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ABSTRACT

We explore physicians' role in moderating compliance with recommended vaccinations. Using administrative data on the universe of Danish children and their healthcare providers, we first construct and validate a measure of providers' propensities to comply with recommended vaccinations from birth to age 6 based on a two-way fixed effects model. We then show the measure meaningfully affects uptake of the Human Papillomavirus (HPV) vaccine among adolescent patients, and speeds recovery from a media-induced crisis to perceived HPV vaccine safety. Providers affect decisions beyond those of their own patients, influencing patients' younger cousins' uptake by one-fifth as much as own patients.

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1. Introduction

Childhood immunization programs are among the most effective preventative public health measures. Yet, achieving compliance with recommended vaccinations has been notoriously difficult. The consensus from the literature elucidating factors that promote and deter compliance is that primary care providers play an important role (e.g., Carpenter and Lawler, 2019; Kessels et al., 2012; Moghtaderi and Adams, 2016; Omer et al. 2009). Most recently, physician attitudes towards COVID-19 vaccines are shown to affect their patients' vaccination rates (Steinmayr and Rossi, 2022). As the site for many preventative healthcare investments, the primary care provider is the natural place for patients to turn. However, physicians vary in their abilities to communicate the importance of vaccinations, the determination with which they promote the benefits, and general attitudes towards vaccinations (Callaghan et al., 2022; Dempsey et al., 2018; Paterson et al., 2016). In this paper, we explore the role of providers in influencing vaccine uptake among their own patients, moderating the impacts of unwarranted shocks to confidence in vaccine safety, and affecting the choices of individuals in their patients' familial networks.

Using administrative data on the universe of Danish children and their healthcare providers, we first construct and validate a measure of providers' propensities to induce compliance with recommended vaccines from the early part of the Danish childhood vaccination program (that applies to ages 0-5). Taking advantage of age-varying recommendations, our provider vaccination propensities are estimated using a two-way fixed effects model that allows for unobserved patient and provider heterogeneity, and that is identified from patient transfers. Despite imposing simplifying assumptions, such as provider impacts being additive and constant over time, we show that much of the variation across providers (i.e., clinics) can be interpreted as causally affecting patients. For example, for child patients aging into new vaccine requirements who switch to new providers, about 75% of the upcoming change in estimated provider vaccination propensities (PVPs) is reflected in the change in own vaccine compliance. A one standard deviation increase in PVP is associated with a 1.8 percentage point increase in the fraction of recommended vaccine visits that have been completed, which reflects a 16% reduction in noncompliance. The variation across providers is more than 10% as great as the variation across families.

We next explore the determinants of providers' propensities. Greater scale, in terms of the number of providers in the clinic, is associated with greater provider compliance. Female

providers also induce more compliance. Patients of high-propensity providers are more likely to adhere to other childhood preventative care schedules and, to a lesser extent, preventative care recommended for older populations. Another important variable differentiating providers is reflected in the vaccination decisions they make for their own children. A one standard deviation increase in the average up-to-date status of own children is associated with a 0.08 standard deviation increase in PVP.

As far as implications for patients, we move on to show that the constructed measure of provider vaccination facilitation meaningfully affects uptake of the Human Papillomavirus (HPV) vaccine among girls in the adolescent (age 12) phase of the childhood vaccination program. A girl with a provider whose PVP is one standard deviation above average is more than 3% more likely to have initiated the vaccination series within 6 months of eligibility, which is important since the vaccine's effectiveness in preventing future cancers depends on administration prior to sexual debut. Additionally, we find that higher-propensity providers speed recovery from a media-induced crisis that reduced perceived HPV vaccine safety. When one-year uptake fell by more than 40 percentage points for cohorts exposed to an influential but unsubstantiated documentary about possible side effects, the gap in the uptake of patients of top- versus bottom-quartile providers widened by 5 percentage points. Finally, we demonstrate that providers affect decisions beyond those of their own patients, influencing HPV vaccine uptake for patients' younger cousins affiliated with other providers by about one-fifth as much as own patients. Taken together, our results provide novel quantitative evidence on the importance of physicians' abilities to facilitate compliance with vaccination recommendations among those in their spheres, and thus to help or hinder progress toward achieving public health goals.

Our study contributes to several related literatures. The first is the literature on policies and interventions targeted at increasing childhood vaccination rates. Rates are found to respond to school and childcare vaccination requirements (Carpenter and Lawler, 2019; Hair et al., 2021b) and permitted exemptions (Churchill, 2021a; Hair et al., 2021a; Richwine et al., 2019), insurance coverage and financial incentives (Banerjee et al., 2021; Churchill, 2021b), and media campaigns (Hansen et al., 2020; Loft et al., 2020).[1] Most related to our work is the evidence related to levers

[1] A related recent literature studies the role of mandates, incentives, and media campaigns in COVID-19 vaccine uptake (e.g., Barber and West, 2022; Chang et al., 2021; Galasso et al., 2022; Ho et al., 2022; Karaivanov et al., 2022).

primary care physicians can pull, such as providing reminders and communicating the importance of compliance to their patients. Reminder letters and text messages have been found to be effective (Banerjee et al., 2021; Hirani, 2021; Hirani and Wüst, 2023; Milkman et al., 2021), as has training providers to promote vaccination and manage patient hesitancy (Brewer et al., 2021; Real et al., 2022; Szilagyi et al., 2021).

We also contribute to the growing literature documenting variation in primary care provider practice styles and the implications for patient outcomes. It is established that there is geographical variation in healthcare, largely driven by supply-side factors, including physician beliefs about treatment (Callison et al., 2021; Currie and Zwiers, 2021; Cutler et al., 2019; Deryugina and Molitor, 2021; Finkelstein et al., 2016). Efforts to isolate the causal impacts of primary care physicians reveal significant impacts on the medical decisions and health outcomes of their patients.[2] In the US context, Fadlon and Van Parys (2020) find that primary care physicians have large and long-lasting effects on both healthcare utilization and quality of care, and Currie and Zhang (2021) show that the most effective primary care physicians utilize fewer resources. In the Norwegian context, Ginja et al. (2022) similarly find that higher-quality primary care providers (in terms of reduced patient mortality) have lower per-patient costs. In Denmark, Huang and Ullrich (2021) find that physician practice style accounts for more than half of the between-practice variation in antibiotic consumption and that low prescribing style has no adverse effects on patient health, whereas Simeonova et al. (2020) demonstrate that a physician's ability to facilitate adherence with prescription drugs improves patient health.[3]

Finally, our paper is related to the literature on spillovers in health behaviors, particularly with respect to the decision to get vaccinated. Ibuka et al. (2014) find evidence of free riding using a laboratory experiment: the probability to get vaccinated is lower in groups with higher vaccination rates in previous rounds. Similar free-riding behavior is found outside of the lab for deworming pill take-up (Kremer and Miguel, 2007). Despite these marginal disincentives from peer uptake of contagious-disease preventions, peers are found to be positive influences for vaccination both in developing (Sato and Takasaki, 2019) and developed (Humlum et al., 2022;

[2] Studies find important impacts of specialists as well (Currie et al., 2016; Gowrisankaran et al., 2022), and benefits to improving their diagnostic skills (Chan et al., 2022; Currie and MacLeod, 2017).
[3] Interestingly, Frakes et al. (2021) find that doctors as patients are only slightly better than the general population at obtaining cost-effective care, and Finkelstein et al. (2022) find that doctors and their close relatives are, in fact, less likely to adhere to medication-related guidelines.

Ibuka et al., 2018; Rao et al., 2007) country contexts. Notably, since HPV is sexually transmitted, our case of learning from cousins is one where scope for free riding is largely shut down.

The remainder of the paper proceeds as follows: Section 2 provides background on childhood vaccination and primary care in Denmark. Section 3 describes our data and how we measure compliance with vaccination recommendations. Section 4 lays out our strategy for estimating provider vaccination propensities under the early phase of the childhood vaccination program. The section also provides evidence that the estimates have validity. Section 5 explores the correlates of provider vaccination propensities, while Section 6 documents the implications of provider propensities for their adolescent patients. Section 7 concludes.

2. Background on the childhood vaccination program and primary care in Denmark

In Denmark, the childhood vaccination program is part of universal healthcare that is publicly provided and free of charge. All recommended vaccines, which currently cover 10 infectious diseases, are either administered or overseen by the child's primary care provider.

The vaccination program consists of an early childhood phase (ages 0-5) and an adolescent phase (age 12). Table 1 shows the vaccination schedules that applied to the children born between 1997 and 2013 from ages 0-5. Several vaccination recommendations coincide with the timing of regular physician check-ups, and the child is sometimes given multiple shots during one visit. A key change in the number of vaccination visits required for young children born after March 2004 is the addition of a visit at age 4, when the second measles-mumps-rubella (MMR) shot was shifted down from the previously recommended age of 12. For adolescents, the HPV vaccine was incorporated in 2009 (first affecting those born in 1996) and was covered only for age-eligible girls until recently.[4]

While there are no mandatory vaccination requirements to attend day care or school, the last column in Table 1 shows that compliance with the childhood vaccines is generally high, typically near the targets that range from 90-95% coverage. However, there have been cases where noncompliance in terms of delaying or failing to initiate vaccination has raised public health concerns. Some of the under-vaccination has been attributed to parents forgetting to vaccinate, particularly for the shots at older ages that are not timed with regular check-ups. In

[4] Boys turning 12 on or after July 1, 2019, have also been offered the vaccine in the childhood vaccination program.

response to rates for the second MMR vaccination and diphtheria-tetanus-pertussis-polio (DTaP/IPV) booster falling below 90%, public health authorities were able to increase compliance by sending reminders to parents whose children were not up to date (Suppli et al., 2017). On the other hand, the results presented in Hirani (2021) indicate that vaccine hesitancy is the main reason for under-vaccination in the Danish context.

The more dramatic anti-vaccine episodes in Denmark have had to do with highly publicized but unsubstantiated concerns about safety due to possible side effects. First, the MMR vaccine was the subject of substantial media attention when Wakefield et al. (1998), which has since been retracted, postulated a link between the vaccine and autism. Over the subsequent five years, Hansen et al. (2019) find that months with more extensive pro-vaccination media coverage are associated with recoveries in uptake. Second, though early uptake of the HPV vaccine was around 90% for the first few eligible cohorts, uptake fell to 80% in 2014 due to media publicity of reports about possible adverse side effects (Suppli et al., 2018). There were then more dramatic declines in coverage (to around 50%) following the March 2015 airing of a particularly influential documentary skeptical of the vaccine (Hansen and Schmidtblaicher, 2021; Humlum et al., 2021). Take-up has recovered following the "Stop HPV, Stop Cervical Cancer" information campaign launched in May 2017 to build confidence in the vaccine and remind parents of its importance (Hansen et al., 2020; Loft et al., 2020).

In addition to concerted nationwide efforts, there are several channels through which individual providers can influence the immunization rates of their patients. Some have to do with knowledge and competence, such as communicating with parents about upcoming scheduled care and providing information about the vaccines. Some have to do with attitudes. For example, unvaccinated patients tend to have doctors who lack confidence in the safety of vaccines and the health benefits to individuals and communities (Omer et al., 2009). Under-vaccinated patients also tend to have doctors who are hesitant to follow the recommendation to administer multiple doses at the same time when children fall behind (Pedersen et al., 2020). One of our goals is to quantify these differences across providers, in terms of how they translate into systematic differences in patient compliance with vaccine recommendations across practices.

The providers we study are general practitioners who operate family primary care clinics. These providers are self-employed contractors. They are compensated partly by capitation (about 30% of income) and partly on a fee-for-service basis (about 70% of income), with the fees

centrally bargained. The number of practicing physicians per region is regulated by the central government, including management of the medical school pipeline and restrictions on the number of licenses. Setting up a practice is expensive and there is relatively little turnover prior to retirement (Hasvold, 2015).

The matching between patients and providers is based on geographic markets defined by patients' residential locations. Patients can freely choose between primary care physicians who are operating within a certain distance from where they live and accepting new patients. Transfers are subject to a nominal switching fee, or for no cost if the family moves to a new area that is sufficiently far away. These choices are made in a context where public information about physician practices is quite limited, restricted to factors such as the number of physicians, their genders, and tenures. At birth, children are assigned to their mothers' physicians by default. These providers manage standard preventative healthcare and serve as gatekeepers for specialized healthcare as long as the children remain on their patient lists.

3. Analysis datasets and measurement of vaccination compliance

In this section, we describe how we construct our two analysis datasets and how we measure compliance with vaccination recommendations in each one. First, we use a child sample to estimate provider vaccination propensities, leveraging the age-varying recommendations under the early childhood phase of the vaccination program. Then, we use an adolescent sample to estimate impacts of these propensities during the later age-12 phase of the vaccination program, which was subject to a crisis of confidence during our sample period. Both datasets are constructed by merging data from a variety of administrative registries.

3.1 Child dataset

The child dataset that we use to estimate provider vaccination propensities contains information on all males and females born in Denmark in the years 1997 through 2013.[5] This constitutes more than 1 million children.[6] We follow these birth cohorts through to their sixth birthdays. Then, for each quarter since birth, we determine the child's vaccination coverage as of the end of the quarter, yielding a quarterly panel.

[5] We condition on the child residing in Denmark for the first seven calendar years of life. We drop the 0.1% of children for whom we cannot identify the mother.
[6] Appendix Table A1 provides summary statistics for parental background characteristics for this sample.

We operationalize child compliance with the applicable recommended vaccination schedule by calculating the fraction of vaccination visits completed relative to the number that should have been completed.[7] We use the number of visits with recorded vaccinations rather than the number of vaccinations, since nearly all patients who receive a single childhood vaccine on a visit also receive the second if two are recommended.[8] An advantage of this compliance measure is that it is more continuous than a simple indicator for being up-to-date. It also captures adherence to recommendations in terms of both timing and completion. It is important that children get vaccinations at the recommended ages to minimize the risk of disease (Gras et al., 2016; Pesco et al., 2015) and adverse reactions to the vaccine (Hambidge et al., 2014).

Figure 1 shows the average dynamics of compliance by quarter since birth. All are compliant in the first quarter since there are no requirements prior to 3 months of age. The share of required visits completed dips when children age into new requirements, and then recovers as they are vaccinated. Figure 2 shows that less than a third of mothers are fully compliant (on a quarterly basis) with their children's vaccinations. Average up-to-date status across child-quarters for mothers is 0.89, with a standard deviation of 0.14 across mothers.

When linking children to providers, we treat the provider the child is last affiliated with as the provider in any given quarter, following the algorithm developed by Kjærsgaard et al. (2016).[9] An important limitation is that, if the physician is part of a group practice, the identifier is for the practice and not the specific physician. Approximately 3/4 of physicians operate in solo practices, and group practices typically consist of 2-3 physicians. Thus, in some cases our provider vaccination propensities capture practice-wide propensities, rather than individual physician propensities.

3.2 Adolescent dataset

Relative to the child panel, the adolescent dataset consists of a narrower sample of girls born 1997 to 2007 (n=346,493) observed in a several-year window around their 12th birthdays. These

[7] For the relatively rare cases where the number of visits exceeds the number recommended, we classify the child as being fully compliant.

[8] For each patient visit, primary care physicians register codes for the services provided to receive reimbursement. The codes are recorded in the health insurance register and reflect both the type and the dose of the administered vaccine. These data were used to assess official vaccination coverage prior to the implementation of a new national vaccination register in 2015. The Ministry of Health showed that the computed coverage for the DTaP/IPV booster shot administered at age 5 was about 3-4 percentage points below actual coverage (Statens Serum Institut, 2012). We thus expect to underestimate vaccination coverage by a few percentage points.

[9] If the child's provider is missing, we set this equal to the maternal provider.

girls turned 12 after the HPV vaccine was incorporated into the vaccination program and are old enough that uptake can be observed in the data.[10]

To measure compliance with the HPV vaccination recommendation, we focus on initiation of the series within 6, 12, and 18 months of first becoming eligible at age 12. Though we could apply other definitions, such as completing all required shots, the choice is not consequential in this setting since there are very high completion rates conditional on starting the series. Conditional on completing the series at any point, approximately 90% had the first shot within the first 12 months of eligibility. Further, only a few additional percent initiate by 18 months (76%) relative to 12 months (70%). That most girls get the first shot soon after turning 12 years old is consistent with the recommendation to get the vaccine before sexual debut to maximize its effectiveness.[11]

To evaluate the role of providers in HPV vaccination uptake, we link these girls to their providers at the time they turn age 12. Notably, their providers' vaccination propensities are characterized based on their performance under the childhood phase of the vaccination program, not the adolescent phase.

4. Estimation of provider vaccination propensities

In this section, we describe the strategy that we use to construct a measure of provider effectiveness under the early childhood phase of the vaccination program, and the steps that we take to validate this measure.

4.1 Two-way fixed effects strategy

A provider's practice style or attitude toward vaccines is not directly observable, and therefore we need a proxy for it. One possibility is to use the observed vaccination rate of a provider's patients. The immediate concern is that patients are not generally randomly assigned to primary care providers. On the contrary, in Denmark they are chosen by individuals from a choice set determined by residential addresses. Hence, if individuals with certain characteristics sort into specific neighborhoods, they will tend to sort into providers as well, which implies that individuals with the same provider might behave similarly in terms of vaccination behavior for reasons unrelated to the provider. Thus, it is essential to control for patient composition and their

[10] Appendix Table A2 provides summary statistics for parental background characteristics for this sample.
[11] Appendix Figure A1 shows uptake by months relative to eligibility for the first 12 months.

proclivities when estimating provider vaccination propensities.

We choose to do this in a framework designed for capturing unobserved heterogeneity in two dimensions – the patient and the provider. These two-way fixed effects models were pioneered by Abowd, Kramarz, and Margolis (1999) in the context of workers and firms. They have since been frequently implemented in labor market settings to estimate firm productivity (e.g., Card, Heining, and Kline, 2013), education settings to estimate teacher or school leader effectiveness (e.g., Dhuey and Smith, 2014), and have more recently been applied in healthcare settings to estimate provider quality or practice style (e.g., Markussen and Røed, 2017).

To implement this strategy in our quarterly child panel, we assume that vaccination compliance can be described by the following two-way fixed effects model:

$$U2D_{imjq} = \alpha_m + \gamma_{j(i,q)} + \delta_{q(i)} + X_i\beta + \varepsilon_{imjq} \quad (1)$$

where $U2D_{imjq}$ is the up-to-date status (i.e., fraction of recommended vaccination visits completed) for child *i* with mother *m* affiliated with provider *j* in quarter q. Here, $q = [2, \dots, 24]$ indicates the quarter relative to the birth quarter, and fixed effects at that level ($\delta_{q(i)}$) absorb the evolution of requirements and average compliance as children age.[12] Since medical decisions for young children are made by their parents, we include patient fixed effects at the mother rather than the child level. The patient fixed effects (α_m) pick up the contribution of unobserved time-invariant traits, such as attentiveness to preventative care or vaccine hesitancy. We account for predictable differences in preventative care across children within a family by including the child's gender, birth order, and year of birth in the control set (X_i).[13] The primary care provider fixed effects ($\gamma_{j(i,q)}$) capture the extent to which providers facilitate patient compliance with vaccination guidelines.

Identification of the provider fixed effects comes only from patients who move across providers. Further, the values are only comparable within sets of providers that are connected by patient mobility, since they are estimated relative to an arbitrary omitted provider. Fortunately, in Denmark, the patient-provider relationship is characterized by substantial churning for families with children of these ages for several reasons, including residential moves when there are

[12] We exclude the first quarter (q = 1) since no vaccines are recommended until the child turns 3 months old.
[13] Studies find differences in vaccination and other early healthcare investments by gender (e.g., Barcellos, 2014; Borooah, 2004; Jayachandran and Kuziemko, 2011) and birth order (e.g., Lehmann et al., 2018; Pruckner et al., 2021).

changes in family structure or when children first enroll in school. Patient transfers are frequent enough and our sample size is large enough that nearly all providers (99.8%) are connected to one another. Depending on the year of birth of the child, the vaccination program consists of either 5 or 6 recommended visits during early childhood, which provides ample variation for us to utilize patient transfers across providers for identification of provider propensities.

Ideally, we would like to interpret our estimated provider propensities as reflecting the causal impact a provider has on patient childhood vaccination compliance. This interpretation relies on three key assumptions underlying the statistical model in equation (1). These are that i) all patients of the same provider get the same compliance boost (or drop), ii) patient-provider sorting is fully explained by the fixed effects and covariates, and iii) any unmodeled place-based effects are uncorrelated with providers' propensities. Before conducting tests for additivity, conditionally exogenous provider-to-provider mobility, and correlated place-based effects, which use our provider fixed effects estimates as inputs, we first explain how we account for measurement error in those estimates.

4.2 Accounting for measurement error

Our provider vaccination propensities (PVPs) are the ordinary least-squares estimates of the provider fixed effects from equation (1), recentered around the mean across providers.[14] The fact that these are estimates means that the variance will be overstated since it includes both the true variance and the estimation error, and that there will be attenuation bias if these estimates are used as explanatory variables (Andrews at al., 2008). Since the provider fixed effects are more precisely estimated the more patient transfers there are, providers observed with fixed effect estimates in the tails will tend to be those serving the fewest patients.[15]

The first strategy that we use to account for measurement error is to shrink our estimates. This strategy was introduced to the health economics literature by McClellan and Staiger (2000), who use empirical Bayes techniques to adjust estimates of hospital quality.[16] The basic logic is that less reliable estimates are shrunk back toward the mean (of zero) by multiplying them with an estimate of the signal to signal-plus-noise ratio. In the spirit of Chetty, Friedman, and Rockoff

[14] To overcome memory constraints, we use an algorithm (Paulo and Wolak, 2016) to compute the estimates of the patient and provider fixed effects corresponding to equation (1). This algorithm does not deliver standard error estimates that might otherwise have been inputs to measurement error correction.
[15] This "limited mobility bias" issue is illustrated in Appendix Figure A2.
[16] Chandra et al. (2016) and Fadlon and Van Parys (2020) are related recent health applications that use empirical Bayes shrinkage methods to address measurement error.

(2014) and Bacher-Hicks and de la Campa (2020), we use best linear prediction to carry out the shrinkage. These authors show that predicting current period estimates from a regression on past and future estimates (for teachers and police commanders, respectively) is closely analogous to empirical Bayes shrinkage, since the coefficients pick up the reliability of the estimates. Rather than use estimates from different periods, however, we use estimates from different sample splits.

Specifically, we randomly split the set of mothers into two samples, and then estimate equation (1) separately on each sample split. After mean-centering these estimates across providers, we then regress the estimated PVPs from the first sample on those from the second, separately by bins of the number of identifying patient transfers:

$$(2) \qquad \hat{\gamma}_{jb}^{1} = \delta_b \hat{\gamma}_{jb}^{2} + \varepsilon_{jb}$$

where $\hat{\gamma}_{jb}^{s}$ denotes the estimate for provider j from sample $s = \{1,2\}$ in bin $b = \{1,2,\dots,7\}$. The slope of the relationship in equation (2) can be interpreted as the amount of signal in the estimates. As expected, the correlation between the provider fixed effect estimates across the two samples is lowest (with a slope of around 0.2) for the bottom bin with the fewest transfers and then increases (to around 0.7) for higher bins.[17] We then use the bin-specific slope estimates to extract the predicted values:

$$(3) \qquad \tilde{\gamma}_{jb} = \hat{\delta}_b \hat{\gamma}_{jb}^{2}$$

These predicted values are the shrunken fixed effect estimates, denoted $\tilde{\gamma}_{jb}$.

Figure 3 shows the distribution of shrunken PVPs across our sample of 3,701 practices.[18] While the standard deviation of the original fixed effects estimates is 0.021, the standard deviation of the shrunken estimates is 0.016. The former includes estimation error and overstates the variability in provider effectiveness, and the latter excludes prediction error and understates it (under some assumptions). Applying the approach developed by Kline et al. (2020) to calculate an unbiased estimate of the standard deviation yields a magnitude of 0.018. In the context of the statistical model, a natural way to interpret this magnitude is in terms of implications for patient

[17] Appendix Figure A3 shows how the estimate of the slope varies across the seven provider subsamples defined by the number of identifying patient transfers in the second sample split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+).

[18] Appendix Figure A4 compares this distribution to those from the unadjusted estimates to illustrate how the shrinkage works. Appendix Figure A5 shows the variance in the shrunken estimates is less sensitive to the number of patient transfers, as expected. The remaining decline as the number of transfers increases is likely attributable to larger practices having more physicians, since the estimates average out any within-practice heterogeneity.

compliance. For example, a provider that is one standard deviation above average in terms of vaccination facilitation maps to an increase in the average up-to-date status of child patients of 1.8 percentage points.

While we sometimes use the shrunken estimates directly, such as to divide providers into deciles or quartiles, the second strategy we use to address measurement error is to estimate instrumental variables analogues when the propensity is included as a continuous explanatory variable. That is, we include the PVP estimate from the first sample split (of mothers) and instrument using interactions between the PVP estimate from the second sample split and bins for the number of patient transfers. Split-sample approaches recently used for bias correction in the worker-firm literature, when large samples sizes and high mobility support such an approach (e.g., Drenik et al., 2023; Goldschmidt and Schmieder, 2017), have been framed from an instrumental variables perspective. When we include our PVP estimates as regressors, our results are very similar whether we directly include our shrunken estimates or instead use the instrumental variables approach.

4.3 Validity of the provider vaccination propensity estimates

We now turn to validating our PVP estimates. Our estimate for any given provider is the best linear predictor of the compliance with recommendations of the provider's child patients. This will not necessarily measure the expected causal effect in the presence of provider-patient match quality, endogenous dynamic sorting, or correlated place-based effects.

We start by providing evidence in support of additivity. With respect to provider effectiveness by child's age, Figure 4 depicts the evolution of children's up-to-date status in the first 6 years of life by PVP quartile, revealing that the gaps in patient compliance are quite stable across ages. At every age, the patients of the providers with the lowest (bottom quartile) propensity to vaccinate have substantially lower probabilities of being up to date. While the gaps are less dramatic, average up-to-date status is ordered as expected across the other three quartiles.

To get at provider effectiveness across more and less compliant patients, we apply the method put forward by Card, Heining, and Kline (2013). That is, we calculate average residuals from equation (1) across deciles of mother and (shrunken) provider fixed effects. These average residuals can be interpreted as average match effects between patients and providers within cells, which is something the statistical model ignores. If additivity holds, they should all be close to

zero. The patterns from carrying out this exercise do reveal some match effects, with more (less) effective providers having greater positive (negative) impacts on less compliant patients.[19] However, average residuals are generally small relative to the variation in estimated provider propensities, other than for the least compliant bottom-decile mothers and for bottom decile providers, suggesting that imposing additivity is a reasonable simplification.

Next, we consider whether mobility across providers is plausibly conditionally exogenous. The provider fixed effect estimates will be biased if provider-to-provider mobility is systematically related to time-varying unobserved determinants of compliance, such as if patients move to providers that are better facilitators when they become more concerned about preventative care. Importantly, the model does accommodate sorting based on time-invariant factors, such as if families that prioritize preventative care tend to choose doctors that also do. As a first tactic, we estimate provider fixed effects on subsamples where transfers are less likely to be driven by relationships with providers, such as when households make residential moves or clinics close. The correlations between our baseline estimates and estimates based on the mover and clinic closure subsamples are relatively strong, at 0.56 and 0.57, respectively.[20] When analyzing how PVPs estimated on the childhood sample affect adolescent compliance, we test sensitivity to using PVP estimates from these and other alternative subsamples.

As a second tactic, we explore whether the evolution of child *i*'s vaccination compliance leading up to a provider transfer predicts the nature of the upcoming transfer, in terms of the realized change in PVP moving from the origin (*o*) to the destination (*d*) provider, $\Delta_i = \hat{\gamma}_j^{i,d} - \hat{\gamma}_j^{i,o}$. We embed this test for pre-trends in a framework that allows us to also consider what happens after the transfer. If our estimates are forecast-unbiased, a provider with a 1 percentage point higher PVP estimate should in fact cause patients' up-to-date status to increase by 1 percentage point on average.

Adapting the approach pioneered by Chetty, Friedman, and Rockoff (2014), we estimate the following scaled dynamic difference-in-differences model:

$$(4) \qquad U2D_{it} = \beta_{0k} + \beta_{1k}\mathbf{1}_{t=k} + \beta_{2k}\Delta_i + \beta_{3k}(\mathbf{1}_{t=k} \times \Delta_i) + X_i\beta + \varepsilon_{it} \text{ , for } t \in \{-1, k\}$$

[19] Appendix Figure A6 shows the mean residuals by deciles of mother and shrunken provider fixed effects. Related to our finding of increased sensitivity among the vaccine hesitant, Kristiansen and Sheng (2022) find that physician-patient match in terms of socioeconomic status improves compliance and outcomes for low-SES adult patients.
[20] Correlations with estimates based on these and other subsamples are reported in Appendix Table A3, and the notes to Table 4 provide more details on the subsamples.

The model is estimated for each quarter in the window from 4 quarters before to 3 quarters after a transfer, $k \in \{-4, -3, -2, 0, 1, 2, 3\}$, relative to the quarter prior to the transfer ($t = -1$). The control set includes an indicator for the relative quarter ($\mathbf{1}_{t=k}$), the upcoming change in PVP (Δ_i), and an interaction between the two variables (as well as indicators for quarter since birth and mother fixed effects in X_i). So that the PVP estimates are out-of-sample, we use the PVPs estimated on boys for girls, and vice versa. For both genders, the change in PVP estimates from one sample split of mothers is instrumented using those from the second split to account for measurement error.

The estimates on the interactions between the quarters relative to transfer and the upcoming change in PVP (β_{3k}) are graphed in Figure 5. The top panel shows estimates for all patient transfers characterized by 4 quarters of stable provider affiliations before and after. While there is some evidence that those who will transfer to higher-propensity providers may have been rebounding from dips in vaccination compliance, there is also a clear jump and steady increase in compliance after the transfer. If our estimates were forecast unbiased, we would expect an estimated coefficient of 1 in the quarters after the move. What we see instead is an immediate jump of around 0.25 followed by a continued increase over the next few quarters to about 0.6. That the coefficients are below 1 is perhaps not surprising in our setting, since we do not allow for dynamics in provider propensities (such as with experience) and do not condition on the prior up-to-date status of children.[21] Those who are already fully compliant cannot possibly be out of compliance until they age into new requirements. The bottom panel zeroes in on the subset of children switching providers at the same time as they age into new requirements. For this subsample, the coefficient quickly converges to about 0.75.

Finally, we consider whether the provider propensities are separately identified from place-based effects. If there is only one provider in a locality, then the provider propensity cannot be distinguished from common factors that affect vaccine uptake in the area, whether on the supply or demand side. Though provider markets are overlapping since access is rationed by distance to individual residences, we do in fact find that there is some geographic clustering, with the most vaccination-facilitating providers located in the more urban municipalities.[22] In order to

[21] Persistence across the first and second halves of providers' lifespans is high, with a 0.010 increase in the period 1 PVP estimate associated with a 0.006 increase in period 2 (depicted in Appendix Figure A7).
[22] Appendix Figure A8 shows the average estimated PVP by municipality.

condition on common confounding factors that might drive this clustering, we include municipality fixed effects as controls in our subsequent analyses. Around 10% of the variation in provider vaccination propensities is explained by municipality indicators.

All in all, the evidence supports that our statistical model provides a sensible approximation to compliance. Below in Section 6, we further bolster confidence in our PVP estimates by studying impacts on vaccination compliance under the later adolescent phase.

5. Determinants of provider vaccination propensities

In this section, we study factors that predict closer adherence to vaccine guidelines among providers. Many factors could influence a provider's ability to facilitate compliance. Incentives are one, though these are relatively uniform in Denmark since vaccinations are reimbursed on a fee-for-service basis. Time constraints are another, so that a provider's caseload could be relevant. Training regarding how to promote compliance with vaccination could change over time, with more recent evidence suggesting presumptive communication (e.g., "today your child will receive …") is most effective. Own attitudes and knowledge regarding the net benefits of any given vaccination also surely play a role. To separate these from skill, we use information about the adherence of providers' children and grandchildren.

Table 2 presents summary statistics for observable characteristics, attitudes, and practice styles for the clinics in the sample. Most clinics (73%) are single-provider clinics, and average annual caseloads are on the order of 2,000 patients per provider. Consistent with the high cost of entry and low rates of exit, the average age of providers is high – at 53 years of age. Providers' children and grandchildren are on average 87.2% and 89.5% compliant with the childhood vaccination program, which is not too different from the overall child population.

As far as practice styles related to preventative care, we find higher engagement with recommended childhood care (i.e., well-child visits and outreach for the MMR vaccine) than with preventative care for the elderly (i.e., annual flu shots). Our proxy for overall primary care quality is the fraction of patients (aged 50 and older) without avoidable emergency visits, which has been used elsewhere in the literature (Purdy et al., 2009). Potentially avoidable emergency visits are those for ambulatory-care sensitive conditions, such as diabetes and pneumonia, that should be largely prevented if timely and appropriate outpatient services are provided. To succinctly summarize preventative care practice styles and quality, we construct two composite

indices, one that is a straight average and one that is a covariance-weighted average of standardized versions of the four variables. See the notes to Table 2 for more details on variable definitions and construction.

Figures 6 and 7 present the results graphically from ordinary least squares regressions of provider vaccination propensities on different sets of controls, always including indicators for municipality and the first and last years of operation.[23] Point estimates are indicated by circles for the full sample of practices and by squares for the subset that have only one provider. The bars show 95% confidence intervals, based on standard errors that are robust to clustering by municipality. For interpreting the magnitudes, recall that the standard deviation of PVPs is 0.018.

In Figure 6, the provider characteristics shown are included simultaneously. The number of providers in a practice is positively associated with provider compliance: PVPs are 0.003 and 0.006 higher in practices with 2 and 3+ providers, respectively, relative to solo practices. These point estimates map to 0.17 and 0.33 standard deviations. Other aspects of scale, such as caseload per provider and whether the practice has had a trainee assigned to it, are not significant predictors. While there are no detectable differences by the average age of providers in a clinic, clinics with more female providers have higher vaccination propensities. PVPs are 0.006 higher for all-female relative to all-male practices.

The specifications underlying the results shown in Figure 7 start from the specification in Figure 6 and then, one-at-a-time, add proxies for attitudes and categories of practice styles. Those providers whose own children are more compliant with the vaccine program are also better at facilitating compliance among their patients: a one standard deviation increase in the average up-to-date status of own children is associated with a 0.08 standard deviation increase in provider vaccination propensity ($\{0.150\times 0.009\}/0.018$). This suggests there is a role for providers' attitudes regarding the importance of complying with childhood vaccine recommendations – and not just the ability to convey that to patients – in driving variation in patient vaccine uptake across providers.

Regarding practice styles, providers that are more effective in promoting compliance under the childhood vaccination program are also more effective in promoting adherence to other childhood preventative care recommendations. Providers in the top quartile of (non-vaccine-related) well-child visit compliance have propensities that are more than 1.5 standard deviations

[23] Appendix Tables A4 and A5 are the corresponding tables for all and for single-provider practices, respectively.

(0.028) higher than those in the bottom quartile. Interestingly, clinics that conduct reimbursable outreach to those at-risk of missing or those late for the second MMR shot have similar propensities as those that do not, which might reflect offsetting effects of the tendency to be proactive in providing preventative care and the likelihood patients have fallen behind. Measures of preventative care practice style and quality that are derived from older populations are positively correlated with our provider propensity estimates, but to a much lesser extent than the more closely related well-child visit compliance. Being in the top quartiles of flu vaccine compliance and the fraction of patients without avoidable hospitalizations are associated with propensities that are higher by 0.012 and 0.008, respectively.

In summary, what most differentiates providers that more effectively promote childhood vaccination compliance is positive attitudes toward compliance with the program, as evidenced by the compliance rates of own children, and adherence to other childhood preventative care schedules. These providers also provide more effective preventative and primary care for older populations, but the differences on these dimensions are not as striking.

6. Implications of provider vaccination propensities

In this section, we relate provider vaccination propensities (PVPs), estimated on the early childhood phase of the vaccination program, to vaccination uptake in the adolescent phase of the program. For these analyses, we use the narrower sample of adolescent girls born 1997 to 2007 observed in a several-year window around their 12th birthdays. Though links to childhood vaccination outcomes are immediate, the adolescent HPV vaccine recommendation at age 12 is many years after the last childhood recommendation at age 5.[24] It is also a vaccine that was subject to a hesitancy crisis in Denmark over our sample period, so we can explore how that affected patients differentially according to the PVP of their current provider. Finally, we explore whether the reach of provider influence extends beyond own patients to patients' extended family networks.

6.1 Vaccine uptake among own adolescent patients

Figure 8 shows the relative dynamics of HPV vaccine uptake for girls according to the vaccination propensities of the providers they are affiliated with in the month they turn 12. The

[24] Though our baseline PVP estimates include the 1997-2013 birth cohorts, we also calculate and test sensitivity to fully out-of-sample versions that are based only on the 2008-2013 birth cohorts.

point estimates (and 95% confidence intervals) shown are for the coefficients on interactions between relative month indicators and PVP.[25] The estimates are derived from a series of separate instrumental variables regressions that include the period 12 months before aging into the requirement and the relevant relative month. The regressions also include an indicator for the relevant month and a main effect for PVP. The main and interacted PVP variables are instrumented using our split sample binned approach.

As is clear from the figure, girls with more effective vaccination facilitators initiate the HPV vaccine series more quickly and maintain a steady advantage through the first year of eligibility. A girl with a provider whose propensity is one standard deviation above average is more than 3 percentage points (1.85 × 0.018), or 5%, more likely to have initiated the series within 6 months.

The gap between our provider propensities based on young childhood and our focal adolescent vaccination behavior helps to alleviate concerns about unobserved selection to providers based on family HPV vaccination proclivity. However, it does not eliminate them. Thus, we turn to regression analyses where we can control for girls' compliance under the early phase of childhood vaccination program and parental characteristics.

The regression results are shown in Table 3 for three alternative measures of HPV vaccine uptake: initiating the series by getting the first shot within the first 6, 12, and 18 months. For each outcome, we report results from three instrumental variables specifications. In these specifications, the PVP estimate from one sample split is instrumented using the estimate from the other split interacted with the bins for the number of patient transfers. In addition to PVP, the first specification includes municipality and birth year-by-birth month fixed effects. The second adds the average up-to-date status of the girl across quarters under the early childhood vaccination program, and the third adds parental controls.

The point estimates for PVP fall by about 30% when girls' childhood vaccination compliance is added to the control sets. Childhood vaccination compliance reflects family attitudes toward preventative care and vaccination, as well as earlier provider influences. The point estimates are then minimally affected by further adding parental controls. The most saturated specifications reveal that a one standard deviation increase in PVP (of 0.018) is associated with a 2.0 percentage point increase in the likelihood of initiation by 6 months, declining to a 1.8 percentage point increase by 18 months. Recasting, the 6-month (18-month) effect maps to a 5%

[25] Appendix Figure A9 shows an alternative version comparing HPV uptake for top vs. bottom quartile providers.

(8%) reduction in noncompliance.

Table 4 shows that these results are qualitatively robust to PVPs estimated on alternative subsamples.[26] The instrumental variables specifications in this table match the most saturated models from Table 3. The first two subsamples for PVP estimation are defined in ways intended to isolate provider switches that are unrelated to unobserved aspects of the patient-provider match. When the only identifying transfers included in the estimation of PVPs are the first provider shifts following residential moves to new municipalities, columns 1, 5, and 9 reveal that the estimated impacts of PVP on HPV uptake are around 15% smaller at all time horizons. In contrast, the magnitudes in columns 2, 6, and 10 are around 50% smaller when only transfers related to clinic closures are used for identification. Notably, the implied movements associated with a one standard deviation change in PVP are quite stable, since the (unbiased) standard deviations of the PVP estimates from the mover and closure subsamples are 17% and 78% greater than the baseline, respectively.[27]

The other two subsamples considered for PVP estimation are boys and late birth cohorts (2008-2013) from the childhood sample. Both subsamples are fully out-of-sample since the HPV sample consists only of girls born before 2008, and the (unbiased) standard deviations of the PVP estimates are about 25% smaller than the baseline. The associated point estimates for impacts on HPV uptake for the PVP estimates for boys (in columns 3, 7, and 11 of Table 4) are 40-50% greater than baseline, and those for later birth cohorts (in columns 4, 8, and 12) are about 20% greater. We can only speculate as to why the PVP estimates for boys have larger effects, even for one standard deviation movements. However, we might have expected the estimates from the later cohorts to have larger impacts due to the increased relevance, since practice styles are measured in periods that overlap more closely with the years when girls in the HPV sample are making vaccination decisions. Overall, the results in Tables 3 and 4 make it clear that provider adherence to childhood vaccine recommendations meaningfully affects patient compliance with recommended adolescent vaccines as well.

6.2 Resilience to vaccine hesitancy shocks

The HPV sample studied so far spans the crisis in HPV vaccine uptake that occurred in

[26] Appendix Table A6 shows the results are quantitatively robust to alternative instrument sets that differ in the types of interactions that are included between the number of identifying patient transfers and the PVP estimate from the second sample split.

[27] The standard deviations are reported in Appendix Table A3.

Denmark following the 2015 "fake news" documentary attributing disabling symptoms of several girls to HPV vaccination. Figure 9 clearly shows the dramatic negative impact of the documentary, comparing the 2001 (unexposed) to the 2003 (exposed) birth cohort. The 2003 cohort is more than 40 percentage points less likely to initiate the series within 12 months of eligibility, and even after three years rates are still depressed by more than 10 percentage points.

It is natural to ask whether girls differentially weathered the media event depending on their providers' vaccination propensities. One could imagine that a low-propensity-to-vaccinate provider may not be affected by the documentary, as a never-taker. Alternatively, these may be the providers who are most sensitive since they need to be more convinced of the benefits relative to the costs when deciding whether to promote vaccination. The time patterns in Figure 10, recast as dynamic triple differences estimates in Figure 11, are more consistent with this second story. The gaps in HPV vaccine uptake across provider types widened from the 2001 to the 2003 cohort as patients of top-quartile PVP providers rebounded from the shock more quickly than those of bottom-quartile doctors.[28]

Table 5 presents triple differences estimates for HPV vaccine initiation over the three horizons for providers by PVP quartile. Relative to bottom-quartile providers, other providers do not detectably improve relative uptake for the exposed cohort at the 6-month horizon, which is close in time to the airing of the documentary. However, as time passes, relative uptake improves with provider PVP quartile. At the 18-month horizon, relative uptake is 3.2, 6.6, and 7.2 percentage points higher for providers in quartiles 2, 3, and 4, respectively. Thus, providers that are better at facilitating compliance with childhood vaccine recommendations are also better at rebounding from unfounded shocks to confidence in vaccine safety.

6.3 Spillovers to patients' networks

There are many ways to define networks and, here, we choose to focus on the family. For the nuclear family, we have only a small fraction of sisters from the 1997-2007 birth cohorts with different providers at age 12 – typically due to residential moves. Thus, we turn to cousins, who almost always have different providers.

In Table 6, we relate HPV vaccination initiation within the first 6, 12, and 18 months of younger cousins to the PVP of paired older cousins. We focus on the older cousin that is closest

[28] Relatedly, Humlum et al. (2021) find that vaccination rates decreased less among daughters of health professionals relative to other girls.

in age and restrict the sample to cousins who have different providers, though most (90.3%) do. For each time horizon, we report results from three instrumental variables specifications. The first includes only own PVP for reference, along with controls for own childhood vaccination compliance, parental characteristics, and municipality and birth year-by-birth month fixed effects. The second adds older cousin's PVP at the time of aging into eligibility, and the third adds older cousin's compliance under the early childhood vaccination program to control for extended family attitudes and prior provider influences. In the most saturated specifications, cousin's PVP has impacts that are about one-fifth as large as own PVP at the 6-month (column 3) and 12-month (column 6) horizons, declining to a statistically insignificant 8% at the 18-month horizon (column 9).

The results suggest that exposure to providers who are differentially effective in promoting vaccine compliance not only directly affects patient behavior, but also indirectly affects others in patient familial networks. Since the effects fade by 18 months, the spillovers seem to primarily affect the timing of the initiation of HPV vaccination. Timing is particularly important in this case, though, since initiation prior to sexual debut is critical for effectiveness. For comparison, the size of the spillover observed at the 12-month horizon is half as great as those found in the Norwegian workplace for physician-induced changes to worker absenteeism (Dale-Olsen and Godøy, 2018). That may be surprising given that absenteeism is directly observable and affects coworkers, whereas HPV vaccination is not observable and does not affect extended family members.

7. Conclusion

In this paper, we quantify primary care providers' impacts on their child patients' compliance with recommended vaccinations. Our estimates reveal substantial heterogeneity in the degree to which providers facilitate compliance: the patients of a provider that is one standard deviation above average are 1.8 percentage points more likely to be up-to-date. This is more than 10% as great as the impact of being in a family that is one standard deviation above average in childhood vaccination compliance.

Extrapolating to adolescent vaccines reveals that a one standard deviation higher provider propensity to vaccinate under the childhood program is associated with a 2 percentage point increase in the likelihood of HPV vaccination initiation by 6 months. Patients with more

compliant doctors also have speedier recoveries from a media-induced crisis to perceived HPV vaccine safety. Finally, providers influence HPV vaccine uptake for patients' younger cousins by about one-fifth as much as own patients.

Our statistical model for estimating provider vaccination propensities relies on several simplifying assumptions, such as that provider impacts are additive and constant over time. Despite these restrictions, we find that most of the variation in our estimates translates to causal impacts on patient compliance. In future work, it would be interesting to study match and experience effects, including provider experiences with patients that might shake confidence in vaccine safety.

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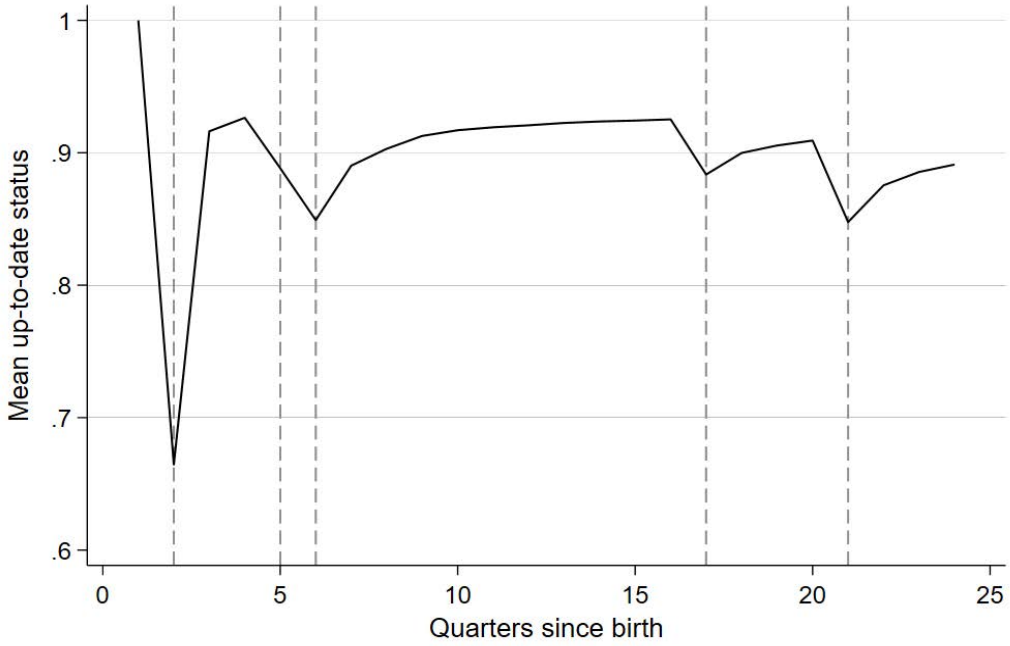
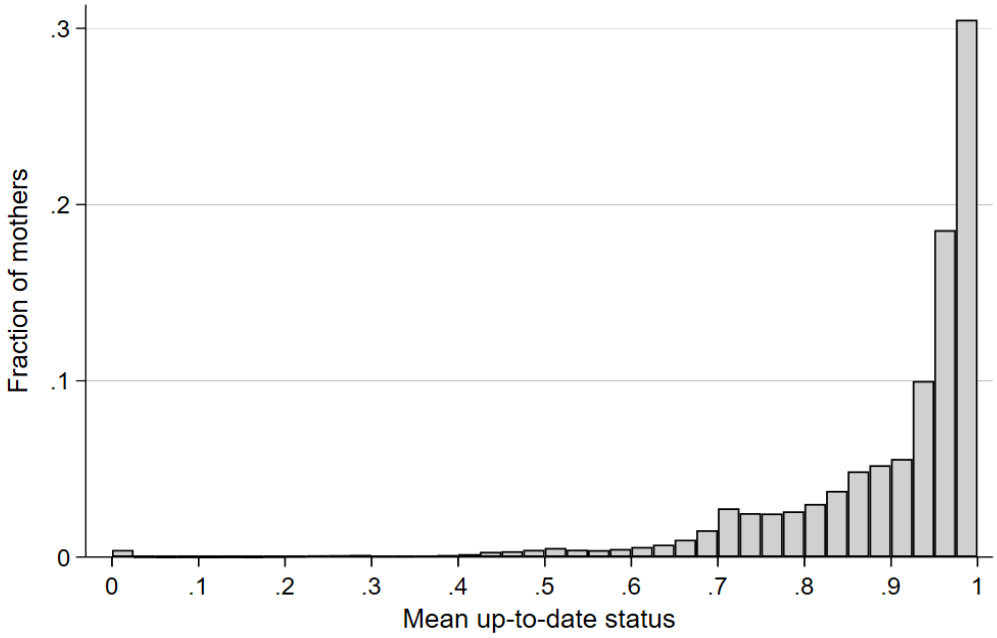
Figure 1 Mean up-to-date status by quarters since birth



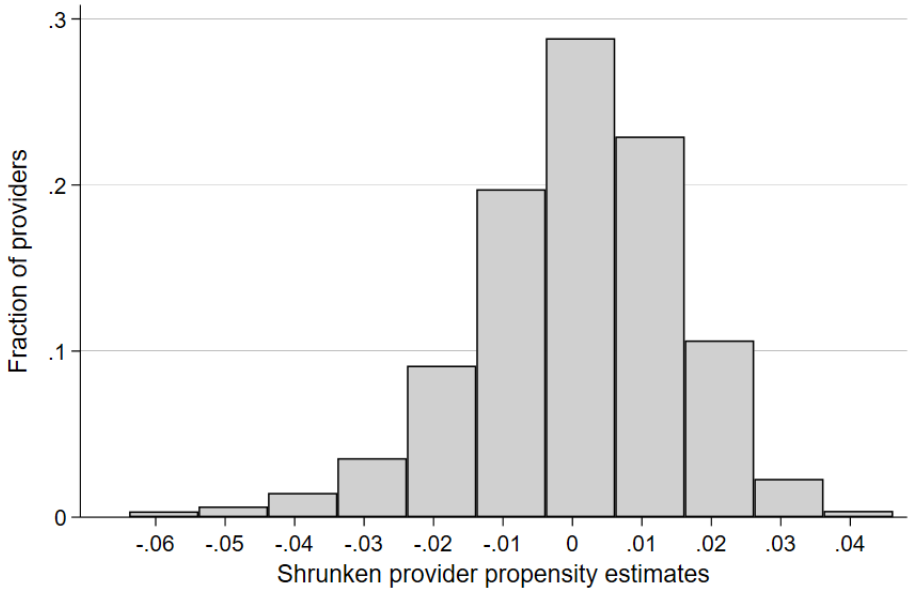
Notes: This figure shows average up-to-date status (i.e., fraction of recommended vaccination visits completed) for children by quarters since birth for all children in birth cohorts 1997 to 2013. The vertical dashed lines indicate when children age into new vaccine recommendations. The vaccination data are aggregated from the daily level to the monthly level, and compliance is measured at the end of the quarter. Quarters are defined based on month of birth. All are in full compliance in the first quarter since there are no requirements prior to 3 months of age.

Figure 2 Distribution of mean up-to-date status of children across mothers



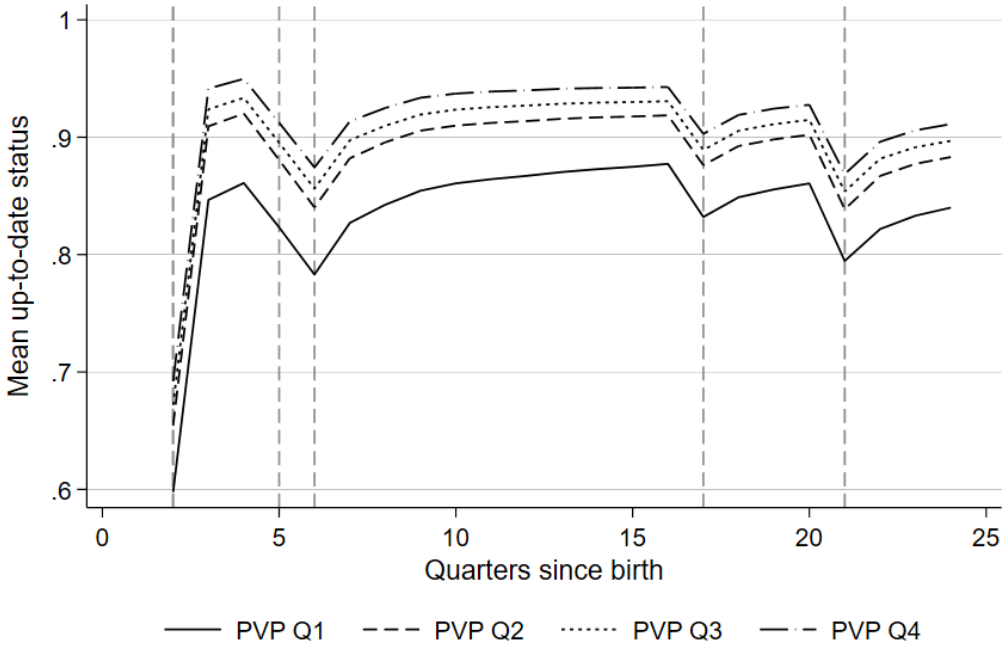
Notes: This figure depicts the distribution of compliance (i.e., average up-to-date status across child-quarters, excluding quarter of birth) with the vaccination program for mothers in our sample. Up-to-date status is measured as the fraction of recommended vaccine visits that a child has completed by the end of any given quarter since birth. The mean (standard deviation) across mothers is 0.89 (0.14), and the number of mothers is 606,021.

Figure 3 Distribution of shrunken provider vaccination propensities



Notes: This figure depicts the distribution of shrunken provider vaccination propensity (PVP) estimates. The shrunken estimates are predicted provider fixed effects from a regression of (recentered) provider fixed effects estimated from one random sample split of mothers on those estimated from the second sample split, with the regression carried out separately for different bins of the number of identifying patient transfers per split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+). Providers in the tails are omitted from the figure. The standard deviation of the shrunken PVP estimates is 0.0164, which is 7% smaller than the unbiased estimate of the standard deviation of PVPs (0.0176).

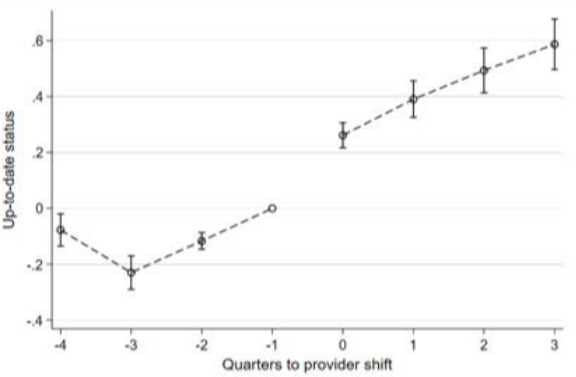
Figure 4 Mean up-to-date status by quarters since birth and provider vaccination propensity quartile

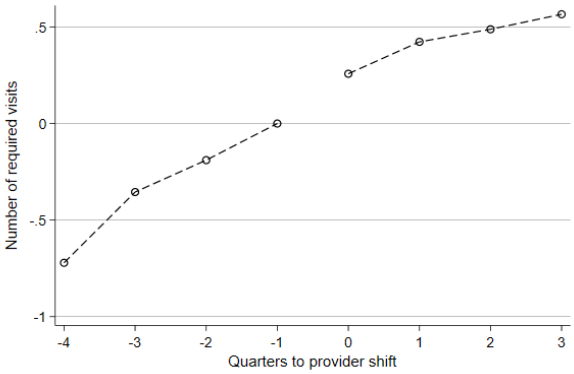


Notes: This figure depicts the evolution of compliance with the child vaccination program as a function of quarters since birth by quartile of provider vaccination propensity (shrunken using the method described in the notes to Figure 3). The compliance rates are estimated using a regression (for providers in each PVP quartile) of their child patients' up-to-date status on indicators for quarters since birth, so implicitly weight providers by their child patient caseloads.

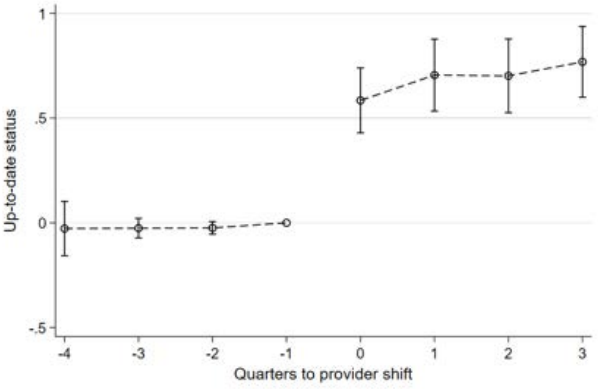
Figure 5 Relative dynamics of child vaccination compliance, by upcoming change in provider vaccination propensity

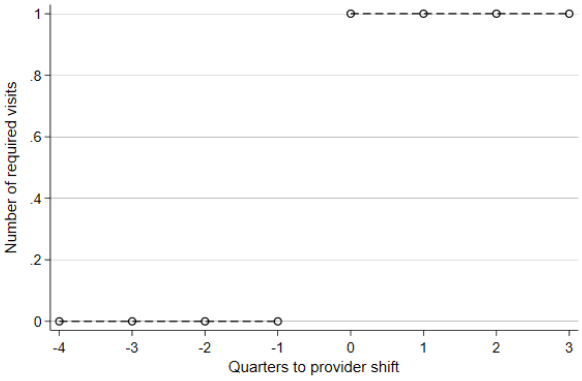
a. All provider transfers





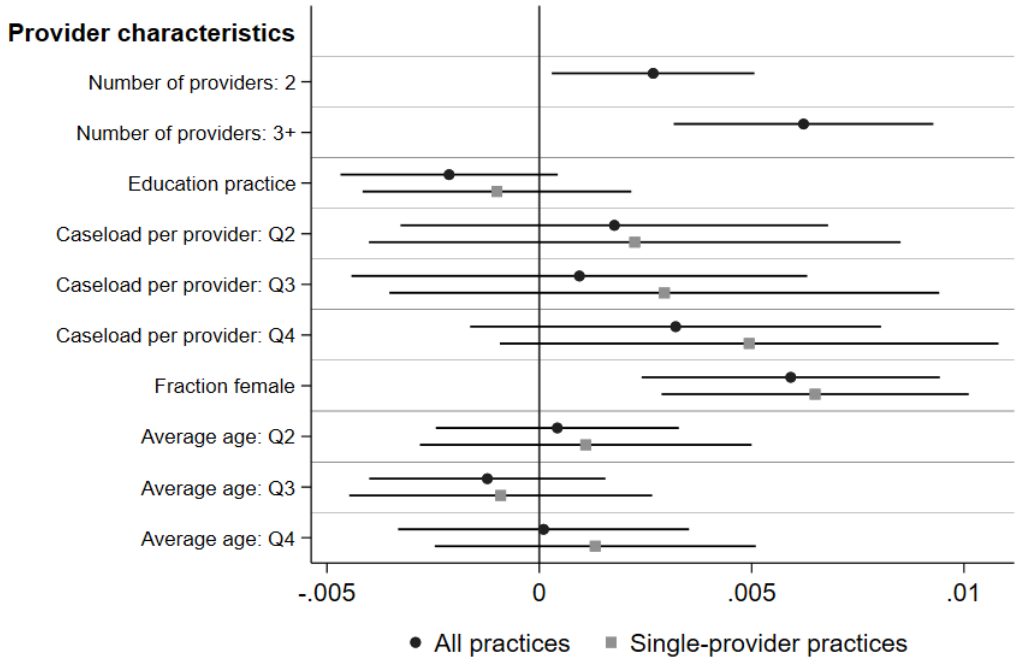
b. Provider transfers that coincide with aging into a new vaccination requirement





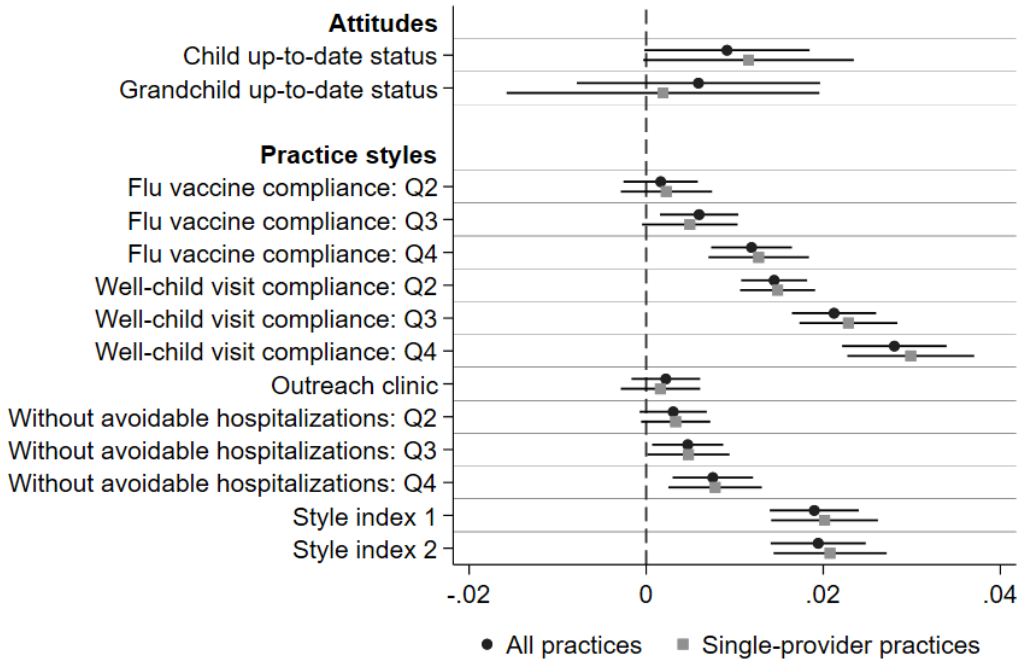
Notes: The first figure in each panel depicts the estimated coefficients for the relevant quarter indicator interacted with the upcoming change in provider vaccination propensity, from estimating the scaled dynamic difference-in-differences model shown in equation (4). To account for measurement error, the model is estimated by a series of IV regressions, using our split-sample binned approach to instrument for the upcoming change, separately for each period relative to the period before the switch (t = -1). To ensure the PVP estimates are out-of-sample, the PVPs estimated on boys only are used for girls, and vice versa. The standard error bars show the 95% confidence intervals, where standard errors are robust to clustering by municipality of origin. The second figure in each panel shows the evolution of the number of required vaccination visits. Starting with all child-quarters in the child vaccination sample (from birth cohorts 1997-2013), we limit the analysis to spells that involve a provider transfer that follows at least 4 quarters of affiliation with the same provider and that is followed by at least 4 quarters of affiliation with the new provider. All such transfers are included in the top panel, representing 263,243 children. The bottom panel restricts the estimation sample to the subset (n=17,468) for whom the provider transfer coincides with aging into a new requirement (with requirements otherwise stable over the 8 quarters).

Figure 6 Correlations between provider vaccination propensities and provider characteristics



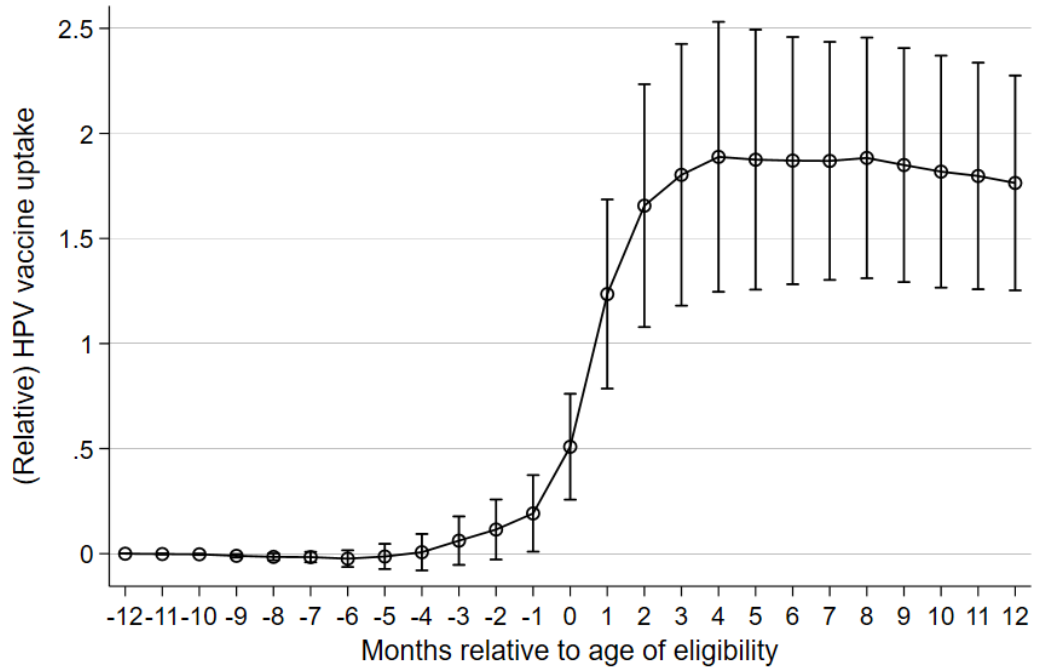
Notes: This figure depicts coefficient estimates (and 95% confidence intervals) from OLS regressions of provider vaccination propensities on provider characteristics, estimated separately for all practices (circles) and for single-provider practices (squares). The full set of provider characteristics shown is included simultaneously, and the models also include indicators for missing values for each of the provider characteristics, indicators for the first and last year we observe the practice, and municipality fixed effects. In all cases, Q1 is the bottom quartile and is the omitted reference category. The confidence intervals are based on standard errors that are robust to clustering by municipality. Exact point estimates are depicted here, whereas the values shown in the corresponding tables (the first column of Appendix Tables A4 and A5) are rounded to the third decimal place. See the notes to Table 2 for more details on how the provider characteristics are defined.

Figure 7 Correlations between provider vaccination propensities and attitudes/practice styles



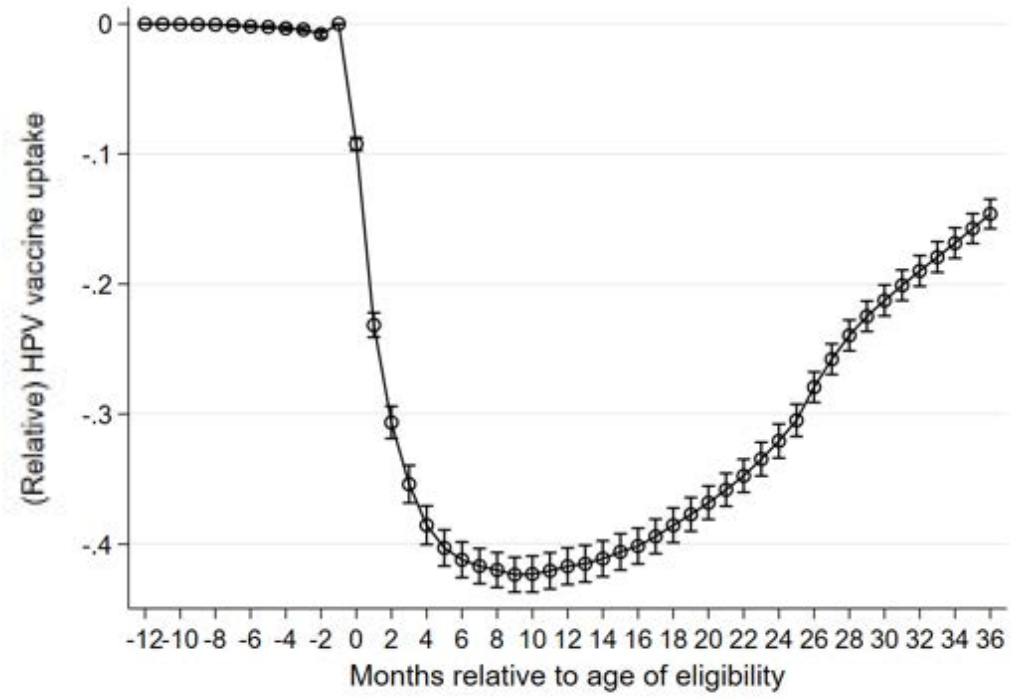
Notes: This figure depicts coefficient estimates (and 95% confidence intervals) from regressions of provider vaccination propensities on measures of provider attitudes and practice styles (as reported in columns 2-8 of Appendix Tables A4 and A5). Each of these models includes the full set of provider characteristics shown in Figure 6 and otherwise matches those specifications, but results are shown for specifications that add the variables for attitudes and each category of practice style separately. Results are shown for all practices (circles) and for single provider practices (squares). See the notes to Table 2 for more details on how the attitude and practice style variables are defined.

Figure 8 Relative dynamics of HPV vaccine uptake by provider vaccination propensity



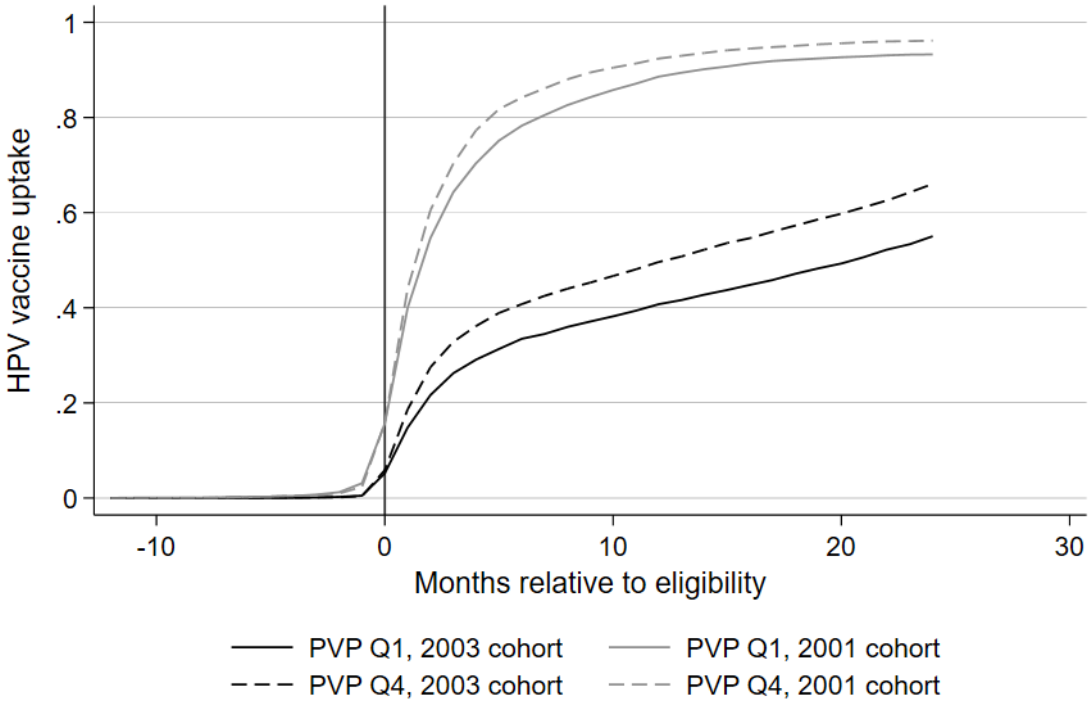
Notes: This figure shows coefficient estimates (and 95% confidence intervals) from a dynamic difference-in-differences regression of HPV vaccination initiation around the month when girls age into the recommendation. The models are estimated by a series of separate IV regressions that include the period 12 months before aging into the requirement and the relevant relative month. The coefficients shown are for the relevant relative month indicator interacted with the provider vaccination propensity (PVP under the childhood vaccination program) for the provider at the time the girl turns 12. The regressions also include the main effect for PVP and the relative month indicator. The main and interacted PVP variables are instrumented using our split sample binned IV approach to address measurement error. Standard errors are robust to clustering by municipality. The sample includes all girls from the 1997-2007 birth cohorts (n=346,366).

Figure 9 Event study of HPV vaccine uptake for post- vs. pre-documentary birth cohorts



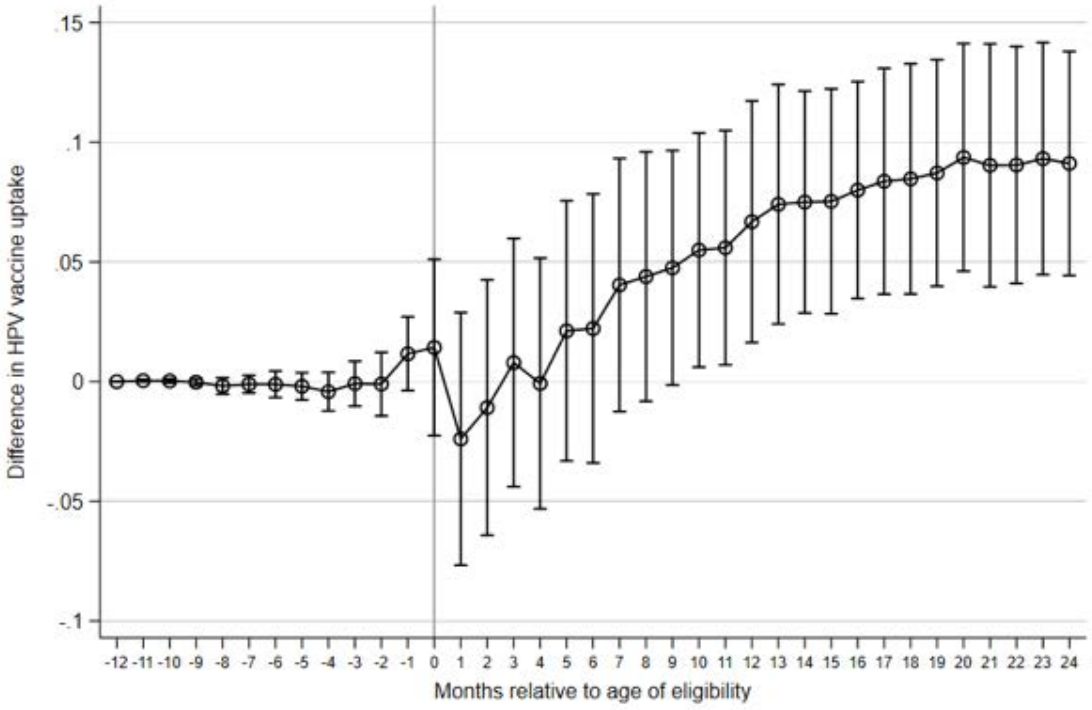
Notes: This figure depicts the impact of the 2015 documentary attributing disabling symptoms for several girls to HPV vaccination. The coefficient estimates are from a dynamic difference-in-differences model comparing the 2003 birth cohort (treatment) to the 2001 birth cohort (control) in terms of initiation of the HPV vaccine series. In addition to individual fixed effects, the underlying regression model includes indicators for months relative to aging into the HPV vaccine recommendation (at age 12) and interactions of those indicators with an indicator for the 2003 cohort, which are depicted here. The standard error bars show the 95% confidence intervals, where standard errors are robust to clustering by municipality at the time of aging in. The sample is all girls born in 2001 and 2003 (n=107,780).

Figure 10 HPV vaccine uptake by birth cohort and provider vaccination propensity



Notes: This figure compares the uptake of the HPV vaccine according to months relative to the age of eligibility (12 years) for the 2003 (treatment) and 2001 (control) birth cohorts, separately for the top and bottom quartiles of provider vaccination propensity (shrunken using the method described in the notes to Figure 3).

Figure 11 Relative dynamics of HPV vaccine uptake for post- vs. pre-documentary birth cohorts, by top- vs. bottom-quartile provider vaccination propensity



Notes: This figure estimates the differential impact of the media event on uptake of the HPV vaccine according to months relative to the age of eligibility (12 years) for the 2003 (treatment) and 2001 (control) birth cohorts, for those with providers in the top vs. bottom quartile of vaccination propensity (shrunken using the method described in the notes to Figure 3). Shown are the point estimates (and 95% confident intervals) for interactions between indicators for month relative to eligibility, for being born in 2003, and for having a top quartile PVP provider. Standard errors are clustered at the municipality level.

Table 1 Childhood vaccination schedule and coverage, by birth cohort

Child age	Scheduled vaccinations	Coverage
a. Date of birth: January 1, 1997 – March 31, 2004		
3 months	Diphtheria, tetanus, pertussis, polio, Hib	92.2%
5 months	Diphtheria, tetanus, pertussis, polio, Hib	93.3%
12 months	Diphtheria, tetanus, pertussis, polio, Hib	93.2%
15 months	Measles, mumps, rubella (MMR1)	92.6%
5 years	Booster: Diphtheria, tetanus	86.2%
b. Date of birth: April 1, 2004 – December 31, 2013		
3 months	Diphtheria, tetanus, pertussis, polio, Hib, pneumococcal disease	92.3%
5 months	Diphtheria, tetanus, pertussis, polio, Hib, pneumococcal disease	92.6%
12 months	Diphtheria, tetanus, pertussis, polio, Hib, pneumococcal disease	92.5%
15 months	Measles, mumps, rubella (MMR1)	92.9%
4 years	Measles, mumps, rubella (MMR2)	86.7%
5 years	Booster: Diphtheria, tetanus, pertussis, polio (DTaP/IPV)	86.0%

Notes: The first two columns show the recommended timing for administration of the vaccines used to calculate the up-to-date measures through the first 6 years of life, and the last column shows average uptake by age 6 across the 1997-2013 birth cohorts. The Hib vaccine can prevent Haemophilus influenzae type b disease. The visits at 5 months, 12 months, 4 years, and 5 years correspond with regular check-up exams. Not shown and not included in calculation of the up-to-date measure are oral polio vaccines recommended at ages 2, 3 and 4 for children born before July 1, 1999. If the visits at ages 3, 5, and 12 months include 2 shots, we base the up-to-date measure on the shot that includes diphtheria, tetanus, pertussis, and polio. Additional variations over time that are included in the calculation but not shown in the table include: i) the diphtheria-tetanus booster also included pertussis (polio) starting September 1, 2003 (July 1, 2004), and ii) the vaccine for pneumococcal disease recommended at 3, 5, and 12 months was not added to the childhood vaccination program until October 1, 2007. The childhood vaccination program also included shots recommended at age 12: MMR2 (for the 1997-2004 cohorts) and Human papillomavirus (HPV, 3 shots at 0, 2, and 6 months after turning 12). In August 2014, the recommendation for the HPV vaccine was changed from 3 to 2 shots (at 0 and 6 months). We derived the timing by birth cohort from Statens Serum Institut (2002, 2003, 2007, 2008, 2014) and from the Danish Health Authority Vaccination Guidelines (1996, 2007, 2008).

Table 2 Descriptive characteristics of practices

	All practices		Single-provider practices	
Number of providers: 1	0.729	(0.445)	1	(0)
Number of providers: 2	0.165	(0.371)	0	(0)
Number of providers: 3+	0.106	(0.308)	0	(0)
First year of operation	1998	(6)	1999	(6)
Last year of operation	2011	(5)	2010	(5)
Education practice	0.234	(0.423)	0.157	(0.364)
Caseload per provider	2,049	(767)	2,059	(802)
Average fraction female of providers	0.324	(0.388)	0.296	(0.425)
Average age of providers	52.5	(6.2)	53.2	(6.6)
Average up-to-date status of own children	0.872	(0.150)	0.865	(0.168)
Up-to-date status of children: missing	0.589	(0.492)	0.714	(0.452)
Average up-to-date status of own grandchildren	0.895	(0.112)	0.892	(0.123)
Up-to-date status of grandchildren: missing	0.414	(0.493)	0.485	(0.500)
Well-child visit compliance	0.735	(0.103)	0.724	(0.111)
Outreach clinic	0.628	(0.060)	0.622	(0.063)
Flu vaccine compliance	0.341	(0.157)	0.335	(0.166)
Patients without avoidable hospitalization	0.819	(0.385)	0.780	(0.414)
Practice style metric 1	0.000	(0.567)	-0.060	(0.593)
Practice style metric 2	0.000	(0.557)	-0.056	(0.586)
Observations	3,253		2,370	

Notes: This table provides summary statistics (means, with standard deviations in parentheses) for all practices in the first two columns, and single-provider practices in the next two columns. We calculate vaccination propensities for 3,701 practices, however only 3,253 are present in the dataset with provider characteristics. These 3,253 providers account for more than 99% of child-quarter observations. Statistics are taken over the years we observe the practice during the period 1997-2016. The number of providers is the number of physicians owning the practice. An education practice is one that has ever had a trainee assigned. For the remaining variables, we first average by year, and then average across years. Caseload per provider is the annual number of patients affiliated with the practice divided by the number of physicians. Compliance with the childhood vaccination program is average quarterly up-to-date status for all (grand)children, with these variables missing if no (grand)children are observed for providers working in the practice. Well-child visit compliance is the fraction of visits that occur within 1 month of the recommended timing for eligible patients, excluding recommended visits that coincide with vaccinations. Outreach clinic is an indicator for ever conducting outreach for the MMR vaccine, which was reimbursed starting mid-period for those late for or at risk of missing the booster. Flu compliance is the fraction of patients aged 65+ who get the vaccine, starting in 2002 when first recommended. Patients without avoidable hospitalization are those aged 50+ that do not have admissions for ambulatory care sensitive conditions. The practice style metrics are averages of standardized versions of the flu, well-child, outreach, and without avoidable hospitalization variables. Metric 1 is a simple mean effects index which is a strict average, while metric 2 is an inverse covariance weighted average designed to maximize the amount of information captured in the index. The correlation between the two practice style metrics is 0.98.

Table 3 Effects of provider vaccination propensity on HPV vaccine uptake

	6 months after HPV eligibility			12 months after HPV eligibility			18 months after HPV eligibility		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Provider vaccination propensity	1.587	1.136	1.093	1.569	1.131	1.067	1.530	1.106	1.027
	(0.128)	(0.130)	(0.127)	(0.128)	(0.135)	(0.130)	(0.136)	(0.144)	(0.137)
Childhood vaccination compliance		0.344	0.322		0.335	0.311		0.324	0.298
		(0.010)	(0.010)		(0.010)	(0.010)		(0.010)	(0.010)
Municipality FEs	x	x	x	x	x	x	x	x	x
Birth year x birth month FEs	x	x	x	x	x	x	x	x	x
Parental controls			x			x			x
Mean of outcome variable	0.581	0.581	0.581	0.704	0.704	0.704	0.760	0.760	0.760
F-statistic instruments	181	177	176	181	177	176	181	177	176

Notes: Each column reports point estimates (with standard errors robust to clustering by municipality in parentheses) from a separate instrumental variables regression. Across columns 1-3, 4-6, and 7-9, the dependent variables are indicators for initiating the HPV series within 6 months, 12 months, and 18 months of eligibility, respectively. All models contain municipality and birth year-by-birth month fixed effects. The key control variable of interest is the provider vaccination propensity (under the childhood vaccination program) for the provider at the time the girl turns 12 and becomes eligible for the HPV vaccination. To address measurement error, the estimated propensity from one sample split (of mothers in the childhood vaccination sample) is instrumented using interactions between the estimate from the second sample split and indicators for bins of the number of identifying patient transfers per split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+). What differs across the columns is whether the models include the girl's average up-to-date status under the childhood vaccination program and maternal and paternal controls (detailed in Appendix Table A2). The sample is girls (n=346,493) from birth cohorts 1997-2007. All estimates shown are statistically significant at the 1% level.

Table 4 Effects of provider vaccination propensity on HPV vaccine uptake, different childhood vaccination samples

	6 months after HPV eligibility				12 months after HPV eligibility				18 months after HPV eligibility			
PVP sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Relocators	0.961				0.902				0.850			
	(0.122)				(0.119)				(0.115)			
Closures		0.543				0.510				0.509		
		(0.067)				(0.069)				(0.069)		
Boys			1.637				1.527				1.421	
			(0.189)				(0.182)				(0.184)	
Late cohorts				1.305				1.280				1.240
				(0.159)				(0.143)				(0.150)
F-statistic instr.	113	181	71	106	113	181	71	106	113	181	71	106

Notes: Each column reports point estimates (with standard errors robust to clustering by municipality in parentheses) from a separate regression. Across columns 1-4, 5-8, and 9-12, the dependent variables are indicators for initiating the HPV series within 6 months, 12 months, and 18 months of eligibility, respectively. All models contain municipality and birth year-by-birth month fixed effects, the girl's average up-to-date status under the childhood vaccination program, and parental characteristics. The key control variable of interest is the provider vaccination propensity (PVP) estimated using different subsamples of childhood vaccination data. In each case, the estimated propensity from one sample split (of mothers) is instrumented using interactions between the estimate from the second sample split and indicators for bins of the number of identifying patient transfers per split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+). In the relocator sample, the only transfers included are the first provider shift observed within a year of a residential move to a new municipality. In the closures sample, the only transfers included are for those individuals experiencing a practice closure, and we only keep observations associated with the first provider following the closure. When estimating PVPs using the relocator and closure samples, all other children included in the estimation sample are those who remain with the same provider throughout. The "boys" PVP estimates are based on boys' childhood vaccination data only, while the "late cohorts" PVP estimates use data from children born in 2008 onward only. In all cases, the HPV uptake sample is girls from birth cohorts 1997-2007. Notably, we can estimate provider propensities from the childhood subsamples for nearly every girl (n=346,493) for whom we are able to estimate a provider propensity from the full childhood sample. All estimates shown are statistically significant at the 1% level.

Table 5 Documentary chilling effect on HPV uptake, by provider vaccination propensity

		6 mos. after eligibility	12 mos. after eligibility	18 mos. after eligibility
		(1)	(2)	(3)
PVP quartile	Q2	0.035***	0.015*	0.011*
		(0.009)	(0.007)	(0.006)
	Q3	0.052***	0.031***	0.025***
		(0.009)	(0.007)	(0.005)
	Q4	0.060***	0.038***	0.029***
		(0.009)	(0.007)	(0.005)
Born in 2003		-0.448***	-0.478***	-0.450***
		(0.014)	(0.013)	(0.011)
Born in 2003 x PVP quartile	Q2	-0.009	0.024	0.032**
		(0.017)	(0.017)	(0.014)
	Q3	0.015	0.053***	0.066***
		(0.016)	(0.015)	(0.012)
	Q4	0.013	0.050***	0.072***
		(0.016)	(0.014)	(0.011)

Notes: Each column reports point estimates and standard errors from a separate regression. The dependent variable is an indicator for initiating the HPV series within 6 months, 12 months, and 18 months of eligibility in columns 1, 2, and 3, respectively. All models contain municipality fixed effects, the girl's average up-to-date status under the childhood vaccination program, and parental characteristics. The PVP quartile (based on the shrunken estimate) is for the provider at the time the girl turns 12 and becomes eligible for the HPV vaccination. The omitted quartile is the bottom quartile Q1. Standard errors are block-bootstrapped using 1,000 replications (since the shrunken PVP quartile regressors are generated) and robust to clustering by municipality at the time of aging into eligibility. The sample is all girls from the 2001 and 2003 birth cohorts (n=107,780). The 2003 cohort is the one that has been treated by the 2015 documentary attributing disabling symptoms for several girls to HPV vaccination. *** p<0.01, ** p<0.05, * p<0.1

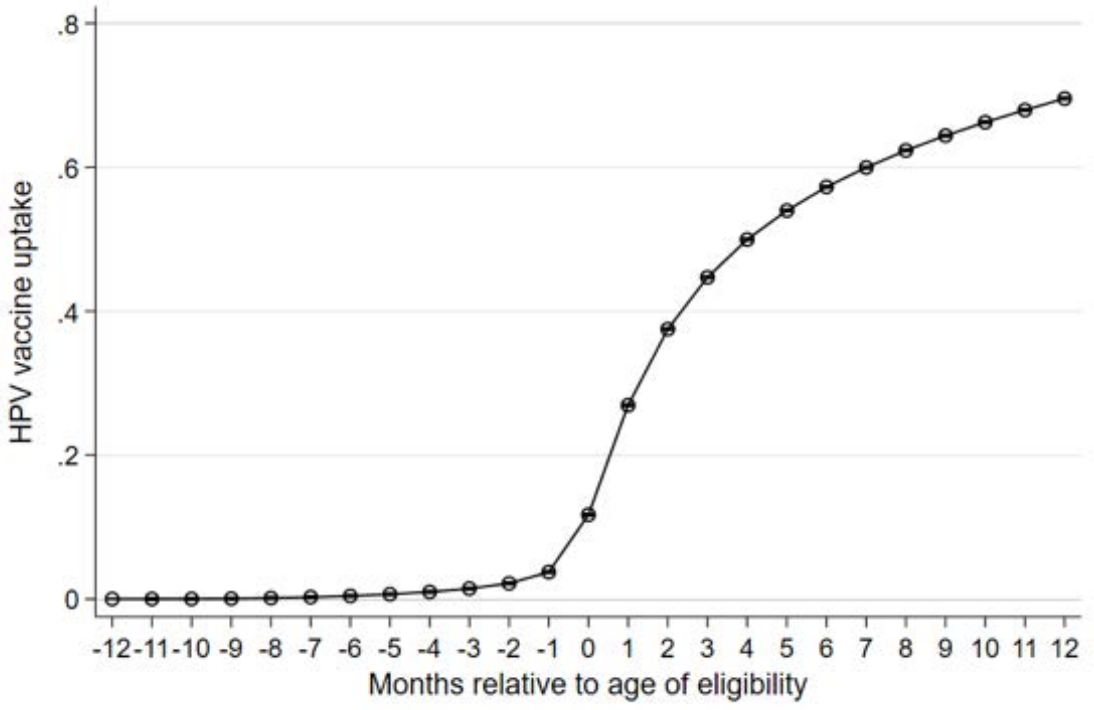
Table 6 Spillover effects of provider vaccination propensity on HPV vaccine uptake of cousins

	6 months after HPV eligibility			12 months after HPV eligibility			18 months after HPV eligibility		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Own PVP	1.252***	1.262***	1.260***	1.122***	1.113***	1.111***	1.130***	1.122***	1.120***
	(0.166)	(0.168)	(0.168)	(0.175)	(0.176)	(0.176)	(0.174)	(0.176)	(0.176)
Older cousin's PVP		0.270**	0.224**		0.274**	0.230**		0.140	0.094
		(0.103)	(0.103)		(0.112)	(0.112)		(0.108)	(0.109)
Own childhood vax compliance	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cousin's childhood vax compliance	No	No	Yes	No	No	Yes	No	No	Yes
Mean outcome variable	0.541	0.541	0.541	0.671	0.671	0.671	0.733	0.733	0.733

Notes: Each column shows the point estimates and standard errors from a separate instrumental variables regression. Across columns 1-3, 4-6, and 7-9, the dependent variables are indicators for initiating the HPV series within 6 months, 12 months, and 18 months of eligibility, respectively. All models contain municipality and birth year-by-birth month fixed effects, the girl's average up-to-date status under the childhood vaccination program, and parental characteristics. What differs across the columns is whether the models also include older cousin's provider vaccination propensity (PVP) and average childhood up-to-date status. The sample consists of all younger cousins (n=107,696) for whom an older cousin can also be found within the HPV initiation sample (birth cohorts 1997-2007). If a younger cousin has more than one older cousin, we consider only the cousin that is closest in age. We only include cousins that have different providers at age 12 (90.3% of all cousin pairs have different providers at age 12). To address measurement error in own and older cousin's PVP, we use our split-sample binned IV approach. *** p<0.01, ** p<0.05, * p<0.10

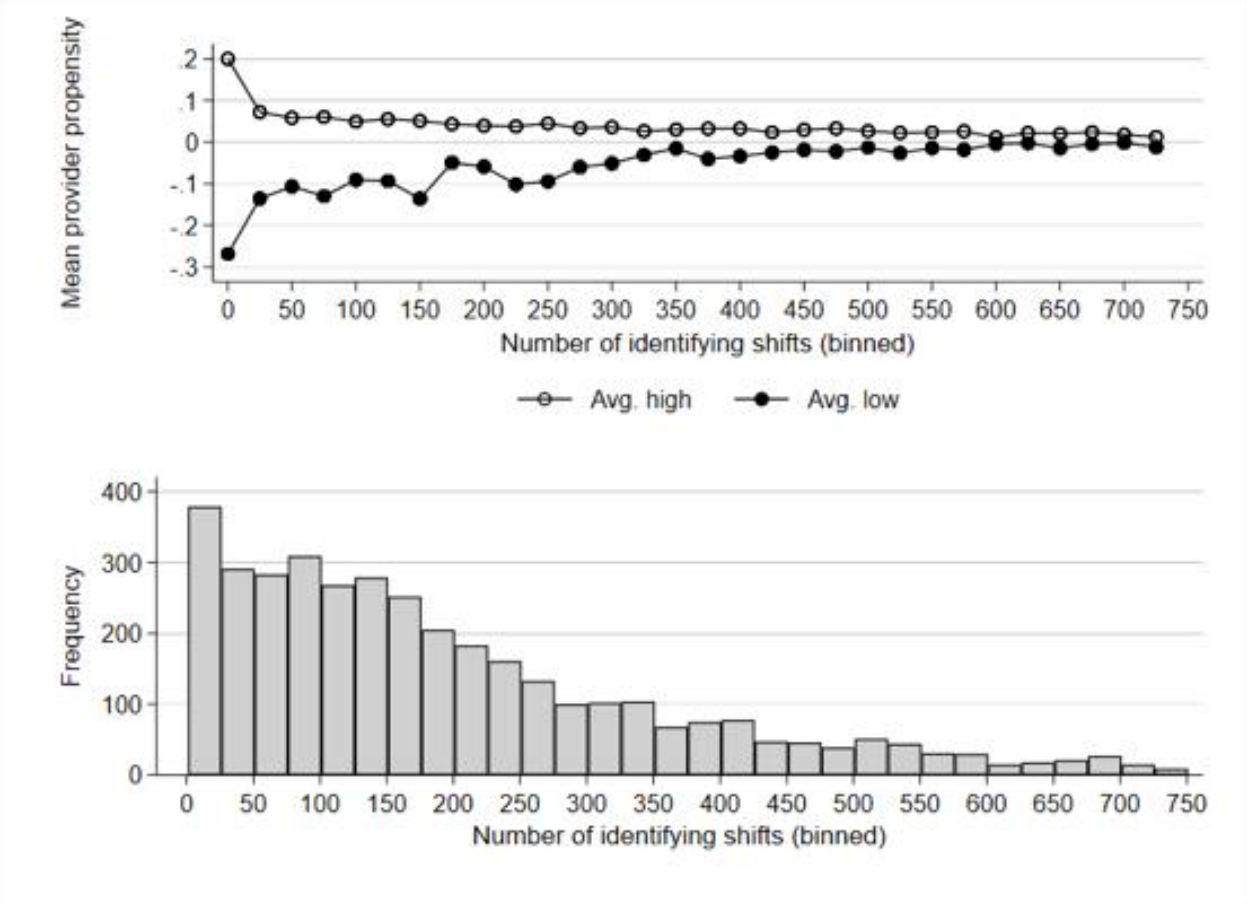
APPENDIX FIGURES AND TABLES – FOR ONLINE PUBLICATION

Figure A1 Uptake of the HPV vaccine by months since eligibility



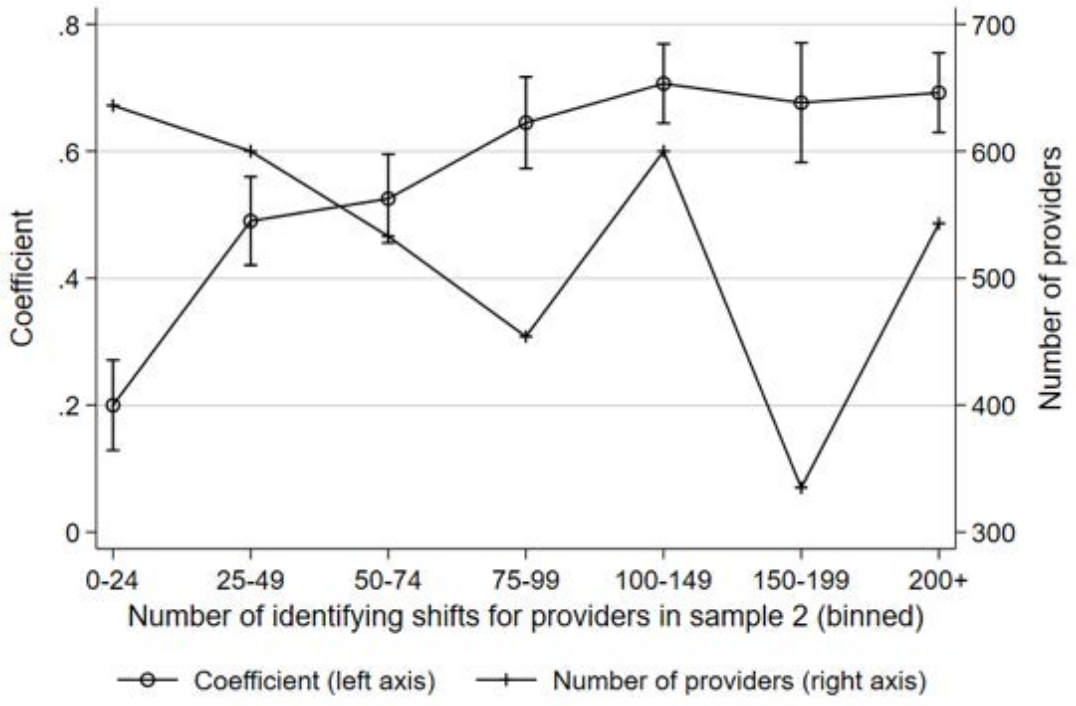
Notes: The figure shows uptake of the initial HPV vaccine according to months relative to the eligibility age of 12 years. It depicts coefficient estimates on relative month indicators (the base month is -12). The standard error bars show the 95% confidence intervals. The figure is based on vaccination data for 346,493 girls born 1997-2007.

Figure A2 Distribution of provider vaccination propensities by number of identifying transfers



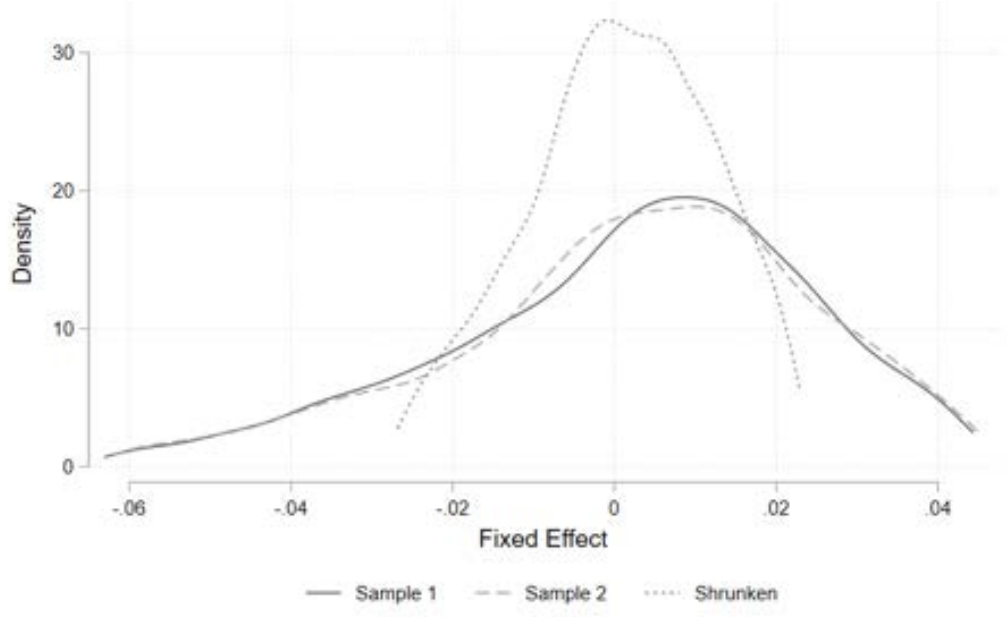
Notes: The top panel in this figure depicts the mean estimated provider vaccination propensity for the top (hollow circle) and bottom (solid circle) 5 providers by the number of identifying patient transfers (binned by ranges of 25). The bottom panel shows the distribution of the number of identifying transfers across providers. Note that the distribution is truncated, so that practices with more than 750 transfers are omitted. These are the original estimates centered at the mean (in the provider sample). A transfer is only counted once per mother if the mother moves multiple children at the same time (i.e., in the same quarter).

Figure A3 Signal to signal-plus-noise ratio by number of identifying transfers



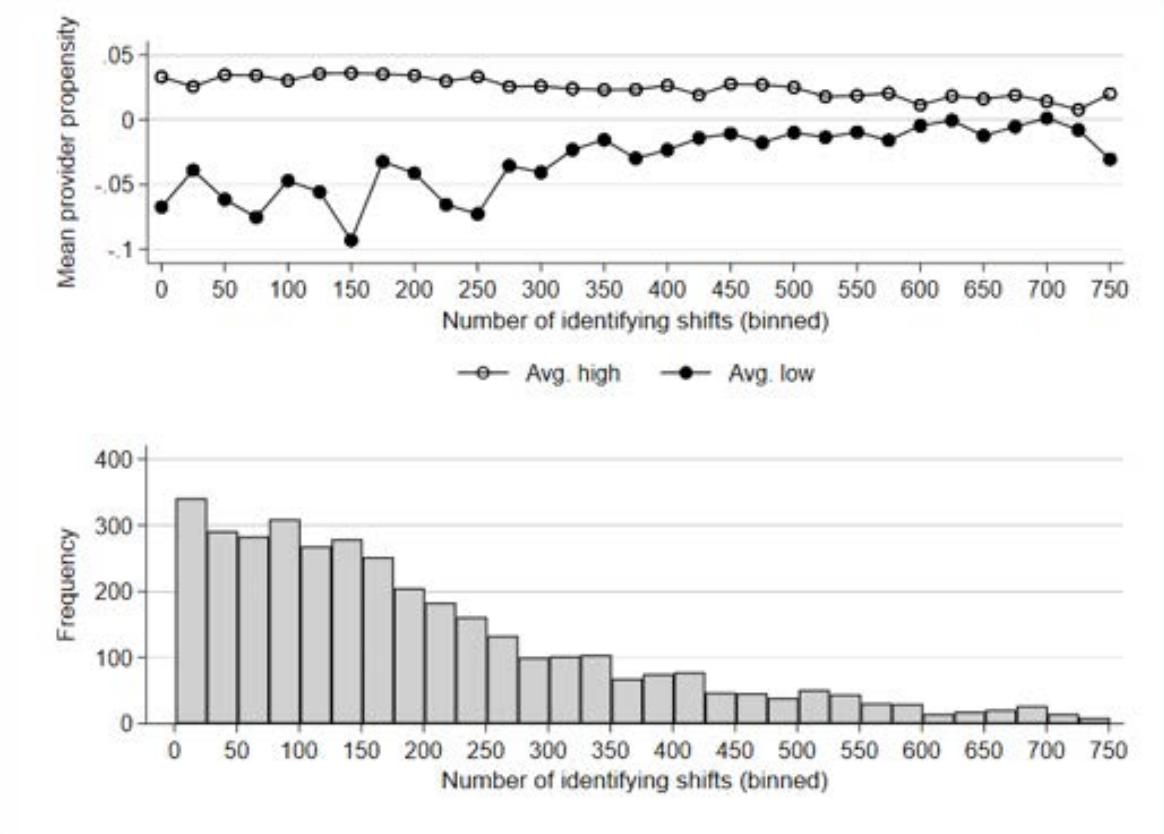
Notes: The circles (bars) depict the estimated slope coefficients (95% confidence intervals) from regressions of the centered provider fixed effect estimates from one random sample split (of mothers) on the other, separately by the (binned) number of identifying patient transfers across providers per split. For example, the first coefficient shows the estimated slope for providers with 0-24 transfers in the second sample split, and the second coefficient shows the estimated slope for providers with 25-49 transfers in the second sample split. The plus signs show the number of providers represented in each bin.

Figure A4 Distributions of unadjusted and shrunken provider fixed effects estimates



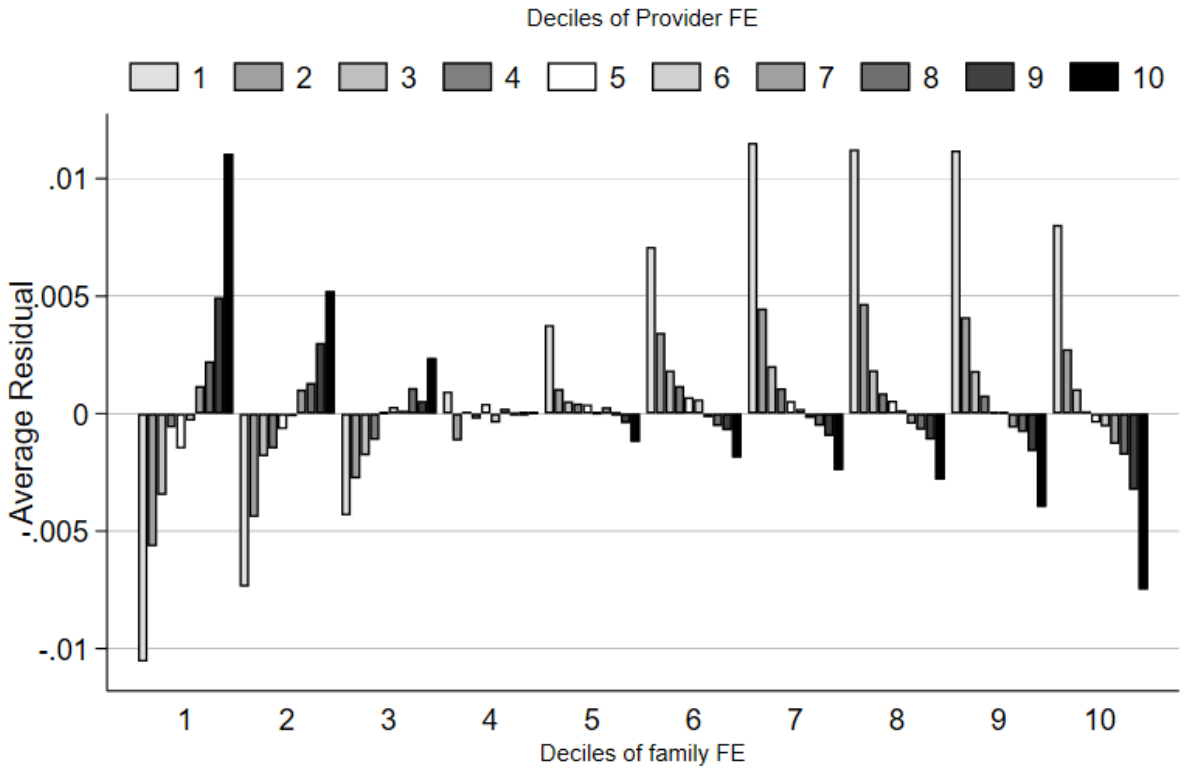
Notes: The two wider distributions depicted are for the unadjusted provider fixed effects estimates from equation (1) for the two random sample splits of mothers. The narrower distribution is for the shrunken provider fixed effects estimates, based on estimating equations (2) and (3) separately for subsamples of providers defined based on 7 bins of the number of identifying patient transfers in the second sample split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+).

Figure A5 Distribution of shrunken vaccination propensities by number of identifying transfers



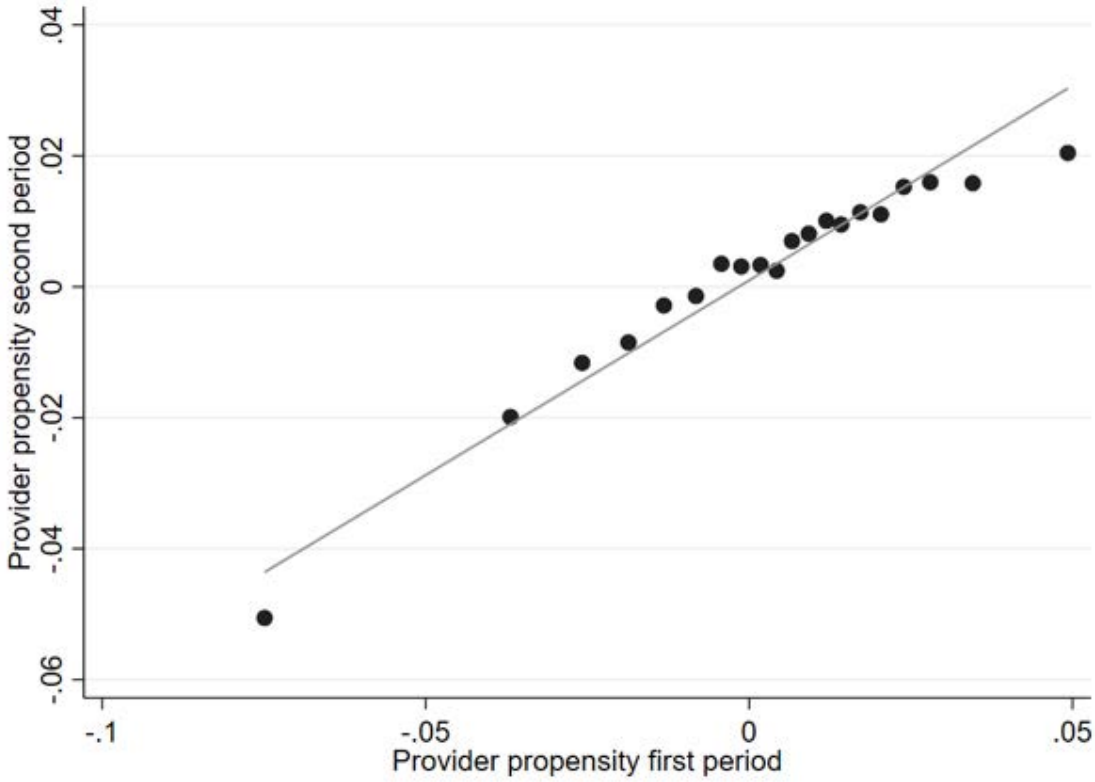
Notes: The top panel in this figure depicts the mean estimated shrunken provider propensity for the top (hollow circle) and bottom (solid circle) 5 providers by the total number of identifying patient transfers (binned by ranges of 25). The shrunken estimates are predicted provider fixed effects from a regression of (recentered) provider fixed effects estimated from one random sample split (of mothers) on those estimated from the other sample split, separately by bins based on the number of identifying transfers per split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+). The bottom panel shows the distribution of the number of providers according to the total number of identifying transfers. Practices with more than 750 shifts are omitted from the figure.

Figure A6 Mean residuals by provider fixed effect decile, by mother fixed effect decile



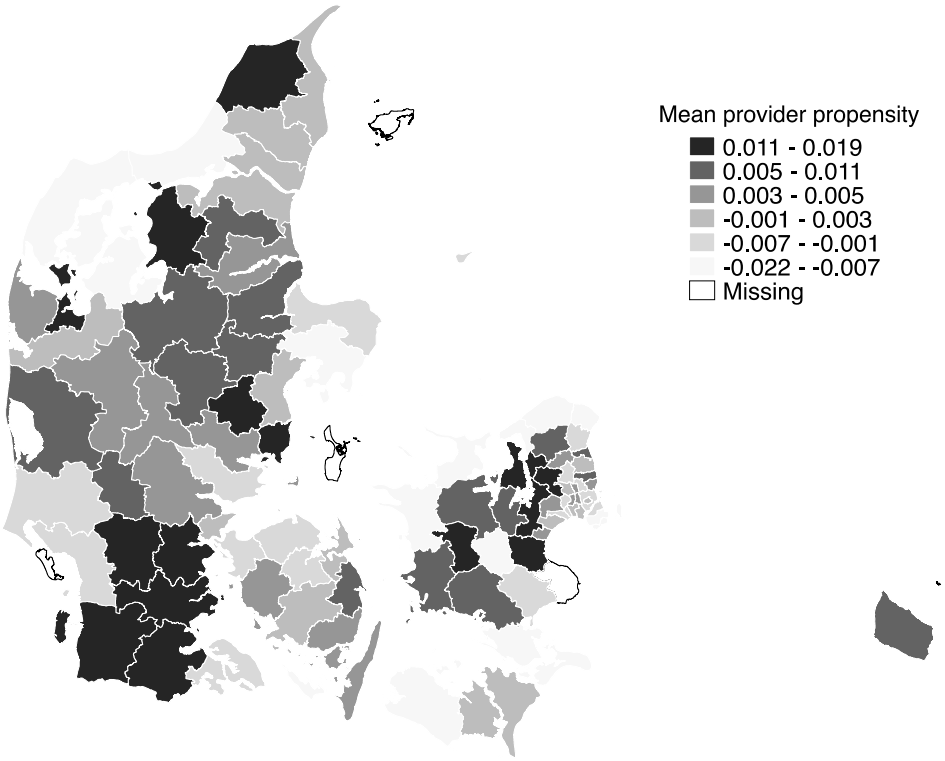
Notes: This figure illustrates the mean residuals from estimating equation (1) by deciles of mother and (shrunken) provider fixed effects.

Figure A7 Correlation between provider vaccination propensities across time periods



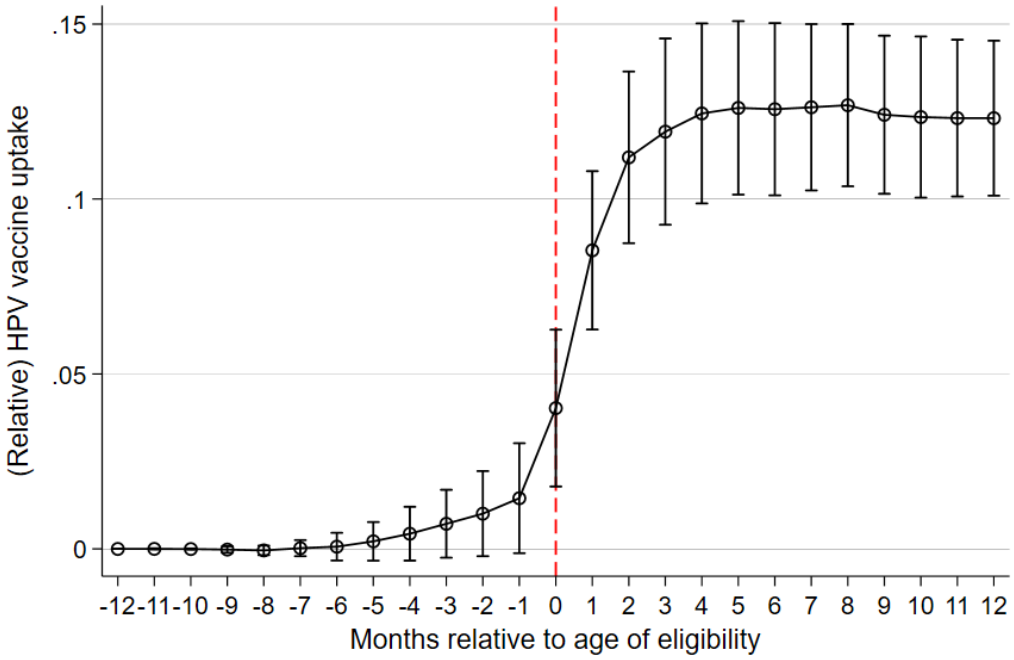
Notes: This figure depicts a binned (20-dot) scatterplot that assesses the persistence in (shrunken) provider vaccination propensities. Provider vaccination propensities are estimated separately for the first and second halves of providers' lifespans. The solid line is a fitted linear regression with the slope coefficient (standard error) equal to 0.595 (0.015), where the standard error is robust to clustering by municipality.

Figure A8 Geographical distribution of provider vaccination propensities across Denmark



Notes: This figure shows the geographical distribution of average (recentered) provider vaccination propensity estimates across clinics for municipalities in Denmark. White areas are missing due to too few providers (5 or fewer), but they are included in the analysis.

Figure A9 Event study of HPV vaccine uptake for top- versus bottom-quartile provider vaccination propensities



Notes: This figure shows coefficient estimates from a dynamic difference-in-differences model comparing rates of initiation of the HPV vaccine series for girls with providers with top vs. bottom quartile (shrunken) vaccination propensities. The sample is limited to girls from the 1997-2007 birth cohorts with providers in the top or bottom quartile of vaccination propensity when the individual becomes eligible (i.e., turns 12). The relative month indicators are interacted with an indicator for having a provider in the top quartile. In addition to these interactions depicted here, the underlying regression model includes main effects for the relative month indicators and individual fixed effects. The standard error bars show the 95% confidence intervals, where standard errors are robust to clustering by municipality of residence at the time of aging in.

Table A1 Parental characteristics of the child vaccination sample by birth cohort

		1997-2000		2001-2004		2005-2008		2009-2013	
		Pat.	Mat.	Pat.	Mat.	Pat.	Mat.	Pat.	Mat.
Age	<25	0.08	0.18	0.06	0.15	0.06	0.13	0.06	0.14
	25-30	0.27	0.37	0.26	0.37	0.24	0.34	0.22	0.32
	30-35	0.39	0.35	0.40	0.37	0.41	0.40	0.39	0.39
	>35	0.20	0.08	0.23	0.10	0.25	0.12	0.26	0.14
	Missing	0.07	0.01	0.05	0.01	0.05	0.01	0.06	0.01
Marital status	Married	0.51	0.51	0.50	0.50	0.49	0.49	0.46	0.46
	Missing	0.07	0.01	0.05	0.01	0.05	0.01	0.06	0.01
Country of origin	Denmark	0.82	0.88	0.83	0.87	0.84	0.87	0.81	0.84
	Missing	0.07	0.01	0.05	0.01	0.05	0.01	0.06	0.01
Education	At most high school	0.36	0.38	0.33	0.34	0.31	0.30	0.31	0.29
	Vocational	0.40	0.35	0.39	0.32	0.38	0.29	0.35	0.25
	Low tertiary	0.16	0.22	0.19	0.27	0.20	0.31	0.20	0.32
	High tertiary	0.08	0.06	0.09	0.08	0.11	0.10	0.14	0.14
Earnings	Q1	0.28	0.27	0.26	0.26	0.23	0.23	0.23	0.24
	Q2	0.28	0.30	0.25	0.25	0.25	0.23	0.22	0.23
	Q3	0.27	0.24	0.26	0.25	0.25	0.27	0.24	0.25
	Q4	0.17	0.20	0.23	0.24	0.28	0.28	0.32	0.28
	Any earnings	0.82	0.88	0.84	0.89	0.87	0.90	0.87	0.90
Number of children		266,571		258,222		258,363		299,941	

Notes: The sample consists of all children born 1997-2013, broken down by several year bins across columns. The cells show the fractions falling in the categories indicated by the row headings. Statistics are shown separately for the fathers ("Pat.") and mothers ("Mat.") of these children. All parental variables are measured in the year prior to birth of the child. Missing values of education are coded as "At most high school," and missing and zero values of earnings are coded as Q1. Labor market earnings are deflated using Statistics Denmark's Net Price Index for 2013 (https://www.dst.dk/en/Statistik/emner/priser-og-forbrug/forbrugerpriser/nettoprisindeks). The quartiles of earnings are determined separately for mothers and fathers based on the entire sample.

Table A2 Parental characteristics of the adolescent HPV vaccination sample (1997-2007 birth cohorts)

		1997-2007 Cohorts		Younger Cousins		Older Cousins	
		Paternal	Maternal	Paternal	Maternal	Paternal	Maternal
Age	<35	0.06	0.07	0.03	0.04	0.05	0.07
	35-39	0.13	0.22	0.11	0.20	0.15	0.24
	40-44	0.34	0.39	0.35	0.42	0.38	0.43
	>44	0.47	0.31	0.51	0.34	0.42	0.27
	Missing	0.04	0.02	0.02	0.00	0.03	0.01
Marital status	Married	0.67	0.67	0.68	0.68	0.68	0.68
	Missing	0.04	0.04	0.02	0.02	0.03	0.03
Origin country	Denmark	0.88	0.87	0.94	0.94	0.94	0.94
	Missing	0.04	0.02	0.02	0.00	0.03	0.01
Education	At most high school	0.28	0.24	0.24	0.19	0.26	0.21
	Vocational	0.39	0.34	0.41	0.35	0.41	0.36
	Low tertiary	0.20	0.31	0.22	0.34	0.21	0.32
	High tertiary	0.12	0.11	0.13	0.12	0.12	0.11
Earnings	Q1	0.24	0.24	0.21	0.22	0.22	0.22
	Q2	0.24	0.25	0.24	0.24	0.25	0.26
	Q3	0.24	0.25	0.26	0.26	0.25	0.26
	Q4	0.24	0.25	0.27	0.28	0.25	0.25
	Any earnings	0.85	0.83	0.87	0.85	0.86	0.85
	Missing	0.02	0.02	0.00	0.00	0.01	0.01
Number of children		346,493		107,696		87,555	

Notes: The sample consists of girls born 1997-2007. The first two columns are for the full sample, and the samples in the remaining columns are broken down by whether the girl is a younger or older cousin in our analysis of spillovers across extended families. The cells show the fractions falling in the categories indicated by the row headings. Statistics are shown separately for the fathers ("Paternal") and mothers ("Maternal") of these girls. Characteristics are measured in the year prior to turning 12, which is the age of eligibility for HPV vaccination. Missing values of education are coded as "At most high school," and missing and zero values of earnings are coded as Q1. Labor market earnings are deflated using Statistics Denmark's Net Price Index for 2013. The quartiles of earnings are determined separately for mothers and fathers based on the entire sample.

Table A3 Bias-corrected standard deviations and correlations, PVPs based on different samples

	PVP – Baseline	PVP – Relocators	PVP – Closures	PVP – Boys	PVP – Late cohorts
PVP - Baseline	**0.018**				
PVP - Relocators	0.556	**0.021**			
PVP – Closures	0.574	0.483	**0.032**		
PVP - Boys	0.796	0.636	0.638	**0.014**	
PVP - Late cohorts	0.833	0.630	0.675	0.433	**0.013**

Notes: This table presents bias-corrected standard deviations of provider vaccination propensities (PVPs) estimated on various subsamples in the on-diagonal cells (in bold). The bias correction is carried out using the leave-out estimation approach developed by Kline, Saggio, and Sølvsten (2020). The off-diagonal cells report correlations between the PVP estimates from one sample split (of mothers) for the subsample indicated in the column heading on the other sample split (of mothers) for the subsample indicated in the row heading. See the notes to Table 4 for more details on how the subsamples for estimating the PVPs are defined.

Table A4 Correlations between provider vaccination propensities and practice characteristics, all practices

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of providers	2	0.003**	0.003***	0.000	0.002**	0.003**	0.002**	0.002	0.002
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	3+	0.006***	0.007***	0.003*	0.006***	0.007***	0.006***	0.006***	0.006***
		(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Education practice		-0.002	-0.001	-0.002*	-0.002	-0.002	-0.002*	-0.003**	-0.003**
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Caseload per provider	Q2	0.002	0.002	0.000	0.002	0.002	0.002	0.000	0.000
		(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
	Q3	0.001	0.001	-0.001	0.001	0.001	0.001	-0.001	-0.001
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
	Q4	0.003	0.003	0.001	0.003	0.002	0.003	0.001	0.001
		(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Average fraction female of providers		0.006***	0.006***	0.003	0.006***	0.007***	0.005***	0.004**	0.004**
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Average age of providers	Q2	0.000	-0.000	0.000	0.000	0.001	0.000	0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
	Q3	-0.001	-0.002*	-0.001	-0.001	-0.001	-0.001	-0.002	-0.002
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	Q4	0.000	-0.002	0.001	0.000	0.001	-0.000	-0.000	0.000
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Up-to-date status of own children			0.009*						
			(0.005)						
Up-to-date status of own grandchildren			0.006						
			(0.007)						
Well-child visit compliance	Q2			0.014***					
				(0.002)					
	Q3			0.021***					

				(0.002)					
	Q4			0.028***					
				(0.003)					
Outreach practice					0.002				
					(0.002)				
Flu vaccine compliance	Q2					0.002			
						(0.002)			
	Q3					0.006***			
						(0.002)			
	Q4					0.012***			
						(0.002)			
Patients without avoidable hosp.	Q2						0.003		
							(0.002)		
	Q3						0.005**		
							(0.002)		
	Q4						0.008***		
							(0.002)		
Practice style metric 1								0.019***	
								(0.003)	
Practice style metric 2									0.019***
									(0.003)
Observations		3,253	3,253	3,253	3,253	3,253	3,253	3,253	3,253
R-squared		0.175	0.177	0.241	0.175	0.186	0.178	0.229	0.238

Notes: Each column reports point estimates (with standard errors robust to clustering by municipality in parentheses) from a separate OLS regression. In each case, the dependent variable is the provider vaccination propensity estimated on the full child sample, and the estimation sample includes all practices. All models contain municipality fixed effects and indicators for the first and last year we observe the practice, as well as indicators for missing values for each of the provider characteristics. Different columns include different measures of provider attitudes/practice styles. *** p<0.01, ** p<0.05, * p<0.10

Table A5 Correlations between provider vaccination propensity and provider characteristics, single-provider practices

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education practice		-0.001	0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Caseload per provider	Q2	0.002	0.002	0.000	0.002	0.002	0.002	0.000	0.000
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	Q3	0.003	0.003	0.001	0.003	0.003	0.003	0.001	0.001
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	Q4	0.005*	0.005	0.003	0.005	0.004	0.005	0.003	0.002
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Average fraction female of providers		0.006***	0.006***	0.003	0.006***	0.007***	0.006***	0.004**	0.004**
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Average age of providers	Q2	0.001	0.000	0.001	0.001	0.001	0.001	0.002	0.002
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	Q3	-0.001	-0.002	-0.001	-0.001	-0.000	-0.001	-0.001	-0.001
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	Q4	0.001	-0.000	0.003	0.001	0.002	0.001	0.002	0.002
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Up-to-date status of own children			0.012*						
			(0.006)						
Up-to-date status of own grandchildren			0.002						
			(0.009)						
Well-child visit compliance	Q2			0.015***					
				(0.002)					
	Q3			0.023***					
				(0.003)					
	Q4			0.030***					
				(0.004)					
Outreach practice					0.002				

					(0.002)				
Flu vaccine compliance	Q2					0.002			
						(0.003)			
	Q3					0.005*			
						(0.003)			
	Q4					0.013***			
						(0.003)			
Patients without avoidable hosp.	Q2						0.003*		
							(0.002)		
	Q3						0.005**		
							(0.002)		
	Q4						0.008***		
							(0.003)		
Practice style metric 1								0.020***	
								(0.003)	
Practice style metric 2									0.021***
									(0.003)
Observations		2,370	2,370	2,370	2,370	2,370	2,370	2,370	2,370
R-squared		0.179	0.181	0.248	0.179	0.189	0.182	0.236	0.246

Notes: Each column reports point estimates (with standard errors robust to clustering by municipality in parentheses) from a separate OLS regression. In each case, the dependent variable is the provider vaccination propensity estimated on the full sample, but the estimation sample is limited to single-provider practices. All models contain municipality fixed effects and indicators for the first and last year we observe the practice, as well as indicators for missing values for each of the provider characteristics. Different columns include different measures of provider attitudes/practice styles. *** p<0.01, ** p<0.05, * p<0.10

Table A6 Effects of provider vaccination propensity on HPV vaccine uptake, by instrument set

	6 months after HPV eligibility			12 months after HPV eligibility			18 months after HPV eligibility		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Provider vaccination propensity	1.094	1.181	1.101	1.073	1.142	1.066	1.039	1.098	1.025
	(0.122)	(0.173)	(0.125)	(0.128)	(0.163)	(0.128)	(0.137)	(0.161)	(0.136)
Instrument set	1	2	3	1	2	3	1	2	3
F-statistic instruments	763	241	114	763	241	114	763	241	114

Notes: Each column reports point estimates (with standard errors robust to clustering by municipality in parentheses) from a separate instrumental variables regression. Across columns 1-3, 4-6, and 7-9, the dependent variables are indicators for initiating the HPV series within 6 months, 12 months, and 18 months of eligibility, respectively. All models contain municipality and birth year-by-birth month fixed effects, the girl's average compliance under the childhood vaccination program, and parental characteristics. The key control variable of interest is the provider vaccination propensity for the provider at the time the girl turns 12 and becomes eligible for the HPV vaccination. To address measurement error, the estimated propensity from one sample split (of mothers) is instrumented using the estimate from the second sample split in columns 1, 4, and 7. In columns 2, 5, and 8, the instrument set includes the estimate from the second sample split interacted with the number of identifying patient transfers. In columns 3, 6, and 9, the instrument set includes interactions between the estimate from the second sample split and indicators for more differentiated bins of the number of identifying patient transfers per spit than the baseline approach (i.e., by ranges of 25 transfers up to a limiting bin of 300+). The sample is girls (n=346,493) from birth cohorts 1997-2007. All estimates shown are statistically significant at the 1% level.