[Appendix. *For Online Publication Only*] Supplement to "In Vaccines We Trust? The Effects of the CIA's Vaccine Ruse on Immunization in Pakistan"

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Contents

1 Theoretical Model

In this section, we present a simple model of Bayesian updating that provides a conceptual framework for our analysis.

Consider a setting where parents take a one-time decision about whether to vaccinate their children. There are two possible states of the world $\omega \in \{0, 1\}$. If $\omega = 1$ vaccines are safe and beneficial for children and health workers are trustworthy. If $\omega = 0$, the opposite is true. We assume that parents obtain a positive utility payoff if they vaccinate their children when $\omega = 1$ and the same negative payoff when $\omega = 0$. Hence, parents will vaccinate their children when the posterior probability of $\omega = 1$ is larger than $1/2$. Parents have a common prior, θ, that the state of the world is such that vaccines are safe and health workers are trustworthy. Parents update their prior based on new information.

Two pieces of information get revealed before parents take their vaccination decisions. The first one is the realization of a public signal, $\lambda \in \{0, 1\}$, which is observed by everyone. The signal is informative. In particular we assume: $Pr(\lambda = 1|\omega = 1) = Pr(\lambda = 0|\omega = 0) = \delta > 1/2$. We interpret the disclosure of the CIA vaccine ruse as a negative realization of this public signal (i.e., $\hat{\lambda} = 0$): it suggests that vaccines and health-workers are not trustworthy. Note that this is not incompatible with the state of the world being one where vaccines are good and with the public signal being informative. In a counterfactual world were the CIA vaccine ruse had not happened, the realization of the public signal could have indicated that vaccines were good: for instance a celebrity endorsing vaccines, or good news about vaccines leading to the eradication of a disease. In other words, we interpret the CIA vaccine ruse as an (ex-ante unlikely) negative realization of an informative public signal, in a context where the state of the world is one were vaccines are good.[1]

The second piece of information that gets disclosed is a message sent by the Taliban. We assume that the Taliban get a private signal, $x \in \{0, 1\}$, and decide what message m to send to parents. The Taliban's private signal is informative. We assume: $Pr(x = 1|\omega = 1) = Pr(x = 0|\omega = 0) = \delta_T > 1/2$. There are two types of Taliban: honest Taliban

[1]It is important to point out that the CIA vaccine ruse was quite an unlikely event. To the best of our knowledge, this was the only time when the CIA had used vaccination campaigns or other forms of health-related activities as a cover for espionage operations in the context of Pakistan. We conducted extensive searches in the CIA's Freedom of Information Act Electronic Reading Room as well as supplementary web searches and we could not identify any additional incident. Hence, despite the occurrence of the vaccine ruse, we believe the most accurate depiction of the state of the world is one were parents' decisions to vaccinate their children is largely beneficial for them. This is particularly true for the context of Pakistan, where diseases like poliomyelitis—which mainly affects children and can lead to paralysis—is still endemic. See for instance McNeil. 2019. "Polio Cases Surge in Pakistan and Afghanistan". https://nyti.ms/2XKX695 (last accessed 07/16/2019).

always truthfully report their private signal to parents, $m = x$. In contrast, 'dishonest' or 'ideological' Taliban always send a message reporting that vaccines are bad and health-workers not trustworthy. Hence, they always send message $m = 0$. Parent's perception of the probability that the Taliban is honest is denoted by q.

We assume that only a fraction e of the population receives the signal of the Taliban. This could capture the notion that the network of distribution of Taliban propaganda is more developed in areas where the Taliban had more support.

We interpret the sequence of events that we study in this paper as follows: nature chooses a state of the world $\omega = 1$ where vaccines are good for children. Then parents observe a negative realization of the public signal $\lambda = 0$ – i.e., the CIA vaccine ruse. Parents update their prior about the state of the world. The Taliban receive a private signal and report message m. A fraction e of parents observe the message sent by the Taliban. We interpret the anti-vaccine propaganda campaign that followed the disclosure of the vaccine ruse as the Taliban sending a message that vaccines are bad. The parents that receive the Taliban message further update their posterior about the state of the world. Parents decide whether to vaccinate or not their children.

Updating after the public signal

We conceptualize the CIA vaccine ruse as the realization of a negative signal $\hat{\lambda} = 0$. Hence, we focus on this case. Parents will decide to vaccinate their children if the ratio of posteriors of $\omega = 1$ relative to $\omega = 0$ is larger than 1. Parents that have only observed the public signal at the time of vaccination will decide to vaccinate as long as the following equation holds:

$$\frac{Pr(\omega = 1|\lambda = 0)}{Pr(\omega = 0|\lambda = 0)} = \frac{Pr(\lambda = 0|\omega = 1)Pr(\omega = 1)}{Pr(\lambda = 0|\omega = 0)Pr(\omega = 0)} = \frac{1-\delta}{\delta}\frac{\theta}{1-\theta} \geq 1 \tag{1}$$

The second ratio follows from applying Bayes rule and by the fact the terms $1/Pr(\lambda = 0)$ in the numerator and the denominator cancel out. Note that, by our assumption that the public signal is informative about the state of the world, the term $\frac{1-\delta}{\delta}$ takes value lower than one. This means that the disclosure of the CIA vaccine ruse has a negative effect on parents's posterior probability of the adequacy of vaccines.

Updating after the Taliban message

After the disclosure of the public signal, the Taliban receive a private signal and send message m. A fraction e of parents receive the message. Parents that do not receive the

Taliban message remain with posterior likelihood ratio as in (1). In contrast, parents that receive the Taliban's message further update their posterior likelihood ratio. We focus on the case where parents observe the Taliban message $m = 0$, which is consistent with the anti-vaccine propaganda that followed the disclosure of the vaccine ruse.

$$\frac{Pr(\omega = 1|\lambda = 0, m = 0)}{Pr(\omega = 0|\lambda = 0, m = 0)} = \frac{Pr(m = 0|\lambda = 0, \omega = 1)}{Pr(m = 0|\lambda = 0, \omega = 0)} \frac{Pr(\omega = 1|\lambda = 0)}{Pr(\omega = 0|\lambda = 0)} \geq 1 \tag{2}$$

The second ratio corresponds to the posterior likelihood ratio derived in (1). The first ratio is given by:

$$\begin{aligned} Pr(m = 0|\lambda = 0, \omega = 1) &= q(1 - \delta_T) + (1 - q) \\ Pr(m = 0|\lambda = 0, \omega = 0) &= q\delta_T + (1 - q) \end{aligned}$$

The first expression captures that, if the state of the world is $\omega = 1$, a Taliban message $m = 0$ could be generated by an honest Taliban that gets an incorrect signal $x = 0$, or from a dishonest Taliban. The second expression is derived in a similar way.

Plugging these expressions on the posterior likelihood we obtain:

$$\frac{Pr(\omega = 1|\lambda = 0, m = 0)}{Pr(\omega = 0|\lambda = 0, m = 0)} = \frac{1 - q\delta_T}{1 - q(1 - \delta_T)} \frac{1 - \delta}{\delta} \frac{\theta}{1 - \theta} \geq 1 \tag{3}$$

Note that, given our assumptions that $\delta_T > 1/2$, the first ratio takes value lower than 1. This suggests that the Taliban's message $m = 0$ makes parents update downward their posterior probability that vaccines are good. Since parents assign a positive probability to the possibility that the Taliban are honest, the Taliban's message contains information to form their posteriors. Hence, parents update their posterior accordingly.

A fraction e of parents will update their posterior according to expression (1), while fraction $1 - e$ will derive their posterior according to (3). The following expression captures the "average" posterior likelihood across parents in a given region. This expression is informative about how parents, on average, will change their behavior after receiving the new information.

$$E\Big(\frac{Pr(\omega{=}1|\lambda{=}0, \{m{=}0 \text{ or } m{=}\varnothing\}}{Pr(\omega{=}0|\lambda{=}0, \{m{=}0 \text{ or } m{=}\varnothing\}}\Big) = \Big[(1\text{-}e) + e\frac{1 - q\delta_T}{1 - q(1\text{-}\delta_T)}\Big]\frac{1 - \delta}{\delta}\frac{\theta}{1 - \theta} \tag{4}$$

Comparative Statics

Expression (4) comprises the multiplication of three different ratios. The last one represents the ratios of prior probabilities. The first two ratios correspond of the effects of the updating that takes place upon receiving the Taliban's message $m = 0$ and the public signal $\lambda = 0$, respectively. Both ratios take value lower than one, indicating that the new information lowers parents' assessed probability of vaccines and health-workers being trustworthy. Hence, we expect that parents lower their willingness to vaccinate their children. This is not surprising given that we focus on the realization of signals or messages that indicate that vaccines are not good. However, it is likely that the magnitude of the downward updating is heterogenous across parents and regions. Next, we describe how the posterior likelihood depends on a number of parameters and the potential sources of heterogeneity in these parameters.

1. **The more precise signals, the larger downward updating.** The larger δ and δ_T, the more informative are the public and the Taliban signals about the true state of the world. Hence, the larger the downward updating in the presence of negative signals or messages.

2. **The larger the perceived probability that the Taliban are honest, the larger downward updating.** The higher the perceived probability that the Taliban are honest, q, the larger is the information content of the Taliban's negative message m. Hence, parents will update downward their priors to a greater extent.

3. **The larger the fraction of the population that receives the Taliban's message, the larger downward updating.** The higher e the larger the fraction of parents that will update downward their posterior likelihood ratio according to the messages sent by the Taliban.

There are two parameters that are likely to be heterogenous across parents and regions. First, we assume that parents in districts with higher levels of support for Islamist parties have a higher probability to receive the message sent by the Taliban, i.e. e_i, where the i subindex captures differences across districts. This captures the notion that the network of distribution of information by the Taliban is more developed in areas were the Taliban had more support. For instance, areas with high support for Islamist parties tend to have a larger density of mosques with radicalized clerics that can diffuse some of the messages of the Taliban (Roul, 2014). Second, parents with a higher ideological affinity to the Taliban may be more likely to trust the messages sent by the Taliban. This could be driven by the

presence of confirmation bias (Lord et al. 1979, Mullainathan and Shleifer, 2005) or by the possibility that parents judge the source of information as being of higher quality when it conforms with their priors (Gentzkow and Shapiro 2006). We introduce these notions in a reduced form way, by assuming that parents with a stronger ideological affinity with the Taliban assign a higher probability to the possibility that the Taliban are honest, i.e. q_i.

This simple framework generates a number of predictions that guide the interpretation of the results we find in our *Difference-in-Differences* empirical framework. The disclosure of the CIA vaccine ruse and the subsequent anti-vaccine propaganda campaign of the Taliban make parents update downwards their prior that the state of the world is one where vaccines are good. The downward updating is greater in regions where a larger fraction of parents have an ideological affinity with the Taliban. This is driven by those regions having (i) a larger fraction of parents that receive the anti-vaccine propaganda messages or by (ii) a larger fraction of parents assigning greater credibility to the messages of the Taliban.

2 Data Appendix

2.1 Data Sources

Pakistan Social and Living Standards Measurement (PSLM)

The PSLM Project is designed to provide social and economic indicators at the district level. It is implemented by the Pakistan Bureau of Statistics. We use the PSLM survey waves implemented in 2010/11 and 2012/13 for our main analysis. For robustness, we further complement the analysis with data from the survey wave implemented in 2008/09. The 2008/09 was fielded between August 2008 and June 2009. The 2010/11 was fielded between June 2010 and June 2011. The 2012/13 was fielded between October 2012 and June 2013.

We construct the following outcomes of interest from survey responses in the Vaccination module of the PSLM survey. Firstly, we construct indicators for the receipt of different doses of vaccines. In particular, we consider and construct indicators for polio, DPT, as well as, measles vaccines. Enumerators for the PSLM surveys could choose among the following options in order to record a child's vaccination status: 1) yes (based on vaccination card); 2) yes (based on recall); 3) no; 4) yes (polio campaign). The last option is selected when households report having received the vaccine during regular polio vaccination campaigns. This option is also based on recall. Vaccination status measures based on recall have been shown to be prone to suffer from severe measurement error (Research and Development Solutions (2012); Sheikh et al (2011)). In order to minimize the concern of misreporting, we focus on immunization status that can be verified in the vaccination card. In particular, our outcome variable take value one if the child received a given vaccine as shown in his/her vaccination card, and 0 otherwise. Hence, the immunization rates reported in this study should be considered as a lower bound of immunization rates in this context.

We also construct indicators for full immunization. The PSLM survey only records the first three doses of polio and DPT, as well as the first dose of measles.[2] Hence, we consider children fully immunized against polio or DPT if the three doses reported in the survey have been provided and registered in the vaccination card.[4] Similarly, the survey only recorded information regarding the first dose of the measles vaccine. Hence, we cannot assess full

[2]Three doses of polio and DPT and one dose of measles, were the World Health Organization (WHO, henceforth) recommended dosages prior to 2009. In that year, the WHO updated their guidelines by recommending to administer an additional dose of the polio vaccine at birth, and an additional dose of the measles vaccine at 15 months.[3] However, the PSLM survey did not update their questionnaire according to the new WHO guidelines. That is the reason why only three doses of polio and one dose of measles are recorded in the data.

[4]See Appendix Table 1 for the official vaccination calendar.

immunization for measles. We also combine information on the three vaccines to create a measure of *"complete immunization"*. We consider a child to be completely immunized if all doses of polio, DPT vaccine, and measles, were recorded in the survey.

Secondly, the vaccination & diarrhea module of the PSLM survey also contains some information on general measures of health seeking behavior. The available information allows us to construct the following measures:

- *Dummy for Illness in Last 2 Weeks:* Survey respondents are asked in the survey - for each child separately - whether a child was ill or injured in the two weeks prior to the survey. We use this information to construct a dummy variable that has value 1 if the respondent states that a given child was ill or injured in the two weeks prior to the survey, 0 otherwise.

- *Dummy for Consulted Anyone:* For each child which was reported to have been ill or injured in the two weeks prior to the survey, the survey respondent was then asked whether anyone was consulted regarding the reported illness or injury. We use this information to construct a dummy variable that assumes value 1 if the respondent states that someone had been consulted regarding the illness or injury, 0 otherwise.

- *Dummy for Consulted Formal Medical Sector:* If a respondent reported that a child had been ill or injured in the two weeks prior to the survey and also stated that someone had been consulted regarding the illness or injury, the survey enumerators also elicited which part of the medical sector in Pakistan had been consulted. This allows us to construct a dummy variable that assumes value 1 if the respondent states that the formal medical sector in Pakistan was consulted regarding the illness or injury. In particular, we consider the answer choices *"Private Dispensary/Hospital"*, *"Government Hospital"*, *"Rural Health Clinic/Basic Health Unit"* and *"Lady Health Worker"* as representing the formal medical sector. The categories that correspond to the non-formal medical sector are: *"spiritualist"*, *"homeopath"*, *"chemist"*, *"hakeem"* and *"other"*.

Electoral Data

Provinces elect provincial assemblies as their legislature. The members of these provincial assemblies are directly elected during general elections and serve 5-year terms.

We obtained constituency-level data for the general election to the provincial assembly of 2008. We obtained the names of all the contesting candidates, their respective party

affiliations, and the number of votes obtained by each candidate. We use the official delimitation of 2002 and the amendments of 2008 published in the *Gazette of Pakistan* to locate constituencies within the districts of Pakistan (The Gazette of Pakistan (2002)).

Since electoral constituencies are smaller than districts, we construct a district-level measure of support for different parties. In particular, we calculate the population-weighted average share of votes across all constituencies of a district. The weights correspond to the share of the population living in the respective constituency relative to the overall district population. In the absence of population data, we use number of total votes as a proxy of population numbers. Hence, our main measure of Islamist sentiments is the population-weighted share of votes obtained by the alliance of Islamist parties MMA, across all constituencies within a given district in the 2008 provincial legislative election. The spatial distribution of this measure of support for Islamist political parties across the districts of Pakistan is presented in Figure 1.

Data on 2010 Floods

Pakistan suffered from floods in 2010, which had a severe negative impact on the population and the distribution of health services in particular.[5]

In order to verify robustness of our results to potentially confounding effects, we construct an indicator variable that equals 1 if a district was regarded as severely flood-affected by the FAO in a detailed livelihood assessment of 2012, 0 otherwise. (Food and Agriculture Organization of the United Nations (2012)). There are a total of 28 districts in our sample that were classified as severely flood-affected.

Demographic Health Surveys

We rely on data from two waves of the Demographic Health Surveys (DHS) in Pakistan to obtain further measures of immunization and health-seeking behavior. In particular, we rely on the 2006/07 DHS survey to obtain measures prior to the disclosure of the vaccine ruse, as well as the 2012/13 DHS survey to study outcomes after the vaccine ruse had been disclosed.

In order to make the DHS sample as comparable as possible to the PSLM sample from which we derive our main estimates, we impose the same sample restrictions. In particular, we focus on children that were at most 24 months of age at the time of interview. Thus the sample consists of children born in the years 2004 to 2007 and 2010 to July 2012. Paralleling

[5]Statistics obtained from Pakistan Disaster Knowledge Network. *http://www.saarc-sadkn.org/countries/pakistan/disaster_profile.aspx* (accessed 14.06.2015)

the restrictions applied to the PSLM sample, we exclude partially treated children. In particular, for both the first dose of Polio, DPT and HBV, we exclude children born between March and June 2011. In the case of Measles, we exclude children born between July 2010 and June 2011. This yields a final sample size of 6,023 children.

We construct indicators for the receipt of different doses of vaccines analog to the procedure applied in the PSLM survey. In particular, we consider and construct indicators for Polio, DPT, HBV, as well as, measles vaccines. Enumerators for the DHS surveys could choose among the following options in order to record a child's vaccination status: 1) yes (vaccination date marked on the vaccination card); 2) yes (vaccination marked on the vaccination card); 3) yes (based on mother's recall); 4) no. Analog to the procedure applied in the PSLM data, we focus only on the first two choices as a measure of immunization. Hence, in the outcome variable "received one shot of *vaccine type*", we code answers based on recall as 0.

The 2012/13 wave of the DHS also contains detailed information on the migration status of survey respondents. In particular, for each household member, the survey elicits whether the individual was born in the current district of residence. If the respondent denies this, he is subsequently asked about the district of origin, that is the district where he lived prior to moving to the current district of residence. Moreover, respondents are also asked about the year in which this movement took place. We use this migration data in a series of robustness checks. In particular, we use the available information to calculate approximate in- and out-migration rates in the period after the vaccine ruse disclosure for each district in the sample.

To this end, we classify households as migrant households if at least one member migrated to the current district of residence in the years 2011 or 2012. To calculate the in-migration rate, we count the number of migrant households within a given district and divide this number by the total number of households included in the 2012/13 DHS survey that currently reside in the district of interest. To calculate the out-migration rate, for each district, we count the number of migrant households that are currently observed in a different location and report that at least one family member migrated to this place from the district of interest in the years 2011 or 2012. We then divide this number by the total number of households included in the 2012/13 DHS survey that still reside in the district of interest.

The DHS survey also provides information on morbidity outcomes and individuals' health seeking behavior. In particular, the survey inquires whether children suffered from diarrhea or fever and cough in the two-week period prior to the interview. If so, follow-up information

on health-seeking behavior and the course of medical treatment is elicited. This information enables us to construct the following two indicators: First, we generate a dummy which equals one if any treatment was sought out to treat the respective illness, zero otherwise. Second, we generate an indicator which equals one if a child received formal, medical treatment to treat the respective illness, zero otherwise. In particular, we consider a child to have received formal medical treatment either if the parents declared that the child received medical treatment or if they visited a public, medical facility in order to receive treatment for their child.

South Asia Barometer Data

We use two rounds of the South Asia Barometer (SAB) data to examine trust outcomes before and after the disclosure of the vaccine ruse in Pakistan. The South Asia Barometer data was provided by the Asian Barometer office, located within the Department of Political Science at the National Taiwan University.

In particular, we rely on a first wave of the SAB that was fielded in 2005 in order to obtain trust measures prior to the vaccine ruse disclosure and a second wave of the SAB that was conducted in 2013 to obtain trust measures after the vaccine ruse disclosure. Both datasets are geo-referenced to the province level within Pakistan. Overall, the SAB data provides us with 3,252 observations in the pre- and post-treatment period for which we observe complete trust measures.

Individual can express 4 different levels of trust towards a given institution in the SAB survey. In particular, the available answer choices are: 1) A great deal of trust; 2) some trust; 3) not very much trust; 4) no trust at all. We construct indicators for trust towards a given institution that equal 1 if individuals express that they have either *"A great deal of trust"* or *"some trust"* towards a given institution, 0 otherwise.

Expanded Program on Immunization Data

The Expanded Program on Immunization in Pakistan was established in 1978 and aims to vaccinate children aged 0 to 11 months against nine target diseases, one of which is poliomyelitis. To this end, provincial EPI cells conduct regular immunization activities which take the form of vaccination campaigns. During these campaigns, teams of vaccinators distribute oral polio vaccine to eligible children in a specific target area. While these activities are implemented by the provincial EPI cells, the role of federal cell is restricted to the provision of policy and technical guidelines, coordination for international assistance, surveillance and monitoring.

We obtained administrative data on the polio immunization activities carried out in the period between 2008 and 2013 from the EPI's internal monitoring and surveillance system. This enables us to construct the following measures to control for the supply of polio immunization activities in the districts of the 4 main provinces of Pakistan across the study period of interest: First, we construct measures that record the number of monthly immunization campaigns carried out in a child's district of residence during the first 3 and 12 months of her life, respectively. Moreover, the administrative data also contains the number of children that were targeted during a monthly immunization drive in a given district. We combine this information with district-level population estimates in 2011 from the Pakistan Bureau of Statistics to record the per-capita number of targeted children during the first 3 and 12 months of a child's life, respectively.

ACLED Data

We use the data from the Armed Conflict Location & Event Data Project (ACLED) to account for potential impacts of conflict. This dataset collects the dates, actors, types of violence, locations, and fatalities of all reported political violence and protest events in Pakistan starting from January 2010. In particular, the dataset records information on the following types of conflict events: a) battles, in which the government regains territory b) battles, from which no change of territory resulted c) battles, in which a non-state actor overtakes territory d) events where a headquarter or a base were established e) non-violent transfers of territory f) remote violence g) riots/protests h) strategic development i) violence against civilians.

Given our focus on conflict and violence, we focus on all events except for riots and protests. In particular, we construct a time-varying control which counts the number of conflict events that occurred in a child's district of residence in the first twelve month of her life. In addition to this time-varying control, we also generate a pre-determined measure of conflict and violence by constructing a measure that records the total number of conflict events in the year 2010. In a series of robustness checks, this pre-determined measure is then interacted with cohort fixed effects.

The fact, that the ACLED dataset also provides information on the actors involved in a particular conflict event, allows us to also construct measures of conflict that are directly linked to Taliban activity. In particular, we construct district-specific measures of the number of conflict events associated with the Taliban in the year 2010 as well as in the time period 2010 to 2013.

Data on Cases of Poliomyelitis and other Diseases

We obtained district-level data on polio incidences for the year 2014 from the website http://www.endpolio.com.pk (last accessed 07/18/2019), which is published by the Expanded Program on Immunization of Pakistan. We then complemented this dataset with information on polio cases in the years 2009, 2010 and 2011 by digitizing and geo-referencing polio incidences using maps provided in the annual reports of the Global Polio Eradication Initiative.

We conducted an extensive data search for the other diseases associated with the vaccines that we study. In particular, we searched for district-level information on incidences of measles, diphtheria and tetanus. We were able to find more information for some of these diseases in the World Health Organization (WHO) publication series "Weekly Epidemiological bulletin Disease early warning system and response in Pakistan". However, the data available has a number of limitations. First, there is no information on tetanus. Second, the information on measles and diphtheria is only available for 2010, 2013, and 2014. Furthermore, the 2010 data—our only pre-treatment period—contains information only for the province of Khyber Pakhtunkha (KP). Third, diphtheria is a rare disease: only two districts in Pakistan reported cases in 2013 or 2014, and none of them was in KP. In addition to our evidence on polio cases, the only other disease for which we can conduct a meaningful regression analysis is measles, albeit for the subset of districts in KP. The results are broadly consistent with the evidence on polio cases. The number of cases of measles experiences large and significant increases in the period after the disclosure of the vaccine ruse. However, since the estimating variation is limited—only 10 out of 25 districts experience any cases of measles in the pre-treatment period, and only 3 districts in the post-treatment period—we do not report these results. They are available upon request.

Data on Attitudes & Media Consumption

We obtained access to individual-level survey data on media consumption, trust in different media sources and attitudes towards the Pakistani Taliban collected in 2013 by Fair, Kaltenthaler and Miller (2015). Please find the full citation for this dataset at the end of this subsection.

This data allows us to compute district-level measures of the share of respondents who oppose the Pakistani military attacking the Pakistani Taliban (TTP) as well as the share of respondents who have heard of and oppose US drone strikes in Pakistan's FATA region. Detailed information about respondents' media consumption and trust in different sources

of information allow us to distinguish between districts in which respondents are more or less likely to obtain and trust information from mainstream news sources.

In particular, we are constructing the following district-level measures for our analysis:

- *Share of citizens who do not rely on mainstream media.* We compute this share as the share of respondents within a district that indicate not to obtain information about events within Pakistan from Pakistani public and private or international TV channels as well as Pakistani and international, Urdu- and English-language newspapers. These individuals indicate that they get their news from informal sources, such as religious leaders, traditional gatherings, or family members.

- *Share of citizens whose most trusted source of information is not a mainstream media source.* We compute this share as the share of respondents within a district that do not declare to consider any of the following information sources to be their most trust source of information for events that occur within Pakistan: Pakistani public and private TV channels, international TV channels, Pakistani and international, Urdu- and English-language newspapers. These individuals indicate that their most trusted source of information correspond to an informal source, such as religious leaders, traditional gatherings, or family members.

- *Share of citizens who support the Pakistani Taliban and whose most trusted source of information are gatherings with religious leaders.* We compute this share as the share of respondents within a district which oppose the proposal to put more emphasis on attacks against the Pakistani Taliban (TTP) and who simultaneously consider gatherings with religious leaders to be their most trusted source of information on events that occur within Pakistan. Please note that the second condition is quite extreme, but our results are robust to relaxing this condition to the set of individuals who indicate that they get information on events that occur within Pakistan from gatherings with religious leaders (but do not necessarily indicate that these meetings are their most trusted source of information).

- *Share of citizens who had heard of the US drone strikes.* We compute this share as the share of respondents within a district who indicate to have heard of the US drone strikes in Pakistan's FATA region.

Full citation:

Survey project name: Pakistani Media and Muslim Communities: Identifying and Supporting Critical Thinking. The survey was conducted in 2013 by C. Christine Fair, Karl Kaltenthaler, and William Miller and was supported by a grant from the United States Department of State.

Newspaper Articles Dataset

We construct a dataset of newspaper articles related to the vaccine ruse. In particular, through the Nexis UNI database, we have obtained access to the universe of articles published by a set of Pakistani English-language newspapers between Jan 1, 2009 and Dec 31, 2013. In particular, the newspapers are: Daily Regional Times, Daily Times, Dawn, Pakistan Today, The Express Tribune, The Frontier Star and The Nation. We searched this newspaper article archive for the following keywords:

- vaccination & ruse, vaccination & BinLaden, vaccination & CIA, vaccination & Abbottabad, vaccination & spy, vaccination & espionage, vaccination & polio & sterilize, vaccination & impotent, vaccination & sterilize & Muslim, vaccination & infertility, vaccination & conspiracy, vaccination & DNAsamples, vaccination & unIslamic
- polioeradication & sterilize, polio & unIslamic
- American & undercover, American & espionage
- ShakilAfridi & vaccination, ShakilAfridi & spy, ShakilAfridi & CIA
- Taliban & vaccination, Taliban & polio, Taliban & polio & spy, Taliban & vaccination & spy, Taliban ban & polio

This identifies a total of 880 newspaper articles.

Nighttime Luminosity Dataset

We have collected data on nightlights from the fourth version of the National Oceanic and Atmospheric Administration's (NOAA) DMSP-OLS Nighttime Lights Time Series data. In particular, we rely on images derived from satellite F18 covering the years 2010-2013. We generate the district-level measure of economic activity by averaging nighttime luminosity over all cells located within the boundaries of a particular district in a particular time period. We then assign a luminosity measure for the year in which a child was born and for the year in which the household in which the child lives was interviewed.

2.2 Construction of the Dataset

We combine datasets from multiple sources to conduct our analysis. The different datasets are matched by district and time period (month and year). The matching is performed by current district of residence as well as month and year of child birth.

Over the course of our sample period, Pakistan experienced a mild process of district splitting. In particular, the number of districts in our study provinces increased from 110 to 115 between 2008 and 2012. We refer to the former set of districts as the *parent* districts and to the later set of districts are labeled *current* districts. Given the lower level of aggregation of our electoral data, we are able to calculate our measure of support for Islamist political parties at the level of current districts. Moreover, all regressions use district fixed effects at the current district level. However, in our analysis, we cluster standard errors at the level of parent districts to allow for potentially correlated errors across current districts that originated from the same parent district. Since the two district measures are almost equivalent and to minimize confusion, we describe these standard errors as "clustered at the district level" in the text and table notes.

3 Additional Results and Robustness Checks

3.1 Changes in Household Composition

One possible confounder to our estimates could emerge if the characteristics of children or households changes systematically over time across districts with different levels of Islamist support. This may generate non-parallel trends even in the absence of the disclosure of the vaccine ruse. To empirically examine this, we have conducted a number of tests.

First, we examine whether a number of household characteristics systematically differ across exposed and non-exposed cohorts, and across regions with different support for Islamist groups. In particular, for each child in our baseline sample, we examine a number of socio-demographic characteristics of his/her household. For each of these characteristics, we implement an analysis analogous to Figure 3 (comparison of outcomes by exposed/non-exposed cohorts, across regions with high and low support fo Islamist parties) and Figure 4 (cohort-specific treatment effects) in the paper. The results are presented in Appendix Figure 8. As we can see, there do not seem to be systematic differences between exposed and non-exposed cohorts in important child and household characteristics such as the child's gender, the mother's education and age, the number of rooms in the household, share of rural

households, and household head's literacy. There could be some small effect for number of household members. However, this is only one out of seven outcomes, so this pattern could have been generated by chance. We have verified that all our results are robust to adding this variable as a control. Online Appendix Table 4 reports the regression counterpart to these figures and shows similar results.

3.2 Treatment Effects by Monthly Cohort (Including Confidence Intervals)

In Appendix Figure 4, we replicate Figure 4 which provides estimates of the flexible DID specification described in equation (3). Appendix Figure 4 presents 90% confidence intervals for the estimated cohort-specific treatment effects.

3.3 Flexible Controls for Levels and Changes in the Dependent Variable Pre-Treatment

Next, we investigate the robustness of our estimates to flexibly controlling for pre-treatment levels and changes in the dependent variable. The results are presented in Appendix Table 5. These results further deepen the robustness check presented in column 2 of Table 4 in the main text. Column 1 presents our baseline estimates for comparison. In column 2 we control for the mean of the dependent variable in pre-treatment period (i.e., for non-exposed cohorts) for each district interacted with a full set of monthly cohort fixed effects. These controls address the possibility that mean reversion may affect our estimates, since districts with different levels of vaccination at the beginning of the sample are allowed to have a differential trend in the evolution of vaccination rates. The results are highly robust to these controls. In column 3, we conduct an even more demanding exercise. Instead of using the mean of the pre-treatment dependent variable, we create dummies for the quintiles of this variable and interact them with a full set of monthly cohort fixed effects. This means that our effects are only identified out of changes of districts within the same quintile of the initial vaccination rate. The results are generally robust, although the point estimate is lower for polio and higher for measles (in magnitude). Columns 4 and 5 conduct analogous robustness checks to those in columns 2 and 3, but when the variable of interests are average monthly changes in the mean of the dependent variable, instead of the average

level. These estimates mitigate the concern that the results are driven by differential effects across districts that have a continuous decline or increase in vaccination rates. Overall, the results show a considerable robustness to these flexible controls for differences in pre-existing levels and changes in vaccination rates (only one out of 32 estimates—that in column 3, panel A—experiences a notable change in the point estimate). The results in Panel D, when we consider all vaccines together, exhibit a remarkable robustness to these alternative controls.

3.4 Additional Robustness Checks for Income and Geography

In Appendix Table 13 we present a number of additional flexible controls for income and geography. Column 1 shows the baseline results for comparison. In column 2 we include controls for household income of each child's household. In particular, we aggregate the information on the annualized income of all the household members that reside with the child. Column 2 adds as controls the deciles of this household income measure. In columns 3 and 4, we add time varying controls on local economic conditions using nighttime luminosity measures derived from satelite information. We generate the district-level measure of economic activity by averaging nighttime luminosity over all cells located within the boundaries of a particular district in a particular time period. Column 3 shows the results when our luminosity measure is defined over the year in which a child was born and column 4 uses luminosity for the year of interview of the child. Our results are robust to controlling for these measures.

In column 5 we control for the monsoon floods that took place in 2010 and that severely affected some regions (Fair et al. (2017); Masera and Yousaf (2018)). Our results are robust to the inclusion of an indicator for the affected districts interacted with cohort fixed effects.[6] Column 6 drops the district of Abbottabad, where the operations to capture Bin Laden took place. The results are robust, suggesting that the evolution of vaccination rates in this district is not driving the results.

3.5 Selective Migration

Another potential concern is that the treatment may have induced differential migration across districts. If parents that are complying with (or intending to comply with) the vaccination schedule out-migrate in greater proportions from districts with high support for

[6]See section 2 in the Appendix for details on the construction of the flood affectedness measure.

Islamist groups, our results may be downward biased—i.e., biased towards finding a negative effect. Unfortunately, the PSLM data do not contain information on families' migration history or on parent's place of birth. Hence, in our baseline specification we assign children to the districts they are residing at the time of interview.

We conduct a number of tests to check whether selective migration could confound our estimates. First, we verified that the composition of households did not change differentially across districts with different levels of support for Islamist parties. This is shown in Appendix Table 4.

Second, we conduct additional analyses using information from the Demographic Health Survey (DHS, henceforth). In the 2012 wave, the survey contains information on the migration history of households. We use these data to construct district-specific rates of in-migration and out-migration.[7] The average in-migration rate is 2.5%, the average out migration rate is 3.9%.[8] Given that the fraction of migrants is low, it is unlikely that selective migration could have large effects on our estimates.

In Panel A of Appendix Table 14, we control for the district-specific in- and out-migration rates interacted with a full set of cohort fixed effects. This addresses the concern that districts with different propensities to experience migration may have underlying different trends. The results are very similar to the baseline estimates.

In Panel B, we conduct an exercise to obtain a lower bound on the magnitude of our estimates assuming the most unfavorable scenario of potential selective migration. For each district, we compute the net out-migration rate.[9] We assume that districts with positive out-migration estimates have fewer observations in the post period, relative to a counterfactual scenario where the treatment—disclosure of the vaccine ruse—did not happen. Hence, we add "constructed" observations to those districts equal to the corresponding share of net out-migration.[10] In particular, the "constructed observations" are assigned to the post-treatment cohorts. In order to construct the most unfavorable scenario, we impute successful vaccination outcomes in districts where the level of support for Islamist groups exceeds the median in the sample, whereas we impute unsuccessful vaccination outcomes in districts, where the level of support for Islamist groups lies below the median in the sample. For districts where we estimate negative net out-migration rates, we proceed to drop observations. In particular, we drop observations with a successful vaccination outcome if the level

[7] See section 2 for further details on the construction of these measures.

[8] The maximum rates of in- and out- migration are 11% and 22%, respectively.

[9] The net out-migration rate is equal to the out-migration rate minus the in-migration rate.

[10] We assume that these observations have characteristics equal to the average in that district among the post-treatment cohorts.

of support for Islamist groups is below the median level in the sample, whereas we drop observations with an unsuccessful vaccination outcomes in districts where the level of support for Islamist groups exceeds the median in the sample.[11] Despite the extreme assumptions on the nature of selective migration, our estimates remain negative, large in magnitude and statistically significant, with the only exception of the measles vaccine, which is no longer statistically significant. These estimates constitute a lower bound on the negative effect of the vaccine ruse on vaccination rates. The fact that this lower bound is still large in magnitude is reassuring. In other words, it is unlikely that selective migration could entirely account for our estimates.

In Panel C, we use the fact that for the DHS sample we do have data on the district of origin of households observed in the post period. We estimate our effects when assigning households observed in the post period to their district of origin, instead of to their district of residence. The results are very similar to the baseline effects when using the DHS sample, which are presented in Appendix Table 15.

3.6 Alternative Vaccination Data: the Demographic Health Survey (DHS)

We verify the validity of our estimates by conducting a similar exercise using a different dataset: the Demographic Health Survey (DHS). The results are presented in Appendix Table 15 and show estimates within the same order of magnitude albeit less precisely estimated given the smaller sample size. See section 2 in the Appendix for details on the construction of the sample and measures. The DHS also reports the immunization record for the Hepatitis B vaccine. We find a negative and significant effect for this vaccine as well. This outcome is of particular interest since the CIA vaccination ruse consisted of a Hepatitis B vaccination campaign.

3.7 Results with Fewer Controls

Appendix Table 16 shows our main estimates are similar in a simpler specification where we only include cohort and district fixed effects as controls. The results are very similar to our preferred results presented in the paper.

[11] The observations dropped are selected at random among the observations that have the specified vaccination status.

3.8 Non-Monotonicity of the Treatment Effects

In Appendix Table 17, we explore potential non-monotoncity in the treatment effects. Columns 2, 5, and 8 present the results where we interact the *Post* dummy for fully-exposed cohorts with an indicator for districts above the median support for MMA—instead, of our baseline measure of vote share of MMA in standard deviations. The results are highly significant and large in magnitude. In columns 3, 6, and 9, we interact the fully-exposed cohort dummy with indicators for the quintiles of support for Islamist parties. Districts with support for Islamist parties above the 60th percentile are the ones experiencing the largest declines in vaccination rates, relative to districts below the 20th percentile. Note that districts with support for Islamist parties between the 60th and the 80th percentile have a somewhat larger effect (in magnitude) relative to districts with support above the 80th percentile. One potential interpretation is that the districts between the 60th and 80th percentiles may have had a larger fraction of parents that before the vaccine ruse were *on the margin* on their decision of whether to vaccinate their children. Instead, districts with support for Islamist parties above the 80th percentile may have had a large fraction of parents that were already opposed to vaccination before the vaccine ruse. While this could provide a plausible interpretation for the pattern of coefficients, the differences in the point estimates are not statistically significant. Hence, this interpretation should be taken with caution.

3.9 Longer-Term Effects

We examine the evolution of the effects for subsequent cohorts. Appendix Figure 10 extends our analysis to a larger set of fully-exposed cohorts. The results indicate that, while cohorts born around the time of the disclosure of the vaccine ruse show persistent lower vaccination rates, those born after mid-2012 experience a mitigation of the negative effects. One possible explanation is the fact that, starting in mid-2012, vaccination workers have directly attempted to address misconceptions by involving religious leaders that endorsed the usage of vaccines.[12] It is possible that this made parents regain confidence in vaccines and, that this in turn mitigated the negative effects on immunization rates.[13]

[12]Khan, Taimur. 2017. "How Pakistan got to near zero on polio". *www.devex.com*, November 14. *https://www.devex.com/news/how-pakistan-got-to-near-zero-on-polio-91521* (last accessed 07/18/2019). Khan et al. (2017).

[13]See section 8 for further discussion.

3.10 Full Immunization Results

In Appendix Figures 2 and 3, we examine the age profiles for complete immunization. The PSLM survey only records the first three doses of polio and DPT, as well as the first dose of measles. Hence, we consider a child completely immunized against each disease if she received all dosages recorded in the survey. Similarly, we consider children "completely immunized" once they have received all dosages documented in the survey for the vaccines. See section 2 in the Online Appendix for further details.

The first two panels of Appendix Figure 2 show the age profiles for full immunization of polio and DPT. The last panel shows the age profile of full immunization for the three vaccines.[14] The figures show a steady increase in the likelihood that children are fully immunized during the first 14 months of life. Hence, when the outcome is full immunization, we will consider children born between May 2010 and July 2011 as partially treated.

Appendix Figure 3 presents the age profiles for full immunization, before and after the disclosure of information, and across regions with different levels of support for Islamist parties. The results are similar to the ones documented for the first doses of each vaccine. In regions with low support for Islamist groups there are no differences in the age profiles before and after the treatment. In contrast, regions with high support for Islamist parties experience a decline of full immunization rates after the information on the vaccine ruse was disclosed.

Finallly, Appendix Figure 6 presents estimated regression coefficients for complete immunization of polio, DPT, and the three vaccines all together. We observe significant drops in immunization rates for fully exposed cohorts. Consistent with the age profiles of full immunization, we observe steady declines in immunization rates for those cohorts that were partially affected by the disclosure of information on the vaccine ruse.

3.11 Robustness to Spatial Autocorrelation: Conley Standard Errors

We examine the robustness of our results to using Conley Standard Errors that take into account spatial autocorrelation. We have used different cutoffs ranging from 50 to 400km. Our main results are highly robust and remain statistically significant throughout this range

[14]Note that we only have information on one dose of the measles vaccine. Hence, the "full immunization" figure for the measles vaccine would be equivalent to the one presented in Figure 2 in the main text of the paper.

of distance cutoffs. The results are presented in Appendix Table 6. Given that Conley standard errors lead to robust estimates in terms of significance, we continue to report the district-level clustered standard errors. Since our measure of electoral support for Islamist parties is measured at the district-level, and vaccination campaigns are organized at this administrative level, we think this is the most adequate level for clustering of the standard errors.

3.12 Robustness: Under-reporting of Vaccination Status

In order to minimize measurement error in our measure of vaccination status, we code successful vaccinations only if the enumerator was able to verify the information on children's vaccination card. Vaccination rates based on recall have been shown to be subject to a large extent of measurement error, mainly due to over-reporting of vaccination status (Sheikh et al (2011); Research and Development Solutions (2012)).

A potential concern in our context is that our measure suffers from under-reporting of vaccination status. For instance, parents may destroy or hide the vaccination report card to conceal that they vaccinated their children. Under-reporting may be more prevalent if parents were concerned about Taliban violence or intimidation.

However, we would expect that parents that wanted to under-report their children's vaccination status to comply with the Taliban directives would report *not* having vaccinated their children. In contrast, only 3.6% of parents report not having vaccinated their children against polio.[15] Furthermore, the fraction of parents that indicate not having vaccinated their children declines after the disclosure of the vaccine ruse and this decline is not differential across regions with different levels of support for Islamist parties. Appendix Figure 9 shows these results. While the fraction of parents that report not having vaccinated their children fluctuates over time, it is not differential across regions with high and low levels of Islamist support and it is always below 10%. If parents were strategically destroying or hiding the vaccine card to conform with the views of the Taliban, we would expect a higher fraction of self-reported lack of vaccination in areas with high Islamist support for partially- and fully-exposed cohorts. To set these numbers in perspective, we also report in the same graph the evolution of our measure of vaccination status—as verified in the report card—by level of Islamist support. As we can see, the evolution of these rates is similar across regions for the unaffected cohorts but starts differing for the partially- and fully- affected cohorts.

[15] 55% of parents report having vaccinated their children based on recall measures. The remaining 42% report successful vaccinations based on the vaccination card.

This evidence also mitigates the concern that social desirability of vaccination changed upon the disclosure of the vaccination ruse. Social image concerns have been shown to have important implications for multiple areas of human behavior,[16] including vaccination decisions.[17] If vaccinating children became less socially desirable in areas with high Islamist support, we would have expected a differential increase in the self-reported lack of vaccination, relative to areas with low Islamist support.[18]

3.13 Robustness of Heterogenous Results by Gender of the Child

We have verified that the heterogenous effects as a function of the child's gender are robust to our set of main robustness checks shown in Table 4, as well as to the flexible measures of supply examined in Table 6. The robustness results for the gender heterogeneity are presented as Appendix Tables 18 and 19.

3.14 Additional Results: Effects on Trust Measures

We have also examined the effects on a range of measures of trust using data from the South Asia Barometer. These data report individual-level measures of trust in different organizations for a large sample of individuals. We use the two waves of this survey in closest temporal proximity to the vaccine ruse—waves 2005 and 2013.

Unfortunately, the survey does not explicitly record trust on formal medicine or in health organizations. The closest proxy of trust in the health sector is trust in the civil service. Furthermore, the survey does not have information on the district of residence of individuals. Respondents are geocoded at the provincial level. We estimate a simple *Difference-in-Differences* model comparing measures of trust in the wave before and after the disclosure of information, and across provinces with above or below the median support for Islamist groups. Hence, these results should be interpreted with caution.

[16] See Bursztyn and Jensen (2017) for a literature review.

[17] See Karing (2018).

[18] Note that we also empirically examined whether the likelihood of having the vaccination card changes differentially across districts. This information is given by an additional variable in the PSLM dataset. Appendix Table 20 shows the share of households that have the vaccination card across wave of the survey and quarter of Islamist support. Across all districts (column 1) there is no change in the fraction of households that have the card. While there is a larger decline in the propensity to show the card for high Islamist support areas relative to low support areas, the difference is small. In our standard regression framework, shown in Appendix Table 21 the difference is not statistically significant.

Panel A in Appendix Table 9 presents the results. Column 1 shows the effects on trust in civil service. Provinces with high support for Islamist groups experienced a 7.6 percentage points decline in trust in the civil service after the disclosure of the vaccine ruse. This effect represents a 16% decline over the sample mean. Columns 2 to 9 show the effects on trust in other organizations. With a couple of the exceptions, most of the effects are negative and significant. Column 10 uses as dependent variable a z-score for the different measures of trust. We find that there was an overall decline in trust of 0.08 standard deviations.

Given the coarseness in the geographic measure of support for Islamist groups, we enrich our empirical strategy by examining an individual-level predictor of sympathies for Islamist groups. In particular we add a triple interaction with a dummy that takes value 1 for individuals that do not own a TV. The Taliban have discouraged ownership of TV with the argument that that type of entertainment is contrary to the ultra-conservative lifestyle they advocate for (Roul, 2014). The results, presented in Panel B, suggest that the decline in trust is driven by individuals that do not own a TV. The triple interaction is negative, large in magnitude, and typically statistically significant. Hence, these results suggest that the effects are driven by those individuals that are more likely to hold views aligned with Islamist groups. In Appendix Table 10 we present the coefficients of the post dummy, the "no TV" dummy, and the binary interactions of each of these variables. The results are robust to including measures of wealth of the individual, such as indicators for ownership of other items such as a car, phone, or fridge. The results are available upon request.

3.15 Robustness of Results on Health-Seeking Behavior

In Appendix Table 7, we present the Heckman selection equation corresponding to the main results presented in Table 5. The results indicate that the share of sick children in the same month and district as a proxy for the disease environment is a relevant predictor of a child's own health status.

In Appendix Table 8, we show robustness of the results presented in Table 5 to using data from the DHS survey. The results are less precisely estimated and the effects on the prevalence of sickness are no longer significant. However, the negative effects on contacting formal doctor remain negative and significant for most diseases.

In Appendix Table 22, we examine the robustness of the results presented in Table 5 to our main controls for exposure to Taliban-related conflict events. The negative effects on contacting formal medical providers remain negative and significant when controlling for

conflict in the first year of a child's life or for the level of conflict in 2010 interacted with year fixed effects.

3.16 Additional Results: Relevance of Unani Medicine in the Decline in Vaccination Rates

An alternative channel through which the vaccine ruse could have affected immunization outcomes is by increasing demand for informal (sometimes referred to as 'Unani' in the region) medicine. In particular, the vaccine ruse disclosure could have pushed parents to seek out medical treatments predominantly from informal providers who were not willing or able to provide vaccines which could explain the decline in immunization rates.

To examine this possibility we have conducted a text analysis exercise based on the articles that described the events of interest. Through the Nexis UNI database, we have obtained access to the universe of articles published by the Pakistani English-language newspapers between Jan 1, 2009 and Dec 31, 2013. In particular, the newspapers are: Daily Regional Times, Daily Times, Dawn, Pakistan Today, The Express Tribune, The Frontier Star and The Nation. Within these articles we searched for those that contain the words "Shakil Afridi AND CIA" or "Shakil Afridi AND spy". We found 294 articles. In those articles we searched for a number of words that relate to vaccines and words that relate to traditional medicine. For words that proxy for vaccines we searched for "polio". We found this word in 62 articles. (Note that we did not explicitly searched for the word "vaccine", since that may have been included in the description of the vaccine ruse. In contrast, polio is typically discussed in these articles in the context of the negative effects on the acceptance of the polio vaccine). For words that capture informal medicine we searched for "traditional", "alternative", "Unani", "informal", "herbal", "hakeem", "spiritual", "homeopath" and "chemist". Out of the 294 articles, the terms "traditional" or "alternative" were only found in 8 articles. The terms "informal" and "spiritual" were found in 3 and 1 article, respectively. The remaining keywords—including the term "Unani" and different spellings of this word—were not mentioned in any of the 294 articles. Furthermore, our reading of these articles and other anecdotal accounts do not suggest that the turn to informal medicine was the driving factor leading to the decrease in immunization rates. Rather than this, the qualitative evidence suggests that the vaccine ruse and subsequent anti-vaccine propaganda lead to an increase in distrust in formal medicine, and this in turn lead to more vaccine skepticism and to an increase in demand of some practices of informal medicine. To provide a benchmark on the

relevance of trust, out of the 294 articles described above 58 of them mentioned some of the following words: "trust", "confidence", "mistrust", "distrust", "suspect", "misgiving" and "suspicion".

3.17 Additional Results: Media Consumption and Exposure to Radicalized Information Sources

One key question to better understand the mechanism underlying our results is whether the effects are driven by the disclosure of the vaccine ruse or by the subsequent anti-vaccine propaganda. If the effects are driven by the disclosure of the vaccine ruse, we would expect that the declines in vaccination rates should be larger in areas where a larger fraction of the population is exposed to the news via mainstream media which widely reported on the incident. In contrast, if the anti-vaccine propaganda by Islamist groups played a key role, we would expect that the effects originate from areas where a larger share of the population relies on informal rather than mainstream information sources.

To further explore this issue, we use data on consumption of and trust in different media sources collected by Fair et al. (2015) in 2013. The household survey conducted by these authors spans 80 districts and elicits detailed information about the sources of information that respondents use to learn about events within Pakistan. Moreover, respondents are asked to indicate which of the different information sources they trust most. We use this data to compute the share of citizens who get their news from informal sources such as gatherings with religious or traditional leaders, or from other family members. In particular, we focus on the share of respondents that explicitly say that they do not get their news from Pakistani public or private TV channels, Pakistani Urdu and English-language news papers as well as international TV channels or newspapers in print. In other words, we compute the share of respondents in a particular district that indicate not to rely on any of the mainstream media outlets to obtain information about events that happened within Pakistan. We also compute the share of respondents whose most trusted information source for information about such events is not any of these mainstream media sources. We then investigate how our main results compare when we use either of these two measures instead of our measure of support for Islamist groups based on the MMA vote shares.

The results are displayed in Table 7 in the paper. In column 1, we present our baseline results for the subsample of 80 districts for which the household survey data collected by Fair et al. (2015) is available. The estimate indicates that a one standard deviation increase

in our preferred measure of support for Islamist groups is associated with a 7.2 percentage points lower share of children who obtained the first dose of the polio vaccine in the post-disclosure period. In column 2, we interact the post dummy with the share of respondents who indicate to not get their news about events in Pakistan from any of the aforementioned mainstream media sources. We can see that the negative, differential effect is even slightly larger than the baseline effect and highly statistically significant. In column 3, we focus on the share of respondents who indicated that their most trusted source of information for such events was not any of the mainstream media sources. Again, the interaction term with the post dummy is negative and statistically significant. A one standard deviation increase in the share of respondents who do not trust any of the mainstream information sources is associated with a 6.1 percentage points lower share of children who obtained the first dose of the polio vaccine in the aftermath of the vaccine ruse disclosure.

These results suggest that access to mainstream media reporting on the vaccine ruse is unlikely to explain our findings. In contrast, the results support the hypothesis that access to "alternative", more informal sources of information led to a decline in demand for vaccinations. We should interpret these results with caution – given how patterns of media consumption are likely interrelated with ideology, the effects displayed may not capture causal effects of information exposure. Nonetheless, the results are broadly consistent with the proposed mechanism.

3.18 Additional Results: Support for the Taliban and Exposure to Anti-Vaccine Propaganda

Building on the analysis and data explored in the previous section, we also construct a measure of exposure to potentially radicalized religious sources. In particular, we compute the share of respondents that declare that their most trusted source of information on events that happen within Pakistan are gatherings with religious leaders. Importantly, we also condition on the respondent expressing explicit support for the Pakistani Taliban. This second condition is important as many religious leaders are not supportive of the Taliban's view and are therefore not spreading their propaganda. The underlying assumption is that individuals who express outright support for the Pakistani Taliban are also visiting religious leaders that share and support radicalized perspectives. While the share of individuals for which these demanding conditions are satisfied is small on average, there is a substantial correlation between this measure and our measure of support for Islamist groups based on

electoral data. In Column 4 of Table 7, we substitute our main measure of support for Islamist groups with this share of respondents who obtain most trust religious leaders as an information source and support the Pakistani Taliban. The results indicate that a one standard deviation increase in this measure is associated with a 4.5 percentage points lower share of children who obtained the first dose of the polio vaccine after the disclosure of the vaccine ruse. While the magnitude of this effect is smaller than the effect derived from our preferred measure of support for Islamist groups, the effect is non-negligible and statistically significant.

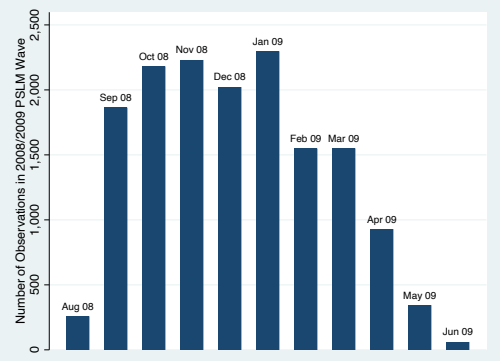
Finally, Column 5 of Table 7 investigates how our baseline results compare to a specification where the measure of support for Islamist groups is replaced with the share of respondents which had heard of the US drone strikes in Pakistan's FATA region. The point estimate indicates that a one standard deviation increase in the share of respondents who had heard of the US drone strikes is associated with a 5.8 percentage point decline in the likelihood that a child had obtained the first dose of the polio vaccine after the vaccine ruse disclosure. This result provides additional evidence that exposure to Taliban propaganda is associated with the differential decline in vaccination rates as Taliban propaganda frequently condemned US drone strikes in the region. The results are similar when using a measure of opposition to the US drone strikes (even if sanctioned by the Pakistani government) instead. These results are available upon request.

While none of the above measures represent an ideal proxy for exposure to Taliban propaganda, the documented empirical patterns are consistent with the hypothesis that exposure to such messaging is a likely driver of the effects discussed in the paper.

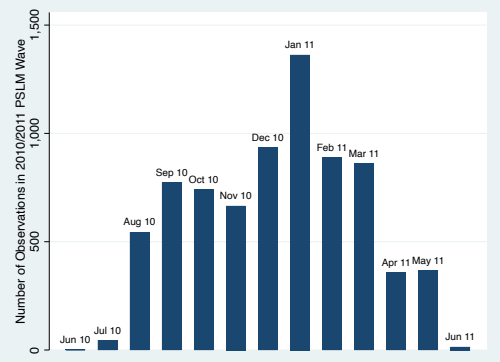
4 Appendix Figures

Appendix Figure 1: Timing of Surveys of the PSLM waves

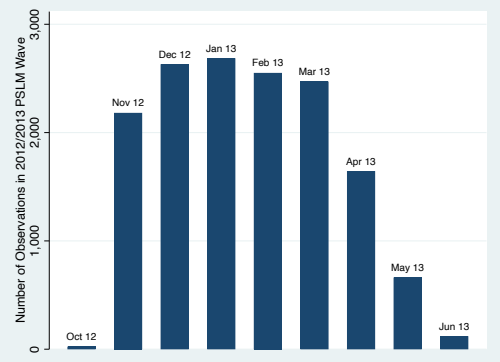
2008/09 PSLM Wave



2010/11 PSLM Wave

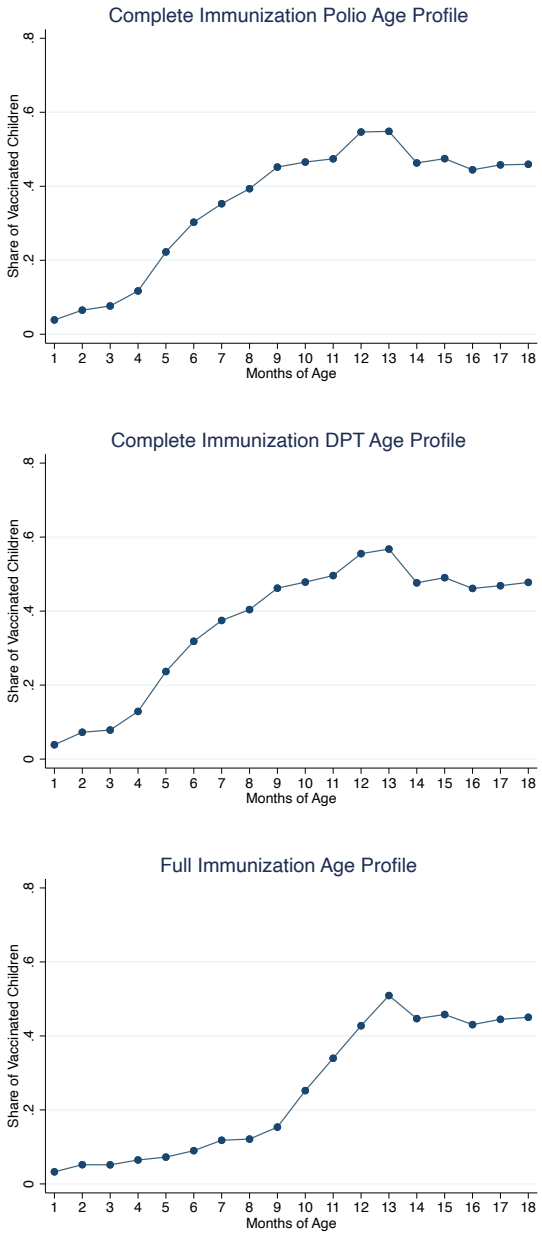


2012/13 PSLM Wave



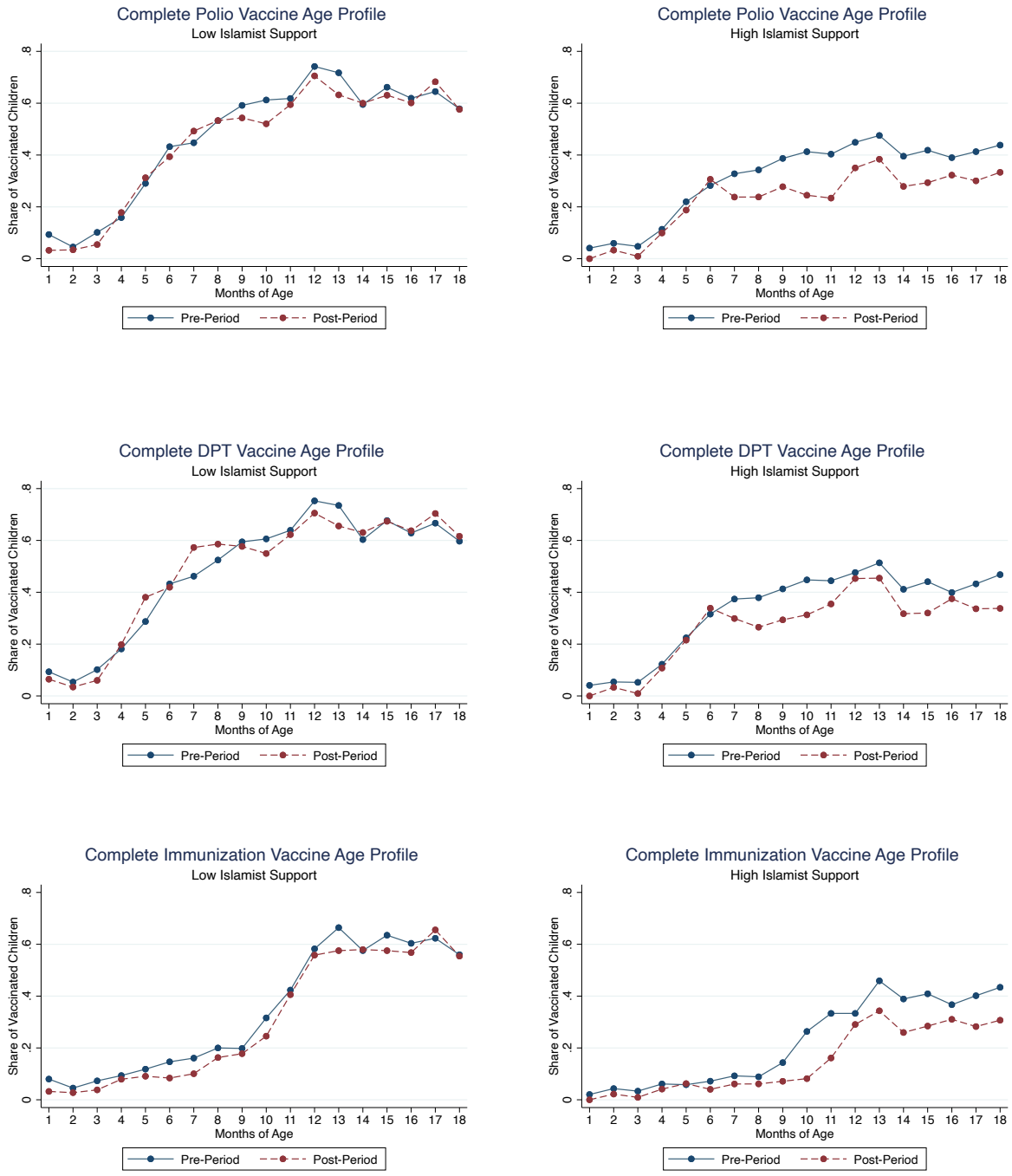
Notes: Distribution of months of interview for the children in our sample by wave of the PSLM survey.

Appendix Figure 2: Age Profile for All Doses of Vaccines. Pre-Treatment Period



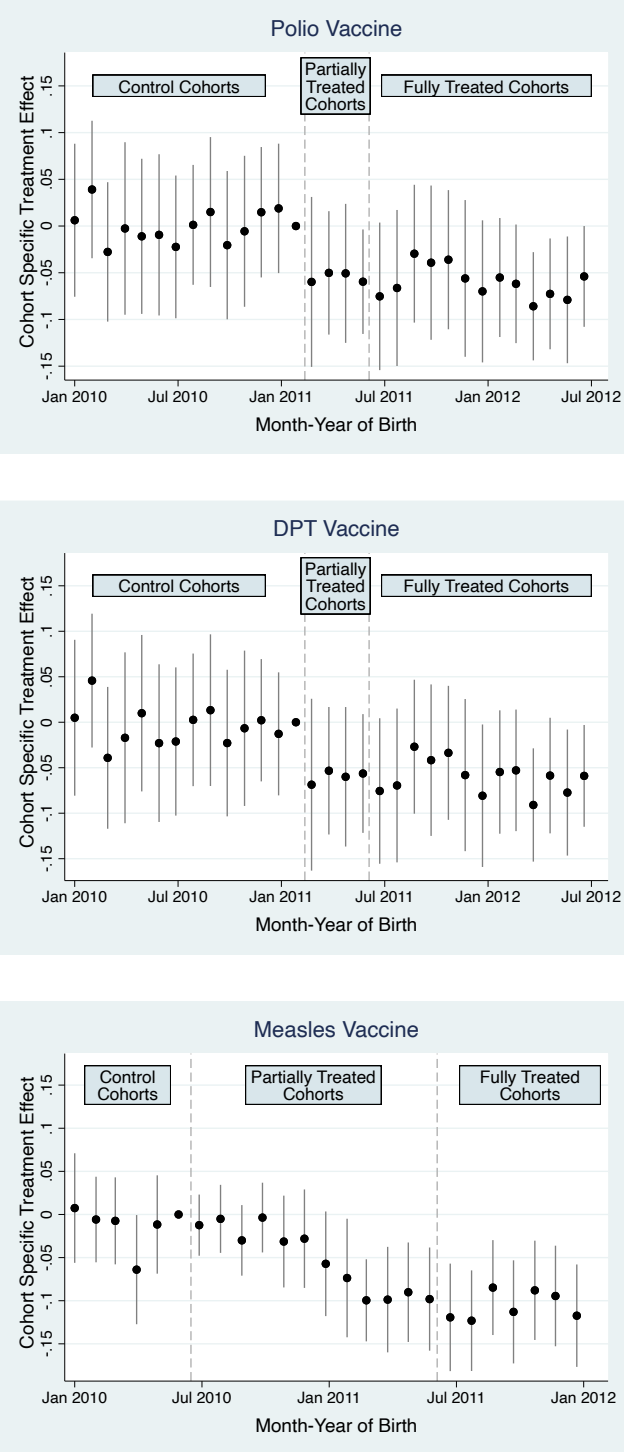
Notes: These figures show the fraction of children that received all the doses of each vaccine by their age at the time of interview. Only the pre-treatment waves of the survey (2008/9 and 2010/11) are used.

Appendix Figure 3: Age Profile for All Doses of Vaccines. Before & After Treatment. By Level of Islamist Support



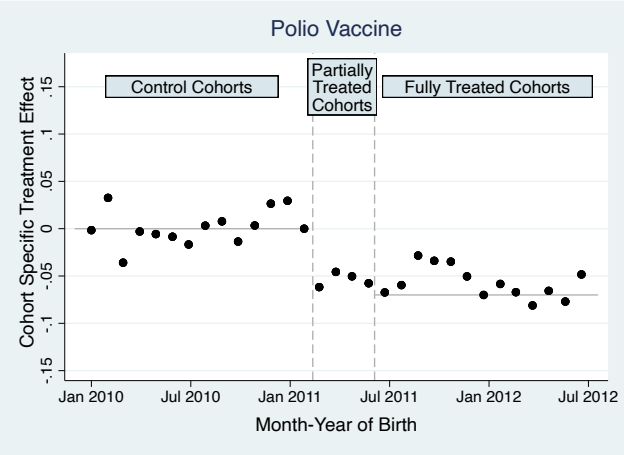
Notes: These figures show the fraction of children that received all the doses of each vaccine by their age at the time of interview. The figures on the left (right) hand side restrict the sample to districts in the bottom (top) quartile of vote shares for Islamist parties. The solid-blue age profiles are obtained from the pre-treatment waves of the survey, 2008/9 and 2010/11. The dashed-red age profiles are obtained from the post-treatment wave, 2012/13.

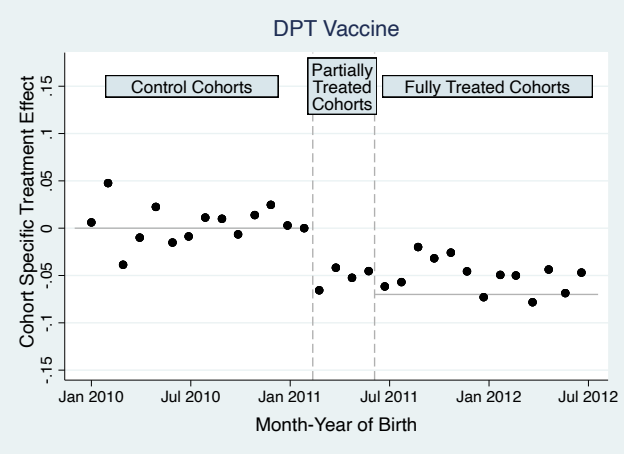
Appendix Figure 4: Treatment Effects by Monthly Cohort (With Confidence Intervals)

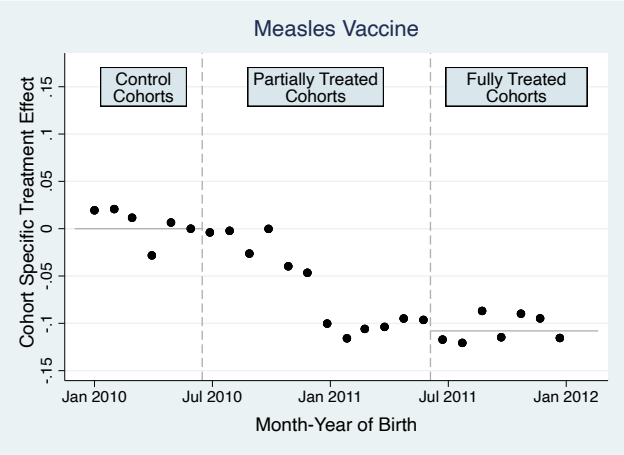


Notes: These figures show cohort-specific treatment effects by month of birth. In particular they show the coefficients on the interaction of Islamist support with the corresponding cohort indicator. The omitted category corresponds to the last cohort of the non-exposed cohorts, i.e., children born in February 2011 for polio and DPT and children born in June 2010 for measles. The vertical lines associated with each point estimate correspond to the 90% confidence intervals.

Appendix Figure 5: Treatment Effects by Monthly Cohort. Only Controlling for Monthly-Cohort and District Fixed Effects

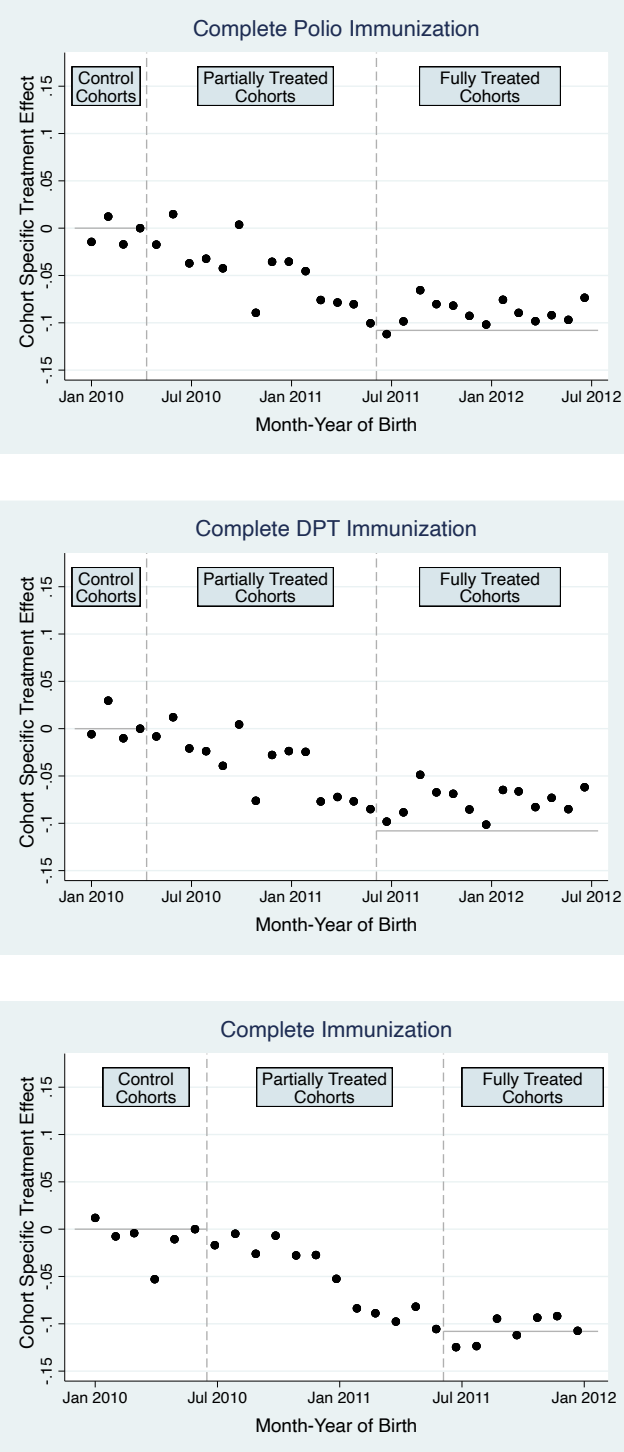






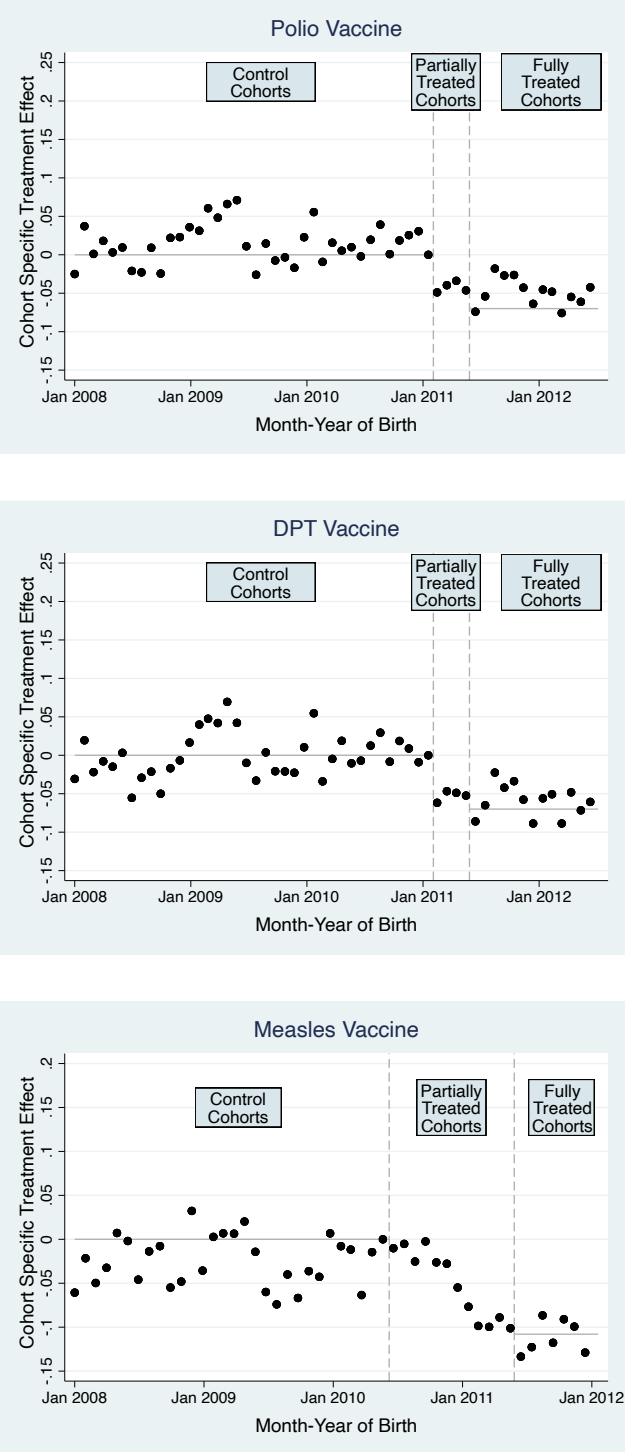
Notes: These figures show cohort-specific treatment effects by month of birth. In particular they show the coefficients on the interaction of Islamist support with the corresponding cohort indicator. The omitted category corresponds to the last cohort of the non-exposed cohorts, i.e., children born in February 2011 for polio and DPT and children born in June 2010 for measles.

Appendix Figure 6: Treatment Effects by Monthly Cohort. Full Immunization



Notes: These figures show cohort-specific treatment effects by month of birth. In particular they show the coefficients on the interaction of Islamist support with the corresponding cohort indicator. The omitted category corresponds to the last cohort of the non-exposed cohorts, i.e., children born in April 2010 for polio and DPT and children born in June 2010 for the complete immunization outcome.

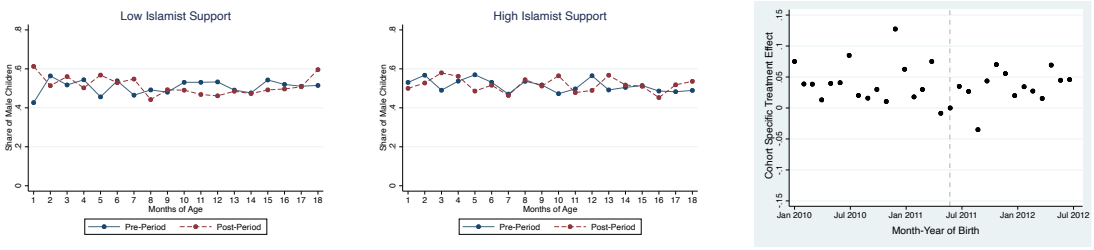
Appendix Figure 7: Treatment Effects by Monthly Cohort. Longer Pre-Treatment Period.



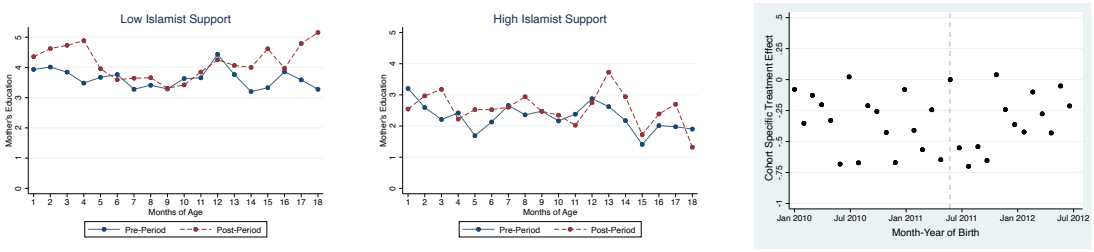
Notes: These figures show cohort-specific treatment effects by month of birth. In particular they show the coefficients on the interaction of Islamist support with the corresponding cohort indicator. The omitted category corresponds to the last cohort of the non-exposed cohorts, i.e., children born in February 2011 for polio and DPT and children born in June 2010 for measles.

Appendix Figure 8: Robustness Checks. Lack of Changes in Household Composition

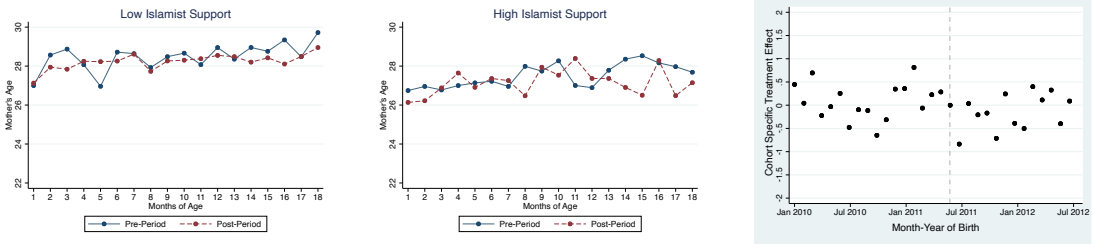
Male Children



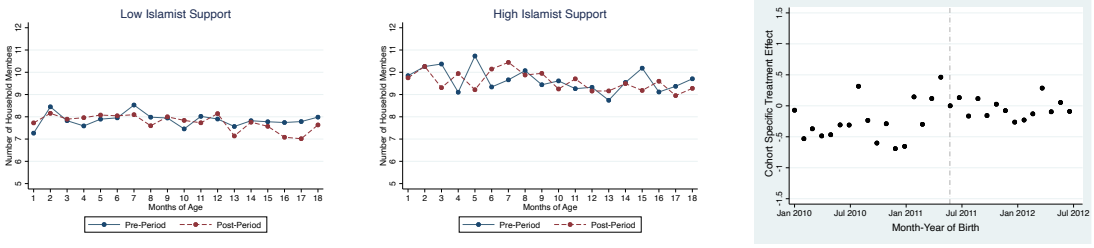
Mother's Education



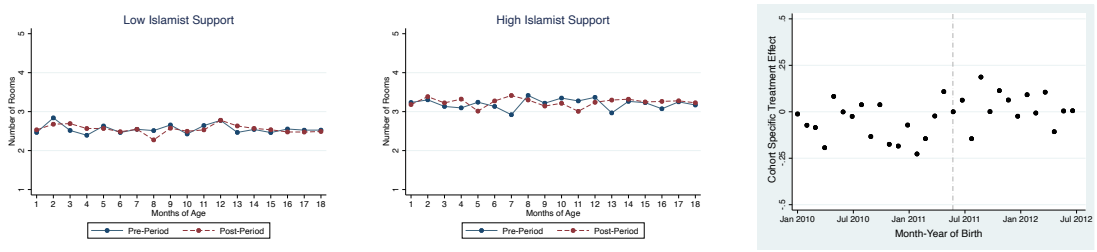
Mother's Age



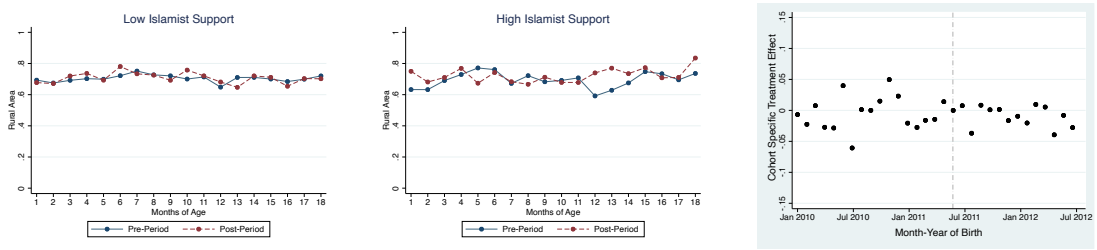
Number of Household Members



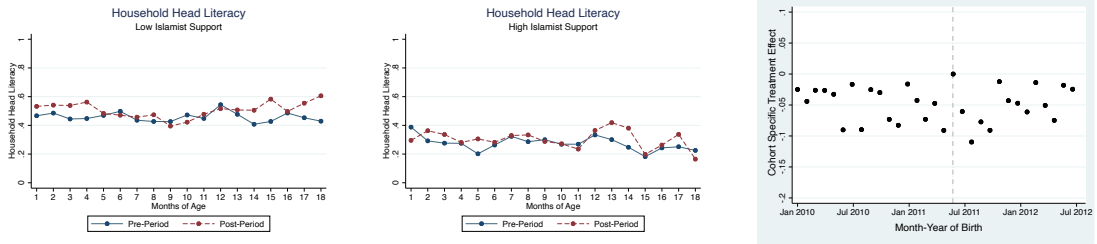
Number of Rooms



Rural Area

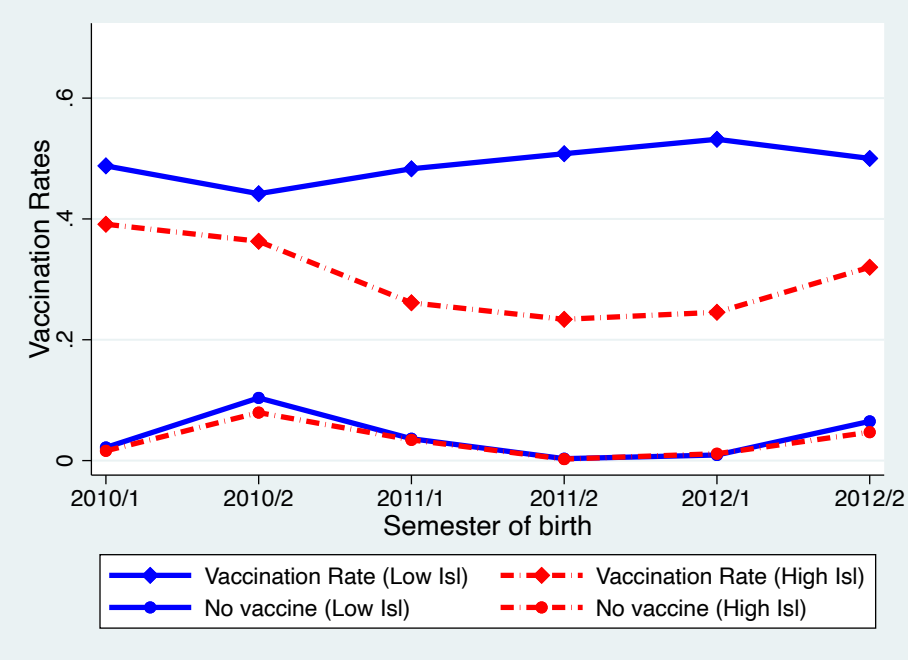


Household Head Literacy



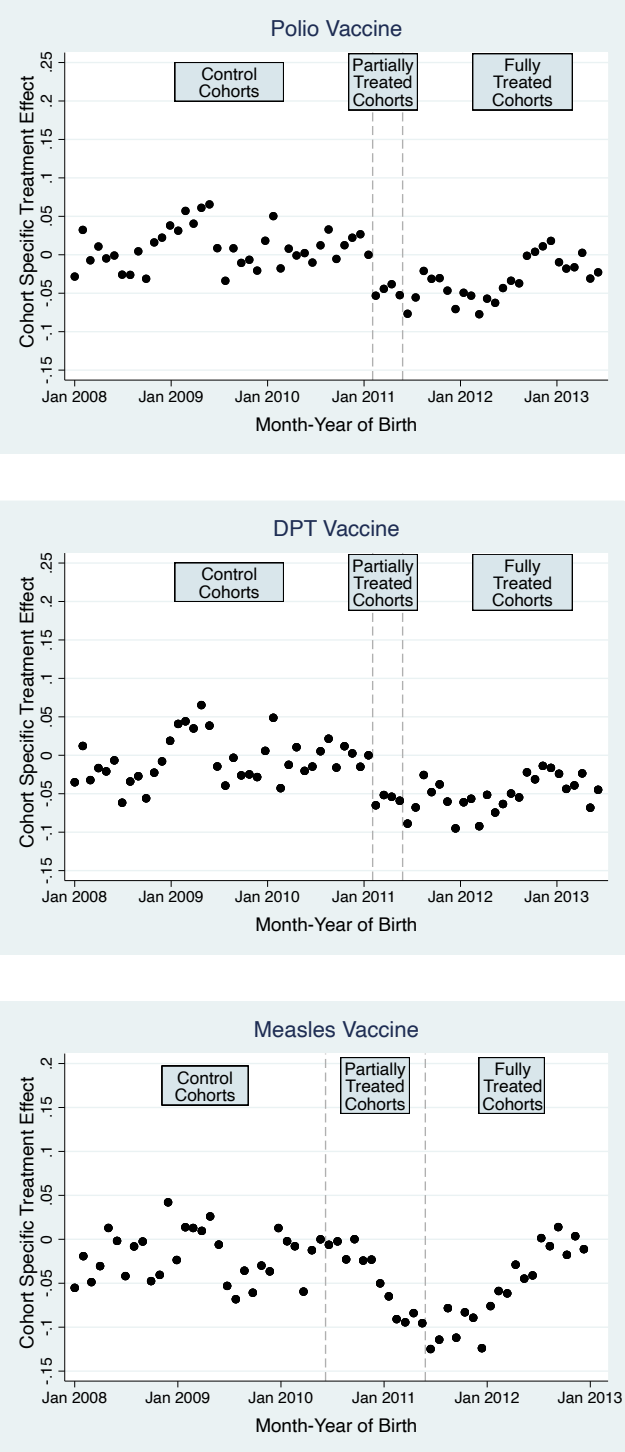
Notes: The figures on the left and in the middle show the average of a particular characteristic by the age of a child by the time of the interview. The figures on the left (in the middle) restrict the sample to districts in the bottom (top) quartile of vote shares for Islamist parties. The solid-blue age profiles are obtained from the pre-treatment waves of the survey, 2008/9 and 2010/11. The dashed-red age profiles are obtained from the post-treatment wave, 2012/13. The figures to the right show cohort-specific treatment effects by month of birth. In particular, they show the coefficient on the interaction of Islamist support with the corresponding cohort indicator. The omitted category corresponds to the the cohort born in June 2011, i.e. the month prior to the vaccine ruse disclosure.

Appendix Figure 9: Evolution of Polio Vaccination Rates



Notes: Diamond-connected lines correspond to the fraction of parents that have vaccinated their children against polio—as verified in the vaccination card—, by semester of birth of the child. Circle-connected lines correspond to the fraction of parents that report not having vaccinated their children against polio. The difference between the sum of these lines and 1 corresponds to the fraction of parents that self-report having vaccinated their children, but no proof of such vaccinations was obtained by the enumerators. Solid-blue lines restrict the sample to districts below the median support for Islamist parties. Dashed-red lines restrict the sample to districts above the median support for Islamist parties.

Appendix Figure 10: Treatment Effects by Monthly Cohort. Medium-Run Effects.



Notes: These figures show cohort-specific treatment effects by month of birth. In particular they show the coefficients on the interaction of Islamist support with the corresponding cohort indicator. The omitted category corresponds to the last cohort of the non-exposed cohorts, i.e., children born in February 2011 for polio and DPT and children born in June 2010 for measles.

5 Appendix Tables

Appendix Table 1: Immunization Calendar of Pakistan

Vaccine	First Dose	Second Dose	Third Dose	Fourth Dose
Polio	At birth	6 Weeks	10 Weeks	14 Weeks
DPT	6 Weeks	10 Weeks	14 Weeks	
Measles	9 Months	15 Months		

Notes: Official immunization schedule of Pakistan for the main three vaccines. Published by the Expanded Program on Immunization (EPI), Pakistan
http://www.epi.gov.pk/immunisation-schedule/ (last accessed July1st, 2019)

Appendix Table 2: Descriptive Statistics

	Observations (1)	Mean (2)	Std. Dev. (3)
	Panel A. Child Characteristics		
Received one dose of Polio vaccine	18,795	0.418	0.493
Received one dose of DPT vaccine	18,795	0.450	0.498
Received one dose of Measles vaccine	18,795	0.256	0.437
Received three doses of Polio vaccine	18,795	0.333	0.471
Received three doses of DPT vaccine	18,795	0.363	0.481
Received all vaccines	18,795	0.230	0.421
Illness or injury (two weeks prior to interview)	18,795	0.190	0.392
Age (in months)	18,795	11.047	6.299
Male	18,795	0.514	0.500
	Panel B. Mother Characteristics		
Mother's education level	18,795	3.531	4.370
Mother's age	18,795	27.981	6.033
	Panel C. Household Characteristics		
Rural region	18,795	0.656	0.475
Radio ownership	18,795	0.232	0.422
Television ownership	18,795	0.580	0.494
Number of rooms	18,795	2.636	1.555
Number of household members	18,795	8.235	3.883
	Panel D. District Characteristics		
Vote Share MMA	115	0.071	0.112
Vote Share PPP	115	0.261	0.203
Vote Share PML (N)	115	0.108	0.143

Notes: In Panel A, B and C, the unit of observation is the child level. The sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. In Panel D, the unit of observation is the district.

Appendix Table 3: Tabulation of Cohorts in the Baseline Sample

Survey Wave	Cohort	Number of Observations	Share of the Sample
2010/11	2010/01	726	3.86
	2010/02	733	3.90
	2010/03	569	3.03
	2010/04	523	2.78
	2010/05	592	3.15
	2010/06	691	3.68
	2010/07	727	3.87
	2010/08	800	4.26
	2010/09	589	3.13
	2010/10	452	2.40
	2010/11	429	2.28
	2010/12	460	2.45
2010/11 & 2012/13	2011/01	449	2.39
	2011/02	305	1.62
	2011/03	400	2.13
	2011/04	412	2.19
	2011/05	517	2.75
	2011/06	678	3.61
	2011/07	733	3.90
	2011/08	720	3.83
	2011/09	710	3.78
	2011/10	757	4.03
	2011/11	631	3.36
	2011/12	554	2.95
2012/13	2012/01	676	3.60
	2012/02	659	3.51
	2012/03	566	3.01
	2012/04	596	3.17
	2012/05	586	3.12
	2012/06	770	4.10
	2012/07	785	4.18

Appendix Table 4: Robustness Checks. Lack of Changes in Household Composition

	Dependent Variables:					
	Dummy for Male Child	Mother's Education	Mother's Age	Number of Household Members	Number of Rooms	Dummy for Rural Region
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Dep. Var.	*0.514*	*3.531*	*27.981*	*8.235*	*2.636*	*0.656*
Post × Islamist Support	-0.005	-0.027	-0.143	0.120*	0.051	-0.007
	(0.010)	(0.059)	(0.140)	(0.072)	(0.045)	(0.009)
Observations	18,795	18,795	18,795	18,795	18,795	18,795
R-squared	0.010	0.265	0.029	0.095	0.109	0.191

Notes: Standard errors clustered at the district-level in parentheses. The unit of observation is the child level. The sample consists of children born bet
2010 and July 2012 that are less than 24 months of age at the time of interview. All regressions include district, monthly cohort, monthly age, and cale
interview fixed effects and a dummy for rural regions (except for the specification displayed in column 6).

Appendix Table 5: Robustness Checks. Expanding on Differential Pre-Trends Analysis

	Baseline	Additional Controls for Pre-Trends (Interacted with Monthly Cohort FE)			
		Mean of Dep Var Pre-Treatment		Average Monthly Change in Dep Var	
		Continuous Variable	Dummies for Quintiles	Continuous Variable	Dummies for Quintiles
	(1)	(2)	(3)	(4)	(5)
	Panel A. First Dose of Polio Vaccine				
Post × Islamist Support	-0.058***	-0.059***	-0.045**	-0.063***	-0.050***
	(0.020)	(0.020)	(0.021)	(0.020)	(0.017)
Observations	16,788	16,788	16,788	16,746	16,746
R-squared	0.260	0.262	0.268	0.268	0.275
	Panel B. First Dose of DPT Vaccine				
Post × Islamist Support	-0.054***	-0.056***	-0.048**	-0.057***	-0.056***
	(0.018)	(0.019)	(0.020)	(0.018)	(0.017)
Observations	16,788	16,788	16,788	16,746	16,746
R-squared	0.240	0.241	0.247	0.245	0.249
	Panel C. First Dose of Measles Vaccine				
Post × Islamist Support	-0.054***	-0.052***	-0.055***	-0.055***	-0.054***
	(0.016)	(0.016)	(0.015)	(0.016)	(0.017)
Observations	12,577	12,439	12,439	12,322	12,322
R-squared	0.252	0.258	0.264	0.258	0.262
	Panel D. All Vaccines				
Post × Islamist Support	-0.057***	-0.055***	-0.058***	-0.059***	-0.057***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Observations	12,577	12,439	12,439	12,322	12,322
R-squared	0.258	0.266	0.271	0.265	0.270

Notes: Standard errors clustered at the district-level in parentheses. There are 110 parent districts in the baseline sample. The unit of observation is the child level. All regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. Column 2 adds as controls the mean of the dependent variable in the pre-treatment period (i.e., for non-exposed cohorts) for each district interacted with monthly cohort fixed effects. Column 3 adds as controls the same variable but expressed with dummies for its quintiles instead of in continuous form. Columns 4 and 5 conduct analogous robustness checks to those in columns 3 and 4, but when the variable of interests are average changes in the mean of the dependent variable, instead of the average level.

Appendix Table 6: Robustness Checks. Spatial Autocorrelation

	Distance Cutoff for Conley Standard Errors							
	50	100	150	200	250	300	350	400
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. First Dose of Polio Vaccine							
Post × Islamist Support	-0.058***	-0.058**	-0.058**	-0.058*	-0.058*	-0.058**	-0.058**	-0.058**
	(0.022)	(0.025)	(0.027)	(0.030)	(0.032)	(0.029)	(0.029)	(0.025)
Observations	16,788	16,788	16,788	16,788	16,788	16,788	16,788	16,788
R-squared	0.260	0.260	0.260	0.260	0.260	0.260	0.260	0.260
	Panel B. First Dose of DPT Vaccine							
Post × Islamist Support	-0.054***	-0.054**	-0.054**	-0.054**	-0.054**	-0.054**	-0.054**	-0.054**
	(0.019)	(0.022)	(0.022)	(0.024)	(0.025)	(0.022)	(0.022)	(0.021)
Observations	16,788	16,788	16,788	16,788	16,788	16,788	16,788	16,788
R-squared	0.240	0.240	0.240	0.240	0.240	0.240	0.240	0.240
	Panel C. First Dose of Measles Vaccine							
Post × Islamist Support	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***
	(0.017)	(0.018)	(0.017)	(0.018)	(0.020)	(0.016)	(0.012)	(0.011)
Observations	12,577	12,577	12,577	12,577	12,577	12,577	12,577	12,577
R-squared	0.252	0.252	0.252	0.252	0.252	0.252	0.252	0.252
	Panel D. All Vaccines							
Post × Islamist Support	-0.057***	-0.057***	-0.057***	-0.057**	-0.057**	-0.057**	-0.057***	-0.057***
	(0.017)	(0.019)	(0.022)	(0.023)	(0.025)	(0.023)	(0.019)	(0.017)
Observations	12,577	12,577	12,577	12,577	12,577	12,577	12,577	12,577
R-squared	0.258	0.258	0.258	0.258	0.258	0.258	0.258	0.258

Notes: Conley standard errors for varying distance cutoffs in parentheses. There are 110 parent districts in the baseline sample. The unit of observation is the child level. All regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions.

Appendix Table 7: Effects on Health Seeking Behavior (Heckman Selection Equation)

	Dependent Variables:	
		Heckman Selection Equation
	Dummy for Illness in Last 2 Weeks	Dummy for Illness in Last 2 Weeks
	(1)	(2)
Mean Dep. Var.	*0.190*	*0.190*
Post July 2011 × Islamist Support	0.025*	0.057
	(0.013)	(0.039)
		[0.032]
Share of Sick Children		1.414***
		(0.148)
		[0.121]
Observations	18,795	18,739
R-squared	0.064	0.073
Number of Clusters	110	109

Notes: Standard errors clustered at the district-level in parentheses. Standard errors derived from a bootstrap procedure with 1,000 bootstrap replications in square brackets. The unit of observation is the child level. All regressions include district fixed effects, quarter of interview fixed effects, monthly age fixed effects, and a dummy for rural regions. The selection equation includes as an excluded instrument a proxy for the disease environment: the share of children that are sick in the same quarter and district (excluding a child's own illness status).

Appendix Table 8: Effects on Health Seeking Behavior (DHS Survey)

	Dependent Variables:		
	Dummy for Illness in Last 2 Weeks	Dummy for Consulted Anyone	Dummy for Consulted Formal Medical Sector
	(1)	(2)	(3)
	Panel A. Children Illness: Diarrhea		
Mean Dep. Var.	*0.319*	*0.751*	*0.678*
Post July 2011 × Islamist Support	-0.016	-0.108***	-0.062**
	(0.019)	(0.026)	(0.027)
Observations	6,507	2,071	2,072
R-squared	0.074	0.162	0.167
	Panel B. Children Illness: Cough		
Mean Dep. Var.	*0.354*	*0.801*	*0.819*
Post July 2011 × Islamist Support	-0.029	-0.022	0.019
	(0.031)	(0.024)	(0.022)
Observations	6,506	2,296	2,056
R-squared	0.084	0.081	0.137
	Panel C.Any Children Illness		
Mean Dep. Var.	*0.524*	*0.940*	*0.903*
Post July 2011 × Islamist Support	-0.031	-0.049***	-0.027
	(0.026)	(0.015)	(0.022)
Observations	6,501	2,889	2,748
R-squared	0.103	0.103	0.101

Notes: Standard errors clustered at the district-level in parentheses. The unit of observation is the child level. All regressions include district fixed effects, quarter of interview fixed effects, monthly age, and a dummy for rural regions. In column 3, the formal sector is defined as seeking medical treatment or treatment in public facilities.

Appendix Table 9: Effects on Trust Measures

	Dependent variables. Trust in:							
	Civil Service	Police	The Courts	Parliament	Political Parties	Army	Central Government	Provin Govern
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean Dep. Var.	*0.462*	*0.533*	*0.494*	*0.473*	*0.584*	*0.504*	*0.534*	*0.49*
	Panel A. Effects on Trust							
Post July 2011 x (Islamist Support > Average)	-0.076**	-0.135***	-0.063	-0.094**	-0.190***	0.144***	-0.052	0.01
	(0.039)	(0.036)	(0.039)	(0.039)	(0.036)	(0.035)	(0.039)	(0.03
Observations	3,252	3,252	3,252	3,252	3,252	3,252	3,252	3,25
R-squared	0.054	0.208	0.029	0.054	0.215	0.204	0.050	0.04
	Panel B. Effects on Trust by Ownership of TV							
Post July 2011 x (Islamist Support > Average)	0.040	-0.100**	-0.031	-0.030	-0.099**	0.158***	0.003	0.107
	(0.050)	(0.045)	(0.051)	(0.050)	(0.047)	(0.042)	(0.050)	(0.05
Post July 2011 x (Isl. Support > Average) x No TV	-0.271**	-0.218**	-0.120	-0.154	-0.305***	0.109	-0.236**	-0.26
	(0.108)	(0.098)	(0.103)	(0.106)	(0.096)	(0.096)	(0.105)	(0.10
Observations	3,212	3,212	3,212	3,212	3,212	3,212	3,212	3,21
R-squared	0.054	0.209	0.034	0.056	0.222	0.215	0.052	0.04

Notes: Robust standard errors in parentheses. The unit of observation is the individual. The dependent variables are indicators for whether the respondent repor organizations "a great deal" or "quite a lot". In Panel A, the regressor of interest is the interaction of an indicator for the 2013 wave of the South Asia Barometer with support for MMA above the average (i.e., Khyber Pakhtunkhwa, Balochistan, Sindh). All regressions include as controls: province fixed effects, wave fix schooling, and type of locality indicators. In Panel B also include interactions for the 2013 wave and province fixed effects with an indicator for TV ownership.

Appendix Table 10: Effects on Trust Measures (Displaying Estimates of All Interaction Terms)

	Dependent variables. Trust in:									
	Civil Service	Police	The Courts	Parliament	Political Parties	Army	Central Government	Provincial Government	Local Government	z-score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean Dep. Var.	*0.462*	*0.533*	*0.494*	*0.473*	*0.584*	*0.504*	*0.534*	*0.497*	*0.578*	*0.000*
	Panel A. Effects on Trust									
Post July 2011	0.139***	0.449***	0.114***	0.152***	0.498***	-0.441***	0.065**	-0.048*	0.063**	0.223***
	(0.026)	(0.023)	(0.026)	(0.026)	(0.023)	(0.023)	(0.026)	(0.027)	(0.026)	(0.032)
Post July 2011 x (Isl Support > Average)	-0.076**	-0.135***	-0.063	-0.094**	-0.190***	0.144***	-0.052	0.012	0.089**	-0.081*
	(0.039)	(0.036)	(0.039)	(0.039)	(0.036)	(0.035)	(0.039)	(0.039)	(0.039)	(0.049)
Observations	3,252	3,252	3,252	3,252	3,252	3,252	3,252	3,252	3,252	3,252
R-squared	0.054	0.208	0.029	0.054	0.215	0.204	0.050	0.041	0.055	0.069
	Panel B. Effects on Trust by Ownership of TV									
Post july 2011	0.039	0.439***	0.073*	0.088**	0.434***	-0.498***	0.005	-0.142***	0.003	0.100**
	(0.038)	(0.032)	(0.039)	(0.038)	(0.035)	(0.030)	(0.038)	(0.038)	(0.037)	(0.046)
No TV	-0.146***	-0.004	-0.030	-0.095**	-0.102***	-0.084**	-0.085**	-0.130***	-0.104**	-0.174***
	(0.042)	(0.035)	(0.043)	(0.042)	(0.039)	(0.033)	(0.042)	(0.042)	(0.042)	(0.052)
Post July 2011 x No TV	0.189***	0.066	0.226***	0.111	0.084	0.110*	0.154**	0.237***	0.033	0.270***
	(0.067)	(0.060)	(0.065)	(0.068)	(0.061)	(0.061)	(0.067)	(0.069)	(0.067)	(0.079)
Post July 2011 x (Isl Support > Average)	0.040	-0.100**	-0.031	-0.030	-0.099**	0.158***	0.003	0.107**	0.153***	0.045
	(0.050)	(0.045)	(0.051)	(0.050)	(0.047)	(0.042)	(0.050)	(0.050)	(0.050)	(0.063)
Post July 2011 x (Isl Support > Average) x No TV	-0.271**	-0.218**	-0.120	-0.154	-0.305***	0.109	-0.236**	-0.268**	-0.084	-0.345**
	(0.108)	(0.098)	(0.103)	(0.106)	(0.096)	(0.096)	(0.105)	(0.107)	(0.103)	(0.136)
Observations	3,212	3,212	3,212	3,212	3,212	3,212	3,212	3,212	3,212	3,212
R-squared	0.054	0.209	0.034	0.056	0.222	0.215	0.052	0.045	0.058	0.071

Notes: Robust standard errors in parentheses. The unit of observation is the individual. The dependent variables are indicators for whether the respondent reported trusting "a great deal" or "quite a lot" the different organizations. In Panel A, the regressor of interest is the interaction of an indicator for the 2013 wave of the SouthAsia Barometer and an indicator for provinces with support for MMA above the average (i.e., Khyber Pakhtunkhwa, Balochistan, Sindh). All regressions include as controls: province fixed effects, wave fixed effects, age, gender, years of schooling, and type of locality indicators. In Panel B also include interactions for the 2013 wave and province fixed effects with an indicator for TV ownership. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 11: Effects on Supply of Health Services

	Dependent Variable:		
	Time travel to Health Facilities	Indicator: Any Immunzation Activity	Number of Targeted Children Per Capita
	(1)	(2)	(3)
Mean Dep. Var.	*1.520*	*0.601*	*0.136*
Post July 2011 × Islamist Support	-0.026	-0.010	-0.004
	(0.040)	(0.014)	(0.006)
Observations	16,781	8,208	8,136
R-squared	0.396	0.578	0.535
Number of Clusters	110	114	113

Notes: Standard errors clustered at the district-level in parentheses in columns. The unit of observation is the child-level in Columns 1. In Columns 2 and 3, the unit of observation is the district-month level. In Column 1, the sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. We exclude children that were partially treated. See the notes of Table 1 for details on the excluded cohorts. In Columns 2 and 3, the sample consists of all districts, observed at monthly frequency for the time period 2008 to 2013. All regressions include district and monthly time of interview fixed effects. The post indicator is defined based on the timing of the interview. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 12: Disentangling Demand Channels: Changes in Beliefs or Intimidation by the Taliban

	Dependent Variables: First Dose of			
	Polio	DPT	Measles	All Vaccines
	(1)	(2)	(3)	(4)
Mean Dep. Var.	*0.421*	*0.454*	*0.279*	*0.250*
	Panel A. Taliban Conflict Events in 2010			
Post × Islamist Support	-0.052**	-0.046**	-0.049***	-0.054***
	(0.020)	(0.018)	(0.017)	(0.016)
Post × Conflict Measure	-0.013	-0.017*	-0.009	-0.006
	(0.009)	(0.010)	(0.008)	(0.008)
Observations	16,758	16,758	12,557	12,557
R-squared	0.259	0.239	0.252	0.258
Number of Clusters	109	109	109	109
	Panel B. Taliban Conflict Events 2010 - 2013			
Post × Islamist Support	-0.051**	-0.045**	-0.051***	-0.057***
	(0.020)	(0.019)	(0.017)	(0.016)
Post × Conflict Measure	-0.014	-0.018	-0.004	-0.001
	(0.010)	(0.012)	(0.008)	(0.007)
Observations	16,758	16,758	12,557	12,557
R-squared	0.260	0.239	0.252	0.258
Number of Clusters	109	109	109	109

Notes: Standard errors clustered at the district-level in parentheses. The sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. We exclude partially treated children. See the notes of Table 1 for details on the excluded cohorts. In Panel A, the measure of conflict is the number of conflict events within a given district in the year 2010 for which one of the actors involved was identified as the Pakistani Taliban. In Panel B, the measure of conflict is the total number of conflict events within a given district in the time period 2010-2013 for which one of the actors involved was identified as the Pakistani Taliban. All regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. The dependent variables take value 1 if the first dose of each vaccine was received, 0 otherwise. The outcome for all vaccines combines all of these requirements. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 13: Robustness to Income and Geography

	Baseline	Household Income	Nightlights at Birth	Nightlights at Interview	Flood-Affected x Cohort FE	Drop District of Abottabad	Travel Distance to Health Facilities
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. First Dose of Polio Vaccine						
Post × Islamist Support	-0.058***	-0.058***	-0.053***	-0.055***	-0.052***	-0.056***	-0.058***
	(0.020)	(0.020)	(0.019)	(0.019)	(0.018)	(0.020)	(0.020)
Observations	16,788	16,788	16,788	16,788	16,788	16,634	16,781
R-squared	0.260	0.261	0.261	0.263	0.263	0.262	0.261
	Panel B. First Dose of DPT Vaccine						
Post × Islamist Support	-0.054***	-0.053***	-0.053***	-0.051***	-0.052***	-0.052***	-0.055***
	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)	(0.018)	(0.018)
Observations	16,788	16,788	16,788	16,788	16,788	16,634	16,781
R-squared	0.240	0.242	0.240	0.242	0.240	0.241	0.242
	Panel C. First Dose of Measles Vaccine						
Post × Islamist Support	-0.054***	-0.053***	-0.047***	-0.046***	-0.052***	-0.052***	-0.054***
	(0.016)	(0.016)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
Observations	12,577	12,577	12,577	12,577	12,577	12,465	12,570
R-squared	0.252	0.255	0.254	0.258	0.253	0.254	0.253
	Panel D. All Vaccines						
Post × Islamist Support	-0.057***	-0.057***	-0.047***	-0.048***	-0.053***	-0.056***	-0.058***
	(0.016)	(0.016)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)
Observations	12,577	12,577	12,577	12,577	12,577	12,465	12,570
R-squared	0.258	0.260	0.263	0.267	0.261	0.259	0.258

Notes: Standard errors clustered at the district-level in parentheses. There are 110 parent districts in the baseline sample. The unit of observation is the child level. All regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. Column 2 adds as controls dummies for the deciles of household income. Column 3 adds as control a district-level measure of nightlight luminosity in the year in which the child was born. Column 4 adds as control a district-level measure of nightlight luminosity in the year in which the interview took place. Column 5 adds as controls a dummy for whether the district was severely affected by floods in 2010 interacted with yearly cohort fixed effects. Column 6 drops the district where Abottabad is located. Column 7 adds controls for travel distance to basic health facilities.

Appendix Table 14: Robustness Checks Selective Migration

	Dependent Variables:			
	Polio	DPT	Measles	All Vaccines
	(1)	(2)	(3)	(4)
	Panel A. Controlling for In- and Out-migration Rates			
Mean Dep. Var.	*0.424*	*0.457*	*0.281*	*0.252*
Post × Islamist Support	-0.060***	-0.054***	-0.054***	-0.058***
	(0.021)	(0.019)	(0.017)	(0.017)
Observations	16,625	16,625	12,447	12,447
R-squared	0.260	0.239	0.254	0.260
Number of Clusters	105	105	105	105
	Panel B. Lower Bound (in Magnitude) if Most Unfavorable Selective Migration			
Mean Dep. Var.	*0.421*	*0.455*	*0.276*	*0.247*
Post × Islamist Support	-0.049**	-0.045**	-0.045**	-0.049***
	(0.021)	(0.019)	(0.017)	(0.017)
Non-parametric cluster bootstrap p-value	[0.004]	[0.004]	[0.004]	[0.002]
Wild bootstrap p-value	[0.024]	[0.022]	[0.012]	[0.012]
Observations	16,460	16,460	12,282	12,282
Number of Modified Observations	632	632	632	632
R-squared	0.259	0.237	0.252	0.258
Number of Clusters	105	105	105	105
	Panel C. Assigning Households to District of Origin (DHS sample)			
Mean Dep. Var.	*0.295*	*0.300*	*0.137*	*0.129*
Post × Islamist Support	-0.040**	-0.036**	-0.009	-0.016
	(0.017)	(0.018)	(0.015)	(0.016)
Observations	6,023	5,940	5,497	5,435
Number of Reassigned Observations	374	374	374	374
R-squared	0.187	0.183	0.164	0.153
Number of Clusters	113	113	113	113

Notes: Standard errors clustered at the district-level are shown in parentheses. The unit of observation is the child level. In Panels A and B, the sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. We exclude partially treated children. See the notes of Table 1 for details on the excluded cohorts. In Panel B, we modify a number of observations in a bounding exercise. In particular, while we drop 165 observations in districts with negative net outmigration rates (as calculated from DHS data), we add 467 observations in districts with positive net outmigration rates. In particular, we drop observations with a successful vaccination outcome if the level of support for Islamist groups is below the median level in the sample. In contrast, we drop observations with an unsuccessful vaccination outcomes in districts where the level of support for Islamist groups exceeds the median in the sample. When adding additional observations, we impute successful vaccination outcomes in districts where the level of support for Islamist groups exceeds the median in the sample. In contrast, we impute unsuccessful vaccination outcomes in districts, where the level of support for Islamist groups lies below the median in the sample. To account for the imputation procedure, we also report p-values derived using a non-parametric clustered and a Wild bootstrap approach. In Panel C, the sample consists of children born in the years 2004 to 2007 and 2010 to July 2012 that are less than 24 months of age at the time of interview. We exclude partially treated children (same as in Panels A and B). In Panels A and B, all regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. In Panel C, all regressions include district, monthly cohort, monthly age, and a dummy for rural regions. The dependent variables take value 1 if the first dose of each vaccine was received, 0 otherwise. The outcome for all vaccines combines all of these requirements.

Appendix Table 15: DHS Immunization Outcomes

	Dependent Variables:				
	Polio	DPT	Measles	HBV	All Vaccines
	(1)	(2)	(3)	(4)	(5)
	Panel A. 1st Dose of Each Vaccine				
Mean Dep. Var.	*0.295*	*0.300*	*0.137*	*0.284*	*0.129*
Post × Islamist Support	-0.038**	-0.033*	-0.008	-0.051**	-0.016
	(0.017)	(0.017)	(0.014)	(0.023)	(0.016)
Observations	6,023	5,940	5,497	5,876	5,435
R-squared	0.187	0.183	0.164	0.170	0.153
Number of Clusters	113	113	113	113	113
	Panel B. All Doses of Each Vaccine				
Mean Dep. Var.	*0.222*	*0.226*	*0.137*	*0.214*	*0.120*
Post × Islamist Support	-0.030*	-0.031*	-0.008	-0.042**	-0.018
	(0.016)	(0.016)	(0.014)	(0.020)	(0.015)
Observations	5,577	5,498	5,497	5,443	5,585
R-squared	0.177	0.177	0.164	0.165	0.150
Number of Clusters	113	113	113	113	113

Notes: Standard errors clustered at the district-level in parentheses. There are 113 districts in the sample. The unit of observation is the child level. The sample consists of children born in the years 2004 to 2007 and 2010 to 2013 that are less than 24 months of age at the time of interview. We exclude partially treated children. In particular, for both the first dose of Polio, DPT and HBV, we exclude children born between March and June 2011. In the case of Measles, we exclude children born between July 2010 and June 2011. For the receipt each dose of Polio, DPT as well as for all vaccines, we exclude children born between May 2010 and June 2011. All regressions include district, monthly cohort, monthly age, and a dummy for rural regions. The dependent variables in Panel A take value 1 if the first dose of each vaccine was received, 0 otherwise. The dependent variables in Panel B take value 1 if a child has received all doses of a given vaccine, 0 otherwise. In particular, for Polio, DPTand HBV, we require that the child has received the first 3 shots of vaccine and for measles we require that the child has received the first dose of measles vaccine. The outcome for all vaccines combines all of these requirements. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 16: Only Controlling for District and Cohort Fixed Effects

	Dependent Variables:			
	Polio	DPT	Measles	All Vaccines
	(1)	(2)	(3)	(4)
	Panel A. 1st Dose of Each Vaccine			
Mean Dep. Var.	*0.420*	*0.453*	*0.278*	*0.249*
Post × Islamist Support	-0.055***	-0.051***	-0.065***	-0.066***
	(0.020)	(0.019)	(0.018)	(0.018)
Observations	16,788	16,788	12,577	12,577
R-squared	0.249	0.226	0.226	0.235
Number of Clusters	110	110	110	110
	Panel B. All Doses of Each Vaccine			
Mean Dep. Var.	*0.381*	*0.418*	*0.278*	*0.263*
Post × Islamist Support	-0.060***	-0.058***	-0.065***	-0.061***
	(0.019)	(0.019)	(0.018)	(0.018)
Observations	11,294	11,294	12,577	11,294
R-squared	0.265	0.235	0.226	0.249
Number of Clusters	110	110	110	110

Notes: Standard errors clustered at the district-level in parentheses. The unit of observation is the child level. The sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. We exclude partially treated children. See the notes of Table 1 for details on the excluded cohorts. All regressions include district and monthly cohort fixed effects. The dependent variables in Panel A take value 1 if the first dose of each vaccine was received, 0 otherwise. The dependent variables in Panel B take value 1 if a child has received all doses of a given vaccine, 0 otherwise. The outcome for all vaccines takes value 1 if the child has obtained the corresponding dosage of the three vaccines.

Appendix Table 17: Non-Monotonicity of Treatment Effects

	Dependent Variables: Dummy for Receipt of 1 Vaccine Dose								
	Polio			DPT			Measles		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean Dep. Var.	*0.420*	*0.420*	*0.420*	*0.453*	*0.453*	*0.453*	*0.278*	*0.278*	*0.278*
Post × Islamist Support	-0.058***			-0.054***			-0.054***		
	(0.020)			(0.018)			(0.016)		
Post × 1(IslSup>P50)		-0.148***			-0.130***			-0.091***	
		(0.033)			(0.031)			(0.026)	
Post × Isl Support in 20th - 40th Percentile			0.021			0.042			-0.014
			(0.047)			(0.039)			(0.043)
Post × Isl Support in 40th - 60th Percentile			-0.028			-0.038			-0.085**
			(0.060)			(0.059)			(0.040)
Post × Isl Support in 60th - 80th Percentile			-0.163***			-0.136***			-0.123***
			(0.048)			(0.047)			(0.044)
Post × Isl Support in 80th - 100th Percentile			-0.134***			-0.118***			-0.128***
			(0.038)			(0.036)			(0.039)
Observations	16,788	16,788	16,788	16,788	16,788	16,788	12,577	12,577	12,577
R-squared	0.260	0.263	0.264	0.240	0.242	0.242	0.252	0.253	0.254

Notes: Standard errors clustered at the district-level in parentheses. The sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. We exclude partially treated children. See the notes of Table 1 for details on the excluded cohorts. All regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. The dependent variables in Panel A take value 1 if the first dose of each vaccine was received, 0 otherwise.

Appendix Table 18: Robustness of Heterogenous Effects to Main Controls

	Baseline	Mean of Dep Var Pre-Treatment x Cohort FE	Initial Health x Cohort FE	Initial Education x Cohort FE	Nightlights at Birth	Conley Standard. Errors (Cutoff: 250km)	Conflict Events in the First Year of Life	Conflict Events in 2010 x Cohort FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. First Dose of Polio Vaccine							
Post × Islamist Support	-0.045**	-0.046**	-0.041**	-0.031	-0.041**	-0.045	-0.045**	-0.045**
	(0.020)	(0.020)	(0.018)	(0.020)	(0.019)	(0.029)	(0.019)	(0.019)
Post × Islamist Support x Female	-0.028**	-0.028**	-0.026**	-0.027**	-0.027**	-0.028*	-0.027**	-0.028**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.013)	(0.013)
Observations	16,788	16,788	16,788	16,788	16,788	16,788	16,758	16,758
R-squared	0.260	0.261	0.263	0.261	0.262	0.260	0.260	0.261
	Panel B. First Dose of DPT Vaccine							
Post × Islamist Support	-0.039**	-0.041**	-0.041**	-0.041**	-0.038**	-0.039	-0.040**	-0.039**
	(0.018)	(0.019)	(0.016)	(0.019)	(0.018)	(0.024)	(0.018)	(0.018)
Post × Islamist Support x Female	-0.031**	-0.032**	-0.029*	-0.031**	-0.031**	-0.031*	-0.030*	-0.031**
	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)	(0.018)	(0.015)	(0.016)
Observations	16,788	16,788	16,788	16,788	16,788	16,788	16,758	16,758
R-squared	0.240	0.240	0.244	0.240	0.240	0.240	0.240	0.241
	Panel C. First Dose of Measles Vaccine							
Post × Islamist Support	-0.042**	-0.039**	-0.035**	-0.037**	-0.036**	-0.042***	-0.042**	-0.042**
	(0.017)	(0.017)	(0.016)	(0.018)	(0.016)	(0.016)	(0.017)	(0.017)
Post × Islamist Support x Female	-0.024	-0.028	-0.024	-0.025	-0.024	-0.024	-0.024	-0.024
	(0.018)	(0.019)	(0.018)	(0.018)	(0.017)	(0.015)	(0.018)	(0.018)
Observations	12,577	12,439	12,577	12,577	12,577	12,577	12,557	12,557
R-squared	0.253	0.254	0.256	0.254	0.254	0.253	0.252	0.253
	Panel D. All Vaccines							
Post × Islamist Support	-0.044***	-0.040**	-0.032**	-0.026	-0.034**	-0.044**	-0.044***	-0.043***
	(0.016)	(0.017)	(0.016)	(0.017)	(0.015)	(0.019)	(0.016)	(0.016)
Post × Islamist Support x Female	-0.029	-0.033*	-0.029	-0.030	-0.028	-0.029**	-0.029	-0.029
	(0.018)	(0.019)	(0.018)	(0.018)	(0.018)	(0.011)	(0.018)	(0.018)
Observations	12,577	12,439	12,577	12,577	12,577	12,577	12,557	12,557
R-squared	0.258	0.261	0.263	0.262	0.264	0.258	0.258	0.259

Notes: Standard errors clustered at the district-level in parentheses. There are 110 parent districts in the baseline sample. The unit of observation is the child level. All regressions include district, monthly cohort, monthly age, gender and calendar month of interview fixed effects and a dummy for rural regions. Column 2 adds as controls the mean of the dependent variable for the non-exposed cohorts interacted with yearly cohort fixed effects. Column 3 adds controls for district-level measures of access to health services as reported in the 2008/9 PSLM survey, respectively interacted with yearly cohort fixed effects. The health measures are the share of mothers that received pre-natal care, post-natal care, and tetanus vaccine during previous pregnancy. Column 4 adds controls for share of mothers that had no formal education in 2008/9 interacted with yearly cohort fixed effects. Column 5 adds as control a district-level measure of nightlight luminosity in the year in which the child was born. Column 6 re-estimates the baseline specification, but presents Conley standard errors for the most conservative distance cutoff within the range from 50 to 400 km. Column 7 adds as a time-varying control the number of conflict events in the first year of life (excluding protests and riots). Column 8 adds controls for the number of conflict events in 2010 interacted with yearly cohort fixed effects.

Appendix Table 19: Robustness of Heterogenous Effects to Supply Controls

	Baseline	Number of Immunization Campaigns		Number of Targeted Children per Capita in Immunization Campaigns	
		First 3 months of life	First year of life	First 3 months of life	First year of life
	(1)	(3)	(4)	(5)	(6)
	Panel A. 1st Dose of Polio Vaccine				
Post × Islamist Support	-0.045**	-0.047**	-0.047**	-0.047**	-0.049**
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Post × Islamist Support x Female	-0.028**	-0.028**	-0.028**	-0.028**	-0.028**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Observations	16,788	16,654	16,654	16,612	16,612
R-squared	0.260	0.263	0.263	0.261	0.261
	Panel B. 1st Dose of DPT Vaccine				
Post × Islamist Support	-0.039**	-0.041**	-0.041**	-0.041**	-0.044**
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Post × Islamist Support x Female	-0.031**	-0.032**	-0.032**	-0.032**	-0.032**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Observations	16,788	16,654	16,654	16,612	16,612
R-squared	0.240	0.242	0.242	0.240	0.241
	Panel C. 1st Dose of Measles Vaccine				
Post × Islamist Support	-0.042**	-0.043**	-0.043**	-0.043**	-0.046***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Post × Islamist Support x Female	-0.024	-0.024	-0.025	-0.024	-0.024
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Observations	12,577	12,479	12,479	12,437	12,437
R-squared	0.253	0.253	0.254	0.252	0.253
	Panel D. Full Immunization				
Post × Islamist Support	-0.044***	-0.044**	-0.044**	-0.044***	-0.048***
	(0.016)	(0.017)	(0.017)	(0.016)	(0.017)
Post × Islamist Support x Female	-0.029	-0.029	-0.030*	-0.029	-0.029
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Observations	12,577	12,479	12,479	12,437	12,437
R-squared	0.258	0.259	0.260	0.259	0.259

Notes: Standard errors clustered at the district-level in parentheses. The unit of observation is the child level. The sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. We exclude partially treated children. See the notes of Table 1 for details on the excluded cohorts. All regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. Columns 2 and 3 add controls for the number of polio vaccination campaigns conducted in the district of residence in the first three months of life and in the first year of life, respectively. Columns 4 and 5 add similar controls for number of targeted children during polio vaccination campaigns. The number of observations is slightly lower because of missing information on the number of targeted children for some periods. The dependent variable in Panels A, B and C take value 1 if the first dose of the respective vaccine (Polio, DPT, Measles) was received, 0 otherwise. The dependent variables in Panel D take value 1 if a child has received all doses of a given vaccine, 0 otherwise.

Appendix Table 20: Availability of Vaccination Cards. Averages over Time

Survey Wave	All districts	Quarter of Islamist Support			
		0 - 25th	25th - 50th	50th - 75th	75th -100th
2010/11	0.6847	0.7973	0.6886	0.5938	0.6337
2012/13	0.6851	0.8054	0.7168	0.5759	0.5965
Difference	0.0004	0.0081	0.0281	-0.0179	-0.0372

Notes: This table presents the share of children for which households report having the vaccination card.

Appendix Table 21: Availability of Vaccination Cards. Regression Analysis

	Dependent Variable: Dummy for Having a Vaccination Card	
	(1)	(2)
Mean Dep. Var.	*0.687*	*0.687*
Post July 2011 × Islamist Support	-0.007	-0.007
	(0.020)	(0.016)
Post July 2011	-0.013	
	(0.013)	
Islamist Support	-0.065**	
	(0.027)	
Observations	18,795	18,795
R-squared	0.013	0.207

Notes: Standard errors clustered at the district-level in parentheses. The sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. In Column 1, the regression includes the Post July 2011 dummy, the continuous measure of support for Islamist groups and the interaction of both of these variables. In column 2, the regression includes district, month-year time and monthly age fixed effects, as well as a dummy variable for rural areas. The dependent variable takes value 1 if the parents of a child indicate to have a vaccination card for the child.

Appendix Table 22: Robustness of Results on Health Seeking Behavior to Controlling to Taliban-Related Conflict Events

	Dependent Variables:											
	Heckman Selection Model											
	Dummy for Illness in Last 2 Weeks			Dummy for Consulted Anyone			Dummy for Consulted Formal Medical Sector			Labor Assisted by Traditional Birth Attendant		
	Baseline	Conflict in First Year of Life	Conflict in 2010 x Year FEs	Baseline	Conflict in First Year of Life	Conflict in 2010 x Year FEs	Baseline	Conflict in First Year of Life	Conflict in 2010 x Year FEs	Baseline	Conflict in First Year of Life	Conflict in 2010 x Cohort FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Mean Dep. Var.	*0.190*	*0.190*	*0.190*	*0.980*	*0.980*	*0.980*	*0.923*	*0.923*	*0.923*	*0.330*	*0.330*	*0.330*
Post July 2011 × Islamist Support	0.025*	0.026*	0.026*	-0.023*	-0.022*	-0.022*	-0.060**	-0.059**	-0.059**	0.038***	0.039***	0.039***
	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.027)	(0.027)	(0.027)	(0.013)	(0.013)	(0.013)
				[0.010]	[0.011]	[0.011]	[0.019]	[0.018]	[0.017]			
Inverse Mills Ratio				-0.052	-0.045	-0.047	-0.109**	-0.106**	-0.107**			
				(0.032)	(0.032)	(0.032)	(0.042)	(0.042)	(0.042)			
				[0.026]	[0.026]	[0.028]	[0.035]	[0.035]	[0.039]			
Observations	18,795	18,762	18,762	3,568	3,560	3,560	3,568	3,560	3,560	18,366	18,334	18,334
R-squared	0.064	0.064	0.064	0.077	0.076	0.077	0.152	0.150	0.151	0.138	0.138	0.138
Number of Clusters	110	109	109	109	108	108	109	108	108	110	109	109

Notes: Standard errors clustered at the district-level in parentheses. Standard errors derived from a bootstrap procedure with 1,000 bootstrap replications in square brackets. The unit of observation is the child level. In Columns 10 to 12, the sample is restricted to only the youngest child born to a mother in the sample. All regressions in Columns 1 to 9 include district fixed effects, quarter of interview fixed effects, monthly age fixed effects, and a dummy for rural regions. The regressions in Columns 10 to 12 include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. While Columns 1 to 9 rely on variation in the interview timing, Columns 10 to 12 exploit variation in the cohort dimension. The selection equation includes as an excluded instrument a proxy for the disease environment: the share of children that are sick in the same quarter and district (excluding a child's own illness status). The columns titled "Conflict in First Year of Life" include the district-level number of conflict events in a child's first year of life as an additional control. The columns titled "Conflict in 2010 x Year/Cohort FEs" introduces a time-invariant, district-level measure of conflict in 2010 interacted with the respective fixed effects as an additional control. The formal medical sector corresponds to hospital, basic health units and lady health workers.

Appendix Table 23: Expressing Support for Islamist Groups per change between 10th and 90th percentile.

	Dependent Variables:			
	Polio	DPT	Measles	All Vaccines
	(1)	(2)	(3)	(4)
	Panel A. 1st Dose of Each Vaccine			
Mean Dep. Var.	*0.420*	*0.453*	*0.278*	*0.249*
Post × Islamist Support	-0.116***	-0.108***	-0.108***	-0.115***
	(0.040)	(0.036)	(0.032)	(0.032)
Observations	16,788	16,788	12,577	12,577
R-squared	0.260	0.240	0.252	0.258
Number of Clusters	110	110	110	110
	Panel B. All Doses of Each Vaccine			
Mean Dep. Var.	*0.381*	*0.418*	*0.278*	*0.263*
Post × Islamist Support	-0.124***	-0.117***	-0.108***	-0.099***
	(0.037)	(0.037)	(0.032)	(0.031)
Observations	11,294	11,294	12,577	11,294
R-squared	0.275	0.245	0.252	0.271
Number of Clusters	110	110	110	110

Notes: Standard errors clustered at the district-level in parentheses. The unit of observation is the child level. The sample consists of children born between January 2010 and July 2012 that are less than 24 months of age at the time of interview. We exclude partially treated children: for the first dose of Polio and DPT, we exclude children born between March and June 2011; for first dose of measles and the first dose of all vaccines, we exclude children born between July 2010 and June 2011. In panel B (with the exception of the results for measles in column 3), we exclude children born between May 2010 and June 2011. All regressions include district, monthly cohort, monthly age, and calendar month of interview fixed effects and a dummy for rural regions. The dependent variables in Panel A take value 1 if the first dose of each vaccine was received, 0 otherwise. The dependent variables in Panel B take value 1 if a child has received all doses of a given vaccine, 0 otherwise. The outcome for *all vaccines* takes value 1 if the child has obtained the corresponding dosage of the three vaccines. Support for Islamist groups is re-scaled by the difference in support between the 90th and the 10th percentile of the district-level distribution of support for Islamist groups.