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Evidence from the US"

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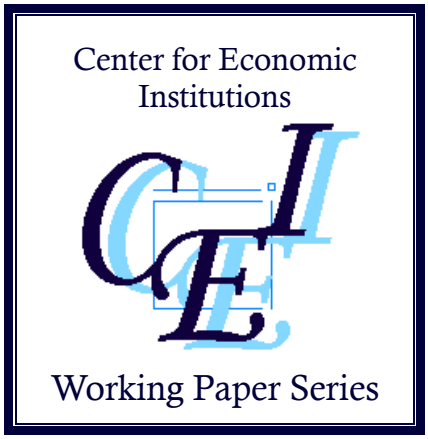

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The Impact of Vaccine Misinformation: Evidence from the US

Meradee Tangvatcharapong *

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Abstract

The increasing amount of misinformation, especially during the Covid-19 pandemic, has generated significant debate about the proper role of government and media platforms in combating it. However, little is known about whether and to what extent misinformation can actually change health behavior. This paper addresses this question by examining how parents responded to the unexpected surge in media coverage in 2007 of the verifiably false claim that the MMR vaccine caused autism. Specifically, I use a difference-in-differences approach to compare the vaccination rates of children whose parents were most and least likely to be affected by the news over time. Results indicate that susceptible parents were 3.3 percentage points less likely to vaccinate their children with an MMR shot by the recommended age of 15 months and 4.1 percentage points less likely to do so by 29 months.

Keywords: vaccination, immunization, misinformation, news

JEL: I12, I18

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1 Introduction

Recent advancements in technology have enabled information to travel faster and reach far more people than before. Unfortunately, this also means that it has become easy to spread misinformation and false stories. For example, Allcott and Gentzkow (2017) report that during the 2016 US presidential election cycle, fake news stories regarding presidential candidates were shared at least 37.6 million times on Facebook. Li et al. (2020) studied the top viewed videos about COVID-19 on YouTube and found that more than 25% of these top videos contained misleading information and have amassed more than 62 million views. The increasing amount of misinformation and its potential consequences has thus generated significant debate about the government's role and (social) media platforms' responsibility in combatting misinformation. However, little is known about the actual impact of misinformation on behavior (Lazer et al., 2018). The purpose of this paper is to ask whether the dissemination of verifiably false information leads to meaningful changes in behavior.

To do so, I study how parents responded to the sudden increase in media coverage of the verifiably false claim that the MMR (Measles-Mumps-Rubella) vaccine caused autism in the late 2000s. There are several reasons why the sudden increase in media coverage of the false claim about the MMR vaccine is the ideal setting in which to study the impact of misinformation. First, the claim that the MMR vaccine caused autism was completely made up and reported only in a now-retracted paper by Wakefield et al. (1998), and could be easily verified as false by 2007. Second, in contrast to some other misinformation, vaccine misinformation can cause vaccine hesitancy and affect important health outcomes at both individual and community levels. Third, the increase in media coverage of the false claim in 2007 was sudden and likely exogenous. This was because the increase in media coverage was largely due to 1. the timing of high-profile court hearings of the cases where parents alleged that the MMR vaccine caused autism and were suing for compensation from the federal government, and 2. high-profile celebrities, such as Jenny McCarthy, publicizing the false claim.

I begin my analysis by looking at the media coverage of the false claim to confirm that there

was indeed a sudden increase in media coverage. To do so, I use coverage on major television networks as a proxy for media coverage. I collect news transcripts from six major TV networks in the US (ABC, CBS, NBC, CNN, MSNBC, FNC) from 2001 to 2012 from Nexis Uni and show that the number of news stories reporting the false claim on these six networks jumped in 2007. Specifically, it increased from an average of 7.5 stories per year between 2001 and 2006 to 33 stories in 2007 and 79.5 stories in 2008.[1]

To identify the effects of this false claim, I exploit this shock in TV news coverage along with its differential impact on different types of parents. In particular, I expect this vaccine misinformation to have larger effects on parents who are ex-ante more likely to be sensitive and receptive to new information about vaccine and autism risks. I then identify the effects of the false claim by comparing immunization behaviors of parents who are ex-ante most and least sensitive to new information about vaccine and autism risks over time. I determine parents' sensitivity using three predetermined characteristics: whether the child was a firstborn, a boy, and the mother was over 30 years old. Parents are classified as most sensitive if they have all these three characteristics present and least sensitive if they have none. The identifying assumption behind this approach is that the least sensitive and most sensitive parents would have changed their vaccination behavior in the same way in the absence of the surge in misinformation about vaccines. While this approach will likely result in an underestimation of effects, given that all parents were likely somewhat exposed to the vaccine misinformation, it enables me to use a difference-in-differences approach to distinguish effects from other common time-varying factors and group-specific factors.

The immunization data used in this paper are individual-level data of 19-35-month-old American children from the 2002-2019 National Immunization Surveys (NIS). The NIS includes not only survey answers by parents but also the immunization records from healthcare providers for approximately 70 percent of the children in the surveys. Only children with immunization data from healthcare providers are included in the analysis to minimize measurement errors.

[1] As explained later in the Data section, a news story is counted as one 'false' story if both research assistants classified it as reporting on the false claim but not explicitly refuting it as false. And 0.5 'false' story if only one research assistant did so.

Because the Centers for Disease Control and Prevention (CDC) recommended that the MMR shot be given at 12-15 months old, I first look at whether the misinformation about the MMR vaccine caused parents to delay vaccinating their children past the recommended ages. I find that the misinformation caused sensitive parents to become 3.3 percentage points less likely to vaccinate their children with an MMR shot by 15 months old. To assess whether parents were only delaying the MMR shot or completely forgoing it, I examine the effects on take-up at 29 months old, the oldest age at which immunization data are consistently recorded in the survey. I estimate that the misinformation caused the MMR vaccine take-up at 29 months to drop by 4.1 percentage points. This result indicates that, at a minimum, the false claim caused parents to delay vaccinating their children by over a year and, at most, prevented them from ever immunizing their children.

These results are robust to including time-varying controls and group-specific trends and allowing for controls to have different effects each year. Importantly, I also test whether the results are dependent on how I define the treatment and control groups and find that the results are qualitatively similar when using more loosely defined treatment and control groups. Additionally, using the same method to look at the effects on other vaccine take-ups, results show that there is little evidence of spillover effects to other vaccines.

Indeed, the estimated reduction of 3 to 4 percentage points in the MMR vaccine take-ups, which is likely an underestimate given the approach, is economically meaningful. A 3.3 percentage point (4.2 percent) drop in the MMR vaccine take-up at 15 months is equivalent to an increase of 15 percent in unvaccinated 15-month-olds. And a decrease of 4.13 percentage points (4.4 percent) in the MMR vaccine take-up at 29 months translates to an increase of 59 percent in unvaccinated 29-month-olds.[2] Since measles is highly infectious, a drop of 3-4 percentage points in take-ups could easily result in the loss of herd immunity. For example, Lo and Hotez (2017) predicted that a similar-sized decline of 5 percent in the MMR vaccine coverage of children 2-11 years old in the US would result in a three-fold increase in annual measles outbreaks.

In providing evidence that vaccine misinformation led to meaningful changes in immunization

[2]Based on Table 2, an average of 21 percent of 15-month-olds and 8 percent of 29-month-olds were unvaccinated with an MMR shot.

behavior, this paper contributes to two bodies of literature. First, it complements the literature studying vaccine controversies. Smith, Ellenberg, Bell, and Rubin (2008), Anderberg, Chevalier, and Wadsworth (2011), and Chang (2018) studied the impact of when the claim linking the MMR vaccine to autism was first published in the Lancet in 1998. They found that the controversy resulted in a decrease in the MMR vaccine take-up rates. This study differs from the aforementioned studies in that this paper studies the effects of the claim after it had been clearly refuted to be false. In other words, this study identifies the effect of a verifiably false claim.

This paper also complements Carrieri, Madio, and Principe (2019) which studied the impact of misinformation about the MMR vaccines in Italy in 2012 and found a decrease in child immunization for all types of vaccines. The main difference between this paper and Carrieri, Madio, and Principe (2019) is in the nature of the events that triggered the surge in media coverage. The surge in the coverage of the misinformation about the MMR vaccine in Italy was due to a regional court officially recognizing a causal link between the MMR vaccine and autism. On the other hand, in my setting, the false claim was not endorsed by any government body or authority figure. Rather the increase in media coverage of this false claim in the US was mainly driven by celebrities publicizing the false claim and vaccine court hearings. Despite the hearings, it is important to note that the US court never officially endorsed this false claim and eventually ruled against it in 2009 and 2010. Therefore, the main difference between the two papers is that the misinformation in Carrieri, Madio, and Principe (2019) was endorsed by an authority figure and could be deemed reliable whereas the misinformation in my paper was not. All in all, combined with these previous findings, my paper shows that misinformation about vaccines reported by the media can affect people's decisions as much as or even more than perceived reliable information.

Second, this paper complements the literature on misinformation and fake news. Consistent with the theoretical framework of fake news provided by Allcott and Gentzkow (2017), results here suggest that misinformation does change people's behavior, at least in the context of immunization. Furthermore, the results indicate that the general population does not easily detect

misinformation, even when it is easily verifiable. This is in line with the finding that consumers do not accurately determine the reliability of health content on the internet, documented in Allam, Schulz, and Nakamoto (2014), Knapp, Madden, Wang, Sloyer, and Shenkman (2011), and Kutner, Greenburg, Jin, and Paulsen (2006).

2 Background: Media Coverage of the Anti-Vaccination Claim in the US

Although vaccines are regarded as one of the most successful medical interventions of the 20th century (CDC, 1999), some opposition to vaccines has always existed (Hussain et al., 2018). In 1998, however, the claim that vaccines are dangerous was propelled into the mainstream when an article by Wakefield et al. (1998) suggested a causal link between the MMR vaccine and autism. The article was published in the Lancet, a highly-regarded British medical journal. Anderberg, Chevalier, and Wadsworth (2011) studied the effects of this 1998 vaccine controversy and found that the MMR vaccine take-up rate declined sharply in the immediate years following the controversy. While the controversy did not garner as much media attention in the US as in the UK, Smith, Ellenberg, Bell, and Rubin (2008) and Chang (2018) also observed that the MMR vaccine take-up rates in children 19-35 months old in the US dropped by approximately 1-2 percentage points immediately following the Wakefield publication, but returned to pre-controversy levels by 2003. Importantly, the Wakefield et al. article was eventually retracted by the Lancet in 2010. While this retraction process took some time, I note that 10 out of 12 coauthors of the paper retracted the paper in 2004 and issued a statement stating that they no longer interpreted the results of their study as suggesting a causal link between the MMR vaccine and autism.

In the US, the topic of vaccine safety gained popularity again in 2007 when the media coverage of vaccine safety increased dramatically. This rise in the coverage was due in part to several vaccine court hearings of a case alleging that vaccines cause autism, and in part to the increasing number

of celebrities publicly claiming that vaccines cause autism.[3] Notably, Jenny McCarthy, an actress and TV host, famously went on talk shows, including the Oprah Winfrey Show, to talk about her belief that the MMR vaccine causes autism and how her son was diagnosed with autism after the MMR shot. Mnookin (2011) estimated McCarthy's message to have reached at least 15-20 million viewers based on her appearance on The Oprah Winfrey Show, Larry King Live, and Good Morning America alone.

Figure 1 shows the number of news coverages of the false claim that vaccines cause autism on six major television networks (ABC, CBS, NBC, CNN, MSNBC, and FNC) from 2001 to 2012. As stated earlier, coverage was few and far between 2001 and 2006 before rising dramatically in 2007.

A critical aspect of the surge in media coverage on vaccine safety in 2007 is that, at that point, prominent medical bodies had already refuted the claim of any link between vaccines and autism. This includes the Institute of Medicine (IOM) in May 2004, the Food and Drug Administration (FDA) in September 2006, and the Centers for Disease Control and Prevention (CDC) in July 2007. However, Figure 1 still shows an increase in the number of news stories reporting on the false claim without explicitly refuting it as false in 2007. This means that although the alleged link between vaccines and autism had been thoroughly debunked by that time, the public was exposed to a dramatic increase in misinformation alleging the link between vaccines and autism in 2007. I leverage this unanticipated increase in misinformation to estimate the causal impact of misinformation.

3 Data

To examine the volume of misinformation about vaccine safety over time, I look at the number of television news stories reporting on the alleged link between vaccines and autism. I use coverage on major television networks as a proxy for media coverage because although many people also access news through other sources, 44% of Americans still report television as their

[3] Despite the hearings, the US court never officially endorsed the false claim and ruled against it in 2009 and 2010.

most preferred platform for news consumption(Mitchell, 2018). I obtained the news transcripts of six major television networks in the US from January 2001 to December 2012 from Nexis Uni. The six networks were ABC, CBS, NBC, CNN, MSNBC, and FNC. To quantify the volume of misinformation, I look at the number of 'false claim only' stories, i.e., the news stories that only reported on the false claim without also reporting that the claim has been debunked by major scientific and medical bodies. I do so by first identifying new stories that mentioned vaccines (or vaccination) and autism in the same section. I then hired two research assistants to read these news transcripts and classify them into two categories: 1.) the stories that only reported on the false claim about vaccine safety without refuting it and 2.) the stories that reported on the false claim but also made it clear that the claim had been refuted as false by major medical and scientific bodies. I classify a new story as a 'false claim only' story if both research assistants flagged the story as reporting on the alleged link between vaccines and autism without explicitly refuting the claim. If only one research assistant did so, I classify the story as 0.5 'false claim only' story.[4] [5] Table 1 reports the number of 'false claim only' stories over time and matches the visual representation in Figure 1. The number of 'false claim stories' about vaccine safety rose dramatically from an average of 7.5 stories per year between 2001 and 2006 to 33 stories in 2007 and then 79.5 stories in 2008. To be clear, I do not claim that the number of 'false claim only' stories here covers all the news stories that reported the false claim without refuting it in each year. The real volume of misinformation could be higher as it could also be reported on television channels other than the six channels that I collected the data from or on other platforms such as the internet. I use the number from the six big television networks here as a proxy to show how

[4]The research assistants were instructed to sort and read the news transcripts in random order, rather than chronologically. This is to avoid any bias that could occur if they associate a certain time period with news of certain types.

[5]Additionally, Figure A.1 in the Appendix shows that the treatment of misinformation exposure is robust to the research assistants. In my main analysis, as explained in the Data Section, I count a story as 1 'false claim only' story if both RAs flagged the story as reporting on the false claim but not explicitly refuting it and 0.5 'false claim only' story if only one of the two RAs did so. I can change how I define one 'false claim only' story. For example, I can change it so that a story is counted as 1 'false claim only' story when either of the RAs flagged the story and 0 'false claim only' story when neither did. I can also only count a news story as 1 'false claim only' story when both of the RAs flagged the story and 0 'false claim only' story otherwise. The plots in Figure A.1 showed that regardless of how I define one 'false claim only' story, the trends are the same. The number of 'false claim only' stories surged in 2007.

the misinformation about vaccine safety was covered over the years. Based on this data, the numbers show that the coverage of the false claim abruptly increased in 2007 and 2008.

To identify the impact of misinformation about vaccines on individual behavior, I look at parents' decisions regarding vaccination. In particular, since the MMR vaccine is the vaccine at the center of the vaccine-autism claim, I look at the MMR vaccine take-up rate as my main outcome. Individual-level data on vaccination decisions used in this paper comes from the 2002-2019 National Immunization Survey (NIS), which is conducted yearly by the Centers for Disease Control and Prevention (CDC). For each survey, the CDC surveys parents of 19-35-month-old children about their children's vaccination history. In addition, the CDC also asks for consent to obtain vaccination records from their medical providers. Approximately 70% of the parents consent to the CDC acquiring vaccination records from their healthcare providers. Since healthcare provider records offer much more accurate information than parents' memory or a shot card, I only include children whose provider data is available in my analysis. For the analysis in this paper, I only include the data starting from 2002 to avoid the confounding effects from the first MMR vaccine controversy in 1998, when the Wakefield et al. paper was first published. My main analysis includes the data up until 2012 because I only have media data up until 2012, but I also estimate the effects for the period after 2012 and show them alongside the main estimates as well.

The National Immunization Surveys classify children into three age groups: 19-23-month-olds, 24-29-month-olds, and 30-35-month-olds. I use the vaccination information of children from all age groups, i.e., all 19-35 month-olds whose provider data is available, to look at the MMR vaccine take-up rate at 15 months old. Since the CDC recommends that the first MMR shot is given to a child at 12-15 months old, looking at the MMR vaccine take-up rate at 15 months old allows me to see if parents follow the CDC's recommendation. In addition, it is also important to see if parents only delay vaccinating their children or decline to vaccinate altogether. To address this question, I examine the MMR vaccine take-up rate of older children. The oldest children in my data set are 30-35 months old. This means that I have complete vaccination information up to when these children

were 29 months old. I thus use the vaccination information of children 30-35 months old to look at the MMR vaccine take-up rate at 29 months old to see if parents have caught up to the vaccination schedule.

Table 2 provides summary statistics of the children included in my analysis. Panel 1 reports on all children in the 2002-2019 National Immunization Surveys whose provider data is available, i.e., all 19-35-month-olds, while Panel 2 reports the statistics of only 30-35-month-old children. Overall, 79% of children are vaccinated with an MMR shot by 15 months old, and 92% are vaccinated by 29 months old. This suggests that at least approximately 13% of parents do not strictly follow the CDC's recommendation, but eventually vaccinate their children. In addition, the vaccination rates at both ages are, in general, higher among the children most likely to be affected by misinformation about vaccines (boy&firstborn&mother$\geq$30) than those least likely to be affected (girl&not firstborn&mother<30).

4 Empirical Method

4.1 Measuring Misinformation Exposure and Identifying the Post Period

I begin my analysis by identifying first which cohorts of children were affected by the sudden increase in 'false claim only' news stories. I do so by looking at the number of 'false claim only' stories to which parents are exposed. I first define the period when parents are most likely to pay attention to information about vaccine recommendations and vaccine safety as the 'exposure period'. For each child, I consider the exposure period to start in the month that the child was born and end in the month that I measure the child's MMR vaccine take-up. If I had information on each child's birthdate, I would identify each child's exposure period and then count the number of 'false claim only' stories reported on television in this exposure period and use this number as a measure of parents' *misinformation exposure*. However, although the National Immunization Survey (NIS) data is rich in many ways, it does not provide information on the date of birth, the date of the interview, or age at the time of the interview. Therefore, I cannot directly back out the

birth month and calculate parents' misinformation exposure for each child in my dataset individually. The NIS data does, however, provide information on which age group the child falls into at the time of the interview (19-23, 24-29, 30-35 months old). I thus calculate the average misinformation exposure for children in each age group in each interview year using this age group information along with two hypotheses. First, I assume that children of all ages are as equally likely to appear in the survey. Second, I assume that the probability of getting interviewed in each month is uniformly distributed throughout the year.

Figures 2 and 3 show the average misinformation exposure of parents interviewed in each survey year. Figure 2 shows the misinformation exposure up until when the child was 15 months old. Since the information that the parents received after the child turned 15 months old could not affect the vaccine take-up at 15 months old, I focus on only the exposure up until 15 months for the analysis of the MMR vaccine take-up at 15 months old. Panel A shows that for parents whose child was 19-23 months old at the time of the interview, the first cohort that experienced the increase in misinformation was those interviewed in 2008. Panels B and C show the misinformation exposure of parents whose child was 24-29 months old and 30-35 months old at the time of the interview, respectively. Both panels show that for both groups of parents, the first cohort that experienced the increase in misinformation was the one interviewed in 2009. Therefore, for the analysis of the MMR vaccine take-up at 15 months old, the post-period starts with the 2008 cohort for parents of 19-23 months olds and the 2009 cohort for parents of 24-35 months olds. Figure 3 shows the misinformation exposure up until when the child was 29 months old. I only look at the misinformation exposure for parents whose child was 30-35 months old at the time of the interview here because they are the only group with relevant information on children at 29 months old. We can see that for this group of parents, the misinformation exposure rose dramatically for the cohort interviewed in 2008. Therefore, for the analysis of the MMR vaccine take-up at 29 months old, which only includes parents of 30-35 months old, the post-period starts with the 2008 cohort. Again, to be clear, the *misinformation exposure* number here comes from the number of 'false claim only' stories that were reported by the six major

television networks during the parents' exposure period. I do not claim that the *misinformation exposure* number here shows the total exposure of parents to misinformation as parents could have been exposed to misinformation from other media platforms or television networks too. I use this number as a proxy to show how parents' exposure to misinformation about vaccine safety changed over the years.

4.2 Classifying Treatment and Control Groups

To identify the effects of misinformation, we would ideally compare a group that was randomly exposed to misinformation to a group that was not exposed to misinformation. However, this is difficult for several reasons. First, people usually choose what they watch on television. For example, it could be the case that people who are less likely to vaccinate are the ones more likely to watch reports of the false claim about vaccines on television. Second, since more than 95% of US homes have television service (EIA, 2005), it is likely that almost everyone was exposed to the misinformation about vaccines that was reported on television to some degree. This makes it hard to identify a control group. In this paper, I overcome these issues by using a difference-in-differences approach that compares the groups that are *ex-ante* most and least sensitive to misinformation about vaccines over time. Using this approach, the least sensitive group serves as the control group. The advantage of this approach is that I am able to distinguish the effect of misinformation exposure from other common time-varying factors, as well as group-specific factors. The disadvantage is because all parents are, to some extent, treated, this approach will underestimate the effect of misinformation on immunization behavior.

To identify which group of parents is the most sensitive and which group is the least sensitive to the misinformation about vaccines, it is important to consider which factors would make some parents more sensitive to the misinformation than others. Here, I propose that parents' sensitivity to misinformation about vaccines is based on both their parenting experience and their child's risk of being on the autism spectrum. There are two major reasons why misinformation about vaccines should be less impactful on experienced parents. First, because experienced parents would have

started paying attention to information about vaccines earlier than first-time parents, the news stories about the false claim after 2007 would account for a smaller percentage of information for experienced parents. Therefore, the misinformation about vaccines, which increased dramatically in 2007, should be less impactful to experienced parents than first-time parents. Second, experienced parents are also more likely to have already formed their opinion on the issue from past experience and, therefore, less likely to be receptive to the new information than new parents. Therefore, among parents of same-age children in the data, experienced parents would likely be less sensitive to new information and thereby less affected by the increase in misinformation about vaccines.

Next, since the false claim links vaccines to autism risk, parents whose children are at higher risk of being on the autism spectrum would likely be more sensitive to the false claim. In terms of autism risk, two characteristics—parental age and gender—have been consistently reported by both the CDC and media outlets to be associated with higher autism risk. For example, the CDC reported in February of 2007 that autism spectrum disorder is 3-5 times more common among boys than girls (CDC, 2007). Similarly, several news networks reported on a study by Reichenberg et al. (2006) that found that children of men over 40 years old were 5.75 times more likely to have autism spectrum disorder compared with children of men under 30 years old.[6] A large study by Durkin et al. (2008) also found that firstborn children of two older parents were three times more likely to develop autism than third- or later-born offspring of 20-34 years old mothers and fathers under 40 years old.

I, therefore, determine parents' sensitivity to the news using three predetermined characteristics: whether the child is a firstborn, a boy, and the mother is over 30 years old. Mother's age is used as a proxy for parental age as it is the only consistent information about parental age available from the survey and the majority of couples are not more than 5 years apart in age.[7] Parents are classified

[6]McNamara, M. (2006) 'Men's Biological Clocks Are Ticking, Too', CBS, 15 November (https://www.cbsnews.com/news/mens-biological-clocks-are-ticking-too/)
Robin, R. (2007) 'It Seems the Fertility Clock Ticks for Men, Too', The New York Times, 27 Feb (https://www.nytimes.com/2007/02/27/health/27sper.html)

[7]Based on the 2013 Current Population Survey, for 76.7% of heterosexual married couples, the husband and wife are less than 5 years apart in age.

as most sensitive to the false claim if they have all three characteristics present and least sensitive if they have none. As a result, within my sample, I define the group that is the most sensitive to the misinformation about vaccines as boys who are firstborn and whose mother is over 30 years old, and the group that is the least sensitive as girls who are not a firstborn and whose mother is younger than 30 years old.

Using these treatment and control groups, I implement a generalized difference-in-differences approach to identify the impact of misinformation about vaccines. Specifically, I compare the MMR vaccine take-up rate of boys who are firstborn and whose mothers are over 30 years old to the take-up rate of girls who are not firstborn and whose mothers are younger than 30 years old before and after the surge in misinformation. Formally, I estimate the impact of the increase in misinformation on parents' decision to vaccinate their children using the following model:

$$MMR_{it} = \alpha + \gamma_t + \theta MostSensitive_i + \beta_x X_{it} + \beta MostSensitiveXPost_{it} + u_{it} \quad (1)$$

Where the outcome, MMR_{it}, is a binary variable indicating whether child i whose parent was interviewed in year t has been given at least one shot of MMR vaccine. In this paper, I focus on looking at this outcome at two points in time: when child i was 15 months old and 29 months old. I look at whether child i has been given any MMR shot at 15 months old because the CDC recommends that parents vaccinate their children with a dose of MMR vaccine at 12-15 months old, and therefore this will show whether parents stop following the CDC's recommendation. Additionally, it is also important to assess whether misinformation has long-run effects on vaccination take-up, or if it only delays it. This is why I test for effects on children in the oldest age group in my sample, who are 30-35 months old at the time of the interview, to look at MMR vaccine take-up rate at 29 months old.

γ_t is survey year fixed effects. $MostSensitive_i$ is an indicator variable for whether child i's parents are classified as the most sensitive, i.e. whether child i is a boy, a firstborn, and has a mother who is over 30 years old. X_i is a matrix containing child i's characteristics including state fixed effects,

race, poverty status, mother's education, mother's marital status, child's age group at the time of the interview, whether they live in the state they were born in, and whether their state allows personal belief exemption from vaccination. $MostSensitiveXPost_{it}$ is an indicator variable for whether child i is in the most sensitive group in the post period. The post-period starts in the year when we first see the dramatic increase in misinformation exposure, i.e. 'false claim only' news exposure, as discussed in the earlier subsection.[8] Importantly, the coefficient of interest here is β which measures the effects of misinformation on parents' decision to vaccinate. Specifically, it measures whether parents most sensitive to the surge in misinformation vaccinate their children differently than parents who are the least sensitive.

In all specifications, survey weights are used, and cluster-robust standard errors and their corresponding p-values are reported. I follow Donald and Lang (2007) and cluster at the year level. In addition, following Cameron, Gelbach, and Miller (2008), I also use a wild bootstrap method that clusters at the year level and report these p-values as well. Lastly, I perform a randomization inference exercise. Specifically, I randomly reassign the child's gender, mother's age, and firstborn status based on the true distribution of each variable in each year and then estimate the effect (β) based on the reassignment. I do this for 10,000 replications and plot the distribution of the 10,000 coefficients estimated. I then compute the proportion of these 10,000 coefficients that have a larger absolute value than the actual estimate and interpret this number as the two-tailed empirical p-value.

As with any difference-in-differences design, the underlying assumption for this approach is that MMR vaccine take-up rates of children in the control group and treatment group would have changed similarly over time in the absence of the increase in misinformation. I provide support for this assumption by first showing the visual representation of the raw data that shows the MMR

[8] When the outcome is MMR vaccine take-up at 15 months old, I base the treatment period on parents' exposure to misinformation up until when the child was 15 months old. The post-period starts with the first cohort that saw a dramatic increase in misinformation exposure. Therefore, the post-period starts in 2008 for parents of 19-23 months old and 2009 for parents of 24-35 months old. Likewise, when the outcome is MMR vaccine take-up at 29 months old, I base the treatment period on the parent's exposure to misinformation up until the child is 29 months old. As mentioned in the earlier subsection, the analysis for the MMR vaccine take-up at 29 months old only includes parents of 30-35 months old, and the post-period for this analysis starts in 2008.

vaccine take-up rates for control and treatment groups tracking each other prior to the post-period. Second, I formally test for the divergence in outcomes between the treatment and control groups in the pre-period using a dynamic difference-in-differences approach.

One potential concern with this approach is that perhaps results would differ for alternative definitions of treatment and control groups. To provide further support for my identification strategy, I also perform multiple analyses using more loosely defined treatment and control groups. Specifically, I do this in three different ways. First, I include more children in my control group. Namely, instead of excluding children who have one or two predetermined characteristics[9], I include them in my control group. Second, I include more children in my treatment group, i.e. instead of excluding children who have one or two predetermined characteristics, I include them in my treatment group. Lastly, I use two instead of three predetermined characteristics to determine treatment and control groups. With more loosely defined treatment and control groups, we would expect the effects to be weaker, but of the same sign.

Additionally, another potential concern is that exposure to misinformation might have caused some parents to become less (or more) likely to allow the CDC to obtain their official vaccination records from their healthcare providers. If this is the case, the estimate might simply just reflect the change in the consent rates and not parents changing their vaccination behavior. For example, a lower consent rate from parents who did not vaccinate their children would result in a lower number of unvaccinated children being included in the data, even when the parents did not change their vaccination behavior. This, in turn, would affect the vaccination rates of the treatment and control groups and then result in treatment effects, even when there is no actual change in the vaccination behavior. To provide supporting evidence that this is likely not the case, I look at the consent rates of the treatment and control groups over time. Figure A.2, Figure A.3 and Table A.1 in the Appendix indicate that there is no significant effect of misinformation exposure on the consent rate.

[9] In the main specification, the control group is children with zero of the three predetermined characteristics, and the treatment group is children with all three predetermined characteristics. The three predetermined characteristics are: whether the child is a firstborn, a boy, and the mother is over 30 years old.

5 Results

5.1 Main Results: MMR Vaccine

In this section, I look at the MMR take-up rates at 15 and 29 months old. Figures 4 and 5 plot the average take-up rates at 15 and 29 months old, respectively, for the treated and control groups over the years. Both figures show that before treatment, i.e., the increases in TV coverage of the false claim, take-ups among the treatment and control groups tracked each other well. This is important since the validity of a difference-in-differences approach hinges on the parallel trend assumption. Take-ups were higher among children in the treatment groups, but the gap between the two groups shrunk in the immediate years following treatment. Especially, Figure 5 shows that the gap in take-ups at 29 months old between the treatment and control groups closed completely in the four years following treatment, before gradually returning to the levels before treatment.

To assess the parallel trend assumption more rigorously, I estimate a dynamic difference-in-differences model, controlling for year fixed effects, group fixed effects, and observable characteristics to check if the treatment group diverged from the control group in any year before treatment. Figures 6 and 7 plot the dynamic difference-in-differences estimates for the MMR vaccine take-ups at 15 and 29 months old, respectively. Both figures reaffirm that there is little evidence of divergence in trends before the increase in TV coverage of the false claim for both outcomes.

Next, I formally estimate the average treatment effect with a difference-in-differences approach explained in the prior section and report the estimates in Tables 3 and 4. In both tables, Columns 1-4 report the estimates when the sample is restricted to survey years 2002-2012, the period in which I have detailed information on TV coverage. Columns 5-8 report the estimates using the sample up to the survey year 2019. For each set of the sample, successive columns include additional controls. Columns 2 and 6 show the estimates from the preferred specification that includes controls of observable characteristics, state fixed effects, and the state's personal-belief exemption law, established in Equation 1. The estimates from Column 2 of these

two tables indicate that the increase in coverage of misinformation about the MMR vaccine resulted in a drop of 3.3 percentage points in the MMR take-ups at 15 months old and a drop of 4.1 percentage points in the take-ups at 29 months old. The estimates in Column 6 also indicate a decline of similar magnitude in the same period up to 2012. Additionally, the estimates in Column 6 also suggest that the effects fade away in the later years. The treatment effects from 2013 to 2019 are not statistically significant from zero for either the take-ups at 15 or 29 months old.

Columns 3 and 7 allow controls to affect the take-up rates differently in each year by interacting each control with year dummies. This is because the children in the control and treatment groups are different in some characteristics, notably family income and mother's education, as shown in table 2, and these characteristics may respond differently to year-to-year shock. For example, more affluent parents might have better vaccine access in the year when there is a vaccine shortage. The estimates reported in Columns 3 and 7 show that the estimates are robust to the inclusion of these characteristic-by-year controls. While the estimate for the MMR vaccine take-ups at 15 months old dropped slightly to -2.3 percentage points, the estimates for the take-ups at 29 months old remain steady at -4.1 percentage points.

Columns 4 and 8 add group-specific time trends to the preferred specification. This allows the treatment and control groups to trend in different directions. Doing so lets us see whether the estimates are driven by pre-existing trends. Importantly, if the media coverage of the false claim about the MMR vaccine had been smoothly increasing since before the 2007-2008 period and there had been no spike in treatment, we would expect the group-specific time trends to account for most, if not all, of the effects. Looking at the results, we can see that for both the take-ups at 15 and 29 months old, although the estimates in Column 4 lose their significance, their magnitude remains similar to the magnitude estimated in Column 2. The estimates in Column 8, which include data from the later survey years, also remain statistically significant and similar in magnitude to the estimates from the preferred specification in Column 6 for both outcomes.

Finally, I also compute randomization inference p-values for the estimates from the preferred specification. The randomization inference p-values for both the effects at 15 and 29 months old

also tell the same story. Both estimates are significant at the conventional level. Specifically, the randomization inference p-values are 0.012 and 0.003 for the take-up rates at 15 and 29 months old, respectively. The distributions of the coefficients from this randomization exercise are shown in Figure 8.

Overall, the results here suggest that the increased media coverage of the false claim about the MMR vaccine caused both the MMR take-up rates at 15 and 29 months old to drop by 3-4 percentage points. This indicates that, at a minimum, misinformation about the MMR vaccine caused parents to delay vaccinating their children by over a year and, at most prevented them from ever immunizing their children.

5.2 Other Vaccines

To further our understanding, I next investigate the impact of the MMR vaccine misinformation on take-ups of other recommended childhood vaccines. Specifically, I look at the effects on the take-ups of the following vaccines by 29 months old: Varicella (VRC), Hepatitis B (HepB), Diphtheria and Tetanus (DT), Haemophilus Influenzae type B (HIB), and Inactivated Poliovirus (Polio). The recommended schedule for the first shot of the VRC vaccine is from 12 through 15 months old, the same as the recommended schedule for the MMR vaccine. HepB, DT, and Polio vaccines are all recommended at much earlier ages. [10] Table 5 reports the estimates from the preferred specification using the sample from 2002 to 2012, with the take-ups of the aforementioned vaccine at 29 months old as the dependent variable. Results here indicate no significant effects on the take-ups of any vaccine except for Varicella. The estimate in Column 2 suggests that vaccine misinformation decreased take-ups of the Varicella vaccine by 4.4 percentage points, a comparable level to the drop in the take-ups of the MMR vaccine. However, Figure 9 and Table 6 show that the parallel trend assumption does not hold for the VRC vaccine take-ups and that estimated effects were driven by the divergence in pre-existing trends. Therefore, all these results seem to suggest that the misinformation about the MMR vaccine only

[10] The recommended immunization schedule is presented in Figure A.4 in the Appendix

decreased the MMR vaccine's take-ups and not other recommended vaccines. Notably, although the Varicella vaccine is recommended to be administered during the same period as the MMR vaccine, its take-ups do not seem to have suffered spillover effects.

5.3 Effects on the MMR Vaccine Take-Ups at Other Ages

Next, I also look at the effects of misinformation on the MMR vaccine take-up rates at other ages besides 15 and 29 months old. I estimate the average treatment effects on the take-up rate at each age from 15-29 months old using the preferred specification.[11] Table 7 shows that the estimates are relatively similar across ages. They are all negative and range from -1.3 to -4.6 percentage points, with 80% of them being statistically different from zero at the 10% level. This indicates that the negative effects of misinformation observed in the earlier section are not driven by the selection of the 15- and 29-month ages.

5.4 Robustness Check

Using More Loosely Defined Treatment Group and Control Group

In the main analysis, I compare children who are most and least likely to be affected by the treatment. I classify children into these two groups using three characteristics that are associated with susceptible parents: whether the child is a firstborn, the child is a boy, and the mother is over 30 years old. Children with all of these three characteristics present are classified as most likely to be affected, whereas children with none of these characteristics are classified as least likely to be affected. These two groups are then used as my treatment and control groups. In this section, I perform multiple analyses using more loosely defined treatment and control groups to test the robustness of my findings to alternative classifications. As explained in the earlier section, I redefine my control and treatment groups in three major ways: 1. expanding the definition of the control group, 2. Expanding the definition of the treatment group, and 3. defining treatment and

[11] As the age at which I measure the MMR vaccine take-up changes the exposure period, I re-examine the exposure period, the exposure to misinformation, and revise the first post year for each estimation.

control groups using only two characteristics. Using the more loosely defined treatment and control groups, we would expect to see the treatment effects become smaller in magnitude, but not completely disappear.

The results of this exercise for the MMR vaccine take-up rate at 15 months old are reported in Table 8 and the same results for the MMR vaccine take-up at 29 months old are reported in Table 9. Column 1 shows the results of the main identification strategy. Columns 2-3 show the estimates when I add more children into my control groups by including children with only one or two of the three characteristics associated with susceptible parents in the control group as well. Columns 4-5 show the estimates when I increase my treatment group by including children with only one or two of the three characteristics associated with susceptible parents in my treatment group. Columns 6-8 show the estimates when I only use two characteristics in defining my control and treatment groups. For any two characteristics I use, my treatment group is the children with both characteristics present, and the control group is the children with neither of the two characteristics present. All the estimates reported are, as expected, smaller in magnitude than the estimates from the main identification. Although some estimates are no longer significant at conventional levels, all of them are still negative, and all but one of them still report a relatively low p-value. In particular, the estimates for the MMR vaccine take-up at 29 months old are very robust to alternative definitions of treatment and control groups.

Specification Check

In addition, since the dependent variable is binary, I also use logistic regression to estimate my main results. The results are shown in Tables A.2 and A.3 in the Appendix. Similar to the linear regression results, the logistic regression results show reductions in the MMR vaccine take-ups.

It is also worth considering what would have to be true for a confounding factor to drive the results estimated in this paper. The confounder must i) have caused a coincidental divergence in the MMR vaccine take-up rates between the most and least sensitive group in the post-period, but not in the years before; ii) be orthogonal to any of the observable characteristics; iii) affect boys,

firstborns, and children of older parents more than girls, later-borns, and children of younger parents. This seems unlikely. In addition, if the surge in TV coverage of the false claim about the MMR vaccine was not exogenous and was actually a result of growing concern about vaccine safety in the population, it seems unlikely that the divergence in the MMR vaccine take-ups between the most and least sensitive group would only start in the post-period but not in the years before. For these reasons, I therefore interpret estimates as the causal impact of misinformation about vaccines.

6 Discussion and Conclusion

This paper studies the effect of misinformation on individuals using the unanticipated rise in television coverage of the false claim alleging a link between vaccines and autism in 2007 as an exogenous shock in misinformation to parents. Using vaccination data of 19-35-month-old children surveyed in the 2002-2012 National Immunization Surveys (NIS), I find that misinformation about vaccines resulted in a drop of at least 3.3 percentage points in the MMR vaccine take-up rate at 15 months old which is the CDC's recommended age. In addition, misinformation also led to a drop of at least 4.1 percentage points in the MMR vaccine take-up rate at 29 months old. This indicates that, at a minimum, misinformation caused parents to delay vaccinating their children by over a year and, at most, prevented them from ever immunizing their children.

The estimates here are economically meaningful, especially considering that my identification strategy of comparing the most and least sensitive groups likely results in the underestimation of effects. The estimated drop in the MMR vaccine take-up at 15 months old is equivalent to an increase of 15 percent in unvaccinated 15-month-olds, while the estimated decrease in the MMR vaccine take-up at 29 months old is equivalent to an increase of 59 percent in unvaccinated 29-month-olds. In addition, Lo and Hotez (2017) predict that a similar-sized reduction in the MMR vaccine coverage of children 2-11 years old in the US would result in a three-fold increase in annual measles outbreaks. Importantly, results here suggest that people can change behavior in important ways that not only affect their own welfare but also the welfare of those around them.

Additionally, these estimates are comparable or even bigger than the reported effects of new and reliable information found in prior literature. For example, Smith, Ellenberg, Bell, and Rubin (2008) reports that the number of American children who received all childhood immunizations except for the MMR vaccine rose from 0.8 percent to 2.1 percent after the publication of Wakefield et al. (1998) which first suggested a link between the MMR vaccine and autism. Chang (2018) also examines the effects of the 1998 vaccine controversy in the US and reports that the overall MMR vaccine take-up declined by 1.1 to 1.5 percentage points in the immediate year following the Wakefield et al. (1998) publication. Combined with these findings, my results suggest that misinformation reported by the media can change individual behavior as much as reliable information and that the general public is not able to discern false information even when it is easy to verify.

These results also have clear relevance for public policy regarding fake news and misinformation. Much of the debate over the responsibility of social media companies and the government in combating misinformation depends on whether misinformation actually matters. Results presented here provide clear evidence that misinformation can change a behavior that not only affects those individuals but also potentially imposes negative externalities on those around them. This suggests that there are potentially large social benefits from preventing the dissemination of misinformation.

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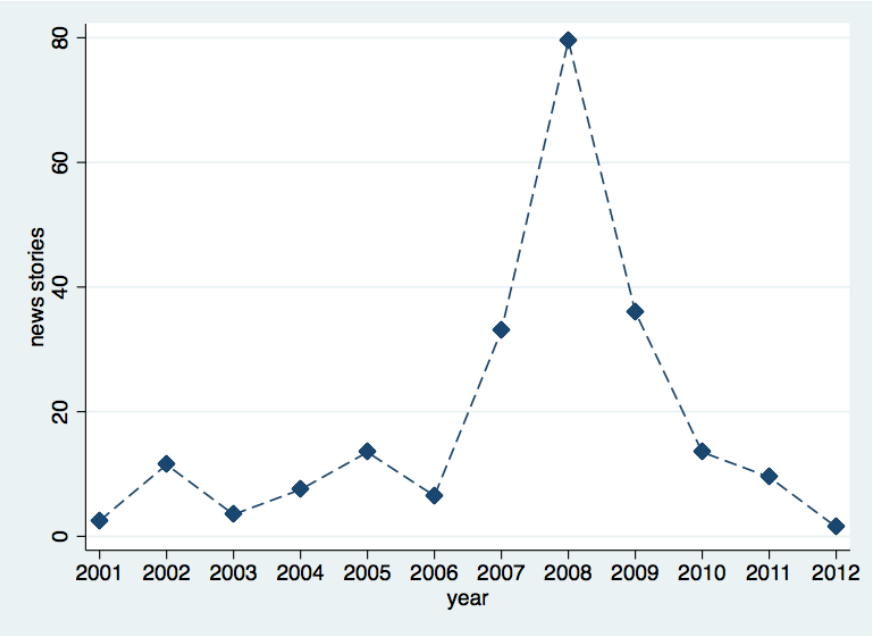
7 Figures

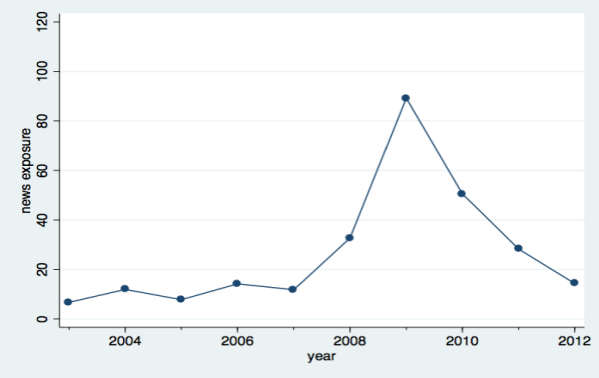
Figure 1: Number of television coverage on the topic of vaccines and its link to autism



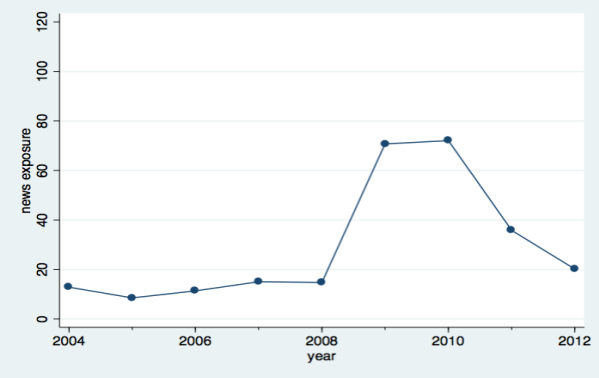
Notes: This figure demonstrates the number of news stories reporting on the alleged link between vaccines and autism without explicitly refuting it as false. The numbers are based on the coverage on 6 television networks: ABC, CBS, NBC, CNN, MSNBC, and Fox News. This figure is a visual representation of Table 1. Data source: LexisNexis.

Figure 2: Misinformation exposure from when child was born to 15 months old

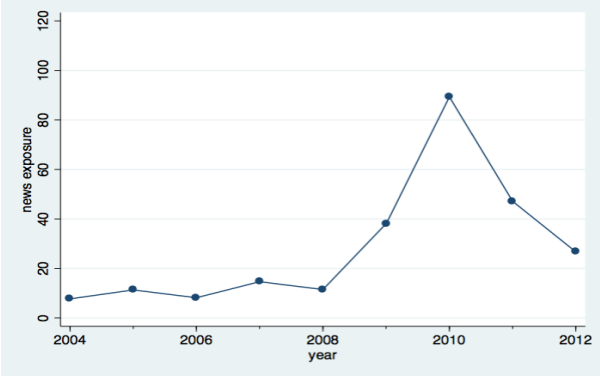
(A) 19-23 months old at time of interview



(B) 24-29 months old at time of interview



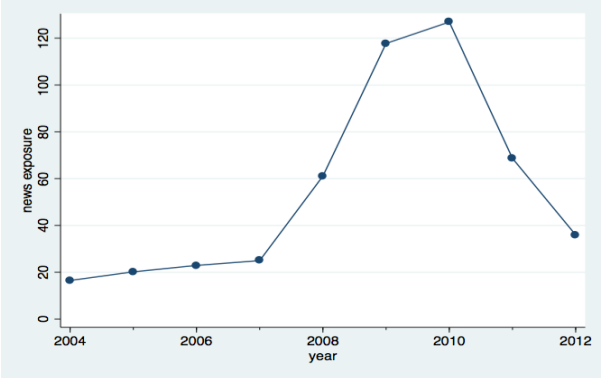
(C) 30-35 months old at time of interview



Notes: This figure shows the average misinformation exposure of parents in each interview cohort. Exposure is the number of 'false claim only' news stories that parents were exposed to from when the child was born to when the child was 15 months old. Data source: LexisNexis.

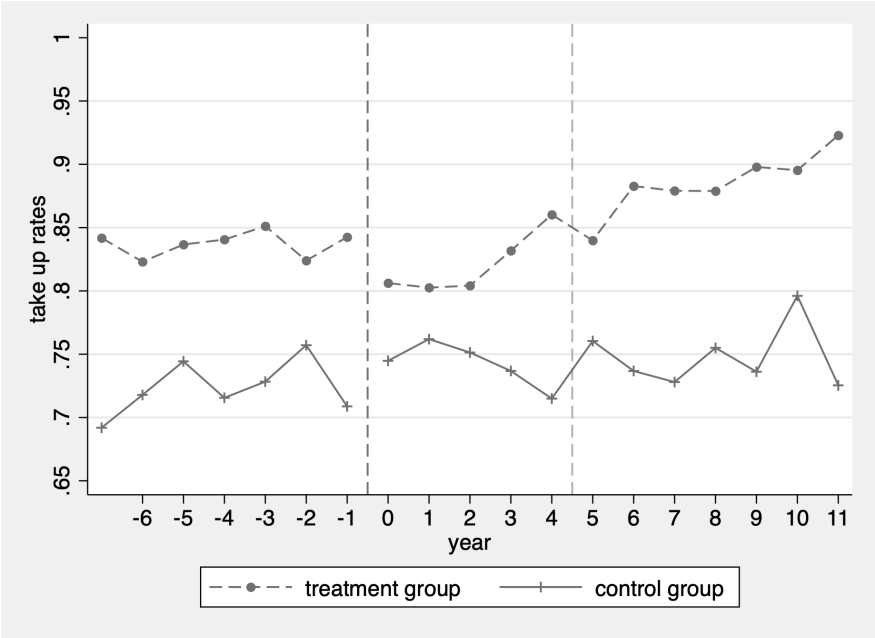
Figure 3: Misinformation exposure from when child was born to 29 months old

Child was 30-35 months old at time of interview



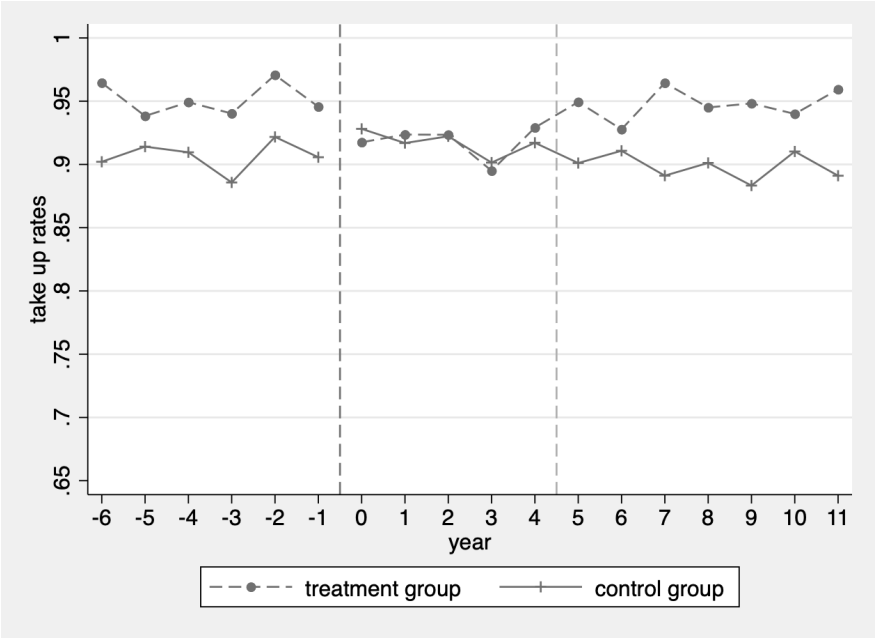
Notes: This figure shows the average misinformation exposure of parents in each interview cohort. Exposure is the number of 'false claim only' new stories that parents were exposed to from when the child was born to when the child was 29 months old. Data source: LexisNexis.

Figure 4: MMR vaccine take-up rates at 15 months old



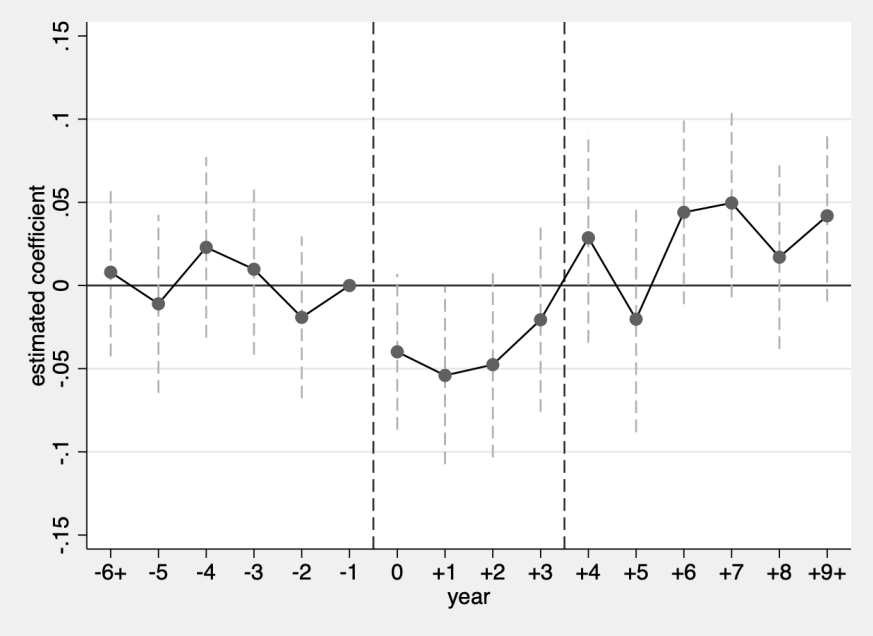
Notes: This figure shows the MMR vaccine take-up rate at 15 months old of children in the treatment and control groups. The treatment group is children with all 3 risk factors present, i.e., boys who are a firstborn and whose mom is over 30 years old. The Control group is children with none of the risk factors present, i.e., girls who are not a firstborn and whose mother is under 30 years old. The first dash line signifies the start of the post-period. The second dash line indicates the last year that I have media information. Data source: 2002-2019 National Immunization Surveys.

Figure 5: MMR vaccine take-up rates at 29 months old



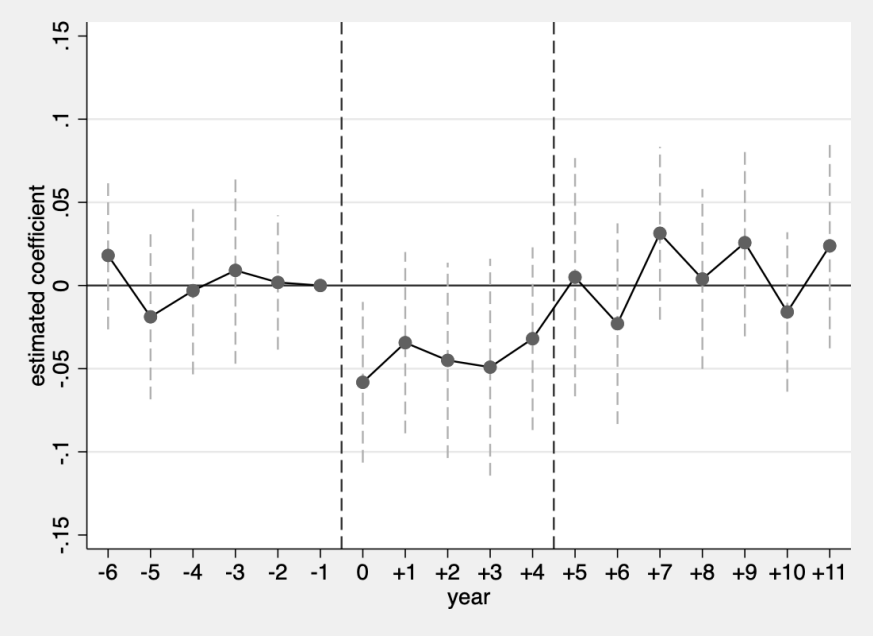
Notes: This figure shows the MMR vaccine take-up rate at 29 months old of children in the treatment and control groups. Children included in the sample here are from the age group 30-35 months old. The treatment group is children with all 3 risk factors present, i.e., boys who are a firstborn and whose mom is over 30 years old. The Control group is children with none of the risk factors present, i.e., girls who are not a firstborn and whose mother is under 30 years old. The first dash line signifies the start of the post-period. The second dash line indicates the last year that I have media information. Data source: 2002-2019 National Immunization Surveys.

Figure 6: Dynamic difference-in-differences estimates of the effects on the MMR vaccine take-up rates at 15 months old



Notes: This figure shows the coefficients estimated from the dynamic difference-in-differences estimation for the MMR vaccine take-up at 15 months old. The treatment group is children with all 3 risk factors present, i.e., boys who are a firstborn and whose mom is over 30 years old. The Control group is children with none of the risk factors present, i.e., girls who are not a firstborn and whose mother is under 30 years old. The first dash line signifies the start of the post-period. The second dash line indicates the last year that I have media information. Data source: 2002-2019 National Immunization Surveys.

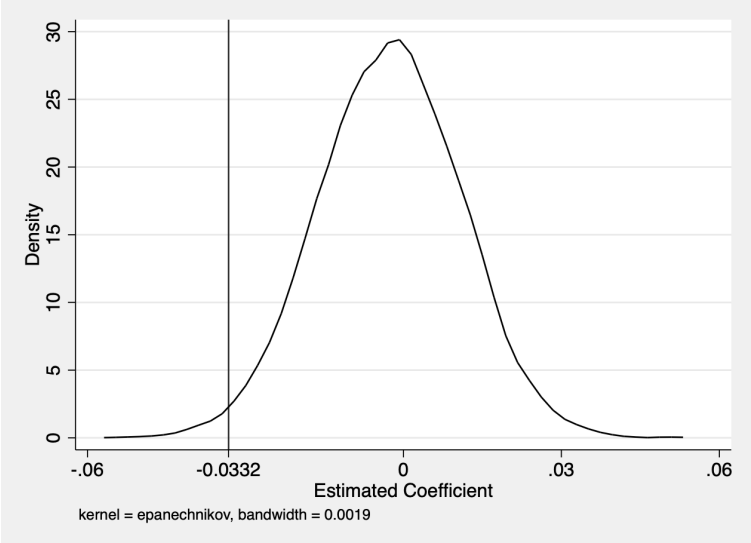
Figure 7: Dynamic difference-in-differences estimates of the effects on the MMR vaccine take-up rates at 29 months old



Notes: This figure shows the coefficients estimated from the dynamic difference-in-differences estimation for the MMR vaccine take-up at 29 months old. Children included in the sample here are from the age group 30-35 months old. The treatment group is children with all 3 risk factors present, i.e., boys who are a firstborn and whose mom is over 30 years old. The Control group is children with none of the risk factors present, i.e., girls who are not a firstborn and whose mother is under 30 years old. The first dash line signifies the start of the post-period. The second dash line indicates the last year that I have media information. Data source: 2002-2019 National Immunization Surveys.

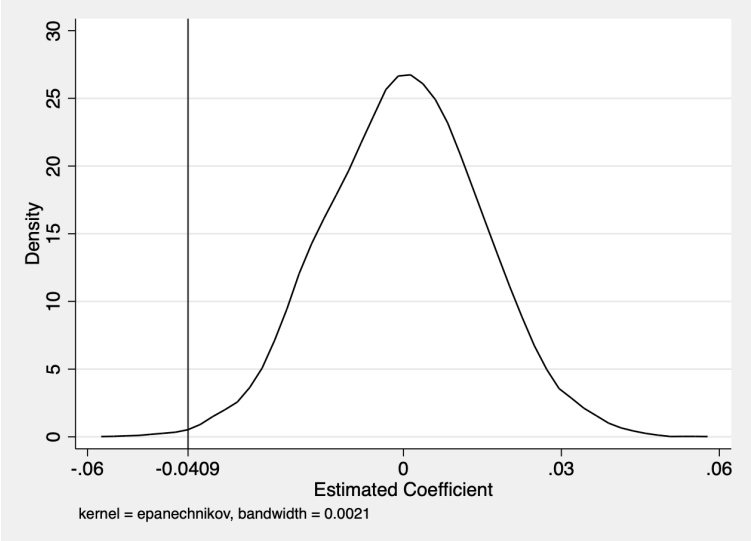
Figure 8: Distribution of coefficients obtained from the randomization exercise

(a) Take-ups at 15 months old



Note: empirical p-value is 0.0118

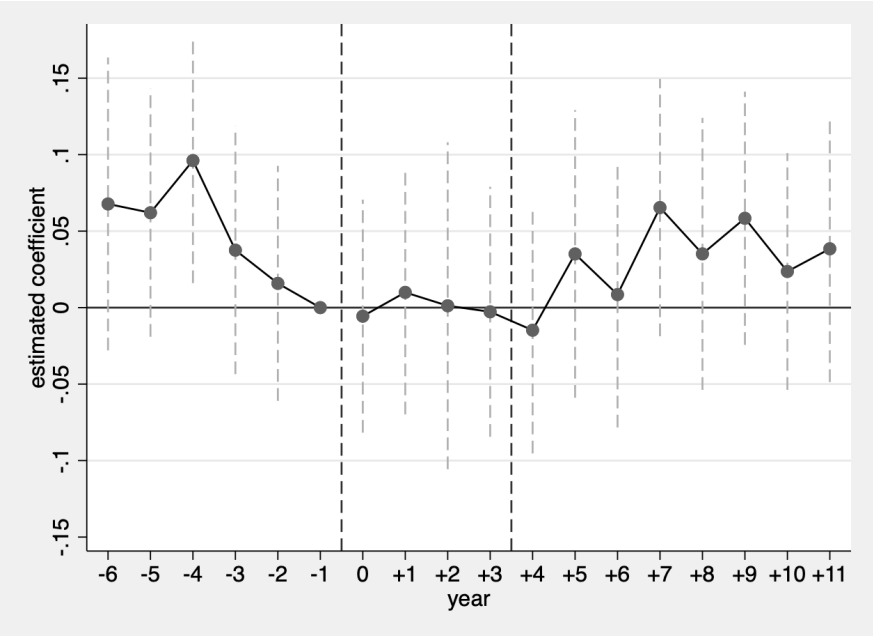
(b) Take-ups at 29 months old



Note: empirical p-value is 0.0027

Notes: This figure shows the distribution of estimates obtained from a randomization exercise. Specifically, I randomly reassign child's gender, mother's age, and firstborn status based on the true distribution of each variable in each year, and then estimate the effect (β) based on the reassignment. I do this for 10,000 replications and plot the distribution of the 10,000 coefficients estimated.Data source: 2002-2012 National Immunization Surveys.

Figure 9: Dynamic difference-in-differences estimates of the effects on the Varicella vaccine take-up rates at 29 months old



Notes: This figure shows the coefficients estimated from the dynamic difference-in-differences estimation for the Varicella vaccine take-up at 29 months old. Children included in the sample here are from the age group 30-35 months old. The treatment group is children with all 3 risk factors present, i.e., boys who are a firstborn and whose mom is over 30 years old. The Control group is children with none of the risk factors present, i.e., girls who are not a firstborn and whose mother is under 30 years old. The first dash line signifies the start of the post-period. The second dash line indicates the last year that I have media information. Data source: 2002-2019 National Immunization Surveys.

8 Tables

Table 1: Number of 'false claim only' news stories alleging the link between vaccines and autism

year	number of news stories
2001	2.5
2002	11.5
2003	3.5
2004	7.5
2005	13.5
2006	6.5
2007	**33**
2008	**79.5**
2009	36
2010	13.5
2011	9.5
2012	1.5

Notes: This table shows the number of news stories reporting on the alleged link between vaccines and autism without explicitly refuting it as false over the year. The numbers are based on the coverage on 6 television networks: ABC, CBS, NBC, CNN, MSNBC, and Fox News. Data source: LexisNexis.

Table 2: Summary statistics

Panel 1: children 19-35 months old

	All		**Least Sensitive to Misinformation**		**Most Sensitive to Misinformation**	
	mean	sd	mean	sd	mean	sd
MMR shot at 15 months	0.79	(0.41)	0.74	(0.44)	0.85	(0.36)
Male	0.51	(0.50)	0.00	(0.00)	1.00	(0.00)
Firstborn	0.41	(0.49)	0.00	(0.00)	1.00	(0.00)
Mother $\geq$ 30	0.58	(0.49)	0.00	(0.00)	1.00	(0.00)
White	0.72	(0.45)	0.68	(0.47)	0.74	(0.44)
Black	0.15	(0.35)	0.20	(0.40)	0.11	(0.31)
In poverty	0.31	(0.46)	0.51	(0.50)	0.14	(0.35)
Mother with college degree	0.34	(0.47)	0.09	(0.29)	0.59	(0.49)
Mother is married	0.67	(0.47)	0.49	(0.50)	0.81	(0.40)
age1	0.30	(0.46)	0.32	(0.47)	0.29	(0.45)
age2	0.34	(0.47)	0.34	(0.47)	0.34	(0.47)
age3	0.36	(0.48)	0.34	(0.47)	0.37	(0.48)
Moved state after birth	0.09	(0.29)	0.09	(0.28)	0.10	(0.30)
Observations	298965		25238		34159	

Panel 2: children 30-35 months old

	All		**Least Sensitive to Misinformation**		**Most Sensitive to Misinformation**	
	mean	sd	mean	sd	mean	sd
MMR shot at 29 months	0.92	(0.27)	0.91	(0.29)	0.94	(0.24)
Male	0.51	(0.50)	0.00	(0.00)	1.00	(0.00)
Firstborn	0.41	(0.49)	0.00	(0.00)	1.00	(0.00)
Mother $\geq$ 30	0.60	(0.49)	0.00	(0.00)	1.00	(0.00)
White	0.72	(0.45)	0.68	(0.47)	0.74	(0.44)
Black	0.15	(0.36)	0.20	(0.40)	0.10	(0.30)
In poverty	0.31	(0.46)	0.52	(0.50)	0.14	(0.35)
Mother with college degree	0.33	(0.47)	0.09	(0.28)	0.58	(0.49)
Mother is married	0.66	(0.47)	0.47	(0.50)	0.79	(0.40)
Moved state after birth	0.10	(0.31)	0.10	(0.30)	0.11	(0.31)
Observations	111967		8668		13253	

Notes: All estimates obtained using sampling weights provided by the National Immunization Survey.The 'least sensitive to misinformation' group refers to girls who are not a firstborn and whose mother is $<$30 years old. The 'most sensitive to misinformation' group refers to boys who are a firstborn and whose mother is $\geq$ 30 years old. Data source: 2002-2019 National Immunization Surveys.

Table 3: Difference-in-differences estimates of the effects on the likelihood of ever receiving a shot of the MMR vaccine by 15 months old

	(1) MMR at 15 mo (2002-2012)	(2) MMR at 15 mo (2002-2012)	(3) MMR at 15 mo (2002-2012)	(4) MMR at 15 mo (2002-2012)	(5) MMR at 15 mo (2002-2019)	(6) MMR at 15 mo (2002-2019)	(7) MMR at 15 mo (2002-2019)	(8) MMR at 15 mo (2002-2019)
Most Sensitive	-0.0489***	-0.0332**	-0.0230**	-0.0256	-0.0495***	-0.0446***	-0.0227**	-0.0449**
× Post	[0.0021]	[0.0199]	[0.0259]	[0.3334]	[0.0020]	[0.0057]	[0.0289]	[0.0311]
(-2012)	{0.0004}	{0.0084}	{0.0198}	{0.0917}	{0.0006}	{0.0025}	{0.2160}	{0.0348}
	(0.0094)	(0.0102)	(0.0083)	(0.0137)	(0.0145)	(0.0147)	(0.0183)	(0.0213)
Most Sensitive					0.0254*	0.0307**	0.0163	0.0302
× Post					[0.0792]	[0.0394]	[0.2609]	[0.4542]
(2013-2019)					{0.0685}	{0.0330}	{0.3064}	{0.4288}
					(0.0140)	(0.0144)	(0.0159)	(0.0381)
Outcome mean	0.7880	0.7880	0.7880	0.7880	0.7999	0.7999	0.7999	0.7999
Age group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes		Yes	Yes	Yes
Controls X Year			Yes				Yes	
Group specific time trend				Yes				Yes
N	39225	39225	39225	39225	59396	59396	59396	59396

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. Data source: 2002-2019 National Immunization Surveys.

Table 4: Difference-in-differences estimates of the effects on the likelihood of ever receiving a shot of the MMR vaccine by 29 months old

	(1) MMR at 29 mo (2002-2012)	(2) MMR at 29 mo (2002-2012)	(3) MMR at 29 mo (2002-2012)	(4) MMR at 29 mo (2002-2012)	(5) MMR at 29 mo (2002-2019)	(6) MMR at 29 mo (2002-2019)	(7) MMR at 29 mo (2002-2019)	(8) MMR at 29 mo (2002-2019)
Most Sensitive	-0.0446***	-0.0409***	-0.0413**	-0.0466*	-0.0446***	-0.0459***	-0.0412**	-0.0536***
× Post	[0.0004]	[0.0016]	[0.0222]	[0.0509]	[0.0003]	[0.0004]	[0.0239]	[0.0056]
(2008-2012)	{0.0000}	{0.0003}	{0.0181}	{0.0133}	{0.0000}	{0.0000}	{0.0112}	{0.0005}
	(0.0064)	(0.0075)	(0.0146)	(0.0155)	(0.0063)	(0.0067)	(0.0145)	(0.0125)
Most Sensitive					0.0044	0.0055	-0.0034	-0.0110
× Post					[0.6465]	[0.6050]	[0.8284]	[0.7014]
(2013-2019)					{0.6309}	{0.5865}	{0.8199}	{0.6602}
					(0.0090)	(0.0098)	(0.0148)	(0.0246)
Outcome mean	0.9270	0.9270	0.9270	0.9270	0.9259	0.9259	0.9259	0.9259
Controls		Yes	Yes	Yes		Yes	Yes	Yes
Controls X Year			Yes				Yes	
Group specific time trend				Yes				Yes
N	13851	13851	13851	13851	21921	21921	21921	21921

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. Data source: 2002-2019 National Immunization Surveys.

Table 5: DID estimates of the effects on take-ups of other vaccines by 29 months old (2002-2012)

	(1) Measles, Mumps, and Rubella (MMR)	(2) Varicella (VRC)	(3) Hepatitis B (HepB)	(4) Diphtheria & Tetanus (DT)	(5) Haemophilus Influenzae type B (HIB)	(6) Inactivated Poliovirus (Polio)
MostSensitive	-0.0409***	-0.0442**	-0.0052	0.0038	0.0039	-0.0007
× Post	[0.0016]	[0.0265]	[0.6324]	[0.6643]	[0.6195]	[0.9440]
(2008-2012)	{0.0003}	{0.0257}	{0.5893}	{0.6150}	{0.6078}	{0.9416}
	(0.0075)	(0.0169)	(0.0092)	(0.0073)	(0.0074)	(0.0090)
Outcome mean	0.9270	0.8933	0.9781	0.9825	0.9799	0.9768
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	13851	13849	13851	13851	13851	13851

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. Data source: 2002-2012 National Immunization Surveys.

Table 6: DID estimates of the effects on take-ups of other vaccines by 29 months old (2002-2012) when allowing for group-specific trends

	(1) Measles, Mumps, and Rubella (MMR)	(2) Varicella (VRC)	(3) Hepatitis B (HepB)	(4) Diphtheria & Tetanus (DT)	(5) Haemophilus Influenzae type B (HIB)	(6) Inactivated Poliovirus (Polio)
MostSensitive	-0.0466*	0.0141	0.0050	0.0096	0.0133	0.0062
× Post	[0.0509]	[0.6527]	[0.6989]	[0.5712]	[0.4821]	[0.7383]
(2008-2012)	{0.0133}	{0.5624}	{0.6894}	{0.5115}	{0.3599}	{0.7020}
	(0.0155)	(0.0235)	(0.0122)	(0.0142)	(0.0139)	(0.0159)
Outcome mean	0.9270	0.8933	0.9781	0.9825	0.9799	0.9768
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Group-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
N	13851	13849	13851	13851	13851	13851

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. Data source: 2002-2012 National Immunization Surveys.

Table 7: Treatment effects at different ages

	(1) MMR at 15 months	(2) MMR at 16 months	(3) MMR at 17 months	(4) MMR at 18 months	(5) MMR at 19 months
Most Sensitive X Post	-0.0332**	-0.0235**	-0.0253*	-0.0244**	-0.0306
	[0.0199]	[0.0217]	[0.0624]	[0.0341]	[0.1336]
	{0.0084}	{0.0283}	{0.0722}	{0.0300}	{0.1111}
	(0.0102)	(0.0092)	(0.0126)	(0.0096)	(0.0175)
Outcome mean	0.7880	0.8260	0.8456	0.8724	0.8841
Age group fixef effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	39225	39225	39225	39225	27503

	(1) MMR at 20 months	(2) MMR at 21 months	(3) MMR at 22 months	(4) MMR at 23 months	(5) MMR at 24 months
Most Sensitive X Post	-0.0272	-0.0167	-0.0131	-0.0134	-0.0421**
	[0.1720]	[0.3501]	[0.4318]	[0.4184]	[0.0185]
	{0.1464}	{0.3092}	{0.3918}	{0.3892}	{0.0119}
	(0.0173)	(0.0156)	(0.0146)	(0.0149)	(0.0137)
Outcome mean	0.8923	0.8983	0.9024	0.9065	0.9123
Age group fixef effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	27503	27503	27503	27503	13851

	(1) MMR at 25 months	(2) MMR at 26 months	(3) MMR at 27 months	(4) MMR at 28 months	(5) MMR at 29 months
Most Sensitive X Post	-0.0382**	-0.0398***	-0.0455***	-0.0410***	-0.0409***
	[0.0150]	[0.0070]	[0.0003]	[0.0020]	[0.0016]
	{0.0068}	{0.0036}	{0.0003}	{0.0008}	{0.0003}
	(0.0112)	(0.0105)	(0.0083)	(0.0087)	(0.0075)
Outcome mean	0.9168	0.9201	0.9227	0.9249	0.9270
Age group fixef effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	13851	13851	13851	13851	13851

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. All estimates are obtained using the main specification, i.e., difference-in-difference with year fixed effects, state fixed effects, observable controls. Data source: 2002-2012 National Immunization Surveys.

Table 8: Effects on take-up rate at 15 months old using more loosely defined treatment and control groups

	Baseline	**Increase control group**		**Increase treated group**		**Only using 2 characteristics to define treatment group**		
	3 characteristics vs. 0 characteristic	3 characteristics vs. 0/1 characteristic	3 characteristics vs. 0/1/2 characteristics	3/2 characteristics vs. 0 characteristic	3/2/1 characteristics vs. 0 characteristic	boy & mother≥30 vs. girl & mother<30	boy & firstborn vs. girl & not firstborn	mother≥30 & firstborn vs. mother<30 & not firstborn
Most Sensitive	-0.0332**	-0.0141*	-0.0143	-0.0264***	-0.0243***	-0.0154	-0.0043	-0.0285**
× Post	[0.0199]	[0.0972]	[0.1180]	[0.0086]	[0.0047]	[0.2526]	[0.5495]	[0.0203]
(-2012)	{0.0084}	{0.1129}	{0.1266}	{0.0007}	{0.0010}	{0.1681}	{0.5362}	{0.0236}
	(0.0102)	(0.0081)	(0.0086)	(0.0055)	(0.0053)	(0.0104)	(0.0067)	(0.0107)
Outcome mean	0.7880	0.7880	0.7880	0.7880	0.7880	0.7880	0.7880	0.7880
Age group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	39225	114533	196675	121367	196675	98826	98008	78291

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. All estimates are obtained using the main specification, i.e., difference-in-difference with year fixed effects, state fixed effects, observable controls. Data source: 2002-2012 National Immunization Surveys.

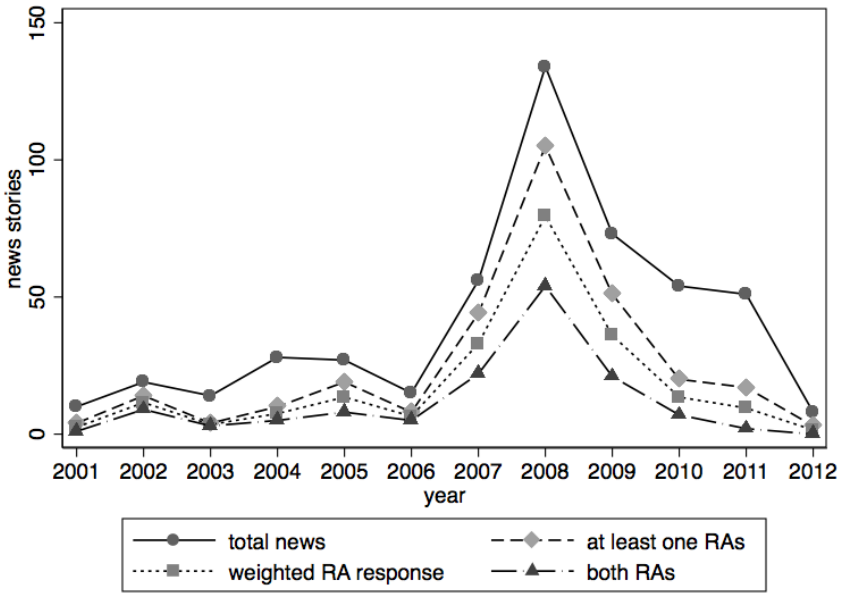
Table 9: Effects on take-up rate at 29 months old using more loosely defined treatment and control groups

	Baseline	**Increase control group**		**Increase treated group**		**Only using 2 characteristics to define treatment group**		
	3 characteristics vs. 0 characteristic	3 characteristics vs. 0/1 characteristic	3 characteristics vs. 0/1/2 characteristics	3/2 characteristics vs. 0 characteristic	3/2/1 characteristics vs. 0 characteristic	boy & mother≥30 vs. girl & mother<30	boy & firstborn vs. girl & not firstborn	mother≥30 & firstborn vs. mother<30 & not firstborn
Most Sensitive	-0.0409***	-0.0254*	-0.0218*	-0.0264***	-0.0234**	-0.0235*	-0.0136	-0.0278***
× Post	[0.0016]	[0.0584]	[0.0552]	[0.0024]	[0.0198]	[0.0881]	[0.1117]	[0.0071]
(-2012)	{0.0003}	{0.0524}	{0.0521}	{0.0028}	{0.0069}	{0.0565}	{0.0912}	{0.0034}
	(0.0075)	(0.0115)	(0.0099)	(0.0067)	(0.0069)	(0.0109)	(0.0073)	(0.0073)
Outcome mean	0.9270	0.9270	0.9270	0.9270	0.9270	0.9270	0.9270	0.9270
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	13851	40625	70694	43920	70694	35646	35092	27658

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. All estimates are obtained using the main specification, i.e., difference-in-difference with year fixed effects, state fixed effects, observable controls. Data source: 2002-2012 National Immunization Surveys.

9 Appendix

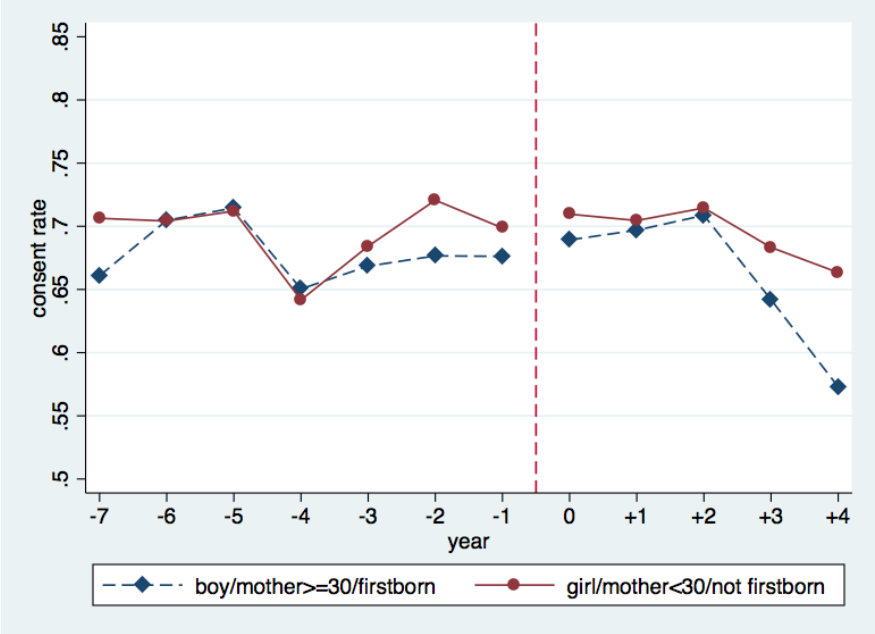
Figure A.1: Number of television coverage on the topic of vaccines and its link to autism



Notes: This figure demonstrates the number of news stories reporting on the alleged link between vaccines and autism on 6 television networks: ABC, CBS, NBC, CNN, MSNBC, and Fox News. 'Total news' plots to the number of news stories that reported on the topic of vaccines and autism, i.e. the stories that mention vaccines and autism in the same section, regardless of whether they refuted the false claim or not. 'At least one RA' plots the number of news stories that were flagged by at least one of the RAs as reporting on the false claim without refuting the claim as false. 'Weighted RA response' plot the number of news stories that reported on 'false claim only' as defined in the Data section. A story is counted as one 'false claim only' story if both RAs flagged the story and half a 'false claim only' story if only one flagged the story. 'Both RAs' plot the number of news stories that were flagged by both RAs. Data source: LexisNexis.

Figure A.2: Percent of parents who consent to the CDC obtaining vaccination records from healthcare providers

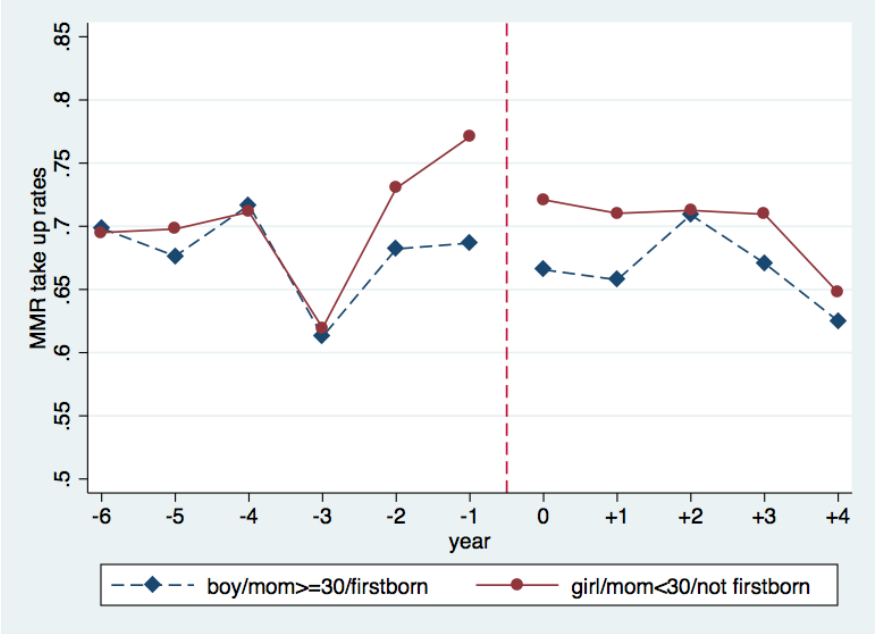
Using the same cohorts and treatment as the analysis for the MMR vaccine take-up at 15 months



Notes: This figure shows the consent rate of parents in the control and treatment groups over time. The consent rate is the percentage of the parents who were surveyed by the CDC who allowed the CDC to obtain vaccination data from healthcare providers. Data source: 2002-2012 National Immunization Surveys.

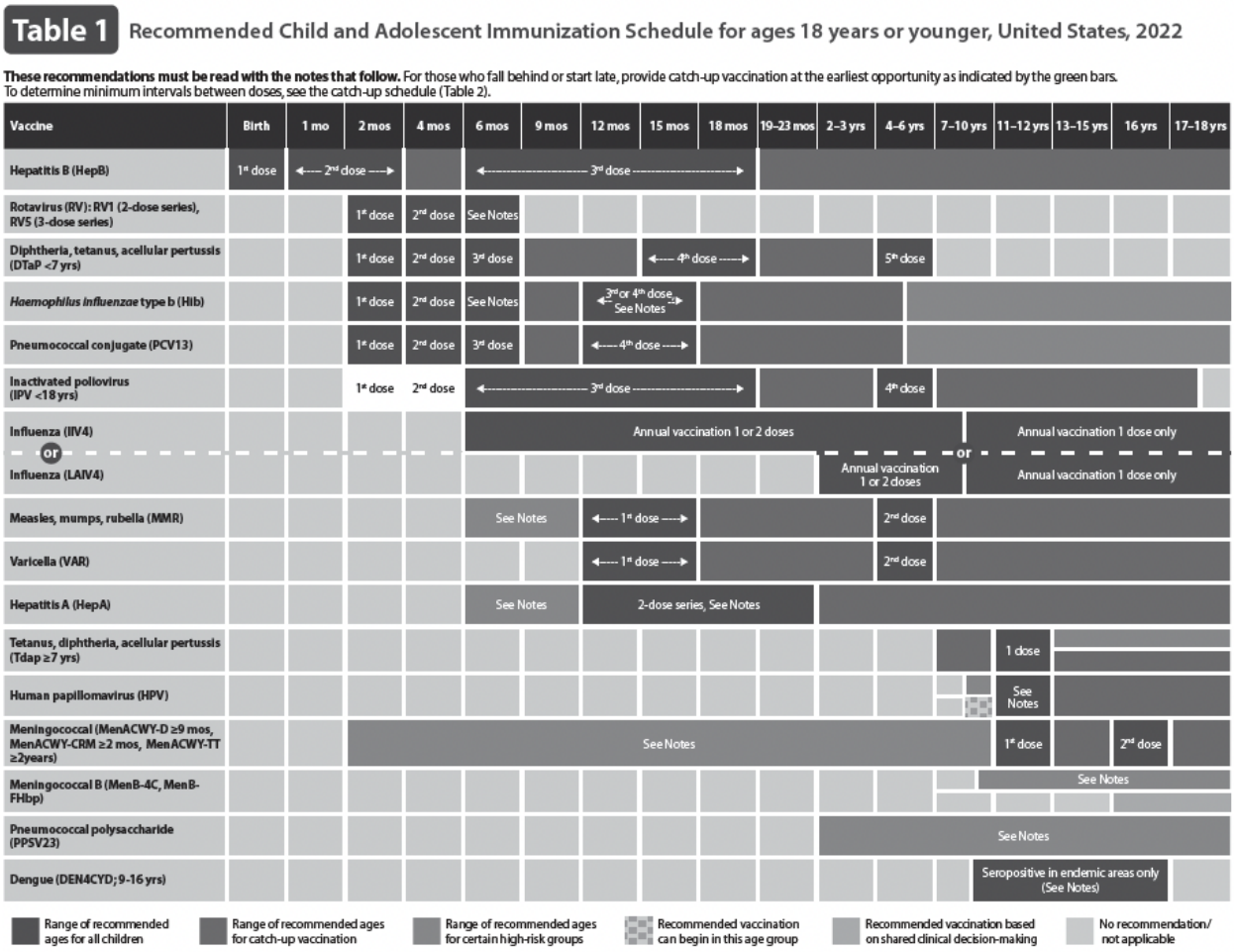
Figure A.3: Percent of parents who consent to the CDC obtaining vaccination records from healthcare providers

Using the same cohorts and treatment as the analysis for the MMR vaccine take-up at 29 months



Notes: This figure shows the consent rate of parents in the control and treatment groups over time. The consent rate is the percentage of the parents who were surveyed by the CDC who allowed the CDC to obtain vaccination data from healthcare providers. Data source: 2002-2012 National Immunization Surveys.

Figure A.4: Recommended Immunization Schedule



Sources: the CDC

Table A.1: Effects of misinformation on parents consenting to the CDC acquiring vaccination record from healthcare provider

	(1) 15-month treatment	(2) 15-month treatment	(3) 29-month treatment	(4) 29-month treatment
MostSensitive X Post	-0.0033	0.0063	-0.0099	-0.0014
	(0.0135)	(0.0139)	(0.0232)	(0.0238)
P-value	0.8086	0.6522	0.6697	0.9521
N	56360	56360	19895	19895
Year FE	Yes	Yes	Yes	Yes
Controls		Yes		Yes

*p<0.10, **p<0.05, ***p<0.010

Notes: The first two columns report the estimates from the analysis using the same sample and treatment as the one used in the analysis for the MMR vaccine take-up at 15 months old. The last two columns report the estimates from the analysis using the same sample and treatment as the one used in the analysis for the MMR vaccine take-up at 29 months old. The outcome variable is a binary variable indicating whether the parent consent to the CDC to obtain immunization records from the healthcare provider. Robust standard errors are in parentheses. Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys.

Table A.2: Difference-in-differences estimates of the effects on the likelihood of ever receiving a shot of the MMR vaccine by 15 months old (Logit)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	MMR at 15 mo (2002-2012)	MMR at 15 mo (2002-2012)	MMR at 15 mo (2002-2012)	MMR at 15 mo (2002-2012)	MMR at 15 mo (2002-2019)	MMR at 15 mo (2002-2019)	MMR at 15 mo (2002-2019)	MMR at 15 mo (2002-2019)
outcome								
Most Sensitive	-0.3000***	-0.2157**	-0.1373**	-0.1857	-0.3040***	-0.2833***	-0.1353**	-0.3552***
× Post	[0.0025]	[0.0200]	[0.0387]	[0.1257]	[0.0020]	[0.0026]	[0.0441]	[0.0058]
(-2012)	{0.0000}	{0.0005}	{0.0138}	{0.0302}	{0.0005}	{0.0017}	{0.2278}	{0.0092}
	(0.0568)	(0.0622)	(0.0558)	(0.0857)	(0.0879)	(0.0904)	(0.1122)	(0.1364)
Most Sensitive					0.2673**	0.2945***	0.1842*	0.1348
× Post					[0.0112]	[0.0073]	[0.0895]	[0.5800]
(2013-2019)					{0.0032}	{0.0018}	{0.0820}	{0.5872}
					(0.0906)	(0.0945)	(0.1059)	(0.2483)
Outcome mean	0.7880	0.7880	0.7880	0.7880	0.7999	0.7999	0.7999	0.7999
Age group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes		Yes	Yes	Yes
Controls X Year			Yes				Yes	
Group specific time trend				Yes				Yes
N	39225	39225	39225	39225	59396	59396	59396	59396

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. Data source: 2002-2019 National Immunization Surveys.

Table A.3: Difference-in-differences estimates of the effects on the likelihood of ever receiving a shot of the MMR vaccine by 29 months old (Logit)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	MMR at 29 mo (2002-2012)	MMR at 29 mo (2002-2012)	MMR at 29 mo (2002-2012)	MMR at 29 mo (2002-2012)	MMR at 29 mo (2002-2019)	MMR at 29 mo (2002-2019)	MMR at 29 mo (2002-2019)	MMR at 29 mo (2002-2019)
outcome								
Most Sensitive	-0.6993***	-0.6641***	-0.6463**	-0.7606**	-0.6993***	-0.7364***	-0.6432**	-0.8513***
× Post	[0.0003]	[0.0011]	[0.0152]	[0.0275]	[0.0004]	[0.0000]	[0.0168]	[0.0072]
(2008-2012)	{0.0000}	{0.0000}	{0.0029}	{0.0030}	{0.0000}	{0.0000}	{0.0026}	{0.0000}
	(0.1090)	(0.1245)	(0.2166)	(0.2566)	(0.1070)	(0.1150)	(0.2139)	(0.2094)
Most Sensitive					0.0156	0.0160	-0.1000	-0.2278
× Post					[0.9233]	[0.9257]	[0.6590]	[0.6120]
(2013-2019)					{0.9189}	{0.9220}	{0.6496}	{0.5840}
					(0.1529)	(0.1638)	(0.2201)	(0.4160)
Outcome mean	0.9270	0.9270	0.9270	0.9270	0.9259	0.9259	0.9259	0.9259
Controls		Yes	Yes	Yes		Yes	Yes	Yes
Controls X Year			Yes				Yes	
Group specific time trend				Yes				Yes
N	13851	13851	13851	13851	21921	21921	21921	21921

Notes: Wild bootstrap p-values in square brackets. Cluster-robust p-values in curly brackets. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01 Wild bootstrap p-values are obtained using the method explained in Cameron, Gelbach and Miller (2008). Controls include state fixed effects, race, poverty status, mother's education, mother's marital status, child's age group, mover status, and state's personal belief exemption law. All regressions are estimated using the sampling weights provided by the National Immunization Surveys. Data source: 2002-2019 National Immunization Surveys.