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PhD Thesis

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Universal Child Policies, Child Development and Parental Behavior

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Contents

Acknowledgements

First, I thank my parents. They affected my educational choices more than I would like to admit. My mother, Lone Lau-Jensen, is an economist and my father, Shabir Hirani, is a chemistry ph.d. Somehow and unknowingly (until very recent), I have ended up combining my parents' educations. So much for free will.

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Introduktion

Børns sundhed udvikler sig ulige, og ulighederne fortsætter og forstærkes op i voksenlivet.[1] På trods af disse fakta er yderligere forskning i årsagerne til ulighed i tidlig sundhed, samt i hvad samfundet kan gøre for at løfte sundheden hos den generelle befolkning inklusive de svageste, påkrævet. Desuden er årsager og løsninger uløseligt forbundet: identificerer vi årsagerne, kan denne viden bruges til at designe effektfulde indsatser, og identicerer vi virkningsfulde indsatser, udleder vi samtidig viden om årsagerne til ulighed i sundhed. Barndommen repræsenterer et oplagt tidspunkt at sætte ind med indsatser, da en stor empirisk litteratur viser, at forhold i tidlig barndom har permanente effekter på fremtidig udvikling.[2] Sideløbende er der vokset en teoretisk ramme, som har skabt forståelse og forklaringer for det samfundsøkonomiske rationale bag tidlige investeringer.[3]

Dansk social- og sundhedspolitik for nydfødte og forældre har siden midten af forrige århundrede hvilet på store universelle programmer. Tidlige universelle børneindsatser har to primære og overordnede egenskaber. For det første skal indsatsen være forebyggende ved at informere, screene og henvise familier og børn før ugunstige udfald indtræffer eller udvikler sig i for alvorlig grad. For det andet skal indsatsen nå alle dele af samfundet—herunder, og væsentligst, den udsatte del af befolkningen. Selvom de universelle børneprogrammer har eksisteret i lang tid og udgør betydelige offentlige udgifter, er vores viden om deres effekter fortsat begrænsede. For forskere med interesse i de danske universelle børneprogrammer, er den primære udfordring mangel på data. Selvom Danmark er internationalt anerkendt for registerdata af høj kvalitet, er data om kommunalt administrede børneprogrammer (f.eks. sundhedsplejerskeordningen og daginstitutionerne) på individniveau ikke en integreret del af registrene. Denne ph.d.-afhandling er en del af Center

[1] Case, A., Lubotsky, D., & Paxson, C. (2002). Economic status and health in childhood: The origins of the gradient. *American Economic Review*, 92(5), 1308-1334. Currie, J., & Stabile, M. (2003). Socioeconomic status and child health: why is the relationship stronger for older children?. *American Economic Review*, 93(5), 1813-1823.

[2] Almond, D. and J. Currie (2011). Killing me softly: The fetal origins hypothesis. *Journal of Economic Perspectives*, 25(3), 153–72 and Almond, D., J. Currie, and V. Duque (2018). Childhood circumstances and adult outcomes: Act ii. *Journal of Economic Literature*, 56(4), 1360–1446.

[3] Cunha, F., Heckman, J., Lochner, L., & Masterov, D. V. (2006). Interpreting the evidence on life cycle skill formation. *Handbook of the Economics of Education*, 1, 697-812; Heckman, J. (2006). Skill formation and the economics of investing in disadvantaged children. *Science*, 312(5782), 1900-1902 og Cunha, F., & Heckman, J. (2007). The technology of skill formation. *American Economic Review*, 97(2), 31-47.

For Research on Universal Child Policies (CRUNCH): et forskningsprojekt finansieret af Innovationsfonden med målsætning at indsamle data, samt at udvide forståelsen for de danske universelle børneprogrammer. En del af projekterne i denne afhandling anvender disse data, men mulighederne, dataene indeholder, er langt fra udtømt med dette arbejde.

Denne afhandling består af fire selvstændige artikler, hvis formål er at bidrage til forståelsen af, hvordan forældres adfærd og offentlige indsatser interagerer og påvirker børns udvikling. Jeg anvender naturlige eksperimenter, kvantitative metoder og forskellige datakilder til at afdække kausale sammenhænge mellem indsats og familiers udvikling og adfærd. Kapitel 1 beskæftiger sig med et af landets første universelle programmer - mødrehjælpsinstitutionen - i et historisk perspektiv. Institutionerne blev grundlagt i 1939, og var en politisk reaktion på en årrække med faldende fertilitet og udpræget brug af illegal abort og var et politisk instrument for at modvirke de to tendenser. Kapitel 2 undersøger den moderne sundhedplejerskeordning for nyfødte og deres familier ved at anvende variation skabt af en national sundhedsplejerskestrejke i 2008. Strejken varede i 61 dage, hvor 90 pct. af alle sundhedsplejerskebesøg blev aflyst. Kapitel 3 beskæftiger sig med det danske børnevaccinationsprogram og undersøger, hvorfor nogle børn ikke følger programmets anbefalinger. Det undersøges ved at se på deres forældres respons på at få tilsendt et påmindelsesbrev om manglende vaccinationer. Kapitel 4 bygger bro mellem sundhedsplejerskeordningen og vaccinationsprogrammet ved at undersøge, hvorvidt sundhedsplejersker gennem deres hjemmebesøg formår at få nybagte forældre til at deltage i to universelle forbyggende børneprogrammer: De forebyggende helbredsundersøgelser til børn og det danske børnevaccinationsprogram.

Første kapitel "*Fertility and Family Planning Programs - Evidence from a Historical Policy Experiment*" undersøger mødrehjælpsinstitutionernes indflydelse på fertiliteten i og udenfor ægteskab. Den tidligere filantropiske organisation blev en del af den offentlige sundhedssystem i 1939, og blev i den forbindelse kraftigt oprustet. Fra udelukkende at have opereret i København, åbnedes institutioner i de største danske byer samt Sønderborg og Næstved. Sønderborg og Næstved fik, som de eneste to mellemstore byer i landet, oprettet mødrehjælpsinstitutioner, hvilket udgør ideelle naturlige eksperimenter til at studere institutionernes effekt, da de resterende sammenlignelige byer først fik institutioner i 1948. Jeg anvender historisk data for danske byer og amter, som er indsamlet fra henholdsvis skrevne kilder og materialer fra Rigsarkivet. Resultaterne viser, at antallet af børn født udenfor ægteskab blev femdoblet blot et par år efter institutionernes åbning. Fertiliteten blandt gifte var ikke påvirket af mødrehjælpens tilstedeværelse. De heterogene fertilitetseffekter kan forklares ud fra måden, som insitutitionerne arbejdede på samt deres politiske opdrag: Institutionerne forsøgte at få abortsøgende gravide væk fra ideen om abort (som på daværende tidspunkt var ulovlig) ved at oplyse om risiciene og rådgive om alternativer til abort. På dette tidspunkt i Danmarks historie var det typisk ugifte gravide kvinder, som søgte at abortere.

I andet kapitel "*Timing of Early Interventions and Child and Maternal Health*" undersøger jeg i samarbejde med Miriam Wüst og Hans Henrik Sievertsen, hvordan timingen af sundhedplejerskebesøg påvirker børns og mødres udvikling. Fra 15. april til 15. juni 2008 strejkede sundhedsplejersker i hele Danmark, da overenskomstforhandlinger mellem FOA, Sundhedskartellet og arbejdsgiverne endte i konflikt. Strejken medførte massive aflysninger af planlagte sundhedsplejerskebesøg, således at børn, født med blot få ugers mellemrum, modtog forskellige forløb med hensyn til timingen af hjemmebesøg. Eksempelvis mistede et barn, født 10 dage før strejkestart, det første hjemmebesøg, men modtog alle andre besøg uændret, mens et barn, født 20 dage før strejken, mistede det andet hjemmebesøg, men modtog de andre besøg upåvirket. Strejken fungerede dermed som et randomiseringsredskab, hvor børn, kun afhængigt af fødselstidspunkt relativt til strejken, mistede et af de fire universelle besøg i sundhedsplejerskeordningen. Ved at anvende variationen skabt af strejken, estimerer vi den relative betydning af de fire universelle hjemmebesøg. Resultaterne viser, at tidligere besøg er vigtigere end senere besøg. Særligt det tidligste besøg (14 dage efter fødsel) har stor betydning, da børn og mødre, som mistede dette besøg, havde markant flere konsultationer ved praktiserende læge og vagtlæge i de efterfølgende år. Vi finder også evidens for, at mødrene, hvis børn mistede det tidligste besøg, havde øget sandsynlighed for at se en psykolog eller psykiater, hvilket indikerer, at det tidlige besøg har betydning for mødres mentale sundhed.

I tredje kapitel "*Inattention or Reluctance? Parental Responses to Vaccination Reminder Letters*" undersøger jeg effekten af påmindelsebreve på tilslutningen til det danske vaccinationsprogram. Jeg knytter forældrenes respons på at blive påmindet til deres årsag til manglende tilslutning. Intuitionen er, at forældre, som ikke reagerer på påmindelsesbrevet, aktivt må have fravalgt deltagelse i vaccinationsprogrammet, og derfor kan kategoriseres som modvillige. Omvendt må forældre, som reagerer på et påmindelsesbrev, have været uvidende om det faktum, at deres børn ikke havde fået alle anbefalede vaccinationer. Påmindelsesbreve blev introduceret den 15. maj 2014, og blev fra den dag sendt til alle forældre med et barn, som manglede en eller flere af de planlagte vaccinationer, når barnet fyldte to år. Jeg estimerer, at 8.7 % af forældre, hvis børn manglede en eller flere vaccinationer, var uvidende, mens 72 % ikke har tilsluttet sig vaccinationsprogrammet på baggrund af en aktiv beslutning, og derfor må have en modvillighed mod at deltage i programmet. Resultaterne viser, at manglende tilslutning til det danske børnevaccinationsprogram ved 2 års alderen i høj grad skyldes modvillighed, og at tiltag udover påmindelsesbreve er nødvendige for at generere et markant løft i tilslutningen. Derudover finder jeg, at forældre uddannet indenfor sundhed og pædagogik, forældre med børn født for tidligt eller med lav fødselsvægt samt forældre med en universitetsuddannelse ikke reagerer på påmindelsesbreve. At de pædagogisk- og sundhedsuddannede forældre ikke reagerer på påmindelsesbreve, og dermed udelukkende er modvillige, når de fravælger tilslutning, er forventet, da de må have kendskab til vaccinationsprogrammet gennem deres profession.

I fjerde kapitel "*Nurses and Parental Health Investments*" studerer jeg i samarbejde med Miriam Wüst, sundhedsplejerskernes påvirkning på forældres deltagelse i to centrale forebyggende børneprogrammer: De forebyggende helbredsundersøgelser til børn og det danske børnevaccinationsprogram. Helbredsundersøgelserne og børnevaccinationsprogrammet adskiller sig fra sundhedsplejerskeordningen ved, at forældre selv skal kontakte deres praktiserende læge for at modtage ydelserne. Det står i modsætning til sundhedsplejerkseordningen, hvor forældre kontaktes og tilbydes at få tilknyttet en sundhedsplejerske samt modtage hjemmebesøg. Vi benytter variation, som naturligt forekommer, i placeringen af hjemmebesøg tæt på de anbefalede aldre for vaccinationer og helbredsundersøgelser. Således sammenligner vi to grupper, som har modtaget hjemmebesøg med få ugers mellemrum, men adskiller sig ved, at den ene gruppe gruppe modtog besøget kort før den anbefalede alder for at blive eksempelvis vaccineret, mens den anden gruppe modtog besøget efter. Vi viser, at sundhedsplerjeskehjemmebesøg positivt påvirker rettidig vaccination. I vores analyse finder vi ingen til små permanente effekter, hvilket tyder på, at sundhedsplejersker primært fungerer som menneskelige påmindelser uden at ændre forældres holdning om vigtigheden af rettidig vaccination.

Introduction

A large literature documents substantial early-life health inequalities, which persist and widen into adulthood.[4] However, knowledge on the causes of early-life health inequalities and policies to increase the health of the general population and of the most disadvantaged, are topics which require additional research. Expanding our knowledge on the causes of early-life health inequalities enable us to design effective policies and identifying effective policies allow us to infer information on the driving forces of differential health formation. Childhood represents a suitable stage for public interventions as a vast amount of empirical evidence shows that early-life events have long-lasting effects on future development.[5] Along with the mounting empirical evidence, theories of life-cycle health formation have added a framework to understand and explain the economics of early investments.[6]

Danish social and health policy for families with newborns has since the mid-1900's been structured around large-scale universal programs (e.g. nurse-home-visiting, childcare, preventive health checks, vaccination program). Early universal child programs have two main purposes. First, the program aims at preventing adverse situations from developing by screening, informing and referring families. Second, the program should reach all layers of society – most notably parents who would otherwise not seek assistance from the health care sector. Although these programs constitute significant public investments and have a long history, we know relatively little concerning their effects on child development and parental behavior. The primary obstacle for researchers interested in the Danish universal child programs has been poor access to individual-level data. This type of data is not part of the registers at Statistics Denmark but registered and stored locally

[4] Case, A., Lubotsky, D., & Paxson, C. (2002). Economic status and health in childhood: The origins of the gradient. *American Economic Review*, 92(5), 1308-1334. Currie, J., & Stabile, M. (2003). Socioeconomic status and child health: why is the relationship stronger for older children?. *American Economic Review*, 93(5), 1813-1823.

[5] Almond, D. and J. Currie (2011). Killing me softly: The fetal origins hypothesis. *Journal of Economic Perspectives*, 25(3), 153–72 and Almond, D., J. Currie, and V. Duque (2018). Childhood circumstances and adult outcomes: Act ii. *Journal of Economic Literature*, 56(4), 1360–1446.

[6] Cunha, F., Heckman, J., Lochner, L., & Masterov, D. V. (2006). Interpreting the evidence on life cycle skill formation. *Handbook of the Economics of Education*, 1, 697-812; Heckman, J. (2006). Skill formation and the economics of investing in disadvantaged children. *Science*, 312(5782), 1900-1902 and Cunha, F., & Heckman, J. (2007). The technology of skill formation. *American Economic Review*, 97(2), 31-47.

in each municipality. This ph.d.-project is part of Center for Research on Universal Child Policies (CRUNCH): a project funded by the Innovation Fund Denmark with the research agenda to collect individual-level data on universal child programs for research purposes. Some of the articles in this dissertation use these unique and newly available data but their applications remain far from exhausted.

This dissertation consists of four self-contained chapters. Each chapter can be read independently but jointly they contribute to the understanding of the interactions between parental behavior and public policies in shaping child development. I use natural experiments, quantitative methods and various data sources to investigate the causal links between aspects of universal programs and family well-being and behavior. Chapter 1 studies one of the first universal child policies in Denmark, namely a family planning program introduced in 1939 as a political response to decades of declining fertility and widespread use of illegal abortions to terminate pregnancies. The family planning program served as an instrument to reverse both these tendencies. Chapter 2 investigates the contemporary nurse home visiting program for infants in Denmark. Using a national nurse strike in 2008, the chapter studies how the timing of nurse visits impacts child and maternal health. Chapter 3 studies why some parents fail to adhere to the recommendations of the Danish Childhood Vaccination Program by exploiting parental responses to vaccination reminder letters. Chapter 4 explores the interaction between the nurse home visiting program and other preventive care programs for infants in Denmark. Specifically, the chapter studies if nurses during home visits encourage parental health investments measured as adherence to the recommendations in the vaccination and GP health check programs.

Chapter 1 "*Fertility and Family Planning Programs - Evidence from a Historical Policy Experiment*" studies the non-marital and marital fertility effects of a historical family planning program in Denmark. In 1939, the family planning program became part of the public health care sector and expanded from Copenhagen in two waves. The first expansion wave in 1939 introduced the program in the four next largest towns in Denmark and in two medium-sized towns. In the second expansion wave in 1948 the program was introduced across the country. I use data from Danish towns and counties collected from historical statistical documents and materials from the Danish National Archives. I find that access to the family planning program significantly increased the non-marital birth rate but had no effect on marital birth-giving. The design of the family planning program explains these heterogeneous responses: The program aimed at reducing the use of illegal abortions by informing on the potential risks and by providing less costly alternatives such as assistance with adoption, limited aid (milk, food and clothes) and legal advice in paternity cases. During this period of Danish history, the typical pregnant woman seeking an abortion was unmarried.

Chapter 2 (joint with Hans Henrik Sievertsen and Miriam Wüst) "*The Timing of Early Interventions and Child and Maternal Health*" investigates the impact of timing in the provision of universal nurse home visiting. In 2008 from April 15 to June 14, nurses across the country went on strike as the collective bargaining between the trade union and the employers broke down. The strike caused permanent mass cancellations of scheduled nurse visits which generated plausibly exogenous variation in the timing of forgone nurse visits. For example, a child born 10 days prior to the start of the strike lost the initial visit (scheduled within the first 14 days of life) but received all other visits unaffected, while a child born 30 days prior to strike start lost the second nurse visit but received the other visits unaffected. Thus, the strike randomized (conditional on date of birth relative to the strike) which nurse visit a child had cancelled. We show that, while children born prior to the strike all had increased probabilities of missing a nurse visit, depending on the date of birth relative to the strike, children differed in age at the forgone visit. Furthermore, we show that exposure during the initial months of a child's life is relatively more influential for child and maternal health development. Specifically, we find that children who missed a nurse visit within the first three months of life had more future GP and hospital contacts. For mothers, we estimate negative health and mental health effects of losing an early nurse visit in line with an emerging literature documenting the importance of different aspects of early circumstances for maternal postpartum health.

In chapter 3 "*Inattention or Reluctance? Parental Responses to Vaccination Reminder Letters*" I study the effects of vaccination reminder letters on adherence to the Danish Childhood Vaccination Program. Reminder letters were introduced May 15, 2014 and from that day and onward every parent, with a child lacking at least on scheduled vaccination, receives a reminder letter. I move beyond studying the effect of reminder letters, by providing a framework that links parental responses to reminder letters to their causes for non-adherence. The intuition is that parents, who receive a reminder letter, but fail to respond, must have actively decided against adherence and are thus reluctant while responsive parents must have been inattentive of the fact that their child was non-adherent. I estimate that 8.7 % of non-adherent parents respond to the reminder while 72.1 % are non-responsive. The results show that the leading cause for non-adherence in the Danish vaccination program is reluctance and that other policies beyond reminder letters are necessary in order to substantial increase coverage above the current level. Furthermore, I estimate that parents educated in health or childcare, parents with preterm and low birth weight children and parents with a university degree do not respond to reminder letters. The heterogeneous responses – especially that health and childcare educated parents are non-responsive to reminder letters — support the interpretation that responsive parents are non-adherent due to inattention.

In Chapter 4 "*Nurses and Parental Health Investments*" Miriam Wüst and I study the impact of nurse home visiting for new families on the timely uptake of recommended preventive care using newly-collected data on nurse registrations merged with administrative register data. While the national preventive care programs (the vaccination program and the GP health checks) require parents to actively reach out to their family GP to receive the care, nurses in the nurse home visiting program pro-actively contact parents to offer the visits. Thus, we investigate whether nurses encourage parents to adhere to the recommendations of the vaccination and GP health check programs. We exploit variation in the timing of nurse home visits in a narrow time window around the recommended age for preventive care. We compare the behavior of parents who receive a nurse visit during the week of the recommended age for preventive care, to the behavior of similar parents who receive a nurse visit shortly after. We find that parents respond with substitution by delaying GP health checks, if they receive a nurse visit at the recommended age for the health check. Concerning vaccinations, we find that parents are more likely to receive timely vaccinations, if they receive a nurse visit at the recommended age for vaccinations indicating that nurses encourage timely take-up of vaccines.

Chapter 1

Fertility and Family Planning Programs - Evidence from a Historical Policy Experiment

Fertility and Family Planning Programs - Evidence from a Historical Policy Experiment*

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Abstract

A large literature considers family planning programs with a focus on birth control and finds that access reduces fertility. In this paper, I study the fertility effects of access to a Danish family planning program introduced in 1939 and designed as a political response to decades of declining fertility. The aim of the program was to increase fertility by advising against illegal abortions. I exploit variation in the timing of program implementation and use digitized data for Danish towns and counties from 1921-1947 to estimate the causal fertility effects using the synthetic control method. I find significant and positive non-marital fertility effects of program access but no effects on marital fertility. Suggestive evidence indicates that a combination of increased adoption options and legal, health related and social advice worked as mechanisms.

JEL Codes: H51, J12, J13, J18, N34

Keywords: Family planning, fertility, abortion, economic history

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1 Introduction

Economic and demographic research has a long-standing interest in fertility management and control. An extensive literature considers the fertility effects of family planning programs (Miller and Babiarz, 2016). Today, family planning programs have approached global coverage (de Silva and Tenreyro, 2017) and research highlights the importance of family planning on world population growth (Bongaarts et al., 1990).[1] Existing evidence considers family planning programs which offer services and information concerning contraceptive methods and fertility control with reductions in population growth and fewer unwanted births as policy targets.

This paper adds to the understanding of how public family planning programs can influence fertility in a historical context. In 1939, a Danish family planning program – a program that earlier operated as a small-scale non-profit organization based on volunteers – became part of the public health care sector and expanded from Copenhagen in two waves. In the first expansion wave in 1939, the family planning program was introduced in the five largest towns in Denmark and two medium-sized towns (Sønderborg and Næstved) and in 1948, the second wave expanded the program to universal coverage (The Medical Reports for the Kingdom of Denmark, 1947). The political background for the expansion of the family planning program was declining fertility through the early part of the 20th century and the program became an instrument to increase fertility (The Population Commission, 1938). Thus, the Danish family planning program had the opposite policy target compared to programs predominantly considered in the literature.

The program was a free service offered to all pregnant women and had the aim to reduce the number of illegal abortions and thus mechanically increase fertility. Hence, unmarried pregnant women received particular attention as they were most likely to pursue illegal abortions (Skalts and Nørgaard, 1982).[2] Illegal abortions were widespread with an estimated 12,000-22,000 pregnancies terminated illegally each year (Sturop, 1967). The program advised women against aborting illegally and recommended giving birth while offering assistance after birth. This assistance consisted of: i) in-kind (milk, food, clothes) and legal aid in paternity cases if the mother decided to keep and raise the child and ii) information and mediation of the adoption system if the mother decided to give up the child.

[1] According to de Silva and Tenreyro (2017) the negative relationship between income and fertility has shifted to the left during the last 60 years. The same GDP per capita level is associated with lower fertility today compared to 1960 in cross-country comparisons. They mention the global spread of family planning programs as the underlining driver. Bongaarts et al. (1990) project that in absence of family planning programs the population in the developing world would reach 14.6 billion in 2100 instead of 10 billion (World Bank projection).

[2] Abortions were illegal in Denmark until 1973.

In this paper, I analyze the non-marital and marital fertility effects of the 1939 family planning program in Denmark. I digitize historical data for Danish towns and counties to construct a panel dataset with yearly observations on the number of live births in and outside of marriage covering 1921-1947. I combine the fertility measures with data on income and wealth among other relevant covariates obtained through various historical sources.

To examine the fertility effects of the program, I use the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015). The main identification challenge is to construct a credible control group which in the absence of treatment would mimic the evolution of fertility in the treated areas. This challenge is amplified in small sample sizes and when data is aggregated. The synthetic control method addresses this by constructing a synthetic control unit that most closely resembles the treated unit prior to treatment. I use the first expansion wave in 1939 as a natural experiment which created a group of seven treated towns and a group of control towns. However, as the top five largest towns in Denmark were all treated and none of the control towns represent suitable comparisons, I focus my analysis on the two treated medium-sized towns and 25 suitable control towns.

I find that access to the family planning program had an impact on fertility. However, the impact differed as intended across marital status of the mother: the non-marital birth rate increased while the marital birth rate was unchanged in response to program access. I explore two potential explanations: The results may either be driven by pregnant (unmarried) women from neighbouring towns and rural upland travelling to the treated towns to give birth (i.e. migration but no change in birth-giving) or an actual increase in fertility for residents exposed to the program.

The evidence shows that both explanations play a role. The findings indicate that the family planning program reduced the cost of giving birth outside of marriage by providing alternatives to illegal abortion. The alternatives were easier access to adoption and legal, social and health related advice. In line with this, marital fertility and the marriage rate did not respond to the family planning program. The results are robust to a range of different specifications and estimation methods and pass various placebo tests.

My analyses cannot determine whether lifetime fertility was affected as it focuses on short-term effects. However, the results indicate that unmarried pregnant women who were inclined to pursue illegal abortions substituted toward the alternatives as recommended by the family planning program. This directly increased non-marital fertility through a reduction in the use of illegal abortions. The results of this paper, combined with the previous literature, suggest that family planning policies have the ability to both increase and reduce fertility. The exact content and context of the program determines how fertility responds. My study serves as a further proof-of-concept for the effectiveness of family planning programs in matters of fertility and population management.

By studying a program uniquely designed to increase fertility, this paper contributes to the literature on how public policies and access to family planning, contraceptives, abortions impact fertility. Previous research into family planning programs consistently indicates that increased access reduces fertility and unwanted pregnancies (Phillips et al., 1982; Angeles et al., 2005a,b; Kearney and Levine, 2009; Bailey, 2013; Bailey et al., 2019).[3] Studies directly investigating oral contraception and abortion likewise find that access affects fertility decisions (Goldin and Katz, 2000, 2002; Bailey, 2006; Guldi, 2008; Myers, 2017). Moreover, an extensive literature documents that a range of public policies (Medicaid, China one-child policy, Affordable Care Act) influences fertility decisions (Joyce et al., 1998; Zavodny and Bitler, 2010; DeLeire et al., 2011; Zhang, 2017; Abramowitz, 2018). Related and recent studies from the US (Packham, 2017; Lindo et al., 2017; Fischer et al., 2018; Lu and Slusky, 2019) evaluate the effects of restricted access to family planning services and abortion through clinic closures in Texas and find that restricted access reduces the number of abortions and increases birth rates.[4] As family planning programs usually provide information and supply contraceptives (condoms, birth control pills and abortion services), most of the existing literature considers programs which target reductions in population growth and unwanted pregnancies and births. The studies document robust evidence that such programs reduce fertility. This paper fills a gap in this literature by studying a program with the opposite policy target in a context without effective contraceptive methods.

The paper unfolds as follows: Section 2 discusses the history of family planning in Denmark

[3] Phillips et al. (1982) evaluate a large scale experiment with family planning services in Bangladesh in 1977. They find that the program led to an increase in the use of contraceptives which reduced fertility by 22-25 % in the first years of the project. They conclude that the family planning program met an unsatisfied demand for contraception which fuelled the decline in fertility. Angeles et al. (2005a,b) study family planning programs in Indonesia and Peru in the 1990's and find that access to family planning services reduced fertility both directly and indirectly by allowing women to obtain more education which increased the alternative cost of having a child. Evidence from the USA supports those from the developed world, (Bailey, 2013; Kearney and Levine, 2009). Bailey et al. (2019) show that family planning programs in the US and the avoidance of unwanted children through increased fertility control for women improved these families economic resources and benefited future children. Some research shows mixed results as reviewed by DiCenso et al. (2002). The reviewed studies are randomized experiments and might suffer from small sample sizes.

[4] Lindo et al. (2017) estimate statistically significant effects on abortion but argue that the effects are to small to detect them in the birth rate. Packham (2017) finds that restricted access increases teen birth rates by 2-3 %. Fischer et al. (2018); Lu and Slusky (2019) also estimate positive fertility effects from restricted access using the same source of variation.

and the lead up to the program with a specific focus on non-marital pregnancies. Section 3 presents my empirical strategy. Section 4 describes the data sources, outcomes, covariates and presents descriptive evidence. Section 5 contains the main results and robustness checks and section 6 presents data on potential mechanisms. Section 7 concludes.

2 Background

2.1 Non-marital Pregnancies and Children in 1700-1900 Denmark

In 1763, King Frederik V of Denmark issued a public statement emphasizing the responsibility of fathers of children born outside of marriages. The King ordered fathers to bear an equal share of the burden associated with a childbirth. If a father failed to do so, the King encouraged the local community to force the father to hand over a part of his income to the mother. According to Skalts and Nørgaard (1982) this might be the first law in the world advocating the rights for unmarried mothers and their children.

At the same time in Copenhagen, a birth ward specializing in assisting unmarried pregnant women opened. Help was free of charge and included supervised births, clothes, food and nourishment for the infant. Unmarried women had the right to remain anonymous when admitted at the hospital during childbirth.[5] In 1771, the hospital placed a box outside the hospital for women to deposit unwanted infants. The hospital would then place the child in care. The founding box was a measure to combat killings of infants born out of wedlock. In the 1800's the progress to assist unmarried pregnant women stopped due to moral concerns. The argument was that too much help might spur promiscuity outside of marriages (Skalts and Nørgaard, 1982).

2.2 Declining Fertility and the Lead Up

The main motivation for expanding the family planning program in Denmark was low birth rates in the first part of 1900's (The Population Commission, 1938; Skalts and Nørgaard, 1982). Fertility declined by 25 % in Denmark in the beginning of the 1900's as shown in Figure 1. From 1921 to the mid 1930's the crude birth rate in Denmark dropped from 24 live births per 1,000 people to 18. However, from 1940 and the following five years the crude birth rate recovered to the 1921-level. This development can be tracked in both urban and rural areas (right panel). The crude birth rate in urban areas increased more than in rural areas.

[5] The right to remain anonymous was abandoned in 1938.

Fig. 1 The total crude birth rate in Denmark

(a) Country **(b)** Urban and rural

Notes: Own calculations based on data from The Medical Report for the Kingdom of Denmark, various years.

The low birth rate concerned politicians and to develop solutions they formed the Population Commission in 1935 (Skalts & Nørgaard, 1982). The Population Commission was instrumental in the formation of the family planning program. The Commission had three main recommendations. First, direct financial support for mothers. Second, clinics to teach basic sexual education across the country. Third, an expansion of family planning services to provide aid and care to pregnant women. The only recommendation implemented immediately – and throughout the period considered in this paper – was to increase the availability of family planning services.

2.3 The Family Planning Program

The Danish parliament adopted the government-funded family planning program by law in 1939. Implementation began April 1, 1939 where the public health care sector took over and expanded a private organization in Copenhagen called *Mothers Aid Institutions*. Branches opened in the five largest Danish towns and two medium sized towns - Sønderborg and Næstved.[6] Financial considerations restricted the implementation although the aim was to eventually expand across the country. A key feature in Sønderborg was that a public birth ward had opened in 1934 (Medical Report for the Kingdom of Denmark, 1934). Sønderborg was the first town outside the five largest towns to have a public birth ward which may have been important in the allocation of the family planning program in Sønderborg as the presence of a well-developed health care

[6]The five largest towns in Denmark were Copenhagen (capital), Aarhus, Aalborg, Odense and Esbjerg. The five largest towns were chosen as locations for the program on the grounds of demand and efficiency. Their large populations secured demand and social workers, hospitals etc. could increase efficiency by complementing the family planning program. The Population Commission note that Sønderborg and Næstved have the potential to establish the required infrastructure in the future.

infrastructure was a factor in the allocation process (The Population Commission, 1938).

When designing the program, The Commission had three targets in mind: i) accessing unmarried pregnant women, ii) preventing illegal abortions and iii) improving the public perceptions of non-martial pregnancies. The Commission feared that unmarried and abort-wanting women would not contact the program because of their decision to abort. Handing the power to grant legal abortions to the program, gave the targeted women incentives to contact the program. Thus, the program acted as the entrance into the abortion system. When in contact, the staff should advice against aborting illegally.[7] Besides the legal issue, the program provided in-kind aid (milk, food and clothes), guidance through the adoption system and legal aid in paternity cases to provide women with alternative options to abortion. Finally, to improve the public perception of non-marital pregnancy, the program was available for all pregnant women and had high-quality staff.

The program was designed based on recommendations described in The Population Commission (1938). The Population Commission (1938) highlighted a need to change the social stigma associated with non-marital birth-giving by showing and signaling that the society equally cared for and valued children born outside marriages. The commission did not regard financial aid to be an effective instrument in this pursuit but recommended a family planning program to help legitimize non-marital pregnancies, births and children. Similarly high on the agenda was to reduce illegal abortions among unmarried pregnant women, as this would have a direct positive effect on the birth rate. In their report from 1938 an entire section "*Family planning as a mean against abortions*" deals with the exact strategy. The following points summarize the program:

1. To get in contact with women without ambitions to keep their children and make them attend the program by placing the authority to grant legal abortions within the program.

2. To advice women with a wish to abort - but were not granted a legal abortion - to keep the child by explaining the dangers of illegal abortions and that they could face legal prosecution and by providing alternatives.

3. To make the program an integrated and socially accepted part of every pregnancy by being a universal offer and by attracting married and well-off women, too.

4. To facilitate acceptance, usage and impact by employing educated and professional staff

[7] Abortion was illegal unless severe medical reasons justified it. It was debated heavily whether social considerations should be taken into account but a law from 1937 only had medical reasons as a factor (Skalts and Norgaard, 1965).

ranging from doctors to midwives, nurses and social workers.

5. To provide health examinations and advice on topics such as nourishment and hygiene.

6. To provide legal counsel in paternity cases in order to inform unmarried pregnant women on their legal rights as potentially single parents.

The program lowered the cost of non-marital birth-giving compared to having illegal abortions by offering adoption options, in-kinds and legal assistance for unmarried pregnant women. This could increase the non-marital birth rate by incentivizing more women to avoid a costly, risky and illegal abortion in favour of giving birth to either raise the child or give it up for adoption. Marital fertility was not targeted with any specific actions and should show minor or zero response to the program. However, we might imagine two forces pulling in opposite directions. First, legal advice in paternity cases could improve the incentive for men to marry their pregnant partner as the likelihood of legal and financial obligations increased with the introduction of family planning. Second, easier access to plausibly less costly pathways for single mothers (adoption and in-kind aid) could decrease the willingness for men to engage in marriage (Akerlof et al., 1996), if fathers care about the utility of the mother and see these services as utility improving for unmarried mothers. The same two forces affect the number of marriages – in particular shotgun marriages – as legal advice in paternity cases could make it more costly for fathers not to marry the mother after birth. If so, then some of the response in non-marital fertility could be traded in to more marriages. The empirical analysis investigates these hypotheses in depth.

3 Empirical Method

The synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015; Abadie, 2019) is a technique to estimate treatment effects in comparative case study settings. The interest in comparative case studies is the evaluation of a treatment (intervention) at an aggregated level (town, regions, country etc.) where treatment occurs at a single point in time and exposes a few units. Another common feature is that the pool of untreated units are different with respect to pre-treatment trends in outcome and other characteristics compared to the treated unit(s) and thus do not constitute suitable control units on their own.

While difference-in-differences (DiD) is the traditional framework to estimate treatment effects when panel data is available and when treatment varies across time and space, I use the synthetic control method instead of DiD for several reasons. First, the introduction of the family planning program in the two medium-sized towns fits the description of a comparative case study. Second, the synthetic control method optimally identifies a control group allowing the counterfactual to be based on a set of control units that most accurately resemble the treated unit prior to treatment. Third, inference in the synthetic control method relies on randomization – and not asymptotic inference – which is suitable in small sample sizes (Conley and Taber, 2011).

The basic application of the synthetic control method is the case of a single treated unit, a number of control units and treatment occurring at a single point in time. In period T_0 the treated unit is exposed to a treatment. J units are untreated and remain so for the entire period of interest. The sample of untreated units is the donor pool. Thus the total sample contains $J+1$ units. These units are observed in periods $T_\tau, T_{\tau+1}, ...T_{-1}, T_0, T_1, ..., T_T$. $T_\tau, ..., T_{-1}$ is the pre-intervention period and $T_0, T_1..., T_T$ the post-intervention period. Let y_{it}^N be the outcome in the absence of treatment and y_{it}^I the outcome under treatment. Before treatment $\{T_\tau, ..., T_{-1}\}$ these outcomes are equal $y_{it}^I = y_{it}^N$ if units do not respond with anticipation to future treatment. The treatment effect for the treated unit in period $t \geq T_0$ is,

$$\alpha_{it} = y_{it}^I - y_{it}^N \tag{1}$$

In practice either y_{it}^I or y_{it}^N is observed. Specifically, in the post-intervention period, it is necessary to estimate y_{it}^N in order to calculate the treatment effect for the treated unit because $y_{it} = y_{it}^I$ for $t \geq T_0$. The post-intervention outcomes for the synthetic control unit can be used as an estimate for the treated units counterfactual outcome. The estimated treatment effects are then,

$$\hat{\alpha}_{0t} = y_{0t} - \sum_{j=1}^{J} w_j y_{jt} \tag{2}$$

The optimal synthetic control unit implies choosing a set of weights that minimize the weighted discrepancy between the treated unit and the synthetic control units pre-intervention outcomes and covariates,

$$W^* = \arg\min \sqrt{(X_I - X_N W)' \, V \, (X_1 - X_N W)} \tag{3}$$

where X_I is a $(K \times 1)$ vector of pre-intervention covariates and outcomes for the treated unit. X_N is a $(K \times J)$ matrix of the same pre-intervention covariates and outcomes for the J units in the donor pool. W is a $(J \times 1)$ vector of weights which are restricted to sum to 1. V is a $(K \times K)$ positive and semidefinite matrix. V assigns weights to each linear combination and reflects how important a specific variable in X is for the prediction of the evolution of the outcome. In practice V is chosen to minimize the root mean squared prediction error (RMSPE) between the pre-intervention outcome for the treated unit and the synthetic control.

Inference Inference in the synthetic control method should reflect uncertainty in the estimated treatment effects. This uncertainty arises due to uncertainty concerning the validity of the synthetic control unit and the implied counterfactual. Abadie et al. (2010) suggest to evaluate significance based on exact inference through placebo or permutation tests. For each unit in the donor pool, I estimate placebo treatment effects. The true treatment effect is significant if it stands out as an extreme event in the distribution of placebo effects. A second graphic representation of inference is to evaluate the distribution of the ratios of post and pre-intervention root mean squared prediction errors (RMSPE) for all units in the donor pool. The estimated treatment effect is significant if the ratio for the treated unit is an extreme observation in the distribution. This approach can be used to calculate rudimentary p-values for the treatment effect by calculating the probability of estimating a pre/post RMSPE ratio at the size of the true effect if treatment was assigned at random.

Validity of the Method The synthetic control method requires three assumptions. First, the treatment of one unit does not spill-over to the outcomes of untreated units (similarly to the stable unit treatment value assumption (SUTVA) in the potential outcomes framework, see Rosenbaum (2007) for details). If violated, the estimated effect cannot be interpreted as the causal effect of the treatment. There exist no formal test to assess this assumption but its validity should be argued on background knowledge from the context of the study. In practice, I deal with this assumption by using aggregation levels where spill-overs between units in the sample are less likely. Specifically, I estimate at both town and county level. In the county specification, I allow for within-county spill-overs but spill-over across counties are not allowed (e.g. selective mobility of pregnant women across counties).

Second, treated units do not respond in anticipation of the treatment. In my case, implementation of the program fell in the same year as the law was passed which limits the scope for the anticipatory behavior.

Third, treated units' counterfactual can be estimated by a weighted average of donor pool units post-treatment outcomes. The validity of this assumption can be evaluated by the quality of the pre-treatment fit between the treated unit and the synthetic control unit. Abadie et al. (2010); Abadie (2019) note that in some cases it may be impossible to construct a suitable synthetic control unit due to poor pre-treatment fit. In such instances, they recommended not to use the method. However, it lies with the researcher to subjectively decide the quality of the fit and whether to proceed with the analysis. I evaluate the fit of the synthetic control unit by graphical inspection and when the pre-treatment fit is bad, I avoid causal claims.

Furthermore, Abadie et al. (2010) mention that interpolation bias may be present even if the pre-treatment fit is good. To minimize interpolation bias, Abadie et al. (2010) recommend that the treated unit(s) and the donor pool should be somewhat comparable prior to implementation of the synthetic control method. This implies that researchers should restrict the donor pool to units with pre-treatment characteristics in the neighbourhood of the treated unit. In order to comply with this recommendation, I evaluate the effects of the family planning program for the two medium-sized towns (Næstved and Sønderborg) while excluding the five largest – and treated – towns.

A critique of the synthetic control method is a lack of guidance when choosing the predictors and their functional forms (Ferman et al., 2018). Ferman et al. (2018) recommend specifications where the number of predicting pre-treatment outcomes increases when the pre-treatment period increases and that the sensitivity of the specification is thoroughly tested.[8] In Section 5.5, I test a range of different specifications as robustness checks to test if the results are sensitive to changes in the specification.

4 Data and Descriptive Statistics

4.1 Sample Construction

I combine data from several sources. From *Causes of Death Statistics*, I obtain demographic variables (population size, number of live births and number deaths) across Danish towns and counties. Counties are administrative regions and may include several towns, villages and rural areas. From *Income and Wealth Tax Records*, I collect annual income and wealth data from

[8] Specifications to avoid: the mean of the pre-treatment outcome on its own and specifications rules such as the first, the middle and the last value of the pre-treatment outcome.

1921-1936 at town-level.[9] From *Business Statistics* – a decennial publication – I obtain data on workforce sector shares. From *Marriages, Births and Deaths* and *Births, Deaths and Population Movements*, I digitize the number of marriages and non-marital live births at county and town level.[10] *The Medical Reports for the Kingdom of Denmark* contain data on aggregated and county-level population and demographic variables that I use in the descriptive data section and in the county-level analysis. The reports also include data on the number of children in different kinds of out-of-home care.

The final panel data set contains yearly observations from 1921-1947 for 27 Danish towns with a population of at least 7,000 and 16 counties (excluding the top-five largest towns and their counties). Appendix Figure A1 shows a map of Denmark and the geographical location of towns in the sample. Sønderborg is remotely located far away from large towns like Copenhagen, while Næstved is closer to Copenhagen. The two treated towns are located far apart. The map also shows that towns in the donor pool are scattered across Denmark.

As outcomes, I use the crude marital birth rate, the crude non-marital birth rate, the share of non-marital births and the number of marriages. I define the crude (non-)marital birth rate as the number of (non-)marital live births per 1,000 people. The share of non-marital births is the number of non-marital births as a share of the total number of live births.

An important data issue arises from the reporting practices used in the majority of years in the sample (1921-1943). In 1921-1943 the number of live births - both non-marital and marital - is reported at birth-place level. In 1944-1947 the numbers are reported both in terms of birth-place and residence. Thus, I have to use birth-place data in the analyses. A significant treatment effect in the birth-place measured birth rates can be caused by two potential channels; i) changes in fertility and/or ii) changes in the number of births in the town by women not living in it. Though, home births were the norm at the time (Vallgårda, 1996) supervised births outside of home cannot be excluded a priori. I try to pin down the exact channels by estimating the treatment effects at both town and county level in order to reduce the discrepancy between birth-place and residence. Counties are larger geographical areas than towns. A county includes rural upland and often several towns. Thus, pregnant women would have to travel beyond their home-county at the time of birth in order for me not to be able to interpret the county-level

[9] In 1936 the outlook of the publication changes. Therefore, I stop in 1936 which also coincides relatively well with treatment initiation in 1939.

[10] From 1910-1925 these data are available in *Marriages, Births and Deaths* and from 1934-onward in *Population Movements*. In the intermediate period (1930-1933) marriage and non-marital live birth data are collected directly from original documents at The Danish National Achives, Statistics Denmark. The remaining years (1926-1929) are interpolated using the number of total live births and population.

results as actual fertility effects.

The synthetic control method uses both pre-treatment outcomes and relevant pre-treatment covariates (correlated with the outcome) in the construction of the synthetic control unit. As pre-intervention covariates, I use income and wealth per capita (yearly), the population density and population size (selected years and averaged), industry workforce share (selected years) and the share of women (selected years). Income and wealth are important predictors for fertility, (Becker, 1981). The share of women places a natural restriction on fertility. Higher population density decrease fertility through access to education, better infrastructure and health services (De la Croix and Gobbi, 2017). The industry workforce share might proxy the degree of industrialization and development at town-level. These measures might predict the development of fertility and also the responsiveness of the treatment. Furthermore, I include pre-intervention outcomes as predictors.

4.2 Summary Statistics

Table 1 shows summary statistics for the two treated towns and the donor pool. There are relatively large differences between both treated towns and the donor pool. Towns in the donor pool are more densely populated compared to the treated towns. Income and wealth per capita are relatively similar in the treated towns and in the donor pool. Both treated towns are more industrialized than the average donor town but with smaller populations. The bottom-four rows shows means for the pre-intervention outcomes. Sønderborg has higher non-marital and marital birth rates compared to Næstved and the donor pool. Næstved and the donor pool have similar non-marital and marital birth rates before the family planning program. The share of non-marital births is 8 % in both treated towns and the donor pool while the marriage rates are slighter higher in the treated towns.

Sønderborg is remotely located far away from large towns like Copenhagen, where privately run organizations supporting unmarried women were already present, while Næstved is closer to Copenhagen (see Appendix Figure A1 for a map of Denmark and the geographical location of towns in the sample). A key feature in Sønderborg was that a public birth ward opened in 1934 (Medical Report for the Kingdom of Denmark, 1934). Sønderborg was the first town outside the five largest towns to have a public birth ward. The birth ward might have been important in the allocation of the family planning program in Sønderborg as the presence of well-developed health care infrastructure was a factor in the allocation process (The Population Commission, 1938). Furthermore, Appendix Figure A1 shows that towns in the donor pool are scattered across Denmark.

Tab. 1 Pre-intervention descriptives for treated towns and donor pool

	Sønderborg	Næstved Means	Donor pool
Population density	53.89	85.73	116.85
Income per capita	992.33	943.24	898.35
Wealth per capita	2235.27	2040.43	2205.06
Female share	0.53	0.52	0.53
Industry workers per 1000	185.50	212.50	166.70
Population	10607.72	11229.72	14382.51
Non-marital birth rate	1.97	1.56	1.53
Marital birth rate	22.29	17.19	17.47
Share of non-marital births	0.08	0.08	0.08
Marriages per capita	9.26	9.18	8.46

Notes: The pre-intervention period is 1921-1938. Income and wealth per capita are averaged across 1921-1936. Industry workers per capita are averaged across 1925 and 1935. Women shares are averaged across 1921 and 1925 and across 1930 and 1935 and population densities are averaged across 1925, 1930 and 1935. Population is averaged over 1921-1938. The means for the non-marital and marital birth rate, share of non-marital births and the marriage rate are averages across 1921-1938. The donor pool includes 25 towns. From the original sample of 32 towns, I exclude the top 6 largest Danish towns and the two treated towns.

4.3 Descriptive Evidence

Figure 2 presents the relationship between population size and the number of non-marital births in averages before and after the family planning program for all towns in the donor pool and the two treated towns. Panel (a) shows the number of non-marital births reported at birth-place level and thus uses data for the entire period. Panel (b) shows the same relationship but uses data reported at residence-level in the post-intervention period and therefore only utilizes a subset of the sample period.[11]

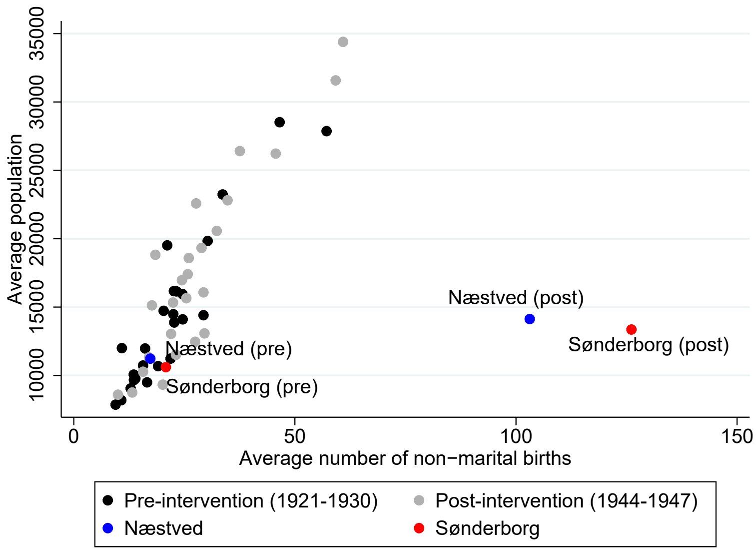
Figure 2 shows that population size and the number of non-marital birth follow a close linear relationship. Prior to the program the number of non-marital births in the treated towns lies within the linear relationship. After treatment both treated towns are to the right of the relationship between population and non-marital births - even when measured at residence-level in panel (b). Compared to control towns with the same population, the treated towns have almost 100 more non-marital births. Appendix Figure A2 shows the same relations for the number of marital births. Only Sønderborg - and not Næstved - breaks the linear relationship between population size and marital births in the post-treatment period and only when measured

[11] In the residence-level plot in panel (b), I reduce the pre-intervention period to 1921-1930 in order to reduce the potential effect of mobility of pregnant women from the areas surrounding the towns while the post intervention period is reduced to 1944-1947 where residence-level data is available.

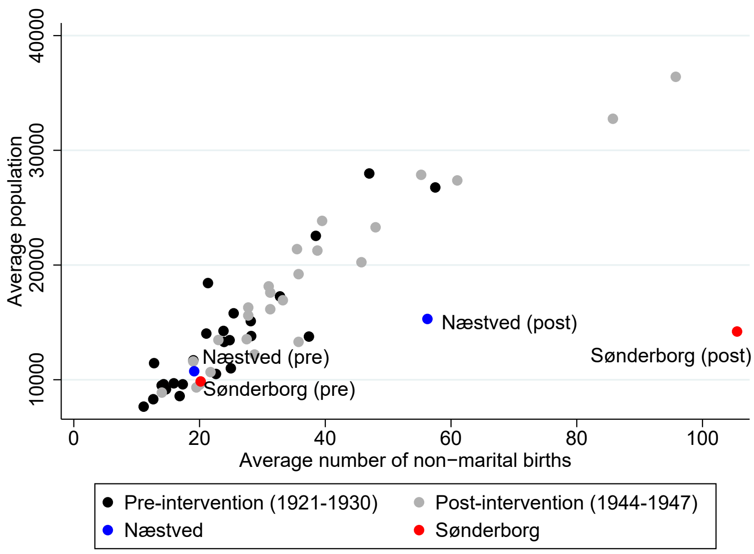
at birth-place level. A possible explanation for this might be the introduction of a birth ward in Sønderborg in 1934 (to be assessed in Section 5.2). At residence-level neither towns break the linear relationship suggesting that marital fertility was unaffected by the program.

Fig. 2 Pre/post intervention relationship between population size and non-marital births

(a) Reported at birth-place level



(b) Reported at residence level



Notes: Own calculations based on data from Marriages, Births & Deaths, Population Movements and The Causes of Death Statistics (all various years). The pre-intervention period is 1921-1938 and the post-intervention period is 1939-1947. In panel (b) the non-marital birth at residence-level averages are taken from 1921-1930 and 1944-1947 due to data limitations. See the data section for further details. Blue dots are observations for Næstved and red dots are Sønderborg.

5 Results

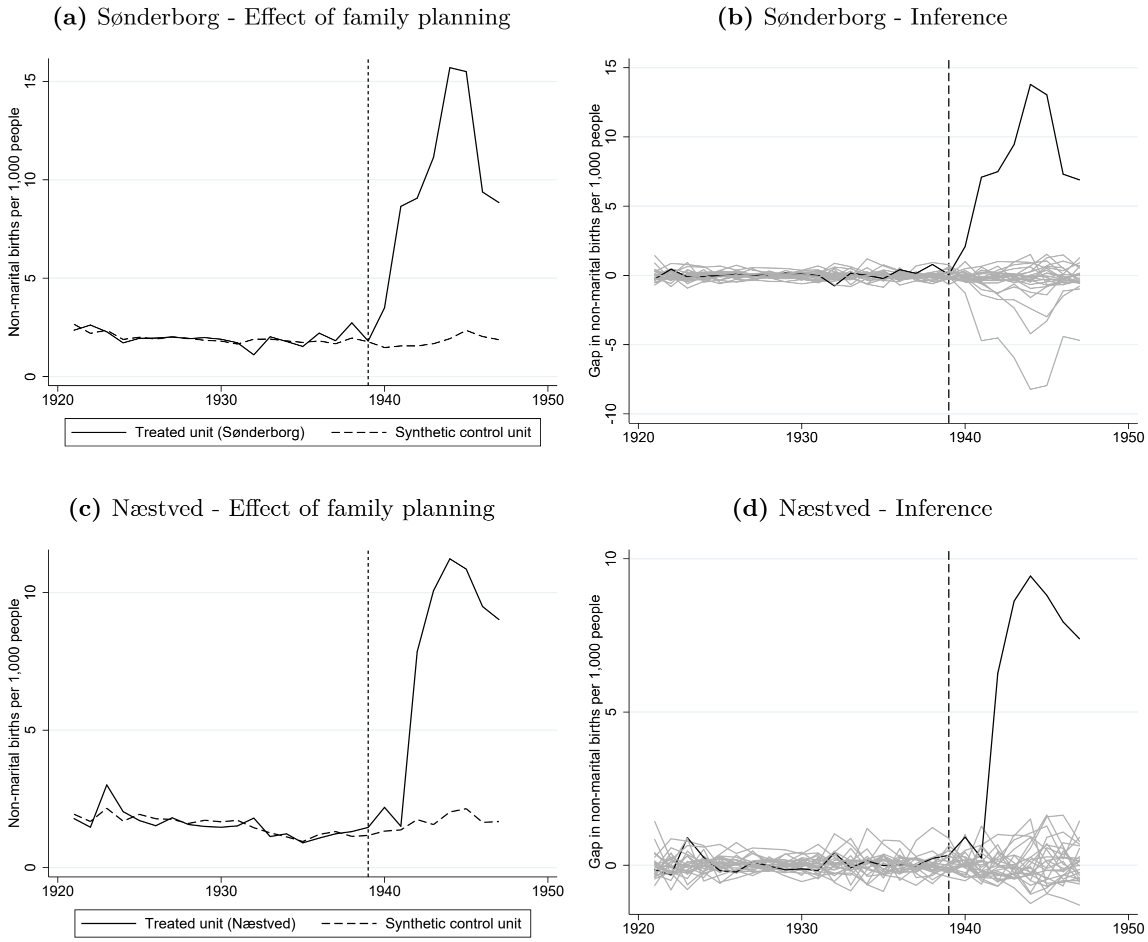
5.1 Non-marital Birth Rate

Figure 3 displays the evolution of the crude non-marital birth rate at town level for the two treated towns and their synthetic control towns and show that the family planning program increased the non-marital birth rate in both treated towns. Overall the effects of the family planning program in the two towns are similar. The crude non-marital birth rate is fairly constant at 2 births per 1,000 people prior to the program. Shortly after treatment initiation the crude non-marital birth rate in the treated towns start diverging from the synthetic controls. The gab widens until 1945 from where it narrows a bit. In 1945 the number of non-marital births per 1,000 people was above 15 in Sønderborg and above 10 in Næstved. These are fairly substantial treatment effects of access to the family planning program. The right graphs of Figure 3 display randomization inference and the dynamic treatment effects. The effects are highly significant as the true effects are extreme events in the distribution of placebo effects.

The effects in Figure 3 might be caused entirely by an influx of pregnant unmarried women from surrounding areas into the treated town at the time of birth. To detect if the family planning program increased non-marital fertility, I increase the aggregation level to counties. Figure 4 presents the estimated treatment effects on the non-marital birth rate at county-level. The family planning program significantly increased the non-marital birth rate at county-level. The treatment effects are between 1-2 non-marital births per 1,000 people when estimated at county-level and the effects are significant as shown in the right panels. The effect on the non-marital birth rate is smaller in Næstved than Sønderborg. The results indicate actual non-marital fertility responses and are in line with the content of the program: to increase fertility by targeting abort-seeking - and often unmarried women - and advising them to give birth.[12] Appendix Tables A1-A2 show the synthetic control weights for all combinations of fertility outcomes (non-marital birth rate, marital birth rate and non-marital birth share), treated unit (Sønderborg and Næstved) and aggregation level (town and county). In all estimation, the weights are sparse, i.e. most assigned weights are zero and relatively few control units contribute to the synthetic control which is typically not the case in DiD regressions (Abadie, 2019). Sparsity allows for an accurate interpretation of the estimated counterfactual.

[12] Figure A3 in the Appendix provide further support that the estimated effects are significant. Figure A3 contain inference based on the ratios of RMSPE for the effects on non-marital birth rate at both aggregation levels in both treated areas.

Fig. 3 Evolution of the crude non-marital birth rate at town level



(a) Sønderborg - Effect of family planning

(b) Sønderborg - Inference

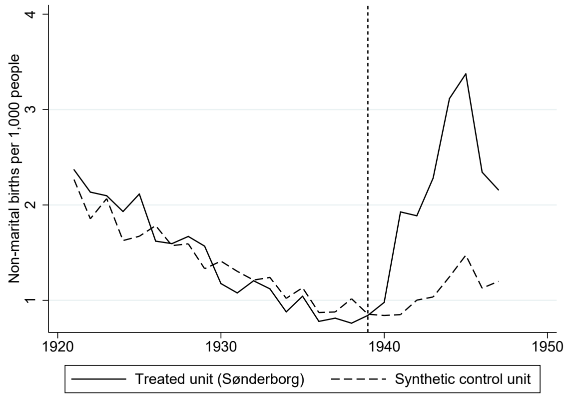
(c) Næstved - Effect of family planning

(d) Næstved - Inference

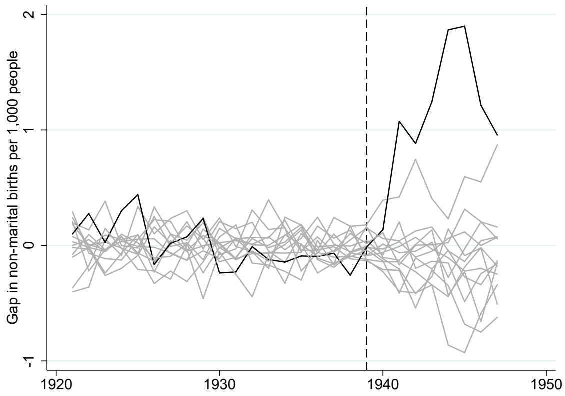
Notes: The treatment effect of the family planning program is estimated using the synthetic control method. The pre intervention period is 1921-1938 and post-intervention 1939-1947. The following variables act as covariates; Income and wealth per capita are averaged across 1921-1936. Industry workers per capita are averaged across 1925 and 1935. Women shares are averaged across 1921 and 1925 and across 1930 and 1935 and population densities are averaged across 1925, 1930 and 1935. Population is averaged across 1921-1938. The pre-intervention outcome is included as a predictor for the years 1922,1924, 1928, 1930, 1934-1938. The donor pool includes 25 towns. Right panels show inference based on placebo testing.

Fig. 4 Evolution of the crude non-marital birth rate at county level

(a) Sønderborg - Effect of family planning

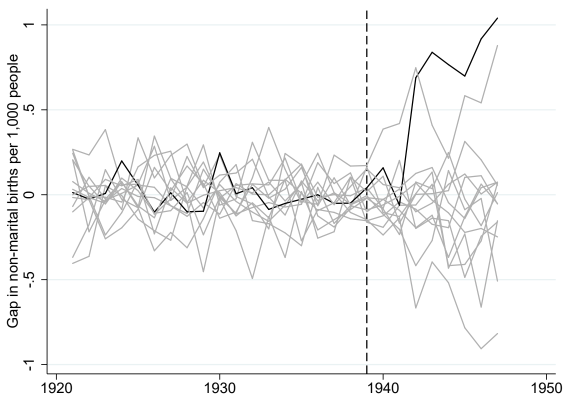


(b) Sønderborg - Inference



(c) Næstved - Effect of family planning

(d) Næstved - Inference



Notes: The figures shows treatment effects and inference of the family planning program. The unit of treatment is counties. Panels (a) and (b) show results for Sønderborg and panels (c) and (d) for Næstved. As covariates average population and yearly outcomes are used. Right panels shows placebo inference where the black solid line indicate the true treated county. The donor pool includes 14 untreated counties.

The estimated number of additional children born outside of marriage in the treated counties can be calculated based on these results.[13] In the county of Sønderborg, 821 children were born from 1939-1947 caused by the increase in non-marital fertility. In the county of Næstved the number is 529 children. In total, the estimate across the two counties is 1,350 children.

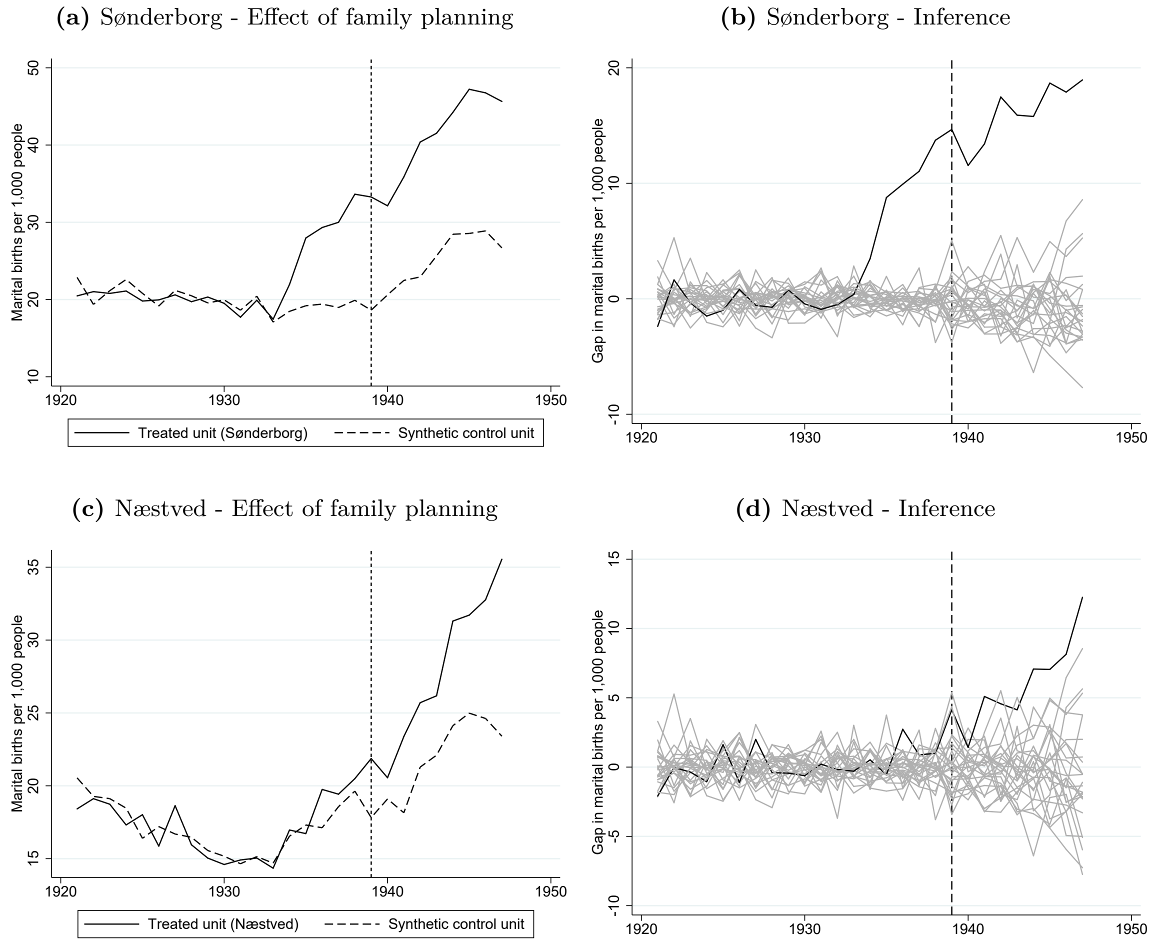
5.2 Marital Birth Rate

Figure 5 shows the synthetic control analysis for the marital birth rate at town-level. In panel (a), a gap opens for Sønderborg between the two groups from 1934 - five years before treatment. The

[13] For each year after the introduction of the program, I multiply the estimated effects α_{0t} with the population at time t. Afterwards, I accumulate the annually estimated non-marital births caused by the program. This total is an estimate of the number of children born outside of marriages from 1939-1947 as a consequence of the family planning program.

gap coincides with the construction of the birth ward. I cannot conclude how the marital birth rate in Sønderborg was affected by the family planning program due to the poor pre-treatment fit of the synthetic control.[14] In Næstved, there is evidence of a positive effect on the marital birth rate reaching above 10 marital births per 1,000 people in 1947. From a pre-treatment level of 20 marital births per 1,000 people this corresponds to a 50 % increase. However, the divergence between Næstved and the synthetic control occurs prior to treatment.[15] Overall, the town-level results do not provide enough evidence for me to establish whether the program affected the marital birth rate.

Fig. 5 Evolution of the crude marital birth rate at town level



Notes: See notes to figure 3.

To investigate if marital fertility responded to the program, I estimate the treatment effects

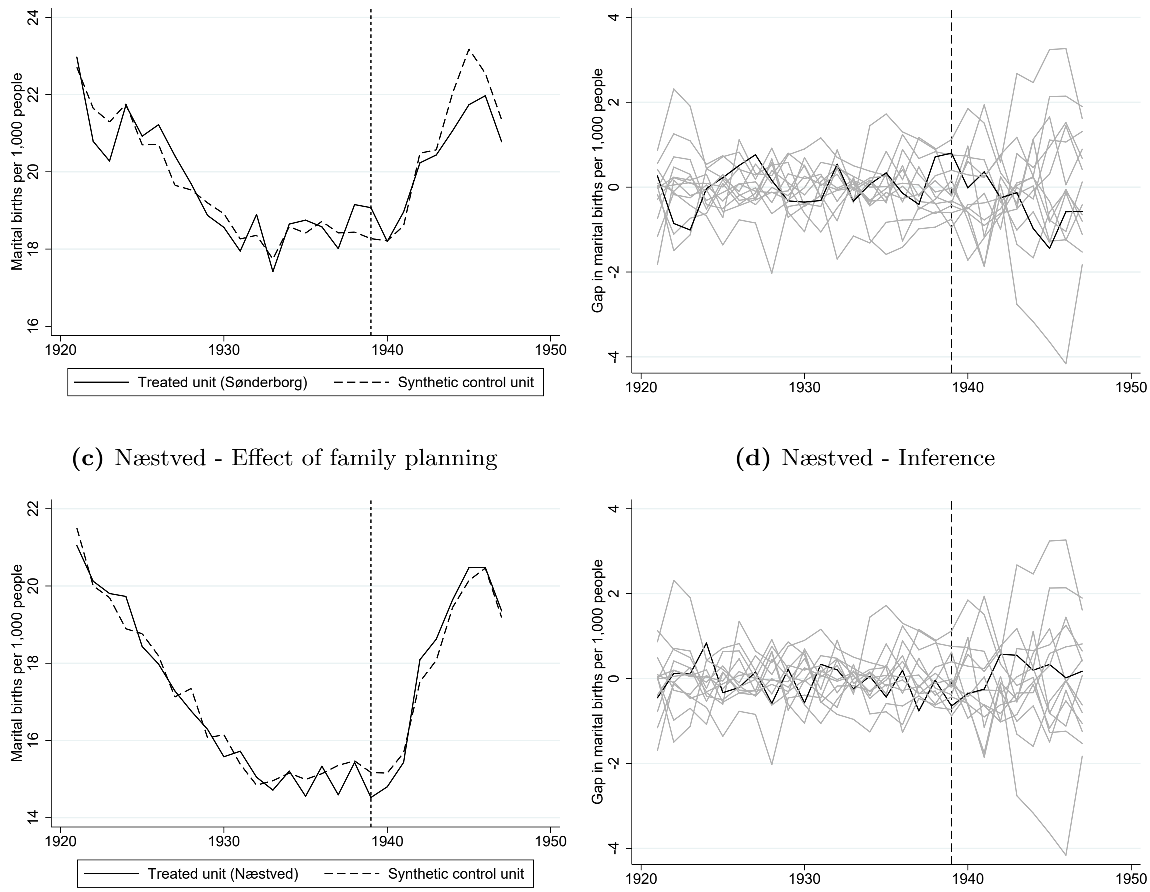
[14]The estimated effect is not significant based on inference from the ratios of RMSPE as presented in the Appendix Figure A4.

[15]Figure A4 panel (b) shows inference from the ratios of RMSPE. The probability (p-value) of estimating a ratio the size of the treated town or larger by random treatment assignment is $2/26 = 0.08$ which is borderline significant on usual confidence levels.

using county-level data. Figure 6 shows the results: In neither of the treated counties did the crude marital birth rate responds significantly. This allows me to conclude three things: i) marital fertility was unchanged by the program, ii) increases in the town-level marital birth rate is caused by within-region movements of pregnant women into the treated town and iii) the 1934 birth ward in Sønderborg only attracted pregnant women from the surrounding municipalities but had no effect on fertility.

Fig. 6 Evolution of the crude marital birth rate at county level

(a) Sønderborg - Effect of family planning
(b) Sønderborg - Inference



(c) Næstved - Effect of family planning
(d) Næstved - Inference

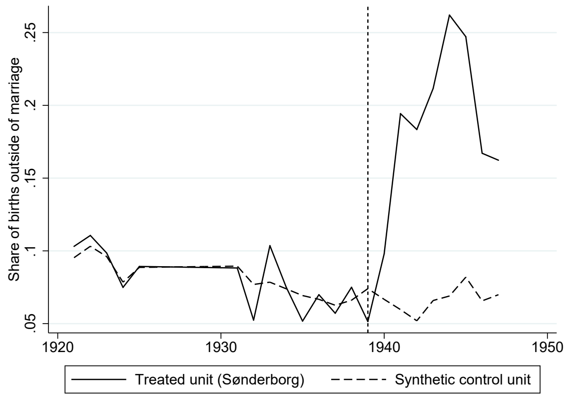
Notes: See notes to figure 4.

5.3 Non-marital Birth Share

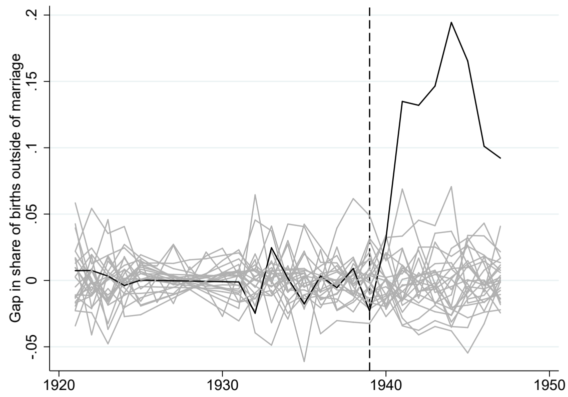
Non-marital fertility increased and marital fertility was unchanged in response to the program. Together, this implies that the share of non-marital births must have increased relatively in the treated towns. Figure 7 shows how the family planning program affected the share of non-marital births. The evidence supports the previous patterns as the share of non-marital births in the treated towns went from 6-7 % to over 20 % while the share stayed roughly constant in the synthetic control towns. The effects are highly significant as shown in the right panels as non of the placebo runs are close to matching the magnitude of the true effects. The fact, that I once again obtain the same results across the two treated towns are strong indications that the effects are caused by the family planning program.

Fig. 7 Evolution of the share of non-marital births at town level

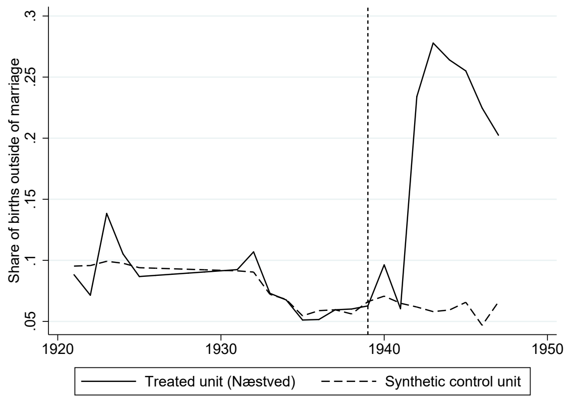
(a) Sønderborg - Effect of family planning



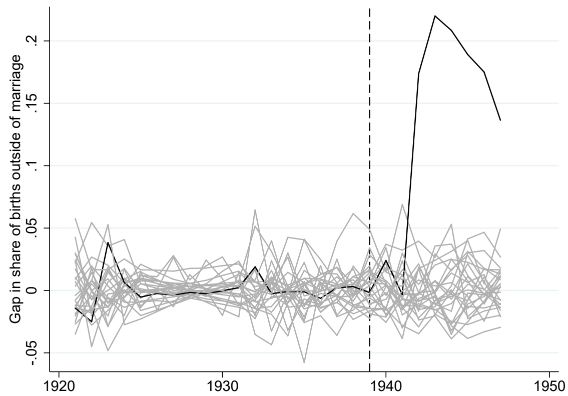
(b) Sønderborg - Inference



(c) Næstved - Effect of family planning



(d) Næstved - Inference



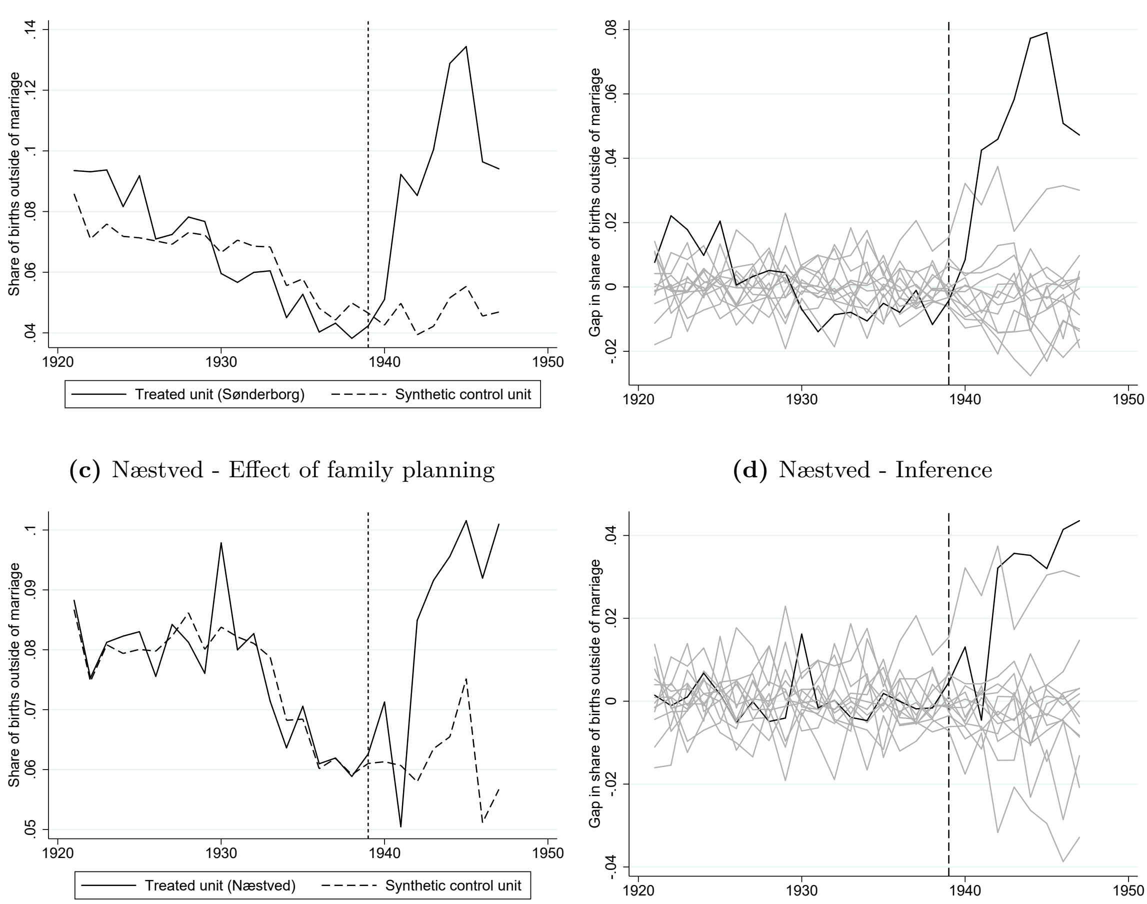
Notes: See notes to figure 3.

The town-level results might be biased if the degree of mobility of non-residential pregnant women differs across married and unmarried women. To account for this, I estimate the effects on the share of non-marital births at county-level presented in Figure 8. At county-level the estimated effects range from 5-8 %-points in Sønderborg and 3-4 %-points in Næstved.[16] At town-level the estimated effects are homogeneous across the two towns. The heterogeneous effects at county-level are caused by mobility from pregnant non-residential women which seems to be more prevalent in Næstved than Sønderborg. Figure A5 shows the post-treatment average

[16] Inference based on RMSPE supports that all the estimated effects are significant as shown in Appendix Figure A7.

non-marital birth rate at both birth-place and residence-level for donor pool towns and the two treated towns.[17] The difference between the birth-place and residence-level birth rates indicate the degree of mobility from non-residential pregnant women. The figure shows that this type of mobility from unmarried pregnant women is more prevalent in Næstved than Sønderborg.[18]

Fig. 8 Evolution of the share of non-marital births at county level



(a) Sønderborg - Effect of family planning

(b) Sønderborg - Inference

(c) Næstved - Effect of family planning

(d) Næstved - Inference

Notes: See notes to figure 4.

5.4 Marriages

I estimate the effects on the marriage rate to determine if the increase in non-marital fertility is subsequently traded into more marriages. It might be that even though born outside of marriage, the parents marry after the childbirth. Such findings would moderate the conclusions made earlier. If not, then the extra number of children born outside of marriage grew up in

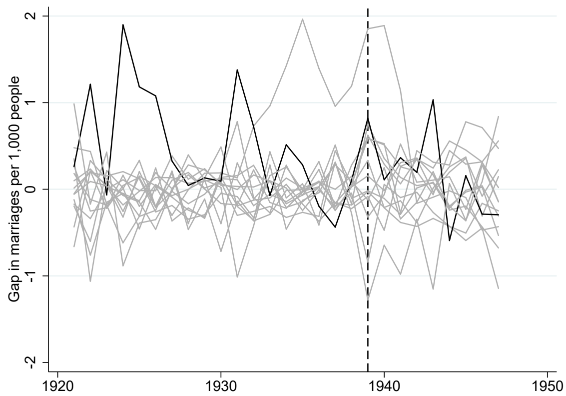
[17] The statistics are calculated based on data from 1944-1947 due to data availability. For more information consult the data section.

[18] Appendix Figure A6 shows that marital births from non-residential women are a much more common phenomenon in Sønderborg than Næstved most likely caused by the birth ward.

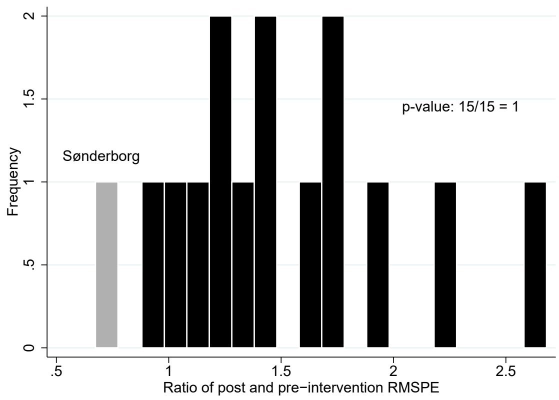
non-marital arrangements or with adoptive parents. Figure 9 shows the estimated treatment effects on the number of marriages per 1,000 people along with inference.[19] In both counties there are no significant evidence to suggest that the number of marriages increased in response to the program. The estimations for Sønderborg - in panel (a) - are noisier and the pre-treatment fit is bad. Nevertheless, none of the treated counties experienced significantly different marriage rates compared to their synthetic controls. This indicates that the children born, as non-marital fertility increased, grew up in non-marital living arrangements. In section 6, I explore this further.

Fig. 9 Evolution of the marriage rate at county level

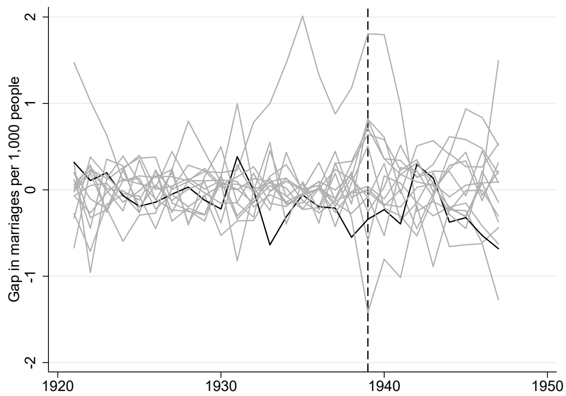
(a) Sønderborg - Effect of family planning and placebo inference



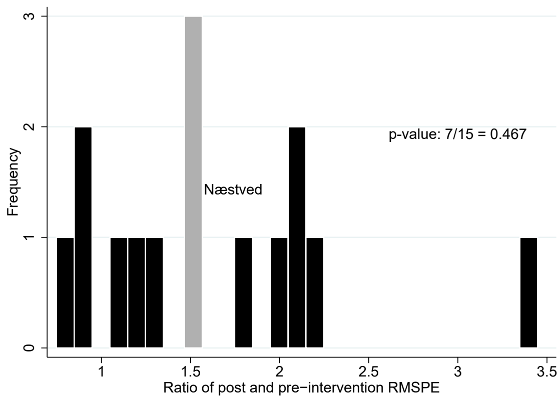
(b) Sønderborg - RMSPE inference



(c) Næstved - Effect of family planning and placebo inference



(d) Næstved - RMSPE inference



Notes: The figures shows the estimated treatment effects on the number of marriages per 1,000 people from the 1939 family planning program along with placebo and RMSPE based inference. The black solid lines in the left panels are the treatment effects for the true treated counties. The pre-intervention period is 1921-1938 and post-intervention 1939-1947. As pre-intervention covariates I include averages of population size and non-marital fertility and yearly outcomes. The donor pool includes 14 untreated counties. In right panels are inference based on ratios of the RMSPE. The gray bars indicate the treated counties.

[19] I estimate the effect at county-level as that is the aggregation level where I draw my fertility conclusions from. Town-level results are available on request.

5.5 Robustness

I perform several robustness checks to assess the sensitivity of the results. First, I estimate placebo effects on outcomes that are unrelated to the treatment. Null effects strengthen the causal interpretation of the estimated fertility effects while significant effects indicate that other uncaptured differences between the treated and synthetic control units might explain the previous results.[20] Appendix Figure A8 shows results on two placebo outcomes; 1) The overall death rate and 2) the death rate from suicides, homicides and accidents. I find no significant effects in either placebo tests.

I test the sensitivity of the synthetic control specification using five different specifications. [21] For each specification, I rerun the synthetic control procedure and compare the estimated treatment effects. Ideally, the estimated treatment effects and inference should be approximately equivalent regardless of specification. Appendix Figure A9 shows the evolution of the non-marital and marital birth rate at town and county-level for Sønderborg along with counterfactuals estimated with the synthetic control method for the alternative specifications. All specifications produce virtually identical counterfactuals at both aggregation levels and for both outcomes. The reason is that the weights which form the synthetic control from the donor pool do not change much across specifications. Appendix Table A3 shows the correlation between weights across the alternative specifications. The exact specification of the predictors used in the construction of the synthetic control unit is not central to the results. Regardless of the predictor set, the weights, the counterfactual and the estimated effects are stable.

Finally, I compare the synthetic control findings to those produced by the DiD approach. I estimate the treatment effects with and without controlling for covariates. I run the regressions for three outcomes: i) non-marital birth rate, ii) marital birth rate and iii) share of non-marital births. Moreover, I estimate event studies to evaluate the common trend assumption. Appendix Table A4 presents the DiD results. The estimates generally support the conclusions from the synthetic control analysis. However for the marital birth rate, the estimates are highly significant at town-level while being insignificant in the synthetic control analysis. DiD hinges on the

[20] The placebo estimations are only carried out and showed for Sønderborg. The same set of results for Næstved are available on request.

[21] The five specifications are: 1) Pre-treatment outcome values for all years, 2) the first 3/4 of the pre-treatment outcome values, 3) the first half of the pre-treatment outcome values, 4) odd pre-treatment outcome values and 5) even pre-treatment outcome values. Note, that these five specifications are different from the one I use in the main specification. My specification is more sporadic in the middle of the sample and denser closer to the intervention and at the start of the sample and therefore lies somewhere in between these five alternatives. I do the sensitivity analysis with Sønderborg as example. Results for Næstved are available on request.

common trend assumption. To evaluate the common trend assumption the applied literature usually estimates event studies. Appendix Figure A10 shows event studies for the non-marital and marital birth rate and the share of non-marital births at town- and county-level. The event studies for the non-marital birth rate show that the pre-treatment estimates are small and mostly insignificant (although at town-level some pre-treatment estimates are slightly negative and significant) and the subsequent treatment dynamics resemble those obtained from synthetic control. For the marital birth rate, the pre-treatment estimates are significantly negative and trending at town-level. With the share of non-marital births as outcome, the event studies produce insignificant pre-treatment estimates at both aggregation levels. At county-level there is a tendency towards positive and significant estimates in the pre-treatment period. The DiD results underline two things. First, the overall evidence supports those obtained from the synthetic control analysis. Second, the event studies suggest differences in the trends between treatment and control prior to treatment which invalidates causal inference as the unobserved components cannot be differenced out. This suggest that the synthetic control method is a more appropriate tool to match the unobserved component by choosing the most suited control group.

6 Mechanisms

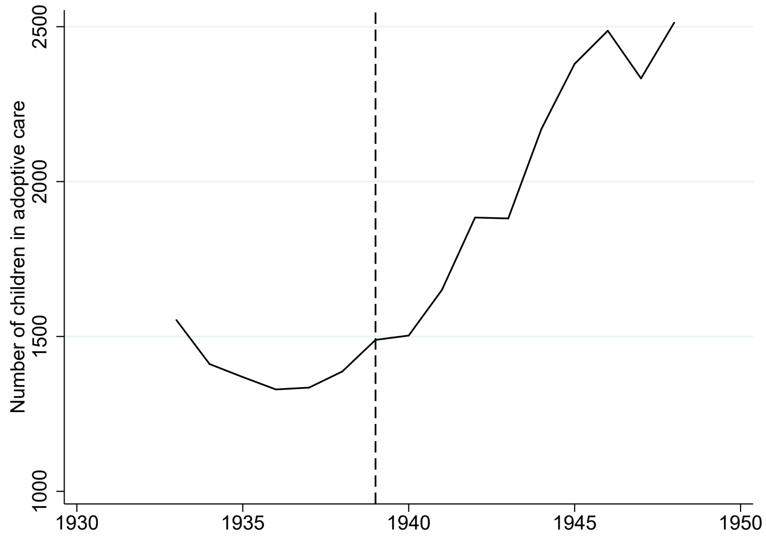
In this section, I explore possible mechanisms for my main results. Specifically, I investigate whether the strategy of the family planning program plausibly explains the fertility effects. The family planning program had a policy that preferred adoptions over abortions if the circumstances were not too severe. Moreover, the program offered in-kinds (milk, food, clothes) to single mothers who decided to keep the child. If these alternatives were viable substitutes to illegal abortions there should be abrupt increases in the number of children living in non-marital households and/or in adoptive care in the period following the introduction of the program.

Adoption data is not detailed enough to perform a synthetic control estimation. Neither is data on abortions since these were illegal and not registered even at aggregated level. Country-level data on adoptions is available from 1933 and onward (The Medical Reports for the Kingdom of Denmark). The number of children in foster care and orphanages and living in households where the parents are unmarried are available from the same source for the entire period.

Figure 10 plots the number of children in adoptive care from 1933-1948 in Denmark. The number of children in adoptive care increased from 1,500 to 2,500 children from 1933-1948.

The introduction of the family planning program coincides with the increase in the number of children in adoptive care. This is suggestive evidence that the adoption policy in the family planning program worked and that increased adoption options were a mechanism for the increase in non-marital fertility.

Fig. 10 The number of children in adoptive care 1933-1948



Notes: The line plots the number of children living in adoptive care each year from 1933-1948 at aggregate country level. Source: The Medical Report for the Kingdom of Denmark (various years).

The number of children living in households with unmarried parents increased in the aftermath of the introduction of the family planning program suggesting that not all children born outside of marriage were given up for adoption by their biological mother (see Appendix Figure A11). This is in line with the services provided by the family planning program. Legal aid in paternity cases and non-financial aid (milk, food and clothes) made it easier to keep the child for single mothers. The number of children in foster care and orphanages does not seem to change its pre-intervention trend in the post-treatment period (see Appendix Figure A11). This is not surprising as the program focused on adoptions and not foster care or orphanages as alternative to abortion.

The welfare effects of less illegal abortions are difficult to evaluate from the perspective of the children. For the mothers, studies show associations between abortion and subsequent mental health disorders (Fergusson et al., 2006, 2008) but it is unclear how adopting away affects mental health relative to abortion.[22] Moreover, the birth and subsequent raising of an unwanted child

[22]Some studies investigate the psychological effects of openness in the adoption process and find that an increase in openness (e.g. phone calls, visitation, involvement in choosing the adoptive parents) has positive effects on mental health (Cushman et al., 1997). In terms of child well-being, Case and Paxson (2001) show that adoptive

is not cost-free (Gipson et al., 2008; Miller et al., 2020). While relevant, these considerations were secondary to the policy makers at the time as their primary focus was to address the low fertility and halt illegal abortions.

7 Conclusion

Most family planning programs provide information and supply contraceptives, such as condoms and birth control pills, and abortion services. The political target of these programs is to reduce population growth and the frequency of unwanted pregnancies and births. The current state of the literature documents robust evidence that such programs reduce fertility by up-takes in both contraceptives and abortions. In contrast, in the 1930's Denmark the demographic issue was low and stagnating fertility. Thus, the family planning program introduced in 1939 was designed to increase fertility by improving the conditions for unmarried pregnant women and advising against illegal abortions. The program was introduced in the five largest towns and in two medium-sized towns. The two medium-sized towns form ideal natural experiments to study the causal effects of the program.

I combine several historical data sources to build a panel dataset of Danish towns and counties from 1921-1947. Using the synthetic control method, I estimate the causal effects on marital and non-marital fertility from the family planning program. The results show that non-marital fertility increased significantly in response to the program while marital fertility was unaffected. This heterogeneous impact is a consequence of the intentions of the program as the services provided by the program were largely focused on unmarried pregnant women.

The effects are comparable across the two treated towns minimizing the likelihood that other factors drive the estimated effects. Marital and non-marital live births were reported at birth-place level at the time. The consequence of this reporting practice is that the very large town-level effects might not be actual fertility responses but caused by pregnant women from neighbouring areas migrating to the treated town at the time of birth. To test for actual non-marital fertility responses, I increase the aggregation level to counties. The county-level results show that non-martial fertility did in fact increase. The results show that particularly women with unplanned pregnancies respond to the program. Married pregnant women – where the pregnancy is more likely to be wanted and planned – do not respond to the incentives provided

children are equally well-off as children raised by a biological mother while Bramlett et al. (2007) find that adoptive children do have worse health and cognitive development but receive more parental investments compared to biological children.

by the program. 1,350 children were born by unmarried women in the two treated areas caused by the program. In line with the policy of the program, the number of children in adoptive care increased in the years after the introduction.

The results are consistent with changes to the timing of birth-giving as lifetime fertility might be unchanged. Nevertheless, the 1939 Danish family planning program could have had long-term effects. David et al. (1990) discuss the differences between Denmark and the US in approaches to family planning. Denmark has less unwanted pregnancies and induced abortions compared to the US. According to David et al. (1990) two factors explain the differences: i) a more positive public opinion on sexuality and ii) the universality of family planning services. The universal principle in family planning services originated with the program from 1939. Furthermore, the program also actively worked on the public perception to break the stigma around non-marital pregnancies and births. The large effects on non-marital fertility, I document in this paper, prove a societal impact which could have long-lasting effects contributing to these factors.

The evidence from this study – coupled with the existing literature – show that family planning programs can affect fertility in either direction depending on the content and context of the program.

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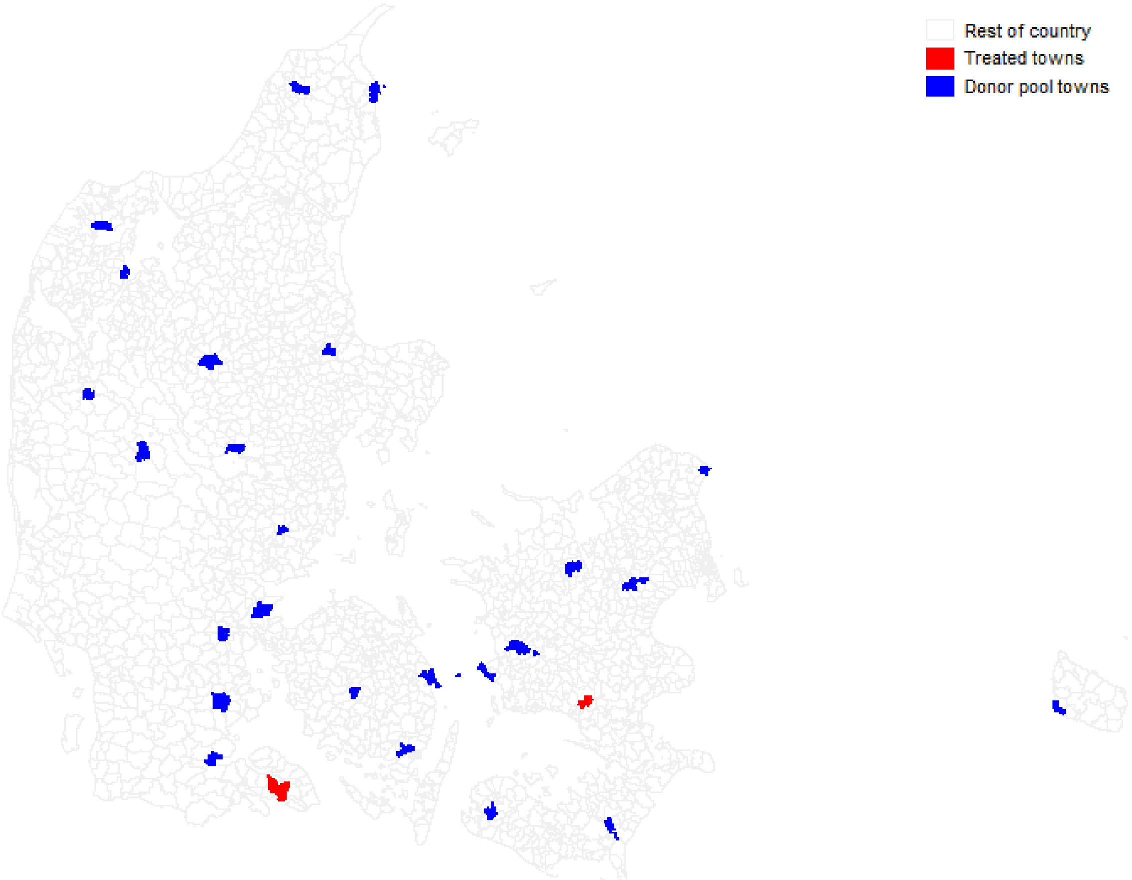
A Appendix

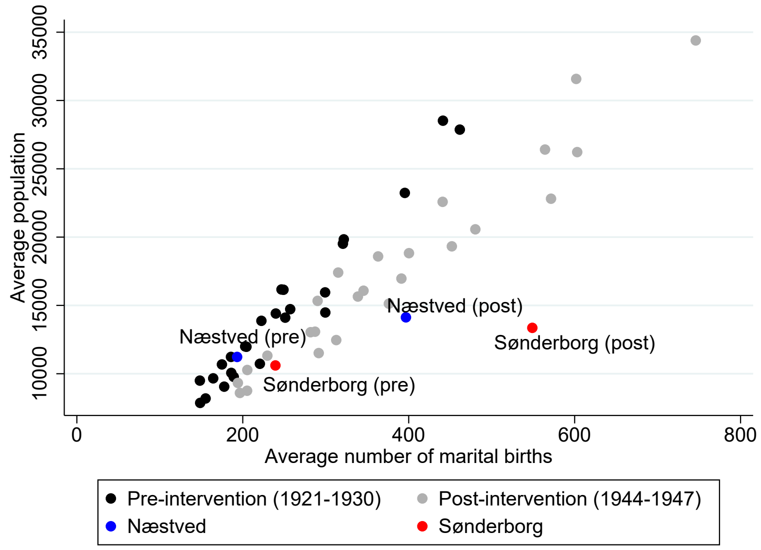
Fig. A1 Map of Denmark and geographical location of towns in the sample



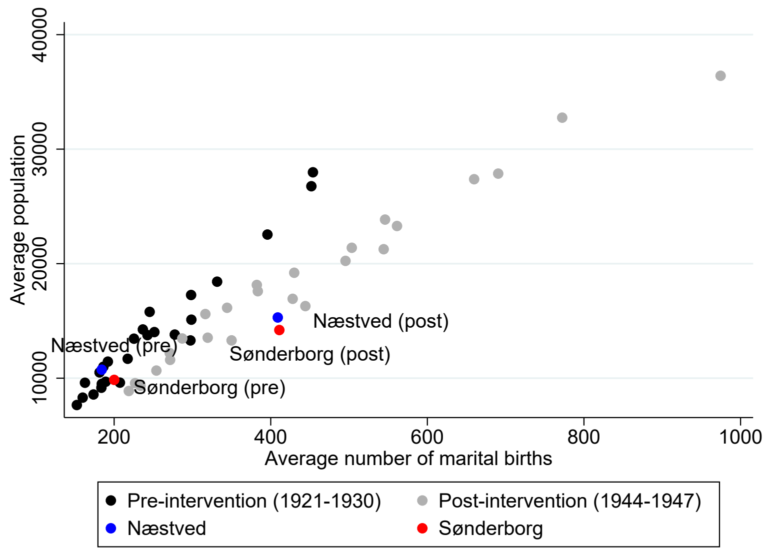
Notes: Towns in red are the treated towns used in the analysis. Towns in blue are donor pool towns. Areas in white are the rest of country.

Fig. A2 Pre/post intervention relation between population and marital births

(a) Reported at birth-place level



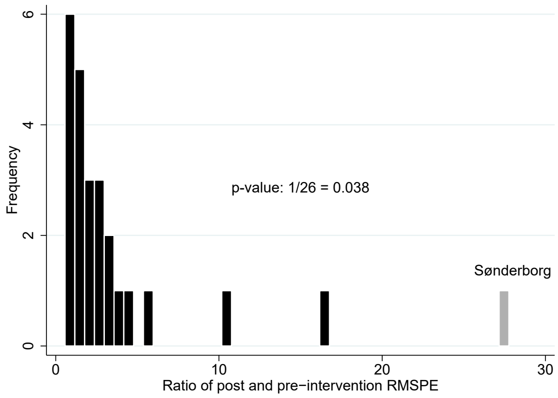
(b) Reported at residence level



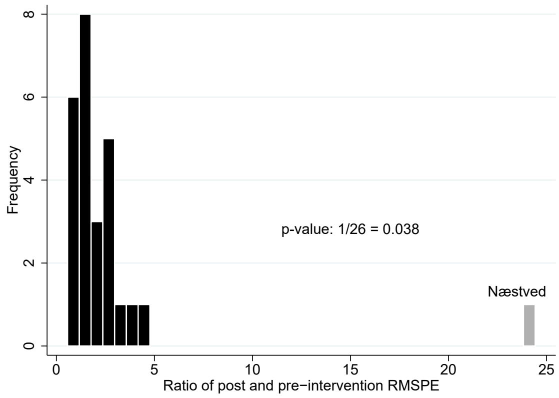
Notes: Own calculations based on data from Marriages, Births & Deaths, Population Movements and The Causes of Death Statistics (all various years). The pre-intervention period is 1921-1938 and the post-intervention period is 1939-1947 in panel (a). In panel (b) the periods are 1921-1930 and 1944-1947 due to data limitations. See the data section for further details. Blue dots are observations for Næstved and red dots are Sønderborg.

Fig. A3 Inference based on RMSPE ratios - Outcome: Non-marital birth rate

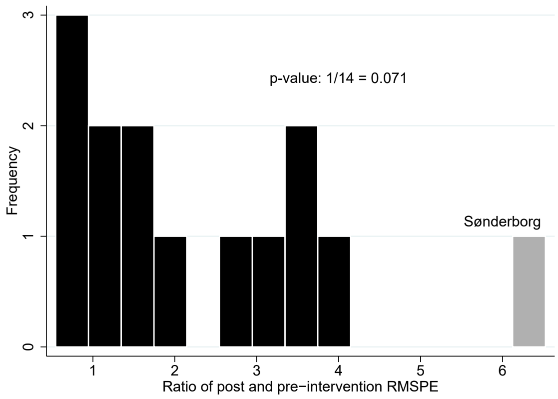
(a) Town-level: Sønderborg



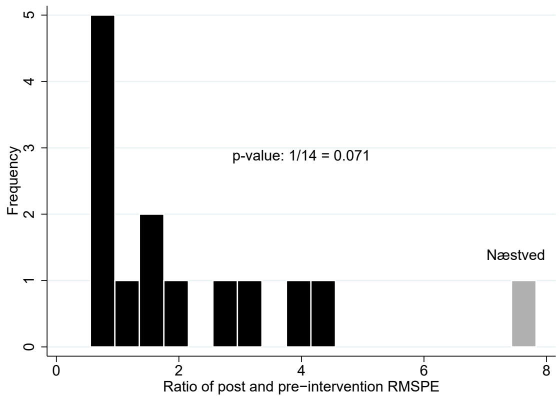
(b) Town-level: Næstved



(c) County-level: Sønderborg



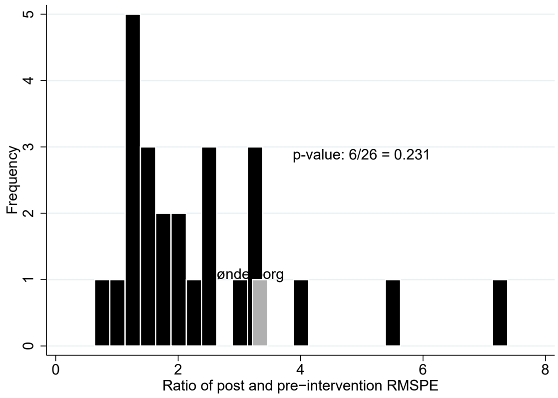
(d) County-level: Næstved



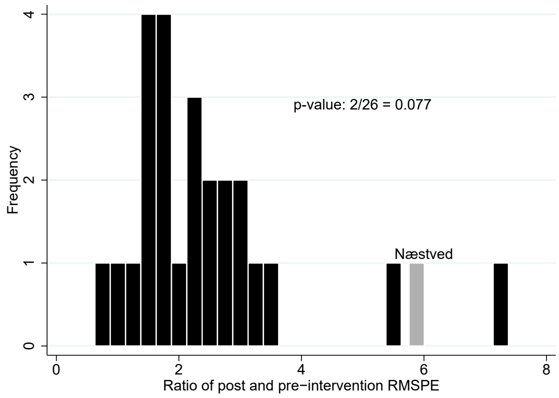
Notes: The bars are the ratios of RMSPE in the post and the pre-intervention period for each unit in the donor pool. A large value relative to the distribution indicate significance. The grey bar en each plot is the treated unit.

Fig. A4 Inference based on RMSPE ratios - Outcome: Marital birth rate

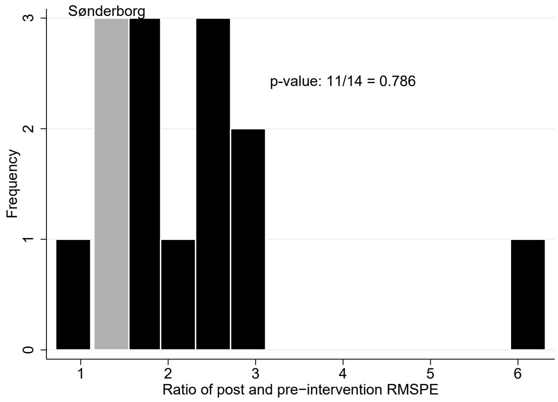
(a) Town-level: Sønderborg



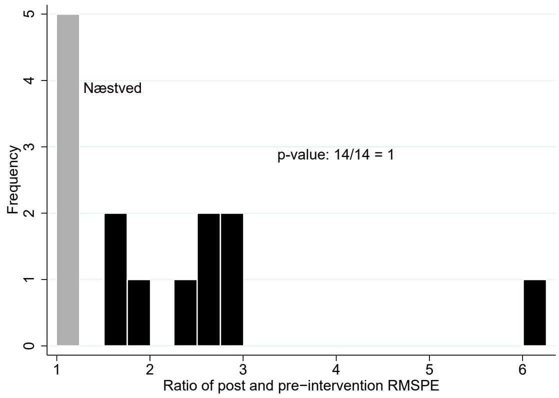
(b) Town-level: Næstved



(c) County-level: Sønderborg

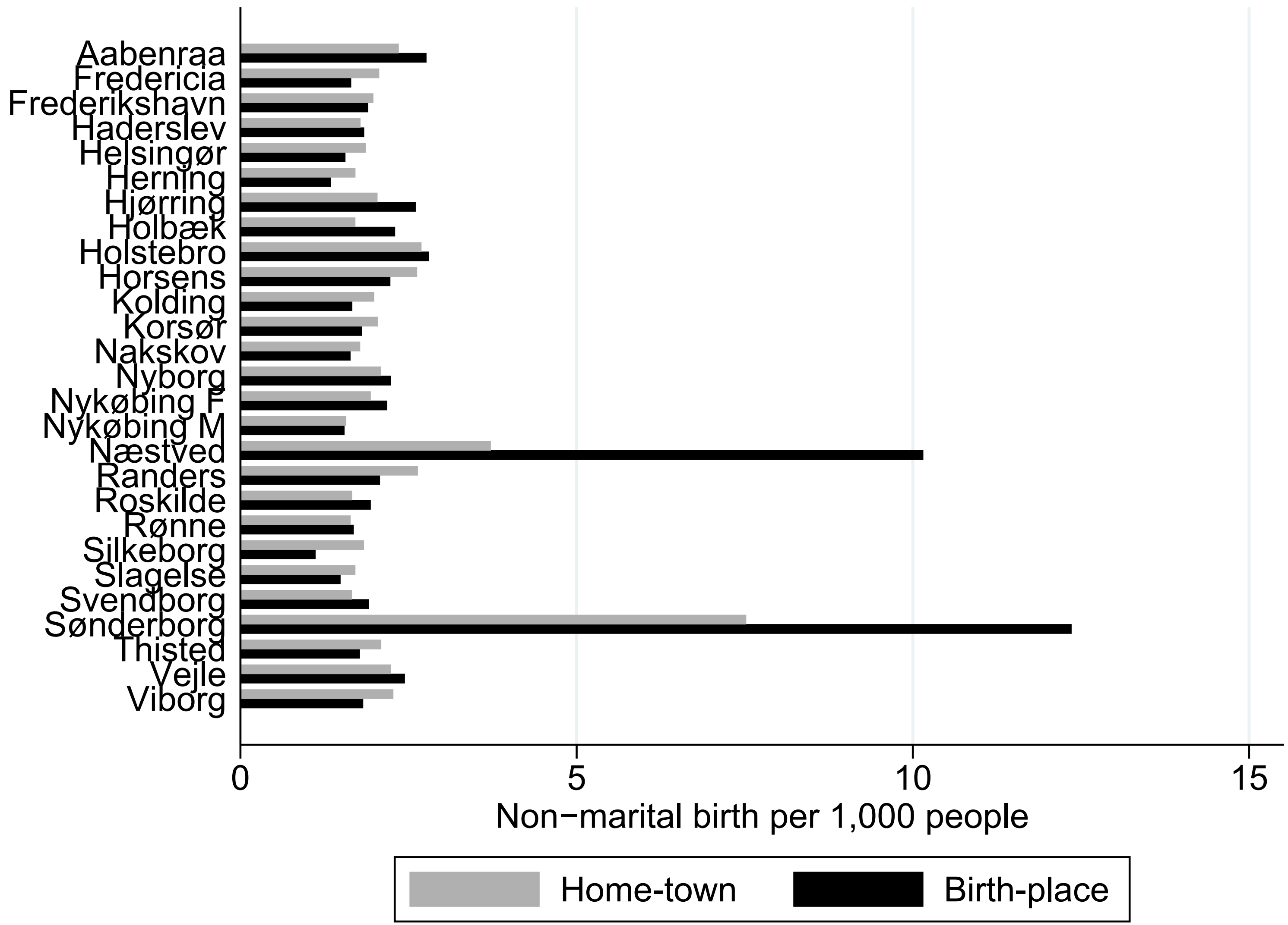


(d) County-level: Næstved



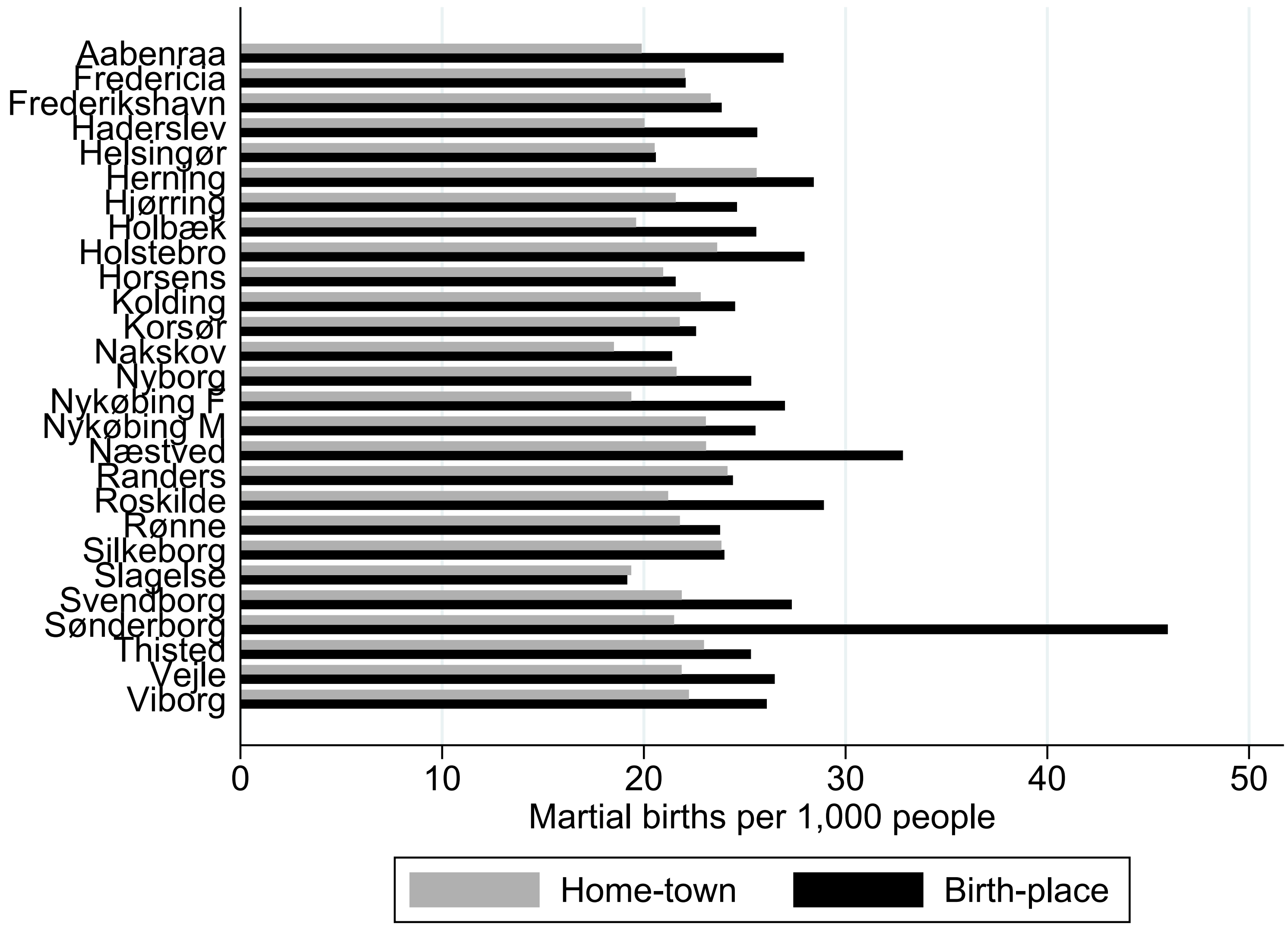
Notes: The bars are the ratios of RMSPE in the post and the pre-intervention period for each unit in the donor pool. A large value relative to the distribution indicate significance. The grey bar en each plot is the treated unit.

Fig. A5 Crude non-marital birth rate at home-town and birth place level - 1944-1947 average



Notes: The figure shows the average crude non-marital birth rate at home-town (gray) and birth place (black) level between 1944-1947.

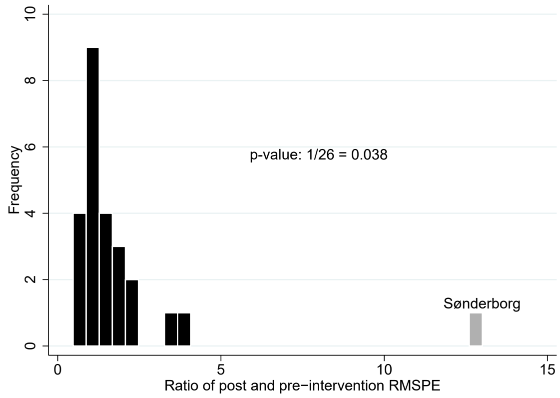
Fig. A6 Crude marital birth rate at home-town and birth-place level - 1944-1947 average



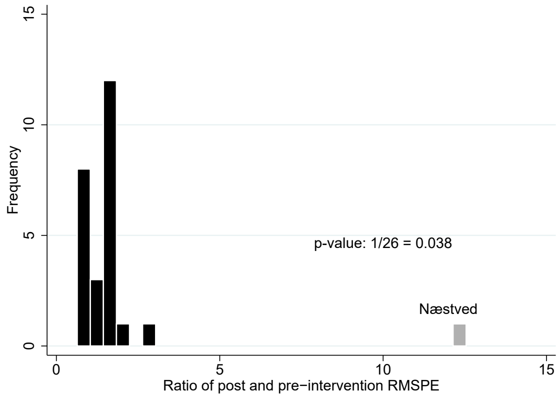
Notes: The figure shows the average crude marital birth rate at home-town (gray) and birth-place (black) level between 1944-1947.

Fig. A7 Inference based on RMSPE ratios - Outcome: Share of non-marital births

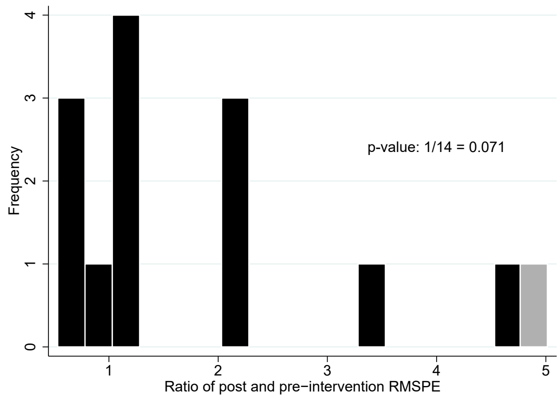
(a) Town-level: Sønderborg



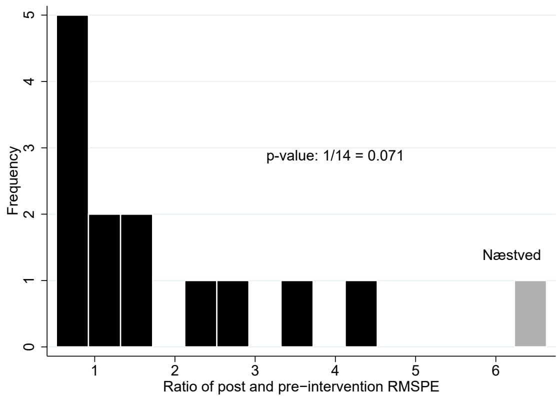
(b) Town-level: Næstved



(c) County-level: Sønderborg



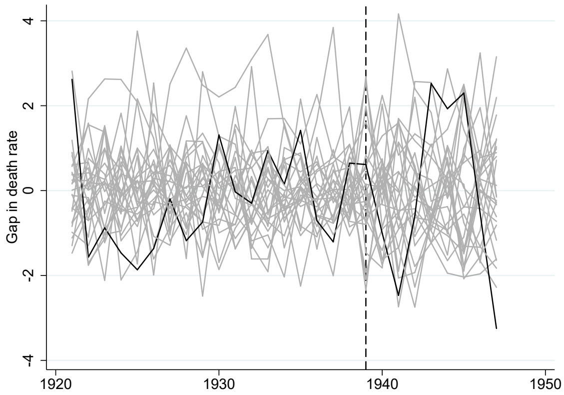
(d) County-level: Næstved



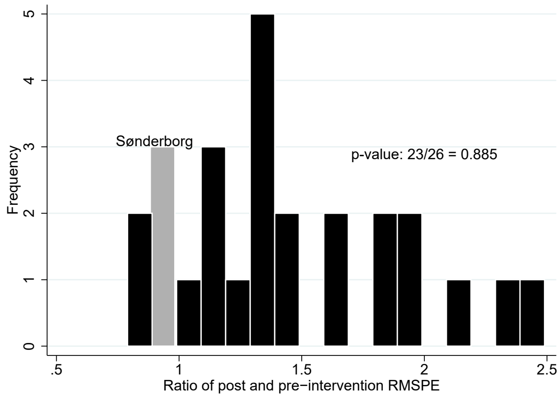
Notes: The bars are the ratios of RMSPE in the post and the pre-intervention period for each unit in the donor pool. A large value relative to the distribution indicate significance. The grey bar en each plot is the treated unit.

Fig. A8 Placebo test: Death rate and suicides etc. in Sønderborg and synthetic control town

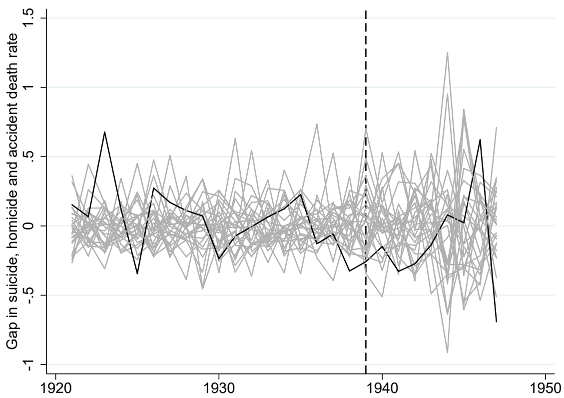
(a) Treatment effect on death rate and placebo inference



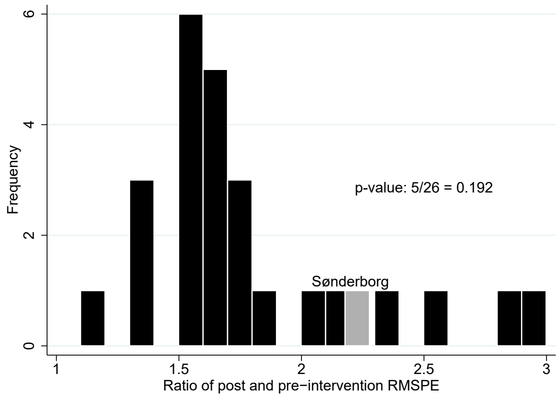
(b) RMSPE inference for significance



(c) Treatment effect on suicides, homicides and accidents and placebo inference



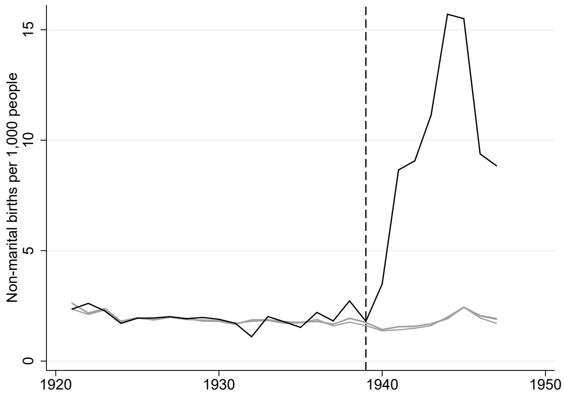
(d) RMSPE inference



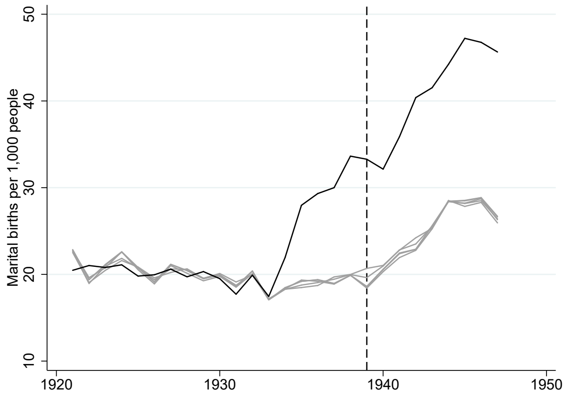
Notes: The figures shows the estimated treatment effects on placebo outcomes from the 1939 family planning program along with placebo and RMSPE based inference. The black solid lines in the left panels are the treatment effects for the true treated town. The pre-intervention period is 1921-1938 and post-intervention 1939-1947. The same covariates as in figure 3 are used. The donor pool includes 25 untreated towns. In right panels are inference based on ratios of the RMSPE. The gray bar indicate the treated town. Estimations with placebo outcomes are only for Sønderborg.

Fig. A9 Treatment effect on non-marital and marital birth rate - Specification sensitivity

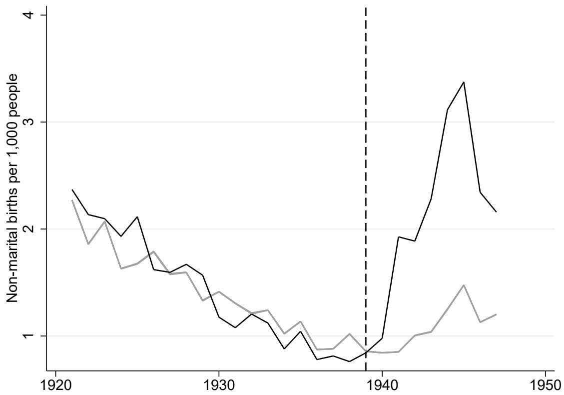
(a) Non-marital birth rate - Town-level



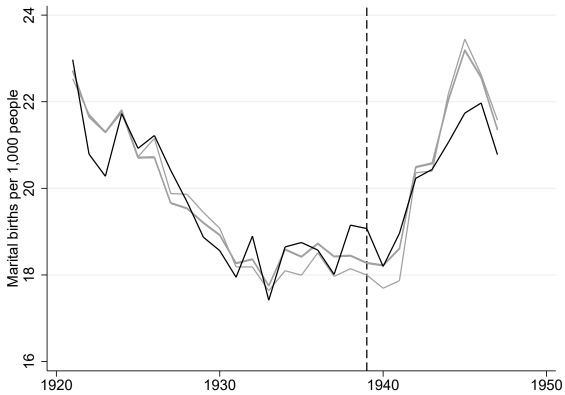
(b) Marital birth rate - Town-level



(c) Non-marital birth rate - County-level



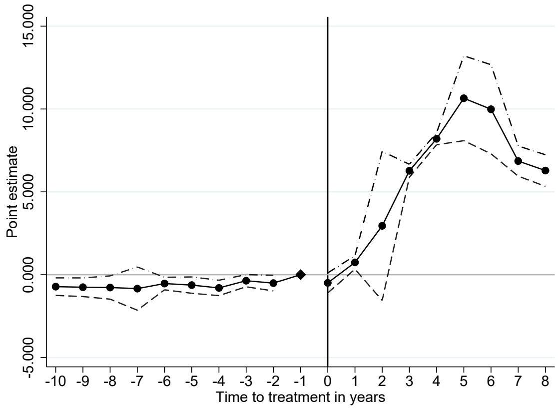
(d) Marital birth rate - County-level



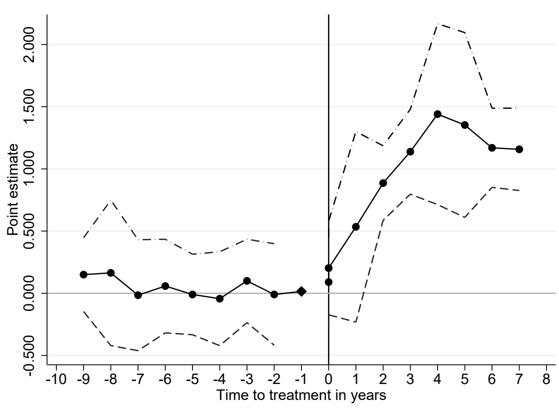
Notes: The figure shows estimated counterfactuals for five different specifications of how the outcome enters the predictor set.: 1) Outcome values for all years, 2) outcome values for the first 3/4 of the pre-treatment period, 3) outcome values for the first half of the pre-treatment period, 4) odd years of pre-treatment values and 5) even years of pre-treatment values. The outcome is the non-marital and marital birth rate at town-level in panels (a) and (b) and at county-level in panels (c) and (d). The treated unit throughout is Sønderborg.

Fig. A10 Event study analysis

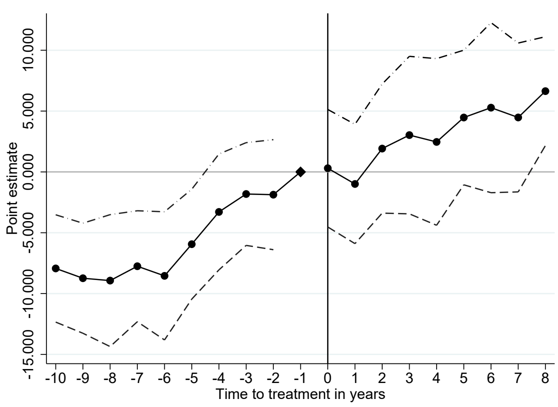
(a) Non-marital birth rate - Town-level



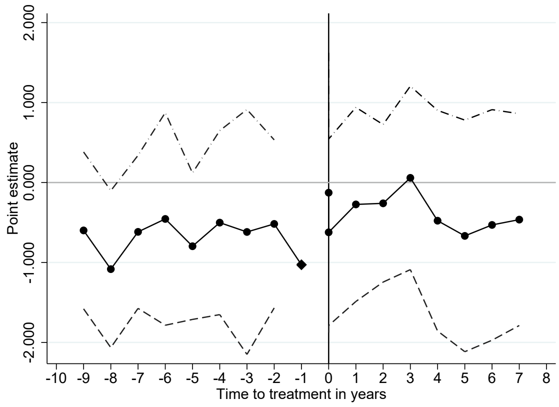
(b) Non-marital birth rate - County-level



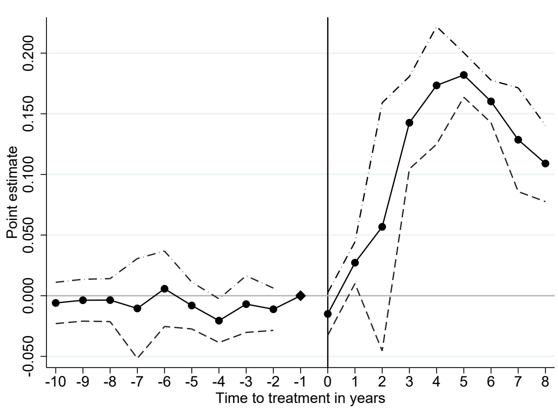
(c) Marital birth rate - Town-level



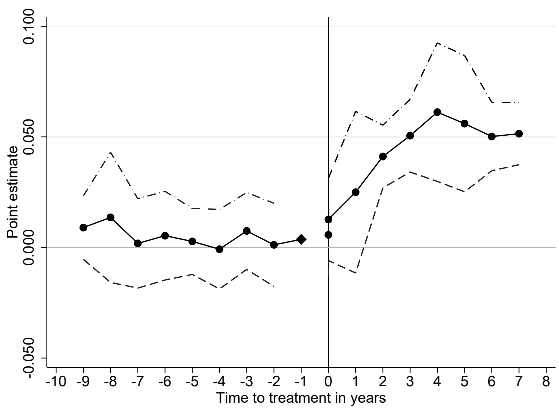
(d) Marital birth rate - County-level



(e) Share of non-marital births - Town-level



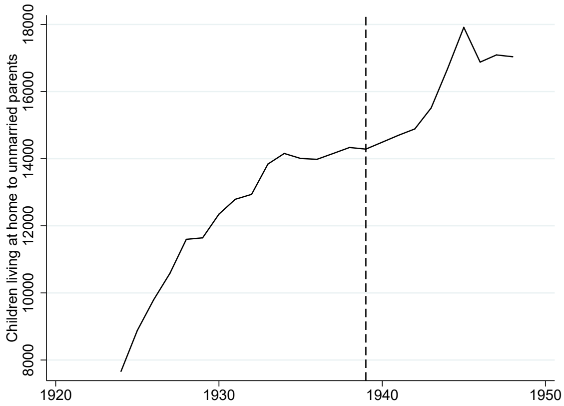
(f) Share of non-marital births - County-level



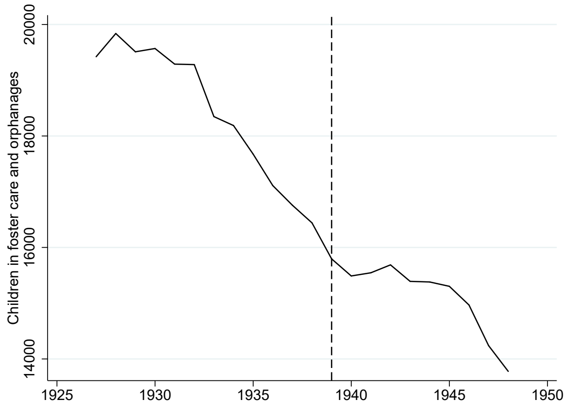
Notes: The figures shows the coefficients and 95 % confidence bans from event study estimations on non-marital (top), marital (middle) birth rates and the share of non-marital births (bottom). All regressions include year and unit FE and no controls. Black dots and solid lines show point estimates while dashed lines show 95 pct. confidence bans based on robust standard errors.

Fig. A11 Living arrangements of children

(a) Children living in households with unmarried parents



(b) Children living in foster care and orphanages



Notes: The lines plot the number of children living in household where the parents are not married in panel (a) and children living in foster care and orphanages. Source: The Medical Report for the Kingdom of Denmark (various years).

Tab. A1 Synthetic control weights with Sønderborg as treated unit

	(1) Non-marital birth rate	(2) Marital birth rate	(3) Non-marital birth share
		Town-level	
Aabenraa	0.00	0.00	0.00
Fredericia	0.00	0.00	0.00
Frederikshavn	0.00	0.00	0.06
Haderslev	0.00	0.00	0.00
Helsingør	0.00	0.00	0.25
Herning	0.42	0.00	0.00
Hjørring	0.00	0.00	0.09
Holbæk	0.00	0.00	0.00
Holstebro	0.58	0.00	0.00
Horsens	0.00	0.00	0.00
Kolding	0.00	0.00	0.00
Korsør	0.00	0.00	0.00
Nakskov	0.00	0.15	0.02
Nyborg	0.00	0.21	0.00
NykøbingF	0.00	0.00	0.19
NykøbingM	0.00	0.00	0.00
Randers	0.00	0.64	0.00
Roskilde	0.00	0.00	0.34
Rønne	0.00	0.00	0.00
Silkeborg	0.00	0.00	0.00
Slagelse	0.00	0.00	0.04
Svendborg	0.00	0.00	0.00
Thisted	0.00	0.00	0.00
Vejle	0.00	0.00	0.00
Viborg	0.00	0.00	0.00
		County-level	
Bornholm	0.26	0.00	0.00
Frederiksborg	0.23	0.00	0.68
Haderslev	0.11	0.06	0.19
Hjørring	0.00	0.00	0.00
Holbæk	0.00	0.00	0.00
Maribo	0.00	0.60	0.00
Randers	0.00	0.00	0.00
Ringkøbing	0.40	0.00	0.00
Sorø	0.00	0.00	0.00
Svendborg	0.00	0.00	0.00
Thisted	0.00	0.00	0.00
Tønder	0.00	0.34	0.13
Vejle	0.00	0.00	0.00
Viborg	0.00	0.00	0.00

Notes: The table shows synthetic control weights (the weights assigned to each control unit in the donor pool) with Sønderborg as treated unit and outcomes given by the labels in the first row. The top panel shows weights for the town-level analyses and the bottom panel shows weights for the county-level analyses.

Tab. A2 Synthetic control weights with Næstved as treated unit

	(1) Non-marital birth rate	(2) Marital birth rate	(3) Non-marital birth share
		Town-level	
Aabenraa	0.00	0.00	0.00
Fredericia	0.00	0.00	0.00
Frederikshavn	0.11	0.20	0.02
Haderslev	0.47	0.00	0.00
Helsingør	0.13	0.00	0.00
Herning	0.00	0.03	0.26
Hjørring	0.00	0.00	0.00
Holbæk	0.00	0.00	0.00
Holstebro	0.00	0.00	0.00
Horsens	0.00	0.00	0.00
Kolding	0.00	0.08	0.00
Korsør	0.00	0.00	0.00
Nakskov	0.00	0.00	0.00
Nyborg	0.00	0.05	0.06
NykøbingF	0.00	0.00	0.00
NykøbingM	0.00	0.00	0.00
Randers	0.00	0.07	0.00
Roskilde	0.00	0.00	0.00
Rønne	0.00	0.00	0.13
Silkeborg	0.28	0.00	0.20
Slagelse	0.00	0.07	0.33
Svendborg	0.00	0.50	0.00
Thisted	0.00	0.00	0.00
Vejle	0.00	0.00	0.00
Viborg	0.00	0.00	0.00
		County-level	
Bornholm	0.00	0.07	0.17
Frederiksborg	0.11	0.00	0.00
Haderslev	0.00	0.00	0.00
Hjørring	0.00	0.21	0.30
Holbæk	0.00	0.06	0.04
Maribo	0.42	0.00	0.00
Randers	0.00	0.00	0.00
Ringkøbing	0.00	0.00	0.00
Sorø	0.00	0.05	0.21
Svendborg	0.47	0.00	0.00
Thisted	0.00	0.00	0.00
Tønder	0.00	0.30	0.28
Vejle	0.00	0.31	0.00
Viborg	0.00	0.00	0.00

Notes: The table shows synthetic control weights (the weights assigned to each control unit in the donor pool) with Næstved as treated unit and outcomes given by the labels in the first row. The top panel shows weights for the town-level analyses and the bottom panel shows weights for the county-level analyses.

Tab. A3 Correlation between weights from alternative specifications of how to include the outcome (non-marital birth rate) as predictor

	W_1	W_2	W_3	W_4	W_5
W_1	1				
W_2	0.997	1			
W_3	0.961	0.957	1		
W_4	0.997	1	0.957	1	
W_5	0.997	1	0.957	1	1

Notes: The table shows correlation coefficients between the weights constituting the synthetic control unit in each of five different specifications to include the outcome as predictor. The subscript i in W_i refer to: 1) Outcome values for all years, 2) outcome values for the first 3/4 of the pre-treatment period, 3) outcome values for the first half of the pre-treatment period, 4) odd years of pre-treatment values and 5) even years of pre-treatment values. The outcome is the non-marital birth rate at town-level and the treated town is Sønderborg.

Tab. A4 Diff-in-Diff results - Average treatment effect on the treated

	(1)	(2)	(3)
	Town-level		County-level
	Non-marital birth rate		
Family planning	6.355***	6.480***	0.711***
	(0.902)	(0.920)	(0.130)
	Marital birth rate		
Family planning	10.22***	11.04***	0.260
	(1.082)	(1.104)	(0.205)
	Non-marital birth share		
Family planning	0.111***	0.111***	0.0290***
	(0.017)	(0.017)	(0.006)
Controls	No	Yes	No
Observations	729	729	432

Notes: The table shows the coefficient from regressing a dummy for family planning status on the various outcomes. All regression include year and unit FE. The controls are the average pre-1939 values of income and wealth per capita, population density, women share and share employed in manufacturing interacted with year dummies. Robust standard errors in parenthesis. *p<0.1, **p<0.05 and ***p<0.01.

Chapter 2

The Timing of Early Interventions and Child and Maternal Health

The Timing of Early Interventions and Child and Maternal Health*

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[2]University of Bristol

[3]The Danish Center for Social Science Research - VIVE

Abstract

This paper shows that the timing of universal nurse home visits during the first year of life impacts child and maternal health. Exploiting variation from a national nurse strike in Denmark in 2008, we show that strike exposure increases child (and mother) general practitioner contacts in the first four years only for early-exposed individuals. Moreover, mothers who forgo an early nurse visit (rather than a later one) have a higher probability of mental health specialist contacts in the first two years after birth. We highlight two channels for these results: The finding, that nurses perform well in control years in identifying maternal mental health risks during early home visits (likely preventing longer-term problems), points to the importance of early screening. The finding, that first-born children and children of parents with no educational background in health drive our results, highlights the importance of provision of information to new parents. A stylized calculation confirms that the short-run health benefits from early universal nurse home visiting outweigh its costs.

JEL Codes: I11, I12, I14, I18, I21

*We gratefully acknowledge financial support from the Innovation Foundation Denmark grant 5155-00001B. The Copenhagen City Archives generously provided the data on nurse home visiting in the municipality of Copenhagen. We thank Boel Leth Emanuel (municipality of Copenhagen) for valuable input on the data. We thank Søren Leth Pedersen, Maya Rossin-Slater, Christine Valente, Stephanie Vincent Lyk-Jensen and seminar participants at briq (Bonn), SOFI (Stockholm), CEBI (University of Copenhagen), Copenhagen Business School, the NBER Children's Program Meeting 2019, the Danish Ministry of Health, CINCH Essen and at the 2019 EALE conference for helpful comments.

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1 Introduction

In this paper we study the importance of the timing of a popular early-life program, universal nurse home visiting (NHV), for child and maternal health. Thus we assess whether early post-birth contacts to primary health care professionals are more important for child and maternal health than later ones. Evidence on this question is sparse but instrumental for policy.

In Denmark, the setting of this paper, all new families are eligible for up to five universal home visits during the first year of a child's life. These visits focus on health screening, the provision of information, and counseling to new parents (on topics such as infant feeding, infant development and child-parent interactions). Additionally, nurses refer families with identified needs to other health care professionals, such as general practitioners (GPs) or hospitals.

To identify the impact of timing of NHV, we exploit exposure to a 2008 nurse strike in Denmark for families with children born in the seven months prior to the strike. During the 61 days of the strike, the vast majority of non-emergency nurse care was canceled. In Copenhagen, nurses only performed ten percent of the home visits performed in the same weeks in the years prior to and after the strike. Importantly, canceled visits were not rescheduled. We exploit the strike-induced variation in nurse home visits, together with information on children born in non-strike years in a difference-in-differences design.

To make our study feasible, we have collected individual-level data on program take-up (number and timing of nurse visits) in largest municipality in Denmark (Copenhagen) and link these records to administrative data on family background and health outcomes.[1] Thus we break new grounds by compiling data on actual program take-up (the policy-relevant margin in a universal program), which allows us to be specific about the intensity of the treatment that we study. The link to administrative data allows us to analyze the credibility of our empirical design (by assessing the compliance with the nurse strike across different

[1]While Scandinavia is well-known for its high-quality administrative data in many domains, national administrative data sources typically lack individual-level data on municipal programs—such as NHV, nurseries or preschools.

groups of families).

In our first set of results, we show that, while children born in the 210 days prior to the strike on average missed one scheduled postnatal nurse visit, depending on their date of birth *relative* to the strike, children had a different age at the forgone visit. Moreover, exploiting the merged nurse records and administrative data on family background, we show that the strike impacted families similarly across characteristics, likely observed by nurses. This finding illustrates the broad coverage of the strike in the population and relieves concerns that nurses to a large degree chose the families that would forgo their visit. Additionally, we show that (given that all children were born before the onset of the strike) other aspects of care around birth (such as prenatal midwife contacts or hospital admissions at birth) were not impacted by children's strike exposure.

Moving on to our reduced form analysis of the impact of strike exposure at specific ages, we show that exposure during the initial months of a child's life is relatively more influential for child and maternal health than later exposure. We measure health by the uptake of additional medical care: Children, who were born in the two to three months up to the strike, and thus were likely to miss the early nurse visits due to the strike, have more contacts to general practitioners (GP) in the first four years of life than children, who were older at their exposure to the strike. This result holds for both regular an emergency GP contacts (the latter not being performed by the family's regular GP and outside GP office hours). Moreover, our results for yearly measures of GP contacts confirm this finding, i.e. our results are not driven by a closer relationship with the family GP or a substitution of nurse visits with GP visits during the strike period.[2] We also find suggestive evidence for a higher probability of hospital contacts in the second year of the child's life for early strike-exposed children. This finding further substantiates that our results for an increased uptake of health care reflect children's underlying health.

Finally, we study maternal health care usage as a consequence of strike exposure. First,

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

we find that 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. Second, we find suggestive evidence for early strike-exposed mothers being *more* likely to have at least one contact with a psychologist or psychiatrist in the first two years of the child's life. This finding suggests that early strike-exposed mothers (who thus lack a nurse visit focusing on early screening for mental health issues) end up receiving more specialist treatment. While missing an early nurse visit initially (and mechanically) may result in fewer mothers being referred to other specialists, our finding suggest that (in the longer run) early strike exposure leads to an increased likelihood of mothers experiencing mental health problems that require specialist attention. Thus our finding is in line with an emerging literature 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 effects of missing an early nurse visit, we consider two possible mechanisms by exploring the role of screening and information. First, early nurse visits may help to identify adverse conditions in a timely fashion and prompt additional care by other health professionals, such as GPs. As we show in our data from the non-strike exposed control period, at initial visits, nurses predominantly record issues related to feeding, child physical health and maternal well-being. Furthermore, those initial registrations correlate with both future nurse registrations of health issues and the increased use of health care services among children, as well as the likelihood of future maternal psychiatric contacts. These correlations suggest that early nurse visits act as an important screening device to identify vulnerable children and mothers. In absence of early nurse visits, for the marginal child, her own health problem and—potentially more importantly—maternal mental health problems may go unnoticed for a longer period and contribute to longer-term adverse health effects. Our results for the impact of early strike-exposure on maternal contacts to psychologists or psychiatrists are in line with this reasoning. Moreover, given documented correlations of maternal postnatal mental health and child-parent interactions and child development (Cooper and Murray, 1998; Lovejoy et al., 2000; Paulson et al., 2006; Wachs et al., 2009), screening for postnatal maternal well-being issues may be one driver also for impacts of early NHV on children.

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

Our stylized analysis of the direct costs and benefits of early nurse visits relative to later visits (based on a limited set of outcomes from the domain of health and thus ignoring other potential benefits of earlier nurse visits) shows that the immediate benefits (in terms of averted child and mother GP visits) of initial universal nurse visits clearly outweigh their costs (with 331-426 EUR). Thus our findings indicate that early universal visits are a cost-effective intervention to promote children's and mothers' health in settings that resemble the Danish health care system. Given our findings, universal child programs should have a strong focus on the initial period of family formation 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 (for an overview see Almond and Currie, 2011; Almond et al., 2018). We make three contributions: First, when studying the causal effects of early-life investment programs (such as nurse home visiting, or childcare and early education provisions), the majority of work has considered the effects of program exposure. However, we still lack insights on the causal effects of important design aspects of early-life investment policies, such as timing or intensity. In our paper, rather than studying the margin of program exposure, we consider the so far largely unexplored impacts of *the relative timing* of access to early-life health programs. Our study extends earlier work by Kronborg et al. (2016), who study mothers giving birth *during* and shortly prior to the nurse strike and only find short-lived effects of strike exposure on the take-up of GP care for children. However, in their paper all strike-exposed mothers and children forgo the earliest home visits (the ones that we show are influential) and vary in their access to various treatments: prenatal midwife consultations, hospital stays after birth and the early postnatal nurse visits.[3]

Second, a large share of the work on early-life investment policies has been set in a U.S. context and as a consequence has considered *targeted* programs.[4] Existing work on NHV

[3]Strike-exposed mothers in their analysis received less pre- and postnatal care: Mothers, who gave birth during the strike received fewer prenatal midwife consultations, were more likely to be discharged from hospital on the day of birth, and received fewer nurse home visits. Mothers, who gave birth in the two weeks prior to the strike had higher probability of not receiving the initial nurse visit but were unaffected with respect to the access of prenatal care. Mothers, who gave birth earlier (two weeks to two months prior to the strike) were unaffected with respect to the prenatal care offers, hospital care around birth but had an increased probability of a canceled second nurse visit. Given that all mothers in the sample lack the early home visits after birth, our analysis identifies a different margin of treatment (focusing only on the importance of timing of postnatal care). Moreover, while Kronborg et al. (2016) cannot link data on NHV to data on family background and health outcomes, we perform a complier analysis, i.e. assess the "coverage" of strike exposure in the population of families. Finally, we both analyze a broader set of relevant outcomes (including maternal well-being) and the potential channels for our main results.

[4]Examples include RCT studies on the targeted Perry Preschool Program, the Abecedarian project (Heckman et al., 2013; Conti et al., 2016), and observational studies on the short- and long-run impact of Head Start (Currie and Thomas, 1995; Garces et al., 2002; Masse and Barnett, 2002; Schweinhart et al., 2005; Belfield et al., 2006; Ludwig and Miller, 2007; Anderson, 2008; Deming, 2009; Heckman et al., 2010a,b; Carneiro and Ginja, 2014; Campbell et al., 2014; García et al., 2016; De Haan and Leuven, 2016; García et al., 2017; Thompson, 2017). Also in a US context, there are a few examples for studies considering universal provision of preschool (see, for example, Cascio, 2009, 2015).

has primarily focused on contemporary targeted programs as well (Olds et al., 1986, 1998, 2002; Vaithianathan et al., 2016; Doyle et al., 2015; Sandner et al., 2018; Sandner, 2019).[5] However, many countries offer *universal* programs and the results from studies on targeted programs do not easily generalize to settings with universal implementation. Our study is the first to analyze the causal impacts of a contemporary universal program.[6] Evidence on the impact of universal health programs and their design is instrumental for policy design in many settings.

Third, we shed light on two relevant mechanisms for the impact of timing of NHV on child and mother health: Screening (and potential referral of families to other health professionals) and information (e.g., about infant feeding or age-specific child-parent interactions). This information may matter in its own right (i.e., it may be new to parents) or modify parental beliefs (i.e., it may help parents to update their reading of information that they have access to). Recent research documents the importance of parental beliefs—their interpretation of rather than their pure awareness of information—for both child health outcomes and parental investment behaviors (see, for example, Cunha et al., 2013; Attanasio et al., 2015; Boneva and Rauh, 2018; Biroli et al., 2018). Our unique data allow us to explore the question of which

[5]Existing evidence suggests that targeted NHV can be effective in improving a large range of short- and long-run child outcomes and points to the role of the structure of the programs and the qualifications of service providers (for an overview on existing studies and a discussion of the impact of provider quality, target group and program features, see Almond and Currie, 2011): Focusing on the targeted Nurse Home Visiting Partnership program in the US, Olds et al. (1986, 1998, 2002) show that high-frequency pre- and postnatal visits for at-risk mothers conducted by trained nurses reduced child abuse, decreased children's emergency room visits and their criminal convictions in adolescence. Similarly, Vaithianathan et al. (2016) provide evidence from New Zealand showing that targeted nurse visits reduced infant mortality and increased both vaccination rates and children's participation in early childhood education. Doyle et al. (2015) study the targeted Preparing for Life-program in Ireland and find some positive effects on child health (such as asthma issues) and accidents. Sandner et al. (2018) and Sandner (2019) document that the German implementation of the "Pro Kind" program, a home visiting program for low-income first-time mothers, did not impact child health but had impacts on mothers in the RCT: treated mothers reported lower levels of depression. In the longer run, the program increased fertility and decreased maternal labor supply. Work from developing country contexts highlights the important role for child development and long-run outcomes that intensive home visiting can play (Attanasio et al., 2014; Gertler et al., 2014).

[6]Another line of research has documented positive long-run impacts of the historical introduction of universal NHV in Scandinavia of the 1930s and 40s (Wüst, 2012; Hjort et al., 2017; Bhalotra et al., 2017; Butikofer et al., 2018). All existing evidence on the causal short- and long-run effects of NHV in Scandinavia comes from historical data and considers the extensive margin of treatment exposure. These studies have documented positive long-run effects on the health and socio-economic outcomes of exposed cohorts.

elements matter in NHV by using specific nurse registrations and the heterogeneity of effects of NHV across different types of parents. While we cannot formally distinguish whether nurses provide new knowledge to parents or modify their beliefs about its importance, identifying the relative importance of different components of NHV (information, screening) and their timely provision is important for understanding the channels for the impact of program timing.

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

2 Background and Data

2.1 Institutional Background: Pre and postnatal care in Denmark

In Denmark, pre- and postnatal care is provided in the public health care system and all residents have access to care free of charge. Midwives and general practitioners provide prenatal care that consists of regular consultations during pregnancy.[7] The majority of uncomplicated births are midwife-assisted and take place in public hospitals only. Hospital births account for around 98 percent of all births.

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

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

services include additional home visits or other services.[8] Moreover, Danish GPs provide the child preventive health program and administer recommended vaccines in the vaccination program. The Danish preventive care schedule offers eight (voluntary) GP health checks for all children: at around five weeks, at around five months, and yearly for children aged one through six years (Sundhedsstyrelsen, 2007). Additionally, GPs offer one postpartum health check for mothers. In the first year of the child's life, the Danish vaccination program for children consists of three rounds, at three, five and twelve months, respectively.[9]

2.2 NHV in Copenhagen

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

Home visits usually last between 30 minutes and one hour. During the visits, nurses provide information and counseling to parents and examine the infant. The visits take their point of departure from a general set of main topics (which are of different importance at different ages of the child) outlined in the national guidelines for NHV. At the same time,

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

[9]Each round consists of two separate vaccinations. First, a combined vaccination to immunize against diphtheria, tetanus, pertussis, polio and hib infection. Second, a pneumococcus bacteria vaccination to prevent infant meningitis. We focus in this paper on the vaccinations given in the first year of life but the vaccination program continues with a number of other vaccinations throughout childhood and adolescence.

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

those guidelines explicitly state that nurses should focus on the needs of the specific family. Thus nurses have large discretion to focus their time in the family home on what they regard as most important. While some topics, typically related to screening (such as tests for certain infant reflexes, monitoring of maternal postnatal well-being and the monitoring of child weight and height), are part of visits to all families, other topics are only covered if the family or the nurse find them relevant.

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

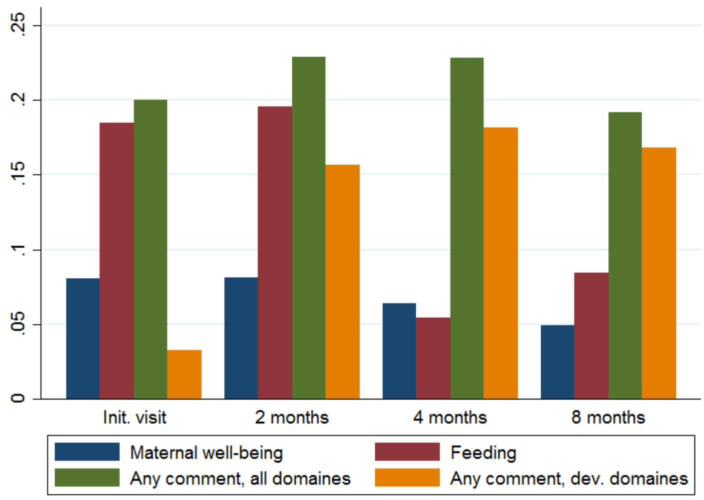


Fig. 1 Share of children with nurse registrations of issues at a given nurse visit (initial visit through eight months visit)
Notes: The share of children with registered issues in each domain for all children with a performed visit and born between September 17, 2008 - April 15, 2009 (the control period). Each domain aggregates a set of binary measures. Children's issue indicator is one if at least one binary measure is registered as problematic by the family's nurse.

To illustrate the typical content of the nurse visits, Figure 1 presents nurse registrations made by Copenhagen nurses during or shortly after their home visits to families from our

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

		Topic	Examples for items (some visit-specific)	- - - *Visit* - - - A 0-14days	B 2months	C 4 months	D 8 months
		Background	Issues related to pregnancy and birth, health risks (parental smoking, alcohol, BMI), family structure, etc	✓			
(1)		Postpartum maternal health	Physical and mental well-being, formal depression screening	✓	✓	✓	✓
(2)		Feeding	Breastfeeding, supplementary feeding, introduction of solid food	✓	✓	✓	✓
(3)		Parent-child interactions	Activities, parental recognition of infant needs/signals	✓	✓	✓	✓
(4)		Child signals and reactions	Sleep patterns, mood, smile/contact, differentiating btw adults	✓	✓	✓	✓
(5)		Child Examinations					
	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 the schedule for topics at the given home visit and the nurses' options for registrations in the registration system used by nurses in Copenhagen in the period that we study. Visits A-D refer to the type of the four scheduled universal visits. Additionally, nurses can offer a targeted pregnancy visit (around week 30 of the pregnancy), visits based on identified needs in the family, and a visit at age 1.5 and 3 years (on parental demand), respectively.

control cohort.[11] We aggregate nurse registrations into broader categories and plot for each of those categories the share of families with a recorded issue by type of visit (conditional on having received the visit). As the figure illustrates, the visits focus on different domains: While the share of families with "registrations of an issue in any domain" remains rather stable over the course of the four visits, there are important differences especially when comparing the first two and final two universal visits. During the initial visits, nurses typically record issues related to maternal mental well-being and infant feeding issues. The former is very well-defined, mother-specific and highly correlated for women across visits. The latter is child-related but rather unspecific in its content. While registrations on feeding issues are common during the initial visits, nurse observations and registrations on child developmental problems (a summary measure of various dimensions of child development) are more prevalent in the visits at around four and eight months. Using this characterization of the composite nurse treatment, we will return to the importance of different aspects of the treatment in section 4.4.

2.3 Data and Variable Construction

In our analysis, we use data from two sources. First, we access archived records on the universe of home visits from the municipality of Copenhagen for the 2007-2009 period.[12] These registrations were either completed at the family home (using a laptop) or at the nurse's office directly after a completed visit. For each visit, the data contain the date and type of visit. Additionally, nurses register their observations regarding factors such as child and maternal health, feeding problems, or relevant risk factors in the family (see Table 1 for

[11] As we will detail in section 2.3, we use data on several cohorts of children and mothers, one of them exposed to the nurse strike. In Figure 1, we focus on non-strike exposed children and mothers as strike-exposed families naturally lack nurse registrations.

[12] These data come from an archive version of the municipality's administrative system. The full archive of nurse records from Copenhagen includes data on all visits and examinations of children resident in the municipality from January 1, 2007 to December 31, 2010—a total of 35,213 children. These records were transferred to the Copenhagen city archive due to a change of the software used by the Copenhagen nurses. As we are interested in studying the impact of timing of nurse visits in the first year of the child's life, we do not consider data from the 2010 cohort as they are right-censored, i.e. we do not observe information on all visits before the end of the data period.

examples of focus areas and registration options at different visits).

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

Using data for the years 2007-2014, we create three sets of health outcome measures from the administrative data: First, to study child and maternal health, we examine both the yearly and accumulated number of GP contacts from child age zero to four. GP contacts include both physical meetings and phone and e-mail correspondence with a GP.[14] Given that we only measure health care usage in our data, we are concerned as to whether we pick up actual impacts of strike exposure on child health: Parents may behave more cautious and—in the short run—substitute nurse care with GP care. In the longer run, parents may continue to demand more care, for example, because they build a strong relationship with their family GP due to increased initial contacts.

While we cannot fully disentangle true health effects from alternative explanations for changes in health care take-up, we attempt to provide more insights by dividing our measure of GP contacts into two categories: i) regular (scheduled) GP contacts that typically involve the family GP, and ii) emergency GP contacts (i.e., GP contacts on weekends or outside default opening hours, which are not performed by the family GP).[15] While not perfectly

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

[14] GPs offers regular phone consultation hours (typically in the early morning).

[15] Emergency GP care was restructured in 2015 and thus there is a data break in the administrative data. Therefore, in our main analysis, 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 emergency GP care. Analyses that also include 2015 and later years (and only consider non-emergency GP care) lead to very similar results that are available on

independent, emergency GP contacts may be a more direct measure of poor health that requires attention. Importantly, we do not include child GP contacts in the preventive care program in our main outcome measure, but analyze those contacts separately. Thus our measures of GP contacts (scheduled contacts and emergency contacts) do not measure the participation in the voluntary preventive care program but focus on contacts due to health problems or parental concerns about the child's health. 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 first-year effects on GP contacts may be caused by substitution, the scope for substitution in the longer-run is likely small.

As alternative measures of child health, we also consider two types of hospital contacts: Hospital admissions and outpatient contacts. Around 25 and 39 percent of children are admitted to the hospital or have an outpatient contact during their first year of life, respectively. While contacts to hospitals may capture more extreme health problems, these figures illustrate that, in general, hospital contacts are not rare and often related to routine check-ups. One aspect worth noting is that the 2008 strike covered all unionized nurses and thus hospital care for non-emergency patients was restricted. Therefore, GPs may have been more reluctant in referring children to hospitals in the strike period.

Second, we consider the impact of strike exposure on maternal postpartum mental health problems. These potential effects are interesting in their own right and also as mechanisms or reinforcing factors for longer-run effects of strike exposure on children. We create an indicator that is equal to one if mothers have any contact with a psychologist and/or psychiatrist in the first two years after the child's birth. We also consider the more extreme margin of maternal outpatient and inpatient contacts with psychiatric specialists up to two years after the child's birth.[16]

request.

[16] We include diagnoses (using the International Statistical Classification of Diseases and Related Health Problems (ICD) system) between F01-F99.

Third, we study the impact of strike exposure on parental health investment decisions. As we exploit information on a sample of children exposed to the nurse strike (and thus the absence of nurse visits at specific ages), we are constrained in our ability to use nurse registrations on parental inputs as outcome measures in our main analyses.[17] Relying on administrative data instead, we consider indicators for participation in the GP preventive care program, participation in the vaccination program, and the timely completion of rounds in the vaccination program. As the nurse visits are closely spaced around the recommended age for the first year vaccinations, we assess whether missing a specific nurse visit impacts the probability of a timely vaccination, which we take as a proxy for parental health investments.

3 Empirical Methods

To examine the effects of the timing of NHV, we exploit children's exposure to the nurse strike in a difference-in-differences framework. Specifically, we estimate the following reduced form relationship:

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

where y_{it} is an outcome measure, such as GP contacts in the first year of life for child i born at time t. In our 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).[18] We split each period in seven 30-days bins and include indicators

[17]In supplementary analyses, we have constrained our sample to early strike-exposed children and study their outcomes at the nurse visit around eight months (D visit). We have considered indicators for nurse-observed issues concerning mother well-being, feeding, child-parent contact as well as indicators for any nurse comments at all and referrals by nurses. However, these analyses rely on a very small sample relative to the expected effect sizes (and the expected noise in the measurement of outcomes by nurses) and is thus not very informative. Unfortunately, the nurse data on infant feeding (duration of breastfeeding) in the archived data are of very poor quality and we cannot use them at all.

[18]As mothers given birth during the strike also had a larger probability of being discharged on the day

that are equal to one if child i's date of birth is within a particular bin. We include a set of fixed effects for the relevant cohort, $\boldsymbol{\lambda}$.[19] The interactions of the period bins with an indicator for the 2008 cohort (the year of the strike) identify our estimates of interest: Children born prior to the strike in 2008 are treated while children born at the same dates in 2009 and 2010 are untreated. We omit the bin furthest from April 15 and children in this group constitute the reference group.

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

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

of birth and fewer midwife visits (Kronborg et al., 2016), including children born during the strike would confound the impact of NHV with the impact of other aspects of care.

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

3.1 Identifying assumptions

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

One way of empirically assessing the untestable common trend assumption is to study pre-determined variables, which should be unrelated to treatment exposure. In other words, we estimate model (1) using parental and birth characteristics as dependent variables. Appendix Tables A2 and A3 show that our treated and control groups are balanced across observable pre-treatment characteristics. Very few coefficients are significant and only at modest levels of significance.[20] Another informal test is the assessment of pre-trends in outcomes across groups. As we do not observe children's GP visits prior to treatment, we consider maternal pre-birth outcomes: Appendix Figure A2 plots pre-birth averages of maternal GP contacts and mothers receiving medical contacts with a psychiatric diagnosis for the treated and control children (born in the 210 day period up to the strike in treated and control years).[21] The figure shows similar trends and levels for both measures of maternal health prior to birth.

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

[20] We have also tested the joint significance of the interaction between the age bins and the strike indicator in each of these regressions. None of the joint tests are significant at the 10 percent-level. Results are available on request.

[21] We include hospital contact diagnoses (using the ICD system) between F00-F99.

to Appendix Figure A2, Appendix Figure A3 plots 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 year and control years. The graphs do not indicate systematic differences or trends in any of these types of care around birth across the groups that we consider.

A final concerns that we address is individuals' selection out of the strike treatment or out of our sample. First, families could not to manipulate their treatment status since all children in our analysis sample were born either prior to the strike or a minimum of four month after the strike ended. In Appendix Figure A4 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 either treated with default care in Copenhagen or 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. If strike exposed families are more (or less) inclined to move, our estimates could be biased.[22] Appendix Figure A5 shows that this concern is not important as the share of children that we observe as Copenhagen residents during their first year of life is not impacted for treated and control cohorts. However, as a robustness check, we include domestic movers into our main analyses (so that only death and migration abroad cause exclusion).[23]

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

[23]We know individuals municipality of residence at January 1 each year. We restrict children born 210 days prior to April 15, 2008, 2009 and 2010 to still reside in Copenhagen at January 1, 2009, 2010, 2011 respectively.

Table 2 Variable means, strike exposed and control period

	Treated group		Control group	
	Mean	Obs.	Mean	Obs.
Total GP 1st year	4.44	4081	4.55	8725
Total GP 2nd year	10.69	4049	10.35	8649
Total GP 3-4 years	11.10	3955	10.22	8451
Emerg. GP 1st year	1.42	4081	1.47	8725
Emerg. GP 2nd year	3.75	4049	3.46	8649
Emerg. GP 3-4 years	3.50	3955	3.18	8451
Vacc., 1st round	0.85	4081	0.90	8725
Vacc., 2nd round	0.87	4081	0.91	8725
Vacc., 3rd round	0.88	4081	0.91	8725
Prev. care, 5 weeks	0.88	4081	0.92	8725
Prev. care, 5 months	0.92	4081	0.93	8725
Prev. care, 12 months	0.93	4081	0.93	8725
Emerg. GP 1st year mothers	0.72	4081	0.70	8725
Emerg. GP 2-4 years mothers	2.10	3950	1.97	8445
Mother psych. diag. 1st year	0.01	4081	0.01	8725
Mother psych. hosp. adm. 3 years	0.01	4081	0.01	8725
Mother psych. outpat. cont. 3 years	0.03	4081	0.03	8725
Midwife visits	4.80	3970	4.75	8507
Smoking status, Mother	0.10	4014	0.09	8587
Child sex	0.48	4081	0.48	8725
Low birth weight	0.04	4009	0.06	8598
Preterm birth	0.06	4014	0.06	8587
C-section	0.21	4081	0.21	8725
Home birth	0.01	4081	0.01	8725
Cohabiting	0.76	4081	0.78	8725
Married	0.37	4081	0.39	8725
Prim. school, mother	0.15	4081	0.12	8725
Uni. degree, mother	0.30	4081	0.32	8725
Student, mother	0.05	4081	0.05	8725
Employed, mother	0.77	4081	0.77	8725
Danish, mother	0.76	4081	0.74	8725
Young mother	0.02	4081	0.02	8725
Young father	0.01	4014	0.01	8551
Income, mother	281.78	4081	289.58	8725
No. of nurse visits	3.77	4081	4.40	4269
Number of registered A-D visits	2.70	4081	3.28	4269
No initial visit	0.16	4081	0.08	4269
No 2-month visit	0.44	4081	0.25	4269
No 4-month visit	0.44	4081	0.24	4269
No 8-month visit	0.26	4081	0.15	4269

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

4 Results

4.1 Descriptive Statistics

Table 2 presents summary statistics for our main sample of children born in Copenhagen across the groups of treated children (born September 18, 2007 - April 14, 2008) and children in the control group (born September 17, 2008 (2009) - April 14, 2009 (2010)). In the top panel, we present summary statistics for outcomes and covariates from the administrative data. In the bottom panel, we present variables on nurse visits from the nurse records from Copenhagen. In this panel, we further constrain our sample to the data periods in the years 2008 and 2009 as the nurse data is right-censored for the children born in 2010.

Control children have on average 4.6 and 10.4 GP contacts during the first and second year of life respectively. During the third and fourth year of life children have 10.2 contacts. Regular GP contacts constitute around two thirds of the total number of contacts. The infant vaccinations and preventive health checks have high coverage rates at around 90 percent. The treated and control groups are well-balanced across covariates.

Focusing on the bottom panel of Table 2, we find that the four universal nurse visits are well attended. The average number of universal visits per child is 3.3 for control children. This figure implies that the average child receives three out of the four universal visits. On average, children additionally receive one home visit scheduled due to a specific need. This average masks heterogeneity across children. Table 2 also illustrates the impact of strike exposure on the program coverage: For all types of visits, treated children have a higher probability of missing the given visit. The difference in the number of universal visits across groups is identical to the difference in their total number of visits. This finding indicates that the average number of extra visits was not affected dramatically by the strike. In the following, we will in analyze these patterns greater details.[24]

[24]To assess the representativeness of our sample of families from the capital of Denmark, Appendix Table A4 compares children and parents from Copenhagen to the general Danish population of parents. Children and parents from Copenhagen differ from the general population on a number of characteristics: they are more likely to cohabit and less likely to be married. Mothers from Copenhagen have a higher educational

4.2 First Stage and Compliers

In Denmark, both private and public wages are to a large degree determined by collective bargaining (Ibsen et al., 2011). In 2008, the negotiations for all publicly-employed nurses, midwives and a large fraction of other employees in the public health sector broke down and resulted in a conflict. Thus on April 15, 2008 the unionized employees in the health care sector went on a national strike. As a result, a total of 45 percent of public employees were on strike in the following weeks (Due and Madsen, 2008). The strike lasted 61 days and ended on June 14, 2008.[25] During the strike period, only managing nurses and a small fraction of regular nurses (employed on specific terms and thus not participating in the strike) were on duty in Copenhagen, the setting for our analysis. These nurses carried out around one tenth of the expectable non-strike default of nurse visits.[26] Moreover, they provided phone services for families that were affected by the strike.

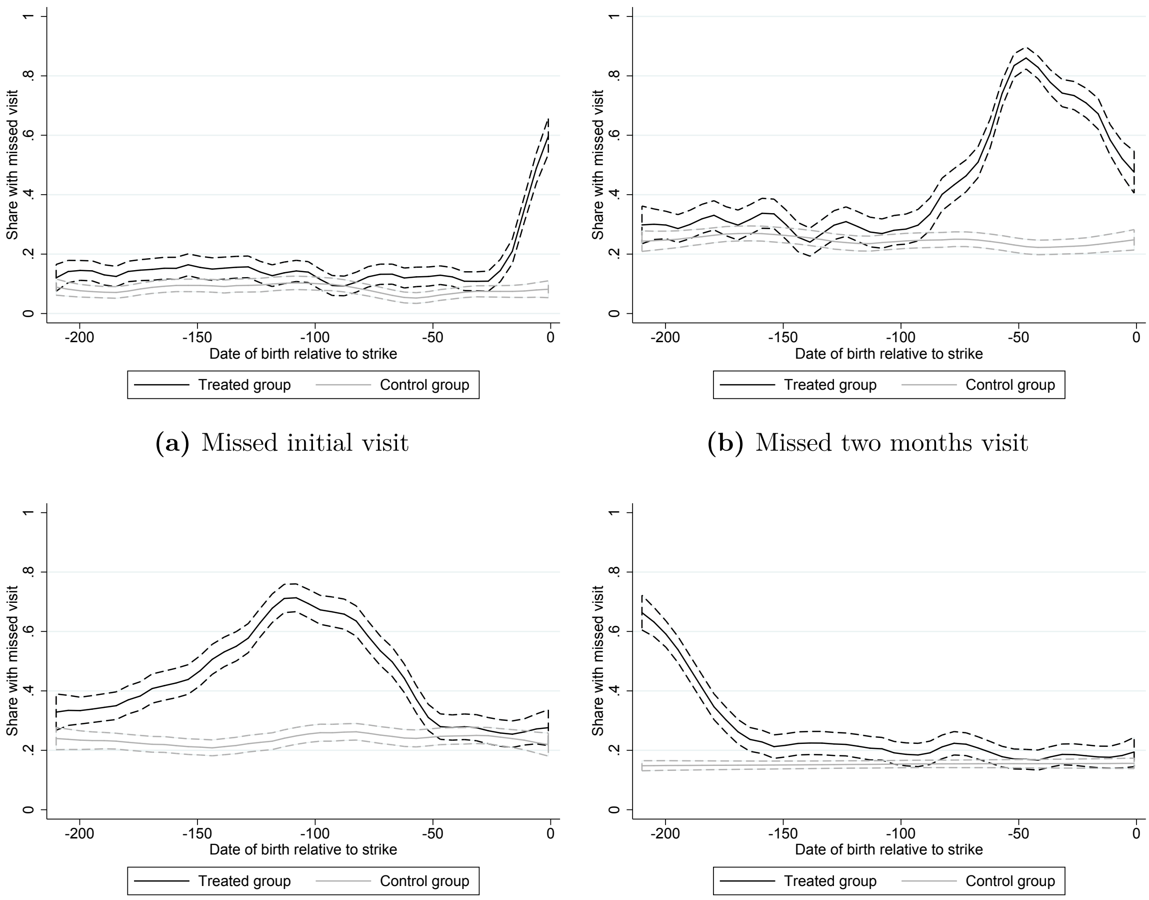
Appendix Figure A1 presents graphically the impact of strike exposure on the number of nurse visits for children in the treated and control cohorts in Copenhagen (2007/2008 and 2008/2009, respectively). Strike exposure impacted the total number of nurse visits that children received. Panel (a) of Appendix Figure A1 shows that control children receive an average of 3.3 visits while treated children receive 2.7. Panel (b) shows the total number of visits (universal + extra) divided by treatment status. The youngest strike exposed children appear to not only loose one but two nurse visits. This finding reflects that early hospital-discharged children receive two visits within the first 14 days of life - one universal visit and one extra visit. In section 4.5, we examine the robustness of our general conclusions to the

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

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

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

omission of this group of children (a doughnut hole-approach).



(a) Missed initial visit

(b) Missed two months visit

(c) Missed four months visit

(d) Missed eight months visit

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

To further examine the impact of the strike on nurse visits and to illustrate the identifying variation that we use (i.e., the overall decrease of the number of visits is driven by a lack of visits at specific ages), Figure 2 shows the impact of strike exposure on the probability of missing a specific nurse visit. The figure shows the raw relationship between date of birth and missing a nurse visit estimated with kernel weighted local polynomials. We use an epanechnikov kernel, a rule-of-thump bandwidth and 42 (5-day) smoothing points through out. Black lines and confidence intervals are for the treated period, grey lines and confidence intervals are for the control periods.[27] The graphs plot the probability of missing a nurse visit for children born in the 210 days before the strike for the years 2008 and 2009.

In absence of strike, the share of children, who miss a specific nurse visit, is stable as indicated by the grey lines in Figure 2. 60 percent of children born immediately before the strike miss the initial visit while all children older than approximately 20 days at strike start miss the initial visit with unaffected probability (20 percent). Panels (b) and (c) show that missing the two and four month visits is also correlated with child age at strike. Finally, only the oldest children in our sample have an increased probability of missing the eight month visit while all the younger children are unaffected at that time (because the strike ended by the time their visit was due).

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

[27] We construct graphs that plot outcomes similarly unless otherwise noted.

universal visits. However, children in the 30-1 day bin loose on average 0.267 nurse visits more than the reference group (significant at the 10 percent level). This result reflects that children below age two weeks at strike start potentially loose two visits, the universal initial visit and an additional early visit if discharged shortly after birth.

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

Table 4 characterizes the compliers with respect to the probability of missing the first nurse visit (analyses for the other three universal visits lead to similar conclusions and are available on request). Following Angrist and Pischke (2008), we characterize the compliers by i) splitting the full sample into relevant subgroups, ii) estimating the model for each subgroup individually and iii) calculating the ratio between the coefficients from each subgroup and the full population. The ratios are the relative likelihood that a complier belongs to that particular subgroup. We look at the first stage estimates across groups of families defined by characteristics that may at least be partly observed by the nurses: child gender, parental education in a health-related field,[28] initial child health,[29] and child parity. ,We show coefficients for the 30-day bin as only children born in that bin were affected by the strike. In general, the complier analysis suggests that the strike affected the considered subgroups relatively similarly and a stronger first stage does not covary with characteristics that may indicate positive potential outcomes. Thus we think it is reasonable to state that nurses did

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

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

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

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

Notes: Each column shows estimates from separate regressions. The coefficients are for the interactions of 30-day bins and a strike year indicator. All regressions include period and bin fixed effects, as well as control variables. Parental covariates are paternal and maternal income, indicators for the highest level of parental education (primary school, high school, university degree), indicators for the mother currently studying or being employed, parental cohabitation and marital status and separate indicators for missing covariates. All covariates are measured one year prior to birth of the focal child. Child/birth covariates include indicators for parental age below 21 at birth, indicators for a C-section, home birth, low birth weight (below 2500g), a preterm birth (below 37 weeks), child gender, maternal smoking status at birth and the number of prenatal midwife visits. The sample includes children born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control period (September 17, 2008 - April 15, 2009). The outcomes in columns (1)-(4) are indicators for the probability of having missed the respective universal home visit. The outcome in column (5) is the number of universal nurse visits received. Column (6) presents results for the total number of nurse visits (universal and additional visits). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

not prioritize to a great degree based on the given characteristics. This finding is relevant for our interpretation of especially heterogeneous effects (because we can rule out that nurses' prioritized certain subgroups during the strike as a main driving factor).

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

4.3 Main Results: Child and Maternal Health

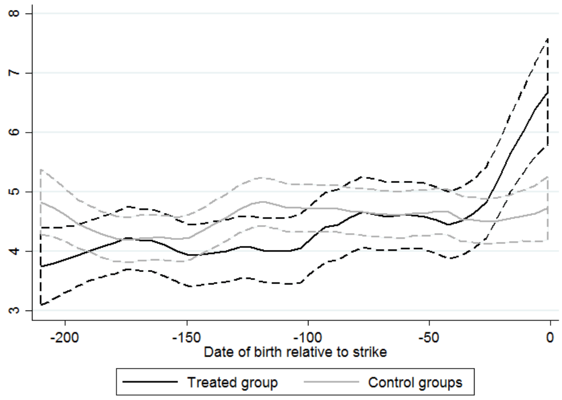
To measure the impact of strike exposure at different ages on children's and mother's health, we use outcomes from the administrative data. Figure 3 presents graphical evidence of the raw relationship between age at strike start and accumulated GP contacts at ages one through four.[30] The number of accumulated GP contacts reveal a clear pattern: Children, who were youngest at strike start in 2008 have significantly more GP contacts relative to children of older age groups and this pattern looks different in the control group. As Figure 3 further illustrates, there is a gradient inside the early strike-exposed group of children such that the youngest children have most GP contacts. This finding indicates that earlier NHV is relatively more important for child health than later NHV. For children older than 100 days at strike start, the average number of GP contacts is similar to the average for control children. Interestingly, the impact of missing an early nurse visit is persistent as the differences increase as the children ages.

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

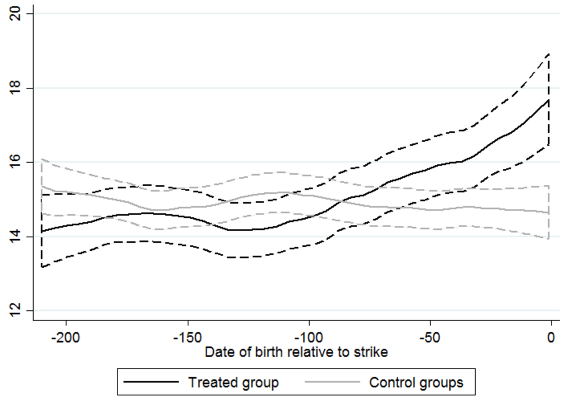
Table 4 Compliers: Effects of strike exposure on the probability of missing the initial visit by subgroup

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

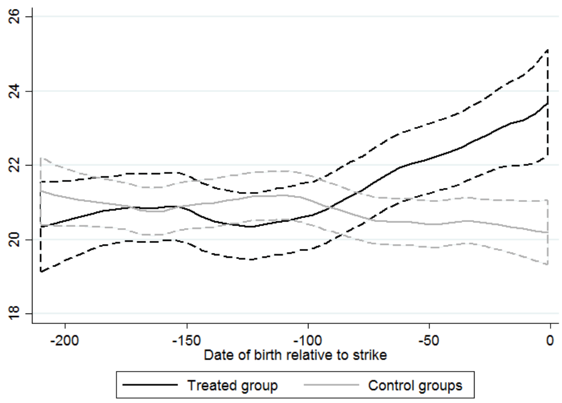
Notes: See notes for Table 3. In this table, we present estimates for the interactions of 30-day bins and a strike indicator from separate regressions for various subgroups along with the ratio between the full-sample estimates and the various subgroup-estimates (both sets of regressions excluding control variables). We only show the estimates for the 30-1 day bin because only children in this bin had their initial visit affected by the strike in the full population. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.



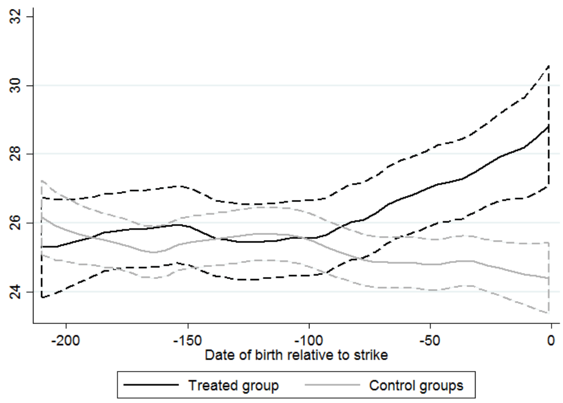
(a) Total GP contacts - one year



(b) Total GP contacts - two years



(c) Total GP contacts - three years



(d) Total GP contacts - four years

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

Table 5 shows our main results for the impact of strike exposure on child health decomposed by the type of GP contact. To rule out that substitution toward GP visits during the first year of life drive our findings, we present estimates for *yearly* outcome measures, i.e., child GP contacts measured in each year of life of the child.[31] Across periods the estimated

[31] We have also estimated the regression equivalents of the graphs for the *accumulated* GP contacts for all years between year one and four in one combined graph. The effects on GP contacts increase as the child ages, in particular during the first two years of life. At age four, treated children have 4.6 (18.3 percent) more GP contacts in total for the 30-1 bin, 2.8 (11.1 percent) for the 60-31 bin and 2.4 (9.5 percent) for the 90-61 bin. For regular GP contacts the percentage effect is 15.8 percent for the youngest age groups and 8.1 percent for the 60-31 age bin (for the 90-61 age bin we see no significant effect on the number of regular GP

effects are significant and the patterns documented in the graphical analyses persist: Children born in the 30-1 days age group have 1.8, 1.6 and 1.3 additional GP visits during the first, second and third to fourth year of life. In percentage terms (evaluated at the average number of GP visits of the control group) our results translate to 40.3 percent, 15.5 percent and 12.4 percent increases. Considering emergency GP contacts, the relative effects are larger at 50.0 percent, 18.4 percent and 18.2 percent during the first, second and third to fourth year of life. Children in the 60-31 days age group have significantly more GP contacts (across types) in their second year of life. For all other age groups the timing of strike exposure has no significant effects on GP contacts.

To assess the impact of strike exposure at other margins, Appendix Table A5 presents results for alternative measures of child health: child hospitalizations and outpatient contacts. While most point estimates for first year hospitalizations are imprecise, we find suggestive evidence that early strike-exposed children are 7-8 percent-points (40 percent) more likely to be hospitalized during the second year of life. These results carefully support that our 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 first year outpatient contacts. While nurses in non-strike years can refer families as outpatients to hospitals in case of health or feeding issues, during the strike this option was likely limited (due to nurses in hospitals also being on strike).[32] Given that we do not see longer-run impacts of strike exposure on outpatient contacts, we conclude that our finding for outpatient care supports the idea of some substitution of care during the strike (from hospital care to GP care).[33]

contacts). The percentage effects on emergency contacts are 23.2 percent, 17.1 percent and 13.4 percent for the 30-1, 60-31 and 90-61 age bins.

[32]However, hospitals were obliged to ensure an adequate level of care provision.

[33]We have also attempted to analyze child outcomes based on nurse registrations at age eight months and longer run outcomes: Constraining our sample to children who received the eight month visit, we do not find precise estimates for the impacts of strike exposure on child development at eight months. However, these analyses are based on around 40 percent of our main analysis. Considering longer-run outcomes, we have explored the impact of timing of strike exposure on the probability of delayed school start of children. We do not detect any effects. Given the age of the strike-exposed and control children, we cannot yet examine longer-run impacts of the 2008 strike on academic test scores (observed for the first time during grade two).

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

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
180-151	0.469	0.129	0.707	0.142	-0.147	0.447	0.327	0.277	0.260
	(0.516)	(0.515)	(0.597)	(0.348)	(0.319)	(0.391)	(0.222)	(0.296)	(0.312)
150-121	0.297	0.402	0.539	0.149	0.191	0.358	0.148	0.211	0.181
	(0.517)	(0.508)	(0.594)	(0.342)	(0.319)	(0.386)	(0.229)	(0.290)	(0.310)
120-91	-0.187	-0.161	0.007	-0.212	0.022	0.193	0.025	-0.183	-0.186
	(0.527)	(0.492)	(0.564)	(0.355)	(0.310)	(0.383)	(0.222)	(0.283)	(0.291)
90-61	0.758	0.701	0.849	0.332	0.445	0.509	0.426*	0.255	0.340
	(0.550)	(0.512)	(0.580)	(0.379)	(0.329)	(0.390)	(0.223)	(0.284)	(0.303)
60-31	0.364	1.707***	0.692	0.087	0.923***	0.408	0.277	0.783***	0.284
	(0.529)	(0.508)	(0.576)	(0.352)	(0.321)	(0.381)	(0.227)	(0.291)	(0.300)
30-1	1.835***	1.614***	1.271**	1.109***	0.977***	0.692*	0.726***	0.637**	0.579*
	(0.555)	(0.519)	(0.584)	(0.376)	(0.332)	(0.394)	(0.233)	(0.288)	(0.296)
Control group mean	4.55	10.35	10.22	3.09	6.89	7.04	1.47	3.46	3.18
Obs.	12078	11982	11709	12078	11982	11709	12078	11982	11709

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

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Total GP 1st year mothers	Total GP 2-4 years mothers	Emerg. GP 1st year mothers	Emerg. GP 2-4 years mothers	Psychologist psychiatrist two years	Mother Psych. hosp. adm. and outpat. cont. two years
Days						
180-151	-0.087	-1.325	0.007	0.046	0.025	-0.002
	(0.375)	(0.986)	(0.097)	(0.217)	(0.016)	(0.002)
150-121	0.024	1.205	0.067	0.204	0.024	0.003
	(0.371)	(0.984)	(0.103)	(0.210)	(0.015)	(0.003)
120-91	0.439	0.802	0.015	0.250	-0.001	-0.000
	(0.394)	(1.013)	(0.098)	(0.215)	(0.015)	(0.002)
90-61	0.366	2.355**	0.124	0.633***	0.025	0.005
	(0.390)	(1.019)	(0.096)	(0.225)	(0.016)	(0.003)
60-31	0.274	1.862*	0.215*	0.702***	0.019	0.008**
	(0.399)	(1.034)	(0.121)	(0.228)	(0.016)	(0.003)
30-1	0.506	2.664***	0.124	0.585***	0.036**	0.001
	(0.391)	(1.007)	(0.097)	(0.217)	(0.016)	(0.002)
Control group mean	7.29	19.92	0.70	1.97	0.05	0.00
Obs.	12078	11698	12078	11698	11982	11982

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

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 6 presents results for maternal (total and emergency) GP contacts, as well as (non-emergency) maternal contacts to psychologists and psychiatrists (after referrals from their GP). Finally, while we also consider maternal psychiatric hospital admission and outpatient contacts within the first two years after birth. However, this outcome is a very rare event limiting our ability to detect impacts given our design and sample size.[34] Table 6 shows that mothers, who are strike-exposed shortly after the birth (90-1 days) of their child, have 1.8-2.6 additional GP contacts (9.5-13.6 percent increase at the mean) during the second to fourth year of life but no additional visits in the first year. Also for mothers, the GP results are both driven by scheduled and emergency GP contacts. For our measure of contacts to a psychologist or psychiatrist two years after birth, we find that mothers with early strike exposure (30-1 days bin) are 3.6 percent-points more likely to have a contact with a specialist (72 percent). In sum, our results indicate that early strike-exposure that resulted in reduced access to early NHV has impacts also on maternal psychical and mental health. Moreover, effects on maternal well-being may constitute a mechanism for or reinforce the health effects on children that we have documented.

4.4 Mechanisms

Our main analyses show that early strike-exposure matters for child and maternal health. We interpret this finding as support for the hypothesis that early NHV matters more for the considered health outcomes than later visits. To speak to potential mechanisms for the observed effects, we focus on the elements of the composite nurse treatment that are of particular importance in the initial visits: information and counseling, and screening and

Assessing the school entry examination of around 75 percent of the children in our sample, we do not see any impact of timing of strike exposure on child BMI or probability of being overweight. In our sample we likely lack power to analyze these outcome (given low level of obesity prevalence at around 7 percent). Furthermore, we miss 25 percent of children in our school entry records that only cover Copenhagen and thus do not include children, who move.

[34]We use contacts with ICD-10 codes F01-F99.

monitoring of infant and maternal health. First, to assess the importance of information and counseling in explaining the negative effects of forgoing an early nurse visit, we study heterogeneous effects across two relevant dimensions: the parity of the child and parental health-related education.[35] Specifically, we hypothesize that first-time parents and parents without professional knowledge about child health and development may see larger effects of early strike exposure if information is an important element that strike-exposed parents lack.

For brevity, we present results for our measure of total GP contacts in year one and year two through four of the child's life.We split our sample into subgroups and additionally estimate a fully interacted model on the full sample. Table A6 presents our split-sample results.[36] Column (1)-(4) show regression results for samples divided into groups of parents with and without an education in a health field. While we do not find significant effects of the timing of strike-exposure for children of parents educated in a health-related field, for children of parents *not* educated in those fields, our results resemble the main results. Similarly, first-born children see stronger effects of early strike exposure and a larger gradient than higher parity children as shown in column (5)-(8) in Table A6. While we formally cannot reject the null hypothesis that the effects are the same across subgroups (see Appendix Table A7), our findings suggest that an information and counseling channel is important for explaining longer-run health impacts of early NHV. At the same time, as illustrated in Appendix Tables A8, we find less systematic differences in estimates across families of high or low socio-economic status, if anything, high SES families appear to see larger effects of early strike exposure.[37] This finding may further underline the importance of specific guidance and information for new parents and, additionally, points to the potential importance of another channel for early life NHV, namely universal screening and health monitoring.

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

[36]Appendix Figure A6 presents the raw relationship between the timing of strike exposure and GP contacts accumulated at age four divided by parental health education and parity.

[37]Appendix Tables A8 and A9 also examine heterogeneity by gender, child initial health, and parental risky behaviors (proxied by maternal smoking during pregnancy). We see indication for boys, children with poor initial health and children of parents with risky parental behavior being relatively more affected by the absence of early NHV (however, also in these analyses, we cannot reject equality of effects in most cases).

Early NHV puts a focus on screening for potential health problems in infants and mothers: Offered as a universal program, it represents an early window of opportunity to detect and confront health problems. Our results for maternal mental health in Table 6 suggest that lack of early screening negatively impacts maternal mental health. Another way of further examining the importance of screening is to assess the performance of nurses with respect to screening in non-strike years.

Figure 4 presents nurse registrations, referrals and maternal health care usage for two groups of mothers in our control year data: first, mothers with registrations of maternal mental health problems at the initial visit (10 percent of mothers) and without those registrations. Conditional on having follow-up visits, we observe interesting patterns that point to the importance of nurse screenings very shortly after birth: Nurses are more likely to register mental health problems in later visits for early-detected mothers. Additionally, mothers with early detected mental health problems receive more referrals to other health professionals and, importantly, among early-detected mothers there is a higher prevalence of externally measured mental health issues.

Relating Figure 4 to the overall prevalence of maternal mental-health related contacts, our calculations suggest that nurses during their first visit identify up to one out of four of those mothers who end up having a mental-health related contact with specialists in the first two years of their child's life.[38] This illustrative figure suggests large potential health returns from early screening efforts.

[38]Nurses screen around 10 percent of mothers in the sample as having a mental health problem. Of those, 13 percent end up having at least one psychologist/psychiatrist contact in the first three years of the child's life. In the population, the prevalence of those contacts is around 5 percent. These figures suggest that nurses may capture around 20 percent of those mothers, who end up with a contact.

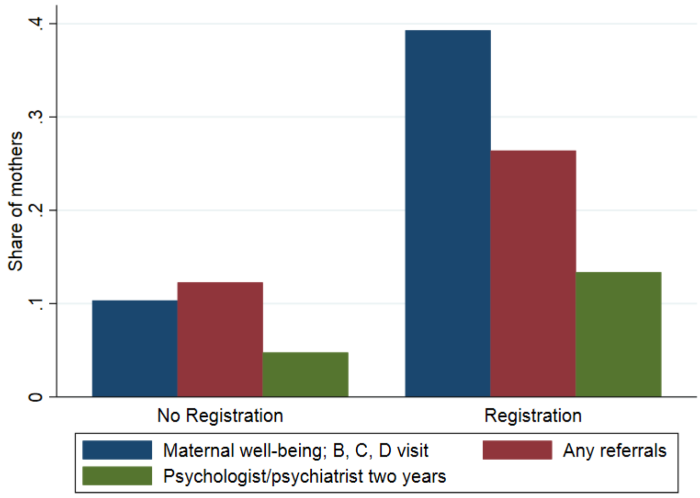


Fig. 4 Nurse registrations of concerns about maternal well-being at initial visit and registrations of concerns and referrals at subsequent nurse visits, mothers of children born in the control period. *Notes:* This figure divides mothers of control children (born in Copenhagen between September 17, 2008 - April 15, 2009) into two groups: The 10 percent of mothers with nurse registrations in their initial nurse visit (concerned about maternal well-being vs not concerned) and the 90 percent of mothers without a registered concern. We constrain the control sample to mothers, who received the initial visit and plot the share of mothers who receive registrations of maternal well-being issues at later visits (B, C and D), 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.

A final and important potential pathway for the effect of early NHV are parental investments in response to those. Nurses provide information and guidance about issues such as other available health care services, appropriate interactions with children at different ages, and aspects such as sleep and child feeding. However, given our sample size in combination with our empirical strategy, we are constrained in an analysis of those parental behaviors: Appendix Tables A10 through A12 study whether strike exposure impacts participation in the childhood preventive care program and vaccination program participation (as well as timely participation) as outcomes.[39] As the tables illustrate, we cannot draw firm conclusions due to very imprecise estimates.

[39] Almost 80 percent of children receive all infant vaccinations and each round of vaccinations are attended by 90 percent of children in Copenhagen. Participation in the vaccination program is voluntary and the decision ultimately rests at the parents. The DNBH specifically mentions nurse visits as a central strategic element to promote the benefits of vaccinations to parents (The Danish National Board of Health [Sundhedsstyrelsen], 2018). The DNBH report highlights the close relationship between the families and their assigned nurse which facilitates dialog if parents are in doubt or have chosen not to participate.

4.5 Robustness Tests

Our main results are robust to a number of changes to our main specification and sample. For brevity, we only discuss the results of robustness tests for our yearly measures of child GP contacts.[40] As we show for those outcomes, our conclusions are not sensitive to the omission of individual-level control variables (Appendix Table A13), reasonable alternative choices of bin size (Appendix Tables A14 and A15), the use of an earlier cohort of children as a control group (Appendix Table A16), and the estimation of our main results on our "first stage" sample only using data from the years 2008 and 2009 (Appendix Table A17).

An alternative way of assessing whether other factors confound our interpretation of the strike impact are placebo regressions. Appendix Table A18 shows placebo tests where we define "treated" children as those born 210 days prior to April 15, 2009 (the year after the strike). We find no significant effects of strike exposure in the placebo regressions.[41]

Two final robustness tests assess the impact of including movers from Copenhagen (Appendix Table A20) and the impact of implementing a doughnut hole approach (where we drop children born within 20 days of strike start, who were likely to loose more than one visit on average) (Appendix Table A21). Our main conclusions – that earlier strike exposure is relative more important for children's health – remain intact.

5 Costs and Benefits

In this section, we perform a stylized analysis of immediate health benefits and the costs of early NHV (relative to later NHV). Specifically, we relate the value of prevented GP visits for mothers and children to the costs of those visits. Given the most consisten evidence for an impact of the strike on the health of children and mothers exposed early, we focus in the following on the initial and two-month nurse visits. The assessment of the benefits of early

[40]Robustness tests for other outcomes are available on request.

[41]Appendix Table A19 shows results from a different placebo test that uses data from the the same periods as the main regressions but considers children aged five years and thus too old to have any nurse visits affected by the strike. We find no significant differences in the health outcomes of these cohorts.

visits is—due to our design—always relative to the benefits of later visits. Put differently, in our calculations, we assume that the the benefits of the later visits are zero.

Benefits Table A22 presents results for the impact of strike exposure on GP fees (for both mother and child) at age four.[42] As we disregard longer-run benefits, such as prevented child hospital admissions, and potential spill-over effects to other domains, such as child cognitive development or maternal timely return to the labor market, our measure of benefits (prevented GP costs) is likely very conservative.

Children born in the 30-1 and 60-31 days age groups (and their mothers) have significantly higher GP expenses, in line with our finding of increased GP contacts for these groups. Specifically, children and mothers impacted by the strike in the given groups have 154.3 and 94.2 EUR higher GP expenses at age four. To translate these costs (or the benefit from preventing them) into a measure directly linked to a forgone visit, we scale the reduced form estimates with the probability of missing the specific visits for the given groups of children and mothers.[43] Thus we estimate the benefits of the initial nurse visit and the two months nurse visit as 554.2 EUR and 184.3 EUR (prevented GP costs for child and mother up to the child's four year birthday).

Costs To quantify costs of a home visit, we only consider the direct costs related to nurses' salaries.[45] Additionally, we assume that all types of home visits have the same average cost. We calculate the cost of a home visit in two different ways that allow us to bound our

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

[43]For the first group (children born 30-1 days prior to strike) both the probability of not receiving the initial and two-months visits are increased by 17.1 and 32.3 percentage points, respectively (see Table 3). Thus to calculate the benefit of the initial visit, we scale the increase in GP fees for the 30-1 day group with the increase in their risk of missing the initial visit while subtracting the share of their increase in GP fees that can be attributed to the higher probability of also missing the two month visit: 154.3 - $184.3 \times 0.323)/0.171 =$ 554.2 EUR.[44] For the 60-31 day age group only the probability of missing the second nurse visit was impacted by the strike (51.1 percentage points). Thus, we scale their increase in GP fees due to strike exposure with the increase in the risk of forgoing the two month visit: $94.2/0.511 = 184.3$ EUR.

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

calculations: first, we conservatively assume that municipal nurses spend all working time on home visits. Second, in the alternative scenario we incorporate that nurses have other tasks beyond home visits (such as supervision of school children, consultancy and phone hours, team meetings, administrative tasks).

We estimate the weekly number of canceled visits during the strike to be 760.[46] After the strike, the municipality of Copenhagen reported daily savings during the strike of 35,500 EUR per workday or 177,500 EUR per (business) week (because the municipality did not pay salaries to the unionized nurses on strike). For our most conservative measure of costs per visit, we divide the weekly divided by the weekly number of canceled visits, $177,500$ EUR/760 visits $=$ 233.6 EUR per visit. For our alternative measure—that takes into account that nurses also have other obligations—we adjust the share of working hours nurses dedicate to home visiting to 55 percent.[47] Dividing the weekly savings during the strike adjusted with the actual time spent on home visits by the number of canceled visits, we find that the cost of a home visit in our alternative scenario is 128 EUR.[48]

Comparing costs and benefits In both described scenarios for our calculations of costs, the initial nurse visit has a positive return of between 330.6 and 425.7 EUR. This represents a substantial return given that we only included savings related to GP care and under the fairly conservative assumption that the four month and eight month visits have zero benefits. For the two month visit, we conclude that the return only related to prevented GP costs is between -39.3 and 55.8 EUR. Thus our simple analysis indicates that early universal NHV is a cost-effective intervention. Our estimates highlight the importance of timing: While the cost of an initial visit is considerably lower than the associated health care savings at age

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

[47]In our data for the control period, 155 nurses performed visits implying that the average nurse had $845/155 = 5.5$ weekly visits. Assuming that one visit lasts 1.5 hours and that nurses spend an additional 1.5 hours on preparation, transportation and registration, nurses spend 5.5 visits $\times$ 1.5 hours at actual visit $\times$ 1.5 hours for tasks related to visit = 16.5 hours weekly on NHV. If we assume that the average nurse work 30 hours per week, we estimate that nurses spend $16.5/30 = 55$ percent of their working time on NHV.

[48](177.500 EUR $\times$ 55percent)/760 visits = 128 EUR per visit

four, the difference in the increase in GP fees and the savings from canceling a two month visit is considerably smaller.

6 Conclusion

Using nurse records linked to administrative data and exploiting exogenous variation in the timing of forgone nurse visits, we provide causal evidence on intensive margin impacts of NHV. Studying the Danish universal program, we find that early NHV (during the initial weeks and first two months of the child's life) impacts both child and maternal health trajectories (measured in our analyses as health care usage). We conclude that earlier visits are more important for children's and mother's health than later visits. While we cannot fully disentangle underlying reasons for increased health care usage for children and their mothers, we show that access to early NHV impacts emergency GP contacts and children's hospitalization—also when we omit first year outcomes. Both findings point to actual health effects rather than substitution.

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

Importantly, our findings highlight that early NHV also plays a role for maternal postpartum mental health outcomes. As a consequence, our results imply that early home visits are likely to impact children through their impact on mothers: Existing research documents strong correlations between maternal postnatal mental health and child outcomes in different domains, and highlights the importance of early detection of maternal mental health problems. Thus early universal home visits can play an important role in securing population maternal and child health through the prevention of undetected and hence untreated mental health problems. In this aspect, our study echoes the finding of other recent work pointing to the importance of supporting the health of new mothers.

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

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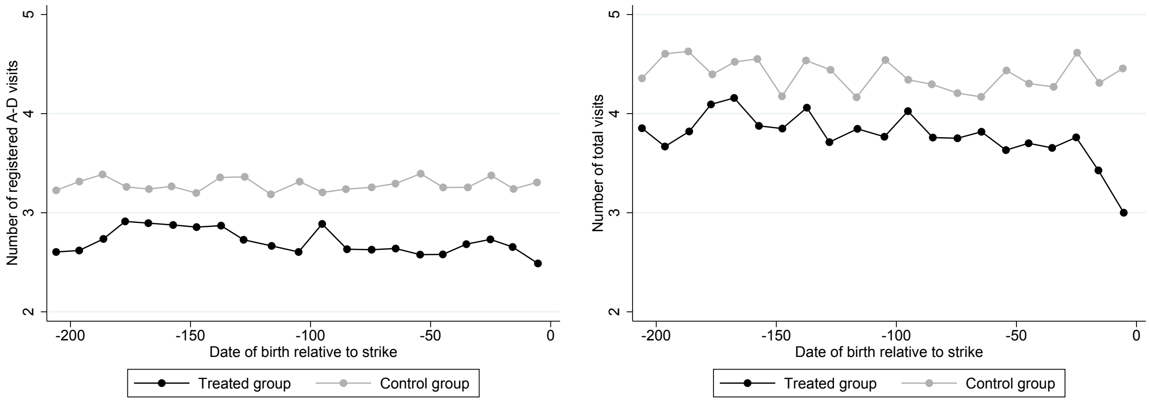
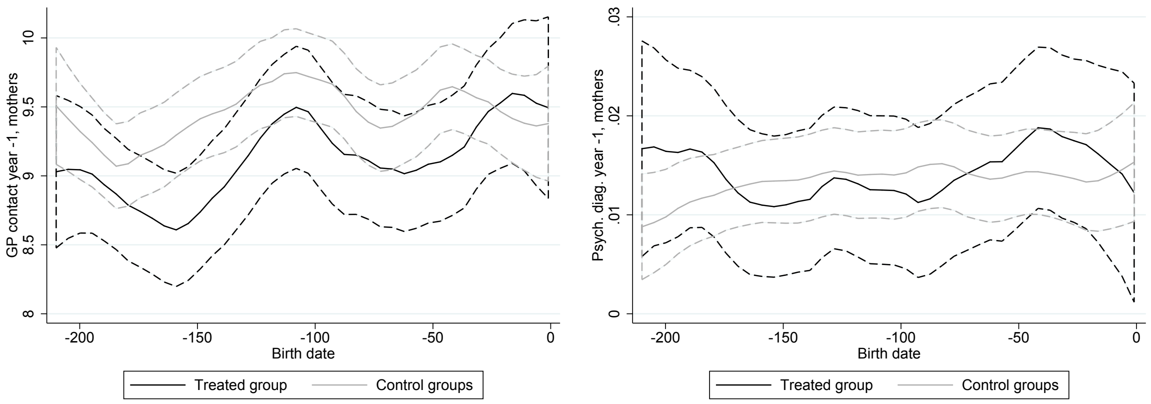
A Appendix - For online publication



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

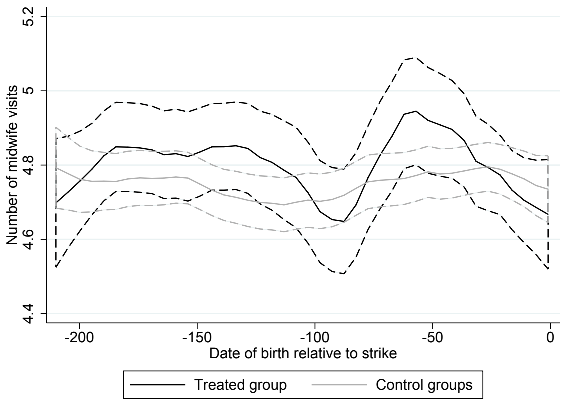
Fig. A1 Average number of universal and total nurse home visits for children in the treated and control period

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

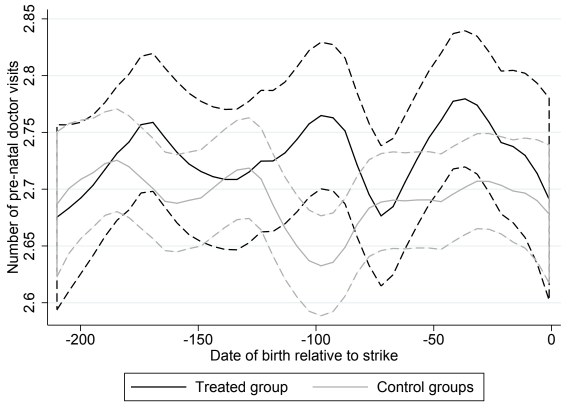


(a) Mothers GP contacts, 365 days prior birth **(b)** Mother psychiatric diagnosis, prior to birth

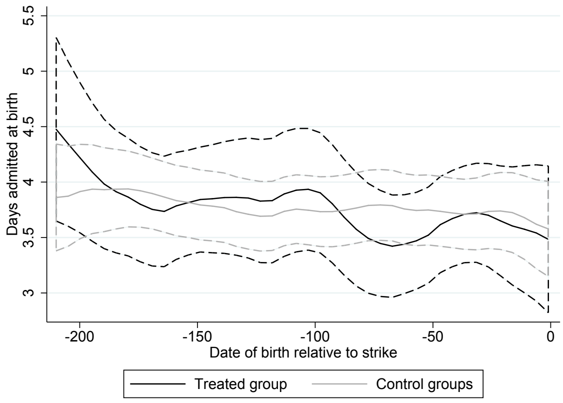
Fig. A2 Common trend in pre-treatment health outcome: Number of mothers' GP contacts (a) and indicator if mother received a psychiatric diagnosis (b) in the year prior to birth
Notes: Se notes to Figure 3. Treated period: September 18, 2007 - April 15, 2008. Control period: September 17, 2008 and 2009 - April 15, 2009 and 2010).



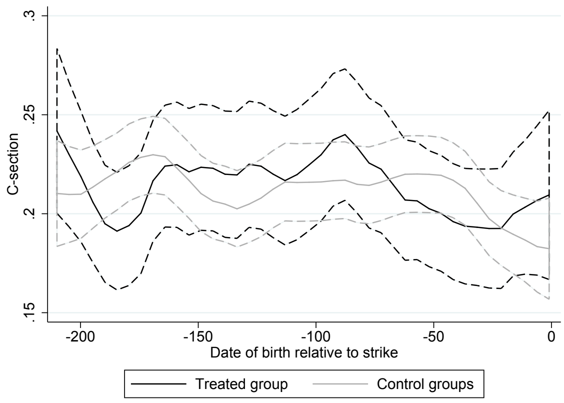
(a) Midwife visits



(b) Prenatal GP contacts



(c) Days admitted at birth



(d) C-sections

Fig. A3 Care around birth for the treated period and control periods

Notes: Panel (a) shows the number of prenatal midwife contacts, panel (b) shows the number of prenatal GP consultations, panel (c) shows the number of days admitted to hospital at birth and panel (d) shows the C-section rate. See notes to Figure 3. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010).

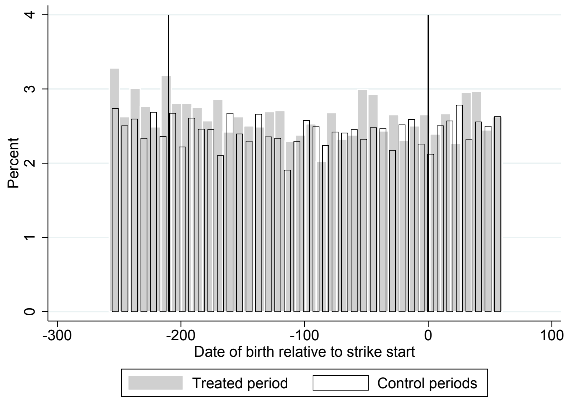


Fig. A4 Density of births

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

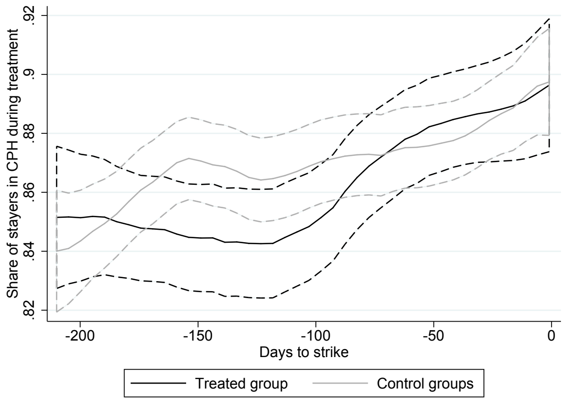
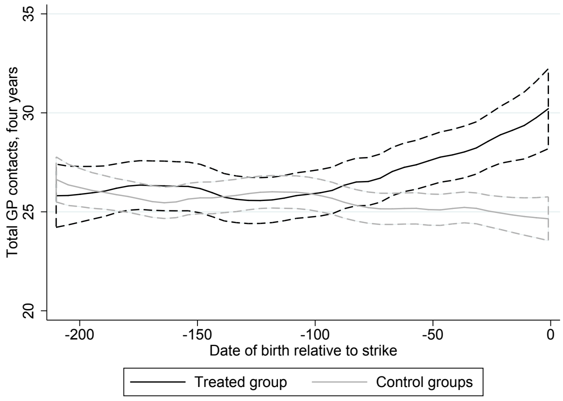
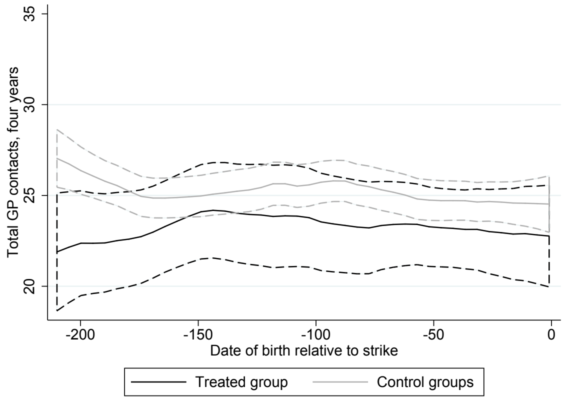


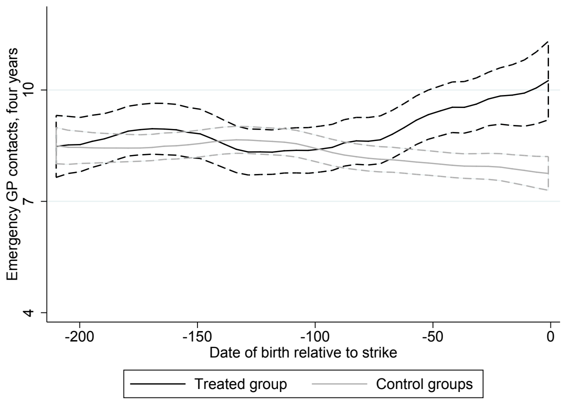
Fig. A5 Share of children observed as Copenhagen residents on January 1 in the treated and control periods
Notes: Se notes to Figure 3. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010).



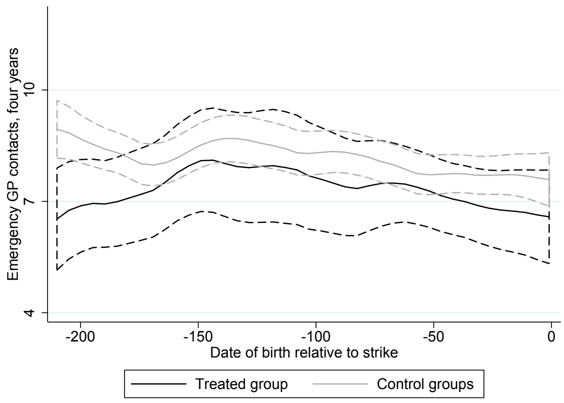
(a) Total GP contacts - Not Health educated



(b) Total GP contacts - Health educated



(c) Emergency GP contacts - Not health educated



(d) Emergency GP contacts - Health educated

Fig. A6 Accumulated GP contacts at age four for children born in the treated period (September 18, 2007 - April 15, 2008) and the control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010) and whether parents are educated in health

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

Table A1 Nurse home visiting in the municipality of Copenhagen

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

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

Table A2 Balance testing: Parental covariates as outcome

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

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

Table A3 Balance testing: Covariates at birth as outcome

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

Notes: See notes for Table A2. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

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

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

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

Table A5 Additional child health outcomes: Effects of strike exposure on child hospitalization and outpatient contacts

	(1) Hospital adm. 1st year	(2) Hospital adm. 2nd year	(3) Hospital adm. 3-4 years	(4) Outpat. cont. 1st year	(5) Outpat. cont. 2nd year	(6) Outpat. cont. 3-4 years
Days						
180-151	0.008	0.007	0.032	0.009	-0.029	0.024
	(0.030)	(0.027)	(0.028)	(0.035)	(0.033)	(0.036)
150-121	-0.032	-0.009	-0.015	0.038	-0.026	0.035
	(0.030)	(0.028)	(0.028)	(0.035)	(0.032)	(0.036)
120-91	0.001	0.015	-0.032	-0.012	-0.021	-0.051
	(0.031)	(0.028)	(0.028)	(0.035)	(0.033)	(0.036)
90-61	0.037	0.024	-0.015	-0.003	-0.032	-0.029
	(0.031)	(0.029)	(0.028)	(0.035)	(0.033)	(0.036)
60-31	0.004	0.067**	0.013	-0.027	-0.005	0.000
	(0.030)	(0.028)	(0.028)	(0.035)	(0.032)	(0.035)
30-1	0.036	0.078***	0.007	-0.060*	0.025	-0.002
	(0.030)	(0.029)	(0.028)	(0.034)	(0.033)	(0.036)
Control group mean	0.27	0.19	0.18	0.39	0.27	0.41
Obs.	12078	11982	11709	12078	11982	11709

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

Table A6 Heterogeneity: Effects of strike exposure on total GP contacts by parental education and parity

	Not health educ.		Health educ.		Higher parity		First-borns	
	Total GP 1st year	Total GP 2-4 years	Total GP 1st year	Total GP 2-4 years	Total GP 1st year	Total GP 2-4 years	Total GP 1st year	Total GP 2-4 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Days								
180-151	0.478	0.737	0.723	2.362	0.564	-1.207	0.478	2.134
	(0.569)	(1.081)	(1.241)	(2.333)	(0.745)	(1.489)	(0.700)	(1.317)
150-121	0.153	0.921	1.313	1.096	0.198	-1.147	0.381	2.225*
	(0.554)	(1.046)	(1.477)	(2.859)	(0.801)	(1.490)	(0.679)	(1.307)
120-91	-0.078	-0.389	-1.318	0.543	-0.482	-0.710	0.020	0.509
	(0.570)	(1.015)	(1.358)	(2.522)	(0.807)	(1.498)	(0.693)	(1.236)
90-61	0.754	1.594	0.459	0.318	0.520	0.462	0.842	2.002
	(0.601)	(1.040)	(1.387)	(2.619)	(0.754)	(1.433)	(0.766)	(1.307)
60-31	0.262	2.347**	0.517	2.244	0.339	1.304	0.397	3.214**
	(0.579)	(1.051)	(1.267)	(2.410)	(0.774)	(1.415)	(0.716)	(1.318)
30-1	1.924***	3.484***	1.375	1.234	2.319***	1.574	1.511**	3.873***
	(0.611)	(1.101)	(1.326)	(2.159)	(0.831)	(1.462)	(0.738)	(1.341)
Control group mean	4.72	20.95	3.52	18.75	4.07	17.88	4.91	22.59
Observations	10445	10114	1633	1584	4750	4605	7328	7093

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

Table A7 Heterogeneity: Effects of strike exposure on total GP contacts, interacted model

	Health education		Parity	
	Total GP 1st year	Total GP 2-4 years	Total GP 1st year	Total GP 2-4 years
	(1)	(2)	(3)	(4)
Days				
180-151	-0.153	1.228	-0.022	3.279*
	(1.336)	(2.657)	(1.018)	(1.987)
150-121	0.953	0.473	0.160	3.289*
	(1.541)	(3.032)	(1.051)	(1.980)
120-91	-1.052	1.807	0.600	1.093
	(1.468)	(2.754)	(1.063)	(1.931)
90-61	-0.173	-0.165	0.177	1.282
	(1.493)	(2.797)	(1.074)	(1.932)
60-31	0.168	0.852	0.101	1.817
	(1.355)	(2.633)	(1.049)	(1.927)
30-1	-0.773	-2.437	-0.757	2.137
	(1.454)	(2.425)	(1.111)	(1.982)
Observations	12078	11698	12078	11698

Notes: Each column shows the estimates from separate regressions. Column labels indicate the relevant subgroup of our sample. The coefficients are for the interactions of 30 day bins, a strike indicator and subgroup. All regressions include period, bin fixed effects and the interaction between bin indicators and strike exposure and full interactions between those and subgroup indicator. Regressions also include all control variables (see notes for Table 3). The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A8 Heterogeneity: Effects of strike exposure on total GP contacts at age four

	Gender		Initial health		SES		Smoking, mother	
	Boys	Girls	Not poor	poor	High	Low	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Days								
180-151	3.288*	-0.946	1.094	5.329	2.289	-0.897	1.038	4.902
	(1.802)	(1.846)	(1.366)	(3.803)	(1.424)	(2.733)	(1.326)	(5.182)
150-121	0.263	2.525	1.410	1.095	3.238**	-3.478	1.211	3.083
	(1.753)	(1.870)	(1.333)	(4.672)	(1.442)	(2.633)	(1.326)	(4.676)
120-91	0.596	-1.493	-0.919	8.509**	1.232	-4.064	-0.892	4.944
	(1.770)	(1.749)	(1.307)	(4.152)	(1.389)	(2.615)	(1.284)	(4.908)
90-61	3.727**	0.616	2.263*	1.520	3.125**	-0.242	1.846	5.659
	(1.837)	(1.807)	(1.358)	(4.148)	(1.484)	(2.569)	(1.360)	(4.350)
60-31	3.580*	1.799	1.911	13.082**	3.869***	0.267	2.224	8.323**
	(1.918)	(1.742)	(1.330)	(5.538)	(1.443)	(2.682)	(1.363)	(4.109)
30-1	6.457***	2.617	4.336***	8.088	5.001***	4.105	4.344***	8.438*
	(1.871)	(1.857)	(1.379)	(5.189)	(1.447)	(2.914)	(1.374)	(4.834)
Control group mean	26.35	24.00	25.17	26.97	24.68	26.51	25.05	27.99
Observations	6085	5644	10803	926	8381	3348	10681	1048

Notes: See notes to Table 3 and A6. Columns (1)-(2) split the sample by child gender. Columns (3)-(4) split the sample by initial health (low birth weight, premature birth or complications during birth). Columns (5)-(6) split the sample by parental socio-economic status (SES). A low SES background is a child born to parents with either incomes in the bottom decile, below age 21 at birth or with only primary schooling. Columns (7)-(8) split the sample by whether the mother smoked during pregnancy. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A9 Heterogeneity: Effects of strike exposure on total GP contacts, interacted model

	Gender (1)	Initial health (2)	SES (3)	Smoking, mother (4)
Days				
180-151	-4.637*	2.944	-3.243	2.921
	(2.579)	(3.871)	(3.062)	(5.300)
150-121	1.960	-1.102	-6.915**	0.911
	(2.557)	(4.512)	(2.968)	(4.741)
120-91	-2.241	8.480**	-5.389*	5.551
	(2.485)	(4.288)	(2.921)	(5.090)
90-61	-3.602	-1.646	-3.354	3.713
	(2.571)	(4.178)	(2.937)	(4.333)
60-31	-1.861	10.523*	-3.439	5.498
	(2.580)	(5.443)	(3.002)	(4.281)
30-1	-4.085	3.040	-1.077	3.051
	(2.634)	(5.074)	(3.229)	(4.954)
Observations	11729	11729	11729	11729

Notes: See notes for Table A7. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

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

	(1) Prev. care, 5 weeks	(2) Prev. care, 5 months	(3) Prev. care, 12 months	(4) Prev. care, 2 years	(5) Prev. care, 3 years	(6) Prev. care, 4 years
Days						
180-151	0.002	0.005	0.005	0.065*	0.057	0.034
	(0.022)	(0.019)	(0.018)	(0.034)	(0.035)	(0.030)
150-121	0.007	-0.008	0.011	0.043	0.036	0.005
	(0.021)	(0.018)	(0.019)	(0.034)	(0.035)	(0.031)
120-91	-0.009	-0.008	-0.009	0.010	-0.034	-0.017
	(0.022)	(0.019)	(0.019)	(0.035)	(0.036)	(0.031)
90-61	0.015	0.004	0.012	0.107***	0.099***	0.039
	(0.021)	(0.020)	(0.018)	(0.034)	(0.036)	(0.031)
60-31	0.017	-0.014	0.029*	0.034	0.090***	0.018
	(0.021)	(0.019)	(0.018)	(0.033)	(0.034)	(0.030)
30-1	0.012	-0.000	0.017	0.056	0.083**	0.037
	(0.020)	(0.019)	(0.018)	(0.034)	(0.035)	(0.030)
Control group mean	0.92	0.93	0.93	0.66	0.58	0.79
Obs.	12078	12078	12078	11982	11832	11729

Notes: See notes for Table 3. Outcomes are indicators for participation in each consultation in the preventive health care program. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A11 Parental investments: Effects of strike exposure on participation in the infant vaccination program

	(1) Vacc., 1st round	(2) Vacc., 2nd round	(3) Vacc., 3rd round
Days			
180-151	-0.025	-0.015	-0.036*
	(0.025)	(0.023)	(0.022)
150-121	-0.005	-0.032	-0.039*
	(0.024)	(0.023)	(0.022)
120-91	0.013	-0.009	-0.045**
	(0.024)	(0.023)	(0.023)
90-61	-0.011	-0.010	-0.021
	(0.025)	(0.024)	(0.023)
60-31	-0.018	-0.026	0.017
	(0.024)	(0.023)	(0.022)
30-1	0.006	0.001	-0.034
	(0.024)	(0.023)	(0.022)
Control group mean	0.90	0.91	0.91
Obs.	12078	12078	12078

Notes: See notes for Table 3. Outcomes are indicators for participation in each vaccination round scheduled within the first year of a child's life. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A12 Parental investments: Effects of strike exposure on the probability of delayed infant vaccinations

	(1) Vacc. 1st round, 2 month late	(2) Vacc. 2nd round, 2 month late	(3) Vacc. 3rd round, 2 month late
Days			
180-151	0.033	0.006	0.025
	(0.028)	(0.028)	(0.030)
150-121	0.002	-0.005	-0.009
	(0.028)	(0.028)	(0.031)
120-91	0.013	0.009	0.034
	(0.028)	(0.029)	(0.031)
90-61	0.024	-0.002	-0.004
	(0.028)	(0.029)	(0.031)
60-31	0.041	0.016	-0.016
	(0.028)	(0.028)	(0.030)
30-1	0.004	-0.022	-0.011
	(0.028)	(0.028)	(0.030)
Control group mean	0.15	0.15	0.22
Obs.	12078	12078	12078

Notes: See notes for Table 3. Outcomes are indicators for delayed or no participation in each vaccination round scheduled within the first year of a child's life. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A13 Robustness: Effects of strike exposure on child GP contacts without pre-treatment covariates

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
180-151	0.470	0.025	0.556	0.166	-0.211	0.359	0.304	0.236	0.197
	(0.513)	(0.513)	(0.595)	(0.344)	(0.318)	(0.387)	(0.221)	(0.296)	(0.313)
150-121	0.312	0.305	0.505	0.174	0.142	0.428	0.138	0.163	0.077
	(0.514)	(0.505)	(0.598)	(0.339)	(0.317)	(0.385)	(0.228)	(0.289)	(0.313)
120-91	-0.294	-0.334	-0.248	-0.272	-0.053	0.036	-0.022	-0.281	-0.284
	(0.518)	(0.486)	(0.563)	(0.348)	(0.305)	(0.378)	(0.218)	(0.283)	(0.295)
90-61	0.808	0.763	1.054*	0.370	0.469	0.687*	0.438**	0.294	0.366
	(0.545)	(0.510)	(0.581)	(0.373)	(0.327)	(0.387)	(0.223)	(0.284)	(0.305)
60-31	0.268	1.571***	0.400	0.068	0.896***	0.257	0.201	0.675**	0.143
	(0.520)	(0.501)	(0.573)	(0.346)	(0.317)	(0.376)	(0.222)	(0.288)	(0.300)
30-1	1.750***	1.362***	1.033*	1.077***	0.781**	0.585	0.674***	0.581**	0.448
	(0.550)	(0.516)	(0.584)	(0.371)	(0.328)	(0.390)	(0.233)	(0.288)	(0.297)
Control group mean	4.55	10.35	10.22	3.09	6.89	7.04	1.47	3.46	3.18
Obs.	12568	12464	12177	12568	12464	12177	12568	12464	12177

Notes: See notes to Table 3. We estimate the effects of strike exposure without pre-treatment covariates. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Table A14 Robustness: Effects of strike exposure on child GP contacts, larger bin size - 35 days

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
175-141	0.266	0.318	0.529	0.107	0.089	0.336	0.159	0.229	0.193
	(0.476)	(0.472)	(0.557)	(0.318)	(0.294)	(0.363)	(0.209)	(0.271)	(0.291)
140-106	-0.544	-0.334	0.189	-0.377	-0.027	0.277	-0.168	-0.307	-0.088
	(0.483)	(0.472)	(0.540)	(0.323)	(0.296)	(0.357)	(0.207)	(0.270)	(0.280)
105-71	0.174	0.412	0.206	0.041	0.355	0.119	0.133	0.057	0.087
	(0.508)	(0.463)	(0.524)	(0.347)	(0.293)	(0.354)	(0.211)	(0.265)	(0.274)
70-36	0.411	1.450***	0.805	0.141	0.788***	0.419	0.270	0.662**	0.386
	(0.490)	(0.465)	(0.523)	(0.328)	(0.298)	(0.348)	(0.205)	(0.265)	(0.271)
35-1	1.454***	1.579***	0.977*	0.875**	1.052***	0.552	0.579***	0.527*	0.425
	(0.512)	(0.485)	(0.536)	(0.346)	(0.308)	(0.363)	(0.218)	(0.272)	(0.270)
Control group mean	4.55	10.35	10.22	3.09	6.89	7.04	1.47	3.46	3.18
Observations	12078	11982	11709	12078	11982	11709	12078	11982	11709

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

Table A15 Robustness: Effects of strike exposure on child GP contacts, smaller bin size - 21 days

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
189-169	0.886	0.00730	1.000	0.463	-0.0659	0.534	0.423	0.0732	0.465
	(0.604)	(0.599)	(0.683)	(0.407)	(0.381)	(0.453)	(0.258)	(0.340)	(0.356)
168-148	0.736	0.373	0.442	0.501	0.0792	0.417	0.235	0.294	0.0249
	(0.605)	(0.615)	(0.722)	(0.411)	(0.378)	(0.470)	(0.258)	(0.353)	(0.379)
147-127	0.293	0.491	0.324	0.297	0.437	0.422	-0.00421	0.0540	-0.0976
	(0.622)	(0.609)	(0.717)	(0.409)	(0.384)	(0.474)	(0.277)	(0.346)	(0.362)
126-106	-0.134	-0.409	0.584	-0.146	-0.0567	0.500	0.0122	-0.353	0.0844
	(0.613)	(0.600)	(0.691)	(0.419)	(0.381)	(0.461)	(0.249)	(0.337)	(0.368)
105-85	0.657	0.731	0.297	0.383	0.504	0.198	0.274	0.227	0.0991
	(0.650)	(0.587)	(0.670)	(0.440)	(0.377)	(0.454)	(0.279)	(0.338)	(0.353)
84-64	0.693	0.260	0.583	0.344	0.259	0.400	0.349	0.00154	0.183
	(0.643)	(0.606)	(0.676)	(0.444)	(0.387)	(0.461)	(0.256)	(0.333)	(0.347)
63-43	1.082*	1.740***	0.965	0.629	1.053***	0.607	0.453*	0.687**	0.358
	(0.627)	(0.595)	(0.675)	(0.423)	(0.388)	(0.454)	(0.260)	(0.336)	(0.349)
42-22	0.527	1.759***	1.435**	0.245	1.172***	0.869*	0.282	0.588*	0.566
	(0.636)	(0.620)	(0.697)	(0.426)	(0.393)	(0.459)	(0.274)	(0.347)	(0.363)
21-1	2.638***	1.749***	1.163*	1.687***	0.942**	0.567	0.951***	0.807**	0.596*
	(0.665)	(0.623)	(0.701)	(0.453)	(0.396)	(0.481)	(0.277)	(0.350)	(0.349)
Control group mean	4.55	10.35	10.22	3.09	6.89	7.04	1.47	3.46	3.18
Observations	12078	11982	11709	12078	11982	11709	12078	11982	11709

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

Table A16 Robustness: Effects of strike exposure on child GP contacts, 210 days prior to April 15, 2008 as strike exposed period and 210 days prior to April 15 2007, 2009, 2010 as control periods

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
180-151	0.467	0.068	0.370	0.141	-0.234	0.240	0.326	0.301	0.131
	(0.485)	(0.485)	(0.570)	(0.327)	(0.301)	(0.373)	(0.207)	(0.278)	(0.298)
150-121	1.147**	0.283	0.378	0.701**	0.093	0.232	0.446**	0.190	0.145
	(0.480)	(0.478)	(0.568)	(0.319)	(0.302)	(0.369)	(0.210)	(0.272)	(0.295)
120-91	0.180	-0.121	-0.113	0.005	0.041	0.212	0.175	-0.162	-0.325
	(0.491)	(0.462)	(0.537)	(0.335)	(0.293)	(0.365)	(0.203)	(0.264)	(0.276)
90-61	0.887*	0.561	0.434	0.398	0.409	0.241	0.489**	0.152	0.193
	(0.518)	(0.482)	(0.555)	(0.359)	(0.312)	(0.372)	(0.208)	(0.266)	(0.289)
60-31	0.465	1.636***	0.705	0.125	0.918***	0.412	0.340	0.718***	0.293
	(0.494)	(0.480)	(0.546)	(0.331)	(0.303)	(0.360)	(0.210)	(0.275)	(0.284)
30-1	1.731***	1.417***	1.007*	1.061***	0.948***	0.563	0.670***	0.469*	0.445
	(0.527)	(0.493)	(0.561)	(0.357)	(0.315)	(0.376)	(0.221)	(0.275)	(0.284)
Control group mean	4.27	10.33	10.47	2.92	6.92	7.18	1.35	3.41	3.29
Obs.	15736	15616	15248	15736	15616	15248	15736	15616	15248

Notes: See notes to Table 3. We add an additional control period (September 17, 2006 - April 15, 2007) to the sample. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.10$.

Table A17 Robustness: Effects of strike exposure on child GP contacts, 210 days prior to April 15, 2008 as strike exposed period and 210 days prior to April 15, 2009 as control period (first stage sample)

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
180-151	0.331	0.181	0.897	0.169	-0.209	0.424	0.162	0.391	0.473
	(0.593)	(0.595)	(0.678)	(0.397)	(0.372)	(0.445)	(0.258)	(0.345)	(0.353)
150-121	0.341	0.546	0.934	0.362	0.349	0.647	-0.021	0.197	0.287
	(0.594)	(0.598)	(0.691)	(0.386)	(0.366)	(0.446)	(0.273)	(0.351)	(0.363)
120-91	0.347	-0.450	0.173	0.311	-0.316	0.223	0.035	-0.134	-0.050
	(0.604)	(0.573)	(0.659)	(0.402)	(0.362)	(0.447)	(0.262)	(0.332)	(0.339)
90-61	0.920	0.418	0.835	0.508	0.267	0.398	0.412	0.151	0.437
	(0.627)	(0.598)	(0.668)	(0.430)	(0.380)	(0.448)	(0.256)	(0.337)	(0.349)
60-31	0.316	1.739***	0.870	0.028	0.779**	0.488	0.288	0.960***	0.382
	(0.612)	(0.588)	(0.677)	(0.410)	(0.374)	(0.448)	(0.263)	(0.336)	(0.350)
30-1	2.399***	2.008***	1.212*	1.577***	0.995***	0.648	0.822***	1.012***	0.565*
	(0.626)	(0.594)	(0.673)	(0.423)	(0.382)	(0.454)	(0.264)	(0.332)	(0.341)
Control group mean	4.29	10.82	10.62	2.86	7.17	7.28	1.43	3.65	3.35
Obs.	7874	7814	7633	7874	7814	7633	7874	7814	7633

Notes: See notes to Table 3. We reduce the sample to only include the first stage sample. The sample includes children who were born in Copenhagen in the treated period (September 18, 2007 - April 15, 2008) and in the control period (September 17, 2008 - April 15, 2009). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A18 Placebo test: The effect of strike exposure on child health measured as accumulated GP contacts by type, data for the two control years 2009 and 2010

	(1) Total GP 1st year	(2) Total GP 2-4 years	(3) Ordin. GP 1st year	(4) Ordin. GP 2-4 years	(5) Emerg. GP 1st year	(6) Emerg. GP 2-4 years
Days						
180-151	0.270	-0.505	-0.076	0.153	0.346	-0.658
	(0.621)	(1.097)	(0.411)	(0.696)	(0.267)	(0.589)
150-121	-0.154	-1.188	-0.455	-0.968	0.300	-0.220
	(0.623)	(1.084)	(0.399)	(0.670)	(0.284)	(0.599)
120-91	-0.990	0.315	-1.019**	0.632	0.029	-0.317
	(0.637)	(1.096)	(0.414)	(0.691)	(0.282)	(0.592)
90-61	-0.341	0.678	-0.387	0.651	0.047	0.027
	(0.629)	(1.089)	(0.420)	(0.688)	(0.265)	(0.585)
60-31	0.032	-0.494	0.057	0.054	-0.025	-0.548
	(0.634)	(1.083)	(0.415)	(0.694)	(0.279)	(0.583)
30-1	-0.991	-0.529	-0.863**	0.114	-0.129	-0.643
	(0.623)	(1.069)	(0.417)	(0.699)	(0.263)	(0.552)
Control group mean	4.55	20.65	3.09	13.98	1.47	6.67
Obs.	8203	7941	8203	7941	8203	7941

Notes: Each column shows the estimates from separate regressions. The coefficients are for the interactions of 30 day bins and a strike indicator. All regressions include period and bin fixed effects, as well as control variables (see notes for Table 3). The sample includes children who were born in Copenhagen in the placebo treated period (September 17, 2008 - April 15, 2009) and in control period (September 17, 2009 - April 15, 2010). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A19 Placebo test: The effect of strike exposure on child health measured as accumulated GP contacts by type, data for untreated (older) cohorts in 2008, 2009, 2010

	(1) Total GP 1st year	(2) Total GP 2-4 years	(3) Ordin. GP 1st year	(4) Ordin. GP 2-4 years	(5) Emerg. GP 1st year	(6) Emerg. GP 2-4 years
Days						
180-151	-0.116	0.064	-0.017	0.264	-0.099	-0.200
	(0.250)	(0.554)	(0.195)	(0.433)	(0.116)	(0.220)
150-121	-0.287	0.198	-0.110	0.128	-0.177	0.070
	(0.257)	(0.574)	(0.196)	(0.441)	(0.125)	(0.239)
120-91	0.168	-0.320	0.205	-0.070	-0.037	-0.250
	(0.262)	(0.562)	(0.206)	(0.428)	(0.122)	(0.238)
90-61	-0.452*	-0.176	-0.278	-0.120	-0.174	-0.055
	(0.252)	(0.573)	(0.196)	(0.450)	(0.118)	(0.219)
60-31	0.095	-0.651	0.094	-0.345	0.000	-0.306
	(0.261)	(0.563)	(0.201)	(0.433)	(0.122)	(0.234)
30-1	0.054	-0.020	0.046	0.066	0.008	-0.086
	(0.254)	(0.567)	(0.198)	(0.445)	(0.118)	(0.225)
Control group mean	3.68	8.85	2.76	6.78	0.93	2.07
Obs.	10260	10260	10260	10260	10260	10260

Notes: See notes to Table A18. The sample includes children who were born in Copenhagen 5 years prior to the treated period (September 18, 2007 - April 15, 2008) and the control periods (September 17, 2008 and 2009 - April 15, 2009 and 2010). Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A20 Robustness: Effects of strike exposure on child GP contacts, including movers from Copenhagen

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
180-151	0.257	-0.197	0.452	0.060	-0.199	0.483	0.198	0.002	-0.031
	(0.478)	(0.484)	(0.557)	(0.322)	(0.301)	(0.366)	(0.203)	(0.279)	(0.294)
150-121	0.296	0.234	0.538	0.157	0.157	0.477	0.140	0.076	0.061
	(0.483)	(0.475)	(0.549)	(0.318)	(0.303)	(0.356)	(0.214)	(0.269)	(0.290)
120-91	-0.318	-0.554	-0.252	-0.231	-0.066	0.191	-0.087	-0.488*	-0.443
	(0.482)	(0.465)	(0.525)	(0.326)	(0.298)	(0.355)	(0.201)	(0.265)	(0.274)
90-61	0.515	0.418	0.593	0.169	0.340	0.456	0.346*	0.078	0.137
	(0.505)	(0.482)	(0.547)	(0.347)	(0.311)	(0.367)	(0.205)	(0.267)	(0.286)
60-31	0.099	1.384***	0.512	-0.004	0.812***	0.529	0.102	0.572**	-0.017
	(0.489)	(0.479)	(0.539)	(0.326)	(0.305)	(0.359)	(0.208)	(0.273)	(0.282)
30-1	1.486***	1.146**	0.811	0.969***	0.720**	0.511	0.517**	0.426	0.300
	(0.518)	(0.490)	(0.547)	(0.352)	(0.316)	(0.367)	(0.216)	(0.271)	(0.279)
Control group mean	4.54	10.41	10.26	3.06	6.94	7.07	1.48	3.47	3.18
Obs.	13918	13611	13316	13918	13611	13316	13918	13611	13316

Notes: See notes to Table 3. We add children who were born in Copenhagen during the sample period but moved from Copenhagen during the first year of life. We drop these children from the main sample. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Table A21 Robustness: Effects of strike exposure on child GP contacts, doughnut approach

	(1) Total GP 1st year	(2) Total GP 2nd year	(3) Total GP 3-4 years	(4) Ordin. GP 1st year	(5) Ordin. GP 2nd year	(6) Ordin. GP 3-4 years	(7) Emerg. GP 1st year	(8) Emerg. GP 2nd year	(9) Emerg. GP 3-4 years
Days									
180-151	0.453	0.121	0.703	0.131	-0.151	0.444	0.322	0.272	0.259
	(0.516)	(0.515)	(0.598)	(0.348)	(0.320)	(0.391)	(0.222)	(0.296)	(0.313)
150-121	0.299	0.394	0.532	0.150	0.191	0.359	0.149	0.203	0.173
	(0.518)	(0.508)	(0.595)	(0.342)	(0.319)	(0.386)	(0.229)	(0.290)	(0.310)
120-91	-0.188	-0.186	-0.002	-0.216	0.008	0.192	0.028	-0.194	-0.194
	(0.527)	(0.492)	(0.564)	(0.355)	(0.310)	(0.383)	(0.222)	(0.283)	(0.291)
90-61	0.766	0.690	0.847	0.334	0.440	0.510	0.432*	0.250	0.336
	(0.551)	(0.512)	(0.581)	(0.379)	(0.329)	(0.390)	(0.223)	(0.284)	(0.303)
60-41	0.239	1.717***	0.582	0.028	0.952***	0.347	0.211	0.766**	0.235
	(0.581)	(0.545)	(0.616)	(0.389)	(0.352)	(0.414)	(0.244)	(0.310)	(0.319)
40-20	0.718	1.585***	1.304**	0.275	1.005***	0.755*	0.442	0.580*	0.549
	(0.613)	(0.582)	(0.655)	(0.404)	(0.371)	(0.425)	(0.271)	(0.324)	(0.341)
Control group mean									
Obs.	11008	10935	10690	11008	10935	10690	11008	10935	10690

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

Table A22 Effect of strike exposure on child and mother health measured as accumulated and yearly total GP fees, Euro

	(1) Total GP mother and child 1st year	(2) Total GP mother and child 2nd year	(3) Total GP mother and child 3-4 years	(4) Total GP mother and child <4y
Days				
180-151	8.586	9.741	17.876	33.850
	(14.174)	(13.573)	(19.877)	(39.289)
150-121	10.476	29.762**	30.077	66.267*
	(13.846)	(13.437)	(19.355)	(38.443)
120-91	0.230	15.548	1.902	12.530
	(14.013)	(13.329)	(18.994)	(37.556)
90-61	8.850	40.319***	37.636*	87.099**
	(14.388)	(13.619)	(19.205)	(38.453)
60-31	14.918	52.904***	31.088	94.165**
	(14.229)	(13.726)	(19.487)	(39.242)
30-1	46.565***	61.200***	48.826**	154.281***
	(14.761)	(13.752)	(19.190)	(39.327)
Control group mean	240.22	307.22	426.41	978.01
Obs.	12078	11982	11709	11698

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

Chapter 3

Inattention or Reluctance? Parental Responses to Vaccination Reminder Letters

Inattention or Reluctance? Parental Responses to Vaccination Reminder Letters *

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Abstract

This paper studies non-adherence in the Danish Childhood Vaccination Program using a nationwide introduction of vaccination reminder letters and administrative data from 2011-2017. First, I provide causal estimates of how vaccination reminder letters affect adherence using a Regression Discontinuity Design (RDD). Second, I link parental responses to reminder letters to parents' causes for being non-adherent. I find that reminder letters positively affect adherence. However, 72 % of non-adherent parents are non-responsive to reminders indicating that reluctance and not inattention is the leading cause for non-adherence. Thus, other policies beyond reminder letters are necessary to substantially increase vaccination coverage.

JEL Codes: I11, I12, I18

Keywords: Early-life health, vaccinations, preventive care, health behavior

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1 Introduction

Socio-economic differences in child and later-life health are well-documented (Case et al., 2002; Mackenbach et al., 2008; Chetty et al., 2016; Kreiner et al., 2018) but our understanding of their origins remains limited. One suggested explanation is differences in parental investments and health behavior. This paper addresses an important part of preventive health behavior by studying non-adherence in the free-of-charge and voluntary Danish Childhood Vaccination Program.

Non-adherence in vaccination programs in developed countries poses a major challenge for public health (Black and Rappuoli, 2010; Shetty, 2010) and scepticism is a growing tendency although scientific evidence strongly documents the importance and safety of existing vaccinations (Centers for Disease Control and Prevention, 1999; Offit et al., 2002; Elliman and Bedford, 2003; Stern and Markel, 2005). Thus, knowledge about the differential causes for vaccination non-adherence is of crucial importance in order to increase coverage rates and understand and mitigate inequalities in health behaviors.

This paper studies the effects of a reminder letter policy on subsequent vaccination behavior, sibling spill-overs and participation in other parts of the preventive care system beyond the vaccination program. To assess the driving forces of vaccination non-adherence, I propose a decomposition into responsive, non-responsive and delaying parents based on their reaction to reminder letters and provide a framework that links parental responses to reminder letters to causes for non-adherence.[1]

I exploit a national policy reform, which introduced vaccination reminder letters at May 15, 2014 in a sharp Regression Discontinuity Design (RDD) to estimate the causal effects of reminder letters. Parents with a child registered as lacking at least one scheduled vaccination

[1]See DellaVigna (2009) and Gabaix (2019) for a review on inattention, the behavioral consequences and empirical strategies to detect and measure inattention. DellaVigna (2009) presents a model of limited attention and derive three testable implications where one of these empirical strategies is to examine the response of consumers/agents to an increase in salience. As an example DellaVigna (2009) mentions Chetty et al. (2009) where the effect on sales is analyzed from posting tax inclusive prices. Gabaix (2019) specifically lists the impact of reminders as one out of five empirical strategies to measure inattention.

at age 2 receive a reminder letter. The letter only informs parents that their child has not received all recommended vaccinations but does *not* campaign for the benefits of vaccinations and highlights that vaccinations are voluntary. I provide evidence for the validity of the design by showing that a wide range of pre-determined covariates are well-balanced and that there is no bunching across the cut-off. Moreover, the results are robust to bandwidth selection, estimation method and pass various placebo tests.

I find that reminder letters increase adherence with 50 % for non-adherent children a year after receiving the reminder letter. The response is immediate as only vaccination behavior in the quarter following the intervention is affected. The timing of the response indicates that reminder letters mainly affect inattentive parents. The resulting coverage rates show that reminder letters push the rate above the minimum herd immunity threshold for measles but fall short for pertussis (whooping cough). Furthermore, I estimate positive effects from reminder letters to participation in the preventive care program but no spill-overs to later vaccination rounds. I detect negative sibling spill-overs suggesting a cost of information as parents postpone vaccinations knowing they will be reminded.

Using my estimate, I decompose the group of parents of non-adherent children at age 2. I find that 8.7 % are responsive, while 19.2 % and 72.1 % are delayers and non-responsive respectively. Moreover, by evaluating heterogeneity, I discover that responsiveness to reminders is stronger among higher parity parents, not health educated parents, parents of children with unproblematic births and parents without a university degree. The finding, that health and childcare educated parents are non-responsive to reminder letters, indicates that responsive parents were non-adherent due to inattention. Finally, a simple comparison of the effect on adherence and the cost of the policy shows that the vaccination reminder letter policy costs an estimated 641 DKK (85 EUR) per additional fully vaccinated child.

The paper contributes twofold to the understanding of vaccination adherence. First, I extend the literature on the impact of recall systems and reminder letters. While research on pro-vaccination campaigns finds modest to no effect (Leader et al., 2009; Chanel et al., 2011;

Sadaf et al., 2013; Nyhan et al., 2014; Dubé et al., 2015; Buttenheim et al., 2016; Baskin, 2018)[2], studies highlight that reminders and recall systems improve coverage in a wide variety of contexts (Vann and Szilagyi, 2005; Harvey et al., 2015). Closely related, Suppli et al. (2017, 2018) evaluate the effects of reminder letters in Denmark using the same natural experiment in a before-after strategy. They use a before-sample one year prior to the intervention and an after-sample one year post the the intervention. They find that during a 6-month follow-up period 2,264 more vaccinations are administered to children in the treatment group. I extend their analysis of the impact of reminder letters in four distinct ways: i) I address causality more rigorously by dealing with time trends and seasonality in a RDD, ii) I relate the impact on adherence to herd immunity thresholds, iii) I study other types of preventive care participation beyond the vaccination program and iv) I test for persistent effects by studying younger siblings of the treated children.

Second, I provide evidence on the factors influencing vaccination non-adherence. Existing evidence has identified factors such as parents' perception of infections and side-effects risks, religion, ideology and social pressure (Tickner et al., 2006; Grabenstein, 2013; Wombwell et al., 2015; Larson et al., 2016; Amin et al., 2017; Karing, 2018). Recently, a growing literature highlights misinformation as a major factor fuelling reluctance (Anderberg et al., 2011; Chang, 2018; Hansen and Schmidtblaicher, 2019; Carrieri et al., 2019).[3] The economic literature furthermore shows the difficulties in reaching full coverage due to free-riding (Philipson, 1996; Geoffard and Philipson, 1997; Oster, 2018): When a substantial share of the population

[2]Leader et al. (2009) show that the framing of the HPV vaccine impacts intentions to vaccinate. Chanel et al. (2011) find that only information of scientific character has positive impact on intentions. Nyhan et al. (2014) conduct a randomized experiment with pro-vaccination messages. Non of their experiments were successful in increasing participation. Buttenheim et al. (2016) study the effect of vaccination vouchers coupled with an video with informational content for a pertussis vaccination for adult household members of infants. They find no effects on vaccination coverage. Baskin (2018) tests the effectiveness of different types of email-content sent to working adults in promotion of an influenza vaccination. Only information on locations increases vaccination coverage suggesting that low information is a barrier for vaccination participation. Sadaf et al. (2013); Dubé et al. (2015) are review articles on interventions to reduce vaccine hesitancy.

[3]The most prominent example is the undocumented link between the MMR vaccination and autism, proposed and later retracted by Wakefield et al. (1998). Numerous subsequent studies (Taylor et al., 1999; Farrington et al., 2001; Godlee et al., 2011; DeStefano and Shimabukuro, 2019; Hviid et al., 2019) refute the causal association between MMR and autism.

is covered, the immunized individuals not only protect themselves but also lend protection to individuals without vaccinations. Consequently, the private benefit of vaccinations might be low in a society with relatively high coverage rates. My results show that reluctance is a leading cause for non-adherence in this setting and that other policies (beyond reminder letters) are necessary to substantially increase vaccination coverage (preferably after identifying the reasons for reluctance).

The paper proceed as follows: In section 2, I provide background on The Danish Childhood Vaccination Program and describe the introduction and purpose of the policy along with aggregated statistics on coverage rates in Denmark. Section 3 covers my empirical strategy. Section 4 describes data and descriptive statistics. Section 5 presents results and section 6 performs a simple cost-effectiveness analysis. Finally, section 7 concludes.

2 Background

2.1 The Danish Childhood Vaccination Program

Introduced in 1951 as a reaction to frequent epidemics and the discoveries of several vaccinations, the program initially offered vaccinations against diphtheria, tetanus (lockjaw), smallpox and tuberculosis. In the following decades, the program underwent changes based on relevance and medical innovations.[4] Today, the program immunizes against ten diseases during eight rounds of vaccinations (The Danish National Board of Health, 2016, 2018). Table 1 summarizes the current content of The Danish Childhood Vaccination Program. Three rounds are within the first year of life (at age 3, 5 and 12 months) and immunize against diphtheria, tetanus, pertussis, polio, haemophilus influenzae and pneumococcus. The fourth round, 15 months after birth, immunizes against measles, mumps and rubella (MMR). The later rounds at age 4, 5, 12 years are revaccinations and the HPV vaccine for females.[5] Below

[4]For instance, in 1955 a vaccination against polio was added to the program. The vaccination had just been discovered and Denmark had been hit by polio epidemics in 1952 and 1953 (Gensowski et al., 2019).

[5]In 2019 (after the study period of this paper) the HPV vaccination are offered to both genders (Valentiner-Brandt and Andersen, 2019)

age 2, adherent children have four vaccinations scheduled at 3, 5, 12 and 15 months after birth. For children below five years of age, most scheduled vaccinations coincide with the preventive care consultations as the consultations and the vaccinations occur at the same age.[6] Nevertheless, participation in each program is voluntary and independent from each other.

2.2 Introduction of Reminder Letters

In 2010, a survey was conducted to investigate non-adherence in the sixth vaccination round at age 5 (revaccination of diphtheria, tetanus, pertussis and polio) (Wójcik et al., 2012). The survey asked parents of children who did not receive that particular vaccination why they missed it. The main reported cause was forgetfulness and *not* vaccination reluctance.

As a results, a law was proposed,[7] which granted SSI access to the Danish Vaccination Register to identify parents of non-adherent children in order to send psychical reminder letters to these parents.[8] Two caveats are worth mentioning concerning the survey study and its relevance for the effectiveness of reminder letters. First, the survey included 574 randomly chosen parents of non-adherent children born between 2000-2003 with a response rate of 67 %. Thus, the sample size was relatively small with a potential nonresponse bias and performed on cohorts of children born 11-14 years prior to the introduction of reminder letters. Second, extrapolating the causes for non-adherence in the vaccination round at age 5 to the causes for adherence in the earlier vaccinations rounds might be biased as well.

Implementation of reminder letters began May 15, 2014 with the following policy design: at ages 2, 6.5 and 12, parents of children lacking at least one scheduled vaccination receive a reminder letter. Both parents receive the letter if the parents do not share address or custody

[6] The Danish preventive care consultations offer eight health checks for all children: around 5 weeks, 5 months and at 1, 2, 3, 4, 5 and 6 years. The only exceptions are the first round at 3 months and the first MMR vaccination scheduled at 15 months after birth.

[7] Law proposal no. L 101 proposed 12 December, 2013 by Minister for Health and Prevention, Astrid Krag Member of Parliament for the Socialist People's Party.

[8] Prior to this, the Danish Vaccination Register was only used to monitor vaccination coverage and for research purposes (e.g. side-effects studies). Until February 6, 2017 the delivery of reminders were by actual letters. After February 6, 2017 that was changed to digital letters in parents E-Boks (Krause, 2017).

Tab. 1 Schedule of the Danish Childhood Vaccination Program

		- - - *Round and Age* - - -							
		1st	2nd	3rd	4th	5th	6th	7th	8th
		3 mth.	5 mth.	12 mth.	15 mth.	4yr.	5yr.	12yr.	12yr., 5 mth.
	Infant vaccinations (Below age 2)								
(1)	Dipheteria-tetanus-pertussis-polio-Hib	✓	✓	✓					
(2)	Pneumococcus	✓	✓	✓					
(3)	MMR				✓				
	Later vaccinations (Above age 2)								
(4)	MMR					✓			
(5)	Diphteria, tetanus, pertussis and polio						✓		
(6)	HPV for women							✓	✓

Notes: The table illustrates the schedule of the Danish Childhood Vaccination Program. *Source*: The Danish Board of Health [Sundhedsstyrelsen] (2016).

of the child otherwise the letter is only addressed to the mother (Krause, 2017). Appendix A.1 shows a full translation of a reminder letter. Reminder letters do not include any pro-vaccination campaigning. The letter notes that according to the records, the child lacks at least one vaccination followed by a table of the vaccinations schedule where red markings indicate the lacking vaccinations.

3 Empirical Framework

3.1 Identification Strategy: Impact of Reminders on Adherence

To identify the causal effects of reminder letters, I rely on the reform date in a Regression Discontinuity Design (RDD). Non-adherent parents to children with date of births prior to May 15 receive no reminder letters while parents to children with date of births at and after May 15 do. The timing of the introduction of the policy creates a sharp discontinuity in treatment assignment,[9]

$$T_i = 1\{s_i \geq 0\} \tag{1}$$

T_i is an indicator for treatment (receiving a reminder) and s_i is a variable for child i's second-year birthday relative to the reform date (May 15, 2014). Let α denote the causal effect of reminder letters on outcome Y_i (e.g. an indicator for adherence at age 3). I identify α in a RDD and estimate the following discontinuity,

[9]A treated family can be in some risk of not receiving a reminder letter. For instance, mail delivery problems can cause failure to receive an intended letter. Another cause can be registration errors regarding previous vaccinations. I have no information on the magnitudes of either types of mistakes. However, they are likely rare and not systematic and will thus not bias the results but change the interpretation of the effects to intention-to-treat (ITT).

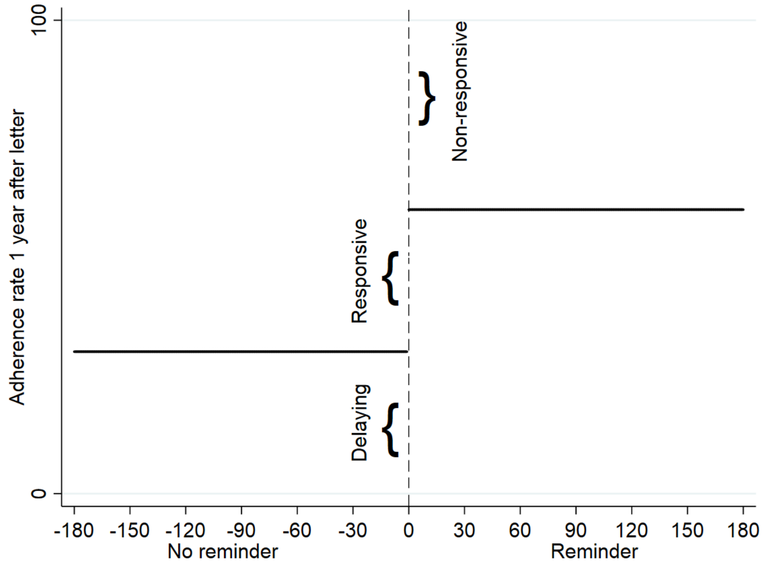
$$\alpha = \lim_{\epsilon \downarrow 0} E[Y_i \mid s_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[Y_i \mid s_i = \epsilon] \tag{2}$$

The RDD approach identifies the average treatment effect locally at the cut-off. The identifying assumptions are that individuals should have imprecise control over their treatment status (no-manipulation) and that other pre-treatment factors develop smoothly around the cut-off (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In Section 5.1, I examine the validity of these assumptions. First, I evaluate covariate balancing around the cut-off. Second, I check for manipulation graphically by plotting the density of children around the cut-off and formally using the McCrary density test (McCrary, 2008). Additionally, I perform various placebo tests in Section 5.5.

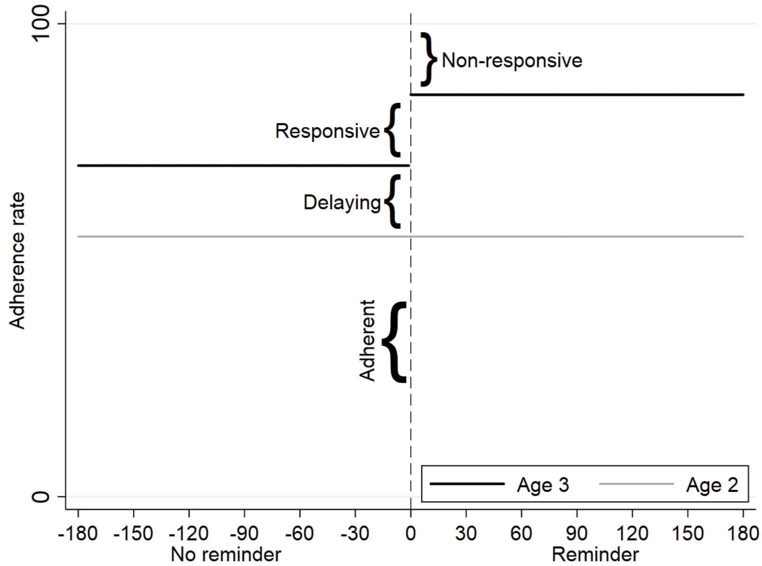
A further assumption is that the treatment of children above the cut-off does not affect the outcome of the control children below the cut-off. We might imagine that parents receiving a reminder letter could communicate with their child's peers' parents in social settings such as daycare facilities. This would cause some parents below the cut-off to be reminded indirectly through parents above the cut-off who receive reminder letters. In the presence of spillovers from the treatment to the control group, the effects will be underestimated. While being unable to assess this assumption formally, in Section 5.3, I test for cousin spillovers. This test provides an indication of the degree to which reminded parents discuss the reminder letters with other parents in their network.

In the main specification, I use local linear regression with a 180-day bandwidth, triangular kernel and bootstrapped standard errors with 300 replications. As robustness checks (Section 5.5), I estimate the discontinuity in a parametric specification with linear and quadratic trends and test for bandwidth sensitivity. Additionally, I include a control group of children from the year before treatment and estimate the effect of reminder letters using a Regression Discontinuity Difference-in-difference (RD-DD) approach.[10]

[10]Research suggests that date of birth is not independent of parental characteristics potentially compro-



(a) Non-adherent children at age 2



(b) Full population: Adherent and non-adherent children at age 2

Fig. 1 Illustration: Decomposing parents of non-adherent children using the introduction of reminder letters in a RD Design

Notes: On the x-axis is the second birthday relative to the reform date and on the y-axis the adherence rate. Children above the cut-off ($x = 0$) receive reminders at age 2 while children below the cut-off do not. Panel (a) depicts a hypothetical effect on vaccination adherence of reminder letters for a group of non-adherent children at age 2 and how the effect at the cut-off can be used to decompose the group of non-adherent children into three categories based on how they respond to reminders. Panel (b) depicts a hypothetical effect on vaccination adherence of reminder letters for all children (independent of adherence) and how the effect at the cut-off and the change in adherence from age 2 to 3, can be used to decompose children similarly.

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mising the results (Buckles and Hungerman, 2013; Currie and Schwandt, 2013)

My empirical strategy not only allows me to estimate the causal effects of reminder letters but also identifies three types of non-adherent parents locally around the cut-off based on how they react to reminders: i) delaying, ii) responsive and iii) non-responsive parents. Panel (a) in Figure 1 illustrates how the change in the adherence rate at age 3 among non-adherent children at age 2 can be used to group children based on their parents' response to the reminder letter. I define delaying parents as being non-adherent at age 2 but adherent at age 3 in the absence of reminders. Responsive parents are non-adherent at age 2 but adherent at age 3 caused by being reminded by the letter. Non-responsive parents are non-adherent at age 2 and at age 3 even though they are reminded by the letter. Graphically in Figure 1, the adherence rate at age 3 just below the cut-off is the estimated share of delaying parents. The discontinuity in the adherence rate at age 3 at the cut-off is the share of parents that are responsive to reminders while the share of non-adherent parents at age 3 above the cut-off is the estimated share of non-responsive parents.

To study heterogeneity, I use a sample of both adherent and non-adherent parents at age 2 as it produces the level of adherence (beyond the share of responsive, non-responsive and delaying parents). The level of adherence is important for a comprehensive understanding of differential vaccination behavior as there is considerable variation in adherence across subgroups. Panel (b) in Figure 1 shows how – using a sample of both adherent and non-adherent parents at age 2 – I can identify i) adherent, ii) delaying, iii) responsive and iv) non-responsive parents by applying the change in adherence rate from age 2 to age 3 around the cut-off. The share of adherent parents is given by the adherence rate at age 2 at the cut-off. The change in the adherence rate from age 2 to 3 below the cut-off is the estimated share of delaying parents. The discontinuity in the adherence rate at age 3 at the cut-off estimates the share of responsive parents, while the non-adherence rate at age 3 above the cut-off estimates the share of non-responsive parents. Using both adherent and non-adherent children in the estimation sample, scales the decomposition of parents by the size of the cohort (i.e. the share of responsive parents is relative to all similarly-aged children as opposed to being relative to non-adherent children).

3.2 Interpretation: Parental Responses to Reminder Letters and Causes for Non-adherence

This section presents a simple framework for decomposing parental responses to reminder letters in the RDD sample into causes for non-adherence at age 2 in three general categories; i) delays, ii) inattention and iii) reluctance. Delaying and inattentive individuals are willing to vaccinate but are non-adherent due to either delays or inattention. Reluctant individuals are non-adherent due to an active decision and are neither non-adherent due to delays or inattention. Thus, parents who are responsive to reminder letters were previously non-adherent due to inattention, while parents who are non-responsive to reminder letters were previously (and continues to be) non-adherent due to reluctance.

In the following I lay out the two conditions under which this generalization holds: 1) every individual intended to receive a reminder successfully does so, reads the reminder and understands the content and 2) only inattentive parents respond to reminders. There are a few threats to these conditions. First, there might be registration errors in the form of underreporting in the administrative vaccination data. The degree to which registration errors occur is unknown. However, GP's are reimbursed based on their registrations hence they are financially incentivized to register correctly. Underreporting violates condition 1). Specifically, some parents registered as non-adherent parents might actually be adherent. In the presence of underreporting some non-responsive parents might be so because they are adherent already and not due to reluctance. Wójcik et al. (2013) and Voss et al. (2019) study underreporting in the Danish vaccination registration practices. They find 0.4-1.1 %-points underreporting for the vaccinations below age 2 by comparing parents' answers to a questionnaire to the administrative registers. This evidence indicates that underreporting is a relatively rare occurrence. Mail failures, parents who disregard the reminder letter without reading it, and parents who fail to understand the content also violate condition 1). Condition

2) rules out that reminder letters cause reluctant parents to re-evaluate their decision. If reminder letters cause re-evaluations, then responsive parents can be non-adherent at age 2 due to reluctance. However, the content of the letter strictly informs on the schedule and reminds that a child is non-adherent and does not include any pro-vaccination campaigning that can cause parents to re-evaluate. In Section 5.2, I informally assess condition 2) by estimating the timing of the response to reminders. A quick response indicates that those who react to the reminders are inattentive. The heterogeneous results presented in Section 5.4 also provide insights into the validity of condition 2). Specifically, I test if plausibly attentive parents (parents with educations in health or childcare) respond to reminders: if attentive parents respond to reminder letters, then reminder letters may have caused them to reevaluate their initial position toward vaccination.

In the following, I assume that the two conditions hold and show how to estimate the share of inattentive and reluctant parents using the introduction of reminders in a RDD. Furthermore, I show that these estimates correspond to the share of responsive and non-responsive parents. Define the share of inattentive parents as Ω and the share of delaying parents as θ. Consequently, the share of reluctant parents is $1-\Omega-\theta$. All individuals within these groups are defined as non-adherent at age 2. In the absence of reminders, the adherence rate at age 3 is given by:

$$D_{3y}^{\text{No letter}} = \theta \tag{3}$$

Where $D_{3y}^{\text{No letter}}$ is the share of adherent children at age 3 in the absence of reminder letters. I estimate θ (the share of delayers) as the level of adherence at age 3 in the absence of reminders. This can be estimated as the adherence rate at age 3, Y_i, just below the cut-off:

$$\text{Share of delayers} = \theta = \lim_{\epsilon \uparrow 0} E[Y_i \mid s_i = \epsilon] \tag{4}$$

To estimate the share of inattentive parents, Ω, first consider the adherence rate at age 3 after introducing reminders:

$$D_{3y}^{\text{Letter}} = \theta + \Omega \tag{5}$$

With reminder letters, the adherence rate at age 3 within the group of non-adherent individuals at age 2 is given by the shares of delaying and inattentive individuals. To find an expression for Ω , I subtract the adherence rate at age 3 with reminders from the adherence rate at age 3 in the absence of reminders:

$$D_{3y}^{\text{Letter}} - D_{3y}^{\text{No letter}} = \Omega + \theta - \theta = \Omega \tag{6}$$

Using RDD, I estimate the share of inattentive individuals as:

$$\text{Share of inattentive} = \Omega = \lim_{\epsilon \downarrow 0} E[Y_i \mid s_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[Y_i \mid s_i = \epsilon] \tag{7}$$

That is, the share of inattentive individuals can be found as the change in aggregated adherence at age 3 caused by the introduction of reminders which corresponds to the share of responsive parents. The share of reluctant parents is then calculated as the residual $1 - \Omega - \theta$ or as:

$$\begin{aligned}\text{Share of reluctant} &= 1 - \Omega - \theta \qquad (8)\\ &= 1 - \lim_{\epsilon \downarrow 0} E[Y_i \mid s_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[Y_i \mid s_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[Y_i \mid s_i = \epsilon]\\ &= 1 - \lim_{\epsilon \downarrow 0} E[Y_i \mid s_i = \epsilon]\end{aligned}$$

which is the adherence rate at age 3 just to the right of the cut-off, exactly corresponding to the share of non-responsive parents.[11]

4 Data and Descriptive Statistics

4.1 Data and Sample

I use administrative data from Statistics Denmark. My sample includes the universe of children turning 2 years in a 180-day window at either side of the cut-off (15 May, 2014); from 17 November, 2013 to 11 November, 2014. I observe parental characteristics: employment history, educational attainment, student status, income, age, marital status and parental cohabitation along birth characteristics including birth weight, preterm birth, birth municipality, birth location (hospital vs. home birth), c-section delivery and child gender. Also from Statistics Denmark, I obtain and merge children's vaccination history including the dates for each vaccination round. Using family linkages in the data, I identify younger siblings and cousins of children in the main sample and create samples of siblings and cousins of focal children.

The number of children born in Denmark between 17 November, 2011 and 11 November, 2012 is 58,394. I drop 2,283 children not residing in Denmark by their third birthday.[12] Out

[11]Considering the entire cohort (not restricting to non-adherent parents at age 2), similar calculations are possible as shown in Appendix A.2.

[12]These children are not part of the registers three years after birth. They may be dead, residing outside of Denmark or any other circumstances that cause them to leave the registers.

of these remaining 56,111 children, 13,926 (24.8 %) miss at least one vaccination at age 2. My main outcome is an indicator for full adherence in the program at age 3. I also use the number of received vaccinations during the third year of life and participation in the 5th vaccination round (2nd MMR) and in the preventive care consultation at age 3 as additional outcomes. The latter two outcomes test for spill-overs across vaccinations and to other preventive care programs.

The 13,926 non-adherent children in the sample have 1,532 younger siblings who are born between November 17, 2013 and December 31, 2014. These 1,532 children constitute the sibling sample. I use measures of vaccination adherence below age 2 as outcomes for the siblings. Reminder letters are sent at age 2 at the earliest, implying the outcomes for the sibling sample will not be affected by reminder letters addressed to them directly. I construct a sample of younger cousins ($n = 574$) in similar fashion and with similar outcomes.

4.2 Descriptive Statistics

Table 2 presents summary statistics for the sample of non-adherent children at age 2 in a 180-day window on either side of the cut-off. In the top panel, I present statistics for birth characteristics. The two groups are balanced across birth characteristics with 37 % of children being first-borns, 5 % having a mother below 21 years at birth, 5 % being born with a low birth weight. However, children born above the cut-off are more likely to be born at home rather than at a hospital (1 % vs. 2 %). The middle panel in Table 2 focuses on parental characteristics for both parents. The two groups differ slightly on cohabitation and marital status as 82 % of children below the cut-off live with cohabiting parents compared to 80 % above the cut-off. Moreover, mothers to children above the cut-off are 2 %-points more often in employment than those below the cut-off. The bottom panel of Table 2 presents variables on vaccination behavior before and after treatment. Vaccination behavior at age 2 (prior to treatment) is similar across the cut-off. There are however differences in participation above age 2 (after treatment). 30 % of children above the cut-off adhere fully at age 3 compared to 19 % below the cut-off suggesting a sizeable effect from reminder letters on adherence.

Tab. 2 Summary statistics: Non-adherent children at age 2 in a 180-day window around the cut-off, 15 May, 2014

	Below cut-off	Above cut-off	Test for significance
	Mean	Mean	P-value
First-time mothers	0.37	0.37	0.875
Young mother	0.05	0.05	0.708
Young father	0.02	0.02	0.871
Low birth weight	0.05	0.05	0.985
Preterm birth	0.06	0.07	0.379
Child sex (female)	0.48	0.47	0.155
Home birth	0.01	0.02	0.005***
C-section	0.22	0.22	0.950
Income, mother	239.53	241.59	0.401
Prim. school, mother	0.20	0.20	0.840
Higher educ, mother	0.24	0.24	0.563
Uni. degree, mother	0.14	0.15	0.010**
Danish, mother	0.79	0.80	0.451
Student, mother	0.05	0.05	0.158
Employed, mother	0.68	0.70	0.003***
Cohabiting	0.82	0.80	0.006***
Married	0.42	0.41	0.017**
Income, father	335.14	333.90	0.758
Prim. school, father	0.20	0.20	0.616
Higher educ, father	0.17	0.16	0.285
Uni. degree, father	0.13	0.14	0.155
Danish, father	0.79	0.79	0.780
Student, father	0.02	0.03	0.274
Employed, father	0.79	0.79	0.574
1st vaccination at age 2	0.81	0.83	0.013**
2nd vaccination at age 2	0.78	0.78	0.580
3rd vaccination at age 2	0.70	0.70	0.531
All vacs. at age 2	0.00	0.00	-
All vacs. at age 3	0.19	0.30	0.000***
Obs.	6737	7189	

Notes: Children who had second-year birthday between 17 November, 2013 and 11 November, 2014 and still reside in Denmark and have not received all age-specific vaccinations by age 2. All variables are measured at birth unless noted otherwise. The *Below cut-off* sample consists of children with second-year birthday before the reform date, 15 May, 2014 while the *Above cut-off* sample consists of children with second-year birthday after the reform date.

The observed differences between children and parents below and above the cut-off likely reflect time trends and seasonality. Thus, a simple comparison of these two groups' vaccination behavior after treatment will likely be biased due to the pre-treatment imbalances between them. To account for time trends and seasonality, I flexibly estimate the relationship between distance to the cut-off and outcome using local linear regression while evaluating the effect of reminders as the discontinuous jump in adherence locally at the cut-off.[13]

Appendix Figure A1 shows which vaccination round non-adherent children lack. Most often, they lack the 4th vaccination round which is the first MMR vaccine (9,190 children lack the 4th round). The first vaccination round has the lowest level of non-adherence and non-adherence steadily increases for later rounds.[14]

5 Results

5.1 Validation of Empirical Strategy

Appendix Figure A2 shows the density of children in the sample in a 180-day window around the cut-off. The figure shows the total number of non-adherent children in the Childhood Vaccination Program at age 2 within 40 equal-sized bins. There is no visible bunching of children just below or above the cut-off. To formally test parents ability to systematically manipulate the selection variable, I perform a McCrary density test. Appendix Figure A3 shows the estimated density of children around the cut-off. I am unable to reject the null hypothesis of no discontinuous jump at the cut-off with an estimated jump in the log density of 0.055 with a standard error of 0.061. In line with the data, it appears unlikely that parents

[13]Appendix Table A1 presents summary statistics to assess how parents of adherent and non-adherent parents compare. These groups differ across most characteristics as parents of adherent children have higher levels of education, higher incomes and they are more often working, married and cohabiting. Moreover, the groups differ across birth characteristics.

[14]Appendix Table A2 shows how far non-adherent children are from following the recommendations of the vaccination program. Relative to the full population of children born in the sample period, 0.9 % receive zero, 1.0 % receive one, 4.3 % receive two, 18.7 receive three and 75.2 % receive all four vaccinations. The regional distribution of non-adherent children is fairly uniform with around 25 % of children having missed at least one vaccination in all regions (see Appendix Table A3). Denmark has 98 municipalities and five regions. The main responsibility of regions are administration of the public health sector. Similar shares of non-adherent children, suggest that access to the vaccination program is available across the country.

manipulate the timing of a birth two years prior to an unexpected policy intervention.

Another worry is that parents adjust their vaccination behavior to control eligibility to receive reminder letters. Such behavior would create a discontinuous change in the share of adherent children at the cut-off. Appendix Figure A4 shows the fraction of eligible individuals around the cut-off. The share of non-adherent (and thus eligible to receive reminders) children is stable at 20-25 % throughout the sample period. The absence of abnormal spikes just below or above the cut-off suggest that parents do not manipulate their eligibility into treatment.

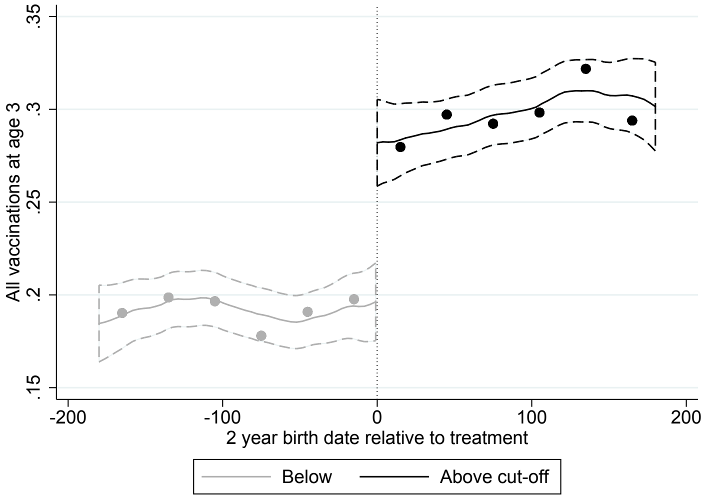
To test for discontinuities in any pre-treatment covariates, Appendix Figures A5 to A7 plot the fitted values from a local linear regression with a bin-width of 20 days and triangular kernel[15] along with regression coefficients and p-values with a range of pre-treatment birth measures, parental characteristics and vaccination behavior as dependent variables. None of the pre-treatment characteristics show signs of any significant discontinuities at the cut-off and develop smoothly across the cut-off.

Taken together, the evidence presented supports the validity of RDD as empirical strategy.

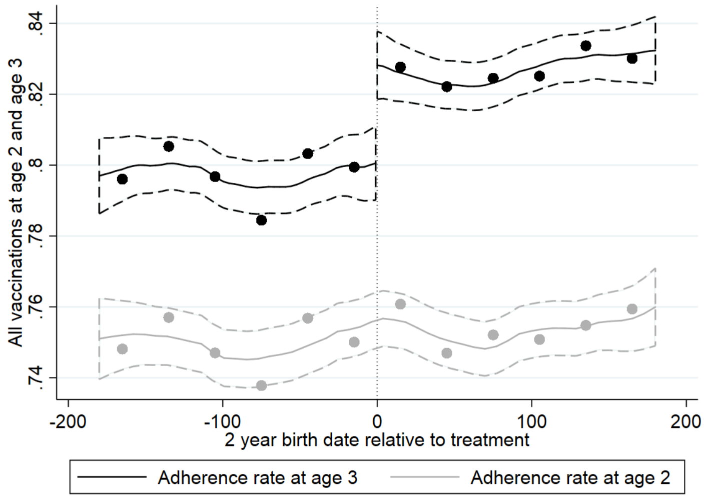
5.2 Main Results

Figure 2 shows the relationship between distance to the cut-off and vaccination adherence in the sample of non-adherent children at age 2 and in the full population. Reminder letters have a positive impact on vaccination adherence: at the cut-off in both samples we see a clear discontinuity in the adherence rate at age 3. Around 19 % of untreated non-adherent children (below the cut-off) catch up with the schedule and reach adherence at age 3 compared to 28 % of children that receive reminder letters (above the cut-off). However, more than 2/3 of the children that receive reminders are still non-adherent a year after treatment. In the full population, adherence at age 3 jumps from 80 % to 82.5 % at the cut-off with 17.5 % of all children still being non-adherent at age 3 despite the introduction of reminder letters.

[15]This is how all similar graphs in the paper are specified unless otherwise noted.



(a) All vaccinations at age 3 - Sample of non-adherent children at age 2



(b) All vaccinations at age 3 - Full pop. sample

Fig. 2 The effect of reminder letters on vaccination adherence

Notes: The outcome is an indicator equal to one if the child has all scheduled vaccinations at age 3. In panel (a), the sample includes 13,926 children with second-year birthdays between 17 November, 2013 and 11 November, 2014 who were non-adherent at age 2. In panel (b), the sample includes 56,111 children with second-year birthdays between 17 November, 2013 and 11 November, 2014 – both non-adherent and adherent children. The cut-off (dashed vertical line) indicates the reform date at 15 May, 2014. Solid lines indicate fitted values from a local linear regression with a width of bin of 20-day and triangular kernel. Dashed lines are 95 % confidence bans. Dots mark 30-day binned means.

Tab. 3 Decomposition of parents based on their response to reminders

	(1)	(2)
	Outcome: All vacs. at age 3	
	Non-adherent at age 2	Full population
Reminder letters	0.087***	0.026***
	(0.016)	(0.007)
Shares of parents		
Responsive	0.087	0.026
Delaying	0.192	0.048
Non-responsive	0.721	0.176
Adherent	-	0.751
Obs.	13926	56111

Notes: Each cell in the top row show coefficients from separate regressions. The outcome is an indicator for full adherence at age 3. Coefficients are estimates of the discontinuity at the cut-off. The cut-off is the implementation date of reminder letters on 15 May, 2014. The sample of non-adherent children consists of children who had second-year birthday between 17 November, 2013 and 11 November, 2014, still reside in Denmark and have not received all age-specific vaccinations by age 2 (non-adherent). The full population sample do not restrict on non-adherence at age 2. I use local linear regression, a triangular kernel and a 180-day bandwidth on each side of the cut-off. See section 3 for details regarding the decomposition. Bootstrapped standard errors with 300 replications in parenthesis. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Having shown the positive effect of reminder letters, I proceed to the decomposition results presented in Table 3. Reminder letters increase adherence with 8.7 %-points in the group of non-adherent children at age 2. Thus, the share of responsive parents of non-adherent children at age 2 is 8.7 %. 19.2 % of non-adherent parents are delaying as they reach adherence in the absence of reminders, while 72.1 % of non-adherent parents are non-responsive to reminders. As 8.7 % of non-adherent parents are responsive to reminders, inattention *is* a cause for non-adherence. However, the share of non-responsive parents clearly outweighs the share of responsive parents indicating that the largest determinant for non-adherence is reluctance and not inattention. Relative to an entire cohort of children (column (2)), reminder letters increase adherence at age 3 with 2.6 %-points implying that 2.6 % of parents are responsive to reminders. 4.8 % of parents are delaying, 17.6 % are non-responsive and 75.1 % are adherent at age 2. The results indicate that knowledge about the vaccination program is widely spread among parents with infants as only 2.6 % of parents are responsive to reminder letters. This limits the scope of reminder letters as an effective tool to raise adherence. My finding that non-adherence is mostly caused by reluctance and not inattention is in line with survey evidence from Gallup (2019). They report that 96 % of Northern Europeans are aware of vaccinations compared to 89 % globally but that only 44 % of Northern Europeans strongly agree that vaccines are safe compared to 61 % globally.

In Appendix Figure A8, I consider other outcomes; a) number of vaccinations in the third year of life, b) participation in the preventive care consultation scheduled at age 2 by age 3 and c) participation in the 2nd MMR vaccination scheduled at age 4 by age 5.[16] I use the number of vaccinations to verify the previous results using a related outcome. To test for spillovers across programs, I use participation in the preventive care program as outcome and the 2nd MMR vaccination. The effect on the number of vaccines during age 3 is 0.115 - an increase of 43 %. Attendance in the preventive care consultation scheduled at age 2 is 5.5 %-points (9.8 %) higher, as a consequence of reminder letters, at age 3. Reminding parents of the vaccination schedule spills over to the preventive care program. However, reminder letters in the early vaccination rounds do not spill over to an increase in adherence in later vaccination rounds contrary to findings from US (Carpenter and Lawler, 2019).

To complement the overall effect, Appendix Figure A9 considers more fine-grained time intervals to study the timing of the response. The figure plots coefficients from separate regressions, estimating the discontinuity at the cut-off with quarterly number of received vaccinations from birth to age 3 as outcomes. The only quarter where reminder letters affect the number of received vaccinations is the quarter immediately following treatment. The quick response suggests that inattention is the primary reason why parents react to a reminder letter: Parents receive a letter that reminds them that their child lacks vaccinations and parents who are unaware of the fact and are willing to adhere, react immediately to the reminder. If reminder letters cause re-evaluations from reluctant parents, a more gradual response would be expected. Appendix Figure A10 shows Google search trends for the terms "vaccination" and "side-effects vaccination". At the introduction of reminder letters, the

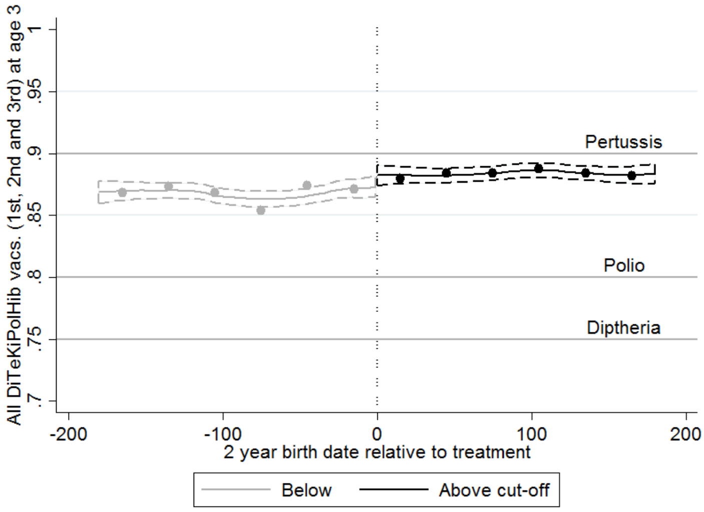
[16] Appendix Table A4 shows estimates and significance of the discontinuities from Appendix Figure A8.

search activity for these terms are unchanged. This suggests and supports that reminder letters do not cause re-evaluations.

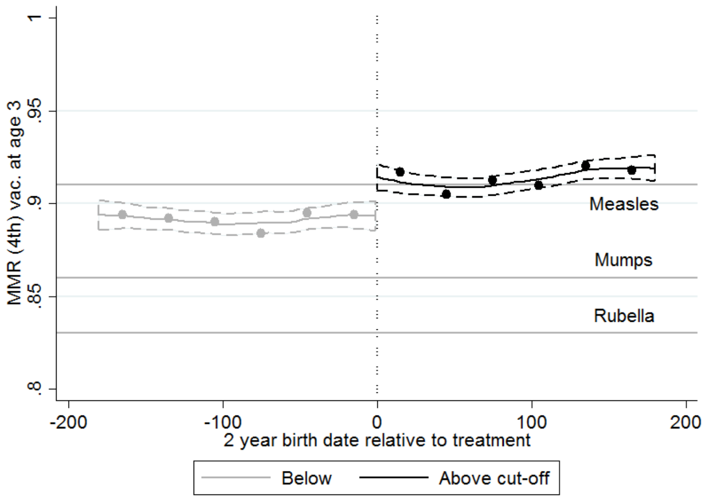
Appendix Table A5 shows the impact of reminder letters for subgroups of children split by their pre-treatment vaccination behavior. I split by whether the child lacks one or more vaccinations and by lacking each of the four vaccination rounds. Parents of children, who lack one vaccination, react stronger to reminder letters than parents of children, who lack more than one. Children, who lack one vaccination in order to reach adherence, are more likely to lack that vaccination due to inattention, while for children, who lack several vaccinations, a deliberate parental decision not to vaccinate is more likely. Moreover, children, who lack later vaccination rounds, respond stronger to reminders. Intuitively, these results make sense: Parents of non-adherent children, who participate in the early rounds but miss some of the later rounds, might be inattentive to the schedule of the vaccination program rather than deliberately opposing.

Did reminder letters increase vaccination coverage rates to herd immunity levels? Herd immunity is the indirect protection for unvaccinated individuals caused by vaccinated individuals. Herd immunity blocks epidemics and causes declining prevalence of a disease in the unvaccinated part of the population and eventually eradicates a disease (John and Samuel, 2000; Fine et al., 2011). [17] Figure 3 shows the effect of reminders on coverage rates relative to minimum herd immunity thresholds for various diseases covered by the vaccination program. In panel (a), the outcome is an indicator for completion of the entire diphtheria-tetanus-pertussis-polio-Hib sequence (1st, 2nd and 3rd vaccination round) at age 3 along with herd immunity thresholds for diphtheria, pertussis and polio. Prior to reminders, coverage rates are above the minimum herd immunity threshold for diphtheria and polio. Reminders increase adherence in the diphtheria-tetanus-pertussis-polio-Hib vaccinations with 1 %-point from 87 % to 88 %. However, the herd immunity threshold for pertussis is 90 %.

[17] The herd immunity threshold for disease i, H_i, is calculated as $H_i = 1 - 1/R_{0,i}$, where $R_{0,i}$ is the basic reproduction rate of disease i. Hence, herd immunity thresholds differ depending on the infectiousness of a disease. The more infectious, the larger is the herd immunity threshold. Appendix Table A6 presents herd immunity threshold for diphtheria, pertussis, polio, measles, mumps and rubella.



(a) All diphtheria-tetanus-pertussis-polio-hib vacs. at age 3



(b) MMR vac. at age 3

Fig. 3 The effect of reminder letters on herd immunity

Notes: The outcome in panel (a) is an indicator equal to one if the child received the entire diphtheria-tetanus-pertussis-polio-hib vaccination sequence (1st, 2nd and 3rd vaccination round) at age 3. In panel (b) the outcome is an indicator equal to one if the child received the MMR vaccination scheduled at age 15 month at age 3. Grey horizontal lines show the minimum herd immunity thresholds (Plans-Rubió, 2012) for the diseases given by the labels in the figure. The sample includes 56,111 children with second-year birthdays between 17 November, 2013 and 11 November, 2014. The cut-off (dashed vertical line) indicate the reform date at 15 May, 2014. Solid lines indicate fitted values from a local linear regression with a width of bin of 20-day and triangular kernel. Dashed lines are 95 % confidence bans. Dots mark 30-day binned means.

Thus, reminder letters fail to push coverage rates above the herd immunity threshold for pertussis. Highlighting this, Denmark experienced pertussis epidemics in 2016 and 2019

(Dalby et al., 2019). Panel (b) presents the effect of reminders on adherence in the MMR vaccination at age 3 along with minimum herd immunity thresholds for measles, mumps and rubella. Reminder letters increase MMR adherence from 88 % to slightly above 91 %. Prior to reminder letters, the MMR coverage rate is above the herd immunity thresholds for mumps (86 %) and rubella (83 %). Measles have a minimum herd immunity threshold at 91 %. Thus, reminder letters increase MMR adherence to above the minimum herd immunity threshold for measles.

5.3 Sibling and Cousin Spillover Effects

The spillover analysis adds to an understudied but growing literature on health behavior spillovers within the family (De Neve and Kawachi, 2017). Al-Janabi et al. (2016) highlight the need to include family spillovers in the economic evaluations of medical interventions and recent work by Fadlon and Nielsen (2019) shows the depth of spillovers to spouses, children and co-workers of a negative health shock.

As outcomes, I use the number of received vaccinations at time intervals prior to age 2.[18] Table 4 shows the sibling and cousin spillover effects of reminder letters.[19] The upper panel in the table presents the effects on siblings. For the siblings, I find no impact of reminders on vaccinations within the first year of life. However during the second year, siblings of children who are sent a reminder receive 0.19 fewer vaccinations compared to siblings of untreated children. There appears to be a negative sibling spillover from reminder letters perhaps explained by an anticipation effect; Parents anticipate the reminder letter and postpone vaccinations longer knowing that at age 2 they will be reminded. Thus, reminders come with a cost as parents reduce the number of vaccinations for future children prior to

[18] Appendix Figure A11 shows the density of siblings around the cut-off and indicates no bunching at either side of the cut-off. Appendix Figure A12 shows the distribution of the siblings in terms of their date of births. A test for whether reminder letters affect future fertility turns up insignificant and results are available on request. Also available on request are balancing tests along a range of pre-treatment characteristics.

[19] Appendix Figure A13 complements the regressions with graphical evidence of the effects. Appendix Table A7 shows results using timely adherence in each vaccination round as outcome. All coefficients are insignificantly different from zero.

receiving a reminder. Focusing on cousins of treated children (presented in the lower panel of Table 4), I find no effects. Younger cousins of children in the main sample have similar vaccination behavior. This suggests that parents do not discuss the content of the reminder letter beyond the immediate family. The evidence suggests negative spill-overs of reminders within the immediate family as younger siblings receive significantly less vaccination during the second year of life.

Tab. 4 Spill-overs part: Siblings and cousins of non-adherent children in the Danish Childhood Vaccination Program born between 17 November, 2013 and 11 November, 2014

	(1) No. vacs, 6 month	(2) No. vacs, year 1	(3) No. vacs, year 2	(4) No. vacs, age 2
	Siblings of non-adherent children around the cut-off			
Reminder letters	-0.033	-0.016	-0.186**	-0.202
	(0.079)	(0.080)	(0.083)	(0.133)
Obs.	1532	1532	1532	1532
	Cousins of non-adherent children around the cut-off			
Reminder letters	0.112	-0.002	0.031	0.029
	(0.086)	(0.060)	(0.097)	(0.097)
Obs.	574	574	574	574

Notes: Each cell show coefficients from separate regressions given by the outcomes listed at the top of each column. The outcomes in column (1), (2), (3) and (4) is the number of vaccinations at age 6 months, first year of life, second year of life and at age 2 respectively. Coefficients are the estimate of the discontinuity at the cut-off. The cut-off is the implementation date of reminder letters on 15 May, 2014. The sample in the upper panel consists of younger siblings of children who had second-year birthday between 17 November, 2013 and 11 November, 2014, still reside in Denmark and have not received all age-specific vaccinations by age 2 (non-adherent). The sample in the lower panel consists of younger cousins of children who had second-year birthday between 17 November, 2013 and 11 November, 2014, still reside in Denmark and are non-adherent at age 2. I restrict children in both panels to have date of births between 17 November, 2013 and 31 December, 2014. I use local linear regression, a triangular kernel and a 180-day bandwidth on each side of the cut-off. Bootstrapped standard errors with 300 replications in parenthesis. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

5.4 Heterogeneous Effects

In order to shed light on differential causes for non-adherence in the vaccination program across subgroups, I explore heterogeneity in the responses to reminder letters. I evaluate heterogeneous effects across the following subgroups: 1) parity (first-borns vs. higher parity), 2) parental education in health (medical doctors, nurses and midwives) and childcare (pedagogues), 3) initial health (low birth weight and/or preterm birth), 4) socio-economic status (SES) (10 % bottom income, primary school as highest education, below 21 at birth for either parents), 5) parental educational levels (university degree vs. no university degree). I perform the analysis by a split-sample approach and decompose each subgroup into adherent and non-adherent parents by the type of non-adherence at age 2. I expect parents educated in health or childcare and parents of children with low initial health to be more aware and attentive to the vaccination program and hence non-responsive to reminder letters.

I present the heterogeneous decomposition results as bar graphs presented in Figure 4.[20] First-time parents are the most adherent subgroup with 80.3 % who adhere at age 2 followed by parents with a university degree and parents educated in health or childcare with shares of adherent parents at 78.9 % and 78.5 % respectively. The least adherent groups are those with low SES and of higher parity with adherent-shares of 70.0 % and 71.3 %. The least responsive groups are first-time parents, parents educated in health and childcare, parents of children with poor initial health and parents with university degrees. For these subgroups, I estimate that 0 % are responsive, indicating that when first-time parents fail to adhere with the vaccination program, the cause for non-adherence is likely reluctance. It appears as if first-time parents are more aware of the vaccination schedule and thus less responsive to reminders. This result is in line with findings from studies suggesting parental investments decline with birth order (Black et al., 2005; Price, 2008; Hotz and Pantano, 2015; Lehmann et al., 2018). When parents educated in health or childcare (and possibly with working knowledge from these sectors) fail to adhere, the results suggest that the causes are either reluctance or delays and not inattention. Non-adherent parents of children with poor initial health do not respond either, suggesting that non-adherence within this group is almost exclusively caused by reluctance. This suggests that parents of children with poor initial

[20] Appendix Table A8 tests for significant differences in a parametric and fully interacted regression model. The tests show that first-time parents, parents educated in health and childcare, high SES parents and parents with a university degree are statistically less responsive to reminder letters relative to their counterpart subgroups. The rest of the heterogeneous responses are insignificantly different. Heterogeneity results across various other characteristics are available on request. For instance, I find similar responses across ethnicity, urban/rural and east/west of Denmark.

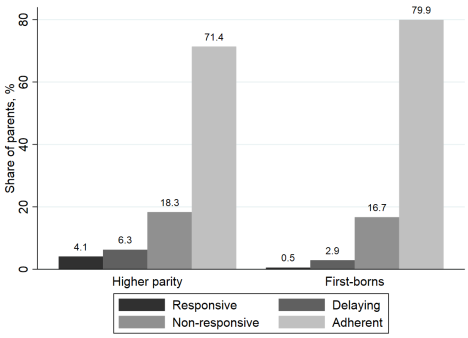
health need more convincing to participate in the vaccination program than a reminder. Parents with a university degree are non-responsive to reminder letters suggesting that, when university educated parents fail to adhere, the cause is not inattention but rather reluctance. Low SES parents and parents of children with poor initial health have the highest shares of non-responsive parents at 20.4 % and 18.9 %, suggesting that reluctance is a particularly important determinant for non-adherence at age 2 for those types of parents. The most responsive groups are higher parity parents and those without university degrees where 4.1 % and 3.8 % of parents are responsive, suggesting a relatively high degree of inattention within these groups.[21]

Sending reminders to higher parity parents, highly educated parents, parents with educations in health and childcare and parents with children with poor initial health have no effect on their children's adherence. To increase participation for children of reluctant parents, policies beyond reminder letters are needed. The track record of pro-vaccination campaigns distributed in videos or written material is mixed. This suggests that reluctant parents cannot be convinced through superficial and impersonal campaigns (Larson, 2013).[22] An explanation could be that reluctant parents already familiarize themselves with the basic pros and cons for vaccinations through their own research. Information conveyed in a brief pro-vaccination campaign or through reminders does not add to the information set of reluctant parents and thus has limited impact on their decision not to vaccinate. One avenue to explore is intensified dialogue and discussion with nurses and GP's within the preventive care system.[23]

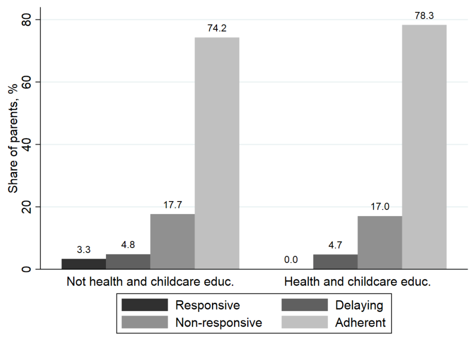
[21]Appendix Table A9 contains estimates, standard errors and the decomposition used to generate Figure 4. Appendix Figures A14 – A16 present graphical evidence of the impact of reminder letters by subgroup.

[22]Larson (2013) discuss the importance of listening and understanding when dealing with reluctant individuals and the ineffectiveness of standard pro-vaccination campaigns.

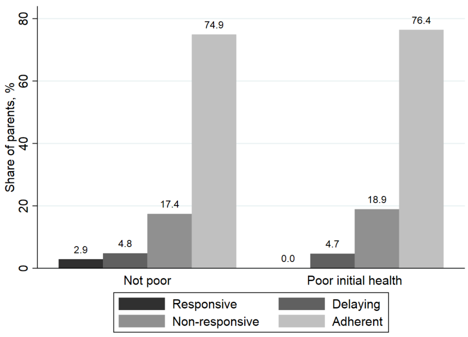
[23]Gallup (2019) shows that 97 % of Danes trust doctors and nurses suggesting that advice communicated directly through them might work. Moreover, Smith et al. (2006) find that health care providers have positive influence on reluctant parents' vaccination behavior.



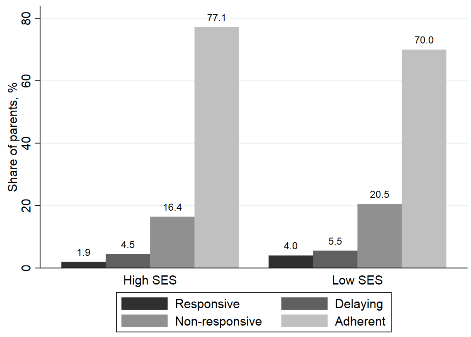
(a) Parity



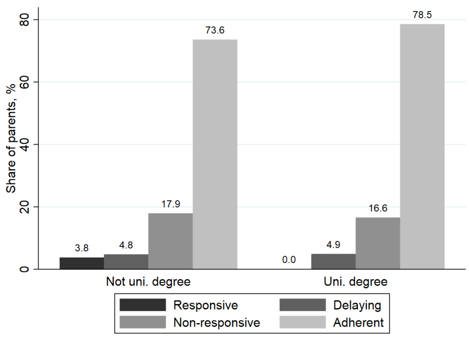
(b) Parental education in health or childcare



(c) Initial child health



(d) SES



(e) University degree

Fig. 4 Decomposition into adherent and non-adherent children by the type of non-adherence at age 2 by parity, parental education in health and childcare, initial child health, SES and parent education

Notes: The figures decompose the sample around the cut-off into four groups by pre-determined characteristics: Adherent at age 2 and non-adherent at age 2 into non-responsive, delaying and responsive parents. The decomposition is described in Section 3 and Appendix A.1. The heterogeneous decomposition is generated by a split-sample approach. The total sample includes 56,111 children with second-year birthdays between 17 November, 2013 and 11 November, 2014.

5.5 Robustness

My main results are robust to a range of robustness checks. Specifically, I reestimate the discontinuity at the cut-off with bandwidths ranging from 90 to 180 days with intervals of 15 days using both local linear regression, OLS estimation with linear or quadratic and separate trends and RD-DD estimation where I add a control group of children born on the same dates but a year earlier (where no treatment took place).[24] The RD-DD design takes out any potential discontinuity in the outcome that occurs naturally at 15 May in the absence of treatment and compares the difference in outcomes of children at either side of the actual reform date while differencing out the same difference from the year prior to the reform. The assumption in the RD-DD setting is that discontinuities (if any) at the cut-off in the reform year are similar to discontinuities in the control year in the absence of treatment.

For brevity I only present robustness results with adherence at age 3 as outcome in both the non-adherent and the full sample.[25] Figure 5 presents the robustness checks graphically as sorted coefficients with 95 % confidence intervals. All point estimates from the alternative specifications are close to those obtained in the main specification. In general, the RD-DD specifications produce slightly larger estimates. The estimates are very stable across bandwidth but the confidence intervals widens at lower bandwidths.

Finally, I run a series of placebo tests. First, I use a sample of children defined similarly to the main sample but a year prior to treatment. As Appendix Table A10 shows there is no discontinuity at the cut-off for the placebo sample. Besides being insignificant, the parameter estimates are markedly smaller than the true effects.[26]

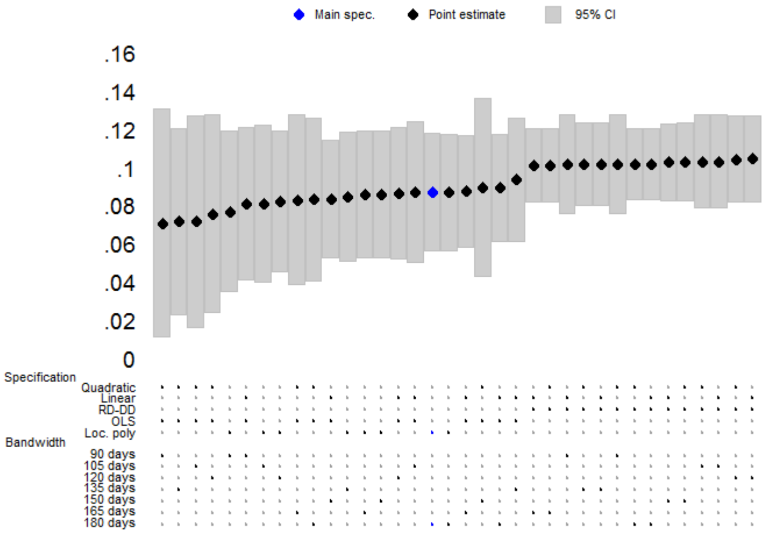
[24] The estimated equation takes the form,

$$Y_i = \beta_0 + \beta_1 1[s_i \geq 0] + f(s) + f(s) \times 1[s_i \geq 0] + \beta_2 T_i + \beta_3 T_i \times 1[s_i \geq 0] + \epsilon_i$$

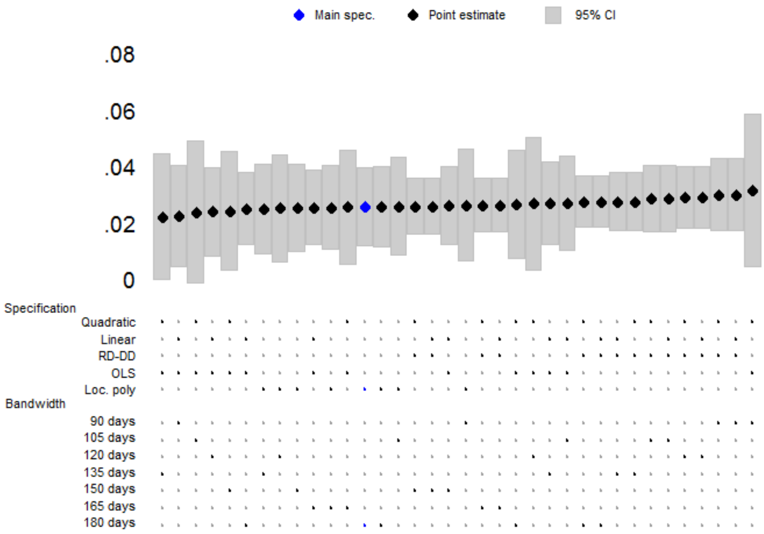
, where T_i is an indicator the the treatment period. β_3 is the difference in discontinuity at the cut-off in the treatment period. I estimate by OLS and robust standard errors.

[25] Robustness checks for the outcomes in Appendix Table A4 are available on request.

[26] Placebo tests for the sibling and cousin analysis are available in Appendix Tables A11.



(a) Non-adherent sample



(b) Full population sample

Fig. 5 Specification curve. Outcome: Indicator for adherence at age 3

Notes: Each dot represents coefficients from various specifications of the effect of reminders on adherence at age 3. The full population sample (panel (b)) includes 56,111 children with second-year birthdays between 17 Nov., 2013 and 11 Nov., 2013. The non-adherent sample (panel (a)) includes 13,926 children with second-year birthdays between 17 Nov., 2013 and 11 Nov., 2014. The RD-DD specification includes a control period of children with second-year birthdays between 17 Nov., 2012 and 11 Nov., 2013. For all specifications, I vary the bandwidth from 90 to 180 days. The grey bars indicate 95 % confidence bans. In the local linear regression estimations, standard errors are bootstrapped with 300 replications. Standard errors are robust in the parametric estimations.

Second, I do random placebo tests where I randomize the selection variable for children in the sample and reestimate the treatment effects for the main outcome. I repeat this procedure 500 times for each outcome and plot the resulting distributions of placebo effects along with the true effects (See Appendix Figure A17).[27] The true effect is an extreme event in the distribution of placebo effects showing that the exact research design is necessary to generate the result.

6 Cost and Effect of Vaccination Reminder Letters

In this section, I compare the cost to the effect on vaccination coverage of the vaccination reminder letter policy. As a measure of the cost of the national reminder letter policy, I rely on the estimated annual additional cost associated with the policy put forth by the government at one million DKK (0.33 million EUR).[28]

The program increase the overall coverage rate (in all recommended vaccinations in the vaccination program) at age 3 with 2.6 %-points implying that the estimated cost of a 1 %-point increase in the coverage rate is 385.000 DKK. An average cohort size (children born during a year) in Denmark is 60.000 children. Thus, around 1560 additional children each year reach full coverage caused by the policy at a cost of 641 DKK (85 EUR) per additional vaccinated child.

Finally two omitted (int the simple calculations above) factors are worth mentioning. First, reminder letters push the MMR coverage rate above the minimum herd immunity threshold for measles. As the infection risk is non-linear around the herd immunity threshold, the benefit of reminder letters might be higher due to the positive externality offered by the presence of herd immunity. Second, the negative sibling spill-overs add an extra cost not captured by the estimated costs of operating the program.

[27] Random placebo tests for the outcomes in Appendix Table A4 are available on request.

[28] The one million DKK covered development of the program as well as operational costs.

7 Conclusion

This paper studies parental responses to vaccination reminder letters in the Danish Childhood Vaccination Program. I use the timing of a policy that introduces reminder letters to parents of non-adherent children at age 2 in a Regression Discontinuity Design. Reminder letters bring attention to parents' failure to adhere with the Danish Childhood Vaccination Program without changing their preferences towards vaccinations as the reminder letters do not include any pro-vaccination campaigning. Furthermore, I provide a framework that links parental responses to reminder letters to a decomposition of causes for non-adherence.

I find that reminder letters increase adherence among non-adherent children at age 2. The fact that reminder letters causally affect vaccination adherence, reveals that inattention is a cause for non-adherence. While I estimate that 8.7 % of non-adherent parents are responsive to reminders, 72.1 % are non-responsive. Thus, reluctance – not inattention – is the primary cause for non-adherence in this setting. This limits the scope of reminder letters to dramatically increase adherence. Particular attentive are parents with university degrees, parents educated in health or childcare and parents of children with low initial health. Because reluctance is the leading cause for non-adherence in the Danish vaccination program at age 2, other types of interventions are necessary to eradicate non-adherence. In order to design alternative interventions one should optimally identify why parents are reluctant.

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A Appendix - For Online Publication

A.1 Translation of Reminder Letter

Dear parent

The Danish Childhood Vaccination Program is a free offer for all children in Denmark. The vaccinations are to ensure that children do not become ill of preventable diseases. According to The Danish Vaccination Register NAME (social security number) lacks those vaccinations marked in red:

Anbefalet alder (Age)	Børnevaccinationer (Childhood vaccinations)	Børneundersøgelser (Child health examinations)
3 mdr.	Difteri – tetanus – kighoste – polio – Hib 1	
5 mdr.	Difteri – tetanus – kighoste – polio – Hib 2	Børneundersøgelse
12 mdr.	Difteri – tetanus – kighoste – polio – Hib 3	Børneundersøgelse
15 mdr.	Mæslinger – fåresyge – røde hunde (MFR 1)	
2 og 3 år		Børneundersøgelse
4 år	Mæslinger – fåresyge – røde hunde (MFR 2)	Børneundersøgelse
5 år	Difteri – tetanus – kighoste – polio revaccination	Børneundersøgelse

We ask you to check whether the child has received the marked vaccinations – either by checking the yellow vaccination card or by contacting your doctor.

Note, that the child might have received all the recommended vaccinations without proper registration in The Danish Vaccination Register, if e.g. your doctor has not billed the vaccination or if the vaccination has been given abroad or at a hospital. Also, vaccination registrations can be delayed up to 3 months. You can disregard the letter if the child has recently received the vaccinations.

You can log any unregistered vaccinations in your medical records at www.sundhed.dk using NEMID.

If you want exemption from future vaccination reminder letters, go to www.ssi.dk/fravalg and follow the instructions.

A.2 Decomposition of Full Population into Adherent, Reluctant, Delaying and Inattentive Parents

I divide the full population of 2-year old children into two broad groups; i) Adherent and ii) non-adherent children. I then split the group of non-adherent children into three subgroups characterized by their cause for non-adherence; ii.1) Reluctance, ii.2) delays and ii.3) inattention. I denote the share of reluctant, delaying and inattentive individuals as Γ, θ, Ω respectively. Thus the share of adherent individuals is $1 - \Gamma - \theta - \Omega$. The adherence rate at age 2 equals,

$$D_{2y} = 1 - \Gamma - \theta - \Omega \tag{9}$$

In the absence of reminder, the adherence rate at age 3 is,

$$D_{3y}^{\text{No letter}} = 1 - \Gamma - \Omega \tag{10}$$

Subtracting the two equals the share of delayers,

$$D_{3y}^{\text{No letter}} - D_{2y} = \theta \tag{11}$$

That is, the change in the adherence rate from age 2 to age 3 gives an estimate of the share of delayers in the population as delayers are defined as individuals that eventually adhere (during year 3) without being reminded (they must be non-reluctant and attentive). I estimate Ω as the difference in adherence between age 2 and 3 just below the cut-off,

$$\theta = \lim_{\epsilon \uparrow 0} E[Y_i \mid s_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[y_i \mid s_i = \epsilon] \tag{12}$$

where Y_i is adherence at age 3 and y_i is adherence at age 2. Following similar arguments, I find Ω as the difference between adherence at age 3 with and without reminders,

$$\begin{aligned} D_{3y}^{\text{Letter}} - D_{3y}^{\text{No letter}} &= \Omega \\ \lim_{\epsilon \downarrow 0} E[Y_i \mid s_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[Y_i \mid s_i = \epsilon] &= \Omega \end{aligned} \tag{13}$$

As well as the share of reluctant individuals as the share of non-adherent children at age 3 after the introduction of reminders,

$$\begin{aligned} 1 - D_{3y}^{\text{Letter}} &= \Gamma \\ 1 - \lim_{\epsilon \downarrow 0} E[Y_i \mid s_i = \epsilon] &= \Gamma \end{aligned} \tag{14}$$

The share of adherent children at age 2 is then simply calculated as $1 - \Gamma - \theta - \Omega$.

A.3 Appendix Figures and Tables

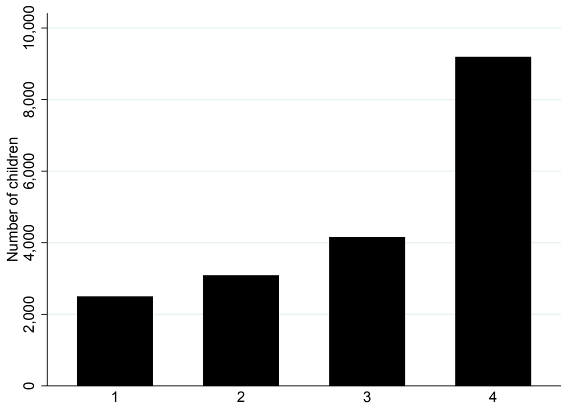


Fig. A1 Number of non-adherent children at age 2 lacking each vaccination round
Notes: The figure shows the number non-adherent children that lacks each vaccination round. For information on the vaccination schedule, consult Table 1. The sample includes 13,926 non-adherent children with second-year birthday between 17 November, 2013 and 11 November 2014.

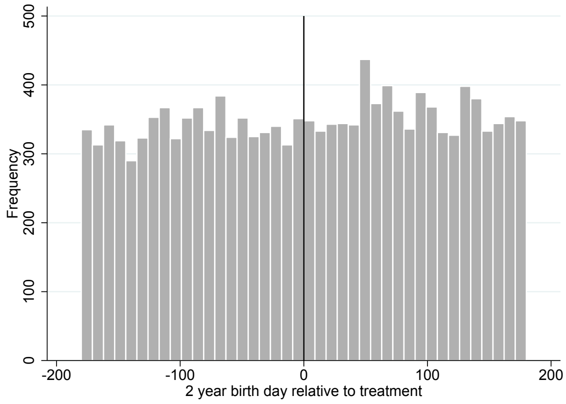


Fig. A2 Manipulation of selection variable: Density of births around the cut-off
Notes: The figure shows the number of children eligible to receive a reminder letter at age 2. Eligibility implies that the child lacks at least one of the four recommended vaccinations scheduled before age 2 in the Danish Childhood Vaccination Program. The sample includes 13,926 children with second-year birthday between 17 November, 2013 and 11 November 2014. The black vertical line marks the reform date: 15 May, 2014. The figure includes 20 bins at either side of the cut-off.

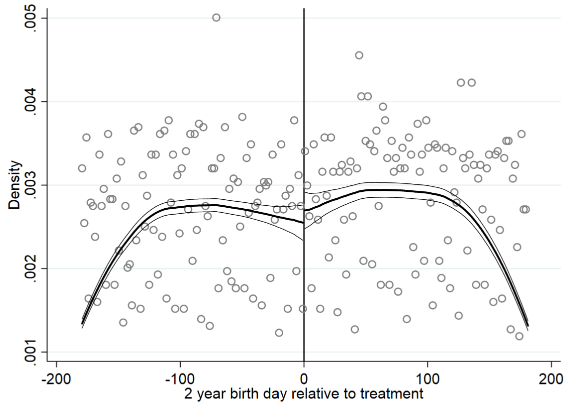


Fig. A3 Manipulation of selection variable: McCrary density test

Notes: The figure shows the estimated density of births around the cut-off. The estimated discontinuity at the cut-off is 0.0555 with a standard error of 0.061 implying that null hypothesis of no jump a the cut-off cannot be rejected.

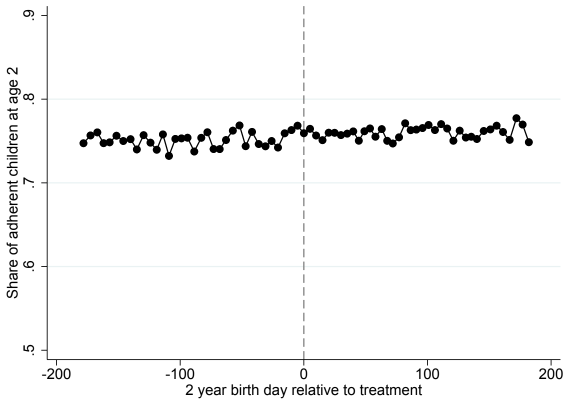
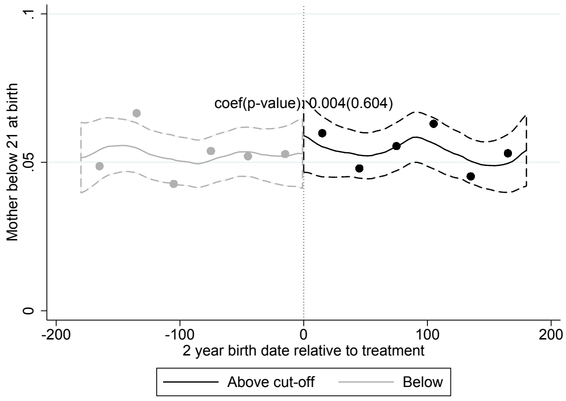
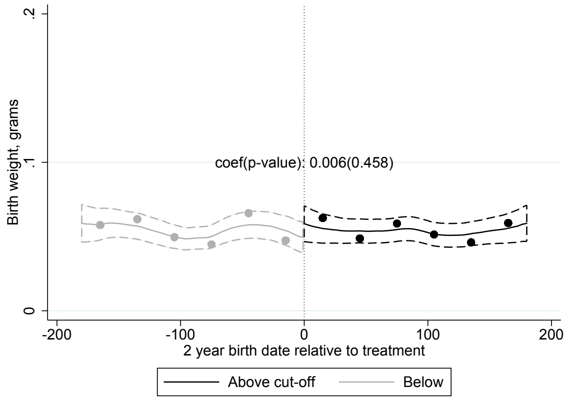


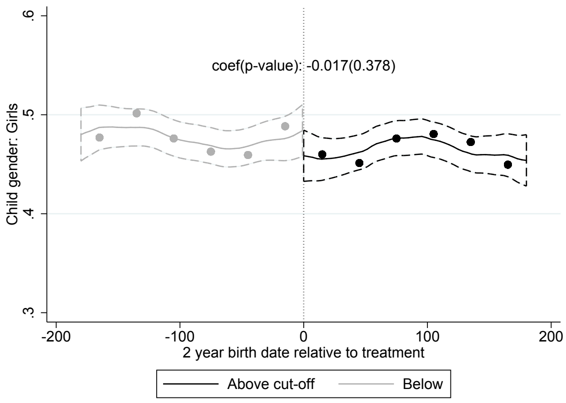
Fig. A4 Share of adherent children: Children with all scheduled vaccinations at age 2
Notes: The figure shows the share of children with all scheduled vaccinations at age 2 and not eligible for reminder letters. Eligibility implies that the child lacks at least one of the four recommended vaccinations scheduled before age 2 in the Danish Childhood Vaccination Program. The sample includes 56,111 children with second-year birthday between 17 November, 2013 and 11 November 2014. The black vertical dashed line marks the reform date: 15 May, 2014. 5-day bin-width.



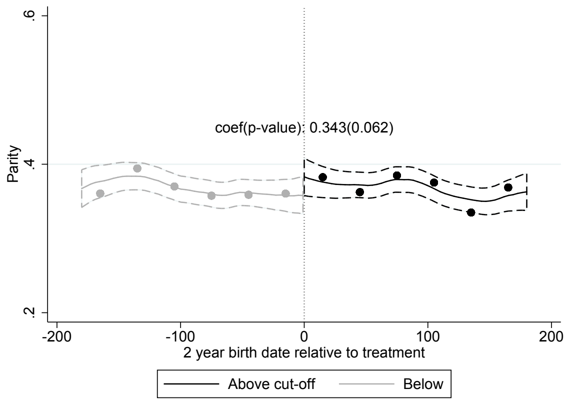
(a) Young mother (below 21 at birth)



(b) Birth weight (grams)



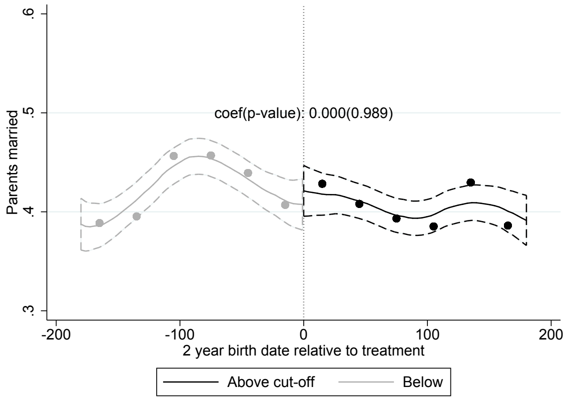
(c) Child gender



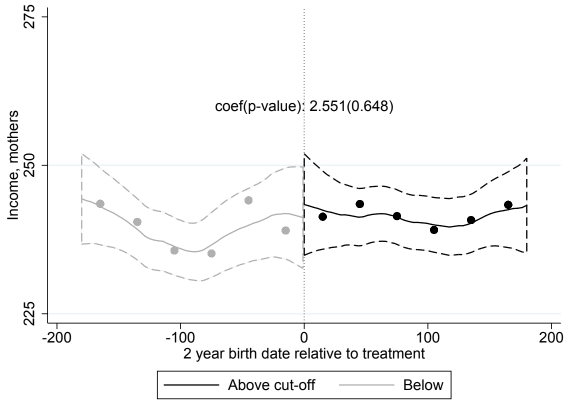
(d) Parity (number of siblings at birth)

Fig. A5 Covariate balance: Birth characteristics

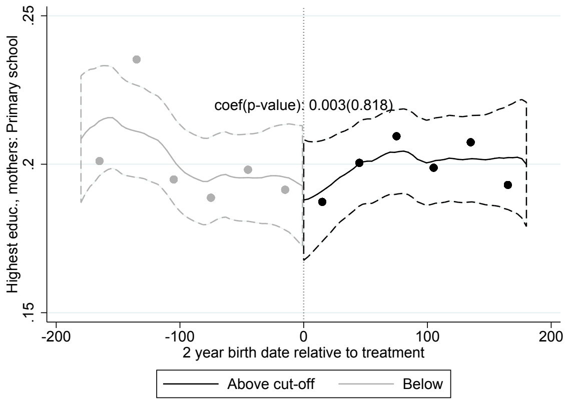
Notes: The figure plots pre-treatment birth characteristics of children without all age-specific vaccinations at age in a 180-day bandwidth at either side of the cut-off. The sample includes 13,926 non-adherent children with second-year birthdays between 17 November, 2013 and 11 November, 2014. The cut-off (dashed vertical line) indicate the reform date at 15 May, 2014. Solid lines indicate fitted values from a local linear regression with a width of bin of 20-day and triangular kernel. Dashed lines are 95 % confidence bans. Dots mark 30-day binned means. The figures include parameter estimates and p-values of tests for whether the discontinuity at the cut-off is significant using local linear regression. Standard errors used to calculate the p-values are bootstrapped with 300 replications.



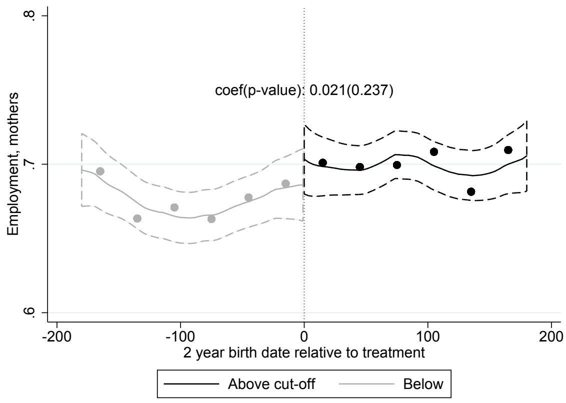
(a) Parents married



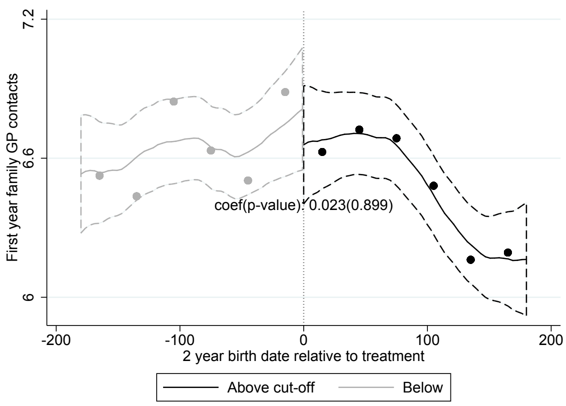
(b) Mothers income



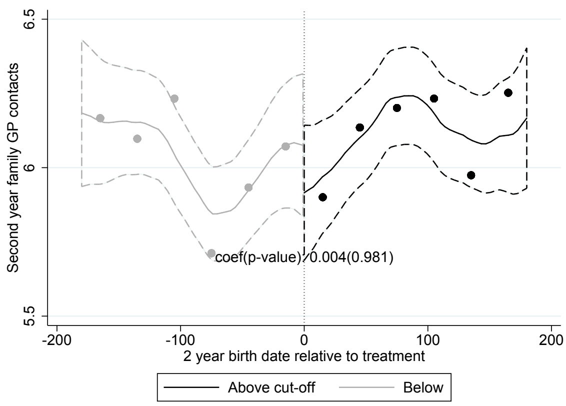
(c) Primary school as highest level of educ., mothers



(d) Mother working

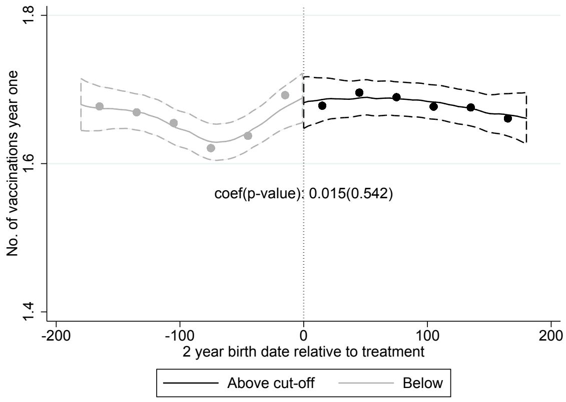


(e) GP contacts - year one

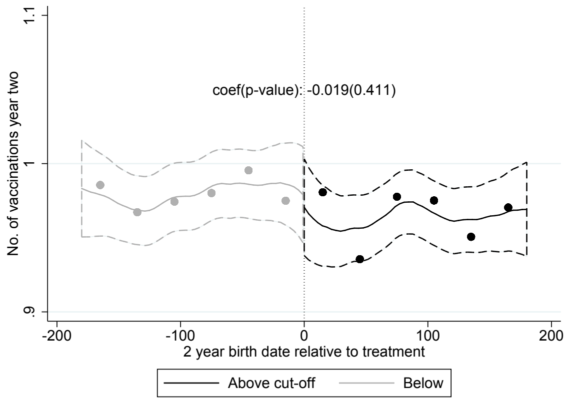


(f) GP contacts - year two

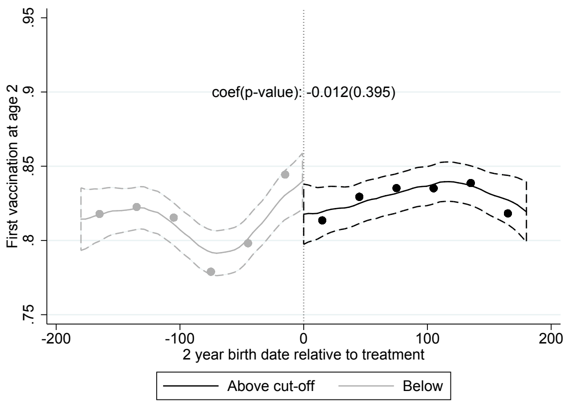
Fig. A6 Covariate balance: Parental pre-determined characteristics and child health care usage
Notes: See notes to Figure A5.



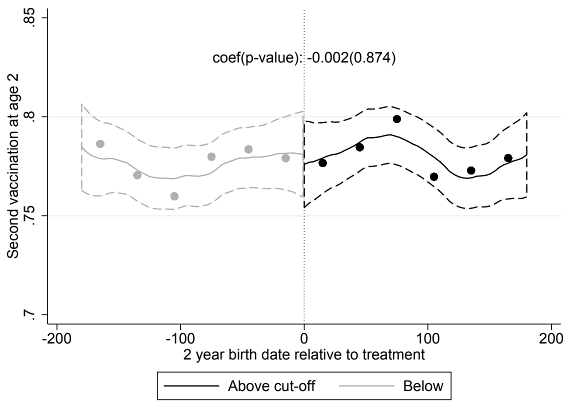
(a) No. of vaccinations - one year



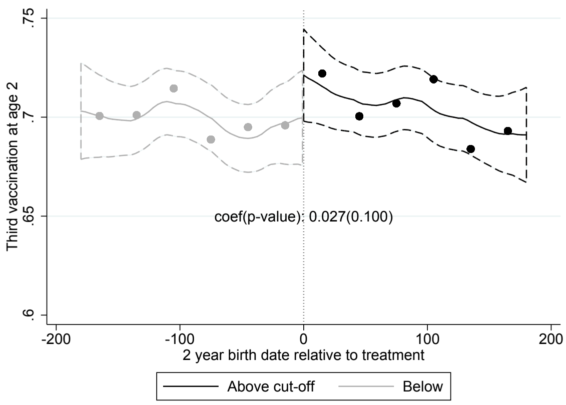
(b) No. vaccinations - two years



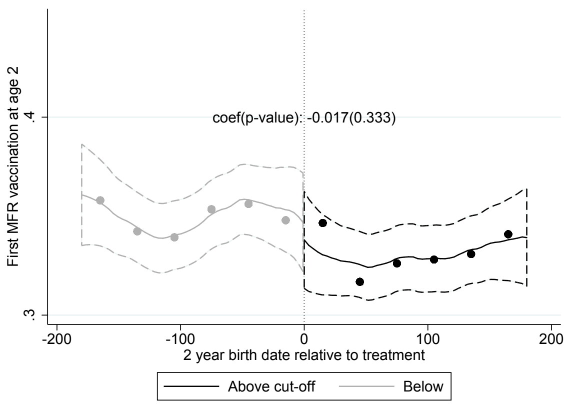
(c) First vaccination round at age 2



(d) Second vaccination round at age 2



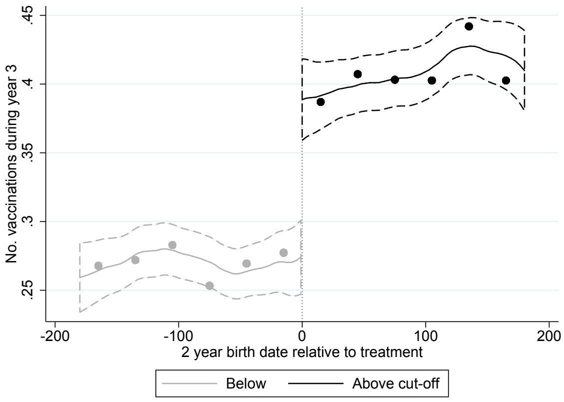
(e) Third vaccination round at age 2



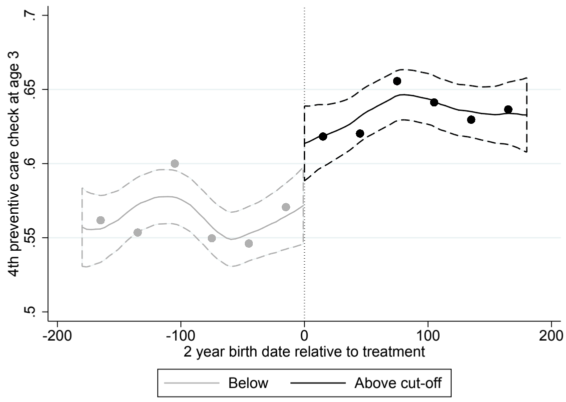
(f) Fourth vaccination round (MMR) at age 2

Fig. A7 Balance of pre-treatment vaccination behavior

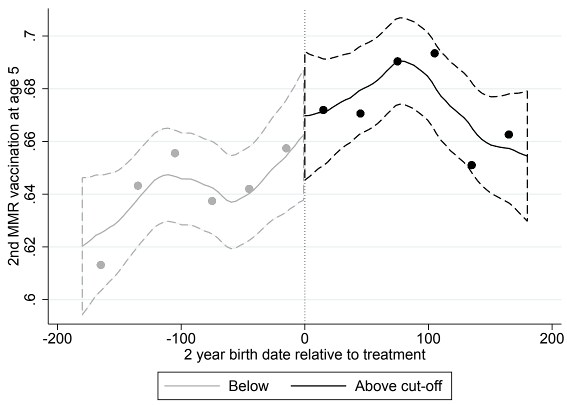
Notes: See notes to Figure A5.



(a) No. vaccinations during year 3



(b) Preventive care check at age 3 - scheduled at 2



(c) 5th vac. (2nd MMR) at age 5 - scheduled at 4

Fig. A8 Impact on vaccination and preventive care participation

Notes: The outcome in panel (a) the outcome is the number of vaccination received during year 3 (the year after treatment). In panel (b) the outcome is an indicator equal to 1 if the child has the preventive care check scheduled at age 2 when the child turns 3 years old and in panel (c) the outcome is an indicator for participation in the 2nd MMR vaccination by age 5. The sample includes 13,926 children with second-year birthdays between 17 November, 2013 and 11 November, 2014 who were non-adherent at age 2. The cut-off (dashed vertical line) indicate the reform date at 15 May, 2014. Solid lines indicate fitted values from a local linear regression with a width of bin of 20-day and triangular kernel. Dashed lines are 95 % confidence bans. Dots mark 30-day binned means.

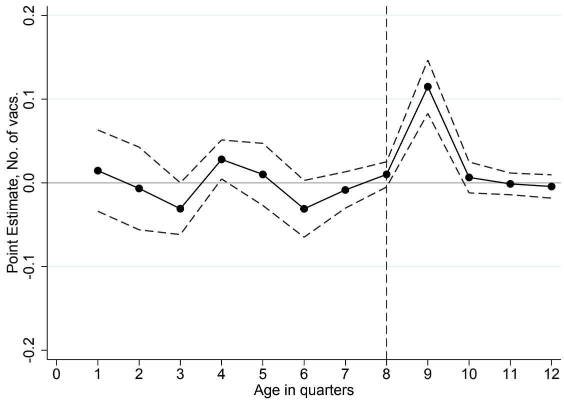
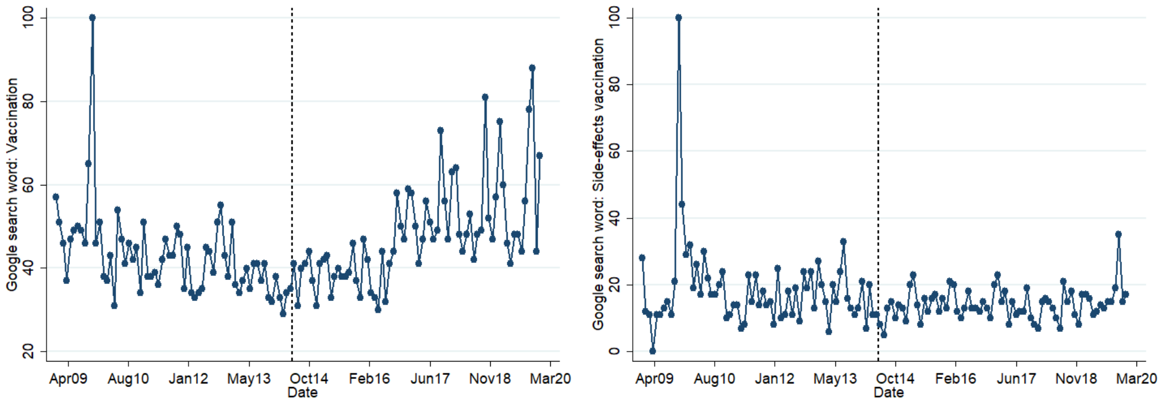


Fig. A9 Timing of vaccination response of reminder letters

Notes: See notes to Table 3. Each dot represent the coefficient from separate regressions. The outcomes are the number of vaccinations a child receives during each quarter of life from birth to age 3. The vertical line shows the age at which reminders are sent. The sample includes 13,926 children with second-year birthdays between 17 November, 2013 and 11 November, 2014 who were non-adherent at age 2. Dashed lines are 95 % confidence bans.



(a) Search word: “Vaccination“ **(b)** Search word: “Side-effects vaccination“

Fig. A10 Google trends around the introduction of reminder letters
Notes: Data from Google Trends. In panel (a), the search word is “vaccination“ and the search word is “side-effects vaccination“ in panel (b).

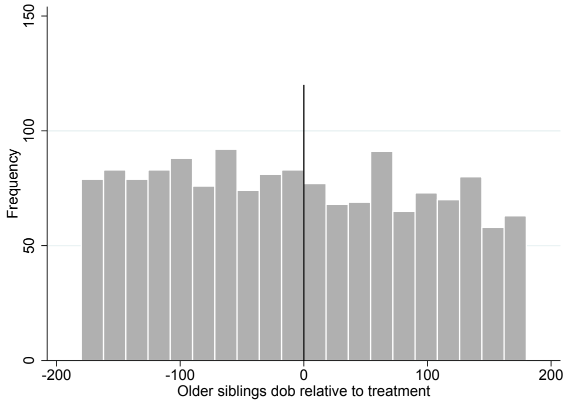


Fig. A11 Density of siblings around the cut-off - the selection variable is the the older siblings second year birthday relative to the reform date

Notes: The figure shows the number of younger siblings of children eligible to receive a reminder letter at age 2 around the cut-off date. The sample includes 1,532 siblings of children who had second-year birthday between 17 November, 2013 and 11 November, 2014, still reside in Denmark and have not received all age-specific vaccinations by age 2 (non-adherent). I restrict siblings to have date of births between 1 November, 2013 and 31 December, 2014. The black vertical line marks the reform date: 15, May 2014. The figure includes 10 bins at either side of the cut-off.

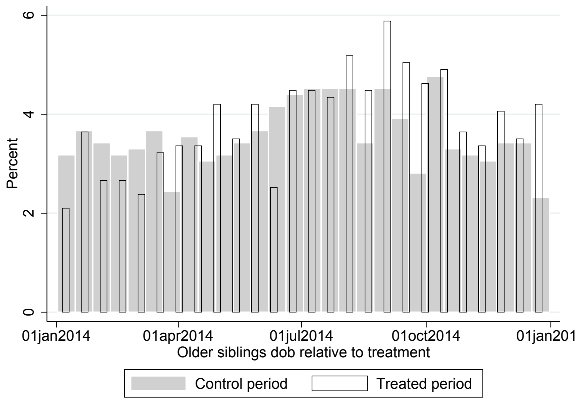
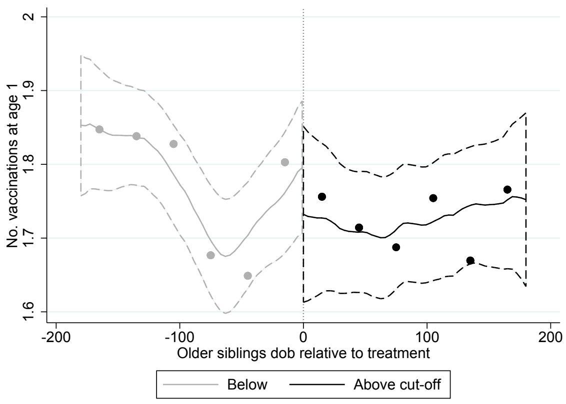
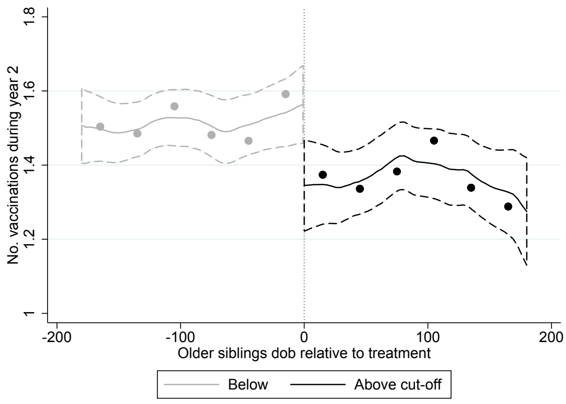


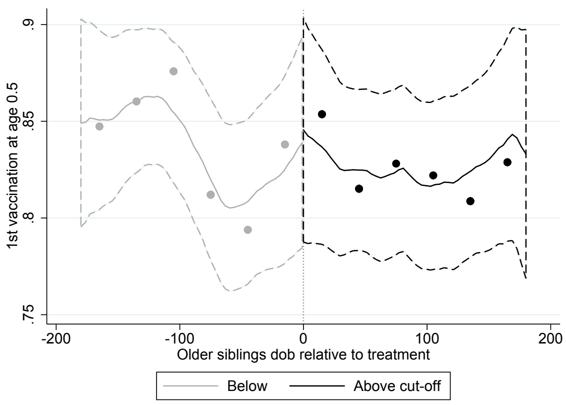
Fig. A12 Date of births of individuals in the sibling sample divided by treatment status
Notes: The figure shows the number of younger siblings – by their date of births – of children eligible to receive a reminder letter at age 2 around the cut-off date. The sample includes 1,532 siblings of children who had second-year birthday between 17 November, 2013 and 11 November, 2014, still reside in Denmark and have not received all age-specific vaccinations by age 2 (non-adherent). I restrict siblings to have date of births between 1 November, 2013 and 31 December, 2014. Grey bars are siblings of children with second year birthdays prior to the cut-off (controls) while transparent bars with black outline are siblings of children with second year birthdays after the cut-off (treated).



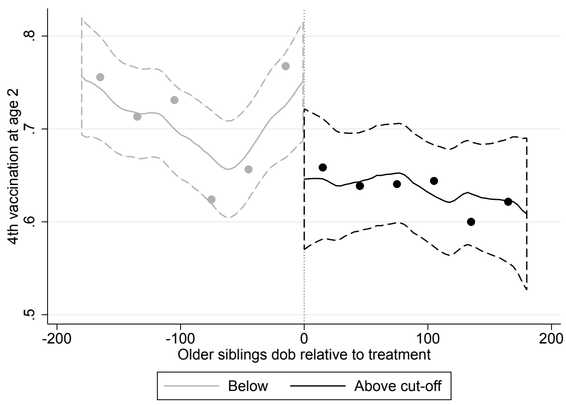
(a) Number of vaccinations at year 1



(b) Number of vaccinations during year 2



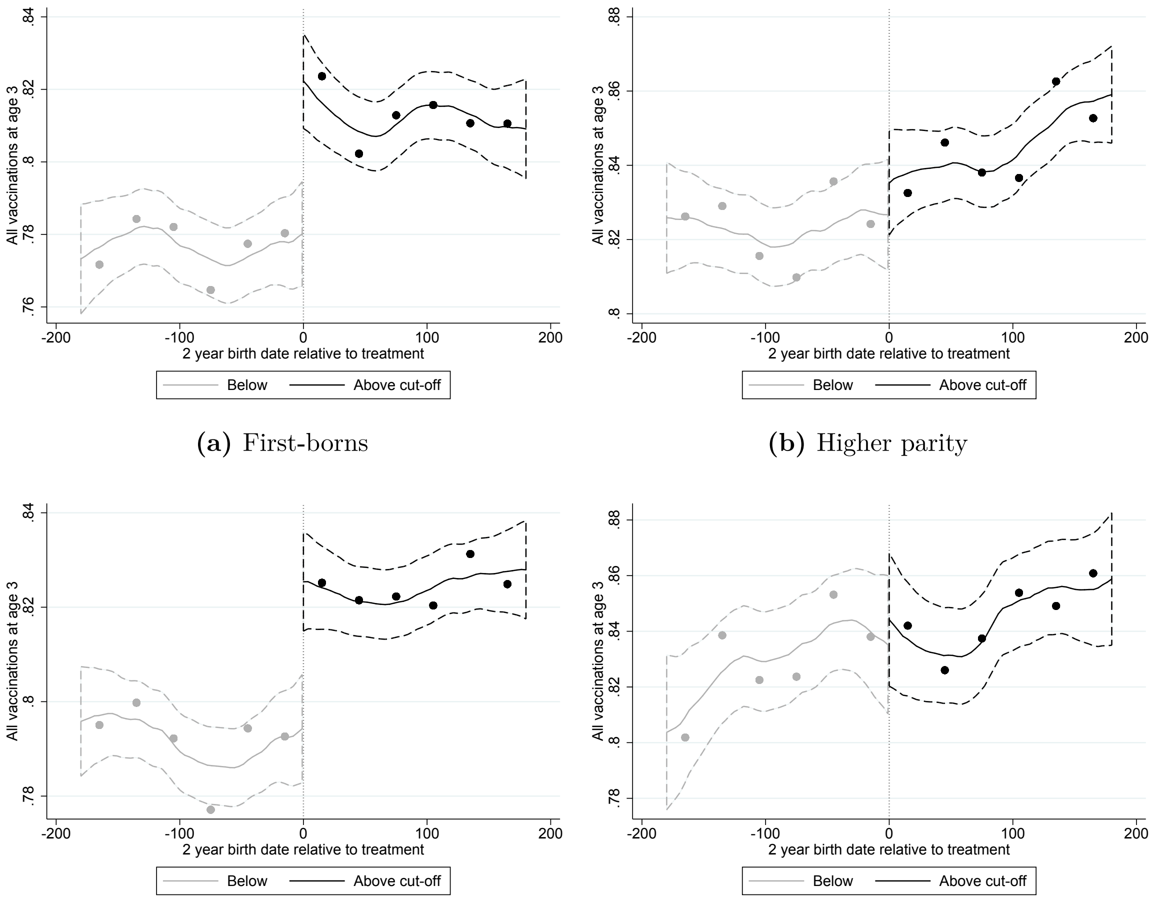
(c) Participation in 1st vaccination round by age 6 month



(d) Participation in 4th vaccination round (MMR) by age 2

Fig. A13 Sibling spill-overs: Impact of reminder letters on vaccination adherence of younger siblings

Notes: The outcome in panel (a) is the number of vaccinations received at age 1. In panel (b) the outcome is the number of vaccination during the second year of life. In panel (c) and (d) the outcomes are indicators equal to 1 if the child has the first and fourth vaccination round at age 6 month and 2 years respectively. The sample includes 1,532 siblings of children who had second-year birthday between 17 November, 2013 and 11 November, 2014, still reside in Denmark and have not received all age-specific vaccinations by age 2 (non-adherent). I restrict siblings to have date of births between 1 November, 2013 and 31 December, 2014. The selection variable is the second birthday of the treated children (older sibling) relative to cut-off (dashed vertical line) at 15 May, 2014. Solid lines indicate fitted values from a local linear regression with a width of bin of 20-day and triangular kernel. Dashed lines are 95 % confidence bans. Dots mark 30-day binned means.



(a) First-borns

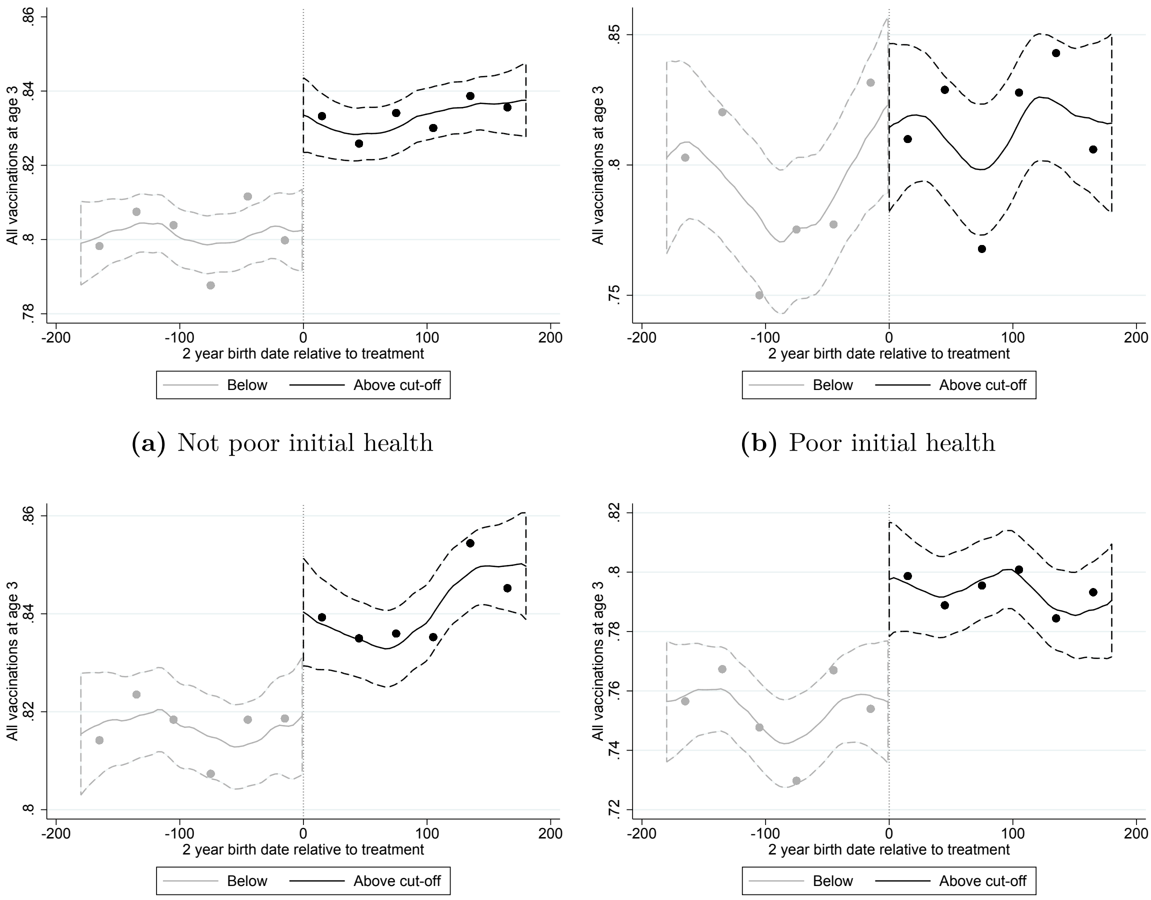
(b) Higher parity

(c) Not health educated

(d) Health educated

Fig. A14 Heterogeneity: Impact of reminder letters on vaccination adherence by parity and parental education

Notes: The outcome in all panels is an indicator for adherence at age 3. The sample before splitting it into subgroups includes 56,111 children with second-year birthdays between 17 November, 2013 and 11 November, 2014. In panel (a) and (b) the sample is split by whether the child is first-born. In panel (c) and (d) the sample is split by parental health education. A health educated parent is educated in health or childcare. The cut-off (dashed vertical line) indicate the reform date at 15 May, 2014. Solid lines indicate fitted values from a local linear regression with a width of bin of 20-day and triangular kernel. Dashed lines are 95 % confidence bans. Dots mark 30-day binned means.



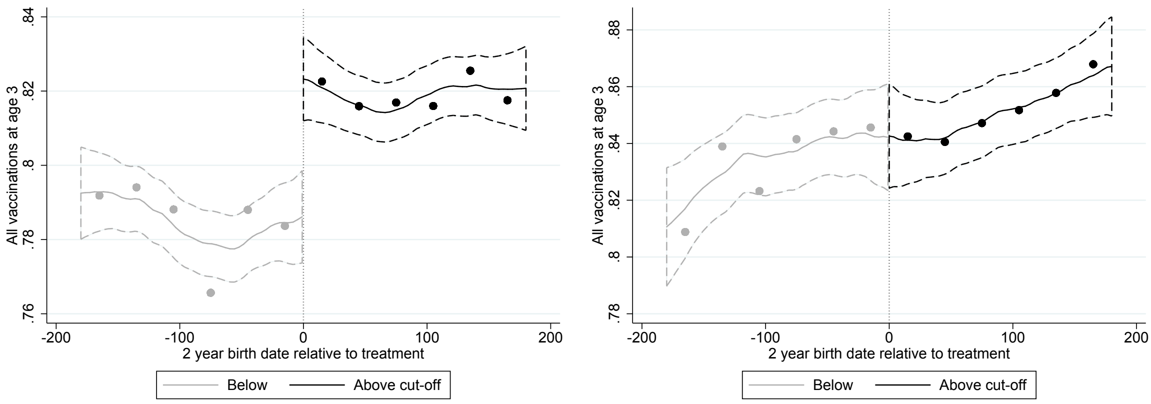
(a) Not poor initial health

(b) Poor initial health

(c) High SES parents

(d) Low SES parents

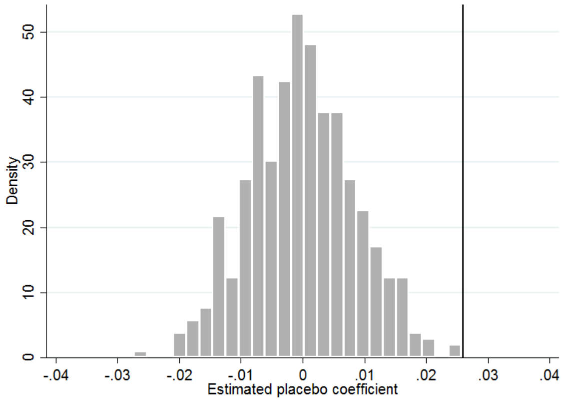
Fig. A15 Heterogeneity: Impact of reminder letters on vaccination adherence by initial health and SES
Notes: See notes to Appendix Figure A14. In panel (a) and (b) the sample is split by initial health. Poor initial health is defined by being born with low birth weight, prematurely or with complications. In panel (c) and (d) the sample is split by parental SES.



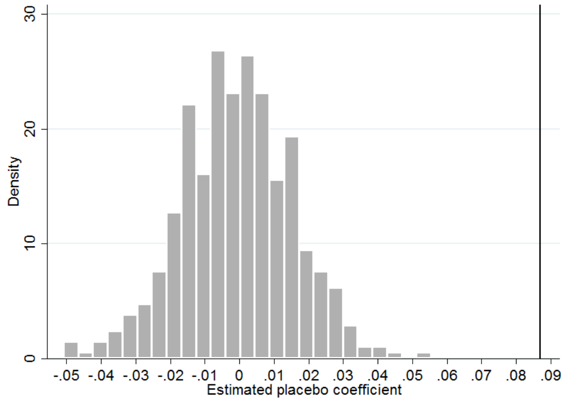
(a) Lower than uni. degree

(b) University degree

Fig. A16 Heterogeneity: Impact of reminder letters on vaccination adherence by parental university education
Notes: See notes to Appendix Figure A14. In panel (a) and (b) the sample is split by whether on parents have a university degree as highest level of education.



(a) All vaccinations at age 3 - Full pop. sample



(b) All vaccinations at age 3 - Sample of non-adherent children

Fig. A17 Random placebo tests

Notes: See notes to Table 3. I randomize the selection variable for children in the sample and reestimate the placebo effect 500 times. The figures plot the distribution of the resulting placebo estimates along with the true effects (black vertical lines). The full population sample includes 56,111 children with second-year birthdays between 17 November, 2013 and 11 November, 2013. The non-adherence sample includes 13,926 children with second-year birthdays between 17 November, 2013 and 11 November, 2014 who were non-adherent at age 2.

Tab. A1 Summary statistics: Adherent and non-adherent children in the Danish Childhood Vaccination Program at age 2

	Not all vacs. at age 2 Mean	All vacs. at age 2 Mean	Test for significance P-value
First-time mothers	0.37	0.48	0.000***
Young mother	0.05	0.04	0.000***
Young father	0.02	0.01	0.000***
Low birth weight	0.05	0.05	0.021**
Preterm birth	0.07	0.06	0.068*
Child sex (female)	0.47	0.49	0.000***
Home birth	0.02	0.01	0.000***
C-section	0.22	0.22	0.375
Income, mother	240.59	261.35	0.000***
Prim. school, mother	0.20	0.13	0.000***
Higher educ, mother	0.24	0.28	0.000***
Uni. degree, mother	0.15	0.19	0.000***
Danish, mother	0.79	0.84	0.000***
Student, mother	0.05	0.05	0.343
Employed, mother	0.69	0.77	0.000***
Cohabiting	0.81	0.86	0.000***
Married	0.41	0.44	0.000***
Income, father	334.50	355.45	0.000***
Prim. school, father	0.20	0.16	0.000***
Higher educ, father	0.16	0.19	0.000***
Uni. degree, father	0.14	0.17	0.000***
Danish, father	0.79	0.84	0.000***
Student, father	0.03	0.02	0.367
Employed, father	0.79	0.85	0.000***
Obs.	13926	42185	

Notes: The sample includes 56,111 children with second-year birthdays between 17 November, 2013 and 11 November, 2014 divided by whether or not they have received all age-specific vaccinations by age 2. All variables are measured at birth.

Tab. A2 Number of vaccinations at age 2 in sample period population

	(1) Children	(2) Share out of total population
Zero vaccinations	521	0.9 %
One vaccination	536	1.0 %
Two vaccinations	2,390	4.3 %
Three vaccinations	10,488	18.7 %
Four vaccinations	42,185	75.2 %
Non-adherent at age 2	13,926	24.8 %
Adherent at age 2	42,185	75.2 %
Total	56,111	100 %

Notes: The total population consists of children who turned two years between 17 November, 2013 and 11 November, 2014 and still reside in Denmark. Non-adherent children have fewer than four vaccinations at age 2. Adherent children have four vaccinations at age 2.

Tab. A3 Summary statistics: Non-adherent children at age 2 in the Danish Childhood Vaccination Program between 15 November, 2013 and 15 November, 2014 by region

	Copenhagen	Zealand	Southern Denmark	Central Jutland	North Jutland
All vacs. at age 2	0.75	0.73	0.76	0.77	0.74
N	18934	7049	8688	16165	4892

Notes: The sample consists of children who had second-year birthday between 17 November, 2013 and 11 November, 2014 (180-days below and above the cut-off at the reform date 15 May, 2014) and still reside in Denmark.

Tab. A4 Impact of reminder letters on vaccination and preventive care participation

	(1) No. vacs., year 3	(2) Prev care, age 3	(3) 2nd MMR, age 5
Reminder letters	0.115***	0.055***	0.015
	(0.019)	(0.019)	(0.017)
Below cut-off mean	0.27	0.56	0.64
Obs.	13926	13926	13926

Notes: Each cell show coefficients from separate regressions given by the outcomes listed at the top of each column. The outcome in column (1) the outcome is the number of vaccination received during year 3 (the year after treatment). In column (2) the outcome is an indicator equal to 1 if the child has the preventive care check scheduled at age 2 when the child turns 3 years old. In column (3) the outcome is an indicator for participation in the 2nd MMR vaccination by age 5. Coefficients are estimates of the discontinuity at the cut-off. The cut-off is the implementation date of reminder letters on 15 May, 2014. The sample consists of children who had second-year birthday between 17 November, 2013 and 11 November, 2014, still reside in Denmark and have not received all age-specific vaccinations by age 2 (non-adherent). I use local linear regression, a triangular kernel and a 180-day bandwidth on each side of the cut-off. Bootstrapped standard errors with 300 replications in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Tab. A5 Impact of reminder letters on vaccination participation by pre-treatment vaccination behavior

	(1)	(2)
	Outcome: All vacs. at age 3	
	Lack 1 vac.	*Lack > 1 vac.*
Reminder letters	0.099***	0.050**
	(0.021)	(0.020)
Obs.	10488	3438
	Missed 1st vac.	*Received 1st vac.*
Reminder letters	0.015*	0.107***
	(0.009)	(0.018)
Obs.	2496	11430
	Missed 2nd vac.	*Received 2nd vac.*
Reminder letters	0.018*	0.107***
	(0.010)	(0.018)
Obs.	3085	10841
	Missed 3rd vac.	*Received 3rd vac.*
Reminder letters	0.089***	0.080***
	(0.021)	(0.020)
Obs.	4153	9773
	Missed 4th (MMR) vac.	*Received 4th (MMR) vac.*
Reminder letters	0.099***	0.050***
	(0.021)	(0.015)
Obs.	9190	4736

Notes: See notes to Table 3. The outcome in all panels is an indicator for adherence at age 3. In the first panel, I split the sample by whether the child lack one vaccination or more than one. In the rest of panels, I split the sample by whether the child lack each vaccination round or not. The sample includes 13,926 children with second-year birthdays between 17 November, 2013 and 11 November, 2014 who were non-adherent at age 2. Bootstrapped standard errors with 300 replications in parenthesis. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Tab. A6 Herd immunity thresholds for diseases covered by The Danish Childhood Vaccination Program

	- - - *Herd immunity threshold* - - -
Diphtheria-tetanus-pertussis-polio-Hib	
Diphtheria	0.75 – 0.80
Pertussis	0.90 – 0.94
Polio	0.80 – 0.86
MMR	
Measles	0.91 – 0.94
Mumps	0.86 – 0.93
Rubella	0.83 – 0.94

Notes: Estimates of herd immunity thresholds from Plans-Rubió (2012).

Tab. A7 Spill-overs part II: Siblings and cousins of non-adherent children in the Danish Childhood Vaccination Program born between 17 November, 2013 and 11 November, 2014

	(1) 1st vac., 6 month	(2) 2nd vac., age 1	(3) 3rd vac., 18 month	(4) 4th (MFR) vac., age 2
	Siblings of non-adherent children around the cut-off			
Reminder letters	0.017	-0.017	-0.050	-0.068
	(0.043)	(0.044)	(0.050)	(0.053)
Obs.	1532	1532	1532	1532
	Cousins of non-adherent children around the cut-off			
Reminder letters	0.005	-0.034	-0.011	0.027
	(0.028)	(0.034)	(0.054)	(0.051)
Obs.	574	574	574	574

Notes: See notes to Table 4. Bootstrapped standard errors with 300 replications in parenthesis. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Tab. A8 Test for significant heterogeneous effects of reminder letters

	(1) Test for significance in fully interaction models
First-borns	-0.033**
	(0.013)
Health educated parents	-0.055***
	(0.018)
Poor initial health	-0.018
	(0.024)
Low SES	0.030**
	(0.015)
University degree	-0.051***
	(0.014)
Obs.	56111

Notes: Each cell shows the estimate, significance and standard errors (in parenthesis) of the interaction between an subgroup indicator and a dummy for being above the cut-off in separate regressions. The interaction models are estimated by OLS with linear and separate trends on either side of the cut-off and a bandwidth of 180 days and fully interaction terms with subgroup indicators. The labels indicate the subgroup that takes the value 1. The sample includes 56,111 children with second-year birthdays between 17 November, 2013 and 11 November, 2014. Robust standard errors in parenthesis. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Tab. A9 Heterogeneity by observed characteristics: Impact of reminders letters on adherence at age 3 in full population and decomposition of parents

	(1)	(2)
	Outcome: All vacs. at age 3	
	Higher parity	First-borns
Reminder letters	0.041***	0.005
	(0.010)	(0.011)
Responsive	0.041	0.005
Delaying	0.063	0.029
Non-responsive	0.183	0.167
Adherent	0.714	0.799
Obs.	30831	25280
	Not health and childcare educ.	Health and chilcare educ.
Reminder letters	0.032***	-0.010
	(0.008)	(0.016)
Responsive	0.032	-
Delaying	0.048	0.047
Non-responsive	0.177	0.168
Adherent	0.743	0.785
Obs.	47827	8284
	Not poor	Poor initial health
Reminder letters	0.029***	-0.004
	(0.008)	(0.025)
Responsive	0.029	-
Delaying	0.048	0.047
Non-responsive	0.174	0.189
Adherent	0.749	0.764
Obs.	51462	4649
	High SES	Low SES
Reminder letters	0.019**	0.040***
	(0.008)	(0.015)
Responsive	0.019	0.040
Delaying	0.045	0.056
Non-responsive	0.165	0.204
Adherent	0.771	0.701
Obs.	39517	16594
	Lower than uni. degree	Uni. degree
Reminder letters	0.038***	-0.007
	(0.009)	(0.014)
Responsive	0.038	-
Delaying	0.048	0.049
Non-responsive	0.180	0.162
Adherent	0.735	0.789
Obs.	41898	14213

Notes: See notes to Table 3 for details on estimation method. Bootstrapped standard errors with 300 replications in parenthesis. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Tab. A10 Placebo test: Sample of children eligible for reminder letter a year prior to the reform

	(1) All vacs., age 3, full pop.	(2) All vacs., age 3, non-adh. pop.	(3) No. vacs., year 3	(4) Prev. care, age 3	(5) 2nd MMR, age 5
Placebo sample: Children with second-year birthdays year prior					
Reminder letter	-0.007	-0.012	-0.019	0.007	0.002
	(0.007)	(0.013)	(0.017)	(0.017)	(0.016)
Obs.	57977	14398	14398	14398	14398

Notes: See notes to Table 3 and Appendix Table A4 for details on estimation method. The sample consists of children who had second-year birthday between 17 November, 2012 and 11 November, 2013. In column (2) - (5) the sample is restricted to non-adherent children at age 2. The cut-off date is 15 May, 2013, a year prior to the actual reform date. Bootstrapped standard errors with 300 replications in parenthesis. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.10$.

Tab. A11 Placebo test: Siblings and cousins of adherent children in the Danish Childhood Vaccination Program born between 17 November, 2013 and 11 November, 2014

	(1) No. vacs, 6 month	(2) No. vacs, year 1	(3) No. vacs, year 2	(4) No. vacs, age 2
	Placebo sample: Siblings of adherent children around the cut-off			
Reminder letters	0.035	-0.003	-0.033	-0.037
	(0.029)	(0.024)	(0.033)	(0.036)
Obs.	6354	6354	6354	6354
	Placebo sample: Cousins of adherent children around the cut-off			
Reminder letters	0.028	0.000	0.016	0.016
	(0.054)	(0.048)	(0.056)	(0.070)
Obs.	1689	1689	1689	1689

Notes: See notes to Table 4 for details on estimation method. The sample consists of younger siblings (top panel) and cousins (bottom panel) of adherent (untreated) children who had second-year birthday between 17 November, 2013 and 11 November, 2014. The reform date is 15 May, 2014. Bootstrapped standard errors with 300 replications in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$.

Chapter 4

Nurses and Parental Health Investments

Nurses and Parental Health Investments*

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Abstract

Encouraging parental investments in children's health is a central goal of public health policies. This paper studies the impact of nurse home visiting for new families on the timely uptake of recommended preventive care. We use newly-collected nurse records merged with administrative register data from Denmark. Nurses pro-actively offer visits to all families with newborn children during the first year of life, but parents have to take action themselves to receive other elements of preventive care, among them vaccinations. We exploit variation in the exact timing of nurse home visits in a narrow time window around the recommended age for preventive care to show that nurses matter for uptake of care: Parents delay their first recommended GP health check if they receive a nurse visit at the relevant age. This finding suggests that parents attempt to distribute personal contacts to different primary health care providers to separate weeks. At the same time, parents are more likely to take up the first (and second) vaccination at the recommended age, if they receive a nurse visit at that age. As we find no or small longer-run differences in vaccination and care uptake across groups, our results suggest that nurses act as reminders rather than change parental beliefs about the importance of preventive care and vaccinations.

*We thank the 62 Danish municipalities that have shared their data on home visiting with us. The use of these municipal data in our research project was approved by the Danish Patient Safety Authority (approval 3-3013-2507/1).

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1 Introduction

Socio-economic gradients in childhood health remain an important policy concern across the world. One factor that may contribute to those inequalities and that has increasingly gained attention is the "investment gap", i.e., variation in parental health investments (Attanasio et al., 2014, 2019). Given the decisive role of parental investments for the formation of children's health, an important question is whether (and which) policies can encourage adequate parental health investments. Evidence on home visiting programs targeted at disadvantaged parents points to potentially large benefits of those policies in terms of both parental investments, and short- and long-run child outcomes (Olds et al., 1986, 1998, 2002; Vaithianathan et al., 2016; Doyle et al., 2015; Doyle, 2017; Sandner et al., 2018; Sandner, 2019; Conti et al., 2020).

This paper studies the impact of universally-offered nurse home visiting (NHV) for families with infants on parental health investments. In Denmark, all new families receive a default offer of 3-5 home visits during the first year of the child's life. A central goal of those visits is to encourage adequate parental health investments: Visits cover (age-related) topics with relevance for parental investment behaviors, such as infant feeding and sleep, support of infant development, infant-parent interaction, and parental mental health. Moreover, nurses explicitly inform about and encourage uptake of preventive health checks and vaccinations at the general practitioner (GP) (The Danish National Board of Health, 2019).[1] While those other services are also universally accessible and free of charge, parents have to make an active choice about uptake (contact the GP to make an appointment).[2] Thus uptake of preventive health checks and vaccinations serve as our main measures of parental health investments in the first year of the child's life.[3]

[1] https://www.sundhed.dk/borger/patienthaandbogen/boern/undersoegelser/besoeg-af-sundhedsplejersken/

[2] Parents also have to accept the offer of a nurse visit but they do not have to actively contact home visiting nurses. Nurses contact all families with newborn children and offer the visits regardless of the families' actions.

[3] As we use administrative data, a central drawback is that we do not observe a wide ranger of other important parental investments.

While provided in many settings, we have little evidence on the impact of large-scale NHV on family health and family health behaviors.[4] The main reasons for this dearth are – even in the Scandinavian setting – a lack of individual (child) level data and credible variation. Our paper relies on newly-collected individual level nurse records from 59 out of 98 Danish municipalities merged with administrative register data.[5] Our data identify children and families, their assigned nurses, nurse treatment decisions along a number of margins, family background characteristics and measures of health care usage, among those the uptake of national preventive care and vaccinations, for the Danish birth cohorts 2012-2015.

Using these new data, we exploit arbitrary variation in the exact timing of nurse visits: The suggested timing of universal home visits overlaps with the recommended age for child vaccinations and preventive health checks at family GPs.[6] Thus we compare the timing of adherence with the recommended vaccinations and health checks for two groups of parents: a group, who receives a nurse home visit shortly prior to the recommended age of the vaccination/health check, and a group, who receives their visit shortly after. Specifically, we focus on the timely uptake of the first GP health check at five weeks of the child's life, the first vaccination round at three months of the child's life, and finally, the second GP health check and vaccination round five months after birth.

We find that nurse visits impact the timely parental uptake of preventive care, and that the content of care (vaccinations or GP health checks) as well as its timing in the first year of the child's life matter: Parents, who have a nurse visit shortly prior to the recommended age for the first GP check at five weeks, have a lower probability of timely adherence to that check compared to parents, who have their nurse visit shortly after the recommended age for the health check. Contrary, we find that parents with a closely-spaced nurse visit prior

[4] For exceptions, see Kronborg et al. (2016); Hirani et al. (2020)

[5] While Danish administrative data exists in many domains, there has earlier not been detailed individual level information on sub-nationally administered social and health programs, such as NHV. We start filling this gap with our newly collected municipal nurse data linked to the well-known administrative data sources at Statistics Denmark.

[6] As an example, nurses are supposed to schedule a home visit in the third month of the child's life and infants are supposed to complete their first vaccination at the age of three months.

to the recommended age for the first vaccination at three months have a higher probability of timely adherence compared to parents with a slightly delayed nurse visit. Around the age of five months, the recommended timing for the second GP check and vaccination, we also find evidence of more timely adherence for families with a nurse visit shortly prior to the recommended age. Thus parents appear to substitute contacts with different providers in one case (where preventive care consists of a personal contact to a primary health care provider), but appear to increase timely adherence in another case (where preventive care consists of a vaccination). The timely pattern of uptake of care supports the suggestion that, in our setting, nurses act as reminders rather than playing a role in convincing parents about the importance of adherence: We find that across vaccinations (and GP health checks), the adherence of families converges in the longer run.

We perform all our analyses on two samples of children, a universal and a high impact sample. In the universal sample, families receive a universally-granted visit closely spaced around the time of recommended care (but no need-based visits). In our high impact sample, families receive additional need-based visits, which nurses can grant to families with identified risk factors or issues. Thus we divide our sample of families along a risk dimension defined by nurses (who decide to grant additional, need-based visits).[7] We find similar impacts of nurse visits on the timing of uptake of care in both samples.

For our estimates to reflect the impact of NHV, we assume that in the narrow time frame that we consider (a total of three weeks around the recommended age for a specific preventive care episode), the timing of nurse visits to families is uncorrelated with other determinants of parental behavior. Informally supporting this claim, we show that – conditional on nurse fixed effects, i.e. comparing families with the same nurse and in the same municipality – a broad range of predetermined observable characteristics do not predict treatment status. Moreover, besides the nurse fixed effect, all our results are conditional on our large set of observable

[7]We also conduct analyses on a pooled sample (using information on the closest visit around the recommended age for preventive care or a vaccination irrespective of visit type) with very similar results.

characteristics of families.[8] Thus we make probable that we do not capture the impact of other family characteristics or the impact of differences across nurses. When interpreting our results and thinking about their generalizability, we have to keep in mind that all parents in our sample receive nurse visits in a narrowly defined time frame: thus our estimates (and the finding that nurses do not persistently impact parental decisions but only the timing) may not capture all dimensions that are relevant. For example, receiving nurse visits versus not doing so may have a strong impact on parental beliefs (about the benefits of vaccination adherence) but we are unable study this effect in our analysis.

Why should we care about the timely uptake of vaccinations and GP preventive care? In many developed countries not only full non-adherence but also non-timely adherence in vaccination programs pose major challenges for public health and are high on the policy agenda. Denmark, for example, has seen recent breakouts of whooping cough among infants and a central policy recommendation to prevent future outbreaks is to ensure timely vaccination uptake (Wolf and Højgaard, 2020; Andersen and Knudsen, 2015).[9] Importantly, the second vaccination round recommended at age five months after birth is scheduled prior to the age at which most children start in formal childcare in Denmark, which exposes children to enhanced infection risks. Similar to the vaccination program, timely uptake of preventive health checks has implications as certain types of screening and timely referral to specialized care in the case of identified need critically depend on timing.

Our study contributes new evidence on the importance of early-life health interventions and the interaction of early-life health policies, an area that is still under-researched for developed health care systems. For the design of preventive health policy (or rather the policy landscape), evidence on the ways in which exposure to *several* interventions impacts both children's outcomes but also parental inputs is instrumental. Moreover, we relate to a growing literature studying the determinants of parental investment behaviors (Aizer and McLanahan,

[8] In robustness analyses we compare families in the same municipality.

[9] Timely vaccinations for infants are important to control infectious diseases because infants are born unprotected and immunity against whooping cough requires two vaccination rounds.

2006; Almond and Mazumder, 2013; Buckles and Kolka, 2014; Biroli et al., 2018). We finally add to a more specialized literature on the determinants of vaccination compliance (Philipson, 1996; Geoffard and Philipson, 1997; Tickner et al., 2006; Grabenstein, 2013; Larson et al., 2016; Amin et al., 2017; Chang, 2018; Karing, 2018; Oster, 2018; Hansen and Schmidtblaicher, 2019) and the effect of policies designed to increase compliance: pro-vaccination campaigns (Chanel et al., 2011; Sadaf et al., 2013; Buttenheim et al., 2016), mandatory vaccination laws (Davis and Gaglia, 2005; Abrevaya and Mulligan, 2011; Holzmann and Wiedermann, 2019; Richwine et al., 2019) and reminders and recall systems (Vann and Szilagyi, 2005; Harvey et al., 2015; Baskin, 2018; Hirani, 2020). This literature documents that pro-vaccination campaigns have zero to modest effects, while mandatory vaccination laws and reminder and recall systems increase compliance. In this paper, we add policy-relevant evidence on the impact of "human reminders", i.e. the impact of personal dialogue with health professionals on timely vaccination compliance.

An interesting question emerging from our findings for immediate parental behavioral responses is whether nurses play a role in shaping longer-run parental habits, i.e. whether parents, who initially are encouraged to comply with scheduled care in a timely fashion continue on this track also beyond the vaccinations and health checks that we study. One way of analyzing this question is to consider later childhood health investments. We find limited evidence for spill-overs to other parental decisions from exposure to timely vaccination reminders during the first year of life.

The rest of the paper unfolds as follows: Section 2 presents relevant background on preventive care for new families in Denmark. Section 3 describes our data and section 4 presents our empirical framework and identifying assumptions. Section 5 presents our main results and extensions. Section 6 concludes.

2 Preventive care for new families in Denmark

In Denmark, the public health care system provides post-natal care free of charge:[10] The 98 Danish municipalities provide the postnatal NHV program. Importantly, families do not have to actively seek nurse care, as all families get the offer to have a family nurse (however, families are free to opt out). Hospitals notify the municipality of residence of the mother about her birth, i.e., for the majority of births, municipalities learn about the birth through the birth notification.[11] Once notified about the birth, all municipalities have assignment mechanisms to assign one primary nurse to each family. The assigned nurse manages the family's progress through the program in the first year of the infant's life.

All municipal programs operate within the frame set by the Danish National Board of Health (DNBH). The DNBH issues guidelines and regulations regarding the number, timing and content of nurse visits. During home visits, trained professional nurses monitor infant and maternal health, provide age-relevant information, counseling and advice, and refer families with identified risks to health professionals for potential treatment. As such, NHV consists of a basic package of services offered to all families with a newborn. Additionally, municipalities can offer supplementary services targeted at specific populations of mothers and children. Those services can include additional need-based home visits or other services.[12] In general, NHV uptake is very high—with over 95 percent of families having at least one NHV contact.

While municipalities provide NHV, GPs provide preventive health checks and the vaccination program. These programs are – as NHV – provided free of charge but require active

[10] Also pre-natal care is free in the public system: Midwives and general practitioners provide pre-natal care that consists of regular consultations during pregnancy. The universal offer consists of 4-7 midwife consultations, 3 GP consultations and 2 ultrasound scans (Sundhedsstyrelsen, 2011). Mothers with at-risk pregnancies receive additional care.

[11] At-risk families or families that have already demanded prenatal support may be known to the municipal nurse programs. The majority of uncomplicated births in Denmark are midwife-assisted in public hospitals organized around five regions. Only public, regionally-steered hospitals perform births in Denmark. A small share of around two percent of births are performed as home births with publicly provided midwife assistance. Also for these births, municipalities are notified about the birth by the hospitals (as assisting midwives are part of the public hospital system).

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

decisions by parents. The Danish preventive care schedule consists of 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. Additionally, mothers are offered one postpartum health check at their GP. The GP health checks involve a dialogue between parents and GP on age-related issues and screening for health problems (such as a monitoring of the infant's growth). In that sense, there is some overlap in the type of service that nurses and GPs provide. As illustrated in Figure 1, some of the GP contacts entail both a personal contact with the GP and a vaccination (typically administered by a nurse in the GP office).

The Danish Childhood Vaccination Program for children consists of three rounds during the first year of a child's life. The schedule recommends vaccinations at three, five and twelve months of the child's life. Each round consists of two separate vaccinations. First, a combined vaccination to immunize against diphtheria, tetanus, pertussis, polio and hib infection. Second, a pneumococcus bacteria vaccination to prevent infant meningitis. After age one, the vaccination program offers five additional vaccinations at age 15 months (measles, mumps and rubella, MMR) and at ages 4, 5, 12 years (revaccinations and the HPV vaccination). Appendix Table A1 shows the schedule of the Danish Childhood Vaccination Program.

The national preventive care programs (health checks and vaccinations provided at the GP) run parallel to the municipal NHV program. Figure 1 illustrates their respective (recommended) timing throughout the first year of life of the child. Families typically receive one to two nurse visits prior to the recommended age for the first health check at five weeks after birth.[13] The third visit is usually recommended during the third month of a child's life and thus planned to occur prior to the first vaccination round, recommended at age 3 months. The fourth nurse visit is scheduled during the fifth and sixth months of a life and coincides with the second health check and vaccination (recommended at age 5 months). Planned at age 8-10 months, the final universal nurse visit is recommended well ahead of the infant vaccination round at age 12 months.

[13] The number of visits is dependent on parity of the child and on whether the mother had an outpatient birth.

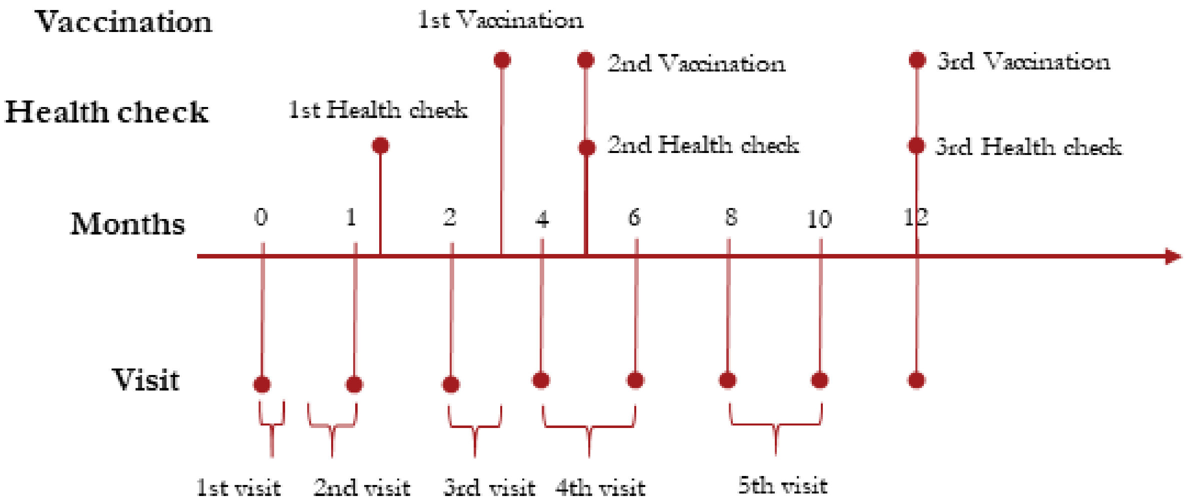


Fig. 1 Nurse visit, GP health check and vaccination schedule

Notes: The figure shows the recommended age for vaccinations, the GP preventive health checks and nurse home visits during the first year of a child's life. The recommendations are issued by the Danish National Board of Health. Nurse visits are under municipal discretion and are thus recommended in a specific period of time rather than a fixed age.

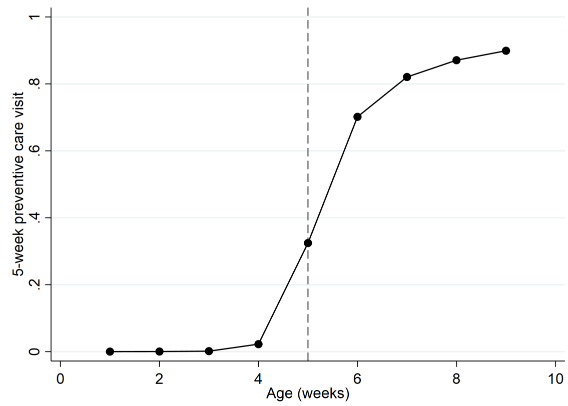
3 Data and Sample

We use data from two sources: first, we use newly compiled data on NHV in 59 out of 98 Danish municipalities. Second, we link these records to population administrative register data for all children born in these municipalities in 2012-2015 and their families.

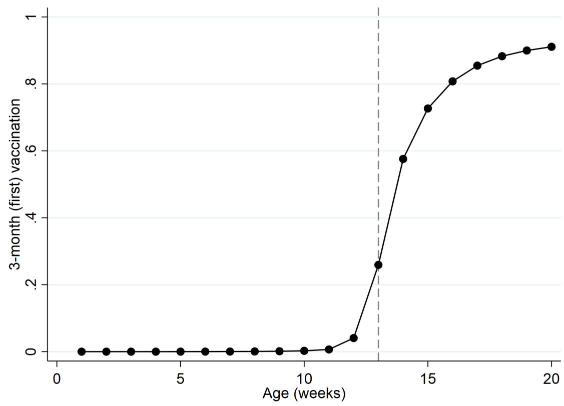
The administrative medical birth register contains information on birth outcomes (birth weight and length, gestational age, the five minute APGAR-score, hospital of birth identifiers, uptake and number of prenatal midwife contacts). We link these data to parental background characteristics lagged with two years prior to the focal child's birth (educational attainment, income, age, civil status and family links irrespective of co-residence, and municipality of residence). Finally, we use data on parent and child contacts with GPs from reimbursement data. We measure uptake of GP provided care using reimbursements to GPs, which are reported on a weekly basis.[14]

[14]While we do not observe information on the specific content of GP consultations or GP-given diagnoses,

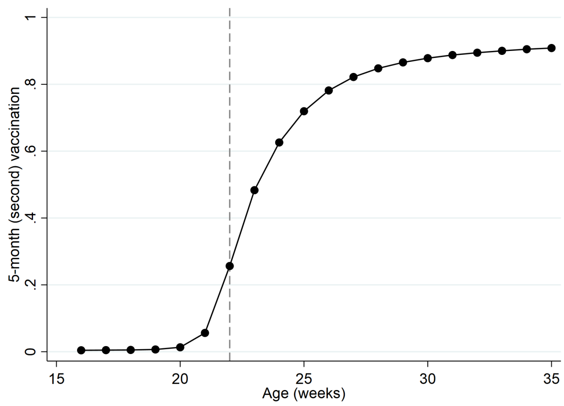
Figure 2 illustrates the uptake of GP health checks and vaccinations for the relevant cohorts.[15] Uptake rates are high in the universal preventive care programs.



(a) First GP health check, 5 weeks



(b) First vaccination, 3 months



(c) Second vaccination, 5 months

Fig. 2 Overall uptake of GP health checks and vaccinations; full sample of children 2012-2015 cohorts.
Notes: The figure shows the timely evolution of coverage rates in our analysis sample of children from 59 out of 98 Danish municipalities and the cohorts 2012-2015.

While the optimal vaccination rate differs across diseases, the herd immunity levels for measles and whooping cough are in the 90 to 95 percent range (Plans-Rubió, 2012). With

we exploit that GPs are reimbursed for different set of services related to preventive care: recommended health checks and vaccinations. Thus they have strong incentives to register those services in their records.

[15]Figures are based on administrative data for children from municipalities covered in our analyses. Figures that use data for the entire cohorts (rather than children from the municipalities with NHV data available) are equivalent.

coverage rates in Denmark at around 90 percent, herd immunity is not guaranteed for measles and whooping cough, reflected by recent recurring whooping cough outbreaks in Denmark (Dalby et al., 2019). Moreover, as expected, not all parents take up care in a timely fashion. For the first vaccination round, 30 percent of children are more then 14 days delayed (which is a significant delay given the child's young age). Furthermore, the share of delaying parents increases for later vaccination rounds with 43, 60 and 71 percent of children not receiving the second, third and fourth vaccination on time.

Using a unique personal identifier, we link the administrative data on outcomes and background characteristics to data on NHV for infants in 59 out of 98 Danish municipalities. All of these municipalities use the same registration system and have agreed to sharing their data for research purposes.[16] The data contain individual-level information on all children and parents, who had at least one contact with a municipal nurse. Appendix Figure A1 shows a map of Denmark and the municipalities covered in the analyses of this paper. Together the children born in these municipalities account for 62 percent of all children of the relevant cohorts.

While the content of the nurse record data varies over time and across municipalities, in general, the data include individual-level registrations on provided services (such as number and timing of visits, phone contacts, emails, open house contacts, allocation of mothers in mother groups), information on screening and health monitoring (maternal and paternal postnatal depression, child weight and height development, recordings of developmental problems and referrals to other providers), and a nurse identifier.[17] Even though the nurse identifier uniquely identifies nurses in a given municipality, we cannot track nurses across

[16]As of 2019, 85 municipalities use the same software for registrations in their NHV program (NOVAX). We have obtained permission to use data for children and their families from an unbalanced panel of 62 municipalities in the years 2000-2017. Our use of these data is also approved by the Danish Patient Safety Authority. The full records cover around 900,000 unique individuals (children (infants and school children), mothers, fathers) with at least one nurse contact in the period 2000-2017. We do not have data on the timing of visits for 3 out of 62 municipalities and thus omit those from the analyses.

[17]The variability of the nurse records stems from the decentralized nature of the data: Municipalities can independently decide on many aspects of their registrations. However, a number of central aspects, such as timing and number of visits, is harmonized across municipalities.

municipalities and have no other nurse background characteristics in our data. Important for our analyses, we observe the dates for each nurse visit allowing us to determine the closest nurse visit around recommended ages for uptake of preventive care.

To arrive at our final analysis data, we impose the following sample restrictions: First, we drop the approximately 10 percent of children who are hospitalized at birth for more than 7 days. These children enter the postnatal care program on different terms. In essence, we focus on a population of children with an uncomplicated birth.

Second, to arrive at our estimation samples around three preventive care episodes, we focus on children, who receive nurse visits close to the recommended age for those (at weeks five, three months and five months): Our treatment group consists of families receiving a nurse visit in the week of the recommended timing for the vaccination/the health check. The control group consists of families receiving the visit in the following two week period. Importantly, families can have other nurse visits (with a larger spacing) around the given care episodes. Third, as we exploit within-nurse variation, we omit the one percent of families, who are assigned to nurses with only one family in our sample.[18]

Finally, we divide our sample into universal and high impact samples, which are defined by the type of the closest-spaced nurse visit the family receives around the recommended age for preventive care: Families either have a universal or a need-based visit as their closest visit around the recommended date for preventive care. This distinction allows us to separately consider the impact of nurse visits for a general population of new families and for families that are identified by nurses as requiring additional support (need-based visits).

Table 1 gives an overview on general NHV coverage in the cohorts that we study and the samples that we use in our estimations (families with any nurse visits around the recommended ages of the three preventive care episodes). Out of the 143,760 children in our 59 municipalities and the cohorts 2012-2015, almost 90 percent receive an initial nurse visit. Coverage with the nurse program remains high at above 70 percent for the last (and least

[18]Omitting this constrain leaves our results unchanged.

attended) universal visit around eight months after birth.[19]

Table 1 NHV in the 2012-2015 cohorts and coverage of the estimation samples

	Mean	
Nurse visit coverage:		
Initial visit	0.89	127891
Two months visit	0.84	120828
Four months visit	0.73	105113
Eight months visit	0.72	103702
Nurse with only one family (omitted from sample):		
In 5-week GP sample	0.01	1285
In 3-month vac. sample	0.01	1209
In 5-month vac. and GP	0.01	1309
Sample: 5-week GP	0.28	39984
Sample: 3-month vac.	0.16	23128
Sample: 5-month vac. and GP	0.13	18180
Observations	143760	

Notes: The full sample includes children born in 2012-2015 and resident in municipalities with data coverage for NHV (59 municipalities). The top panel of the figure shows the share of children of these cohorts, who have a nurse visit around the recommended NHV age. The bottom panel shows the share of the full cohorts, who are covered in our estimation sample: those have a nurse visit in the three weeks around the recommended age for the three episodes of preventive care considered in the paper.

In the bottom panel of Table 1, we show the share of children, who enters our analyses (i.e., children with a nurse visit of any type closely-spaced around the recommended age for preventive care). We use between 13 and 28 percent of all children from the given cohorts. As the five week GP health check is very early in the infant's life, a larger share of infants have a nurse visit in the three weeks around it. For the later episodes, variation around the recommended age is larger and thus we use a smaller share of our full cohorts. We illustrate this point further in the next section that in detail presents our methods and identifying assumptions.

[19]The numbers that we present are not dependent on having received earlier nurse visits. Families can opt in and out of visits, i.e. not all families receive all universally granted visits.

4 Empirical Methods

The ideal experiment to study how nurses affect parental investments behaviors would be to grant visits randomly. We could then compare parental investment behaviour across families. In the absence of a randomized experiment, comparing families with and without nurse visits would likely capture the impact of other, large differences across families (and given high NHV coverage, we would have a very small control group).

While we cannot identify the impact of receiving a nurse visit at all in our observational setting, we can focus our attention on another (and potentially more policy-relevant) margin: the relative timing of nurse visits. More specifically, we can exploit variation in the timing of nurse visits around the recommended age for vaccinations and health checks. Thus we compare families that all received nurse visits but – due to arbitrary factors – received those visits slightly earlier or later. As detailed below, in this setting our main assumption is that, on average and for each of the three episodes of preventive care that we study, the treatment and control groups only differ with respect to the timing of the closest nurse visit.

In our empirical analyses we perform event study estimations of parental behavior around the recommended age for preventive care. We thereby test if parents in the treatment group are more or less likely to have a timely uptake. We estimate the following equation:

$$Y_{i,t,n} = \alpha + \beta treat_{i,n} + \sum_{\tau\neq 1,\tau=2}^{\tau=T} \gamma_\tau \times I_\tau + \sum_{\tau\neq 1,\tau=2}^{\tau=T} \omega_\tau \times I_\tau \times treat_{i,n} + \delta X_{i,n} + \mu_n + \eta_{i,t,n} \quad (1)$$

In equation (1), $y_{i,t,n}$ is the outcome of interest (e.g., an indicator for whether the child has received a vaccination in the given week) for child i measured at week t after birth and assigned to nurse n. The indicator $treat_{i,n}$ is equal to one if the child belongs to the treatment group. I_τ are a set of indicators for weeks of life around the recommended week. γ_τ thus control for the general trend in parental behavior across weeks of life. $X_{i,n}$ is a vector of pre-determined covariates, including parental employment, educational attainment, student status, age, marital status and parental cohabitation, the child's sex, birth weight, an indicator for preterm birth, birth location (hospital vs. home birth), days admitted at hospital after birth, and birth mode (c-section delivery). μ_n is a nurse fixed effect and thus we compare outcomes across families assigned to the same nurse.[20] The parameters of interest are ω_τ, which relate to the weekly indicators interacted with treatment status and estimate the difference in parental behavior between treatment and control group.

Identifying assumptions The identifying assumption in our analysis is that the exact timing of nurse visits in the three week period that we consider (around the recommended ages for the separate episodes of preventive care) is arbitrary. In other words, we assume that the timing of the closest-spaced nurse visits in that period is uncorrelated with other determinants of outcomes (actual uptake of care).

Figure 3 illustrates the distribution of the closest-spaced nurse visit (irrespective of type) around the recommended ages for the preventive care episodes for all families. All figures have a mass point around the most typical age for a universal nurse visit. However, in all three panels, we can see considerable variation for the timing of the closest nurse visit. We constrain our analysis sample to families with the closest nurse visit in a three week period around the vertical lines. Apart from the closest nurse visit, we allow families to have other and further spaced nurse visits (i.e.we do not constrain our main analyses to families who have no other close nurse visits).[21]

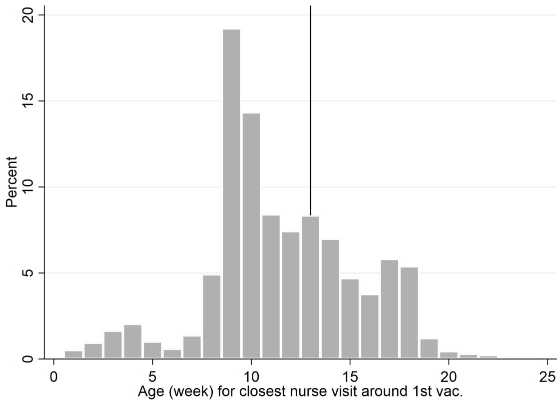
What are the sources of this variation in the timing of nurse visits that we use? Likely driving factors are the need to coordinate nurse working schedules and seasonality (relating to vacation periods or increased work load due to the seasonality in births). One key in our strategy is to focus our analyses on sample of families that receive a closest visit in a narrow window of three weeks: given this small the window, we argue that it is likely that the exact

[20] Recall that nurses are nested in municipalities.

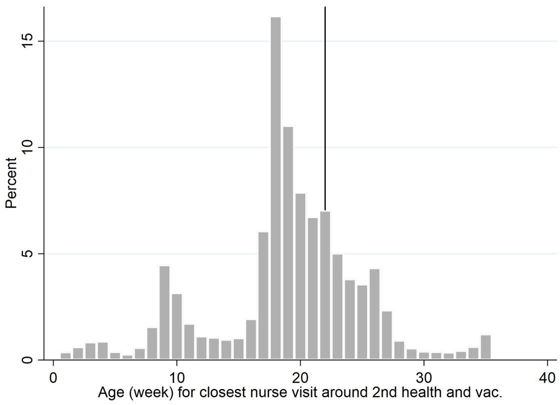
[21] As we show in the robustness section, excluding families with several visits around the timing of preventive care makes our results more pronounced indicating that our main analyses (allowing for other nurse visits around the relevant time) is a conservative approach.

timing of visits is somewhat arbitrary.[22]

(a) First GP health check



(b) First vaccination



(c) Second vaccination/health check

Fig. 3 Distribution of nurse home visits around the recommended dates for preventive care and vaccinations; full sample.

Notes: The figure shows the percentage of families with the closest nurse (both universal and targeted) visit around recommended ages for three preventive care episodes. Each bin represents one week. Vertical lines indicate the recommended age (week) to receive the health check/vaccination.

In support of this claim, Table 2 (and Appendix Tables A2 and A3) present summary statistics for a broad range of family characteristics for two groups (treated and control) in our universal and high impact samples.

[22]While larger time windows may make families less comparable, a larger window increases the number of observations and power in the estimates. Thus we face a trade-off between validity and precision. In the robustness section, we test the sensitivity to changes in the window and in the main specification we choose a window of one week for treatment group and two weeks for the control group. This choice is fairly conservative and mainly chosen to make the treatment and control group equally sized.

Table 2 Variable means, treated and control group in the universal and high impact samples

	Universal sample		High impact sample	
	Treatment	Control	Treatment	Control
	Mean	Mean	Mean	Mean
Female, child	0.49	0.48	0.51	0.49
First-born	0.50	0.52	0.55	0.54
Low birth weight	0.02	0.02	0.03	0.04
Preterm birth	0.03	0.03	0.04	0.04
C-section	0.22	0.21	0.23	0.23
No. of hospital nights at birth, child	2.25	2.27	2.53	2.49
Same-day discharge	0.38	0.37	0.31	0.32
Young mother	0.05	0.05	0.09	0.08
Missing birth obs.	0.02	0.02	0.03	0.03
Danish, mother	0.81	0.79	0.81	0.81
Student, mother	0.07	0.07	0.07	0.08
Prim. school, mother	0.18	0.18	0.26	0.26
Higher educ., mother	0.24	0.22	0.20	0.20
Uni. degree, mother	0.19	0.16	0.13	0.13
Employed, mother	0.72	0.70	0.65	0.64
Missing employment obs., mother	0.03	0.04	0.04	0.04
Missing educ. obs., mother	0.09	0.11	0.10	0.10
Student, father	0.04	0.04	0.04	0.04
Prim. school, father	0.18	0.20	0.24	0.25
Higher educ., father	0.17	0.16	0.13	0.13
Uni. degree, father	0.17	0.15	0.12	0.12
Employed, father	0.80	0.78	0.75	0.75
Missing employment obs., father	0.05	0.05	0.06	0.06
Missing educ. obs., father	0.09	0.10	0.10	0.10
Cohabiting	0.72	0.72	0.66	0.67
Missing cohab. obs.	0.01	0.02	0.02	0.02
Parents educ. in health and childcare	0.13	0.12	0.11	0.11
Pregnancy nurse visit	0.11	0.11	0.21	0.21
Week for visit	13.00	14.46	13.00	14.38
Total no. of visits	7.35	7.29	9.84	9.54
No. of uni. visits	5.72	5.67	4.93	4.92
No. of targeted visits	1.76	1.66	5.08	4.75
Referred by nurse	0.04	0.04	0.07	0.07
No. of nurses	1.63	1.61	1.68	1.68
No. of visits by assigned nurse	7.25	7.36	9.65	9.59
Observations	3291	4625	6309	8903

Notes: The treated and control groups in each sample include children of the cohorts 2012-2015, who have a closely spaced nurse visit around the recommended age for the uptake of preventive care at three months (vaccination). The data in the top panel comes from administrative register data, the data in the bottom panel comes from nurse records.

As for the comparison across the universal and high impact sample, the table shows that, as expected, nurses direct need-based visits to a non-random subset of families: Families in the high-impact sample are more likely to only have one child, mothers are more likely to be classified as young and have a much higher probability of only having completed compulsory schooling. Parents are less likely to cohabit two years prior to the birth, and mothers and fathers are less likely to have been employed. The bottom panel of Table 2 shows that families in the high-impact sample have significantly more nurse visits driven by a large number of need-based visits.

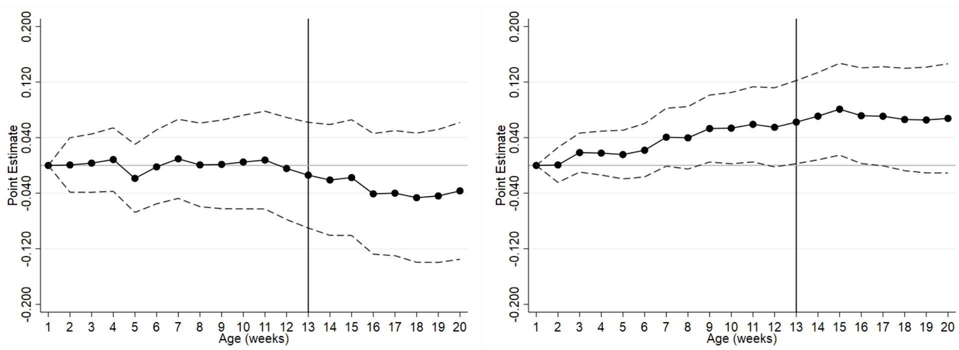
All these differences concern a comparison of families across two samples (universal vs high impact). When comparing families that we classify as treatment and control families within the two samples, we find that those are very similar. This finding lends credibility to the statement that the exact timing of nurse visits is uncorrelated with other determinants of outcomes.

Table 3 Selection into treatment and control groups

	Universal sample		
	1st GP health check	1st vac.	2nd vac. and health check
	(1)	(2)	(3)
F stat for joint significance	1.50	1.23	0.92
p-value	0.04	0.19	0.59
Observations	21382	6637	11113
	High impact sample		
	1st GP health check	1st vac.	2nd vac. and health check
F stat for joint significance	1.09	1.24	0.89
p-value	0.34	0.17	0.63
Observations	15580	13780	4552

Notes: The table shows F-statistics and corresponding p-values for joint significance in separate regressions with treatment status as dependent variable and pre-determined covariates as explanatory variables. We estimate the regression for all combinations of sample (universal vs. high impact) and preventive care period (first GP health check five weeks after birth, first vaccination three months after birth and second vaccination and GP health check five month after birth). All regressions include year-of-birth and nurse fixed effects. Standard errors are robust.

To further probe our claim that families in treated and control groups are as good as randomly assigned to their respective group, we use all available family characteristics to predict whether a family is in the treatment group. Table 3 presents a joint F-test of all family characteristics (conditional on year-of-birth and nurse fixed effects), suggesting that for all three episodes of preventive care (with the exception of the first care episode in the universal sample), we cannot reject the null hypothesis that these characteristics do not jointly predict treatment status. Recall also, that in all main regressions we control for all control variables listed in Table 2.



(a) Care episode: 1st vac. (Universal sample)

(b) Care episode: 1st vac. (High impact sample)

Fig. 4 Common trend assumption for the second preventive care period (first vaccination at age 3 month): Trends in uptake of ordinary (non-preventive) GP care.

Notes: The figures show event study estimates and confidence intervals. The outcome variable is the number of GP visits (excluding preventive care). The dots show the estimated differences in uptake at each week from birth to week 20 between treatment and control group with the first week of life as reference week. The treatment groups consist of children born between 2012 and 2015, who received a nurse visit during the week of the recommended age for each care episode. The control groups consist of children who received a nurse visit during a two week period after. The regression controls for a set of pre-determined covariates and year-of-birth and nurse fixed effects. Standard errors are robust.

A final way to assess whether the treatment and control groups are comparable is to assess whether they follow a common trend with respect to health care utilization prior to the time of treatment. Specifically, we estimate Equation (1) with the number of (ordinary, not preventive) GP contacts prior to the nurse visit as outcome. Figure 4 presents event study estimates and confidence intervals for the comparison of treated and control families around the first vaccination for both the universal and high impact sample.[23] There are no differences in pre-treatment GP contacts between the treatment and control group.

[23] Appendix Figure A2 presents similar figures for the other two episodes that we consider.

5 Results and Robustness

This section presents our results for the impact of closely-spaced nurse visits on parental uptake of i) the five week GP health check, ii) the three month vaccination, and iii) the five months GP health check and vaccination.[24] For each analysis, we construct a separate treatment and control group as described in Section 4. In other words, families can enter the treatment group for our analysis of the first GP health check, but enter the control group for the first vaccination, depending on the respective timing of their nurse visits. In the first part of the analysis we study the immediate effects of nurse visits on parental behavior around the three mentioned episodes. In a second step, we move to analyses that explore whether families form habits of timely adherence to preventive care in the longer-run.

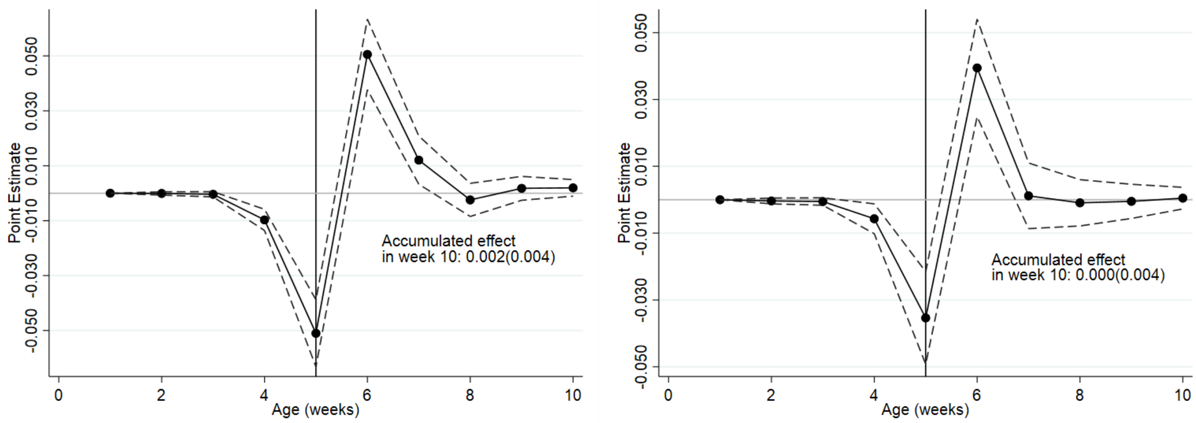
5.1 Main Results

Figures 5 to 7 present graphically our main results for the impact of closely-spaced nurse visits around three episodes of preventive care in the first year of the child's life. The left panels are based on the sample of families with a universal home visit close to the recommended age for preventive care. The right panels focus on families with closely-spaced targeted (need-based) home visits. As discussed earlier, these groups of parents represent a more general and a higher-risk sample respectively, with the latter identified by the families' nurses.

Figure 5 shows event study estimates for the difference in weekly uptake of GP preventive care for the treatment and control group defined around week five (the first care episode). Parents, who receive a nurse home visit in week five are less likely to complete the GP health check in that week. This finding holds in both samples and may indicate that parents substitute contacts to health professionals. At the control mean of 32 percent, treated families are 10-15 percent less likely to have completed the five week GP check up than control families with a later home visit in week five. However, in week six the treatment families are more

[24] While there is also a 12 months health check and vaccination, the final planned nurse visit is around eight months of the child's life and thus we are not able to exploit similar variation for this episode (only 2 percent of children have what we define as a closely spaced nurse visit around 12 months).

likely to attend the GP health check. The effects in week five and week six exactly cancel out each other. This pattern indicates that the delay of the nurse visit around the care episode only impacts the timing of the uptake of the first GP health check. However, the difference in accumulated uptake in week ten between the groups is zero. The figures show a response of treated families already in week four (with a lower probability of completing the GP visit in that week). This finding may further support that parents attempt to distribute contacts to primary health care providers (nurses and GPs) more evenly across weeks.[25]



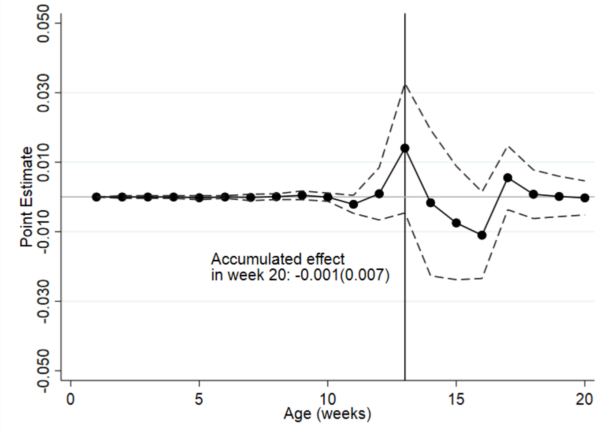
(a) First GP health check, universal sample **(b)** First GP health check, high impact sample

Fig. 5 Uptake of preventive care; event study graphs for the 5-week preventive care episode
*Notes:*The figure shows event study estimates and confidence intervals for universal sample (panel (a)) and the high impact sample (panel (b)). The dots show the estimated differences in uptake at each week from birth to week ten between treatment and control group with the first week of life as reference week. The treatment group consists of children born between 2012 and 2015, who received a nurse visit during the fifth week of life (the week where the first GP health check is recommended). The control group consists of children who received a nurse visit during a two week period after the fifth week of life. The regression controls for a set of pre-determined covariates and year-of-birth and nurse fixed effects. Standard errors are robust and the dashed lines show 95 percent confidence intervals.

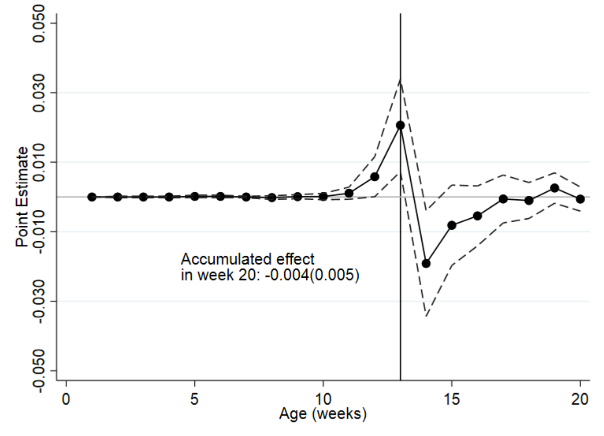
Figure 6 presents event study estimates for the second preventive care episode: the first vaccination round recommended three month after birth. As opposed to our finding for the first GP health check, (in both the universal and high impact sample) we find that treated families have a higher probability of timely adherence. The effects are especially

[25]In the very first weeks of the child's life contacts with nurses and GPs are by definition very closely spaced. We therefore may expect other reactions for later episodes where primary health care contacts are less frequent.

large and significant in the high impact sample where treated families have a 10 percent higher probability of timely adherence (relative to the control mean at 21 percent). As the uptake rate converges between when the control group families receive their nurse visit, the evidence show that nurses act as human reminders.



(a) First vaccination, universal sample

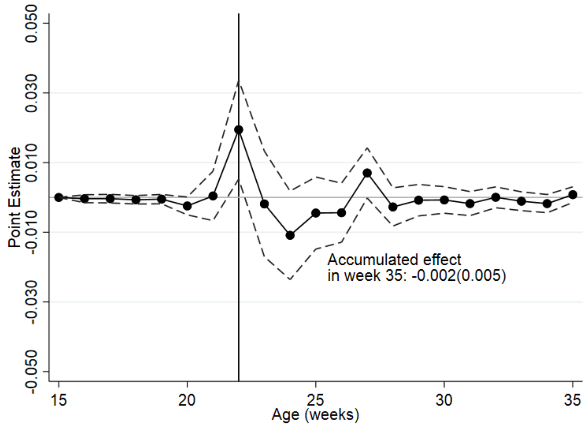


(b) First vaccination, high impact sample

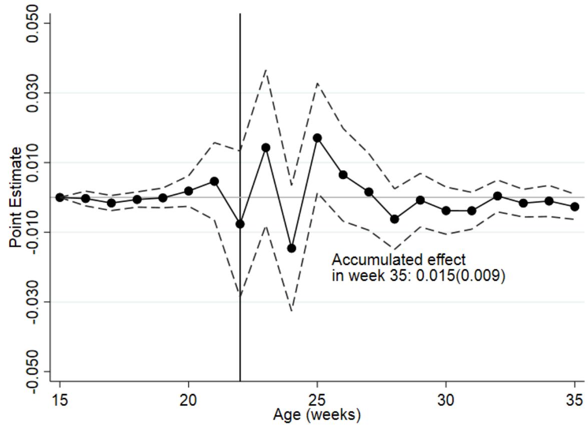
Fig. 6 Uptake of preventive care; event study graphs for the first vaccination episode 3 month after birth

Notes: The figure shows event study estimates and confidence intervals for universal sample(panel (a)) and the high impact sample (panel (b)). The dots show the estimated differences in uptake at each week from birth to week 20 between treatment and control group with the first week of life as reference week. The treatment group consists of children born between 2012 and 2015 who received a nurse visit during the 13th week of life (the week where the first vaccination is recommended). The control group consists of children who received a nurse visit during a two week period after the 13th week of life. For additional details, see notes for Figure 5.

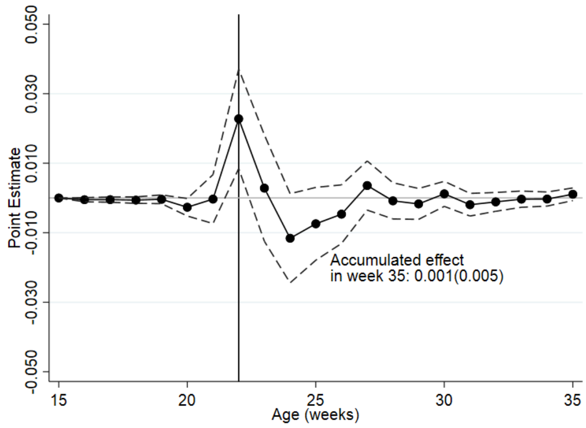
Figure 7 presents results for the third care episode (combined GP health check and vaccination) within the first year of a child's life (recommended at 22 weeks). Similar to the second care episode, receiving a nurse visit during the recommended week to take up the health check and the vaccination, increases the probability of timely adherence. The positive effect on timely adherence is somewhat stronger in the universal sample and for both the GP health check and the vaccination (which are typically combined in one physical visit to the GP office).



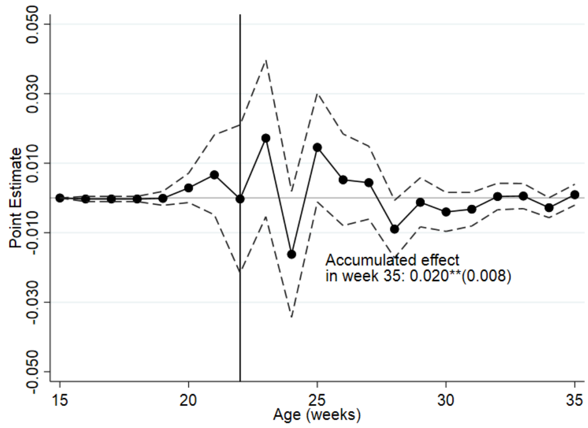
(a) Second vaccination, universal sample



(b) Second vaccination, high impact sample



(c) Second GP health check, universal sample



(d) Second GP health check, high impact sample

Fig. 7 Uptake of preventive care; event study graphs for the second vaccination/GP health check 5 month after birth.

Notes: The figure shows event study estimates and confidence intervals for universal sample(panel (a)) and the high impact sample (panel (b)). The dots show the estimated differences in uptake at each week from week 15 to 35 after birth between treatment and control group with week 15 as reference week. The treatment group consists of children born between 2012 and 2015 who received a nurse visit during the 22th week of life (the week where the second vaccination and GP health check is recommended). The control group consists of children who received a nurse visit during a two week period after the 22th week of life. For additional details, see notes for Figure 5.

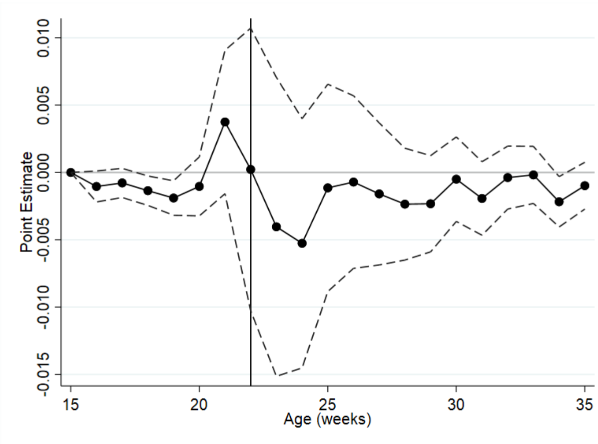
The evidence across all three episodes suggests that nurses remind parents about the upcoming care episodes. Thus, receiving a nurse visit at the recommended age for preventive care, increases timely adherence. The exception of the first care episode scheduled five week after birth, where parents appear to substitute between nurse care and the GP health check, may indicate that parents attempt to more evenly spread out contacts with primary health professionals. For all figures, differences across treatment and control groups go to zero after an initial difference induced by the timing of nurse visits. This finding suggests that the timing of nurse visits does not convince reluctant parents to take up vaccinations, but rather than that reminds parents about uptake in our sample of parents. Note however that we compare families that differ in the timing of nurse visits in a narrow window implying that this finding may not generalize to parents who do not receive nurse visits or receive them with larger spacing.

Do timely nurse visits only affect the uptake of preventive care right at the given time by reminding parents or do they also affect habits in the longer-run? By analyzing the impact of timely nurse visits around the first vaccination on later vaccination timing, we explore whether nurses only serve as reminders for a specific vaccination or whether they affect (timely) adherence in future vaccination.[26]

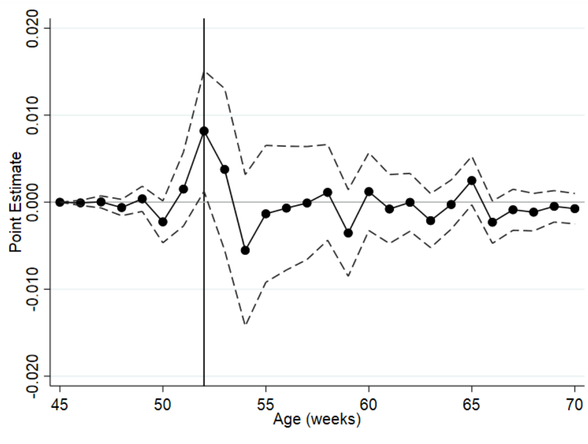
Figure 8 shows event study estimates for the differences between treatment and control group (defined in terms of their nurse visit at the first vaccination round at three months) with respect to uptake of the second vaccination at age five months (panel (a)), third vaccination at age 12 months (panel (b)) and fourth (MMR) vaccination at age 15 months (panel (c)). The graphs suggest a weak effect on timely adherence at later vaccination rounds. In panel (a) we see a weak insignificant effect on timely adherence, in panel(b) (the third vaccination round recommended at age 1) the effect on timely adherence is larger (11 percent at the control mean of 7 percent) and significant at 5 percent confidence levels, while in panel (c) (the MMR vaccination at age 15 months) we observe no clear pattern. A possible explanation could be that close to the second vaccination most families have a nurse visit, while around the third vaccination no universal nurse visit is scheduled suggesting a larger role of habits in the uptake of timely adherence. Furthermore, we do not find any effects on future vaccination uptake when treatment and control groups are defined in terms of the third care episode

[26]Thus we estimate event study regressions for the samples around the second and third care episode while using uptake of future vaccinations as outcomes.

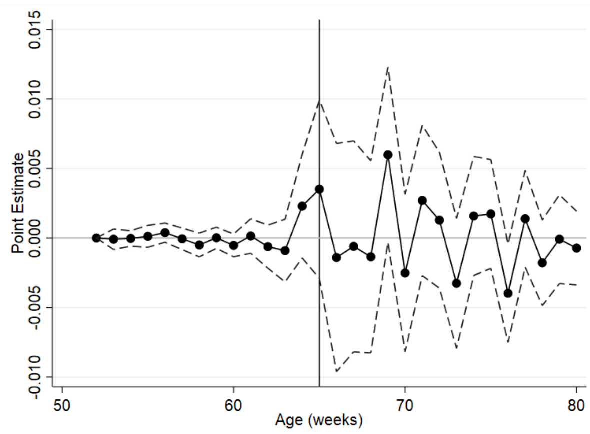
(results available on request). This finding suggests that vaccination habits form early in the vaccination program.



(a) 5 month vaccination



(b) 12 month vaccination



(c) 15 months (MMR) vaccination

Fig. 8 Future uptake of vaccinations; event study graphs for longer-run vaccination adherence
Notes: The figure shows event study estimates and confidence intervals for the universal sample. The treatment group consists of children born between 2012 and 2015 who received a nurse visit during the 13th week of life (the week where the first vaccination is recommended). The control group consists of children who received a nurse visit during a two week period after the 13th week of life. For additional details, see notes for Figure 5.

In sum, our findings indicate that nurses may play a role in the formation of habits around timely vaccination adherence. However, the conservative definition of our treatment and control groups – who receive nurse visits in a total of three weeks around the vaccination at three months – likely plays a role for our results. Future work should reconsider alternative designs for a study of the impact of nurses on longer-run parental behaviors.

5.2 Robustness

Our main results are robust to a range of modifications of our sample and main specification, as well as a placebo test. We consider three changes to our definition of the analysis sample: First, we increase the data window around each care episode by one week on either side of the episode to increase sample size (but we may introduce bias if families are less comparable as a consequence). Second, we include children with hospital stays for more than seven days after birth. Third, we constrain our sample to families, who only have the closely spaced nurse visit but no other nurse visits around the timing of the preventive care episode. In terms of alternative specifications, we consider two alternatives: first, we exclude covariates, and second, we replace nurse fixed effects by municipality fixed effects. This final change allows some of the variation in timing of nurse visits to come from differences across nurses (which may be correlated to nurses' quality or their emphasis on promoting timely vaccination uptake). For brevity, we only present results for the universal sample.[27] Furthermore, as results for changes of our main specification are virtually identical to our main results, we only graphically show the robustness analyses for changes to the analysis sample.

Appendix Figures A3 and A4 present a graphical overview on our results based on sample changes. In general, the main patterns in our results are robust to the considered changes. Some of our robustness tests point to longer-run impacts of timely nurse visits: For the first care episode, considering alternative samples leads to results that confirm initial substitution of the first GP health check but also point to a higher uptake in the weeks after for the treatment group: The estimates for the substitution in week five and "catch-up" in week six do not cancel out and thus point to a higher share of treated children actually taking up the GP health check.[28]

[27] Results for the high-impact sample are very similar and available on request.

[28] In a graph illustrating the accumulated uptake over time, the difference between the treated and control group does not go to zero in the longer run.

Similarly, constraining our analysis sample to families with no other nurse visits in a four week period around the recommended age for preventive care (reducing sample size and likely focusing on a more selected subset of families) leads to more pronounced effects around the first and second care episode where multiple nurse visits are more likely (for both the treatment and control group). This finding (of stronger effects if we condition on only having the focal nurse visit and no other visits) is even explicit in the high impact sample.[29]

Finally, Appendix Figure A5 presents the results from a randomization exercise in the universal sample for all three care episodes: We randomize the timing of the closest universal nurse visit among families, generate new treatment and control groups and re-estimate the event study regressions. We repeat this procedure 200 times and plot the mean placebo estimates, 95 percent confidence intervals from the placebo distributions, and the true estimates. The mean placebo estimates are close to zero and the placebo confidence intervals show that the placebo estimates are distributed around zero.

In sum, our main results are robust to reasonable changes to sample and specification. Some of our reasonable changes carefully suggest that there may be longer-run impacts of timely nurse visits. In any case, our results differ significantly from estimates obtained in a randomization exercise, thereby lending credibility to our research design.

6 Conclusion

This paper has studied the impact of home visits by trained nurses on parental health behaviors measured as timely uptake of recommended vaccinations and GP preventive health checks. Given that preventive care includes screening and aims at immunization of infants at an early age, encouraging adequate (timely) uptake is a central concern of policy makers. We exploit variation in the exact timing of nurse visits around the recommended ages for GP-provided preventive care. We show that families increase timely uptake of vaccinations in

[29] Also in this case a comparison between the parental response in the week of the nurse visit and the following visits indicates that the two effect estimates do not cancel out and thus that there may be longer run impacts of timely nurse visits.

response to a nurse visit – but they also appear to substitute personal contacts with primary health care providers (GPs) with nurse care and thus delay preventive GP heath checks.

There are several alleys for potential future research: First, future work should explore further whether nurses – by impacting parental habits – affect longer-run behaviors and thus may have benefits that extend over and above immediate vaccination uptake. Second, research should consider other margins of nurse decisions (such as timely provision of screening for maternal postpartum mental health problems) that may have lasting impacts on both parental investment behaviors and child and family health.

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A Appendix

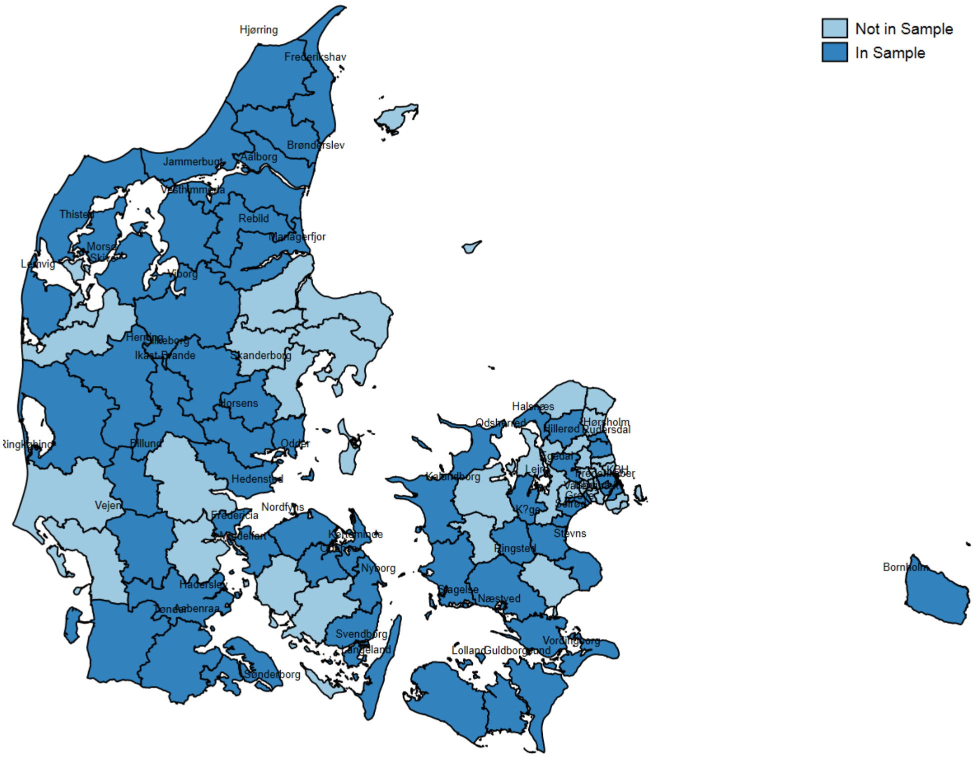
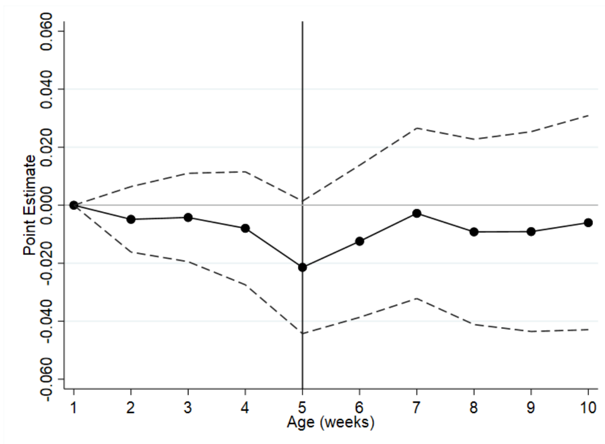
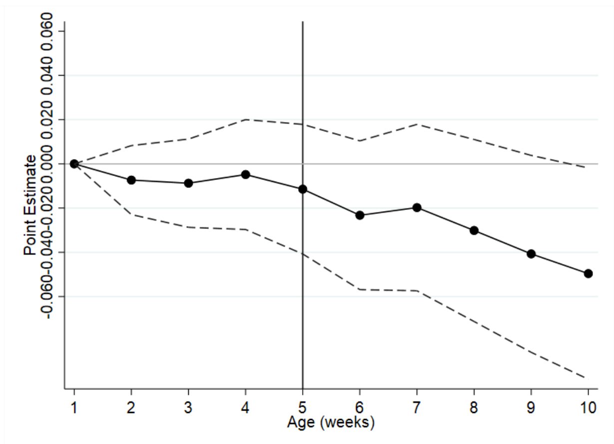


Fig. A1 Analysis sample of municipalities.

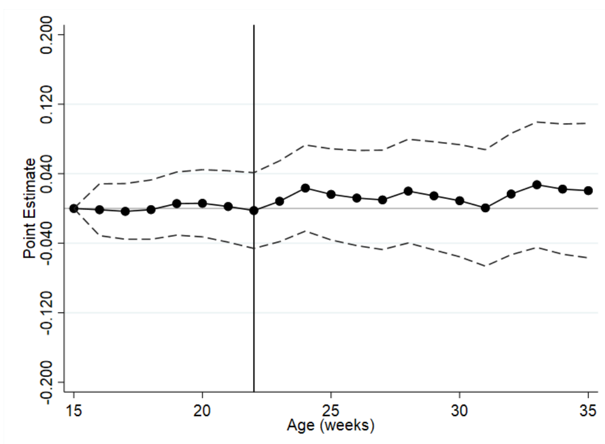
Notes: The figure shows a map of Denmark. Municipalities marked in dark blue are in our sample while municipalities in light blue are not.



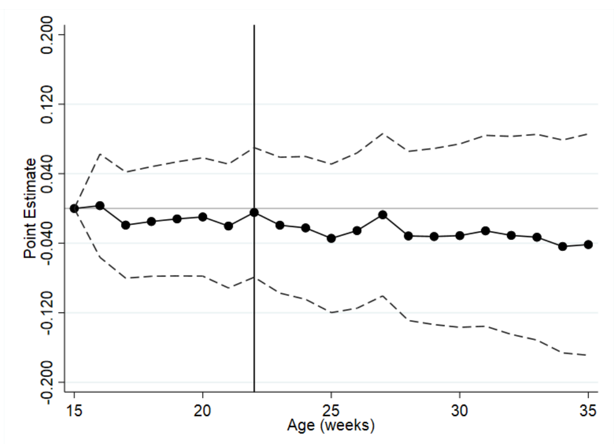
(a) Care episode: 1st GP health check (Universal sample)



(b) Care episode: 1st GP health check (High impact sample)



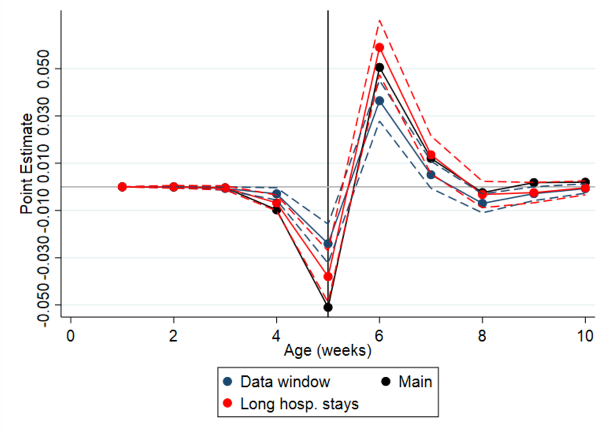
(c) Care episode: 2nd GP health check and vac. (Universal sample)



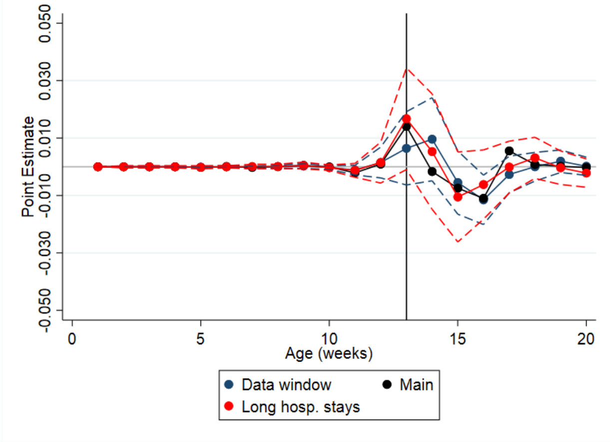
(d) Care episode: 2nd GP health check and vac. (High impact sample)

Fig. A2 Common trend assumption for the first and third care episode

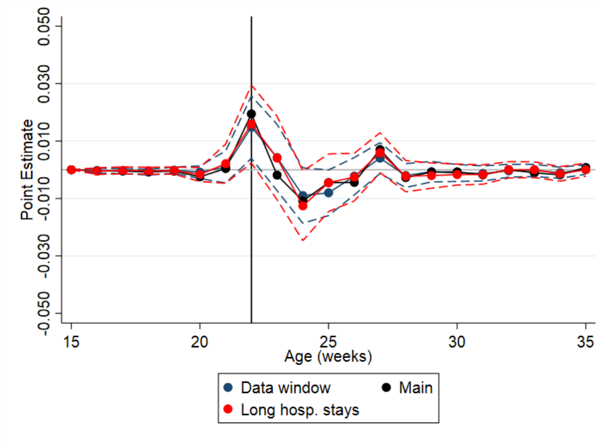
Notes: See notes to Figure 4.



(a) Care episode: 1st GP health check



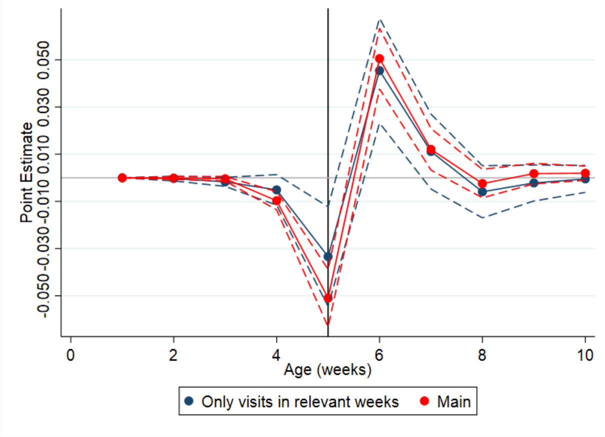
(b) Care episode: 1st vac.



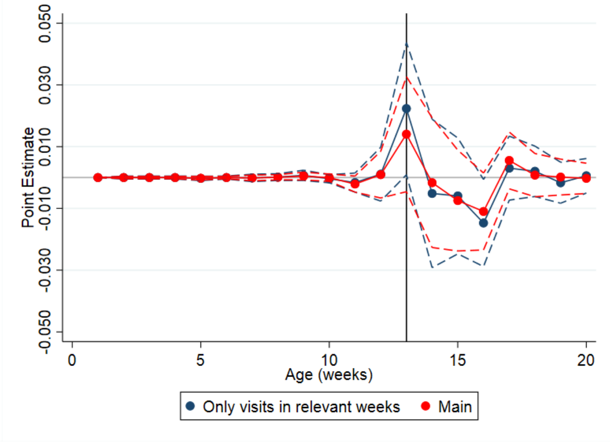
(c) Care episode: 2nd GP health check and vac.

Fig. A3 Robustness tests for the universal sample: Alternative samples

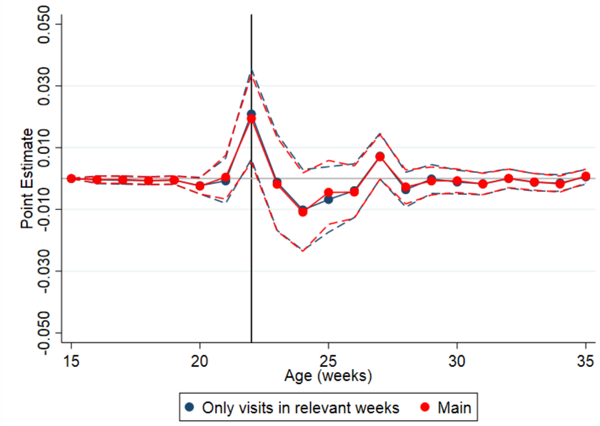
Notes: The figure shows event study estimates for two alternative ways of constraining our main universal sample at all three care episodes. We increase the data window by one week at either side of the care episode and we include children with longer hospital stays than a week during birth (excluded from the main analysis). Dashed lines are 95 percent confidence bans which are shown for the two alternative samples.



(a) Care episode: 1st GP health check



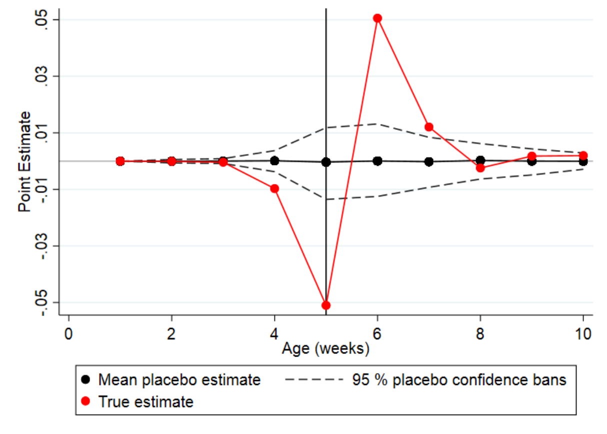
(b) Care episode: 1st vac.



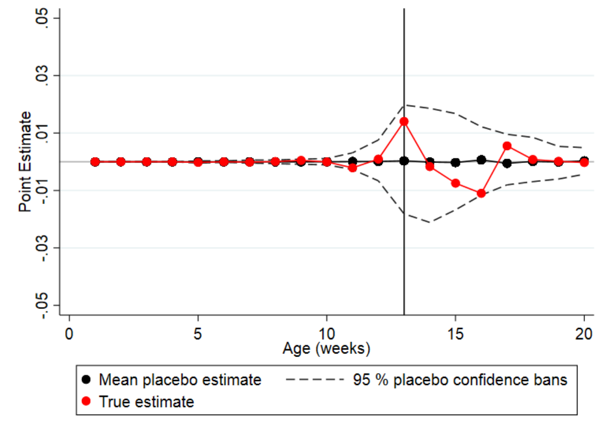
(c) Care episode: 2nd GP health check and vac.

Fig. A4 Robustness tests for the universal sample: Removal of families with other nurse visits in weeks in close promixity to the focal visit

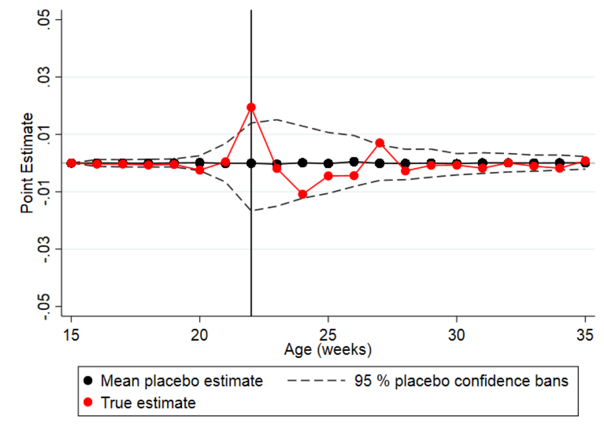
Notes: The figure shows event study estimates for a sample of families, who only have one nurse visit in a seven week period around each care episode and the main specification for the universal sample and all three care episodes. Dashed lines are 95 percent confidence bans. The sample for the first care episode contains 7,410 families (as compared to 22,896 families in the main sample). The sample for the second care episode contains 6,156 families (as compared to 7,916 families in the main sample). The sample for the third care episode contains 11,732 families (as compared to 12,384 families in the main sample).



(a) Care episode: 1st GP health check



(b) Care episode: 1st vac.



(c) Care episode: 2nd GP health check and vac.

Fig. A5 Random placebo tests for the universal sample

Notes: The figures show results for random placebo tests in the universal sample for all three care episodes. We randomize the week for the closest nurse visit, generate placebo treatment and control groups and reestimate the event studies 200 times. The figures plot mean placebo effects (solid black dots and lines), 2.5 and 97.5 percentiles in the distributions of placebo effects (dashed black lines) and the true effects (red).

Table A1 Schedule of the Danish Childhood Vaccination Program

		- - - *Round and Age* - - -							
		1st 3 mth.	2nd 5 mth.	3rd 12 mth.	4th 15 mth.	5th 4yr.	6th 5yr.	7th 12yr.	8th 12yr., 5 mth.
	Infant vaccinations (Below age 2)								
(1)	Dipheteria-tetanus-pertussis-polio-Hib	✓	✓	✓					
(2)	Pneumococcus	✓	✓	✓					
(3)	MMR				✓				
	Later vaccinations (Above age 2)								
(4)	MMR					✓			
(5)	Diphteria, tetanus, pertussis and polio						✓		
(6)	HPV for women							✓	✓

Notes: The table illustrates the schedule of the Danish Childhood Vaccination Program. *Source*: The Danish Board of Health [Sundhedsstyrelsen] (2016).

Table A2 Variable means, treated and control samples (universal and high impact): First GP preventive care visit

	Universal sample		High impact sample	
	Treatment Mean	Control Mean	Treatment Mean	Control Mean
Female, child	0.49	0.49	0.49	0.50
First-born	0.46	0.46	0.59	0.57
Low birth weight	0.02	0.02	0.04	0.04
Preterm birth	0.03	0.03	0.05	0.05
C-section	0.20	0.21	0.25	0.24
No. of hospital nights at birth, child	2.10	2.11	2.62	2.57
Same-day discharge	0.43	0.41	0.28	0.30
Young mother	0.04	0.04	0.07	0.07
Missing birth obs.	0.03	0.03	0.02	0.02
Danish, mother	0.81	0.80	0.80	0.80
Student, mother	0.06	0.06	0.07	0.07
Prim. school, mother	0.17	0.18	0.22	0.23
Higher educ., mother	0.24	0.24	0.22	0.21
Uni. degree, mother	0.17	0.16	0.16	0.16
Employed, mother	0.72	0.72	0.68	0.67
Missing employment obs., mother	0.04	0.04	0.04	0.04
Missing educ. obs., mother	0.10	0.10	0.11	0.10
Student, father	0.03	0.03	0.04	0.04
Prim. school, father	0.19	0.19	0.22	0.23
Higher educ., father	0.16	0.16	0.15	0.14
Uni. degree, father	0.16	0.15	0.14	0.14
Employed, father	0.80	0.79	0.75	0.75
Missing employment obs., father	0.05	0.05	0.06	0.06
Missing educ. obs., father	0.09	0.09	0.10	0.10
Cohabiting	0.73	0.74	0.67	0.67
Missing cohab. obs.	0.01	0.01	0.02	0.02
Parents educ. in health and childcare	0.13	0.13	0.12	0.12
Pregnancy nurse visit	0.11	0.11	0.19	0.20
Week for visit	5.00	6.49	5.00	6.29
Total no. of visits	6.93	6.65	9.49	9.31
No. of uni. visits	5.29	5.13	4.85	4.84
No. of targeted visits	1.66	1.54	4.81	4.59
Referred by nurse	0.03	0.04	0.06	0.06
No. of nurses	1.61	1.58	1.69	1.69
No. of visits by assigned nurse	6.94	6.81	9.21	9.24
Observations	13817	9079	8998	8090

Notes: The sample includes children 41,269. The data in the top panel comes from administrative register data, the data in the bottom panel comes from nurse records.

Table A3 Variable means, treated and control samples (universal and high impact): Second vaccination and GP preventive care visit

	Universal sample		High impact sample	
	Treatment Mean	Control Mean	Treatment Mean	Control Mean
Female, child	0.49	0.49	0.50	0.48
First-born	0.55	0.52	0.51	0.52
Low birth weight	0.02	0.02	0.04	0.03
Preterm birth	0.03	0.03	0.04	0.03
C-section	0.20	0.20	0.23	0.22
No. of hospital nights at birth, child	2.16	2.17	2.53	2.49
Same-day discharge	0.37	0.38	0.33	0.33
Young mother	0.03	0.04	0.11	0.10
Missing birth obs.	0.02	0.02	0.03	0.03
Danish, mother	0.81	0.79	0.79	0.79
Student, mother	0.07	0.07	0.09	0.08
Prim. school, mother	0.12	0.13	0.32	0.31
Higher educ., mother	0.25	0.25	0.17	0.17
Uni. degree, mother	0.25	0.24	0.10	0.12
Employed, mother	0.74	0.73	0.58	0.59
Missing employment obs., mother	0.04	0.04	0.06	0.05
Missing educ. obs., mother	0.10	0.11	0.12	0.11
Student, father	0.04	0.04	0.04	0.04
Prim. school, father	0.14	0.15	0.28	0.29
Higher educ., father	0.18	0.17	0.12	0.12
Uni. degree, father	0.23	0.23	0.09	0.10
Employed, father	0.80	0.80	0.70	0.72
Missing employment obs., father	0.05	0.05	0.07	0.06
Missing educ. obs., father	0.09	0.09	0.11	0.11
Cohabiting	0.74	0.73	0.64	0.64
Missing cohab. obs.	0.01	0.01	0.02	0.02
Parents educ. in health and childcare	0.14	0.14	0.10	0.10
Pregnancy nurse visit	0.08	0.09	0.26	0.23
Week for visit	22.00	23.43	22.00	23.44
Total no. of visits	6.75	6.72	10.98	10.18
No. of uni. visits	5.22	5.13	4.80	4.81
No. of targeted visits	1.61	1.60	6.72	5.50
Referred by nurse	0.03	0.03	0.09	0.07
No. of nurses	1.69	1.66	1.74	1.71
No. of visits by assigned nurse	6.99	7.05	11.15	10.17
Observations	5825	6559	2281	3515

Notes: The sample includes children 19,489. The data in the top panel comes from administrative register data, the data in the bottom panel comes from nurse records.