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ABSTRACT

Sibling Spillovers and the Choice to Get Vaccinated: Evidence from a Regression Discontinuity Design*

We investigate the effects of the introduction of a population-wide Human Papillomavirus (HPV) vaccination program on the vaccine take-up of the targeted group of 15-year-old girls and their older sisters. For identification, we rely on a regression discontinuity design and high-quality Danish administrative data to exploit that date of birth determines program eligibility. We find that the program increased the HPV vaccine take-up of both the targeted girls and their older sisters. While the direct effects of the program reduced vaccine-takeup inequality, the spillover effects, in contrast, contributed to an increase in vaccine take-up inequality.

JEL Classification: I10, I18, I12, I14

Keywords: health investments, health behavior, peer effects, sibling spillovers, HPV, vaccine, health inequality

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1 Introduction

This paper estimates the impact of a vaccine program on the vaccine take-up of eligible and ineligible individuals. Even though vaccines are generally considered one of the most cost-effective health interventions and are estimated to prevent millions of deaths each year(Li et al., 2021; Olshansky and Hayflick, 2017; WHO, 2021) widespread hesitancy regarding vaccinations exist and has been deemed one of the greatest threats to future public health (WHO, 2019). To combat vaccine hesitancy, policymakers must understand the social and behavioral forces driving vaccine reluctance. Previous research has successfully identified several factors that impact vaccine take-up (e.g., cost-benefit, (Brilli et al., 2020); socioeconomic background, (Mullahy, 1999; Schmitz and Wübker, 2011); information, (Anderberg et al., 2011); prevalence of disease, (Philipson, 1996; Oster, 2018), prices of vaccines (Chang, 2016), recommendations (Lawler, 2017),and mandates (Carpenter and Lawler, 2019)). There is also increasing evidence that that health interventions more generally affect not only the targeted individuals but also their peer groups (e.g., Breining (2014); Al-Janabi et al. (2016); Fletcher and Marksteiner (2017); Alsan (2017); Black et al. (2020); Fadlon and Nielsen (2019); Daysal et al. (2020)). Establishing a deep understanding of if and how health interventions may spill over and affect others than the intended individuals is crucial knowledge for the designers of health care policy in their pursuit of herd immunity.

This paper studies the introduction of a national vaccination program that provided the Human Papillomavirus (HPV) vaccine free-of-charge to adolescent Danish girls.[1] HPV is the most common sexually transmitted infection in the United States (Dunne et al., 2007) and the main cause of cervical cancer (Muñoz et al., 2003). The introduction of the vaccination program provides a unique setting to investigate the extent to which the program affected not only the targeted individuals (direct effects), but also individuals not directly targeted by the program (spillover effects) and to explore the mechanisms underlying the decision to get vaccinated.

The decision to vaccinate is of particular interest, since vaccinations play a large role in preventive care, especially for children and the elderly, although the COVID-19 pandemic has demonstrated that vaccination can be of great importance for all population groups. While many individuals choose to get themselves or their children vaccinated, a non-negligible part of the population typically goes unvaccinated.[2] Failure to vaccinate poses a challenge for policy makers in public health since the main objective of childhood vaccination programs is the eradication of diseases or at least the achievement of herd immunity. Our setting allows us to study the effects of a large-scale introduction of a national vaccination program on vaccine take-up. It also allows us to overcome some of the methodological challenges inherent in studies of take-up and spillovers. The program generates plausibly exogenous variation in eligibility (and take-up) of the targeted girls that enables the use of a regression discontinuity design for the identification of direct and spillover effects.

We leverage population-level administrative data from Denmark and link several registers to obtain a data set with rich information on socioeconomic background, detailed vaccination history and family linkages. Our study takes advantage of the fact that eligibility for the HPV vaccination program was determined by date

[1] For Danish legislation concerning the introduction of the vaccination program, see *Bekendtgørelse om gratis vaccination mod visse smitsomme sygdomme m.v.* (BEK nr 903 af 05/09/2008).

[2] For the Danish Childhood Vaccination Program, compliance has traditionally been high. In 2018, child vaccination rates around age 1 were 96 and 94 percent for diphtheria-tetanus-pertussis and measles, respectively. The corresponding U.S. figures were 94 and 92 percent (OECD (2020)). For the HPV vaccine, which is newer, compliance rates were lower; in Denmark 52 percent of girls born in 2005—compared with 35 percent of women aged 18-26 in the U.S.—had received all shots of the HPV vaccine (The Danish Health Authority (2019); NCHS (2020)). Bruni et al. (2016) estimate HPV vaccination coverage for women aged 10-20 years in high-income countries to be 32.1 percent compared to 1.0 percent in low-income countries.

of birth. Girls born January 1, 1993, or later were eligible for the program, whereas girls born earlier were not. We exploit the eligibility criterion to estimate the effects of the program in a regression discontinuity framework. The first step in our analysis is to assess the direct effect of the program on the targeted girls. The program increased the HPV vaccine take-up of targeted girls by 50.5 percentage points. Heterogeneity analysis reveals that the impact of is greatest among low-income and low-education families.

Having established that the program increases the take-up of the targeted girls, we utilize the framework developed by Marbach and Hangartner (2020) to compare the characteristics of girls who get vaccinated *because* of the introduction of the program(the compliers) to those whose vaccination behavior remains unaffected by the introduction of the program (always-takers and never-takers).[3] We find that, relative to compliers always-takers tend to come from more affluent families and/or better educated families. Compared to the compliers, never-takers tend to have lower socioeconomic status (low SES: low income and higher likelihood of having only a basic education). As such, compliers tend to be middle-SES families. Ultimately, the combination of the complier analysis and our heterogeneity analysis reveals how the introduction of the program reduces socio-economic inequality in vaccine take-up for the targeted girls.

In the last step of our main analysis, we estimate the spillover effect of the program on the older sisters of the targeted girls. The older sisters are ineligible for the program. We find a robust and precisely estimated spillover effect of 4.6 percentage points, which corresponds to a take-up increase of about 30 percent for the group of older-sister group. We find evidence of a socioeconomic gradient in the spillover effect in the sense that we find larger spillover effects in high-SES families. This leads us to to conclude that while the implementation of the vaccine program reduced the SES-gap in vaccine take-up among the targeted girls, the introduction led to increased socioeconomic inequality in vaccine take-up among the older sisters.

We hypothesize that there are two main mechanisms underlying the spillover effects: the family *budget constraint* and *information* on vaccine benefits. These mechanisms are potentially difficult to disentangle, since earlier research has documented how more affluent families are more adherent to health recommendations (Oster, 2020). As we estimate small spillovers for families likely to be cash-constrained, we conclude that the cost-reduction is not the primary driver of the spillovers. We argue that information can drive the spillover effects in at least two ways: First, the information provided to families when a girl in the family becomes eligible for the vaccine may impact family vaccination decisions regarding other children in the household than the child targeted by the vaccination program. Second, peer effects in vaccination decisions may also lead to spillover effects. The latter has been put forward as a driver in related studies (Rao et al., 2011; Dahl et al., 2014). We find evidence that the program spillovers in our setting are mainly driven by the information material provided to families as part of the vaccination program. This finding is in line with previous studies that have shown that vaccination reminders increase the likelihood of vaccination within the program (Suppli et al. (2018); Hirani (2021)).

We contribute to a growing literature on within-family spillovers in health interventions (or health shocks). There is a smaller number of studies that estimate peer effects or family spillovers related to vaccine take-up. Rao et al. (2011) investigate peer effects in influenza vaccination among students at a U.S. university. They use the random assignment of students to residence halls as an exogenous variation, as some residence halls have a flu clinic while others do not. They find that the share of your friends in treated houses has a positive effect on your vaccine take-up. Using data from Japan, Itaya et al. (2018) find evidence of positive peer effects in influenza vaccine take-up; both based on geographical peer groups and households. They

[3]Here we use the IV terminology even though we do not estimate a second stage. We consider eligibility for the free vaccine as the instrument, and the HPV vaccine take-up as the treatment.

take advantage of an age-dependent eligibility threshold (age 65) to generate exogenous variation in take-up. The study closest related to ours is Bouckaert et al. (2020), who study spillovers in vaccine take-up within families of a population-based influenza vaccination program in the Netherlands. Like Itaya et al. (2018), they exploit an age-dependent eligibility threshold (age 65) to generate exogenous variation in take-up. They estimate both the direct effect on the targeted individuals and spillover effects on other family members using a regression discontinuity design. The authors find that the program increased vaccine take-up among the targeted individuals and their younger ineligible partners by 10 percentage points. They do not find a spillover to older eligible partners, however, and they find a negative spillover to adult children. The eligible individuals are notified about their eligibility and vaccine benefits in a personal invitation. The authors argue that spillover effect sizes are consistent, with the partners and children learning about the program target group, the risks of getting influenza, and the costs and benefits of vaccination. Hirani (2021) investigates the spillover effects of a reminder vaccination policy in Denmark for siblings and cousins and finds evidence of a negative effect on younger siblings take-up. Alsan (2017) finds that the older sisters of vaccine-eligible Turkish children have more favorable schooling outcomes. Also, Sato and Takasaki (2019) estimate peer effects in vaccine take-up among women in Nigeria using randomized conditional cash transfers that increased vaccine take-up. They find that the female friends of women who received the conditional cash transfers were more likely to take up the vaccine. In addition to the work on vaccine behavior, there is a growing number of related studies of within-family spillovers of health shocks at different ages (e.g., at birth, (Daysal et al., 2020; Black et al., 2020; Breining, 2014); early childhood, (Alsan, 2017); middle age, (Fadlon and Nielsen, 2019; Fletcher and Marksteiner, 2017); late adulthood, (Costa-Font et al., 2021)). The literature on within-family spillovers tends to focus on younger children, and there is little evidence of within-family spillover in response to health interventions aimed at adolescents.

We also contribute to a literature that studies the spillovers of public programs and implications for health equity. Economic research is increasingly focusing not only on the direct effects of a given program, but also derived effects such as spillover or peer effects (e.g., spillovers of medical treatment, (Daysal et al., 2020); peer effects in parental leave take-up, (Dahl et al., 2014); spillovers of retirement to spousal health, (Zang, 2020)). The availability of information regarding the potential effects of a program is crucial for policy makers to be able to weigh the costs and benefits of the program in question (Fletcher and Marksteiner, 2017). Along the same lines, understanding how and why different public programs may affect other individuals than those originally targeted by the program provides both researchers and policymakers with valuable information concerning the behavior and interaction of individual decision makers. Health inequalities in health remain substantial in many countries (see, e.g., Dahl et al. (2020), for a recent study of inequality in life expectancy and mortality inequalities in Denmark and the US), and spillover effects may play an important role, since spillover they possibly have different distributional consequences than the direct effects of a given intervention.

Further, we contribute to the existing economic research on vaccine take-up and compliance. This literature has previously tended to focus on either childhood vaccinations or influenza vaccinations while COVID-19 vaccine take-up is the focus of several current studies (e.g., Karaivanov et al. (2021)). Only a few recent exceptions have considered the take-up of the HPV vaccine. Carpenter and Lawler (2019) investigate the effects of middle school vaccination requirements and document the cross-vaccine spillover effects on HPV vaccine take-up for U.S. adolescents. Using Swedish administrative data, Chen et al. (2019) find that having a doctor or a nurse in the family increases HPV vaccine take-up along with a wide range of other favorable health outcomes. Our analysis is informative regarding the groups of individuals who are likely

to comply with a vaccination program and potential consequences for health inequalities. Such knowledge is important to identify population groups that might be appropriately targeted for additional interventions to improve vaccine compliance. While we investigate HPV vaccine take-up, our results also point to some more general lessons about vaccine decision making and health decisions more generally.

Finally, we contribute to a sparse literature on adolescent health investments. While adolescence is a critical life stage and many important health investments are made in this period, there is relatively little evidence on the causes and consequences of adolescent health investments (Carpenter and Lawler, 2019).

The outline of the paper is as follows: In Section 2, we describe the institutional framework surrounding the vaccination program in Denmark, the HPV vaccine and the epidemiology of the HPV infection. In Section 3, we outline our empirical strategy. In Section 4, we present the data used in the analyses and validity tests. In Section 5, we present our findings and discuss potential mechanisms driving the spillover effects. We discuss the implications of our results for the achievement of herd immunity and cost effectiveness in Section 6. Finally, in Section 7, we conclude.

2 Institutional Settings and HPV

2.1 The Danish Health Care Sector and the Childhood Vaccination Program

In Denmark, five local regions are responsible for providing the publicly funded health care services and managing the provision of health care. The regions are financed by national taxes but formally own and run the hospitals, and they negotiate fees with primary care providers (PCPs) in a collective bargaining agreement every third year. Every individual with a social security license is covered by the National Health Insurance. This entity ensures the free-of-charge provision of both primary and secondary health care services. The PCPs operate in small practices as private entities detached from the hospital. The PCPs act as gatekeepers to practicing specialists and the hospital sector. For children the preventive care includes child well-being visits and vaccinations through the Childhood Vaccination Program (CVP) (The Danish Health Authority, 2007).

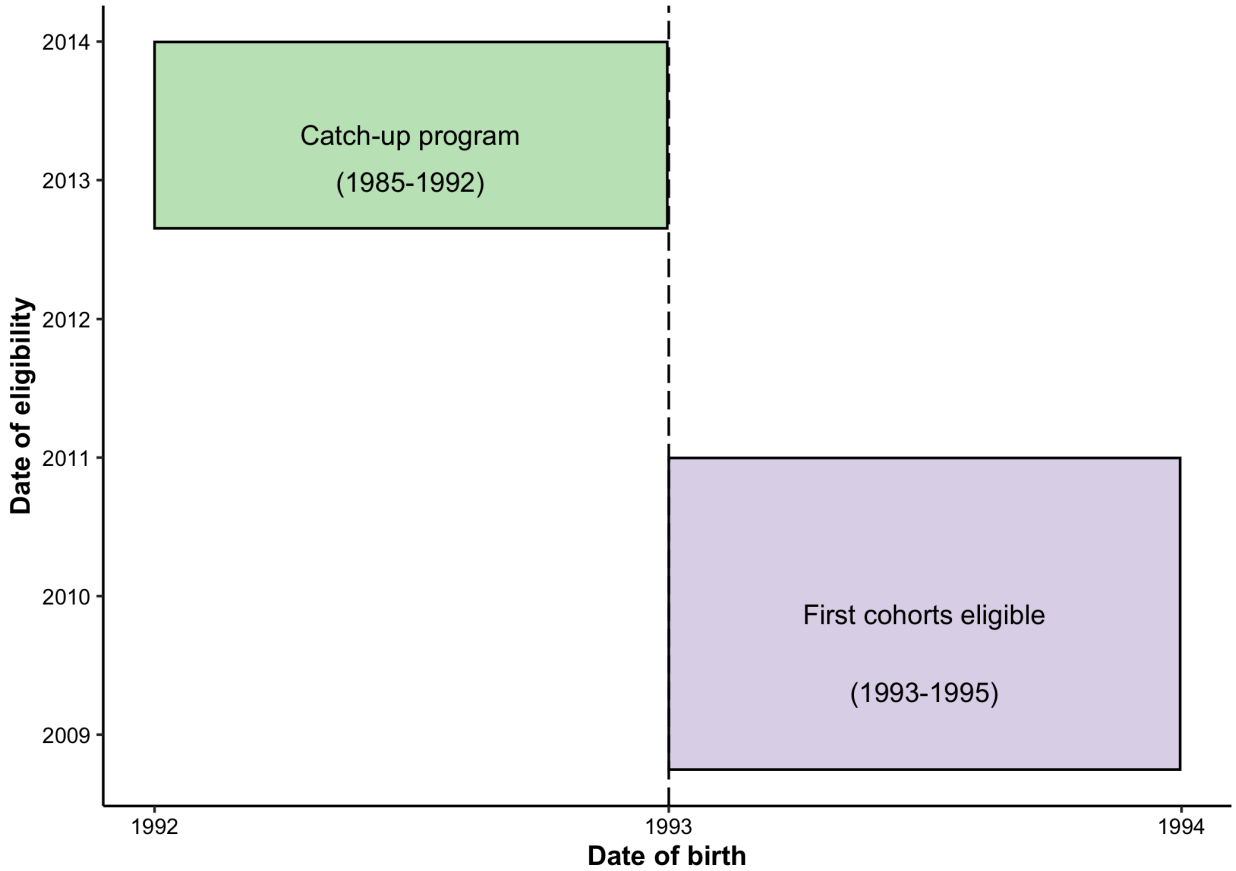
The CVP was launched in Denmark in the 1950s, now including four vaccines against ten infectious diseases. The CVP vaccinations are typically administered by the family's primary care physician free of charge. While all health authorities recommend them, they are voluntary and not required for enrollment in schools or other activities (The Danish Health Authority, 2019). Each of the four vaccines is recommended to be administered at specified ages and intervals (if applicable), most intensively up until age 2[4]. Vaccinations against frequently occurring illnesses (e.g. influenza and chickenpox) are not included in the CVP. Traditionally, the adherence to the CVP is high. Take-up rates exceed 85 percent for most of the vaccines and most participating children are vaccinated at the recommended age (The Danish Health Authority, 2019).The exception is the HPV vaccine, which, since its introduction into the CVP in 2008, has demonstrated take-up levels between 10 and 30 percentage points lower than for other vaccines (The Danish Health Authority, 2019).

[4]In broad terms vaccines are currently recommended at ages: 5 weeks (Di-Ki-Te-Pol-Hib 1), 6 months (Di-Ki-Te-Pol-Hib 2), 9 months (Di-Ki-Te-Pol-Hib 3), 15 months (MMR 1), 4 years (MMR Booster) and 5 years (Di-Ki-Te-Pol-Hib Booster) and age 12 (HPV). For children born before November 2007 the MMR Booster was recommended at age 12 as well instead of at age 4 (The Danish Health Authority, 2019).

2.2 Human Papillomavirus (HPV) and Vaccination in Denmark

Human papillomavirus is a group of highly infectious viruses that can cause several different cancers, most notably cervical cancer, which is the fourth most common form of cancer in women worldwide (Fernández de Casadevante et al., 2015). Genital HPV infections are very prevalent and primarily transmitted through sexual activity (Stanley, 2010). HPVs are highly prevalent and transmissible - according to the CDC there were 43 million HPV infections in the US in 2018, and more than 80 percent of both women and men acquire HPV by age 45 (Chesson et al., 2014; Kreisel et al., 2021). Most HPV infections are cleared by the immune system, but persistent infections of specific types of HPV can, if untreated, in some cases cause cervical and other anogenital cancers as well as genital warts. HPV infections are the sole cause of cervical cancer (Stanley, 2010; Muñoz et al., 2003), and while more than 100 different types of HPV strains exist, their risk level varies. For example, HPV types 16 and 18 are believed to account for 70 percent of all cervical cancer cases and were among the HPV types covered by the quadrivalent HPV vaccine used in the CVP in our study period (Muñoz et al., 2003). In Denmark, the incidence of cervical cancer peaks when women are in their 30s; mortality has been decreasing since the 1970s but remains relatively high (Baldur-Felskov et al., 2015; Larønningen et al., 2021).

Figure 1: Eligibility for the Free HPV Vaccine by Date of Birth (1992-1993 birth cohorts)



Notes: This figure illustrates date of eligibility for the free-of-charge HPV vaccine by date of birth. The purple box represents the vaccination program introduced in 2008 covering girls born in 1993-1995. The green box represents the catch-up vaccination program introduced in 2012 covering girls born in 1985-1992.

The first HPV vaccine (Gardasil) was made available for sale in Denmark (and the European Union,

EU) on September 20, 2006 (European Medicines Agency , EMA). The World Health Organization (WHO) suggested that girls aged 9-13 constituted the primary target group of the vaccine since it was considered to be most effective if administered prior to onset of sexual activity (WHO, 2009).

In September 2006, the HPV vaccine was introduced in Denmark. Initially, the vaccine was only available at full cost to the individual. On October 1, 2008 the vaccine was introduced in the CVP and could be obtained free of charge for girls upon turning 12. At the same time, girls born in the years 1993 to 1995 (ages 13 to 15) were also offered the vaccine free of charge until the end of 2010. In contrast, cohorts born before 1993 had to pay the full price of the vaccine (approximately 470 euros). In the summer of 2012, it was announced that as of August 27, 2012, the vaccine would be made available to girls born between 1985 and 1992 as part of a catch-up program that lasted until the end of 2013. Figure 1 shows the date of eligibility for the free-of-charge HPV vaccine by date of birth for girls born in 1992 and 1993.[5]

Prior to becoming eligible for the free HPV vaccine in October 2008, the girls in the 1993-1995 birth cohorts received a letter together with a leaflet and an appointment card to keep track of their HPV vaccinations. The material was distributed by the Danish Health Authority and addressed to both the girl and her parents. The material provided information about the vaccine, the opportunity for the girl to get the vaccine free of charge, and the prevention of cervical cancer.

3 Empirical Strategy

3.1 Direct Effects of the Vaccination Program

To identify the impact of the vaccination program on both targeted and untargeted individuals, we exploit the quasi-experimental variation in eligibility induced by the introduction of the vaccination program. For the estimation of the direct effects of the vaccination program, we focus on girls born in 1992 and 1993. In October 2008, girls born in 1993 became eligible for the free HPV vaccine through the publicly provided vaccination program while girls born in 1992 would have to pay the non-negligible price of the vaccine out of pocket (see Figure 1). The eligibility rules imply that individuals born before January 1, 1993, were ineligible for the free vaccine, whereas individuals born on or after this date were eligible for the free vaccine. We define eligibility for individual i as

$$Eligibility_i = 1[dob_i \geq 0]$$

where dob_i is the date of birth centered at the discontinuity point (January 1, 1993). We estimate the direct effects of the program using a sharp regression discontinuity design.[6] Consider the following model,

$$HPVvac_i = \alpha_{DE} + \beta_{DE} Eligibility_i + f(dob_i) + \gamma_{DE} X_i + u_i \tag{1}$$

where $HPVvac_i$ is a binary variable indicating HPV vaccine take-up of girl i and $f(\cdot)$ is a flexible function of the running variable, dob_i. X_i is a set of demographic covariates measured prior to eligibility, and u_i is an idiosyncratic error term. β_{DE} is the parameter of interest as it captures the direct effect of eligibility for the vaccination program on HPV vaccine take-up. The choice of bandwidth is central since theoretically the effect is only identified precisely at the cutoff point. For our baseline results we use a bandwidth of 12

[5] See Appendix B for an in-depth description of the eligibility for the HPV vaccine by cohort.

[6] See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for more detailed descriptions of the regression discontinuity design.

months.[7]

The choice of functional form of $f(\cdot)$ is also of great importance, as it should be flexible enough to describe the behavior of individuals precisely, but avoid identification off of functional form at the cutoff point (Gelman and Imbens, 2019). As a baseline, we use a linear specification and use a triangular kernel placing higher weight on observations closer to the cutoff.[8]

3.2 Spillover Effects of the Vaccination Program

We also use the regression discontinuity design setting to study the spillover effects of the vaccination program on older sisters. The empirical strategy resembles that of Dahl et al. (2014) and Bouckaert et al. (2020) who also use a regression discontinuity design to estimate the spillover effects of public programs where eligibility is determined by date of birth.

Consider the following model for the estimation of the spillover effect of eligibility of (focal) girl i on the HPV vaccine take-up of older (peer) sister j,

$$HPVvac_j = \alpha_{SE} + \beta_{SE} Eligibility_{i(j)} + f(dob_{i(j)}) + \gamma_{SE} X_j + u_j \tag{2}$$

where $HPVvac_j$ is the HPV vaccine take-up of older sister j, $Eligibility_{i(j)}$ is the eligibility of the younger sister i of girl j, $dob_{i(j)}$ is the date of birth of the younger sister i of girl j, X_j denotes the demographic characteristics of the older sister measured prior to eligibility of the focal girl.[9] β_{SE} is the parameter of interest as it captures the spillover effect of vaccination program eligibility of the younger sister on the vaccine take-up of the older sister. To further understand the mechanisms behind the spillovers, we document the heterogeneity in the spillover of eligibility by conducting various subgroup analyses.

3.3 Assumptions for the Regression Discontinuity Design

For the outlined regression discontinuity design to identify the direct and spillover effects, individuals must be unable to precisely control the running variable, date of birth, near the discontinuity point, which permits a near-random variation in treatment in the neighborhood of the discontinuity point (Lee and Lemieux (2010)). This is a minor concern in our setting, as the date of birth for the girls in the cohorts born 1992-1993 is determined many years prior to the introduction of the vaccination program. Hence, individual manipulation of the running variable in the context of being eligible for the vaccine is assumed to be highly unlikely. However, since January 1 is also the school starting age cutoff in Denmark [10], there could be other reasons for manipulating date of birth. We test for sorting around the cutoff in subsection 4.5. In subsection 4.5, we also test this assumption by examining whether there is a jump in any of the observable characteristics at the cutoff.

Another assumption that is very relevant in our context is that the behavior of the eligible girls (born in 1993) cannot affect the behavior of the ineligible girls (born in 1992). One may have the concern that girls in adjacent cohorts interact and potentially affect each other's vaccination decisions. While we cannot definitively rule this out, we find it plausible that there would be limited effects of the eligible girls on the

[7] See subsection 5.3 for analyses with alternative bandwidths.

[8] In subsection 5.3, we investigate the sensitivity of our results to choice of functional form and kernel.

[9] As described for the case of the direct effects, the choice of functional form of $f(\cdot)$, bandwidth, and kernel constitute important choices, and we investigate the sensitivity of our results to these choices in subsection 5.3.

[10] In section 5.3, we implement a differences-in-discontinuities strategy to check whether the school starting age cutoff contaminates our results.

ineligible girls. First, the spillover effects we find seem to be limited to sisters and do not extend to more distant family members (e.g., cousins; see Appendix Figure A.5). Second, class mates arguably constitute an important peer group for 15-year-old girls, but since the school cutoff date in Denmark is January 1, eligible and ineligible girls would typically not share a classroom.

External validity is a central issue in regression discontinuity designs; in our case, we believe external validity to be relatively high since there is no obvious reason why the estimated direct effects and spillover effects would vary substantially across individuals with different dates of birth.

4 Data and Descriptive Statistics

4.1 Primary Data Sources

The data used in our study stems from several administrative registers, all maintained by Statistics Denmark or the Danish Health Data Authority. We link data from different registers using the unique Danish civil registration number, which allows matching a given sample of young Danish girls with information on themselves, their parents, and their siblings irrespective of co-residence.

We derive information on vaccine take-up both from the Public Health Insurance Register and the National Prescription Register. The former register contains information on the consumption of publicly provided healthcare, including information on all services provided by primary care providers (PCP) for which the state provides a reimbursement. From this register, we can observe whether a PCP has administered a specific vaccine included in the CVP. The National Prescription Register, on the other hand, contains information on all sales and deliveries of medications. The register includes information on medications sold in pharmacies as well as non-pharmacy outlets, and any medication that is administered by physicians or in the hospital in general. From this register, we identify individuals who have purchased the vaccine.

4.2 Sample

For our empirical analysis, we use a sample comprised of all girls born in 1992 or 1993 and their older sisters born between 1985 and 1992.[11] We refer to girls born in 1992 or 1993 as the younger sisters or focal girls. With the restriction that older sisters must be born between 1985 and 1992, all older sisters in the sample are ineligible for the vaccination program under study, but eligible for a later catch-up vaccination program, which ran from August 27, 2012 to December 31, 2013 (see Figure 1). We include both full and half-sisters in the sample, which contains 18,754 sibling pairs. The 16,829 focal girls account for approximately 26 percent of all girls born in 1992 and 1993, whereas the 18,150 older sisters account for approximately nine percent of all girls born between 1985 and 1992. Of the focal girls, 2,399 have more than one older sister in the sample of older sisters.[12]. A more detailed overview of the sample selection is given in Table A.1 in the Appendix.

4.3 Variables

The primary outcome variable in the analysis is the take-up of the HPV vaccine. As a baseline, we define HPV vaccine take-up for both the focal girls and the older sisters as having received the first dose of the vaccine.[13]

[11] In Section 5.3, we vary this definition of the sample to test the robustness of our results to the choice of sample.

[12] This implies that 2,399 of the focal girls appear more than once in our main analysis where each sister pair constitutes an observation. In Section 5.3 we show that the results are robust to the exclusion of focal girls who appear more than once.

[13] Full HPV vaccination required three doses at this time and we show in subsection 5.3 that our results are not sensitive to whether we define take-up as one or three doses.

We measure HPV vaccine take-up prior to August 27, 2012 when the catch-up vaccination program for the 1985-1992 birth cohorts started to ensure that the 1992 cohort was ineligible for the free HPV vaccine in the period that we consider. Our HPV vaccine take-up measure captures both vaccines purchased privately and those administered through the CVP.

The RD analysis below includes several covariates. The covariates are all predetermined sociodemographic maternal characteristics measured in 1991 (prior to birth of the focal daughter). Maternal characteristics include information on mother's age at childbirth, marital status, ethnicity, education, and income. For each sibling pair, we calculate the age spacing between them and obtain information on whether they share one or both parents and whether they live in the same household. 83 percent of the sibling pairs share the same parents. For these sibling pairs, maternal information will be invariant. For both sisters we also have information on immigrant status. For the focal girls, we have information on birth order and number of siblings in the family.[14]

4.4 Descriptive statistics

Table 1 shows descriptive statistics for our analysis sample. We include both HPV vaccine take-up and predetermined sociodemographic and family-related characteristics for the focal sisters in the first to third columns and for the older sisters in the fourth to sixth columns. Simply by comparing the means, we can see how girls born in 1993 are much more likely to have received the first dose of the HPV vaccine (73.2 percent) than girls born in 1992 (22.9 percent). We see a similar pattern for the older sisters, albeit less pronounced. Approximately 10 percent of the girls are descendants of immigrants. The mothers are on average about 30 years old when they give birth to the focal girl and 26 years old when they give birth to the older sister. More than 20 percent of the mothers have higher education. Birth weight and maternal characteristics are missing for a small number of observations in our sample for both focal girls and peer sisters. We impute the missing values and include an indicator for imputed values.[15] From the standardized difference in means (SDM) we see that focal girls born in 1992 being comparable in terms of observable characteristics to focal girls born in 1993—and this also holds for the older sisters. The presence of any differences across cohorts of the focal girls is formally tested in subsection 4.5. The samples of focal girls and older sisters are also roughly comparable to the corresponding population cohorts, see Appendix Table A.2.

[14] We define a siblingship as both full and half siblings from the mother's side of the family.

[15] For missing birth weight, we replace these with the median birth weight in the sample. For maternal marital status, immigrant status and education and earnings we set missing values to zero. For missing values of maternal age at childbirth we replace these with the median age at childbirth in the sample. We include an indicator for imputed values in all relevant specifications.

Table 1: Descriptive Statistics by Year of Birth of Focal Girls

Variables	Focal girls			Older girls		
	1992	**1993**	**SDM**	**1985-1992 with focal 1992 girl**	**1985-1992 with 1993 focal girl**	**SDM**
HPV-vaccine take-up						
1 dose [0,1]	0.229	0.732	1.165	0.159	0.225	0.168
3 doses [0,1]	0.173	0.709	1.283	0.117	0.174	0.162
Child characteristics						
Descendant [0,1]	0.083	0.098	0.052	0.084	0.098	0.049
Birth weight [grams]	3448.2	3463.1	0.025	3364.3	3363.3	-0.002
	(597.5)	(601.2)		(588.5)	(581.2)	
Birth weight missing [0,1]	0.002	0.007	0.075	0.033	0.034	0.006
Maternal characteristics						
Married [0,1]	0.588	0.504	-0.169	0.644	0.574	-0.143
Immigrant/descendant [0,1]	0.089	0.09	0.004	0.098	0.112	0.046
Age at childbirth [years]	29.589	29.902	0.074	26.019	26.301	0.069
	(4.163)	(4.2469)		(4.041)	(4.133)	
Basic education [0,1]	0.438	0.443	0.01	0.441	0.447	0.012
Vocational education [0,1]	0.315	0.304	-0.024	0.314	0.302	-0.026
Further education [0,1]	0.227	0.216	-0.026	0.226	0.217	-0.022
Yearly earnings [euro]	13494.4	13113.3	-0.034	13415.2	12968.1	-0.04
	(11054.8)	(11148.1)		(11073.6)	(11120.7)	
Mother missing [0,1]	0.02	0.035	0.092	0.02	0.033	0.085
Family characteristics						
Age spacing [in months]	43.738	43.775	0.002			
	(18.737)	(18.713)				
Children in the family	3.855	3.87	0.004			
	(3.598)	(3.651)				
Birth order	3.309	3.328	0.005			
	(3.569)	(3.633)				
N	9,253	9,501		9,253	9,501	

Notes:

The table presents descriptive statistics for the sample of focal girls and their corresponding older sisters across year of birth of the focal girls. Each cell in Columns 1–2 and 4–5 represents the mean of the corresponding variable in the row with standard error in parentheses for non-binary variables. Columns 3 and 6 present the standardized difference in means (SDM) (or the standardized bias); i.e., the difference in means as a share of the square root of the average sample variances of the 1992 and 1993 groups.

4.5 Validity tests

The validity of the regression discontinuity design hinges on the assumption that individuals cannot completely or precisely manipulate the running variable (Lee and Lemieux, 2010). We test this by examining the frequency of sibling pairs in our sample around the cutoff. Appendix Figure A.1 plots the density of sibling pairs in the sample across month of birth. We do not see any indication of significant sorting at the cutoff. A similar pattern emerges if we consider the density for the full 1992-1993 birth cohorts(see Appendix Figure A.2).

An implication of the near random assignment in the regression discontinuity design is that predetermined covariates should be independent of treatment (being born before or after January 1, 1993 in our setting). We test whether there is a discontinuity in the predetermined sociodemographic and family-related characteristics around January 1, 1993. Appendix Table A.3 shows that the predetermined covariates are overall locally balanced. We only find one statistically significant difference at the 5 percent level (for the birth weight of the older sister).

The sample consist of girls born in 1992 and 1993, with at least one older sister born between 1985 and 1992. For the spillover results to be valid there cannot be a discontinuity at the cutoff in the likelihood of having an older sister (i.e., a discontinuity in the likelihood of being in the sample). Appendix Figure A.3 presents the fraction by month of birth for all girls born in 1992 or 1993 with an older sister in the sample. There is no evidence of a discontinuous jump in the probability of being in the sample. Overall, we conclude that the regression discontinuity design has high validity in our setting.

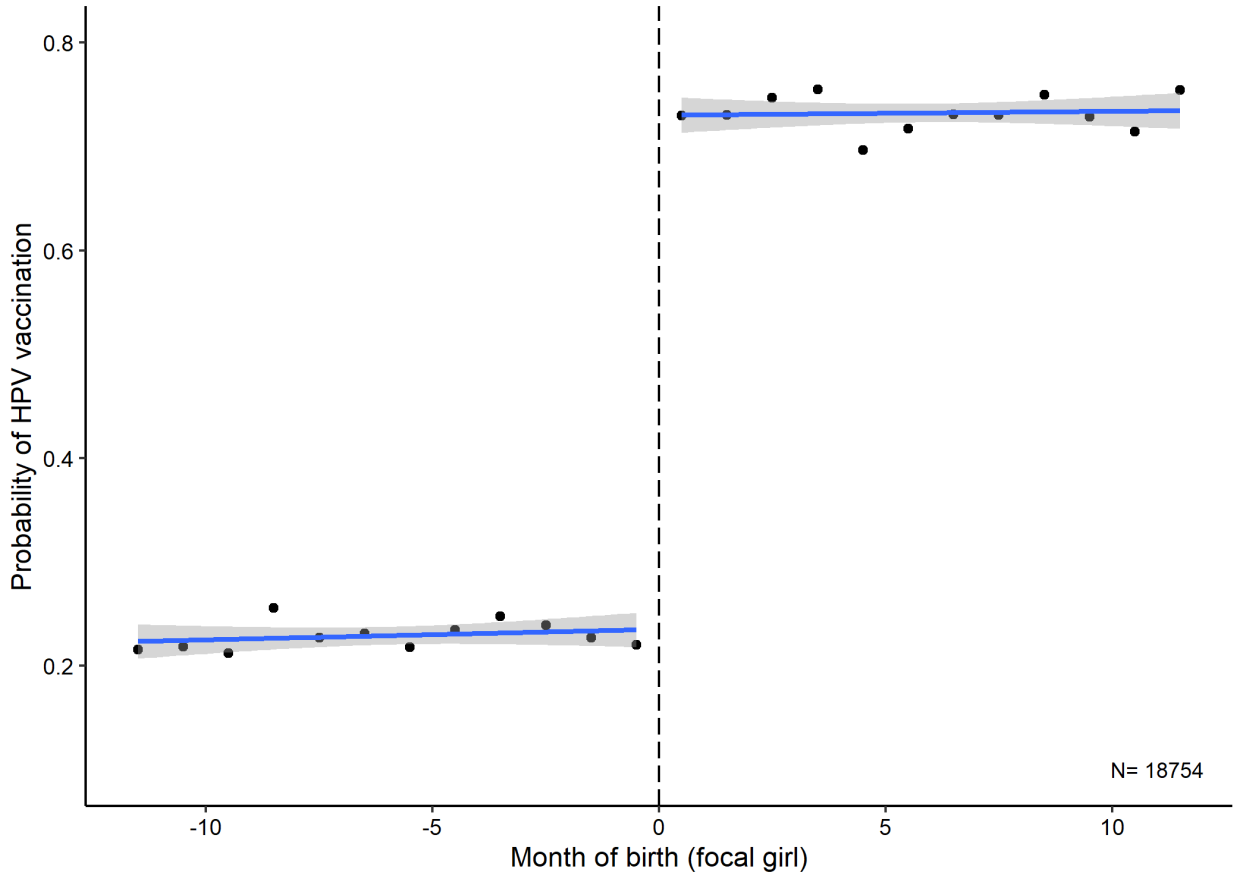
5 Results

5.1 Direct Effects of HPV Vaccination Program on HPV Vaccine Take-up

The first step of our empirical analysis is to establish the effect of the introduction of the HPV vaccination program on focal girls' HPV vaccine take-up. A focal girl's eligibility for the free HPV vaccine is a discontinuous function of her date of birth. Girls born prior to January 1, 1993 were not eligible for the free vaccine through the initial catch-up program, while girls born January 1, 1993 or later were eligible. Therefore, we estimate the effects of eligibility using a regression discontinuity design.

Figure 2 shows the HPV vaccine take-up of focal girls (younger sisters) by month of birth (centered). The graph shows a sharp discontinuity in the take-up rate of focal sisters at the cut-off with vaccination rates increasing from around 20 percent to approximately 70 percent. The graph provides strong evidence that providing the HPV-vaccine free of charge through the CVP had large direct effects on vaccination rates on the girls targeted by the program.

Figure 2: Probability of HPV Vaccination by Month of Birth - Focal Girls



Notes: This figure shows the probability of HPV vaccination (first dose) by month of birth (centered) for the focal girls. Each data point represents the fraction of focal girls vaccinated. The dashed vertical line marks the eligibility cutoff (January 1, 1993) for the HPV vaccination program. Girls born prior to January 1, 1993, are ineligible for the vaccination program while girls born January 1, 1993 or later are eligible for the program. The plot is overlaid with fitted lines from a linear regression of HPV vaccine take-up on eligibility, month of birth and an interaction of the two.

The regression discontinuity estimates of the direct effects of the vaccination program are presented in the first panel of Table 2. In the first column, we report the estimated direct effect when no covariates are included in the regression and the specification in the second column includes covariates. In the third and fourth columns, we vary the sibling pairs included in the analysis; in the third column by excluding pairs where one or both sisters appear in more than one pair in the sample and in the fourth column by including only the older sister who is closer to the focal girl in terms of age. For our baseline specification in the second column, we estimate the direct effect on HPV vaccine take-up of eligibility for the vaccination program to be 50.5 percentage points. The estimate is statistically significant at the 1 percent level. The estimated direct effect varies little across specifications and sample restrictions. Average HPV vaccine take-up for focal girls born in 1992 is 22.9 percentage points (see Table 1). Consequently, the estimated effect constitutes an increase of about 221 percent. Free provision of the HPV vaccine led to a substantial increase in HPV vaccine take-up for the targeted girls.

Table 2: Baseline regression results

Effect	No covariates (1)	Baseline (2)	Families with one sibling pair (3)	Sibling pairs with closest spacing (4)
Direct effect	0.503***	0.505***	0.491***	0.499***
	(0.015)	(0.015)	(0.015)	(0.014)
	[0.229]	[0.229]	[0.247]	[0.238]
Spillover effect	0.047***	0.046***	0.055***	0.051***
	(0.013)	(0.012)	(0.014)	(0.013)
	[0.159]	[0.159]	[0.174]	[0.166]
N	18754	18754	15107	16829

Notes:
This table presents estimates of the direct and spillover effects of eligibility for the vaccination program. Standard errors are in parentheses. For the panel with the direct effects, the number in brackets is the mean HPV vaccine take-up of focal girls that are ineligible for the vaccination program (born prior to January 1, 1993). For the panel with the spillover effects, the number in brackets is the mean HPV vaccine take-up of older sisters with a younger sister who is ineligible for the vaccination program (born prior to January 1, 1993). All regressions include month of birth (the running variable), an eligibility indicator and an interaction of the two. The bandwidth is 12 months. A triangular kernel is used. The baseline specification in column (2) includes all covariates listed in Table 1 as well as corresponding indicators for missing values. In column (3), we exclude sibling pairs where the focal sister has more than one sister in the sample. In column (4), for focal girls with multiple older sisters, we include only the sibling pair with the older sister that is closest to the younger sister (in terms of age). Standard errors are robust to clustering at the family level.
*** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level

As is evident from Figure 2, about 20 percent of the focal girls who are born in 1992 choose to get vaccinated (at a cost of 470 euros). Correspondingly, about 30 percent of the focal girls who are born in 1993 (and therefore eligible to get the vaccine free-of-charge) choose not to receive the vaccine. To investigate the differential impact of eligibility on vaccine take-up across different groups of individuals, we profile the compliers, always-takers and never-takers of the vaccination program following Marbach and Hangartner (2020).[16] Besides increasing our knowledge of who contributes to the estimated effect (the compliers), the complier profiling allows us to deepen our understanding of which socioeconomic groups react more or less to vaccination programs. Thus, the complier analysis allows us to document the distributional impacts of a large public health program, which adds to an active research agenda focusing on inequalities in health and socio-economic gradients in health care take-up (Almond et al. (2018)).

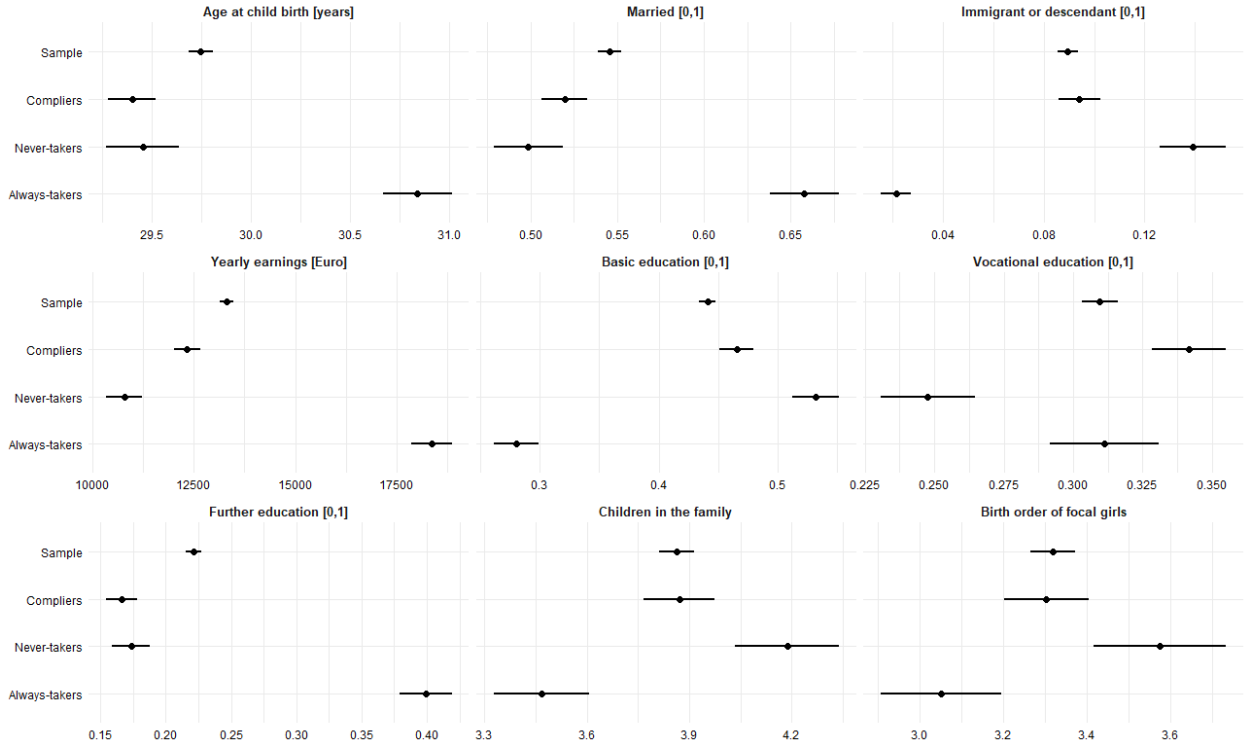
Figure 3 shows the profiling for the key maternal and family characteristics included in our analysis. Compliers constitute 51 percent of the sample while always-takers account for 21 percent and never-takers for 28 percent. For always-takers, mothers tend be older, are more likely to be married, and less likely to be immigrants or descendants. Always-takers have higher socioeconomic status. All in all, this suggests that mothers of always-takers have high levels of human capital. The always-takers also tend to come from smaller families and the focal girls have lower birth order.[17] Conversely, compliers and never-takers have mothers

[16] We think of compliers as the girls who get vaccinated *because* they are eligible for the vaccine. Hence, we adopt the language of instrumental variables, even though we ultimately refrain from estimating IV models.
[17] Pruckner et al. (2021) find that parental health investment in early childhood differs by birth order, for example, lower birth order children have higher vaccination rates.

who are younger, less likely to be married and have a higher likelihood of being an immigrant or descendant (especially the never-takers). The never-takers also come from low-SES families. Lastly, never-takers come from larger families. The compliers generally make out the middle group between the always-takers and the never-takers.

The complier analysis provides descriptive evidence of which socio-demographic groups are more responsive to getting the free-of-charge vaccine. Overall, the profiling shows that the group of always-takers willing to incur the vaccination cost of €470 come from high-SES families. The distinction between compliers and never-takers across the available covariates is less clear, although never-takers come from homes with lower income and higher likelihood of having a mother with basic education, suggesting that never-takers tend to be from low-SES families.[18]

Figure 3: Profiling Compliers, Never-takers and Always-takers of the Vaccination Program



Notes: Each plot shows the covariate mean across the whole sample, compliers, always-takers, and never-takers. The means are estimated using the method outlined in Marbach and Hangartner (2020). Compliers constitute around 51% of the sample while always-takers accounts for 21% and never-takers for 28%.

5.2 Spillover Effects of HPV Vaccination Program on Older Sisters HPV Vaccine Take-up

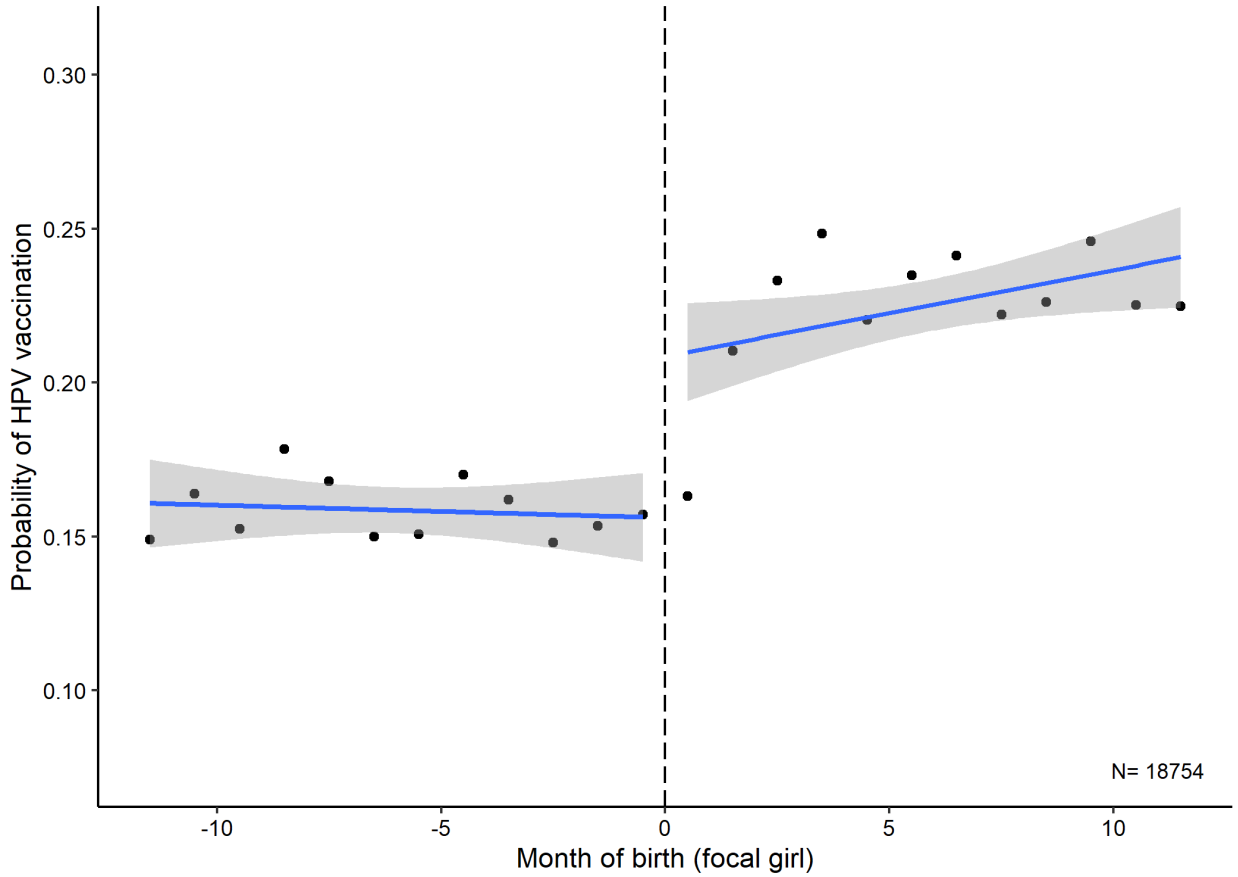
Older sisters of the focal girls born in 1992 and 1993 are not eligible for the free HPV vaccine through the vaccination program considered here. The older sisters later became eligible for the free HPV vaccine through a catch-up vaccination program (see Figure 1), but our analysis focuses on HPV vaccination take-up prior to this catch-up program. The older sisters are born between 1985 and 1992 and are on average 19 years old in 2008. While WHO defines the primary target group of the HPV vaccine to be girls aged 9-14,

[18] This is consistent with e.g. Hirani (2021), who finds that general compliance with the Danish childhood vaccination program is higher for children from high-SES families.

older girls may still benefit from the vaccine (Westra et al., 2011; WHO, 2016). While the eligibility of the younger sister does not affect the monetary cost of getting the vaccine for the older sisters, having a younger sister who is eligible for the vaccine may still affect older sister take-up by changing the price of vaccination for the eligible girl or by changing the perceived benefits of vaccination.

Figure 4 shows the probability of older sister HPV vaccine take-up by the month of birth of the younger sister. By visual inspection, the probability of vaccination jumps from around 15 percent to around 20 percent at the discontinuity point (the eligibility threshold). Since none of the older sisters are eligible for the free vaccine, from the perspective of the older sister, the only thing that changes at the discontinuity point is the eligibility status of her younger sister. Nevertheless, we see a clear spike in the probability of vaccination for the older sister at the eligibility threshold. This implies that whether a girl is eligible for the free HPV vaccine in turn affects the take-up decisions of her older sister(s).[19]

Figure 4: Probability of HPV vaccination by month of birth of focal girl - older sisters



Notes: This figure shows the probability of HPV vaccination (first dose) for the older sister by month of birth (centered) of the focal girl. The dashed vertical line marks the eligibility cutoff (January 1, 1993) for the HPV vaccination program. Girls born prior to January 1, 1993 are ineligible for the vaccination program while girls born January 1, 1993 or later are eligible for the program. The plot is overlaid with fitted lines from a linear regression of HPV vaccine take-up on eligibility, month of birth and an interaction of the two.

The regression discontinuity estimates of the spillover effects of the vaccination program are presented in

[19] In Figure 4, we consider the data point corresponding to January 1993 to be an outlier. The average HPV vaccine take-up for older sisters of girls born in 1993 is only about 16 percentage points, which is substantially lower than the average take-up for older sisters of girls born in February-December 1993. We can think of no meaningful explanation for why this would be the case and consequently consider this point to be an outlier. There is no corresponding outlier in Figure 2.

the second panel of Table 2. For our baseline specification in the second column, we estimate the spillover effect on older sister HPV vaccine take-up of eligibility for the vaccination program to be 4.6 percentage points. The estimate is statistically significant at the 1 percent level. The inclusion of covariates matters little for the estimated spillover effect. When we restrict the sample to one sibling pair per family (the third and fourth columns), the estimated spillover effects tend to be larger. Average HPV vaccine take-up for older sisters of focal girls born in 1992 is 15.9 percent (see 1). The estimated spillover effect corresponds to an increase of about 29 percent in HPV vaccine take-up of the older sisters. This is arguably a substantial increase in take-up for a group of girls who were not targeted by the vaccination program.[20]

5.3 Robustness

The following is a series of analyses with alternative model and sample specifications intended to secure the robustness of the results in Table 2 (referred to hereafter as the baseline model). Tables A.4 and A.5 present the findings of these alternative specifications. Following Gelman and Imbens (2019), we have not included specifications with higher order polynomials as Figures 2 and 4 suggest a linear relationship. In general, the results from the baseline model seem to be stable with positive and statistically significant direct and spillover effect estimates across specifications.

In Table A.4, we vary the regression discontinuity specification along several dimensions: donut RD, choice of kernel, aggregation level of the running variable and level of clustering. Our main results from Table 2 are robust to these changes in specifications and the point estimates and standard errors generally do not change substantially. With the donut specification, the estimated spillover effect is somewhat larger, which is due to the omission of the outlier just to the right of the discontinuity point (cf. Figure 4).

In the first column of Table A.5, we present results where the outcome variable is defined as completing the vaccination series (three doses). This does not substantially change our results. In the second and third columns, we vary the window width, which is one year in our baseline results. We still find similar results for the 6-month window width, but the spillover effects are smaller and statistically insignificant. This is unsurprising given how the outlier data point in Figure 2 will be pulling the estimated spillover effect downward. Using the two-year window width, we find results comparable to our baseline results. In the fourth column, we estimate the direct effect for the full population of girls born 1992-1993; in comparison, our baseline estimates include only focal girls with an older sister in the sample. Interestingly, the direct effect is about 10 percentage points smaller in the full population sample, which may be partly explained by how we also find in the complier analyses that, for example, always-takers tend to be from smaller families (cf. Figure 3). In the fifth column, we relax the age restriction on the older sisters, and we still find positive direct and spillover effects, albeit somewhat smaller than our baseline results.

Finally, in the sixth column of Table A.5, we estimate the direct and spillover effects using a differences-in-discontinuities design. As mentioned above, the school starting age cutoff in Denmark coincides with the vaccination program eligibility cutoff. In Denmark, children start school in the calendar year in which they turn six. Thus, children born on January 1 will on average be older starting school than children born on December 31 in the previous year (Dalsgaard et al., 2012; Landersø et al., 2017). This poses a potential challenge for our identification strategy, since this would imply that girls may not be comparable across the cutoff, as December-born girls will be young-for-grade and January-born girls old-for-grade. If school

[20] If we take a similar approach and estimate spillover effects on brothers and female cousins, we find no evidence of spillover effects (cf. Appendix Figures A.4 and A.5). The estimated spillover effects appear to be gender-specific, which is unsurprising given that the HPV vaccine almost exclusively targeted girls at the time.

starting age matters for vaccine take-up, the simple regression discontinuity design potentially conflates the effect of school starting age with the effect of vaccination program eligibility. As the school starting age is not specific to the January 1*st* 1993 cutoff, we can remove the effect from any imbalance resulting from the school starting age rule (or any other imbalances that may occur around the January 1 cutoff). To test whether the school starting age rule is important for our results, we implement a differences-in-discontinuities design (Carneiro et al. (2015); Grembi et al. (2016)). The differences-in-discontinuities design essentially compares the discontinuity in a particular year where there was no change in eligibility for the vaccination program (e.g., using a window around January 1, 1992) to the discontinuity in the year where there was a jump in eligibility (using a window around January 1, 1993). Letting Δ_{jan} be the general impact of being born in after January 1$^{\text{st}}$ on vaccine take-up, and Δ_{elig} being the direct effect of eligibility on vaccine take-up, then

$$\beta_{DE}^{1993} - \beta_{DE}^{1992} = (\Delta_{elig} + \Delta_{jan}) - (\Delta_{jan}) = \Delta_{elig}$$

More formally, consider the following model,

$$HPVvac_i = \alpha_{DE} + \beta_{DE} Eligibility_i + \phi_{DE} D_i^{1993} + \delta_{DE} Eligibility_i \times D_i^{1993} + f(dob_i) + \gamma_{DE} X_i + u_i \quad (3)$$

where D_i^{1993} is an indicator of belonging to the 1992-1993 cut-off (as opposed to the 1991-1992 cut-off) and the other variables are previously defined. Δ_{DE} captures the differences-in-discontinuities estimate of eligibility for the vaccine program on vaccine take-up. The differences-in-discontinuities estimates in the sixth column of Table A.5 are in line with our baseline results.

The results from the various model specifications underline how the results from the baseline model presented above are robust to alternative specifications, and they underpin the older sister spillover effect of introducing the HPV vaccine into the program.

5.4 Mechanisms Underlying Spillover Effects

We have documented the existence of both direct effects and spillover effects of the HPV vaccination program. In this subsection, we investigate the heterogeneity in both the direct and spillover effects to provide some evidence for what may be driving the spillover effects.

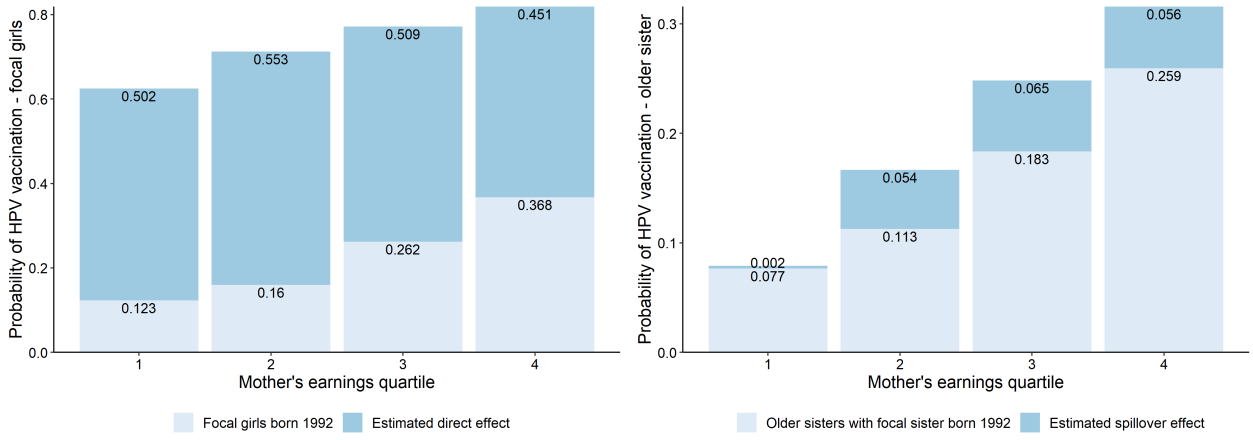
We know that the introduction of the HPV vaccination program led to two immediate changes for girls born in 1993: They became eligible to get the vaccine free-of-charge on October 1, 2008, and they were made aware of eligibility through an information letter and leaflet sent to the families of all eligible girls. From the perspective of the household unit, the households of eligible girls faced a reduced cost of getting the HPV vaccine and received an informational intervention that likely improved parental health education specifically regarding cervical cancer and the HPV vaccine benefits. We hypothesize that eligibility for the vaccine program potentially affects siblings directly (through the reduced cost or the informational intervention) and indirectly (through peer effects). While we cannot separate these channels, we provide different items of evidence that can be informative about what channels may be important.

5.4.1 Vaccine Cost

Vaccine cost has been shown to be an important barrier to vaccination (see e.g. Holman et al. (2014)). The introduction of the vaccination program effectively changes the relative prices of vaccinating the children in a household. The substitution effect would lead to lower take-up of ineligible children, while the income effect

may lead to higher take-up of ineligible children as parents have more resources to distribute across their children.[21][22] If the cost reduction is an important driver of the spillover effects, we expect the eligibility of the younger sister for the free-of-charge vaccine to lead to larger spillover effects in families that are more likely to be credit-constrained. The €470 cost of the vaccine might pose a barrier for families at the bottom of the income distribution. To investigate this, we estimate the effects separately for each quartile of mother's income. The results are presented in Figure 5.[23] While the direct effect is smaller for the high-income families, the overall take-up rate is higher in high-income families, as they are also more likely to take up the vaccine in the absence of the vaccination program.[24] Overall, the vaccination program reduces the inequality in HPV vaccine take-up of the focal girls. In comparison, we also see a strong income gradient in vaccine take-up of the older sisters in the absence of the vaccination program. There is a difference in take-up of 18.2 percentage points between older sisters in the top of the distribution relative to older sisters in the bottom of the distribution. However, since the spillover effect is estimated to be zero for families at the bottom of the income distribution, the vaccination program actually increased inequality in take-up among the older sisters of the focal girls. If the cost reduction was an important mechanism for the spillover effects, we would expect larger spillovers for low-income groups. We conclude that the cost reduction is likely to play a limited role in explaining the observed spillover effects.

Figure 5: Direct and Spillover Effects by Mother's Income Quartile



(a) Direct effect

(b) Spillover effect

Notes: The graphs in Panels A and B display the estimated direct and spillover effects of the vaccination program, respectively. Estimations are performed separately by income quartile of the mother of the focal girl. The graphs also include the probability of HPV-takeup for focal girls born in 1992 and older sisters of focal girls born in 1992, respectively.

5.4.2 Information Provided to Families

We now turn to the role of the informational component of the vaccination program. The information material highlights that the eligible girls can get the vaccine free-of-charge and that they should do so as soon

[21] This intuition is based on a simple two-good world, with good 1 being vaccination of child 1 and good 2 being vaccination of child 2 and vaccinations being normal goods. The introduction of the vaccination program corresponds to a decrease in the price of good 1 in this world.

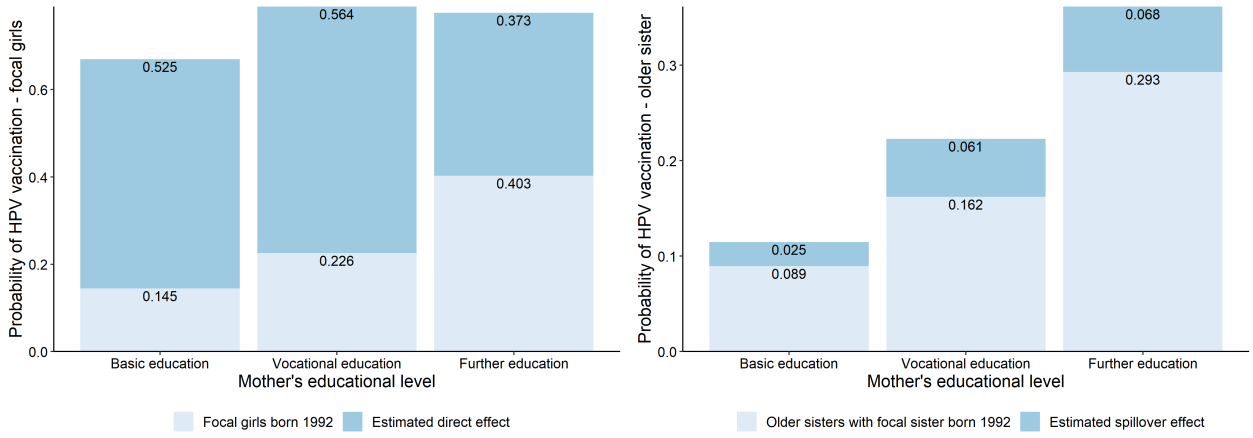
[22] Fitzpatrick and Thornton (2018) provide evidence from Nicaragua on how households change their health utilization in response to a health insurance program that implies differential coverage across family members.

[23] Appendix Table A.6 shows estimated direct and spillover effects by mother's characteristics; earnings quartile, education level, health care education and age at childbirth.

[24] Existing studies have also demonstrated that high-SES families are more likely to be adherent to health recommendations or informational interventions (Suppli et al., 2018; Lübker and Lynge, 2019; Oster, 2020; Humlum et al., 2021).

as possible (after the eligibility date). As the intention of the information supplied was to boost HPV vaccine take-up, we do not expect the information to have had a negative influence on the likelihood of vaccination. Thus, we do not expect that being eligible for the vaccine would induce non-compliance of girls who would have gotten the vaccine had they not been eligible, that is, we assume that the monotonicity assumption holds. Since information is provided to the girls *and* their parents, the informational intervention is likely to improve parental health education and specifically knowledge about cervical cancer and the benefits of the HPV vaccine. The information provided may play an important role in explaining the observed spillover effects. To investigate the potential role of the informational intervention in more detail, we conduct a number of auxiliary analyses. First, we estimate direct and spillover effects by maternal education level. Previous studies have shown that highly educated individuals make better health decisions and are better at adopting new technologies (Lange, 2011; Groes et al., 2017). We hypothesize that highly educated mothers[25] with their higher levels of human capital are better at understanding the information provided and applying that information to other contexts (e.g., their older daughters who are not eligible for the vaccine and not explicitly mentioned in the letter). Figure 6 shows the direct and spillover effects estimated separately by mother's education. The overall pattern is similar to the pattern that we observed for income in Figure 5. When we consider mother's education level, the vaccination program reduces the overall inequality in vaccine take-up for the focal girls. The spillover effects still imply that the vaccination program increased the health inequalities for the older sisters. This evidence is consistent with the informational intervention being an important mechanism underlying the estimated spillover effects.[26]

Figure 6: Direct and Spillover Effects by Mother's Education



(a) Direct effect

(b) Spillover effect

Notes: The figures in Panel A and B display the estimated direct and spillover effects of the vaccination program, respectively. Estimations are performed separately by income quartile of the mother of the focal girl. The figures also include the probability of HPV vaccine takeup for focal girls born in 1992 and older sisters of focal girls born in 1992, respectively.

[25]For concision, we only present results using maternal education level since the mother is likely to be the main decision maker in the household regarding children's health (Daly and Groes, 2017). Results are similar for fathers' education level.

[26]In Appendix Table A.7, we also show that the estimated spillover effects are driven by relatively closely spaced sisters (¡6 years) and sibling pairs where the older sister still lives at home. This evidence also corroborates a hypothesis of information to families being an important factor in the vaccination decisions.

5.4.3 Peer effects

We hypothesized that the fact that a younger sister takes up the vaccine may affect whether or not an older sister takes up the vaccine. Peer effects of this sort may arise, for example, if the younger sister is vaccinated by her primary care provider (who may provide additional information about the benefits of the vaccine) and passes on information about this event to her older sister and/or parents. In this case, information may also drive the spillover effect through the peer-interaction between siblings, but it does not reflect a direct effect of the informational intervention. Obviously, these channels are difficult to disentangle.

In the case of the information material, we interpret the spillover effects as program spillovers driven by the information provided to eligible girls. In the case of sister interaction, we interpret the spillovers as a peer effect. Thus, we assume that the eligibility of the focal girl affects older sister vaccine take-up only through the vaccine take-up of the younger sister. In the peer effects literature, the underlying social or causal mechanisms driving the social interaction (and thereby the spillover) are often very context specific. However, previous studies on peer effects in program participation in general (e.g. Dahl et al. (2014)) and peer effects in vaccine take-up specifically (e.g. Rao et al. (2011)) find some evidence for the transmission of program benefits between peers. The analysis in Section 5 focuses on the direct effect for girls (with an older sister) of the HPV vaccine and the spillover effect to older, non-eligible sisters. This approach is useful especially if we think of the spillover effects as program spillover driven by a boost of information from being eligible. If we instead believe that the spillover effects in fact reflect peer effects transmitted through interactions, we can view the direct and spillover results from an IV perspective. Here, the direct effect can be viewed as a first stage and the spillover effect as a reduced form. If we are willing to assume both monotonicity and excludability, then dividing the reduced form estimates with the first stage estimates provides us with a peer effect of 9.1 percentage points.[27]

Whether the direct effect of information from eligibility or information through peer interactions that potentially drives the spillover effect is relevant from a policy perspective. To investigate which channel may be driving the effect, we investigate the timing of the older sister's vaccination relative to the timing of the younger sister's eligibility. If the direct information channel dominates, we hypothesize that the information material will cause the older sister to get vaccinated around the same time as her younger sister. Conversely, if it is the indirect information channel that drives the effect, we expect to see some lag between the timing of the vaccination of the two sisters. Generally, a peer effect may take more time to materialize than any direct effects of the information provided to parents.

To investigate the extent to which there is a lag between the points in time when siblings get vaccinated, we employ a panel event study. We estimate the probability of the older sister getting vaccinated relative to the timing of eligibility of the younger sister. In practice, we estimate the following model by OLS,

$$Y_{jt} = \delta_t + \sum_{\tau \neq -1} \gamma_\tau Eligibility_{i(j)} \times \mathbb{1}[t = \tau] + \varepsilon_{jt}, \tag{4}$$

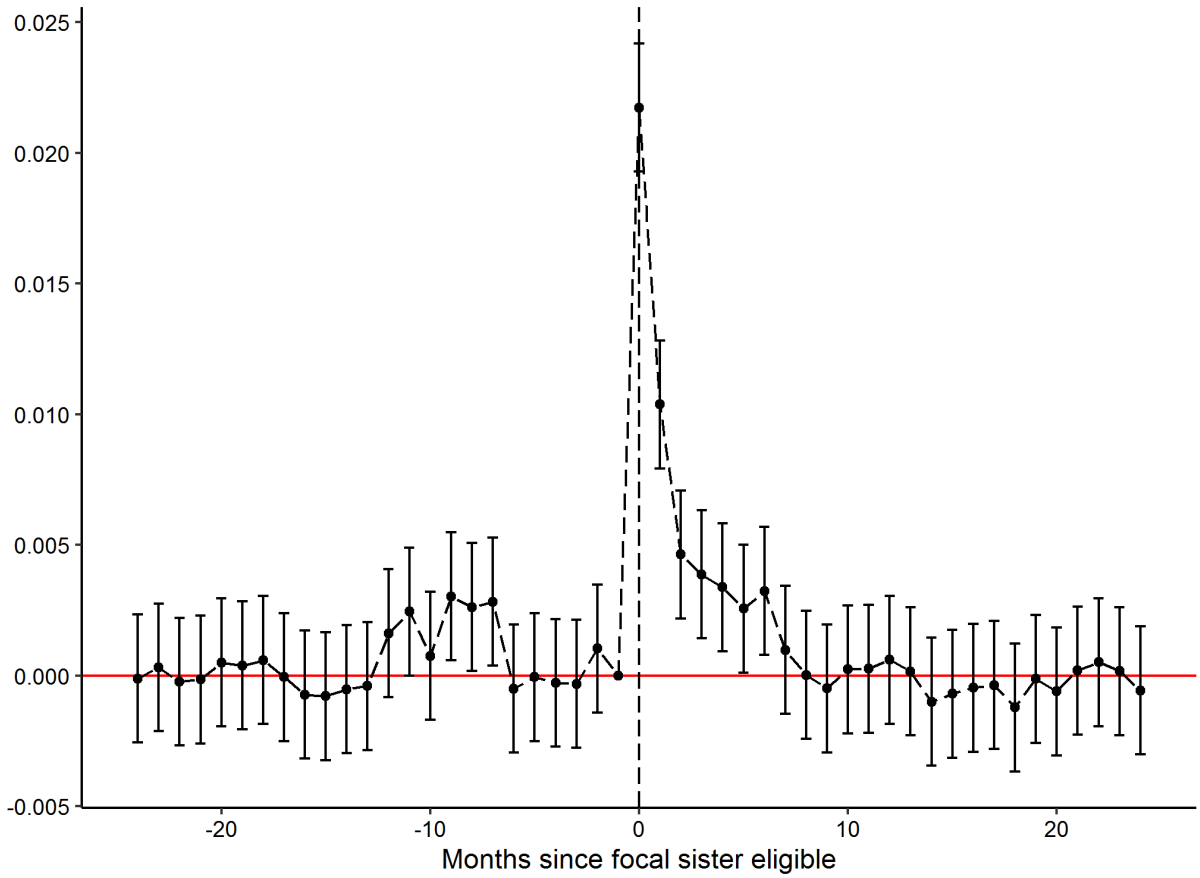
where Y_{jt} is an indicator for whether older sister j has gotten her first HPV shot in period t, where t denotes time relative to the point in time at which younger sister i potentially becomes eligible for the free-of-charge vaccine. We observe individuals on a monthly level for a period of +/- 2 years around October 2008[28]. δ_t are time fixed effects.

The parameters of interest are the γ_τ's measuring the difference in vaccination (initiation) rate between

[27] The approach follows Moffitt (2001) and is applied in the fuzzy regression discontinuity case in Dahl et al. (2014).
[28] $t \in \{-24, -23, ..., -1, 0, 1, ..., 23, 24\}$

individuals with siblings born before or after January 1, 1993. Figure 7 plots the estimates of γ_t. The horizontal axis displays months since the eligibility of the focal sister. The vertical axis displays the probability of the older sister being vaccinated in a given month. The figure shows that having a younger sister who becomes eligible for the free vaccine increases the probability of the older sister getting the vaccine in October 2008 or in the following months. The estimated effects are positive and statistically significant for event times 0-7, but there is clearly a large spike at time 0, indicating that also the older sisters also react very promptly to the eligibility of the younger sister.[29] Given the very immediate reactions of the older sisters to the eligibility of their younger sisters, this evidence is consistent with the information provided to families playing a direct role in the determination of the spillover effects. Ultimately, we cannot disentangle the direct information effect of the program from the potential indirect effects in the shape of peer effects. Our estimated peer effects can be considered an upper bound on peer effects in vaccine take-up. Generally, our results are consistent with both information and peer effects being important drivers of the estimated spillover effects.

Figure 7: Probability of Vaccination of Older Sister



Notes: The figure shows the results of a panel event study estimating the effect of eligibility of the focal girl on older sisters HPV vaccine take-up. Event time is time relative to month of eligibility of the focal sister. The vertical dashed line represents October 2008 which is the month where the focal girls born in 1993 became eligible for the program. The vertical axis displays the probability of vaccination of the older sister in a given month relative to the month before becoming eligible. Number of observations is 18,754.

[29]For the younger sisters, we also see that take-up increases immediately after eligibility and within the first month approximately 30 percent of eligible girls have received their first shot of the HPV vaccine.

6 Implications of Vaccine Spillovers for Herd Immunity and Cost Effectiveness

6.1 Herd Immunity

Vaccines are often put forward as a prime example of *externalities*, as the socially optimal level of vaccination is below the privately optimal level. When an individual chooses to get vaccinated they not only lowers the likelihood of getting infected but also limit the spread of the infectious disease. If a sufficiently large number of individuals get vaccinated, the infection can no longer spread and can possible be eradicated[30]. The level of vaccination required to achieve herd immunity will vary by both the infectious disease and the vaccine in question. However, the higher the number of vaccinated individuals at a given point in time, the faster the infectious disease will stop spreading (The Danish Health Authority, 2019). In the case of HPV, The Danish Health Authority (2007) estimates a vaccination rate of 95 percent of girls in a cohort is needed in order to achieve herd immunity.

When investigating vaccine take-up, accounting for spillover effects to ineligible groups is potentially important. We have shown how the spillover effect of a vaccination program to ineligible older sisters constitutes almost 10 percent of the direct effect among eligible girls. The actual vaccination rates for the 1985-1992 cohorts with (without) sibling spillover is around 20 (15) percent. Assuming a target vaccination level of 95 percent, 80 (95-15) percent remains unvaccinated in the absence of spillover effects. Taking the spillover effect into account, 75 percent remain unvaccinated. A simple back-of-the-envelope calculation thus suggests that the sibling spillovers close 6.25 ((20-15)/(95-15)) percent of the gap between the actual vaccination rate and the vaccination rate required to achieve herd immunity (in the group of women born 1985-1992 – at least for the group with younger sisters).

6.2 Cost effectiveness

Economic evaluation of public health programs or medical interventions in general - and vaccine programs in specific - most often does not take into account spillover effects (Fletcher and Marksteiner, 2017; Jit et al., 2015). The implications of treatment spillovers for cost effectiveness analyses are potentially large and we therefore find it relevant to quantify the importance of the spillover effects.

The point of departure of a typical economic evaluation is a cost effectiveness analysis comparing the treatment effects and costs of two alternative treatments and resulting in an incremental cost effectiveness ration (ICER). The treatment effects can be measured in different ways relevant to the treatment, such as number of hospitalizations, mortality, or quality adjusted life years (QALYs). The ICER indicates the expected cost of avoiding, for example, a hospitalization. Spillover effects are usually not accounted for in costs effectiveness analysis, however Fletcher and Marksteiner (2017) propose the following ICER that considers spillover effects:

$$ICER_{\text{vaccine,no vaccine}} = \frac{C_{\text{vaccine}} - C_{\text{no vaccine}}}{(E_{\text{vaccine}} + mS_{\text{vaccine}}) - E_{\text{no vaccine}}}$$

where C is the cost, E is the effect (on treated individuals), m is spillover population share, and S is the spillover effect.

[30] Only smallpox has been fully eradicated using vaccines thus far.

Olsen and Jepsen (2010) conduct a cost-effectiveness analysis of introducing an HPV vaccination program similar to the actual program that was implemented.[31] They find an incremental cost of introducing the vaccine program of 1,675,982 Euro and an incremental gain in QALYs of 552.4. This results in an ICER of 3,034 Euro/QALY gained. Assuming that the spillover effect that we find constitutes around 10 percent of the direct effect we could expect the incremental QALY gained to increase by 10 and thereby reduce the ICER to 2,758 EUR/QALY.

This result obviously hinges on a number of assumptions but illustrates how spillover effects are important for the economic evaluation of vaccine programs in specific and public health programs more generally.

Earlier in Section 5.4, we showed how the spillover effects exacerbated the inequality in take-up across socio-economic groups. This highlights a trade-off where spillover effects can help achieve herd immunity and improve cost effectiveness results at the expense of an increase in inequality in health and illustrates the need for policy makers to account for spillover effects when designing and evaluating policy.

7 Conclusion

In this paper, we investigate the effects of the introduction of a population-wide vaccination program for the Human Papillomavirus (HPV) on the targeted group of 15-year-old girls and their older sisters in Denmark. Our study takes advantage of the fact that eligibility for the HPV vaccination program was determined by date of birth. Girls born on January 1, 1993, or later were eligible for the program whereas girls born earlier were not. We exploit the eligibility criterion to estimate both the direct and spillover effects of the program in a regression discontinuity framework. We leverage population-wide administrative data and find a direct effect on targeted girls of 50.5 percentage points and a spillover effect on ineligible older sister of 4.6 percentage points.

By profiling the compliers of the vaccination program and showing heterogeneity for both the direct and spillover effects we find evidence of a socioeconomic gradient in the vaccine take-up confirming previous studies in economics as well as public health and medicine. We also find evidence to suggest that the information material provided to families around the time of eligibility may be an important channel driving the spillover effects to older, ineligible sisters.

Our results are relevant from a policy perspective, as spillover effects and evidence on what drives them is important when evaluating public program efficacy (Fletcher and Marksteiner, 2017). We show that the estimated spillover effects are quantitatively important in terms of both the achievement of herd immunity and cost effectiveness. . Our study also provides important insights for policymakers concerned about health inequalities, as our results suggest that high-SES individuals are more responsive to information provided by health authorities and can extrapolate such that not only targeted girls but also their older sisters increase their take-up of preventive care. For equality considerations, the vaccination program under study here also exemplifies that direct and spillover effects may work in opposing directions; while the program lowered inequality in vaccine take-up among the targeted girls, it increased the inequality among the older sisters. This underlines how quantifying public program spillover is important; not only for the purpose of determining the program efficacy but also for determining the distributional effects of the program

[31] Specifically, Olsen and Jepsen (2010) consider vaccination of 12-year-old girls with a booster program to 15 years; see Appendix B for an in-depth description of the implementation of HPV vaccination programs in Denmark.

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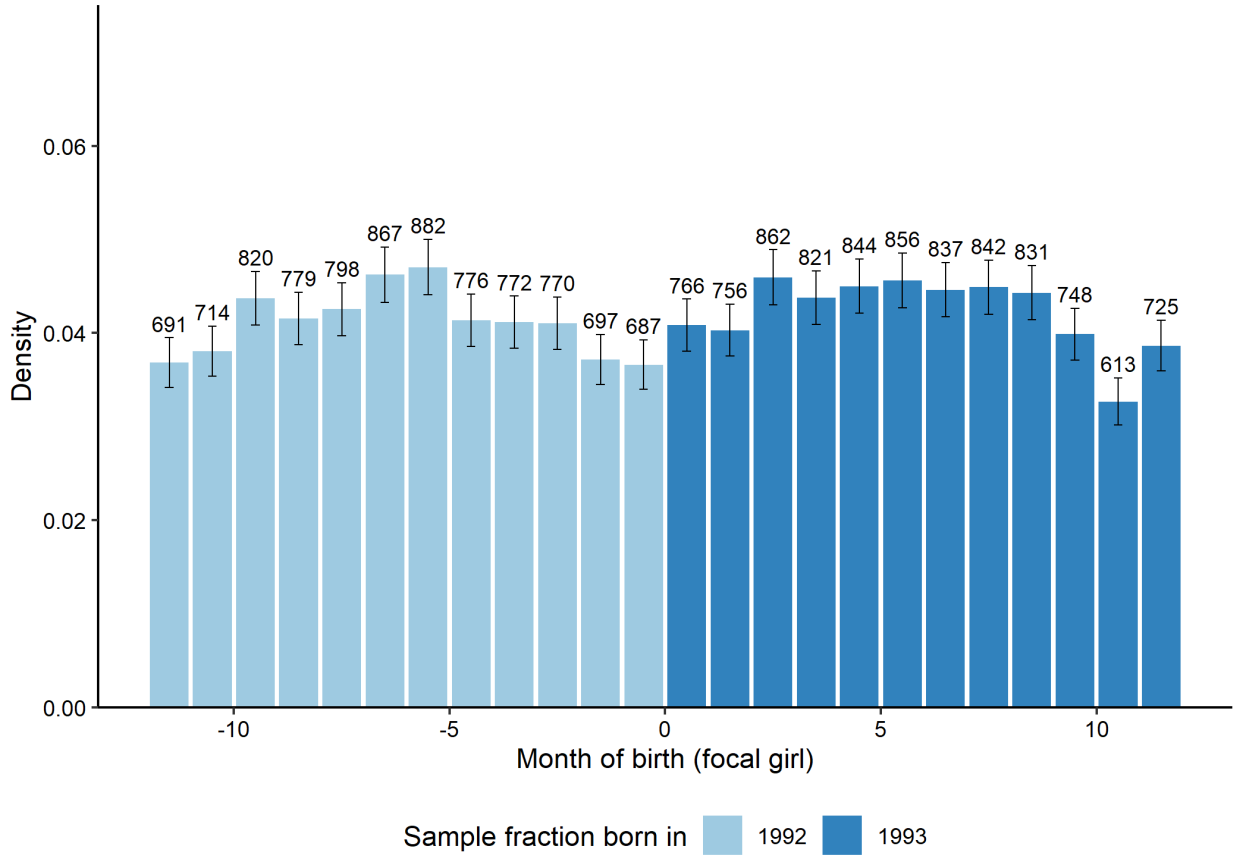
A Appendix Tables and Figures

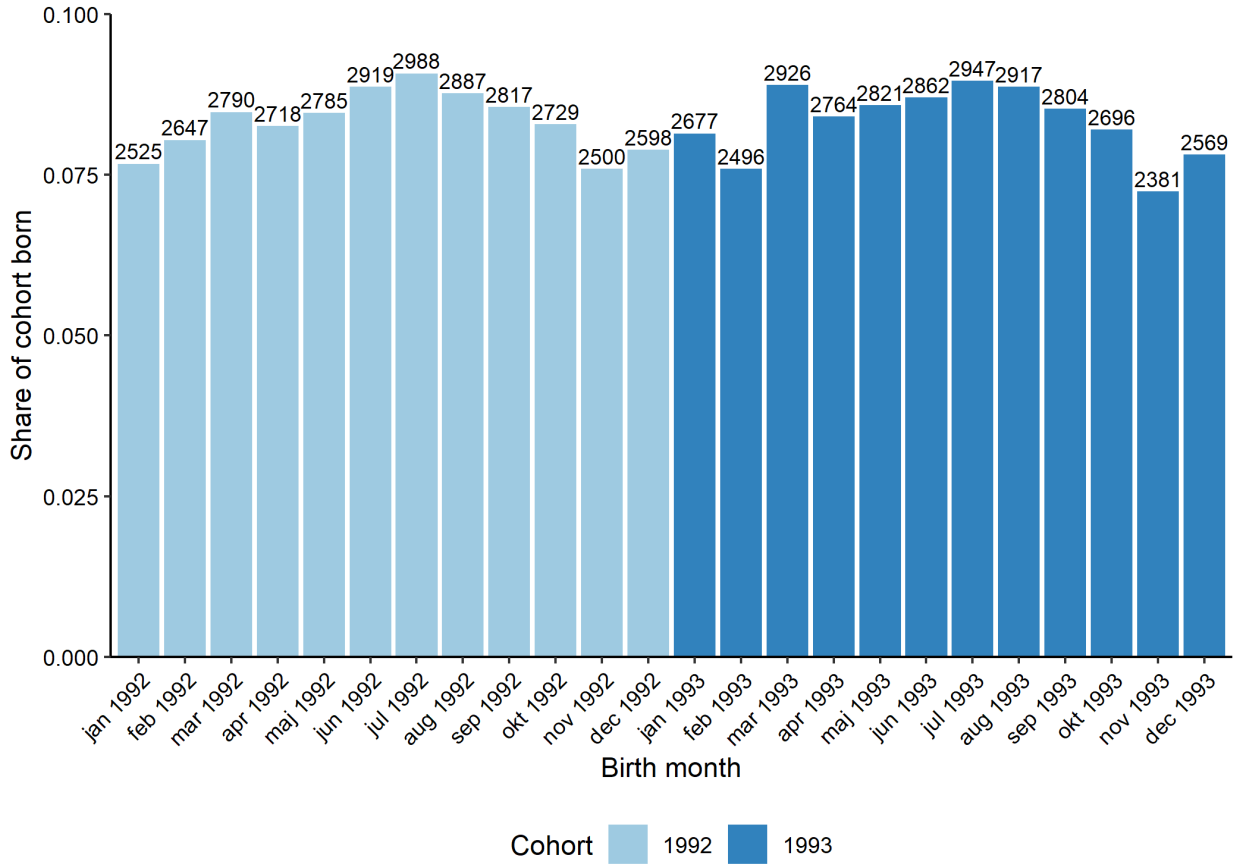
A.1 Appendix Figures

Figure A.1: Sample Density



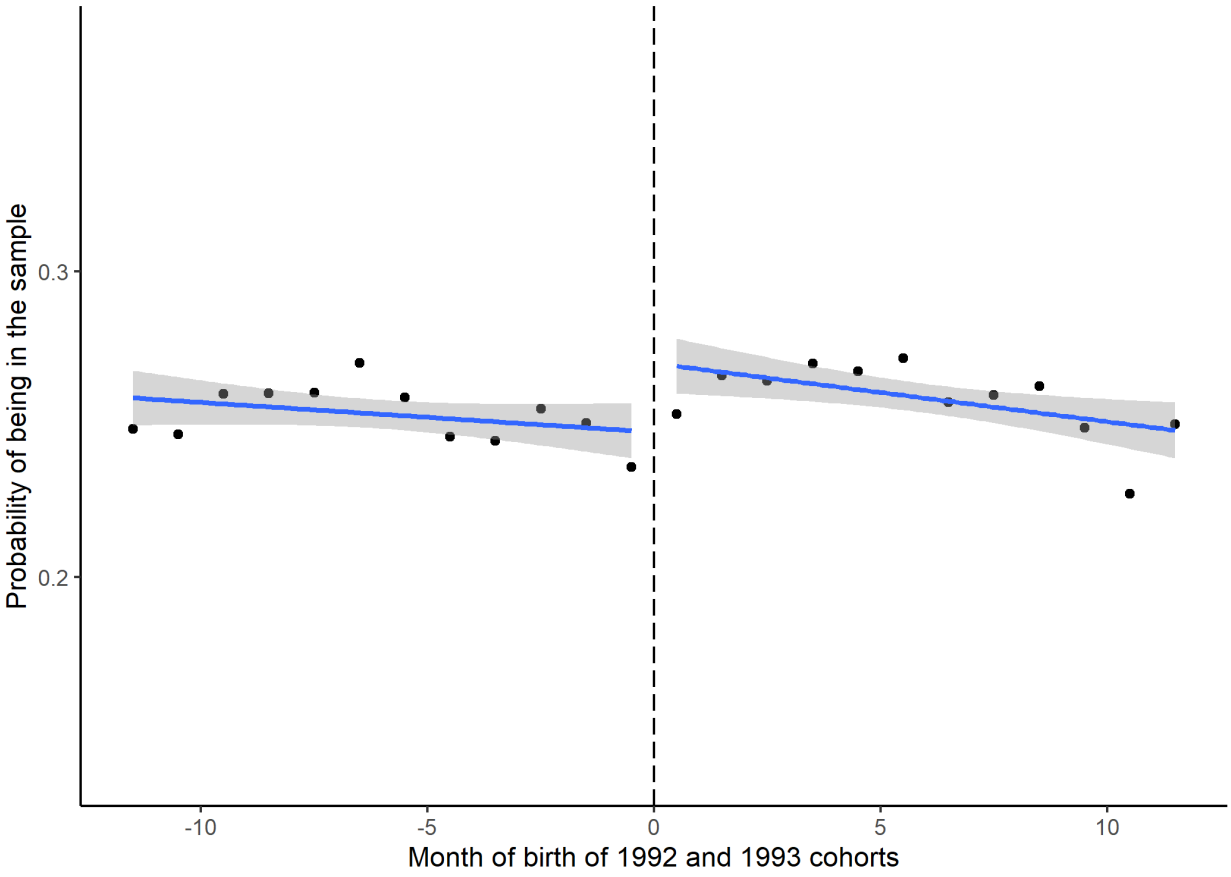
Notes: The graph shows the density of the sample relative to the birth month of the focal girls. Focal girls born in 1992 (1993) are placed to the left (right) of the cutoff. Focal girls born after the cutoff date are eligible for the HPV vaccine free of charge. Number of observations in each bar is depicted above each bar.

Figure A.2: Fraction of a Cohort Born in Each Month for the 1992 and 1993 Cohorts



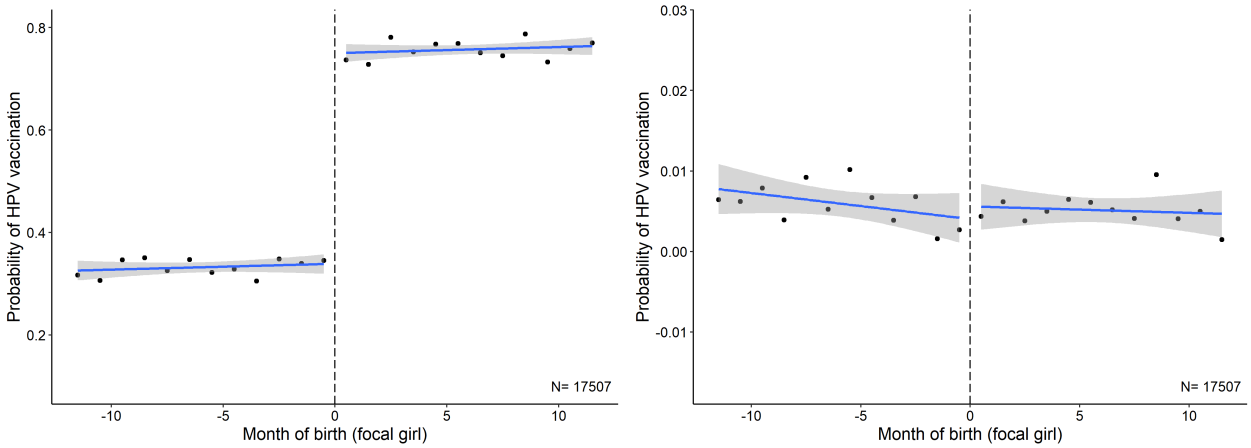
Notes: The figure shows the fraction born in each month for the 1992 and 1993 cohorts.

Figure A.3: Fraction of Girls in the 1992 and 1993 Cohorts with an Older Sister in the Sample



Notes: The figure depicts the fraction of focal girls born in 1992 and 1993 with an older sister born in 1985 to 1992 by month of birth (centered) of the focal girls.

Figure A.4: Direct and Spillover Effect on Brothers

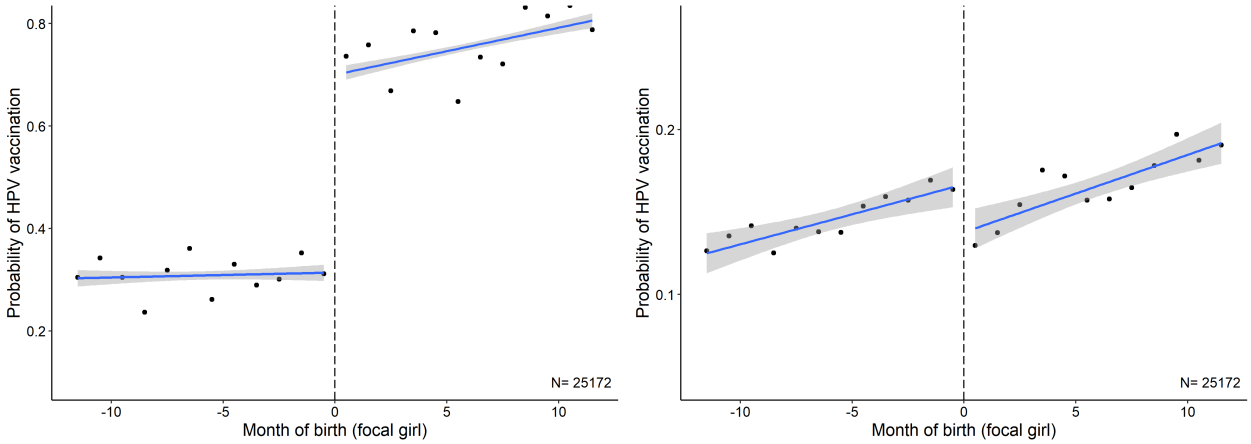


(a) Direct effect

(b) Spillover effect

Notes: The figure shows the probability of vaccination (first dose) for focal girls and their older, ineligible brothers. Panel A shows the probability of HPV vaccination (first dose) by month of birth (centered) for the focal girls, and Panel B shows the probability of HPV vaccination (first dose) for an older brother by month of birth (centered) of the focal girl. Each data point represents the fraction of either focal girls or peer brothers vaccinated. The dashed vertical line marks the eligibility cutoff (January 1, 1993) for the HPV vaccination program. Girls born prior to January 1, 1993, are ineligible for the vaccination program, whereas girls born January 1, 1993, or later are eligible for the program. Each plot is overlaid with fitted lines from a linear regression of HPV vaccine take-up on eligibility, month of birth, and an interaction of the two.

Figure A.5: Direct and Spillover Effect Female Cousins



(a) Direct effect

(b) Spillover effect

Notes: The figure shows the probability of vaccination (first dose) for focal girls and their older, ineligible female cousins. Panel A shows the probability of HPV vaccination (first dose) by month of birth (centered) for the focal girls and Panel B shows the probability of HPV vaccination (first dose) for a female cousin by month of birth (centered) of the focal girl. Each data point represents the fraction of either focal girls or peer cousin vaccinated. The dashed vertical line marks the eligibility cutoff (January 1, 1993) for the HPV vaccination program. Girls born prior to January 1, 1993, are ineligible for the vaccination program while girls born January 1, 1993, or later are eligible for the program. Each plot is overlaid with fitted lines from a linear regression of HPV vaccine take-up on eligibility, month of birth, and an interaction of the two.

A.2 Appendix tables

Table A.1: Sample selection

	Sample size		
Sample	**1992**	**1993**	**Total**
All women born	44722	43064	87786
All women born in Denmark	32903	32860	65763
Living in Denmark at least one year 2006-2012	32373	32297	64670
With older sister (at least 1 year older) (observed 2006-2007)			
Sibling pairs	14525	14775	29300
Accounting for older sister once	13977	14214	28191
Accounting for younger sister once	11232	11353	22585
With older sister (1 to 7 years older) (observed 2006-2007)			
Sibling pairs	9253	9501	18754
Accounting for older sister once	8939	9211	18150
Accounting for younger sister once	8332	8497	16829

Notes:

The table presents the sample size across cohorts throughout the sample selection procedure described in Section 4.2.

Table A.2: Descriptive statistics - comparison between sample and population

	Population		Sample	
Variables	**1992-1993**	**1985-1992**	**1992-1993**	**1985-1992 focal sister 1992-1993**
HPV-vaccine take-up				
1 dose [0,1]	0.553	0.191	0.484	0.192
3 doses [0,1]	0.502	0.146	0.445	0.146
Child characteristics				
Descendant [0,1]	0.063	0.045	0.091	0.091
Birth weight [grams]	3380.8	3368.8	3455.7	3363.8
	(606.6)	(594.9)	(599.4)	(584.8)
Birth weight missing [0,1]	0.006	0.006	0.004	0.033
Maternal characteristics				
Married [0,1]	0.365	0.601	0.546	0.608
Immigrant/Descendant [0,1]	0.057	0.062	0.09	0.105
Age at child birth [years]	28.868	28.209	29.746	26.162
	(4.642)	(4.758)	(4.208)	(4.091)
Basic education [0,1]	0.458	0.439	0.441	0.444
Vocational education [0,1]	0.313	0.313	0.31	0.308
Further education [0,1]	0.192	0.233	0.221	0.221
Yearly earnings [euro]	14940.1	14470.7	13302.9	13190.3
	(10977.9)	(11449.4)	(11103.1)	(11099.2)
Mother missing [0,1]	0.036	0.015	0.028	0.027
N	64670	231446	18754	18754

Notes:

The table presents descriptive statistics for the sample of focal girls and their corresponding older sisters as well as their corresponding population cohorts. Each cell represents the mean of the corresponding variable in the row with standard error in parentheses for non-binary variables.

Table A.3: Indirect test of sorting

Covariate	**Seperate regressions**	**Joint regression**
Focal sister characteristics		
Descendant [0,1]	0.02*	0.021
	(0.011)	(1.80)
Birth weight [grams]	17.797	17.95
	(21.948)	(0.82)
Maternal characteristics		
Married [0,1]	-0.01	-0.00945
	(0.018)	(-0.53)
Immigrant/Descedant [0,1]	0.01	0.0105
	(0.011)	(0.90)
Age at child birth [in days]	37.714	39.15
	(58.509)	(0.65)
Basic education [0,1]	0.019	0.0187
	(0.018)	(1.03)
Vocational education [0,1]	-0.014	-0.0138
	(0.017)	(-0.83)
Further education [0,1]	-0.004	-0.00445
	(0.015)	(-0.30)
Yearly earnings [euro]	273.733	253.4
	(398.659)	(0.63)
Peer sister characteristics		
Descendant [0,1]	0.016	0.0166
	(0.011)	(1.42)
Birth weight [grams]	43.597**	43.81*
	(18.812)	(2.33)
Age spacing [in months]	-0.938	-0.921
	(0.645)	(-1.43)
Family characteristics		
Children in the family	-0.061	-0.0593
	(0.134)	(-0.44)
Birth order	-0.101	-0.0994
	(0.133)	(-0.74)
N	18754	18754
Joint test (Chi2)		0.68

P-value	0.411

Notes:
The table shows the results of both separate regressions and a joint regression (SUR) of predetermined covariates on month of birth (the running variable), an eligibility indicator and an interaction of the two. All variables are predetermined (measured prior to birth of focal girl). The regressions include controls for missing variables. The bandwidth is 12 months. A triangular kernel is used. Standard errors are robust to clustering at the family level.
*** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level

Table A.4: Robustness of regression results 1

Effect	**Including donut around cutoff (1)**	**Epanechikov kernel (2)**	**Uniform kernel (3)**	**Birthday as running variable (4)**	**Clustering on birthday (5)**
Direct effect	0.499***	0.503***	0.497***	0.502***	0.505***
	(0.018)	(0.015)	(0.014)	(0.015)	(0.015)
	[0.23]	[0.229]	[0.229]	[0.229]	[0.229]
Spillover effect	0.069***	0.049***	0.052***	0.042***	0.046***
	(0.016)	(0.012)	(0.012)	(0.012)	(0.013)
	[0.159]	[0.159]	[0.159]	[0.159]	[0.159]
N	17301	18754	18754	18754	18754
Specifications					
Outcome	1 dose	1 dose	1 dose	1 dose	1 dose
Running variable	Month	Month	Month	Day	Month
Window	1 year	1 year	1 year	1 year	1 year
Covariates	Yes	Yes	Yes	Yes	Yes
Donut	Yes	No	No	No	No
SE clustering	Family	Family	Family	Family	Birthday
Kernel	Triangular	Epanechikov	Uniform	Triangular	Triangular

Notes:

This table presents estimates of the direct and spillover effects of eligibility for the vaccination program across different regression specifications. Standard errors are in parentheses. For the panel with the direct effects, the number in brackets is the mean HPV vaccine take-up of focal girls that are ineligible for the vaccination program (born prior to January 1, 1993). For the panel with the spillover effects, the number in brackets is the mean HPV vaccine take-up of older sisters with a younger sister who is ineligible for the vaccination program (born prior to January 1, 1993). All regressions include a running variable based on the focal girls birth day (aggregation level varies), an eligibility indicator and an interaction of the two.

*** Significant at the 1 percent level

** Significant at the 5 percent level

* Significant at the 10 percent level

Table A.5: Robustness of regression results 2

Effect	**3 dose vaccine outcome (1)**	**6 months window width (2)**	**2 years window width (3)**	**Full population born 1992/1993 (4)**	**No age restrictions on peer sisters (5)**	**Differences in Discontinuities (6)**
Direct effect	0.536***	0.516***	0.482***	0.394***	0.486***	0.486***
	(0.014)	(0.021)	(0.01)	(0.008)	(0.014)	(0.019)
	[0.173]	[0.231]	[0.227]	[0.339]	[0.233]	[0.228]
Spillover effect	0.041***	0.026	0.038***		0.032***	0.039**
	(0.011)	(0.017)	(0.008)		(0.009)	(0.017)
	[0.117]	[0.157]	[0.154]		[0.117]	[.158]
N	18754	9489	39900	64796	29300	67504
Specifications						
Outcome	3 dose	1 dose	1 dose	1 dose	1 dose	1 dose
Running variable	Month	Month	Month	Day	Month	Month
Window	1 year	6 month	2 years	1 year	1 year	1 year
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Donut	No	No	No	No	No	No
SE clustering	Family	Family	Family	Family	Family	Family
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

This table presents estimates of the direct and spillover effects of eligibility for the vaccination program across different sample selections. Standard errors are in parentheses. For the panel with the direct effects, the number in brackets is the mean HPV vaccine take-up of focal girls that are ineligible for the vaccination program (born prior to January 1, 1993). For the panel with the spillover effects, the number in brackets is the mean HPV vaccine take-up of older sisters with a younger sister who is ineligible for the vaccination program (born prior to January 1, 1993). All regressions include month of birth (the running variable), an eligibility indicator and an interaction of the two.

*** Significant at the 1 percent level

** Significant at the 5 percent level

* Significant at the 10 percent level

Table A.6: Heterogeneous effects by mother's characteristics

Category	Direct effect	Spillover effect	N
Mother's earnings quartile			
	0.502***	0.002	4616
Earnings quartile 1	(0.029)	(0.019)	
	[0.123]	[0.077]	
	0.553***	0.054**	4616
Earnings quartile 2	(0.029)	(0.024)	
	[0.16]	[0.113]	
	0.509***	0.065**	4615
Earnings quartile 3	(0.03)	(0.026)	
	[0.262]	[0.183]	
	0.451***	0.056*	4615
Earnings quartile 4	(0.032)	(0.03)	
	[0.368]	[0.259]	
Mother's educational level			
	0.525***	0.025*	8794
Basic education	(0.021)	(0.015)	
	[0.145]	[0.089]	
	0.564***	0.061**	5807
Vocational education	(0.026)	(0.023)	
	[0.226]	[0.162]	
	0.373***	0.068**	4153
Further education	(0.034)	(0.033)	
	[0.403]	[0.293]	
Mother has health education			
	0.519***	0.045***	15845
Other education	(0.016)	(0.013)	
	[0.202]	[0.136]	
	0.422***	0.05	2909
Health care education	(0.04)	(0.038)	
	[0.371]	[0.278]	
Mother's age at child birth			
	0.554***	0.037*	5505
Below 28	(0.026)	(0.019)	
	[0.14]	[0.097]	

	0.503***	0.062***	8829
28-32	(0.022)	(0.019)	
	[0.241]	[0.169]	
	0.438***	0.018	4420
Above 32	(0.033)	(0.03)	
	[0.326]	[0.221]	

Notes:

This table presents estimates of the direct and spillover effects of eligibility for the vaccination program across different subgroups of the sample. Standard errors are in parentheses. For the panel with the direct effects, the number in brackets is the mean HPV vaccine take-up of focal girls that are ineligible for the vaccination program (born prior to January 1, 1993). For the panel with the spillover effects, the number in brackets is the mean HPV vaccine take-up of older sisters with a younger sister who is ineligible for the vaccination program (born prior to January 1, 1993). All regressions include month of birth (the running variable), an eligibility indicator, an interaction of the two and all covariates listed in Table 1 as well as corresponding indicators for missing values. The bandwidth is 12 months. A triangular kernel is used. Standard errors are robust to clustering at the family level.

*** Significant at the 1 percent level

** Significant at the 5 percent level

* Significant at the 10 percent level

Table A.7: Heterogeneous effects by sibship characteristics

Category	Direct effect	Spillover effect	N
Age spacing between siblings			
	0.529***	0.051	1103
0-1	(0.087)	(0.078)	
	[0.206]	[0.201]	
	0.497***	0.065**	8738
2-3	(0.022)	(0.021)	
	[0.231]	[0.19]	
	0.517***	0.042*	5877
4-5	(0.029)	(0.024)	
	[0.234]	[0.14]	
	0.489***	-0.015	3036
6 or more	(0.037)	(0.025)	
	[0.222]	[0.088]	
Number of siblings			
	0.476***	0.099***	6135
2	(0.026)	(0.025)	
	[0.279]	[0.204]	
	0.531***	0.04*	6386
3	(0.025)	(0.022)	
	[0.246]	[0.174]	
	0.519***	-0.002	3220
4	(0.037)	(0.029)	
	[0.196]	[0.126]	
	0.483***	0.003	3013
+5	(0.036)	(0.02)	
	[0.125]	[0.068]	
Birth order of siblings			
	0.493***	0.076***	10099
2	(0.02)	(0.019)	
	[0.265]	[0.198]	
	0.549***	0.025	5284
3	(0.028)	(0.023)	
	[0.207]	[0.127]	
	0.437***	-0.037	1745
4	(0.05)	(0.032)	
	[0.163]	[0.106]	

	0.498***	0.016	1626
+5	(0.048)	(0.028)	
	[0.143]	[0.073]	
Older sisters living condition			
	0.508***	0.058***	14751
Lives at home	(0.016)	(0.015)	
	[0.243]	[0.187]	
	0.482***	-0.002	4003
Lives alone	(0.032)	(0.02)	
	[0.187]	[0.075]	

Notes:

This table presents estimates of the direct and spillover effects of eligibility for the vaccination program across different subgroups of the sample. Standard errors are in parentheses. For the panel with the direct effects, the number in brackets is the mean HPV vaccine take-up of focal girls that are ineligible for the vaccination program (born prior to January 1, 1993). For the panel with the spillover effects, the number in brackets is the mean HPV vaccine take-up of older sisters with a younger sister who is ineligible for the vaccination program (born prior to January 1, 1993). All regressions include month of birth (the running variable), an eligibility indicator, an interaction of the two and all covariates listed in Table 1 as well as corresponding indicators for missing values. The bandwidth is 12 months. A triangular kernel is used. Standard errors are robust to clustering at the family level.

*** Significant at the 1 percent level

** Significant at the 5 percent level

* Significant at the 10 percent level

B Eligibility for the HPV Vaccine for Girls in the Danish CVP 2008-19

The first HPV vaccine (Gardasil) was made available for sale in Denmark (and the EU) on September 20, 2006, (European Medicines Agency (EMA)). In 2007 the Danish Health Authorities recommended that the vaccine should be a part of the Childhood Vaccination Program. Embedding the vaccine in the CVP can be viewed in terms of a routine program and three catch-up programs. Figure B.1 shows when in time different cohorts of girls are eligible to get the vaccine through the CVP and whether it is part of the routine or the catch-up program.

To boost vaccine take-up when the vaccine was first embedded in the CVP, cohorts of girls born in the years 1993 to 1995 (age 13 to 15) became eligible as part of a catch-up program (Catch-up Program I). As part of this program these woman were eligible from October 1, 2008, up until the end of 2010.

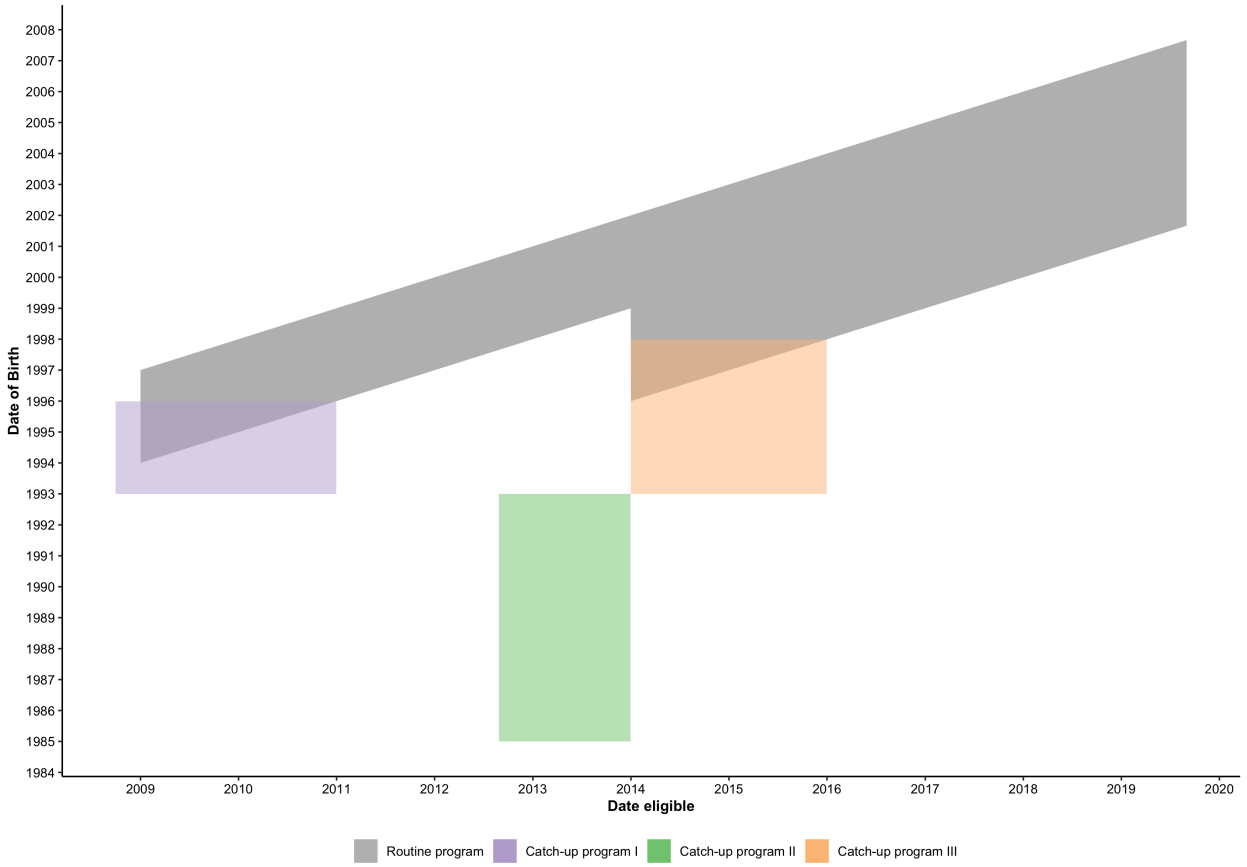
The routine program: From January 1, 2009, all girls aged 12-15 became eligible to get the vaccine as part of the CVP. In the routine program eligibility is based solely on date of birth; and on January 1, 2009, a 12-year-old girl would have a three-year window in which she was eligible to get the vaccine via the CVP. On January 1, 2014, the window of eligibility was extended from three years (12-15) to six years (12-18). The routine program is depicted in Figure B.1 as the grey area.

Catch-up Program I: On October 1, 2008, cohorts of girls born in the years 1993 to 1995 (then aged 13 to 15) became eligible. These cohorts were eligible to get the vaccine as part of the CVP until the end of 2010. This catch-up program was meant to supplement the initiation of the routine program. It is the effect of this program that is studied in this paper. Catch-up program I is depicted in Figure B.1 as the green area.

Catch-up Program II: On August 27, 2012, cohorts of girls born in the years 1985 to 1992 (then aged 19 to 27) became eligible. The program ended December 31, 2013. Catch-up program II is depicted in Figure B.1 as the purple area.

Catch-up Program III: In 2014 and 20015 cohorts of girls born 1993 to 1997 (then aged 21 to 17) again became eligible. Catch-up program III is depicted in Figure B.1 as the orange area.

Figure B.1: Eligibility of girls for the HPV-vaccine as part of the Danish CVP



Notes: This figure shows when in time different birth cohorts of girls in Denmark were eligible to receive the HPV vaccine free of charge as part of the Danish CVP.