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Direct and Spillover Effects of Provider Vaccination Facilitation

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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 facilitate recommended vaccinations from birth to age 6 based on a two-way fixed effects model. We then show that 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 the uptake of patients' younger cousins by one-fifth as much as own patients' uptake.

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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; Moghtaderi and Adams, 2016). Most recently, physician attitudes towards COVID-19 vaccines are shown to affect their patients' vaccination rates (Steinmayr and Rossi, 2024). 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 (Brewer et al., 2021; Callaghan et al., 2022). 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 driven by residential moves. 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, around 50% of the upcoming change in estimated provider vaccination propensities (PVPs) is reflected in the change in own vaccine compliance in the subsequent quarter. A one standard deviation increase in PVP is associated with a 2.1 percentage point increase in the fraction of recommended vaccine visits that have been completed, which reflects a 19% reduction in noncompliance. The variation across providers is 15% 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 their attitudes, which are 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 0.09 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 1.8% more likely to have initiated the vaccination series within 18 months of eligibility, representing a 7.4% reduction in noncompliance. This 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 4.8 percentage points within 18 months of eligibility. 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.<sup>1</sup> Rates are found to respond to vaccination recommendations (Lawler, 2020), school and childcare vaccination requirements (Abrevaya and Mulligan, 2011; Carpenter and Lawler, 2019) and permitted exemptions (Churchill, 2021a; Hair et al., 2021), insurance coverage and financial incentives (Banerjee et al., 2025; Churchill, 2021b), and media campaigns as well as other types of informational treatments

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<sup>1</sup> A related recent literature studies the role of mandates, incentives, and media campaigns in COVID-19 vaccine uptake (e.g., Barber and West, 2022; Campos-Mercade et al., 2024; Ho et al., 2023; Karaivanov et al., 2022).

(Chang, 2018; Giaccherini and Kopinska, 2022; Hansen and Schmidtblaicher, 2021; Hansen et al., 2020).<sup>2</sup> Most related to our work is the evidence related to levers primary care physicians can pull. Sending reminder letters and text messages has been found to be effective (Banerjee et al., 2025; Hirani, 2021; Hirani and Wüst, 2024; Milkman et al., 2021), as has training providers to promote vaccination and manage patient hesitancy (Brewer et al., 2021). Given that we find that provider attitudes seem to play a role in their effectiveness, training and/or incentivizing less-effective providers will likely be a valuable complement to any other interventions.

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 and physician treatment intensity – see Currie et al. (2024) and Deryugina and Molitor (2021) for reviews of this literature. Efforts to isolate the causal impacts of primary care physicians reveal significant impacts on the medical decisions and health outcomes of their patients.<sup>3</sup> 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 (2025) show that the most effective primary care physicians utilize fewer resources.<sup>4</sup> Several papers reach similar conclusions in the Scandinavian context, in which our paper is set. Ginja et al. (2025) find substantial variation in the quality of primary care physicians as measured by patient mortality, but patients seem unable to identify who the high-quality doctors are. Huang and Ullrich (Forthcoming) show that between-practice variation in antibiotic prescribing does not appear to affect patient health, whereas Simeonova et al. (2024) demonstrate that a physician’s ability to facilitate adherence with prescription drugs improves patient health. Related to female health, physicians affect take-up of oral contraceptive pills (Costa-Ramón et al., 2023), and male physicians with daughters have different practice styles and achieve better outcomes for their female patients than those without (Gørtz et al., 2024).

Our paper is different from these studies in that it focuses on the effect of provider practice styles on childhood and adolescent vaccinations. This is an important public health topic in light

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<sup>2</sup> These studies find that media coverage of alleged vaccine-related risks has negative effects on vaccine uptake.

<sup>3</sup> Studies find important impacts of specialists as well (e.g., Currie et al., 2016), and benefits to improving their diagnostic skills (e.g., Chan et al., 2022).

<sup>4</sup> 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.

of below-target vaccination rates and outbreaks of preventable diseases, such as measles, in recent years. Further, we demonstrate that effective providers can shield patients from the spread of misinformation, which has been a particularly salient feature of the vaccine domain.

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 (e.g., Bouckaert et al., 2020; Humlum et al., 2024; Itaya et al., 2018). Notably, since HPV is sexually transmitted, our case of learning from cousins is unique in that the scope for free riding is 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 aged 0-5 years. 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 2019.<sup>5</sup>

While there are no mandatory vaccination requirements to attend daycare or school and all vaccinations are voluntary, the last column in Table 1 shows that compliance with the recommended 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 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, but vaccine hesitancy remains the main reason for under-vaccination in the Danish context (Hirani, 2021).

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 in 2013-14 due to media publicity of reports about possible adverse side effects (Hansen and Schmidtlaicher, 2021). 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 Schmidtlaicher, 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).

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,

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<sup>5</sup> Boys turning 12 on or after July 1, 2019, have also been offered the vaccine in the childhood **vaccination** program.

unvaccinated patients tend to have doctors who lack confidence in the safety of vaccines and the health benefits to individuals and communities (Salmon et al., 2008). 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. In Denmark, 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.

### **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.<sup>6</sup> This constitutes more than 1 million children.<sup>7</sup> 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.

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.<sup>8</sup> 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.<sup>9</sup> 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.

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<sup>6</sup> 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.

<sup>7</sup> Appendix Table A1 provides summary statistics for parental background characteristics for this sample.

<sup>8</sup> For the rare cases where the number of visits exceeds the number recommended, we classify the child as being fully compliant.

<sup>9</sup> 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.

(2016).<sup>10</sup> 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, although physicians tend to converge in their practice styles within a group (Doyle and Staiger, 2025).

### ***3.2 Adolescent dataset***

Relative to the child panel, the adolescent dataset consists of a narrower sample of girls born 1997 to 2007 (n=346,417) observed in a several-year window around their 12<sup>th</sup> birthdays. These 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.<sup>11</sup>

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.<sup>12</sup>

To evaluate the role of providers in HPV vaccination uptake, we link these girls to their providers at the time they turn 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 used to construct a measure of provider effectiveness under the early childhood phase of the vaccination program, and the steps that we take to

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<sup>10</sup> If the child's provider is missing, we set this equal to the maternal provider.

<sup>11</sup> Appendix Table A2 provides summary statistics for parental background characteristics for this sample.

<sup>12</sup> Appendix Figure A1 shows uptake by months relative to eligibility for the first 12 months.

validate this measure.

#### 4.1 Two-way fixed effects strategy

The practice style or attitude of a provider toward vaccines is not directly observable; 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 generally not 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 proclivities when estimating provider vaccination propensities.

We choose to do this in a framework designed to capture unobserved heterogeneity in two dimensions – the patient and the provider. These two-way fixed effects models were pioneered by Abowd et al. (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 et al., 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 model:

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

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.<sup>13</sup> 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 ( $\alpha_m$ ) 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

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<sup>13</sup> We exclude the first quarter ( $q = 1$ ) since no vaccines are recommended until the child turns 3 months old.

child’s gender, birth order, and birth year in the control set ( $X_i$ ).<sup>14</sup> The primary care provider fixed effects ( $\gamma_{j(i,q)}$ ) capture the extent to which providers facilitate patient compliance with vaccination guidelines.

In this model, identification of the provider fixed effects comes only from patients who transfer across providers. To focus on patient resorting that is plausibly unrelated to relationships with providers, we utilize a residential mover estimation sample. The only transfers included are those that occur along with relocation of the family to a new municipality. The other children included in the sample are those who remain with the same provider throughout.<sup>15</sup> Fortunately, our sample size is large enough and move-motivated transfers are frequent enough that nearly all providers (99.8%) are connected. This is important since the fixed effects are estimated relative to an arbitrary omitted provider, so they are only comparable within sets of providers that are connected by patient mobility.

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. 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 provider fixed effects estimates, recentered around the mean across providers, from ordinary least-squares estimation of equation (1) on the residential mover sample.<sup>16</sup> The fact that these are estimates means that the variance

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<sup>14</sup> Studies find differences in vaccination and other early healthcare investments by gender (e.g., Barcellos, 2014; Jayachandran and Kuziemko, 2011) and birth order (e.g., Lehmann et al., 2018; Pruckner et al., 2021), and vaccination requirements vary across birth cohorts.

<sup>15</sup> While we feature estimates from the mover sample, we explore robustness to using alternative childhood estimation samples, such as only including transfers associated with clinic closures. We choose to prioritize the mover sample since it reflects typical patterns of across-provider mobility, while closures lead to systematic shifts from older (retiring) doctors to younger (less experienced) doctors (Simonsen et al, 2021).

<sup>16</sup> 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

will be overstated since it includes estimation error, and that there will be attenuation bias if these estimates are used as explanatory variables (Andrews et al., 2008). Since precision increases with the number of identifying patient transfers, providers observed with estimates in the tails will tend to be those serving the fewest patients.<sup>17</sup>

The first strategy 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.<sup>18</sup> The logic is that less reliable estimates are shrunk toward the mean (of zero) by multiplying them by an estimate of the signal to signal-plus-noise ratio. In the spirit of Chetty et al. (2014), 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 police commanders and teachers, 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 in the mover sample into two groups and then estimate equation (1) for 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:

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

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 effects estimates across the two sample splits 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.<sup>19</sup> We then use the bin-specific slope estimates to extract the predicted values:

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

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estimates that might otherwise have been inputs to measurement error correction.

<sup>17</sup> This “limited mobility bias” issue is illustrated in Appendix Figure A2.

<sup>18</sup> 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.

<sup>19</sup> Appendix Figure A3 shows how the slope varies across the provider subsamples defined by the number of identifying transfers in the second sample split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+).

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

Figure 3 shows the distribution of shrunken PVPs across our sample of 3,614 practices.<sup>20</sup> While the standard deviation of the original fixed effects estimates is 0.025, the standard deviation of the shrunken estimates is 0.020. 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 0.021. In the context of the statistical model, a natural way to interpret this magnitude is in terms of implications for patient compliance. For example, a provider that is one standard deviation above average boosts the up-to-date status of child patients by 2.1 percentage points (pp). This magnitude is meaningful relative to the average rate of noncompliance (11%) and the standard deviation of compliance across families (14 pp).

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. 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 sample sizes and high mobility support such an approach (e.g., Drenik et al., 2023; Goldschmidt and Schmieder, 2017), have been framed from this instrumental variables perspective. When we include our PVP estimates as regressors, our results are very similar whether we directly include the shrunken estimates or instrument for the raw estimates.

#### ***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

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<sup>20</sup> Appendix Figure A4 compares this distribution to that 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 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.

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 quartiles.

To get at provider effectiveness across more and less compliant patients, we apply the method put forward by Card et al. (2013). That is, we calculate the average residuals from equation (1) across deciles of mother and (shrunk) provider fixed effects. These 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.<sup>21</sup> However, average residuals are generally small relative to the variation in estimated provider propensities, other than for the least compliant bottom-decile mothers, suggesting that imposing additivity is a reasonable simplification.

Next, we consider whether mobility across providers is plausibly conditionally exogenous. Importantly, the model does accommodate sorting based on time-invariant factors, such as if families that prioritize preventative care tend to choose (or transfer to) doctors that also do so. What would be problematic is 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. As a first tactic, we have limited the estimation sample to patient transfers that are driven by residential moves.

As a second tactic, we explore whether the evolution of a child's vaccination compliance leading up to a provider transfer predicts the nature of the upcoming transfer, in terms of the realized change in PVP. Adapting the approach pioneered by Chetty et al. (2014), we estimate a scaled dynamic difference-in-differences model in the window from 4 quarters before to 3 quarters after patient transfers.<sup>22</sup> The sample is limited to spells involving a transfer that coincides with aging into a new requirement (with requirements and providers otherwise stable

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<sup>21</sup> See Appendix Figure A6.

<sup>22</sup> Others have adopted similar approaches using patient moves (Finkelstein et al., 2016), physician moves (Molitor, 2018), or both (Badinski et al., 2023).

over the 8 quarters).<sup>23</sup> Estimates of the coefficients on interactions between relative quarter and the upcoming change in PVP are graphed in Figure 5. The estimates prior to the transfer are flat at 0 and then jump to around 0.5 afterward. Though the impact of the change in PVP is less than 1-for-1 (as would be the case if our estimates are forecast-unbiased), the impact is nonetheless sizable and there are no signs of pre-trends.

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 other 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.<sup>24</sup> In order to condition on common confounding factors that might drive this clustering, we include municipality fixed effects as controls in our subsequent analyses. Around 5% of the variation in PVPs is explained by municipality indicators.

All in all, the evidence supports that our statistical model, while simple, provides a sensible approximation to compliance. In Section 6, we further bolster confidence in our PVP estimates when 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 (Brewer et al., 2021). Own attitudes and knowledge regarding the net benefits of any given vaccination also surely play a role. To separate these from skill, we

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<sup>23</sup> We make this restriction to address the fact that compliance cannot shift downward without a change in requirements. This critique also applies to our PVP estimation strategy, and unmodeled dynamics might be expected to mute our estimates. Nonetheless, PVPs from an alternate version estimated only on quarters that coincide with new requirements are highly correlated (0.66) and are shown to yield similar impacts on adolescent compliance in Section 6.

<sup>24</sup> See the maps in Appendix Figure A7.



use information about the adherence of providers' children and grandchildren.<sup>25</sup>

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 52 years of age. Providers' children and grandchildren are on average 87.2% and 89.4% 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.<sup>26</sup> 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 0.021.

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 0.004 and 0.006 higher in practices with 2 and 3+ providers, respectively, relative to solo practices.<sup>27</sup> These

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<sup>25</sup> With our time-invariant PVP estimates, we are unable to study the role of experience. However, we find that persistence across the first and second halves of providers' lifespans is very high, with a 0.01 increase in the period 1 PVP estimate associated with a 0.0095 increase in period 2 (depicted in Appendix Figure A8).

<sup>26</sup> Appendix Tables A3 and A4 are the corresponding tables for all and for single-provider practices, respectively.

<sup>27</sup> Bundorf et al. (2023) similarly find that solo providers are less compliant in terms of opioid prescriptions.

point estimates map to 0.19 and 0.29 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 0.008 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 0.09 standard deviation increase in provider vaccination propensity ( $\{0.151 \times 0.012\}/0.021$ ). 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 1.76 standard deviations (0.037) 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 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 0.015 and 0.012, respectively.

In summary, providers that more effectively promote childhood vaccination compliance appear to have more positive attitudes toward compliance with the program, as evidenced by the compliance rates of own children. As far as their practice styles, what most differentiates these providers is adherence to other childhood preventative care schedules. They 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 346,417 adolescent girls born 1997 to 2007 observed in a several-year window around their 12<sup>th</sup> 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.<sup>28</sup> 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 point estimates (and 95% confidence intervals) shown are for the coefficients on interactions between relative month indicators and PVP.<sup>29</sup> 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, under which the PVP estimate from one sample split of mothers (in the childhood mover sample) is instrumented using the estimate from the other split interacted with bins for the number of identifying patient transfers.

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

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<sup>28</sup> Though our baseline PVP estimates are from 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.

<sup>29</sup> Appendix Figure A9 shows an alternative version comparing HPV uptake for top vs. bottom quartile providers.

percentage points ( $1.61 \times 0.021$ ), or 6%, 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, particularly since we find attentive mothers match to attentive providers under the childhood vaccination program (as we can infer from the positive correlation between patient and provider fixed effect estimates from our statistical model of childhood compliance). Thus, we turn to regression analyses where we can control for girls' compliance under the childhood phase of the 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 of eligibility. For each outcome, we report results from three instrumental variables specifications that differ in their control sets. In all cases, we use our split sample binned approach to instrument for PVP. 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 25% 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 0.021) is associated with a 2.0 percentage point increase in the likelihood of initiation by 6 months ( $0.961 \times 0.021$ ), declining to a 1.8 percentage point increase by 18 months ( $0.850 \times 0.021$ ). Recasting, the 6-month (18-month) effect maps to a 4.8% (7.4%) reduction in noncompliance.

Table 4 shows that these results are robust to PVPs estimated on alternative childhood vaccination samples. The instrumental variables specifications in this table match the most saturated models from Table 3, other than that the PVP estimates are standardized (by dividing by their standard deviations) to ease comparisons.<sup>30</sup> In columns 2 and 7, transfers related to clinic

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<sup>30</sup> The notes to Table 4 provide more details on the construction of the alternative samples. Bias-corrected standard deviations of the PVP estimates and correlations across estimates are reported in Appendix Table A5.

closures (instead of residential moves) are used for identification of PVPs. The next two samples considered for PVP estimation are subsamples of the residential mover sample: boys only (in columns 3 and 8) and late birth cohorts only (2008-2013, in columns 4 and 9). Both subsamples are fully out-of-sample since the HPV sample consists only of girls born before 2008. Finally, we use the full mover sample, like the baseline, but only include quarters that coincide with new vaccination requirements in the estimation of PVPs (in columns 5 and 10). This addresses the issue that a fully compliant child's up-to-date status can only change when aging into new requirements. In all the alternative estimations, the implied movements in HPV uptake associated with a one standard deviation change in PVP are similar to the baseline estimates presented in columns 1 and 6, and range between 1.6 and 1.9 percentage points at 18 months after eligibility.<sup>31</sup>

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 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 40 percentage points less likely to initiate the series within 12 months of eligibility, and rates are still depressed by 30 percentage points after two years.

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

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<sup>31</sup> Appendix Table A6 shows the results are also quantitatively robust to alternative instrument sets that differ in the types of interactions that are included between the number of identifying transfers and the PVP estimate from the second sample split.

the 2003 cohort as patients of top-quartile PVP providers rebounded from the shock more quickly than those of bottom-quartile PVP providers.<sup>32</sup>

Table 5 presents triple differences estimates for HPV vaccine initiation over the three horizons for all 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 5.3 and 4.8 percentage points higher for providers in quartiles 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 eligibility for younger cousins to the PVP of paired older cousins. We focus on the older cousin that is closest 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 effect that is about one-tenth as great 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

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<sup>32</sup> Similarly, Humlum et al. (2021) find that vaccination rates decreased less among daughters of health professionals relative to other girls.

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 approximately half as great as spillovers to younger cousins from relative-age induced ADHD diagnoses (Persson et al., 2024) and to coworkers from physician-induced changes to sick-leave uptake (Dale-Olsen and Godøy, 2018). As to vaccinations, Campos-Mercade et al. (2024) find substantially larger spillover effects of COVID-19 vaccination incentives on partners (more than half of own effect), but no statistically significant effects on parents or children. Overall, we view the magnitudes of the spillover effects we find to be reasonable, considering that HPV vaccination is neither observable to extended family members nor affects them directly.

## 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 2.1 percentage points more likely to be up to date. This is 15% 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 2.0 percentage point increase in the likelihood of HPV vaccination initiation by 6 months after eligibility, which persists up to 18 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.

Back-of-the-envelope calculations reveal that our estimate of a 2.0 percentage point increase in HPV vaccine uptake translates into a reduction of 4.2 avoidable cervical cancer cases per birth cohort in Denmark (equivalent to 1.1% reduction).<sup>33</sup> This number is conservative because we

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<sup>33</sup> This number is obtained as follows:  $0.7 \times (376/39,433) \times 0.02 \times 31,492$ , where 0.7 is the share of cervical cancer cases prevented by the HPV vaccine; 376 is the annual number of cases in Denmark; 39,433 is the average size of cohorts currently aged 35-49; 0.02 is our estimated effect of a one standard deviation increase in PVP on HPV

only consider direct effects on own patients in the calculations and do not include spillover effects. Additionally, Skorstengaard et al. (2017) argue that this is only the “tip of the iceberg,” since HPV vaccination would have a much larger impact on preventing cancers in the absence of costly screening, which prevents many cancers but at a much higher cost. If we assume that there is no screening and the more effective nine-valent vaccine is used, a one standard deviation increase in PVP would be associated with a reduction of 15.8 to 25.9 avoidable cervical cancer cases.

The results of our analysis reinforce that primary care providers matter to patient vaccination decisions and related health outcomes. Given that some providers are less effective than others, it is worth exploring ways to improve providers’ abilities to promote patient adherence to vaccination recommendations. As mentioned in the introduction, provider training programs show promising results (Brewer et al., 2021). Related evidence suggests that providers are responsive to peer-comparison feedback (Navathe et al., 2022) and new medical research (DeCicca et al., 2024).

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 much 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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vaccine uptake; and 31,492 is the average size of our 1997-2007 female birth cohorts. The first two figures are taken from Skorstengaard et al. (2017).



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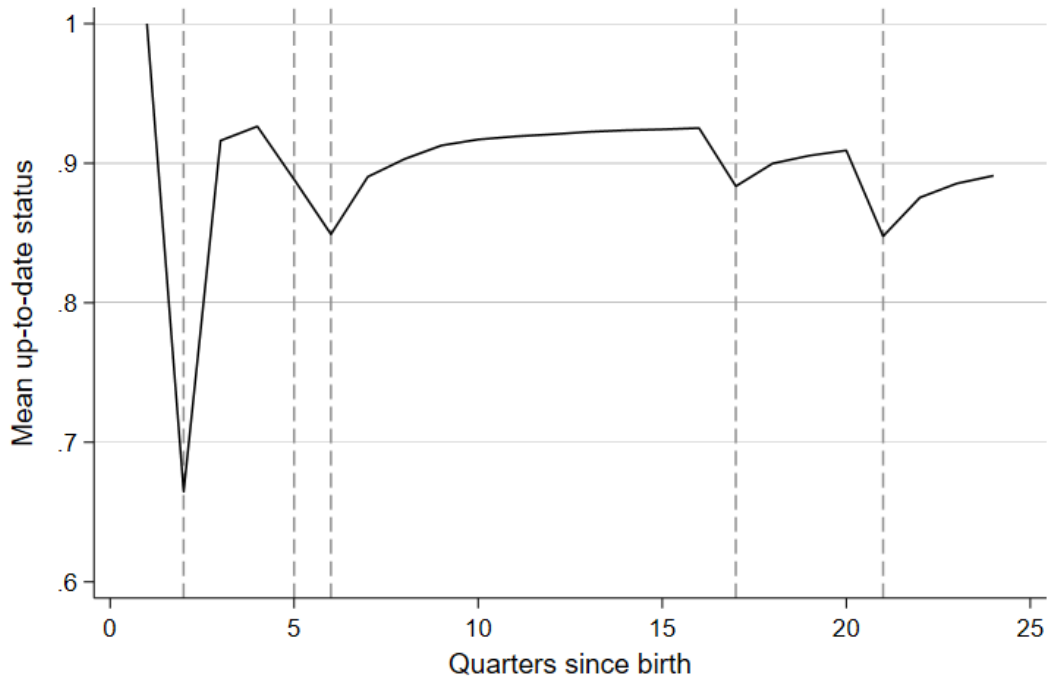
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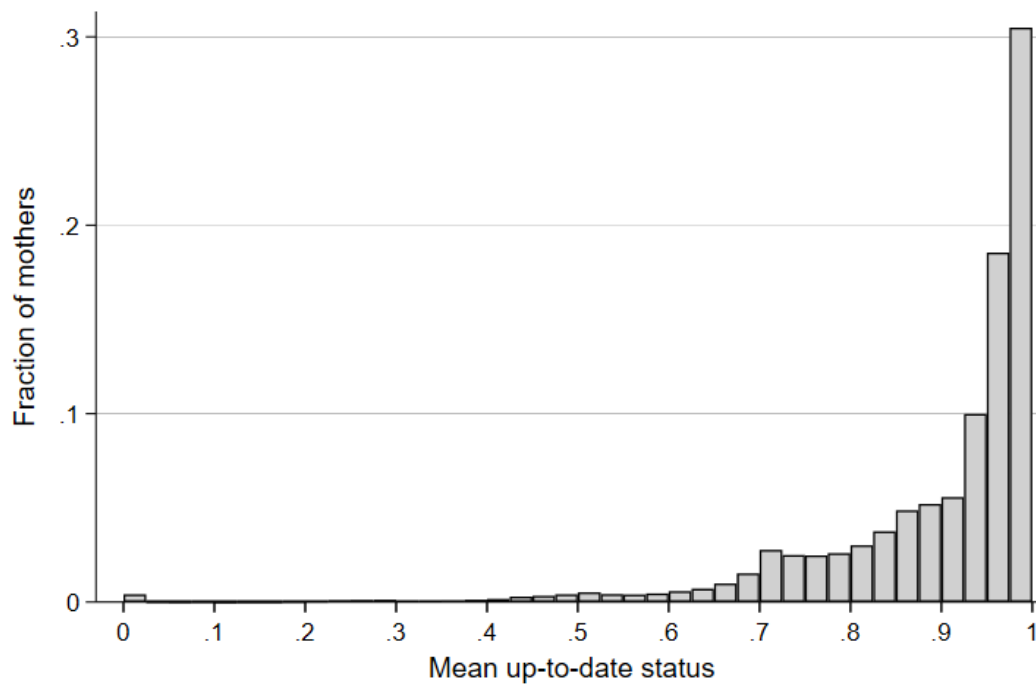
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**Figure 1** Mean up-to-date status by quarters since birth



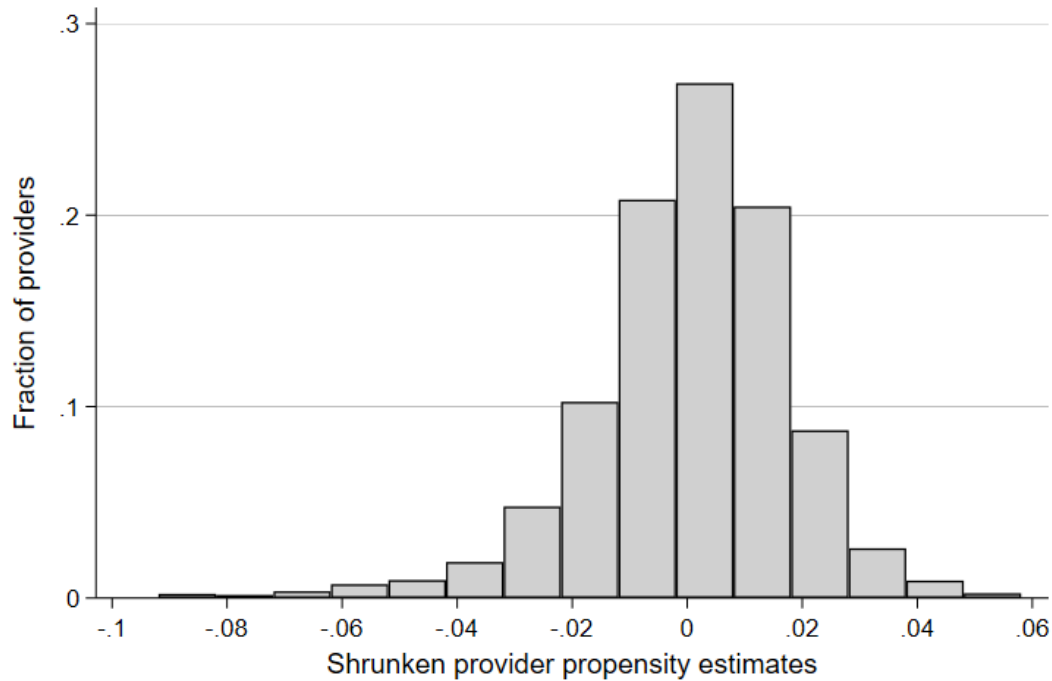
*Notes:* This figure shows average up-to-date status (i.e., fraction of recommended vaccination visits completed) for children by quarters since birth for all children in birth cohorts 1997 to 2013. The vertical dashed lines indicate when children age into new vaccine recommendations. The vaccination 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 the 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 with the vaccination program (i.e., average up-to-date status across child-quarters over the first six years of life, excluding the quarter of birth) for mothers of children in birth cohorts 1997 to 2013. 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.

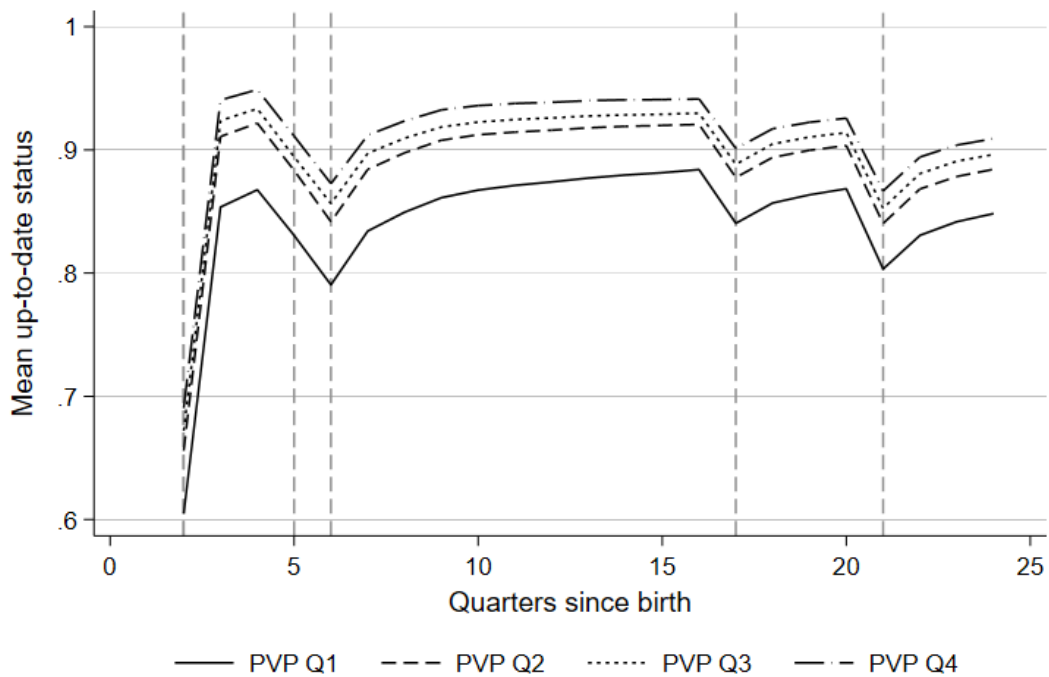
**Figure 3** Distribution of shrunken provider vaccination propensities



*Notes:* This figure depicts the distribution of shrunken provider vaccination propensity (PVP) estimates. The raw PVP estimates are derived from the residential mover sample and shrinkage is carried out using the split-sample instrumental variables approach described in the text. Specifically, 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 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.

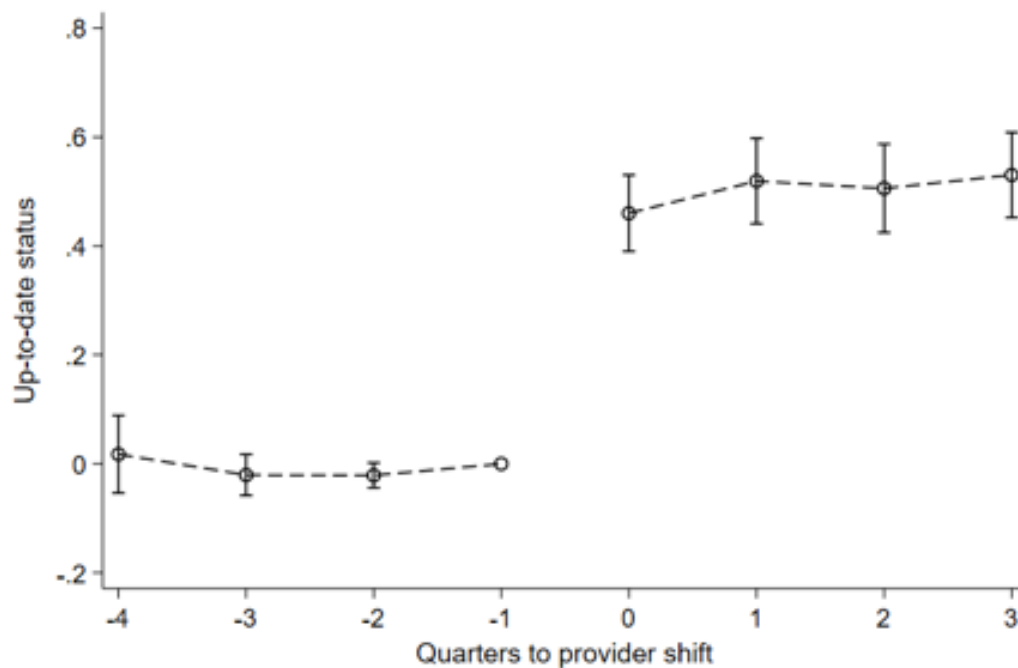


**Figure 4** Mean up-to-date status by quarters since birth and provider vaccination propensity quartile



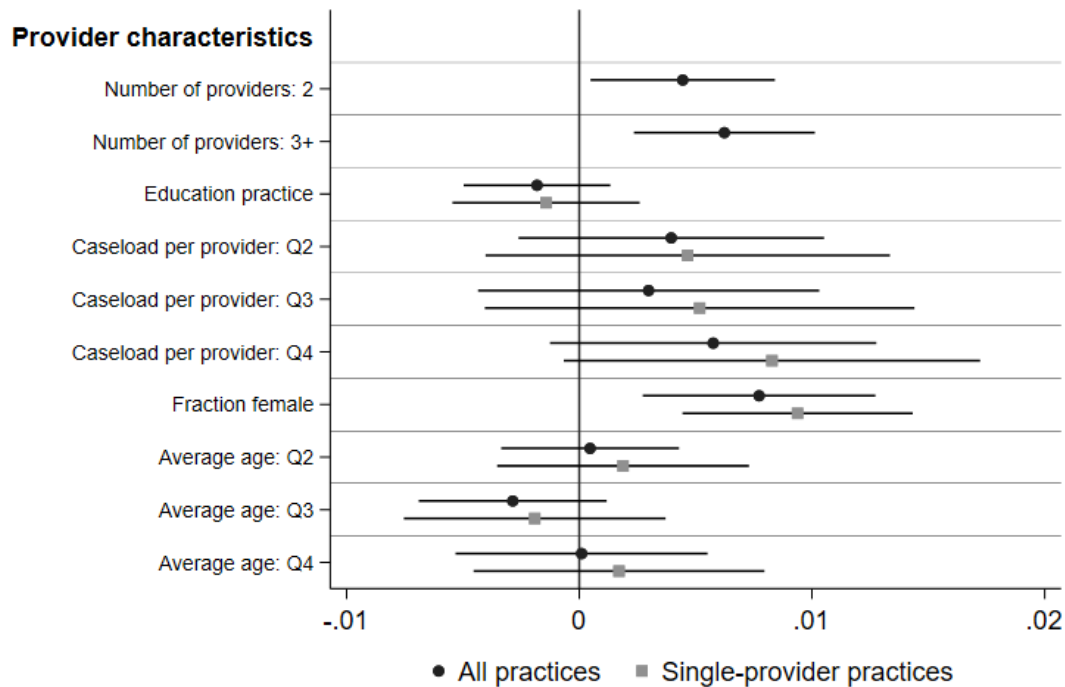
*Notes:* This figure depicts the evolution of compliance with the child vaccination program as a function of quarters since birth by quartile of provider vaccination propensity (shrunk using the method described in the notes to Figure 3). The compliance rates are estimated using a regression (for providers 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. All children from the 1997-2013 birth cohorts are included in the sample.

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



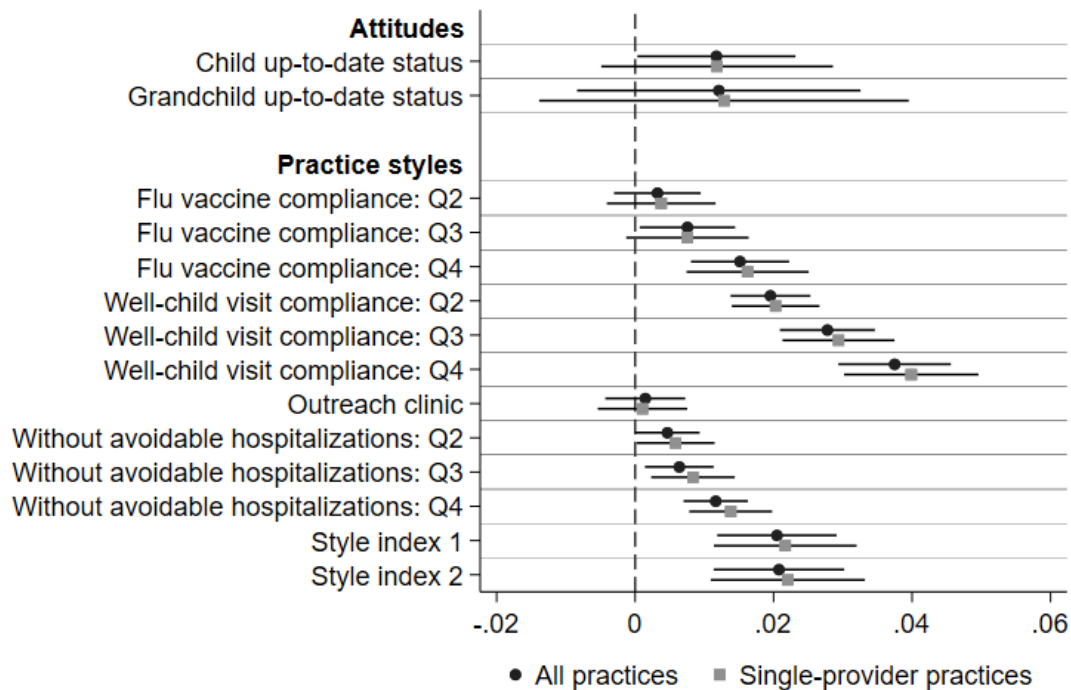
*Notes:* This figure depicts the estimated coefficients for relative quarter indicators interacted with the upcoming change in provider vaccination propensity (PVP). The model is estimated by a series of separate IV regressions for each quarter relative to the quarter before the provider transfer ( $t = -1$ ). Also included in the control set are the main effects for the upcoming change in PVP and quarter relative to the transfer. To ensure the PVP estimates are out-of-sample, the PVPs estimated on boys only are used for girls, and vice versa. The main and interacted upcoming change in PVP variables are instrumented using our split sample IV approach, with estimates from one sample split (from the gender-specific residential mover sample) instrumented using the estimates from the second interacted with the average number of transfers (per sample split) across the origin and destination providers. The standard error bars show the 95% confidence intervals, where standard errors are robust to clustering by municipality of origin. 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 coincides with aging into a new requirement (with requirements otherwise stable over the 8 quarters) and that is bracketed by at least 4 quarters of affiliation with both the origin and destination provider ( $n=17,468$ ).

**Figure 6** Correlations between provider vaccination propensities and provider characteristics



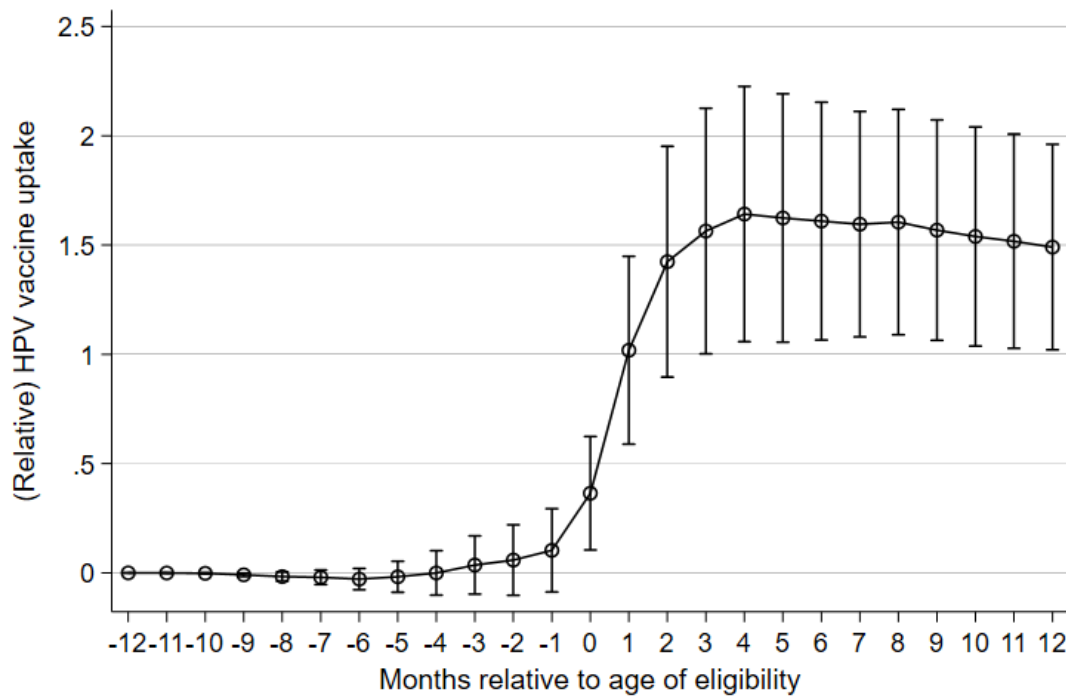
*Notes:* This figure depicts coefficient estimates (and 95% confidence intervals) from OLS regressions of provider vaccination propensities on provider characteristics, estimated separately for all practices (circles) and for single-provider practices (squares). The full set of provider characteristics 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 A3 and A4) 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



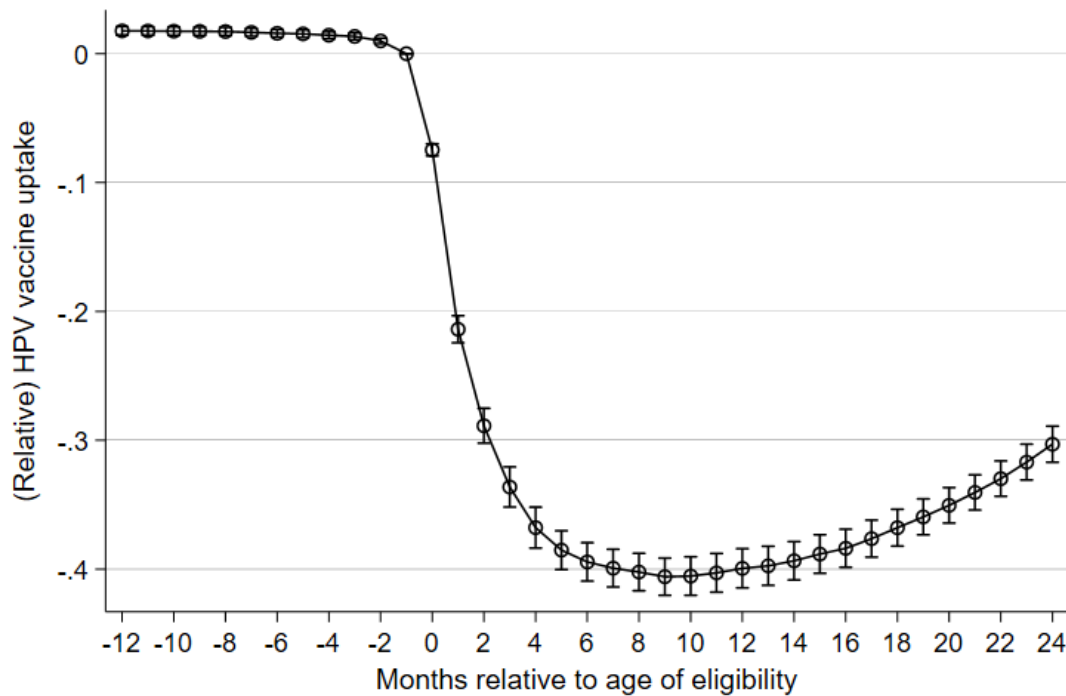
*Notes:* This figure depicts coefficient estimates (and 95% confidence intervals) from regressions of provider vaccination propensities on measures of provider attitudes and practice styles (as reported in columns 2-8 of Appendix Tables A3 and A4). Each of these models includes the full set of provider 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



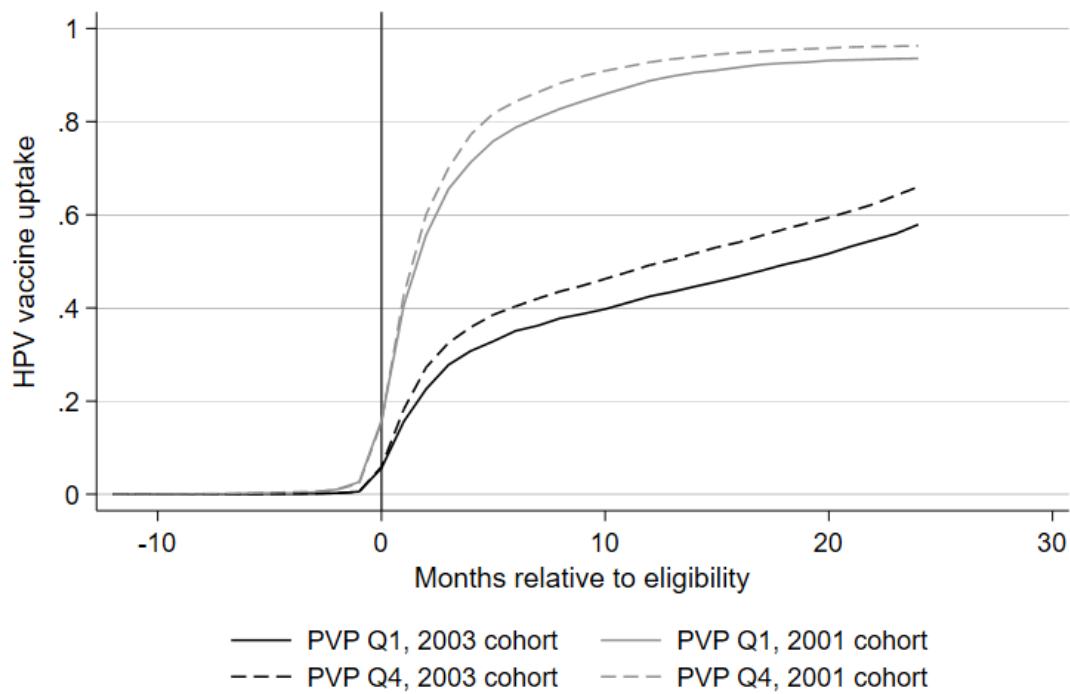
*Notes:* This figure shows coefficient estimates (and 95% confidence intervals) from a dynamic difference-in-differences regression of HPV vaccination initiation around the month when girls age into the recommendation. The models are estimated by a series of separate IV regressions that include the period 12 months before aging into the requirement and the relevant relative month. The coefficients shown are for the 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 effects for PVP and relative month. The main and interacted PVP variables are instrumented using our split sample binned IV approach. Standard errors are robust to clustering by municipality. The sample includes all girls from the 1997-2007 birth cohorts (n=346,417).

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



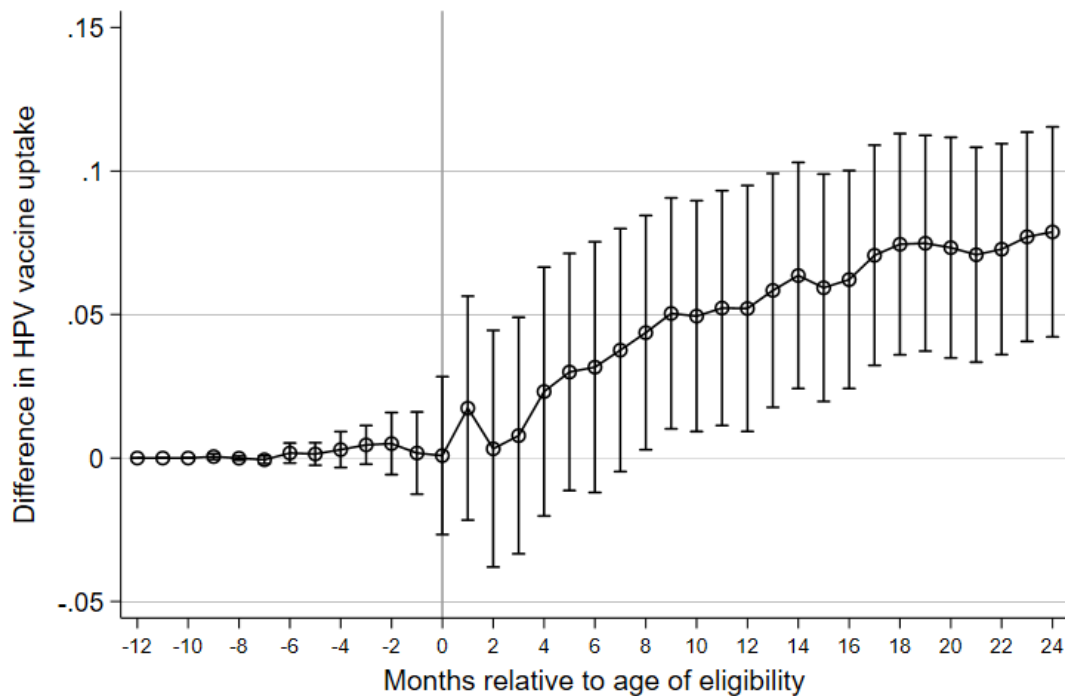
*Notes:* This figure depicts the impact of the 2015 documentary attributing disabling symptoms for several girls to HPV vaccination. The coefficient estimates are from a dynamic difference-in-differences model comparing the 2003 birth cohort (treatment) to the 2001 birth cohort (control) in terms of 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 95% confidence intervals shown are based on standard errors that 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 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 (shrunk 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 (shrunk 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. The regression also includes indicators for month relative to eligibility, for being born in 2003, and for having a top quartile PVP provider and the two-way interactions between these indicators. Standard errors are robust to clustering by municipality at the time of aging in.



**Table 1** Childhood vaccination schedule and coverage, by birth cohort

Child age	Scheduled vaccinations	Coverage
<i>a. Date of birth: January 1, 1997 – March 31, 2004</i>		
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%
<i>b. Date of birth: April 1, 2004 – December 31, 2013</i>		
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. From June 1, 1993 to June 30, 2002, the Hib vaccine was administered as a separate shot. After June 30, 2002, the Hib vaccine was included in the shot for diphtheria, tetanus, pertussis, and polio. 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 children born January 1, 1997 – March 31, 2004) 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	0.728	(0.445)	1	(0)
Number of providers: 2	0.166	(0.372)	0	(0)
Number of providers: 3+	0.107	(0.309)	0	(0)
First year of operation	1998	(6)	1999	(6)
Last year of operation	2011	(5)	2011	(5)
Education practice	0.235	(0.424)	0.158	(0.365)
Caseload per provider	2,054	(763)	2,065	(797)
Average fraction female of providers	0.325	(0.388)	0.297	(0.425)
Average age of providers	52.4	(6.2)	53.2	(6.6)
Average up-to-date status of own children	0.872	(0.151)	0.865	(0.168)
Up-to-date status of children: missing	0.587	(0.493)	0.712	(0.453)
Average up-to-date status of own grandchildren	0.894	(0.112)	0.892	(0.123)
Up-to-date status of grandchildren: missing	0.414	(0.493)	0.486	(0.500)
Well-child visit compliance	0.735	(0.101)	0.723	(0.109)
Outreach clinic	0.822	(0.383)	0.784	(0.412)
Flu vaccine compliance	0.341	(0.156)	0.335	(0.165)
Patients without avoidable hospitalization	0.628	(0.060)	0.622	(0.062)
Practice style metric 1	0.000	(0.568)	-0.061	(0.594)
Practice style metric 2	0.000	(0.558)	-0.058	(0.586)
Observations	3,235		2,354	

*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 estimate vaccination propensities for 3,614 practices, however only 3,235 are present in the dataset with provider characteristics. These 3,235 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 well-child, outreach, flu, 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	1.336 (0.125)	0.997 (0.124)	0.961 (0.122)	1.285 (0.122)	0.955 (0.122)	0.902 (0.119)	1.235 (0.117)	0.916 (0.118)	0.850 (0.115)
Childhood vaccination compliance		0.343 (0.011)	0.320 (0.010)		0.334 (0.010)	0.310 (0.010)		0.323 (0.009)	0.297 (0.009)
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	115	113	113	115	113	113	115	113	113

*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. 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. To address measurement error, the estimated propensity from one sample split (of mothers in the residential mover childhood vaccination analysis sample) is instrumented using interactions between the estimate from the second sample split and indicators for bins of the number of identifying 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 from birth cohorts 1997-2007 (n=346,417). All estimates shown are statistically significant at the 1% level.

**Table 4** Effects of (standardized) provider vaccination propensity on HPV vaccine uptake, different childhood vaccination samples

PVP estimation sample	6 months after HPV eligibility					18 months after HPV eligibility				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Movers	0.020 (0.002)					0.018 (0.002)				
Clinic closures		0.021 (0.003)					0.019 (0.003)			
Movers: boys only			0.019 (0.004)					0.016 (0.003)		
Movers: late cohorts only				0.019 (0.002)					0.018 (0.002)	
Movers: quarters w/new reqs. only					0.017 (0.002)					0.016 (0.002)
F-statistic instruments	107.9	174.5	21.67	109.7	240.8	107.9	174.5	21.67	109.7	240.8

*Notes:* Each column reports point estimates (with standard errors robust to clustering by municipality in parentheses) from a separate instrumental variables regression. Across columns 1-5 and 6-10, the dependent variables are indicators for initiating the HPV series within 6 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. The PVP is standardized using the bias corrected standard error. 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 transfers per split (i.e., 0-24, 25-49, 50-74, 75-99, 100-149, 150-199, and 200+). In our baseline mover sample, the only transfers included are those that coincide with a residential move to a new municipality. In the clinic closures sample, the only transfers included are for those individuals experiencing a practice closure. The "boys only" PVP estimates are based on childhood vaccination data for males, while the "late cohorts" PVP estimates use data from birth cohorts 2008 onward – and in both cases the only transfers included are residential movers. The final subsample uses the full mover sample, like the baseline, but only includes quarters that coincide with new vaccination requirements in the estimation. When estimating PVPs for these mover and closure samples, all other children included in the estimation sample are those who remain with the same provider throughout. In all cases, the HPV uptake sample is girls from birth cohorts 1997-2007. Notably, we can estimate PVPs from the childhood subsamples for nearly every girl for whom we are able to estimate a PVP from the full residential mover sample (n=346,417). All estimates shown are statistically significant at the 1% level.

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

		6 months after HPV eligibility	12 months after HPV eligibility	18 months after HPV eligibility
		(1)	(2)	(3)
PVP quartile	Q2	0.036*** (0.009)	0.018** (0.007)	0.011* (0.006)
	Q3	0.042*** (0.010)	0.022*** (0.007)	0.015** (0.006)
	Q4	0.056*** (0.010)	0.040*** (0.007)	0.028*** (0.006)
Born in 2003		-0.436*** (0.013)	-0.463*** (0.012)	-0.432*** (0.012)
Born in 2003 x PVP quartile	Q2	-0.025 (0.016)	0.007 (0.016)	0.009 (0.016)
	Q3	0.007 (0.014)	0.041*** (0.014)	0.053*** (0.014)
	Q4	-0.003 (0.014)	0.027** (0.013)	0.048*** (0.012)

*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

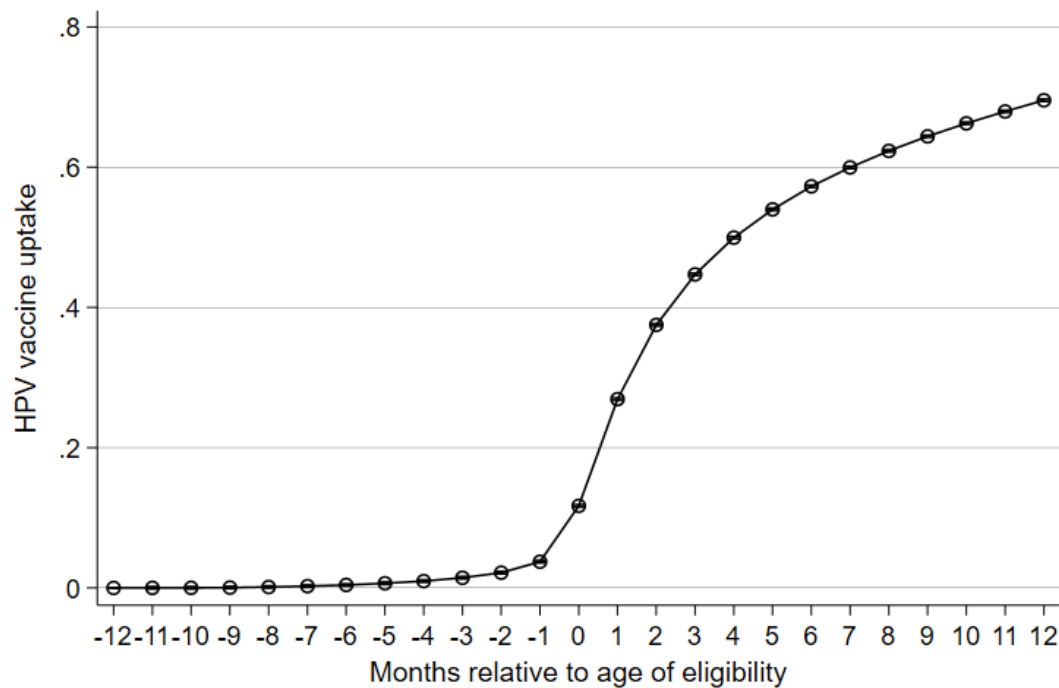
**Table 6** Spillover effects of provider vaccination propensity on HPV vaccine uptake of younger cousins

	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	1.253*** (0.166)	1.263*** (0.168)	1.260*** (0.168)	1.121*** (0.175)	1.113*** (0.176)	1.110*** (0.176)	1.131*** (0.174)	1.123*** (0.176)	1.120*** (0.176)
Older cousin's PVP		0.270** (0.103)	0.224** (0.103)		0.273** (0.112)	0.229** (0.112)		0.138 (0.108)	0.093 (0.109)
Own childhood vax compliance	x	x	x	x	x	x	x	x	x
Cousin's childhood vax compliance			x			x			x
Mean of outcome variable	0.541	0.541	0.541	0.671	0.671	0.671	0.733	0.733	0.733

*Notes:* Each column shows 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=107,701) 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). 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

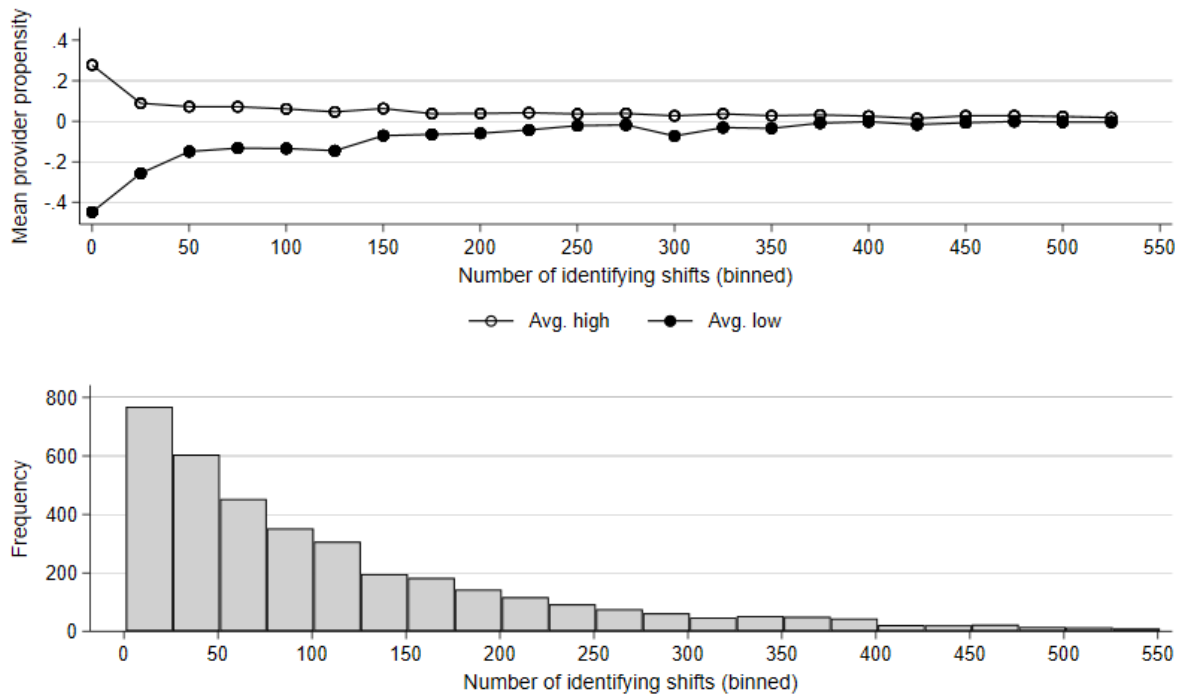
## APPENDIX FIGURES AND TABLES – FOR ONLINE PUBLICATION

**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 95% confidence intervals. The figure is based on vaccination data for 346,417 girls born 1997-2007.

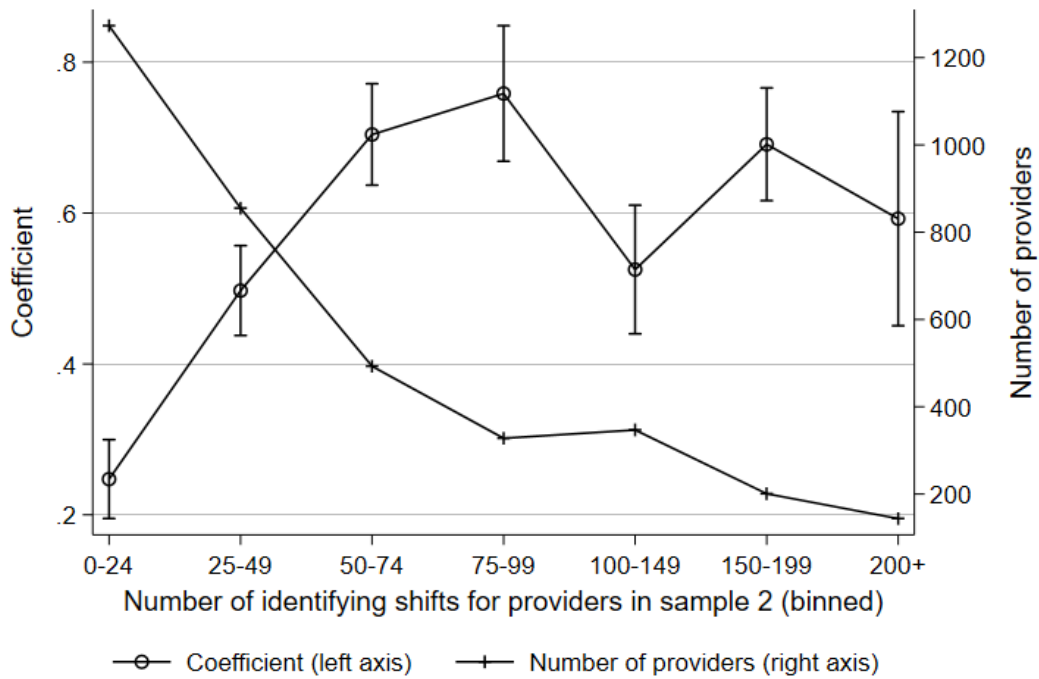
**Figure A2** Distribution of provider vaccination propensities by number of identifying transfers



*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 transfers (binned by ranges of 25). These are the original unadjusted estimates from the full residential mover analysis sample centered at the mean (in the provider sample). 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 550 transfers are omitted.

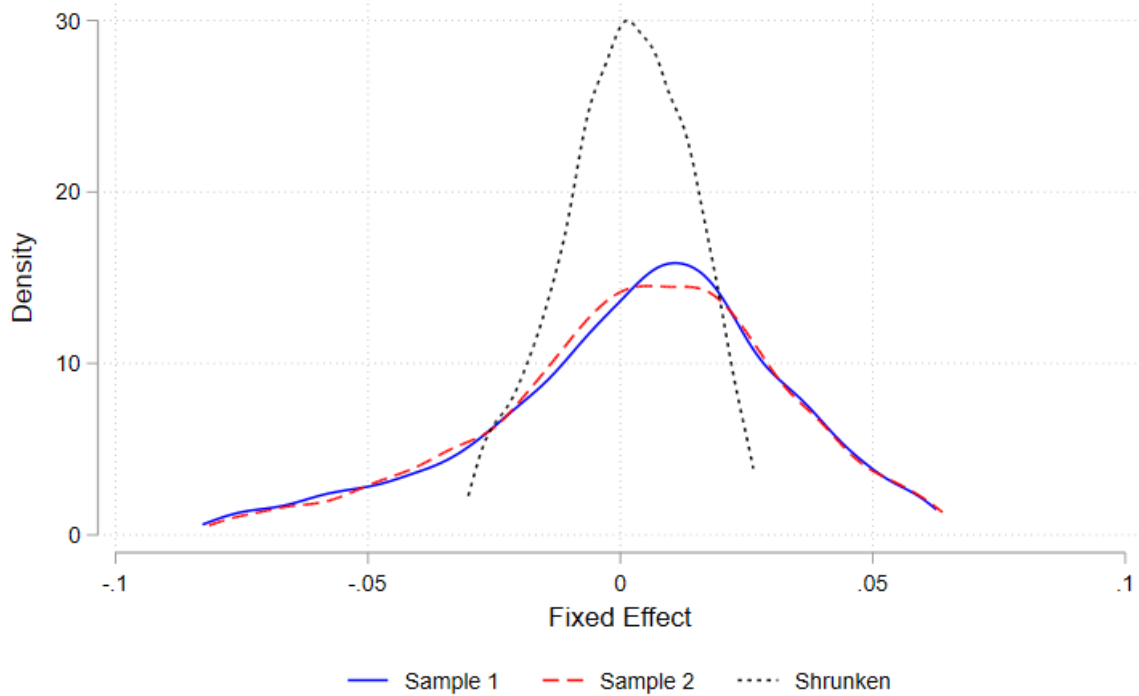


**Figure A3** Signal to signal-plus-noise ratio by number of identifying transfers



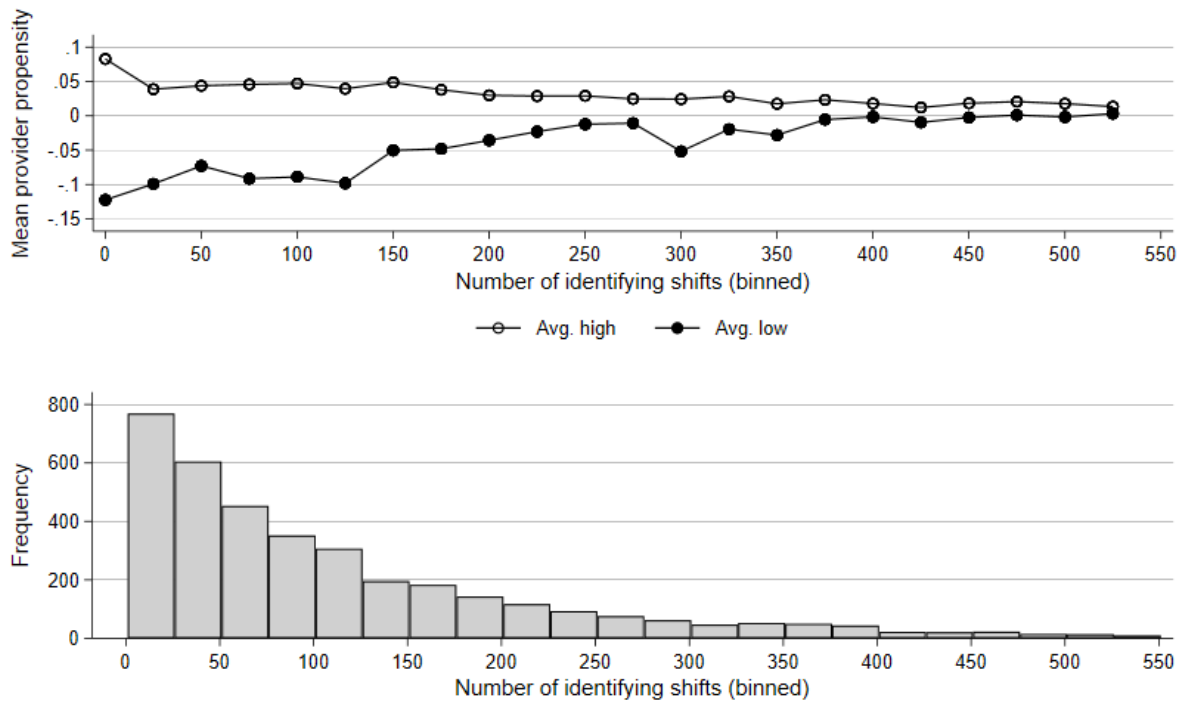
*Notes:* The circles (bars) depict the estimated slope coefficients (95% confidence intervals) from regressions of the centered provider fixed effects estimates from one random sample split (of mothers in the residential mover analysis sample) on the other, separately by the (binned) number of identifying 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 shrunk provider fixed effects estimates



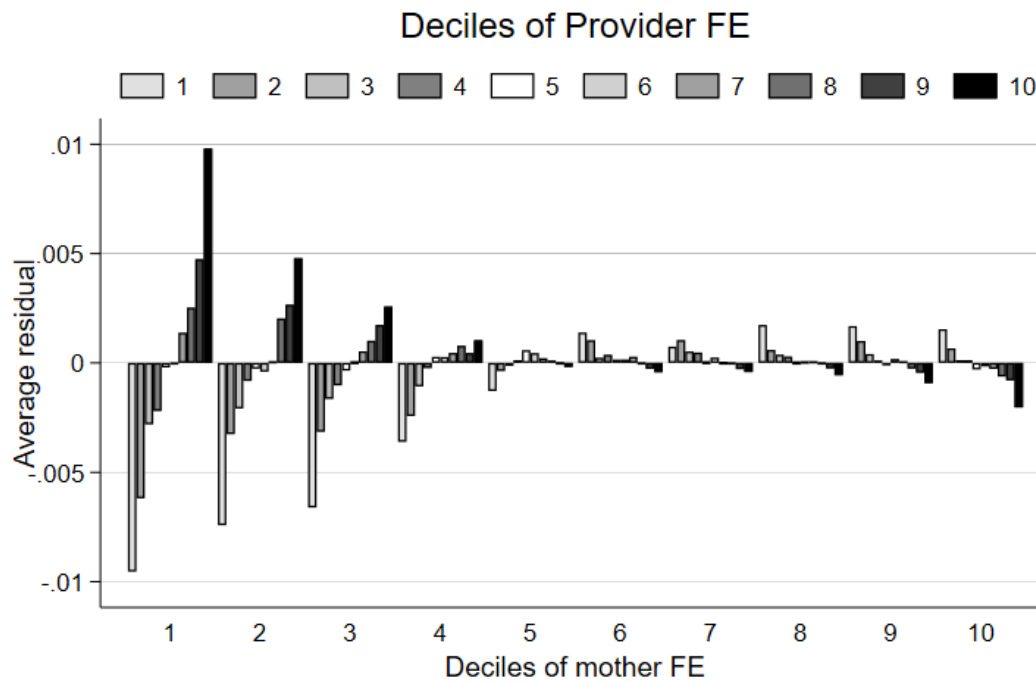
*Notes:* The two wider distributions depicted are for the unadjusted provider fixed effects estimates for the two random sample splits of mothers in the residential mover analysis sample. The narrower distribution is for the shrunk provider fixed effects estimates. The shrunk estimates are predicted provider fixed effects from a regression of (recentered) provider fixed effects estimated from one random sample split on those estimated from the other, 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+).

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



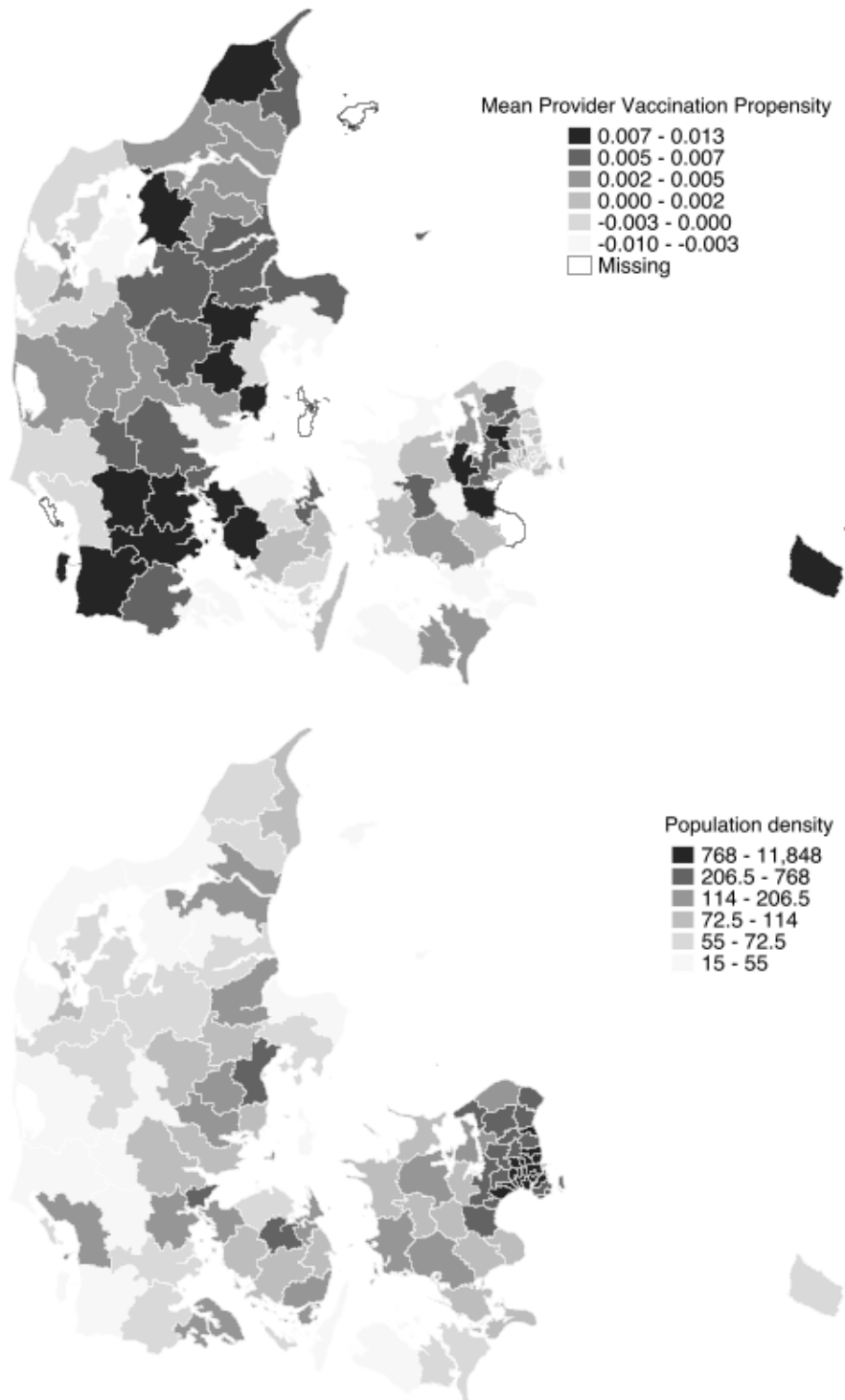
*Notes:* The top panel in this figure depicts the mean estimated shrunken provider propensity for the top (hollow circle) and bottom (solid circle) 5 providers by the total number of identifying transfers (binned by ranges of 25). The shrunken estimates are calculated as described in the notes to Figure A4. The bottom panel shows the distribution of the number of providers according to the total number of identifying transfers. Practices with more than 550 shifts are omitted from the figure.

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



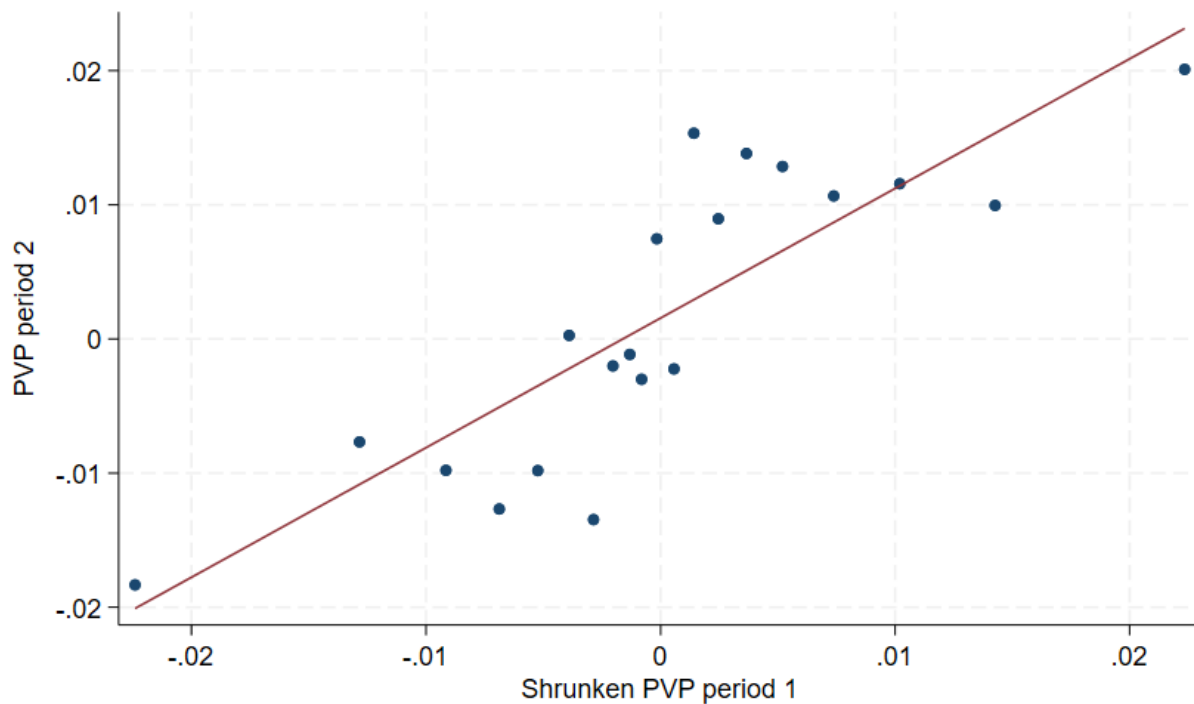
*Notes:* This figure illustrates the mean residuals from estimating equation (1) by deciles of mother and (shrunk) provider fixed effects.

**Figure A7** Geographic distribution of PVPs and population across Denmark



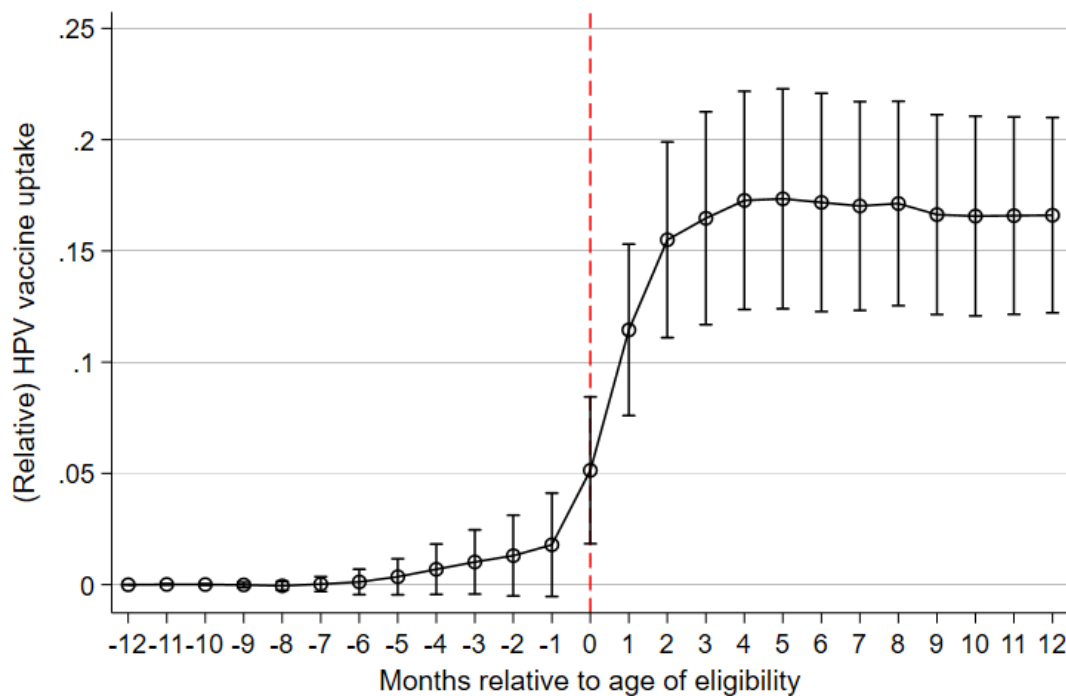
*Notes:* The top figure shows the geographic distribution of average (shrunk) provider vaccination propensity estimates across clinics for municipalities in Denmark. White areas are missing due to too few providers (5 or fewer), but they are included in the analysis. The bottom figure shows the (2015 Q1) population density (persons per km<sup>2</sup>) distribution.

**Figure A8** Correlation between provider vaccination propensities across time periods



*Notes:* This figure depicts a binned (20-dot) scatterplot that assesses the persistence in provider vaccination propensities (PVPs). PVPs are estimated separately for the first and second halves of providers' lifespans, using the childhood residential mover analysis sample. On the x-axis, the PVP from period 1 is shrunken using the split-sample binned IV approach described in the text. On the y-axis, the PVP from period 2 is the raw estimate. The solid line is a fitted linear regression with the slope coefficient (standard error) equal to .95 (0.092), where the standard error is robust to clustering by municipality.

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



*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 an indicator for having a provider in the top quartile of the shrunken PVP distribution at the time the girl turns 12. The regressions also include an indicator for having a top-quartile provider 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 girls from the 1997-2007 birth cohorts with providers in the top and bottom quartiles of the shrunken PVP distribution.

**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
	Missing	0.07	0.01	0.05	0.01	0.05	0.01	0.06	0.01
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 the birth of the child. Missing values of education are coded as “At most high school,” and missing values of earnings are set to 0. 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 (including 0s) 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. For other details see the notes to Table A1.

**Table A3** Correlations between provider vaccination propensities and practice characteristics, all practices

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of providers	2	0.004** (0.002)	0.005*** (0.002)	0.001 (0.002)	0.004** (0.002)	0.005** (0.002)	0.004* (0.002)	0.004** (0.002)	0.003** (0.002)
	3+	0.006*** (0.002)	0.007*** (0.002)	0.001 (0.002)	0.006*** (0.002)	0.007*** (0.002)	0.006*** (0.002)	0.006*** (0.002)	0.006*** (0.002)
Education practice		-0.002 (0.002)	-0.000 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
Caseload per provider	Q2	0.004 (0.003)	0.004 (0.003)	0.002 (0.003)	0.004 (0.003)	0.004 (0.003)	0.004 (0.003)	0.002 (0.003)	0.002 (0.003)
	Q3	0.003 (0.004)	0.003 (0.004)	0.001 (0.004)	0.003 (0.004)	0.003 (0.004)	0.003 (0.004)	0.001 (0.003)	0.001 (0.003)
	Q4	0.006 (0.004)	0.006 (0.004)	0.003 (0.004)	0.006 (0.003)	0.005 (0.004)	0.005 (0.003)	0.003 (0.003)	0.003 (0.003)
Average fraction female of providers		0.008*** (0.003)	0.008*** (0.003)	0.004 (0.002)	0.008*** (0.003)	0.009*** (0.003)	0.007*** (0.002)	0.005** (0.002)	0.005** (0.002)
Average age of providers	Q2	0.000 (0.002)	-0.000 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
	Q3	-0.003 (0.002)	-0.004* (0.002)	-0.003 (0.002)	-0.003 (0.002)	-0.002 (0.002)	-0.003 (0.002)	-0.003 (0.002)	-0.003 (0.002)
	Q4	0.000 (0.003)	-0.001 (0.003)	0.001 (0.003)	0.000 (0.003)	0.001 (0.003)	-0.000 (0.003)	0.000 (0.003)	0.001 (0.003)
Up-to-date status of own children			0.012** (0.006)						
Up-to-date status of own grandchildren			0.012 (0.010)						
Well-child visit compliance	Q2			0.020*** (0.003)					
	Q3			0.028***					

				(0.003)				
	Q4			0.037***				
				(0.004)				
Outreach practice					0.001			
					(0.003)			
Flu vaccine compliance	Q2					0.003		
						(0.003)		
	Q3					0.008**		
						(0.003)		
	Q4					0.015***		
						(0.004)		
Patients without avoidable hosp.	Q2						0.005*	
							(0.002)	
	Q3						0.006**	
							(0.003)	
	Q4						0.012***	
							(0.002)	
Practice style metric 1							0.020***	
							(0.004)	
Practice style metric 2								0.021***
								(0.005)
Observations	3,200	3,200	3,198	3,200	3,200	3,200	3,200	3,200
R-squared	0.085	0.087	0.142	0.085	0.093	0.089	0.117	0.122

*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 child residential mover analysis 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 A4** Correlations between provider vaccination propensity and provider characteristics, single-provider practices

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education practice		-0.001 (0.002)	0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)
Caseload per provider	Q2	0.005 (0.004)	0.005 (0.004)	0.002 (0.004)	0.005 (0.004)	0.004 (0.004)	0.004 (0.004)	0.003 (0.004)	0.002 (0.004)
	Q3	0.005 (0.005)	0.005 (0.005)	0.002 (0.005)	0.005 (0.005)	0.005 (0.005)	0.005 (0.005)	0.003 (0.004)	0.003 (0.004)
	Q4	0.008* (0.005)	0.008* (0.005)	0.005 (0.004)	0.008* (0.004)	0.007 (0.005)	0.008* (0.004)	0.005 (0.004)	0.005 (0.004)
Average fraction female of providers		0.009*** (0.002)	0.009*** (0.003)	0.005** (0.002)	0.009*** (0.003)	0.010*** (0.002)	0.009*** (0.002)	0.007*** (0.002)	0.007*** (0.002)
Average age of providers	Q2	0.002 (0.003)	0.001 (0.003)	0.002 (0.003)	0.002 (0.003)	0.002 (0.003)	0.002 (0.003)	0.003 (0.003)	0.003 (0.003)
	Q3	-0.002 (0.003)	-0.003 (0.003)	-0.002 (0.003)	-0.002 (0.003)	-0.001 (0.003)	-0.002 (0.003)	-0.002 (0.003)	-0.002 (0.003)
	Q4	0.002 (0.003)	0.000 (0.004)	0.003 (0.003)	0.002 (0.003)	0.003 (0.003)	0.001 (0.003)	0.002 (0.003)	0.003 (0.003)
Up-to-date status of own children			0.012 (0.008)						
Up-to-date status of own grandchildren			0.013 (0.013)						
Well-child visit compliance	Q2			0.020*** (0.003)					
	Q3			0.029*** (0.004)					
	Q4			0.040*** (0.005)					
Outreach practice					0.001				

					(0.003)			
Flu vaccine compliance	Q2				0.004			
					(0.004)			
	Q3				0.008*			
					(0.004)			
	Q4				0.016***			
					(0.004)			
Patients without avoidable hosp.	Q2				0.006**			
					(0.003)			
	Q3				0.008***			
					(0.003)			
	Q4				0.014***			
					(0.003)			
Practice style metric 1							0.022***	
							(0.005)	
Practice style metric 2								0.022***
								(0.006)
Observations	2,319	2,319	2,317	2,319	2,319	2,319	2,319	2,319
R-squared	0.098	0.100	0.155	0.098	0.106	0.103	0.130	0.135

*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 child residential mover analysis sample, and the estimation sample is limited to single-provider practices. For other details, see the notes to Table A3. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

**Table A5** Bias-corrected standard deviations and correlations, PVPs based on different samples

PVP estimation sample	Movers	Clinic closures	Movers: boys	Movers: late cohorts	Movers: qtrs. w/new reqs.
Movers	<b>0.021</b>				
Clinic closures	0.483	<b>0.038</b>			
Movers: boys	0.747	0.543	<b>0.016</b>		
Movers: late cohorts	0.630	0.675	0.342	<b>0.015</b>	
Movers: qtrs. w/new reqs.	0.657	0.643	0.352	0.353	<b>0.024</b>

*Notes:* This table reports 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 et al. (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 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	1.000 (0.118)	1.035 (0.163)	0.968 (0.123)	0.951 (0.119)	0.963 (0.147)	0.905 (0.119)	0.899 (0.117)	0.921 (0.136)	0.852 (0.115)
Instrument set	1	2	3	1	2	3	1	2	3
F-statistic instruments	451	191	78	451	191	78	451	191	78

*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 in the child residential mover analysis sample) 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 transfers per split. 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 transfers per split than the baseline approach (i.e., by ranges of 25 transfers up to a limiting bin of 300+). The sample is girls from birth cohorts 1997-2007 (n=346,417). All estimates shown are statistically significant at the 1% level.