	No. of obs.
First stage (data from 2007/2008/2009), pooled CHD and survey data								
Initial home visit	0.03	−0.02	−0.27***	−0.51***	−0.44***	−0.30***	−0.07**	8221
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	
2007/2008, pooled CHD and survey data								
Exclusive breastfeeding at 4 months	−0.04	−0.04	−0.08	−0.13**	−0.05	−0.01	−0.01	4762
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	
2008/2009, pooled CHD and survey data								
Exclusive breastfeeding at 4 months	−0.05	−0.04	−0.11	−0.08	−0.07	−0.10	−0.03	4572
	(0.07)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	
2007/2008/2009, pooled CHD and survey data								
Exclusive breastfeeding at 4 months	−0.05	−0.04	−0.10	−0.11*	−0.06	−0.06	−0.02	6169
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	

Notes: All models include municipality and day of the week fixed effects. We omit the indicator for children born between −56 and −43 days before strike. Robust standard errors in parenthesis. $p^{***} < 0.01$, $p^{**} < 0.05$, $p^{*} < 0.1$.

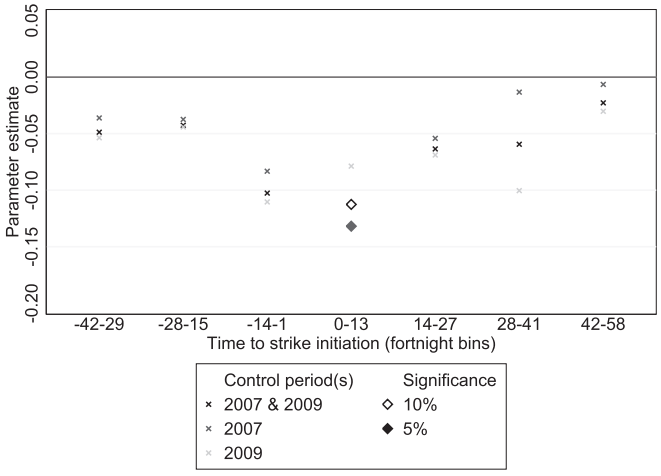


Fig. 5. Parameter estimates for the effect of strike exposure on exclusive breastfeeding at four months. Notes: The figure shows parameter estimates for the biweekly indicators from equation (1) in three samples, which use different comparison years for the 2008 strike year (comparison years are 2007, 2009, or 2007 and 2009 pooled). The diamond-shaped parameter estimates are significant at the 10 or 5 percent level. For further explanations see Section 5.

In additional analyses, we have not found strong differences in the access to care during the strike for mothers of different educational status, which constitutes one margin that is observed by health professionals and may play a role for the allocation of care both before and during the strike. Also for GP contacts in the first month of the child's life, we see no clear indication for heterogeneous effects by education of the mother (There is a very small tendency for more precise effects for highly educated mothers. This finding may indicate that highly-educated parents are more actively searching for substitution.)

6. Discussion and conclusion

Care around birth for low-risk mothers and their children is a cornerstone of developed health care systems. Despite the importance that health care professionals and political actors denote to it, its components and their effects on infant and mother health are still poorly understood. Specifically, we still lack evidence on the effects of care for a general population of infants and mothers, and on the effects of the timing and the composition of the care package.

Our study provides evidence on the short-run consequences of a supply shock that impacted the availability of care around birth for a population of low-risk mothers and children in a developed and

Table 3
The effect of strike exposure on care exposure and maternal and child health. Primiparous births, data for the strike and control periods from 2008 to 2009.

Distance to strike start (days)	−42–29	−28–15	−14–1	0–13	14–27	28–41	42–58	No. of obs.
A. First stage								
Midwife cont.	0.11	−0.05	−0.14	−0.09	−0.31***	−0.35***	−0.32***	16,631
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.08)	
Discharged on the day of birth	−0.00	−0.00	−0.02	0.17***	0.14***	0.12***	0.11***	17,048
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	
Initial home visit	−0.09	−0.03	−0.31***	−0.47***	−0.53***	−0.44***	−0.24***	2011
	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)	(0.07)	
Child's age initial visit (weeks)	1.28	1.25	1.12	2.36**	2.83***	1.99*	2.48**	1402
	(1.04)	(1.13)	(1.05)	(1.09)	(1.08)	(1.15)	(1.25)	
B. Reduced form								
Child GP cont. ≤ 28 days	−0.03	0.10	0.25***	0.38***	0.35***	0.28***	0.06	17,048
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	
Child GP fee ≤ 28 days	−2.62	13.72	34.89***	70.08***	60.91***	57.09***	24.06***	17,048
	(9.03)	(9.14)	(9.21)	(9.48)	(8.88)	(9.85)	(8.64)	
Mother GP cont. ≤ 28 days	−0.12	0.07	0.17*	0.28***	0.23**	0.28***	0.15	17,046
	(0.09)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.09)	
Mother GP fee ≤ 28 days	−15.34	2.02	18.01	38.17***	24.06*	33.48**	15.91	17,046
	(12.56)	(13.09)	(12.81)	(13.96)	(13.01)	(13.52)	(12.46)	

Notes: All models include municipality, hospital and day of the week fixed effects. All models based on administrative data also include mother and child controls (see Table notes for Table 1). We omit the indicator for children born between −56 and −43 days before strike. The regressions for *initial home visit* and *child's age at visit (conditional on having a visit)* are for the full CHD sample of children and without controls. Robust standard errors in parenthesis. $p^{***} < 0.01$, $p^{**} < 0.05$, $p^{*} < 0.1$.

Table 4
The effect of strike exposure on care exposure and maternal and child health. Multiparous births, data for the strike and control periods from 2008 to 2009.

Distance to strike start (days)	−42–29	−28–15	−14–1	0–13	14–27	28–41	42–58	No. of obs.
A. First stage								
Midwife cont.	−0.10	−0.08	−0.17**	−0.26***	−0.51***	−0.53***	−0.48***	21,481
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	
Discharged on the day of birth	0.03	0.01	−0.03	0.18***	0.14***	0.12***	0.18***	22,667
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Initial home visit	0.09**	0.02	−0.16***	−0.43***	−0.27***	−0.25***	−0.01	1978
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	
Child's age initial visit (weeks)	−0.33	−0.08	−0.19	0.51	1.07***	1.00***	0.76***	1594
	(0.36)	(0.19)	(0.18)	(0.38)	(0.29)	(0.26)	(0.19)	
B. Reduced form								
Child GP cont. ≤ 28 days	0.01	0.05	0.24***	0.25***	0.27***	0.19***	0.07	22,667
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	
Child GP fee ≤ 28 days	−3.14	9.69	26.90***	47.35***	47.95***	46.58***	29.12***	22,667
	(7.85)	(7.95)	(7.90)	(8.12)	(7.97)	(8.17)	(7.64)	
Mother GP cont. ≤ 28 days	−0.01	−0.02	0.19**	−0.00	0.14	0.12	0.11	22,666
	(0.08)	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)	(0.08)	
Mother GP fee ≤ 28 days	0.96	3.73	22.79**	9.04	17.91*	10.48	17.04	22,666
	(10.48)	(11.61)	(10.76)	(11.11)	(10.89)	(11.08)	(10.55)	

Notes: All models include municipality, hospital and day of the week fixed effects. All models based on administrative data also include mother and child controls (see Table notes for Table 1). We omit the indicator for children born between −56 and −43 days before strike. The regressions for *initial home visit* and *child's age at visit (conditional on having a visit)* are for the full CHD sample of children and without controls. Robust standard errors in parenthesis. $p^{***} < 0.01$, $p^{**} < 0.05$, $p^{*} < 0.1$.

universal health care system. In this setting, we find only short-run consequences of a decrease in care around birth. The findings suggest substitution of one type of care for another, i.e. parents increase their health care usage at alternative providers (such as GPs) as a response to a decrease in the standard care provided. This finding has implications for many health care systems, which provide free-of-charge care and the option to chose among health care providers. In these settings, policies that constrain the available care by nurses and midwives may lead to unintended increases in health care use at other providers. We also find some indication for the strike impacting breastfeeding duration negatively. This finding suggests potential longer-run consequences of strike exposure for treated children.

Several limitations in our analyses need mention: the strike was only a short interruption in care for mothers and children in Denmark, who went back to "default care" after a maximum of two months strike-exposure. Also, neither midwife nor hospital or home visiting care fully disappeared. Thus the strike most likely hit the most well-off mothers and infants and the emergency staff managed to select at-risk mothers and children for more intensive care. As we only show an average effect of strike exposure we likely understate the potential effects for subgroups in the population. Finally, due to its short duration we only have relatively few treated mothers and children in our analyses.

We also face limitations with respect to the available measures of infant and mother health in administrative data (which is only imperfectly captured by health care usage and diagnoses given) and parental investments (that obviously go far beyond our single indicator of breastfeeding duration). We still believe that our analysis is a good starting point that should be extended by further studies on other aspects of parental investment behaviors.

Power issues and the availability of data complicate our analyses and suggest that our analyses on health care usage is not an appropriate point of departure for a proper cost-effectiveness analysis. If we compare the costs of default care (midwife visits, hospital stay and home visits) with the strike situation (less care in all three areas) in a stylized analysis and only consider one outcome, the increase in GP visits in the first months, we are bound to find that the strike decreased costs to public health care. However, this finding would only factor in the (relatively low) costs to extra GP care and not pay attention to the potential costs that a low-intensity care regime may impose if implemented as a default: In this situation mothers and children would not return to "standard care" after a short period and many more mothers (at the lower end of the health and socioeconomic status-distribution) would be treated with low-intensity care. Thus a simple calculation would most likely underestimate costs. Moreover, our findings carefully suggest that care around birth impacts breastfeeding, a potentially important parental investment in their children, negatively—either through acquired parental skills, knowledge or self-confidence. These behavioral aspects are important to consider as benefits when analyzing the cost-effectiveness of care around birth and when designing policies targeted at new parents.

Acknowledgments

Wüst acknowledges financial support by the Danish Research Council (grant number 11-116669). Kronborg and Sievertsen acknowledge financial support by the Aarhus University Research Foundation through a grant to AU RECEIV (Research Centre for Early Interventions).

We thank Sascha Becker, Thomas S. Dee, two anonymous referees and seminar participants at SFI, the University of Copenhagen, and the University of Southern Denmark for helpful comments.

Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.socscimed.2015.12.034.

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