THREE ESSAYS ON THE ECONOMICS OF CHILD HEALTH

By

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ABSTRACT

THREE ESSAYS ON THE ECONOMICS OF CHILD HEALTH

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The first chapter of this dissertation examines the effect of insurance mandates on infant immunization rates. Immunizations are one of the greatest public health achievements of the 20th century. While US infant immunization rates have been increasing in the last 20 years, the cost of fully immunizing a child with all recommended vaccines has almost tripled. This is partly due to new additions in the list of recommended vaccines, but also due to the use of new, safer, but more expensive technologies in vaccine production and distribution. In recent years, many states have mandated that recommended childhood vaccines be covered by private health insurance companies. Currently, there are 33 states with such a mandate. In this paper, I examine whether the introduction of mandates on private insurers affected immunization rates. Using state and time variation, I find that mandates increased the immunization rate for three vaccines – the diphtheria-tetanus-pertussis, polio and measles vaccines – by about 1.8 percentage points. I also find evidence that the mandates shifted some vaccinations from public to private sources.

The second chapter of this dissertation studies the issue of whether concerns about autism affected vaccine takeup. In the wake of strong claims that there existed a link between autism and the measles-mumps-rubella (MMR) vaccine, which was refuted by later research, I examine whether fewer parents immunized their children. This task becomes difficult as the timing of the controversy in the US coincided with expansions in medical access for children and other programs that affect childhood immunizations, as well as another controversy regarding mercury containing preservatives in childhood vaccines. Using a time trends analysis and a few

differencing strategies that compare the take up of MMR to other vaccines, I find that the MMR-autism controversy led to a decline of about 2 percentage points in the take up of MMR and a negative spillover on other vaccines. I find some evidence that more educated mothers responded more to the controversy, which is consistent with more educated individuals absorbing health information more quickly. However, this disparity persisted even after new research and information about the lack of such link became widespread in the media.

The third chapter of this dissertation analyzes the effectiveness of a peer counseling breastfeeding support program for low income women in Michigan who participate in the Women, Infants and Children (WIC) program. Because there was excess demand for services provided by the program, many women who requested to participate were not subsequently contacted by the peer counselors. We compare the breastfeeding outcomes between the two groups and identify the effectiveness of the program based on the differences between the women who requested to participate and were enrolled relative to those who requested participation, but were not contacted due to lack of capacity. Our analysis uses survey data from the program as well as administrative data from Vital Records, Medicaid, and WIC from the state of Michigan. After providing evidence that our key assumption in identifying the effect if program is consistent with the data, we estimate that the program caused the breastfeeding initiation to increase by about 27 percentage points and the mean duration of breastfeeding to increase by more than 3 weeks. The support program we evaluated was very effective at increasing breastfeeding among low income women who participate in WIC, a population that nationally breastfeeds at rates well below the national average and below what is recommended by public health professionals.

To my family and for God.

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1.1 Introduction

The discovery and availability of vaccines to protect people from infectious diseases such as polio, measles, mumps, rubella and others are widely regarded as one of the greatest public health achievements of the 20th century (Institute of Medicine, 2004). Children are the most vulnerable to vaccine-preventable diseases so obtaining appropriate vaccinations for them is especially important. Indeed, childhood immunizations are one of the most cost effective preventative services provided in a clinical setting (Maciosek et al., 2006). In the US, infant immunizations are usually administered either right after birth at the hospital and during routine visits to the pediatrician, which are recommended at two, four, six, nine, and twelve months after birth (see Figure 1.1 for a detailed recommended vaccine schedule from 2008).

The *Healthy People 2010* initiative established by the U.S. Department of Health and Human Services in 2000 sets the goal of achieving a 90 percent immunization rate for all vaccines and a rate of 80 percent for all recommended vaccines for children less than two years old.[1] These goals are based on epidemiological studies that find a population to be protected from the risk of contracting a certain disease if at least 90 percent of the population has been

[1] The full list of immunization-related goals under this initiative is available at: http://www.healthypeople.gov/document/html/volume1/14immunization.htm

properly vaccinated.[2] Just as importantly, the distribution of vaccinated individuals must be sufficiently uniform across geographical locations. Failure to achieve such uniformity can result

[2] The 90% threshold is a general rule of thumb that comes from widely accepted simulation studies in the epidemiological literature. More details are available in Hethcote (1989).

Figure 1.1 Recommended Immunization Schedule for Infants and Young Children in 2008

<table>
<tr><th>Vaccine ▼ / Age ▶</th><th>Birth</th><th>1mo</th><th>2mo</th><th>4mo</th></tr>
<tr><td>Hepatitis B</td><td>HepB</td><td colspan="2">HepB</td><td></td></tr>
<tr><td>Rotavirus</td><td></td><td></td><td>Rota</td><td>Rota</td></tr>
<tr><td>Diptheria, Tetanus, Pertussis</td><td></td><td></td><td>DTaP</td><td>DTaP</td></tr>
<tr><td>Haemophilus Influenzae type B</td><td></td><td></td><td>HiB</td><td>HiB</td></tr>
<tr><td>Pneumococcal</td><td></td><td></td><td>PCV</td><td>PCV</td></tr>
<tr><td>Inactivated Polio Virus</td><td></td><td></td><td>IPV</td><td>IPV</td></tr>
<tr><td>Influenza</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Measles, Mumps, Rubella</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Varicella</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Hepatitis A</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Meningococcal</td><td></td><td></td><td></td><td></td></tr>
</table>

<table>
<tr><th>Vaccine ▼ / Age ▶</th><th>6mo</th><th>12mo</th><th>15mo</th><th>18mo</th><th>19-23 mo</th><th>2-3 yr</th><th>4-6yr</th></tr>
<tr><td>Hepatitis B</td><td colspan="4">HepB</td><td></td><td></td><td></td></tr>
<tr><td>Rotavirus</td><td>Rota</td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td>Diptheria, Tetanus, Pertussis</td><td>DTaP</td><td></td><td colspan="2">DTaP</td><td></td><td></td><td>DTaP</td></tr>
<tr><td>Haemophilus Influenzae type B</td><td>HiB</td><td colspan="2">HiB</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Pneumococcal</td><td>PCV</td><td colspan="2">PCV</td><td></td><td></td><td colspan="2">PPV</td></tr>
<tr><td>Inactivated Polio Virus</td><td colspan="4">IPV</td><td></td><td></td><td>IPV</td></tr>
<tr><td>Influenza</td><td colspan="7">Influenza (Yearly)</td></tr>
<tr><td>Measles, Mumps, Rubella</td><td></td><td colspan="2">MMR</td><td></td><td></td><td></td><td>MMR</td></tr>
<tr><td>Varicella</td><td></td><td colspan="2">Varicella</td><td></td><td></td><td></td><td>Varicella</td></tr>
<tr><td>Hepatitis A</td><td></td><td colspan="4">HepA (2 doses)</td><td colspan="2">HepA Series</td></tr>
<tr><td>Meningococcal</td><td></td><td></td><td></td><td></td><td></td><td colspan="2">MCV4</td></tr>
</table>

Source: The Centers for Disease Control and Prevention, 2008. The schedule and more detailed information available at: http://www.cdc.gov/mmwr/preview/mmwrhtml/mm5701a8.htm.

in outbreaks among unvaccinated populations if they are in close proximity to each other as was the case with a measles outbreak in San Diego in 2008 (Sugerman et al., 2010).

Despite the potential benefits to public health and the considerable gains in vaccination rates over the last 20 years, the US still falls short of the above goals for childhood immunization, particularly in certain states. For example, the national immunization rate for the '431 series' (which consists of 4 doses of diphtheria-tetanus toxoids-pertussis vaccine, 3 doses of poliovirus vaccine, and 1 dose of measles-mumps-rubella) increased from 76.3% in 1995 to 83.9% in 2006 for children less than three years old. While these figures suggest an increasing national trend in immunization rates that is approaching the above-stated goals, substantial variation across states still exists. For example, 91.6 % of children in Massachusetts were up to date with the 431 series in 2006, compared to only 75.5 % in Nevada.[3]

The increase in national immunization rates has been accompanied by an increase in the cost of becoming immunized. In both the private and public sectors – which includes purchases made by private doctors and insurers and those made by the government under programs such as Medicaid and the State's Children Health Insurance Program (SCHIP) – the cost of fully vaccinating a child with all the recommended vaccines grew substantially since the mid-1980s. This increase is partly due to the discovery of new vaccines that the Advisory Committee on Immunization Practices (ACIP) added to the recommended list, but is also due to the introduction of safer but more expensive technologies in the production and distribution of vaccines. These technologies include the use of acellular compounds for the pertussis vaccine and the elimination of the preservative thimerosal. Davis et al. (2002) find that the cost of

[3] The corresponding immunization rates are lower for the expanded 4313 and 43133 series which include 3 doses of Haemophilus influenzae type B and an additional 3 doses of hepatitis B respectively.

recommended vaccines for a two-year old child before doctors' administration charges, more than doubled between 1999 and 2002.

As of 2008, 33 states have legislation that mandates private insurers to cover childhood immunizations. For 14 of these states, such mandates have been in place since the mid to late 1990s. The mandates lower the price of obtaining the recommended vaccines for those children who relied on private insurance that previously did not cover immunizations. The mandates also lower the time costs associated with going to the public clinics that can provide free vaccines to children without immunization coverage in addition to the routine check up at the doctor's office (IOM, 2004).

In this paper, I assess the effect of vaccination mandates on the likelihood of young children being up-to-date with the 431 series. The contribution of the paper is twofold. First, I identify the effect of mandates on states' immunization rates. Unlike previous studies on immunizations, which focus mainly on the effects of specific governmental programs such as Section 317 funding (Rein et al., 2006) or the introduction of SCHIP (Joyce and Racine, 2003), I attempt to identify the effectiveness of engaging the private sector in immunization policy, which is the key feature of state insurance mandates. Second, I also examine whether they increase the proportion of children who get their vaccines at doctors' offices as opposed to public clinics, since mandates lower the cost of obtaining immunizations at the former This shift in the source of care for children might also result in savings of time and transportation costs for those parents who, in the absence of a mandate, would be required to visit both a doctor's office as part of a regularly scheduled well-child visit, and a public health clinic in order to obtain vaccines at a discounted price. These savings may be even grater in rural areas where public clinics are more sparsely dispersed.

From a public finance perspective, the shift in immunization coverage from public clinics to private insurers results in lower public expenditure for programs that purchase and distribute vaccines, with the cost being passed to private insurers. This shift might be an efficient outcome if the mandated benefit is valued by those who receive it – namely, parents of young children – to the extent that it serves as a benefit tax that has lower deadweight loss than the tax necessary to finance a similar public program (Summers, 1989). While my analysis can not determine whether a shift towards larger vaccine coverage under private insurance is welfare improving over the current system, it does shed light on the questions posed above of whether mandates increase immunization and whether they result in more coverage through the private sector.

I find that state mandates on childhood vaccination coverage had a positive effect on the immunization rates of children less than three years old. I identify the effect of state mandates by using state and time variation in their introduction while controlling for other factors and policies that likely affect childhood immunizations. Specifically, it is found that mandates increased the immunization rate for the 431 series by 1.8 percentage points and shifted the number of children obtaining their vaccines from public health clinics to the doctor's office by 4 percentage points.

1.2 Background

1.2.1 Infant Immunizations

In the U.S., the recommended vaccines for infants are determined by the federal Advisory Committee on Immunization Practices (ACIP), which consists of 15 experts in various fields appointed by the US Department of Health and Human Services (IOM, 2004). In the mid-1980s, the ACIP recommended seven vaccines (that were combined into five shots) for infants under two years of age. By the mid-1990s, six additional vaccines were added to the recommended list, and more still were added in 2000 and 2004 (Hinman, Orenstein and Rodewald, 2004). Currently, children can potentially receive up to 24 doses of vaccine by the time they are two years old, with up to five shots in a single doctor visit. Once children reach school age, most states require proof of full immunization before schooling can begin.[4]

Over the past several decades, the cost of purchasing vaccines has increased substantially. In 1987, the cost for a full course of recommended childhood vaccines was $116 in the private sector and $34 in the public sector; in 1997, the cost increased to $332 in the private sector and $176 in the public sector (IOM, 2000). The price difference between the public and private sectors mainly reflects the cost savings to the federal government that result from large volume purchases of vaccines for Medicaid, SCHIP and other programs. The public share of total vaccine purchases is estimated to be 52 to 55 percent and, on average, the federal government pays roughly half the price that is paid in the private market (IOM, 2004).[5]

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[4] Most states allow parents to waive this requirement on religious grounds. Some states also permit "philosophical" exemptions. For religious exemptions, most states require proof of religious affiliation, while for philosophical exemptions, parents are only required to sign a form stating their opposition to their child getting one or more vaccines.

[5] Detailed price lists for all vaccines in the private and the public sector for the year 2003 are provided in the IOM (2004) report (page 29). The ratios of private to public prices vary by year

More recently, Joyce and Racine (2003) estimate the public sector cost of fully immunizing a child less than two years old to be $525 in 2002. With the new additions and expanded recommendations of 2004, Lee et al. (2007) calculate an increase to $1170 in 2007.[6] These costs are based only on the purchase price of vaccine from pharmaceutical companies and do not include physicians' administration fees or other costs associated with well-child visits. The administration fee for each shot is estimated to average about $15 per dose (IOM, 2000). A recent study by Molinari et al. (2007) estimates the out-of-pocket costs for fully immunizing an infant in the state of Georgia in 2003. It finds that for uninsured children who get free vaccines from public sources, the additional administration costs, which include payments for recommended well-child visits, average about $573 per child for children up to 15 months old receiving the 43133 series.

In general, public intervention and financing for vaccines is warranted due to the positive externalities that arise from vaccination. The social benefits to immunizing an individual from a disease outweigh the individual costs by disrupting the transmission of an infectious disease and lowering the costs associated with its treatment (IOM, 2000). As a result, most developed countries use public funds for immunizations or mandate coverage by private insurance companies for childhood vaccines (Salisbury, 2005; Freed, 2007).

In the US, the vaccine financing and provision framework includes both the public and private sector. Public vaccine financing is split among the federal government and states and involves a number of programs, such as Vaccines for Children (VFC), Medicaid, SCHIP and Section 317 funding. The environment of vaccine financing and provision in the public sector is

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and vaccine and can be found at the website of the Centers for Disease Control and Prevention at www.cdc.gov.

a complicated one, especially since the early 1990s when the National Childhood Immunization Initiative was launched to increase immunization rates through outreach, education and financial support. This federal initiative established the VFC program in 1994 and made vaccines available, through private providers such as doctors' offices and hospitals, to children under 18 years of age who are either eligible for Medicaid, uninsured, Native American or Alaskan Native, or receive medical services at a federally qualified health care center (IOM, 2000). VFC covers the cost of the vaccine but not the related administration fees. Under VFC, the federal government, through the Centers for Disease Control and Prevention (CDC) negotiates the price of vaccines with pharmaceutical companies and then each individual state orders their required quantity for a specific year at the contracted price.

The entitlements for SCHIP, Medicaid, and VFC permit states to fully fund vaccine purchases for eligible children. Even though the VFC legislation constrains administrative spending to the direct cost of vaccine inventory and distribution, overhead costs for the distribution of vaccines are usually covered by Section 317 funding, which is discretionary federal funding allocated to the CDC that gets redistributed to the states (Miller, 2000). State budgets are nevertheless responsible for covering the vaccine administration fee for Medicaid recipients, as well as the purchase and the administration of vaccines for SCHIP beneficiaries if the SCHIP program was setup as a separate program from Medicaid.

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[6] Lee et al. (2008) consider vaccines given to children up to 6 years of age and include the Human Papilloma Virus vaccine in their calculations.

The federal VFC program covers the underinsured – those children who have health insurance that does not cover immunizations – but does so only in public clinics. Certain states have expanded VFC and use state funds to purchase vaccines for the underinsured and provide vaccines at the doctor's office. These states have a so called "universal VFC" program (IOM, 2004). However, Lee et al. (2007) find that in practice the underinsured are not always provided with all recommended immunizations even at public clinics, especially the newly recommended vaccines. State insurance mandates thus bridge the coverage gap for previously underinsured children who do not qualify for direct public assistance and do not have access to public clinics.

The extensive efforts behind the creation of VFC in the 1990s were in large part a response to the resurgence of measles at the beginning of the decade (Joyce and Racine, 2003). Between 1989 and 1991, there were over 55,000 cases of measles, of which 11, 000 involved hospitalizations and 166 resulted in death (CDC, 1992). In its assessment of the outbreak, the National Vaccine Advisory Committee blamed the low rate of vaccination at the recommended age (National Vaccine Advisory Committee, 1991). This episode serves as a stark reminder that if sufficient immunization rates are not uniformly achieved, the dangers and risks are very real.

1.2.2 Insurance Mandates

1.2.2.1 Literature on Health Mandates

Economic theory makes clear predictions about the effect of employer mandates on insurance coverage, wages and employment in the case when the mandate applies to all employees (Summers, 1989) or a benefit applicable to an easily identifiable group (Gruber, 1994a). In perfectly competitive markets, insurance coverage, wages and employment would fall so that the cost of the newly mandated benefits is absorbed either by employees' wages or the overall employment level, if wages cannot adjust. In the case of heterogeneous workers, firms might cut insurance coverage and/or shift employment, but the overall use of a specific health service whose coverage has been mandated would not change.

In imperfect markets with consumer misinformation and adverse selection (so that people wanting to purchase insurance for a service cannot do so even at an actuarially fair price), or in cases where different services are pooled together and prices cannot freely adjust, mandates can affect overall utilization of a health service. A mandate that requires coverage of certain procedures can increase the use of such procedures by lowering out of pocket costs. However, utilization could decrease if the cost of insurance increases substantially as a result of the mandate and the additional benefits are not valued by a part of the population. In this case, some individuals may decide to drop insurance coverage altogether. The empirical evidence on this issue is mixed. Gruber (1994b) finds that the introduction of state mandates that cover treatments for alcoholism, drug addiction and chiropractors did not lead to lower insurance rates, even among workers of small firms who are expected to be the most affected. On the other hand, Finkelstein (2004a) finds lower coverage rates for the elderly in the Medigap insurance market after the introduction of mandates that set minimum standards of coverage. She also finds evidence that these mandates exacerbated the adverse selection problem.

The empirical literature on the effect of mandates on health utilization measures is also mixed. For example, Liu, Dow and Norton (2004) find increased postpartum length of stay after the introduction of drive-through delivery laws, while Schmidt (2007) and Bitler and Schmidt (2008) find increased use of fertility treatments and higher fertility rates among older, highly educated women. Pacula and Sturm (2000) and Bao and Sturm (2004) find that the introduction of health parity legislation that mandated coverage for mental health conditions did not lead to increased mental health utilization, and Klick and Markowitz (2006) find that these mandates were ineffective in reducing suicide rates. However, Harris, Carpenter and Bao (2006) find that, among the subpopulation of those with mild mental health problems who are likely to be more price sensitive, the parity mandates increased access. Bitler and Carpenter (2009) also find that mandates covering mammograms increased screenings among insured women.

An important complicating factor in previous research on the effectiveness of mandates on health utilization measures stems from the fact that mandates are often accompanied by legal loopholes, exceptions and exemptions that practically dilute their effect, resulting only in small changes that are hard to quantify at the population level. For example, some mandates only require insurers to offer at least one type of policy that includes coverage of a specific medical service. Such mandates are likely to be less effective than mandates that require coverage in all policies. A clear illustration of this is the mental parity legislation in some states that requires insurers to offer plans with mental health benefits in their "menu" of policies, but do not require employers to purchase such plans (Buchmueller et al., 2007). Also, some mandates apply only to certain types of insurers; for example, they can include, exclude or apply only to Health Maintenance Organizations (HMOs), as is the case with fertility mandates in some states (Schmidt, 2007). Finally, other mandates might require coverage of certain treatments but permit yearly or lifetime caps (Gruber, 1994b).

Mandate legislation can also vary by the types of exemptions allowed, especially with regard to firm size, potential cost increases, and the Employee Retirement Income Security Act (ERISA) exemptions of state mandates on self-insured plans. For example, the Federal Mental Health Parity Act of 1996 explicitly exempts firms of fifty or fewer employees, while others can claim an exemption if compliance would cause health care costs to increase by more than 1 percent (Buchmueller et al., 2007). Moreover, because about half of employees have coverage through self-insured employer plans, the effect of state mandates would be concentrated on the rest of the population.(Laugesen et al., 2006; Buchmueller et al., 2007).

1.2.2.2 State Mandates on Insurers to Cover Childhood Immunizations

The increases in costs and complexity for the childhood vaccine schedule (as shown in Figure 1.1) highlight the importance of comprehensive pediatric care and access to health insurance. The effect of health insurance on the utilization of health care services by children in general and vaccination rates in particular is well established in the literature (e.g., Lurie et al., 1987; Currie and Gruber, 1996; Newacheck et al., 1998; Dubay and Kenney, 2001; Smith, Stevenson and Chu, 2006). Individual states appear to view the act of mandating private insurers to cover childhood vaccinations as a viable policy to assure the take-up of vaccines in the face of increasing costs.[7] As of 2008, 32 states and the District of Columbia have enacted such mandates. Table 1.1 shows the time of enactment for states that introduced mandates since 1994. The first states to pass mandates did so starting in the 1980s.

A 2004 report from the Institutes of Medicine finds that 10 percent of children under the age of five had private insurance with no immunization benefits and were likely to benefit from the introduction of mandates; 53 percent had private insurance coverage for immunizations; 10 percent had no health insurance; 18 percent had Medicaid coverage;

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[7] In some state legislatures the cost issue was mentioned as a primary factor in introducing the laws mandating childhood vaccine coverage; in other states such as Illinois and North Carolina, references in the public press were made to the increasing cost of vaccines as the primary motivator for these statutes.

Table 1.1: State Mandated Immunization Insurance

State	Month-Year Enacted	Year Coded
Delaware	Jan-99	2000
Georgia	Jul-95	1997
Illinois	Aug-99	2001
Kansas	Apr-95	1997
Mississippi	Jan-99	2000
Missouri	Oct-96	1998
Nebraska	Jan-95	1996
New Jersey	Apr-96	1998
North Carolina	Jan-96	1997
Oklahoma	Jan-98	1999
Texas	Sep-97	1999
Virginia	Jul-00	2002
West Virginia	Jul-94	1996
Wisconsin	Nov-00	2002

Notes: Arkansas, California, Colorado, Connecticut, DC, Florida, Hawaii, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Montana, New Mexico, New York, North Dakota, Ohio, Pennsylvania, Rhode Island all enacted mandates that cover immunizations before the period we study. Source: Rosenbaum et al. and author's findings.

and the remaining 9 percent had other coverage for immunizations. The 10 percent figure on the target population to be affected by the newly introduced state mandates is likely a lower bound as it reflects information from the year 2000 and by that point most states had already passed mandates.

Before continuing to a description of the data and the econometric analysis, it is worth noting that mandates on childhood vaccine coverage suffer to a much lesser extent from the problems mentioned in the previous section regarding loopholes, exceptions and exemptions,. First, out of all the states with mandates only Mississippi has a mandate to offer rather than cover immunizations. Second, the mandates apply to all types of insurers and no state has exceptions for firm size or future increases in coverage costs. Third, while there is variability in the language of the mandates about what vaccines are covered, all states cover the vaccines in the 431 series. Finally, mandates on immunization coverage are estimated to increase insurance policy costs by less compared to other types of mandates and are thus less likely to affect insurance coverage by means of increased premiums. Evidence from some states suggests that the increase in the cost of a policy after mandating coverage for all well-child benefits is at most 1 percent, while other types of mandates can increase the overall cost of the policy by as much as 10 percent.[8] As a result of the absence of complicating factors that are prevalent in other health mandate studies, childhood immunizations provide a different setting in which to study the effectiveness of mandates.

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[8] Cost information data for mandates are hard to find. The information here comes from estimates of the cost of some of the mandates in individual states. The data referred to herein

1.3 Data

The data come from the National Immunization Survey (NIS) for the years 1995 through 2006. NIS is a yearly national probability sample of children aged 19 to 35 months and averages about 30,000 children each year. The age group threshold to participate in the NIS reflects the fact that all recommended infant immunizations should be completed by the 18th month of age. The survey drew its sample from 78 strata until 2004, representing 50 states and 28 metropolitan areas. The number of strata increased to 83 in 2005, and then to 90 in 2006. Each household is drawn randomly within each stratum.

The NIS survey uses random-digit-dialing to identify households with children of the appropriate age and then surveys the most knowledgeable person in the household about the child's immunizations. The survey asks respondents to locate a shot card if available and to answer additional questions about maternal schooling, family income, marital status, and other socio-demographic information. Table 1.2 shows summary statistics for these variables. The table indicates that states with mandates tend to have populations with a lower socio-economic status but also higher proportion of college educated mothers.

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comes from Gruber (1994), Ma (2007) and estimates from the Maryland state legislature, which are available at http://mlis.state.md.us/pdf-documents/2004rs/fnotes/bil_0005/hb0065.pdf.

Table 1.2 NIS Summary Statistics by Mandate Status

	No Mandates	Old mandates	New Mandates
431 Immunization Rate	80.29%	80.91%	80.36%
Number Children in Household			
1 child	27.22%	27.81%	27.66%
2 or 3 children	59.43%	59.52%	60.29%
4 or more	13.35%	12.67%	12.05%
Male	51.12%	51.37%	50.91%
Race/Ethnicity			
White	73.02%	57.75%	62.55%
Black	7.30%	15.34%	16.96%
Hispanic	12.60%	17.47%	14.54%
Other	7.08%	9.44%	5.95%
Age in months			
19-23 months	29.81%	29.46%	29.89%
24-29 months	34.85%	35.54%	34.71%
30-35 months	35.34%	35%	35.40%
Moved from state of birth	12.02%	10.09%	10.02%
Number providers per child			
One provider	80.57%	84.74%	82.07%
Two providers	17.37%	13.95%	16.33%
Three or more providers	2.06%	1.31%	1.60%
Provider Type			
Public	21.49%	16.89%	21.89%
Hospital	6.29%	9.11%	8.23%
Private	59.94%	64.27%	58.25%
Military	2.58%	2.67%	2.24%
Mixed	9.70%	7.06%	9.39%

Notes: NIS provider weights and strata information used in the aggregation to state/year cells. Unemployment and children’s uninsurance data comes from the historical reports of the Current Population Survey. Section 317 funding data comes from CDC's administrative data, HMO penetration rates by state come from Interstudy.

Table 1.2 (cont'd)

	No Mandates	Old mandates	New Mandates
Mother's marital status			
Single mother	16.90%	22.08%	19.81%
Married mother	74.01%	69.76%	71.38%
Widow/Divorce/Sepa'ted/Dec'sed	9.09%	8.16%	8.81%
Mother's Education			
College degree	27.28%	31.38%	29.09%
High school or more	58.93%	54.22%	54.72%
Less than high school	13.79%	14.40%	16.19%
Income			
Income less than 20K	26.07%	28.09%	28.68%
Income between 20K and 50K	43.42%	37.10%	38.60%
Income more than 50K	30.51%	34.81%	32.72%
Poverty	21.50%	23.90%	24%
Urban	6.98%	9.29%	7.98%
Medicaid/Schip eligibility	43.21%	42.10%	41.36%
Unemployment	4.78%	5.07%	4.75%
Uninsurance	11.40%	11.45%	11.83%
Section 317 funding in 000$	4778	10741	10138
HMO penetration rate	16.03%	22.83%	13.93%
State/Year Cells	216	228	168

Notes: NIS provider weights and strata information used in the aggregation to state/year cells. Unemployment and children's uninsurance data comes from the historical reports of the Current Population Survey. Section 317 funding data comes from CDC's administrative data, HMO penetration rates by state come from Interstudy.

Several studies have confirmed the findings on immunization rates using the NIS. For instance, Bartlett et al., (2001) find that immunization rates using NIS in 1995 and 1996 are the same as those from the National Health Interview Survey (NHIS), which is the principal source of information on the health of the civilian population of the United States. However, NIS has a larger sample of children allowing for state-year comparisons and obtains more detailed information about their immunizations.[9] An important drawback of the NIS is that it does not ask specific information on whether the child has health insurance. I address this shortcoming by using historical estimates of children's uninsurance rate by state and year from the Current Population Survey's historical estimates in my analysis.[10] Smith et al. (2001) describe the NIS dataset in greater detail.

At the end of the household interview, NIS asks respondents for permission to contact the child's immunization providers. Over the 12 years in my sample I find a 69 percent consent rate. The second part of the NIS is the provider record check mail-in survey, which obtains provider-reported vaccination histories. Provider response rate in the NIS is very high at an average of 93 percent. The provider data on immunizations is more reliable than parents' recall or shot-card information, but in 2006 only 71% of the 29,880 children surveyed had complete provider data. Children with complete provider information are more likely to be white, have better educated parents, and live in households with higher incomes than those without provider data. In my analysis, I use the post stratification weights provided in the NIS that adjust for these factors.

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[9] Smith and Molinari (2009) examine the bias in the 2006 NIS and find that due to its RDD nature the immunization rates in the NIS are likely underestimated by less than three percent. This work is still in progress, results were made available to the author from conference presentations and personal contacts with Noelle Molinari.

[10] In the CPS, children's insurance status can only be determined for those younger than 18 years of age.

I also use historical estimates from CPS publications for the unemployment level by state and year to account for private insurance availability through employment, overall effect of economic conditions on babies' health, and as a proxy of the time costs associated with immunizing a child.[11]

[11] Dehejia and Lleras-Muney (2004) find that parental characteristics and babies' health outcomes vary depending on the unemployment rate at the time of conception.

1.4 Empirical Strategy

I follow the standard approach in the immunization literature and define my dependent variable as the fraction of children with up-to-date status by the time of the interview for the 431 vaccination series. Immunization status is usually evaluated at the series level as opposed to individual doses because an additional dose has a different marginal effect on building immunity. I focus on the 431 series, which has been recommended since the 1980s, as opposed to other more comprehensive series such as the 43133 series. This is because the Hepatitis B vaccine and Haemophilus influenza type b vaccine (which comprise the last two vaccines in the series) were recommended in the early 1990s and states vary on how the mandates apply to recent additions on the recommended list of vaccines.

Figure 1.2 shows the overall trends in immunization rates for the 431 series in states with and without a mandate. To more accurately compare the immunization rates relative to the year of the mandate enactment, states without a mandate and those that had a mandate enacted before 1995 are randomly assigned an enactment year from the sample distribution of mandate enactment years. The result is plotted in Figure 1.3. which shows overall increasing trends in immunization rates and indicates that states with recently enacted mandates tend to have lower immunization rates before the mandate but are catching up to the other states post mandate.

I estimate the following model, which controls for state (*s*) and time (*t*) difference

$$ImmRate_{st} = \beta_1 mandate_{st} + \delta' X_{st} + \gamma_1' state_s + \gamma_2' year_t + \varepsilon_{st}, \tag{1}$$

where $ImmRate_{st}$ is the state-year immunization rate of children who are up to date with the 431 series, $mandate_{st}$ is a dummy variable which takes on value of 1 if a state had a mandate in place in a given year, and X_{st} is vector of controls comprised of state-year aggregate data obtained

Figure 1.2 Comparing Immunization Rates across States
Comparison of states that recently passed mandates to those that passed them before data became available and those that never had a mandate

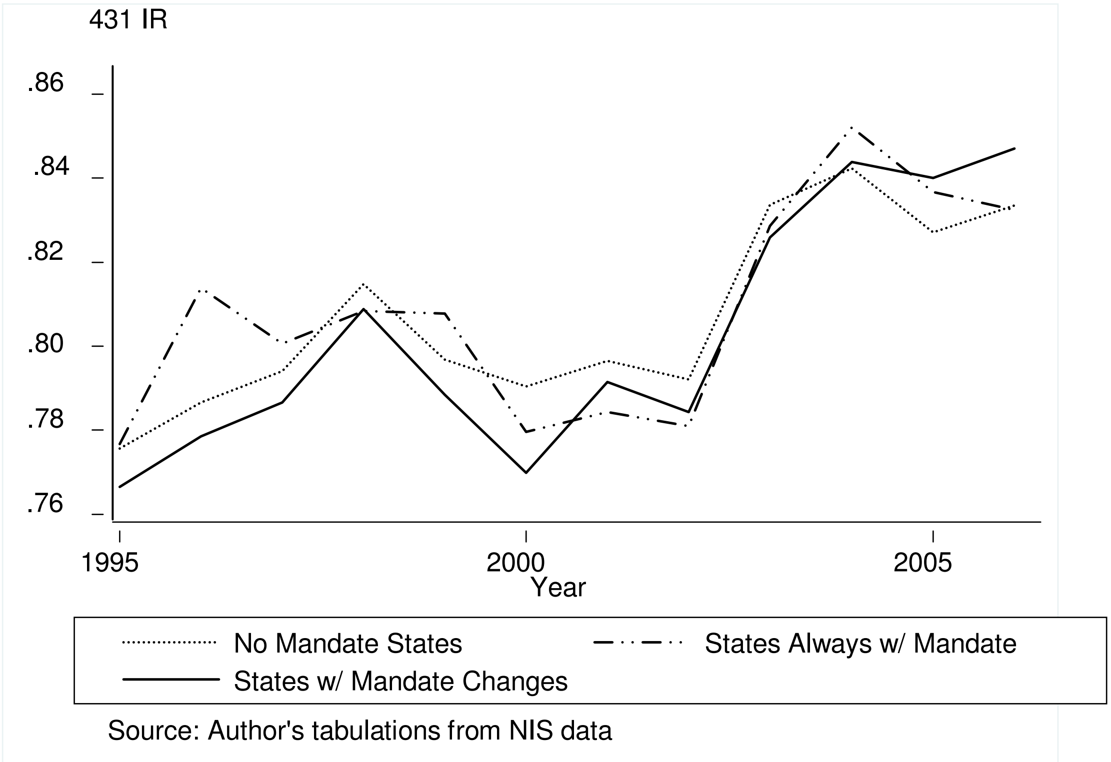
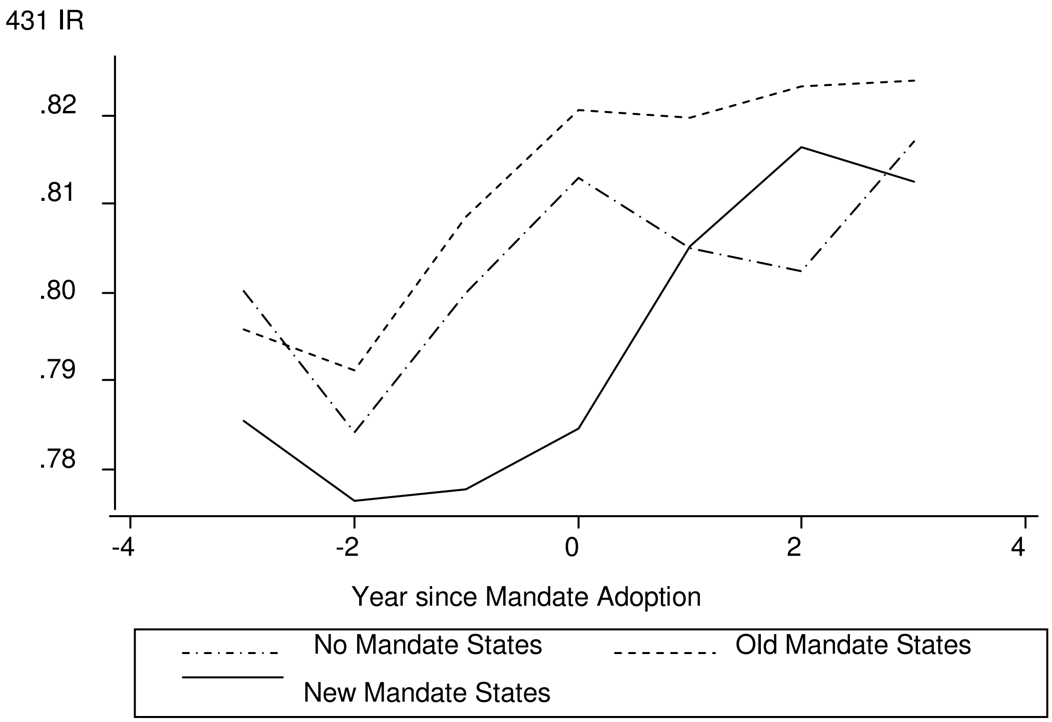


Figure 1.3 Comparing Immunization Rates Before and After the Mandate Enactment
Year of mandate adoption, randomly assigned to states that have no mandates and those who passed them before 1994.



Notes: For states that we observe less than 3 years we include their IR only for the available years.

from individual NIS data and other state level variables obtained from the CPS. The effect of the mandate β_1 is identified by variation in immunization rates across states and the timing of the introduction of mandates. The variables $state_s$ and $year_t$ represent state and year fixed effects, respectively, which account for state-specific changes and overall national trends in immunization rates.

The controls are listed in Table 1.2 and include the proportion of children in each age group, across races, mother's education level and marital status; those eligible for public health insurance; the proportion of children living in urban areas; and those who were born in a different state. I also control for the proportion of children in households with one, two or three, and four or more children; the proportion of children with one, two, or three or more providers; the proportion of children served by different types of provider (public, private, military, hospital or a combination); and the proportion of children in different income categories. Following Bertrand, Duflo and Mullainathan (2004), I report standard errors that are robust to heteroskedasticity and clustered at the state level to account for state level correlation over time.

I conduct my analysis at the state-year aggregate level for several reasons. First, mandate status varies only by state-year and looking at the effect of a state policy on individual level data might result in overestimating the significance of the effect of the policy due to the pitfalls discussed in Moulton (1990). Second, the NIS in 2005 and 2006 introduced new strata in the survey, making it difficult to analyze the data at the individual level in a panel framework, while also taking into account the survey sampling design. The number of observations used in the aggregation for each state-year cell varies substantially, but is relatively large, from 95 in New Hampshire to 2498 in Texas with an average of 595 across all states and years.

In equation (1) I also control for the proportion of children eligible for SCHIP and Medicaid because both of these programs cover childhood immunizations and are thus likely to affect immunization rates. However, since the take-up for SCHIP and Medicaid eligible children is rather low (see Currie and Gruber, 1996), controlling for the proportion of children eligible for public assistance is likely to underestimate the effect of the mandates as some of the eligible but unenrolled children for these programs have private insurance and are likely to benefit from the insurance mandate. I estimate the eligibility for SCHIP and Medicaid, using the cutoff rules for each state for 1-5 year olds based on the program rules by state and year from LoSasso and Buchmueller (2004). I do not observe the actual income of each household but rather income categories, so I assign to each household either the income for the midpoint in their category or the 75th percentile. The findings are similar irrespective of which definition of income is used.

In group average regressions it is common to weigh by the cell size to account for the possible heteroskedasticity that stems from different within-area sample sizes. I use the test proposed in Dickens (1990) and find that weighting by sample size introduces more heteroskedasticity, likely because the group level error component at the state level is larger than the sample average estimation error. In this case, simple OLS with heteroskedasticity robust standard errors is preferred and these are the estimates I report. Alternative weights in a weighted least squares framework that would account for the state average component and the different sample sizes provide similar results to the OLS results presented here and are available upon request.

Because NIS data are yearly in frequency and there is no way of telling what month the interviews were conducted, I consider a mandate to be 'in effect' after at least a full year since it has been enacted. For mandates enacted on or before June of a given year, the following year is taken to be the mandate year, while for mandates enacted after June of a given year, the mandate year is taken to be two years hence.[12] With this measure I am allowing for the children in my sample to have been exposed for about one year to the new legislation enacting mandates. The results are similar, but the effect of the mandate is slightly smaller, if I define the mandate to be in effect the year after its enactment without consideration to the month it was passed.

To examine the dynamics in the timing of the mandates, I allow for short term and long term effects by estimating the following regression:

$$ImmRate_{st} = \beta_1 mandate_{st,1\text{-}2} + \beta_2 mandate_{st,3\text{-}more} + \delta' X_{st} + \gamma_1' state_s + \gamma_2 year_t + \varepsilon_{st}, \quad (2)$$

where $mandate_{st,1\text{-}2}$ is an indicator equal to 1 if the mandate has been in effect for one or two years and $mandate_{st,3\text{-}more}$ is equal to 1 if the mandate has been in effect for three or more years according to the definition discussed above. The excluded categories in these regressions are states before they enacted a mandate and states that never had a mandate.

[12] In Delaware, for instance, the mandate was enacted in January 1999, so I code 2000 as the first year the mandate was in effect. In Georgia the mandate was enacted in July 1995, so I code 1997 as the first year with a mandate in effect.

1.5 Results

1.5.1 Baseline Results

Panel A of Table 1.3 shows the results from estimating equation (1). Column (I) reports results using the immunization rate as the dependent variable, while Column (II) uses the log of the immunization rate. Panel B shows the results of estimating equation (2). It is found that after the introduction of a mandate states' immunization rates have, on average, increased by 1.8 percentage points. The estimated standard error is 0.0074, with the effect being statistically significant at the 5% level. The effect of a mandate appears to be strongest in the first two years immediately following enactment, causing immunization rates to increase by 1.81 percentage points, but the effect persists in subsequent years as well, with an increase of 1.65 percentage points. The results using log immunization rates are similar to those above, suggesting that the introduction of a mandate led to a 2 percent increase in immunization rates. To account for the fractional nature of my dependent variable I also estimate equations (1) and (2) using a fractional Probit model in a quasi-maximum likelihood framework. The results are consistent with the findings presented here.

These results are substantial given that the population of children likely to be affected by the introduction of mandates represents about 10 percent of all children. Since about half of all employees in the country during the period of study had coverage through self-insured plans that, under ERISA, do not have to comply with state regulations, these results become even more robust as the likely affected population is then close to 5 percent of children. Note that these results do not imply that, in the absence of a mandate, the affected children would not become immunized. It is very likely that many of these children would have eventually received a full course of vaccinations, especially in states that require proof of immunization before enrolling in

Table 1.3 Mandate Effect

	Panel A		Panel B	
	(I)	**(II)**	**(III)**	**(IV)**
Dependent Variable:	P-431	Log(P-431)	P-431	Log(P-431)
Mandate	0.0180 **	0.0219 **	---	---
	[0.0074]	[0.0096]		
1-2 Years After	---	---	0.0181 **	0.0223 **
			[0.0078]	[0.0100]
3 or more Years After	---	---	0.0165 *	0.0196 *
			[0.0082]	[0.0106]
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	612	612	612	612

Notes: NIS provider weights used in the aggregation. Analysis conducted at the state/year level. Panel A considers the overall effect of the mandates on the percentage of children up to date with the 431 immunization series (4 doses DTaP, 3 doses of polio and 1 dose MMR) and the log of this rate. Panel B considers the dynamics over time and separates the immediate and the longer term effect of the mandate. In parenthesis are reported heteroskedasticity robust standard errors clustered at the state level. * denotes significance at 10%, ** significance at 5%, and *** at 1%.

day care or school,. However, because these children would not be immunized during an important time in their lives, they would have a higher risk of encountering and spreading disease, the effects of which are much more severe for younger children.

Panel B shows that the biggest change in immunization rates was during the first two years after the enactment of the mandate, but the effect persisted even in the following years.

1.5.2 Policy Endogeneity

It is possible that states enacted mandates as a response to prevailing immunization rates that were abnormally low and that the observed increases in immunization rates following mandates were the result of a natural improvement from such low levels, rather than a result of the mandates themselves. To disentangle these effects, I conduct a series of indirect tests as desribed below. Overall, I find evidence that without the introduction of legislation states would have had similar levels of immunization rates. Moreover, it does not appear that mandates were adopted in response to prevailing low levels of immunization.

As a first test, I include a one-year lead of the mandate in equation (1). If the policy was endogenously adopted by the state in response to low immunization rates in the previous year, we would expect the lead of the mandate status variable to be significant. Column 1 of Table 1.4, shows the results from this test. The excluded category includes states that never had a mandate. Dropping such states and limiting the excluded category to only the "before" period for states enacting a mandate gives similar results. This suggests that the timing of mandates is not endogenous.

Table 1.4 Policy Endogeneity

	(I)	(II)
Mand	0.0182*	---
	[0.0099]	
Lead_Mand	-0.0046	---
	[0.0090]	
T - 4 Years or more Before the Mandate	---	-0.0065
		[0.0140]
T - 2 and T - 3 Years Before the Mandate	---	0.0128
		[0.0080]
T + 1 and T + 2 Years after Mandate Enactment	---	0.0208**
		[0.0081]
T + 3 Years after the Mandate Enactment	---	0.0193**
		[0.0080]
State FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
Observations	612	612

Notes: NIS provider weights used in the aggregation. Analysis conducted at the state/year level. Column (I) estimates equation (1) by adding a lead_mandate variable that turns one the year before the mandate was enacted. Column (II) estimates equation (3), the excluded category are the year the mandate was enacted and the year immediately prior and states that never had a mandate. In parenthesis are reported heteroskedasticity robust standard errors clustered at the state level. * denotes significance at 10%, ** significance at 5%, and *** at 1%.

I estimate the following regression to determine whether mandates were enacted in response to low immunization rates in preceding years and also as a further test of the dynamics in response to the mandates:

$$ImmRate_{st} = \beta_1 mandate_{st,1\text{-}2} + \beta_2 mandate_{st,3\text{-}more} + \beta_3 mandate_{st,-2\text{-}3} + \beta_4 mandate_{st,-4\text{-}less}$$

$$+ \ \delta X_{st} + \gamma_1 ' state_s + \gamma_2 ' year_t + \varepsilon_{st}, \qquad (3)$$

where $mandate_{st,-2\text{-}3}$ is an indicator variable equal to 1 for two to four years before enactment and $mandate_{st,-4\text{-}less}$ denotes four or more years before enactment. The results are relative to the year of and immediately following enactment, consistent with previous definitions.[13] Although all states and years are used in these regressions, the variation in estimating these coefficients comes mostly from the 14 states that passed mandates between 1994 and 2006. For states that had a mandate in place prior to my sample period, I include them in the variable for the years after the enactment but not before. If the mandate was passed in response to prevailing low immunization rates, then we would expect β_3 to be negative and statistically significant. On the contrary, I find that the leads of the mandate are always insignificant, suggesting that the timing of mandates does not coincide with previous immunizations rates. Repeating these tests after restricting the sample to only those states that recently passed mandates and those that never passed mandates, yields similar results (although the coefficients are less precisely estimated due to the smaller sample size of 384 state-year observations). As found previously the largest effect

[13] Years before and after the enactment are grouped into two in order to increase the sample size.

of the mandate was immediately after its enactment relative to the year it was passed and the year prior, but this effect persisted even three or more years after.

1.5.3 Robustness Checks

Since I identify the effect of mandates using only time and state variation, my results could be biased if other changes that coincided with the timing of the mandates were enacted by these states. This problem is exacerbated by the fact that with the available data I do not observe the population likely to be affected by such mandates (i.e., children with private insurance coverage). To address these issues and specifically account for the widespread changes in policy in the area of childhood vaccinations that coincided with the introduction of mandates I conduct a series of robustness checks.

In the 1990s the VFC and SCHIP programs were introduced and additions in Section 317 funding were made. One general solution would be to identify a population that could be affected by these changes in policy, but is not expected to be influenced by the introduction of insurance mandates. One possible candidate is the population of children living in low income households. These children are more likely to be eligible for public health insurance programs such as Medicaid and SCHIP and to obtain their vaccines through VFC, but are also less likely to have parents who have private insurance coverage. It is worth pointing out that for those families that do have private insurance coverage the value of the mandate would be higher as the cost of vaccines would represent a larger portion of their budget.

The NIS has consistent data on household making less than $20,000, and I examine children belonging to such households (this group has a 78.3% poverty rate as measured by the NIS).

Column 1 of Table 1.5 shows the results of estimating equation (1) after restricting the analysis to low-income households. I find a coefficient estimate of 0.0131 and standard error of 0.0144. Thus, as expected, mandates do not affect the take up of the 431 series in this subpopulation. However, while insignificant, the point estimate is quite close in magnitude to that of the whole population of 0.0180, with a standard error of 0.074.

This result might be due to the fact that even among the low-income population there is a small portion that has private insurance coverage. According to LoSasso and Buchmueller (2004) from 1996 to 2000, 22 percent to 28 percent of the population under the age of 18 living in households below the poverty level had private insurance coverage. More recent data from CPS publications indicate that in 2008, 18 percent of children living in households making less than 25,000 dollars a year had private insurance.[14] Given that the cost of vaccines represents a larger budget share for these households, the mandates are likely more effective for these children relative to the rest of the population without immunization coverage, and this might explain a non-zero result for the overall sample of children in low-income households.

In the baseline analysis above, I directly account for the expansions in Medicaid and SCHIP by controlling for the proportion of eligible children in each state and year. However, controlling for the relative expansion of the VFC program across states is problematic as data is not available on the number of VFC providers by state and year. Year fixed effects account for nationwide trends in the expansion of VFC, but there is no reason to believe that in all states the VFC program was adopted with the same speed and to the same extent. Thus, I try to account indirectly for the possible effect of VFC by eliminating states that had a more generous VFC program, knows as a "universal VFC" program, by 2004.

[14] http://www.census.gov/hhes/www/cpstables/032010/health/h08_000.htm

Table 1.5 Robustness Checks

	(I) Less than 20K Income	(II) No Universal VFC	(III) Sample from 1998-2004	(III) ' Sample from 1998-2004 with Sect 317	(IV) Sample from 1996-2001	(IV) ' Sample from 1996-2001 with HMO
Mandate	0.0131	0.0180 **	0.0128	0.0134	0.0131	0.0123
	[0.0144]	[0.0073]	[0.0081]	[0.0082]	[0.0135]	[0.0132]
317 (100$/capita)	---	---	---	0.009 *	---	---
				[0.005]		
HMO pen rate	---	---	---	---	---	0.0852 **
						[0.0342]
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	612	516	357	357	306	306

Note: NIS provider weights used in the aggregation. Column (I) shows the results of estimating equation (1) with the population of children living in households with incomes lower than $20,000 a year. Column (II) shows the results of estimating equation 1 without states that had a universal VFC program enacted. In Column (III) I restrict the sample to those years for which I have Section 317 funding data available by state. In Column (III) ' I use the same sample as in column (III) and add as an additional control Section 317 funding allocations for the state in the previous year. in Column (IV) I restrict the sample to those years for which I have HMO penetration rates available by year, and in Column (IV)' I estimate the mandate effect with the same sample available as column (IV) while controlling for the HMO penetration rate in the state. I report fully robust standard errors clustered at state level. * denotes significance at 10%, ** significance at 5%, and *** at 1%.

States with a universal VFC program have expanded eligibility rules and make publicly funded vaccines available to all children regardless of insurance status. These states are also more likely to have a larger proportion of immunizations delivered by providers that participate in VFC (IOM, 2004).[15] Based on NIS data in 2006, 87% of children had all their vaccines in a VFC provider in states with universal VFC, while in the remaining states this figure was only 78%, with outliers such as Kansas going as low as 42%.[16] Thus, universal VFC status appears to be a good proxy on the extent of the VFC program across states.

In Column (II) of Table 1.5, I report the results of estimating equation (1) after eliminating from the sample universal VFC states. I find that the mandate is effective in this restricted sample and leads to a 1.8 percentage point increase in immunization rates for the 431 series.

In my main analysis I do not control for Section 317 funding because data on its allocation across states is only available from 1998 to 2004. Section 317 funding has been found to affect immunization rates using data from the NIS and its allocation has increased over this time period (Rein et al., 2006; IOM, 2004). In Column (III) of Table 1.5, I report the results from estimating equation (1) with the sample restricted to those years for which I have data available on Section 317 allocations by state. My data stretches from 1997 to 2003, but since immunization rates were likely affected by the allocation in the previous year, I lag the 317 allocations by one year and estimate the mandate effect during 1998-2004. I find that the

[15] Using the most recent information from 2004, there are eight states with universal VFC programs: Alaska, Idaho, Maine, Massachusetts, New Hampshire, New Mexico, Rhode Island and Washington.

[16] Providers were asked whether they participated in VFC only in 2005 and 2006. Prior to 2004, NIS used to ask parents if their children's vaccine provider was a VFC participant, but most of the parents did not know the answer.

estimate on the mandate effect declined, but is still comparable in magnitude to the main specification. However it is no longer statistically significant. This is likely due to the sample restriction that halves the sample size from 612 to 357. Adding Section 317 funding allocations to the main specification for this subsample, Column (III) shows that a $100 per capita increase in Section 317 funding results in roughly one percentage point increase in the 431 immunization rate. This is comparable to the finding in Rein et al. of a 1.6 percentage point increase.[17]

The IOM (2004) report finds that children in health maintenance organizations (HMOs) are more likely to be up to date with their immunizations compared to those in preferred provider organizations (PPOs) or other types of provider organizations. This is probably due to the fact that HMOs need to report immunization rates to state officials, who track the quality of service they provide. NIS does not have information on whether the provider is part of an HMO or PPO, so therefore I am unable to control for HMO or PPO participation in my main specification. However, data on HMO penetration rates by state and year are available from other sources up to 2000. Thus, using the 1996-2001 sub-sample, I estimate the effect of the mandate with and without the previous years' HMO penetration rate as a control. The reports are reported in Columns (IV) and (IV') of Table 1.5. The results are consistent with previous studies that find that states with higher rates of HMO penetration tend to have higher immunization rates. As shown in Column (IV), the mandate variable is no longer statistically significant in this subsample, most likely due to the sample restriction. In fact, by dropping the years after 2001, I lose identification from the four states that passed mandates after this year. There is also a substantial decrease in the number of observations as several states enacted their mandates just before 2000.

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[17] Rein et al. (2006) use a different definition of Section 317 funding, and their dependent

1.5.4 Mandate Displacement Effect

Aside from changes in immunization rates, another interesting question is whether as a result of mandates, more immunizations are being completed at doctors' offices as opposed to public clinics. The extent to which vaccines are being administered more in doctors' offices under private insurance rather than in public clinics represents a cost shift from the public to private sector.

Unfortunately, I do not have information on the insurance status of a child at the time of vaccination, or whether the vaccine the child received was covered through VFC. However, as part of the PRC portion of NIS, providers are asked to identify themselves as private, public, hospital, military or WIC clinic.[18] Thus, for each child, I observe the number of providers and how these providers identify themselves. I can then use this data to test whether the mandate had an effect on the proportion of children who are obtaining their immunizations at the doctor's office in the private sector. Table 1.6 reports the results from estimating equation (1) using three different dependent variables. The first is the proportion of children who obtained one or more vaccines exclusively at a private doctor's office, the second is the proportion of children who obtained their vaccines at a public clinic, and the third the proportion of children who obtained their vaccines at a private doctors' office, a hospital, or at more than one type of provider.

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variable is the immunization rate for the 43133 series.

[18] Most providers fit in the first four categories; there are fewer than 0.1% of children who took their immunizations in a WIC clinic.

Table 1.6 Mandate Effect on Type of Provider

Dependent Variable: Percentage of children who have completed at least one shot and have the following providers

	(I) Private	(II) Public	(III) Private, Mixed & Hospitals
Mandate	0.040 *** [0.0192]	-0.0433 ** [0.0173]	0.0434 ** [0.0171]
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	612	612	612

Notes: NIS provider weights used in the aggregation. In column (I) the dependent variable is percentage of children in each state/year that have obtained their immunizations at a private doctor's office. The children have not necessarily completed the 431 series, but got at least one shot and thus we can observe where they obtained that shot. In column (II) children have obtained the vaccine(s) at a public clinic and in (III) either at a private doctor's office, hospital, or in more than one type of care. Robust standard errors reported, clustered at state level. * denotes significance at 10%, ** significance at 5%, and *** at 1%.

We see that as a result of the mandate there is a shift away from the public sector into private providers. Specifically, there is a 4 percentage point increase in the proportion of children who receive their immunizations exclusively at the doctor's office. The decline in the proportion of children who obtain all their vaccines at a public clinic is 4.33 percentage points, and this effect is still present after accounting for public insurance eligibility. This decline can be accounted for entirely by the shift to hospitals or mixed providers.[19]

VFC should also result in a shift away from public clinics to private doctors' offices and there is evidence that this has indeed been the case. For instance, in Minnesota and Pennsylvania, Zimmerman et al. (2001) find that the introduction of VFC lead to more vaccinations being administered at the doctor's office. In my analysis, I try to isolate the effect of mandates on the proportion of children seeking vaccines across different types of providers from VFC related effects by eliminating universal VFC states from the sample. After doing this, I still find that mandates led to a 3.59 percentage point decline in the proportion of children obtaining their vaccines from public clinics, and a 3.34 percentage point increase in the proportion of children obtaining their vaccines in a private, hospital, or mixed provider setting. These results are statistically significant at the 5 percent level.

The shift away from public clinics might be a positive, if indirect, health outcome to the extent that prior to the mandates some parents missed the well-child visit at the doctor's office and obtained their vaccines for free at the public clinic. Doctors are more likely to have access to the child's medical history and check for other conditions such as ear infections, and if necessary take immediate action. These same services are harder to obtain in a public clinic

[19] A child has mixed providers if she has been getting vaccines in more than one type of setting, for example some of the vaccines at a doctor's office and others in a public clinic.

where the focus is simply in administering vaccines (Joyce and Racine, 2003; Lee et al. 2007; IOM, 2004).

1.6 Discussion and Conclusion

Using policy variation across states and in the timing of mandates, I find that state mandates on private insurers to cover childhood immunizations increased the immunization rate of children for the 431 series by 1.8 percentage points. This is an important effect given that about 10% of US children have private insurance that does not cover immunizations. This effect is comparable to that found in Rein et al. (2006) for a $10 yearly per capita increase in Section 317 funding. The mandates also increased the proportion of children obtaining immunizations at a doctor's office as opposed to a public clinic, suggesting a shift in costs from the public sector to private insurers. This effect is still present after accounting for other concurrent policy changes, such as the introduction of SCHIP, increases in Section 317 funding, and the level of HMO penetration rate across states. I also provide suggestive evidence that the mandates likely affected immunization rates even after taking into account the introduction of the VFC program.

These findings have several policy implications. First, it appears that part of the population is responsive to the price of immunizations. The mandates lowered the effective out-of-pocket cost to parents and, as a possible result, immunization rates increased. By covering immunizations in doctors' offices, the mandates might also have lowered the time costs for parents who do not have ready access to public clinics or who obtain free vaccines at the public clinic but previously made separate visits to their doctor for a regular checkup. In the current situation, a mandate might be necessary even though private insurance companies have an incentive to pay for vaccines in light of their cost effectiveness relative to the expenses associated with a child falling sick. The firms however might choose not to do so if the alternative is to have the vaccines paid through a public program. This suggests that a federal mandate for the coverage of all childhood immunizations could be a viable way to increase immunization rates and achieve the 90 percent immunization benchmark.

Second, it appears that state mandates which require coverage of certain medical services can be effective if implemented without loopholes around key provisions. It is important to note, however, that my results cannot be generalized to other types of mandates. Specifically, mandates that potentially increase the cost of insurance coverage by much more than immunization mandates could instead lead to increases in premiums that drive people to forego private insurance altogether.

Third, from the public finance perspective, my findings have implications for the relative effectiveness and efficiency of different avenues of government involvement in the case of a positive externality. The government could use tax revenue and offer direct provision of vaccines, as it currently does through VFC, and/or mandate private insurance companies to cover immunizations. Summers (1989) discusses the efficiency gains from mandating benefits rather than direct public provisions and one such case is based on a positive externality argument that applies to vaccines. The efficiency gains from mandates result mainly from eliminating the deadweight loss from taxation that is necessary in the case of direct provision. Moreover, in this particular case mandates could also eliminate some of the inefficiencies that result from VFC, which increases the fragmentation of care and thus leads to missed opportunities for immunization or duplicate immunizations, as well as costs associated with doctors having to determine eligibility (IOM, 2004).

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Chapter 2

DID AUTISM CONCERNS AFFECT VACCINE TAKE UP?

2.1 Introduction

In February 1998, Dr. Andrew Wakefield and co-authors published a study in the journal *The Lancet* that suggested there might be a link between the MMR vaccine and autism (Wakefield et al., 1998). Upon publication, the study received substantial attention by the media in the UK and the US which then led to a series of hearings in the House of Representatives on vaccine-safety issues (IOM, 2001 and Vastag, 2001). Subsequent research failed to confirm this conjecture: the Wakefield study itself was retracted in February 2010 because of significant flaws and numerous large-scale epidemiological studies from around the world failed to find any evidence of such a link.[20]

In 2001, a special report by the Institutes of Medicine was assembled to review the safety of immunization practices in the U.S. and concluded that "the evidence favors rejection of a causal relationship at the population level between MMR vaccine and autistic spectrum disorders" (IOM, 2001a). While the government tried to reassure the public about the safety of the vaccine, public confidence in the safety of the combined MMR vaccine declined. In the UK, as a result of the Wakefield study the uptake of the MMR vaccine declined sharply, dropping by over ten percentage points in five years before eventually picking up again (Anderberg, Chevalier, Wadsworth, 2008).

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[20] The official retraction appears here http://press.thelancet.com/wakefieldretraction.pdf.

The case of the MMR controversy provides a rare situation where for a period of time publicized scientific research suggested potential risk of serious negative side effects for a standard medical procedure and provides a setting where to study the spread of information and how it affects behavior. Additionally, it also provides an interesting case to study the effect of education on the uptake of medical services in the context of the arrival of new information, in this case, mothers' education on their children's uptake immunizations.

In the US, previous research has found that the response of MMR take up after the spread of the controversy was less pronounced than in the UK (Smith et al., 2008). However, in the US the MMR vaccine – autism controversy coincided with expansions in the access of medical services for children and more specifically immunizations that could potentially lead to underestimates of the effect of the controversy on the population level immunization rates. These expansions include the 1999 introduction of the States Children Health Insurance Program (SCHIP), and the Vaccine for Children (VFC) program that was enacted in 1994, which specifically targets childhood immunizations. Smith, Stevenson, and Chu (2006) find that lack or breaks in insurance coverage affect childhood vaccination coverage suggesting that further analysis on the effect of the controversy that accounts for changes in access to insurance is warranted.

Two additional factors that complicate an identification strategy that compares the take up of MMR relative to other vaccines are (1) a timing spillover and (2) parents' safety concerns of childhood vaccines in general. The timing spillover refers to the fact that the controversy on the link between MMR and autism likely affected the take up of other vaccines scheduled to be administered at the same doctor's visit as MMR. The MMR-autism link might also instill doubts about the safety of vaccines in general as it coincided with the recommendation of the Academy of Pediatrics to eliminate the commonly used preservative thimerosal, which could potentially negatively affect children's mental development.[21]

To answer these questions I analyze the National Immunization Survey (NIS) from 1995 – 2006 and I find that in response to the MMR-autism link the take up of the MMR and other childhood vaccines fell, departing from an otherwise increasing trend. Even after controlling for household income, race, Medicaid/SCHIP eligibility, other demographic variables, and indirectly accounting for the increased access to health insurance in general and vaccines in particular, the uptake of the MMR vaccine declined. I also find that while there is a positive relationship between mother's education level and the child's likelihood to be immunized with the MMR vaccine, after the controversy college educated women lowered the take up of the vaccine disproportionally more than non-college educated mothers. Controlling for the number of news stories in the national and local media I find some evidence of a negative education gradient in the uptake of the MMR vaccine indicating that at least partially higher education is likely to affect health outcomes by the quicker absorption of advances in medicine.

[21] Thimerosal is a mercury related compound widely used as a preservative in childhood vaccines. In July 1999 the United States Public Health Service and the American Academy of Pediatrics called for vaccines containing the preservative thimerosal to be phased out as soon as possible. The document noted that while babies had received cumulative doses of ethylmercury

2.2 Background

2.2.1 Infant Immunizations and the MMR Controversy

The discovery and availability of vaccines to protect people from infectious diseases is widely regarded as one of the greatest public health achievements of the 20th century (IOM, 2000). Children are most vulnerable to vaccine-preventable diseases and thus obtaining appropriate vaccinations for them is especially important. Just as importantly, to prevent outbreaks of disease, the distribution of vaccinated individuals must be sufficiently uniform across geographical locations.[22]

Over the last two decades the discovery of new vaccines has led to many new additions to the recommended vaccine schedule for infants in the US. As a result since the mid-1980s, the recommended list of vaccines for children less than two years old grew from seven to thirteen vaccines in the mid 1990s and up to 24 doses of vaccines currently (IOM, 2000; Hinman et al., 2004; CDC 2010 Child Immunization Schedule). The expansion in vaccine recommendations has been met with increasing concerns from parents about their safety.[23] While some vaccines are known to cause severe adverse effects in rare cases or that certain individuals can have

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(in thimerosal) that exceeded a federal safety limit for methylmercury, its more toxic chemical cousin, there was no “evidence of harm”.

[22] Failure to do so can result in outbreaks among the unvaccinated population if they are in close proximity to each other. One such example was the measles outbreak in 2008 in San Diego which was spread by an unvaccinated 7-year-old boy who was unknowingly infected with measles during a trip to Switzerland (Sugerman et al., 2010).

[23] Although parents’ concerns about the safety of vaccines have been public since the 1980s when a group of parents of children injured by vaccines created the National Vaccine Information Center (NVIC), a vaccine watchdog group, the exact nature of concern and the problematic vaccine or vaccine component evolved over time. The original goal of the NVIC was to address the allergic reactions of some children to the DTP (diphtheria-tetanus-pertussis vaccine) which was later replaced in 1996 with the DTaP vaccine which uses acellular compounds for the pertussis vaccine and thus has fewer side effects. In 2000, the live virus polio

allergic reactions, childhood vaccines are considered safe for use in the population after considerable review from the Federal Drug Administration (FDA) that initially approves their use or the Centers for Disease Control and Prevention (CDC) that track their safety. The public however has grown skeptical over the safety of childhood vaccines as shown by a quick search over the internet or the court cases against vaccines on the Vaccine Court.[24]

However, the most widespread controversy regarding the safety of childhood vaccine came about with the publication of the Wakefield et al. study. This article received a lot of attention for a number of reasons. First, the article was published in one of the most reputable medical journals, *The Lancet*. Second, unlike other vaccine safety issues that usually applied to a very small number of children the Wakefield et al. study made a claim about autism, a condition that had become increasingly widespread in the western world. When autism was first defined as a diagnosis in the early 1960s, the prevalence of the disease in the U.S. was 4 children for every 10,000, while currently it is between 30 to 60 per 10,000 children. Part of the increase is due to the "improved ascertainment and a considerable broadening of the diagnostic concept", but true increases in the incidence of the disease possibly due to environmental risk factors have not been ruled out (Rutter, 2005).

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vaccine (OPV) was replaced completely by the inactivated polio vaccine and the vaccine paralytic polio cases were eliminated in the US.

[24] The National Vaccine Injury Compensation Program was part of National Childhood Vaccine Injury Act of 1986 and became effective October 1, 1988. It establishes the Vaccine Program as a no-fault compensation scheme whereby persons allegedly suffering injury or death as a result of the administration of certain compulsory childhood vaccines may petition the federal government for monetary damages. Congress intended that the Vaccine Program provide individuals a swift, flexible, and less adversarial alternative to the often costly and lengthy civil arena of traditional tort litigation. Description available at http://www.uscfc.uscourts.gov/vaccine-programoffice-special-masters.

After the initial Wakefield et al. study, the claim of the link between MMR and autism was repeated in spring 2001 (Wakefield and Montgomery, 2001), and in 2002 (Uhlmann et al., 2002). These studies hypothesized that the MMR vaccine causes autism as the measles virus lodging in the intestine of some children releases gut-brain mediators or toxins that lead to autism (Wakefield et al., 2002).[25] In the Wakefield studies, the culprit was the MMR vaccine while other measles containing vaccines that were not combined with mumps or rubella were thought to be harmless.

As a response to these claims, many epidemiological studies were conducted but failed to confirm the hypothesis proposed by Wakefield and his coauthors. Taylor et al. (1999) found no jumps or deviations from the trend in the occurrence of autism with the introduction of the MMR vaccine in the UK. Gillberg and Heijbel (1998) found no differences in the incidence of autism after the introduction of MMR in Sweden, while Honda, Shimizu and Rutter (2005) consider the withdrawal of the MMR vaccine in Japan for reasons unrelated to the autism controversy and found that the increasing trend of children diagnosed with autism persisted. Madsen et al. (2002) used data on all children born in Denmark between January 1991 and December 1998 and did not find increased autism risk for the vaccinated relative to unvaccinated children. These and many other studies have been reviewed on a number of occasions and all have concluded that there is no causal relationship between the MMR vaccine and the incidence of autism (IOM 2001a, 2004; Halsey, et al., 2001; UK Medical Research Council, 2001; Demicheli, et. al., 2005).

[25] Another hypothesis related to MMR vaccine is that children with autism have immune abnormalities in their central nervous system that could be vaccine induced, and the immune-mediated damage could lead to autism (Singh et al., 2002).

2.2.2 Previous Literature on the MMR-autism Controversy

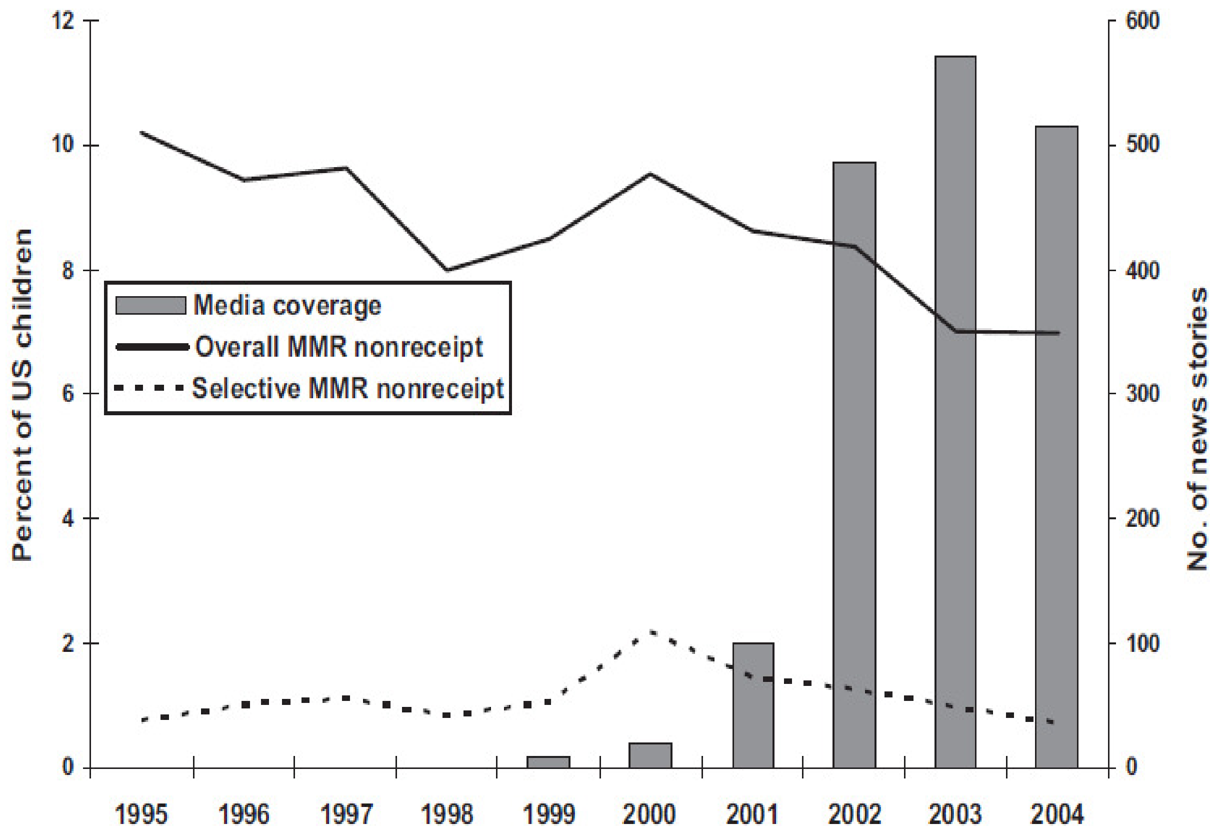
Two previous studies have looked at the response in MMR take up after the MMR-autism controversy: Smith et al. (2008) in the US; and Anderberg, Chevalier and Wadsworth (2008) in the UK. Smith et al. (2008) found a decline in the MMR uptake in the US in 1999 and 2000, and a return to previous levels of vaccination in 2001 and even increases to pre 1998 levels in 2002 and beyond. They also examine the decline relative to the publication of news stories on US media (in 295 newspapers, Associated Press newswire releases and major TV stations), and find no relationship since the news stories about the link increased substantially only in 2001 and the decline in take up was observed in 1999 and 2000. Figure 2.1 shows their main findings.

Anderberg, Chevalier and Wadsworth (2008) on the other side, study the uptake of the MMR vaccine in the UK after the publication of the Wakefield et al. (1998) article. They establish that the pattern of news articles overtime affected the take up of the MMR vaccine, and moreover find that more-educated parents responded more quickly to the information on the possible MMR-autism link, even after controlling for household income and additional variables.

2.2.3 Confounding Factors

Concurrently with the MMR-autism controversy there were a number of policies enacted in the US that were likely to increase immunizations especially for the poor and if not accounted might lead to underestimating the effect of the MMR autism-controversy. These policies include the expansions in Medicaid eligibility, the introduction in 1997 of the State Child Health Insurance Program (SCHIP) and the Vaccines for Children Program (VFC) in 1994 as well as legislation passed by fourteen states mandating private insurance coverage of childhood vaccines

Figure 2.1 MMR Non-receipt and Media Coverage



Notes: MMR nonreceipt and media coverage according to NIS survey year. Overall MMR nonreceipt indicates children who did not receive the MMR vaccine; selective MMR nonreceipt, children who received 3 Hepatitis B, 3 polio, 4 diptheria-tetanus-acellular pertussis and 3 Haemophilus influenzae type b vaccines but not the MMR vaccine; news stories, all newspaper stories, television programs, and radio programs that reported the MMR-autism controversy to which children in each year's NIS may have been exposed.

Source: Figure 1 in Smith, Ellenberg, Bell and Rubin (2008).

between 1995 and 2002. SCHIP was enacted by most states in mid to late 1998 and made health insurance available to many younger children and covered all childhood immunizations (Joyce and Racine, 2005). Moreover, in 2000 most states expanded Medicaid and SCHIP income eligibility thresholds for the one to five year olds that increased insurance rates among the eligible, a move also likely to affect immunizations (LoSasso and Buchmueller, 2004).

The VFC program was established in 1994 and made available for free to providers such as doctor's offices and hospitals, physical vaccines from the states' public health departments to administer in their offices to qualifying children. The program covers children under 18 years old who are either eligible for Medicaid, uninsured or underinsured for vaccine coverage, Native American or Alaskan Native, or receive medical services at a federally qualified health care center (IOM, 2000). While to my knowledge there is no data available on the number of children overtime who have received their vaccines as part of this program, by 2003 about half of all childhood vaccine purchases in the US were conducted as part of VFC (Hinman, Orenstein and Rodewald, 2004). Moreover, on or after 1995, fourteen states also introduced state mandates on private insurers requiring coverage of childhood immunizations for all their policies. Five states, Delaware, Illinois, Mississippi, Virginia and Wisconsin enacted these mandates just as the controversy became more widespread in 1999 and 2000.

Another development that fanned the flames of the vaccine autism controversy was the announcement by the United States Public Health Service and the American Academy of Pediatrics in July 1999 that called for childhood vaccines containing the preservative thimerosal to be phased out as soon as possible. The document noted that while babies had received cumulative doses of ethylmercury (in thimerosal) that exceeded a federal safety limit for methylmercury, its more toxic chemical cousin, there was no evidence of harm.

Thimerosal had been used in infant vaccines since the 1930's as a preservative to prevent microbial growth during manufacturing or after the initial opening of multi-dose vial. Since 2001 however, all vaccines manufactured for the U.S. market and routinely recommended for infants contain no thimerosal or only trace amounts.[26] The final IOM (2001b, p.10) report on the safety of vaccines and the possible effect of MMR and thimerosal on autism, concluded that the body of epidemiological evidence favors the rejection of a causal relationship between thimerosal-containing vaccines and autism and that "potential biological mechanisms for vaccine-induced autism that have been generated to date are theoretical only."

2.2.4 Role of Education in Health Outcomes and the Absorption of New Information

A large literature has documented the positive correlation between an individual's education and health status. Grossman (1972) identifies some of the ways according to economic theory that can lead to better health for highly educated individuals. One of these explanations pertains to the allocative efficiency in the health production that results from higher educated individuals absorbing more quickly and better incorporating information that influences their health behavior.

A number of studies have tested this hypothesis empirically as they consider the rate of adoption of new medical technologies by the more highly educated, but the evidence is mixed. Lleras-Muney and Lichtenberg (2005) for instance found that the more educated were more likely to use recently approved drugs by the FDA while Goldman and Smith (2005) did not find different uptake rates after the introduction in the 1970s of a number of hypertension drugs by

[26] The description in this paragraph is a summary of the information provided from the FDA on the website: http://www.fda.gov/BiologicsBloodVaccines/vaccines/QuestionsaboutVaccines/ucm070430.htm

the college educated. Cutler and Lleras-Muney (2010) find that as much as up to 10% of the education gradient can be attributed to specific factual knowledge, however about 20% of it can be attributed to the way people absorb and use the information in line with their cognitive ability which is affected by education. Price and Simon (2009) examined how specific information published in a peer review journal about the increased risk of vaginal birth after caesarian, affected the probability of women having a vaginal birth if they had a C-section before, and found that more educated women experienced the largest change in treatment in response to the new medical information.

Other studies have found that mothers' education causally affects child health outcomes through a number of possible pathways (Thomas, Strauss and Henriques, 1991; Duflo, 2000; Currie and Moretti, 2003; Chou et al., 2010; Chevalier and O'Sullivan, 2007).[27] Thomas, Strauss and Henriques find evidence that more-educated mothers in Brazil with better access to information such as newspapers and radio are more likely to have taller children, an attribute that is a biomarker of healthy child development. Other pathways discussed by Currie and Moretti (2003) relate college education of women with an increased use of prenatal care, and reduced parity and smoking.

[27] A detailed review of the literature is provided by Currie (2009).

2.3 Data

2.3.1 National Immunization Survey

The data I use comes from the National Immunization Survey (NIS) for the years 1995 through 2006. NIS is a yearly national probability sample of children aged 19 to 35 months and averages about 30,000 children each year. The age group threshold to participate in the NIS reflects the fact that all recommended infant immunizations should be completed by the 18th month of age.[28] The NIS uses random-digit-dialing to identify households with children of the appropriate age and then surveys the most knowledgeable person about the child's immunizations. The survey asks respondents to locate a shot card if available and answer additional questions about maternal schooling, family income, marital status, and other socio-demographic information. Table 2.1 shows summary statistics for these variables and if there is a linear trend overtime.

At the end of the household interview, NIS asks respondents for permission to contact the child's immunization providers. Over the 12 years in my sample I find a 69% consent rate. The second part of the NIS is the provider record check mail-in survey, which obtains provider-reported vaccination histories. Provider response rate in the NIS is very high at an average of 93%. The provider data is more reliable than parents' recall or shot-card information, but in 2006 only 70.9% of the children surveyed had complete provider data. These children are more likely to be white, have better educated parents, and live in households with higher incomes than those without provider data. The NIS makes adjustments for these factors by providing post

[28] The survey drew its sample from 78 strata until 2004, representing 50 states and 28 metropolitan areas. The number of strata increased to 83 in 2005, and then to 90 in 2006. Each household is drawn randomly within each stratum.

Table 2.1 NIS Descriptive Statistics

Variable	Sample Size	Mean [Std. Error]	Annual Trend [Std. Error]
Number Children in Household	363,131		
1 child		0.27	-0.0044***
		[0.0015]	[0.0005]
2 or 3 children		0.598	0.0018***
		[0.0017]	[0.0005]
4 or more		0.131	0.003***
		[0.0013]	[0.0004]
Gender	363,893		
Male		0.512	-0.0001
		[0.0017]	[0.0005]
Race/Ethnicity	363,893		
White		0.553	-0.0099***
		[0.0019]	[0.0005]
Black		0.146	-0.004***
		[0.0013]	[0.0004]
Hispanic		0.233	0.0096***
		[0.0016]	[0.0005]
Other		0.067	0.0044***
		[0.0009]	[0.0003]
Age in Months	363,893		
19-23 months		0.298	0.0005
		[0.0016]	[0.0005]
24-29 months		0.35	-0.0007
		[0.0017]	[0.0005]
30-35 months		0.353	0.0002
		[0.0017]	[0.0005]
Moved from State of Birth	362,440	0.091	-0.0016***
		[0.001]	[0.0003]
Firstborn	298,591	0.408	0.003***
		[0.002]	[0.0006]
Mother's Marital Status	363,893		
Single mother		0.206	0.0049***
		[0.0015]	[0.0004]
Married mother		0.707	-0.0031***
		[0.0017]	[0.0005]
Widow/Divorced/Sep'd/Dec'd		0.086	-0.0013***
		[0.001]	[0.0003]

Table 2.1 (cont'd).

Mother's Education	363,893		
College Degree or more		0.288	0.0034***
		[0.0014]	[0.0004]
Some College		0.169	0.001***
		[0.0012]	[0.0004]
High School		0.363	-0.008***
		[0.0017]	[0.0005]
Less than High School		0.179	0.0036***
		[0.0015]	[0.0005]
Mother's Age	298,591		
Less than 20		0.034	-0.0014***
		[0.0007]	[0.0003]
Between 20 -29		0.45	-0.004***
		[0.0019]	[0.0007]
More than 30		0.517	0.005***
		[0.0019]	[0.0007]
Income	310,765		
Income less than 20K		0.294	-0.0053***
		[0.0018]	[0.0005]
Income between 20K and 50K		0.366	-0.0081***
		[0.0018]	[0.0005]
Income more than 50K		0.339	0.0133***
		[0.0016]	[0.0005]
Poverty	312,834	0.256	0.0035***
		[0.0017]	[0.0005]
Metropolitan Area	363,893	0.202	-0.002***
		[0.0007]	[0.0004]
Number Providers per Child	263,937		
One Provider		0.831	0.0007*
		[0.0013]	[0.0004]
Two Providers		0.153	-0.0009**
		[0.0012]	[0.0004]
Three or more Providers		0.0156	-0.0001
		[0.0004]	[0.0001]

Table 2.1 (cont'd).

Provider Type	216,991		
Public		0.178	-0.0073***
		[0.0014]	[0.0004]
Hospital		0.074	0.0046***
		[0.001]	[0.0003]
Private		0.647	0.004***
		[0.0018]	[0.0005]
Military		0.021	-0.0009***
		[0.0006]	[0.0002]
Mixed		0.079	-0.0006**
		[0.001]	[0.0003]
Medicaid/SCHIP eligibility	204,016	0.359	0.0183***
		[0.0017]	[0.0005]
Up to Date MMR	245,842	0.915	0.0022***
		[0.001]	[0.0003]

Notes: NIS provider weights and strata information used. Medicaid/SCHIP eligibility estimated using income and household information from NIS and state eligibility rules for each year. Mother's age and firstborn status available only since 1997. * denotes significance at 10%, ** at 5% and *** at 1%.

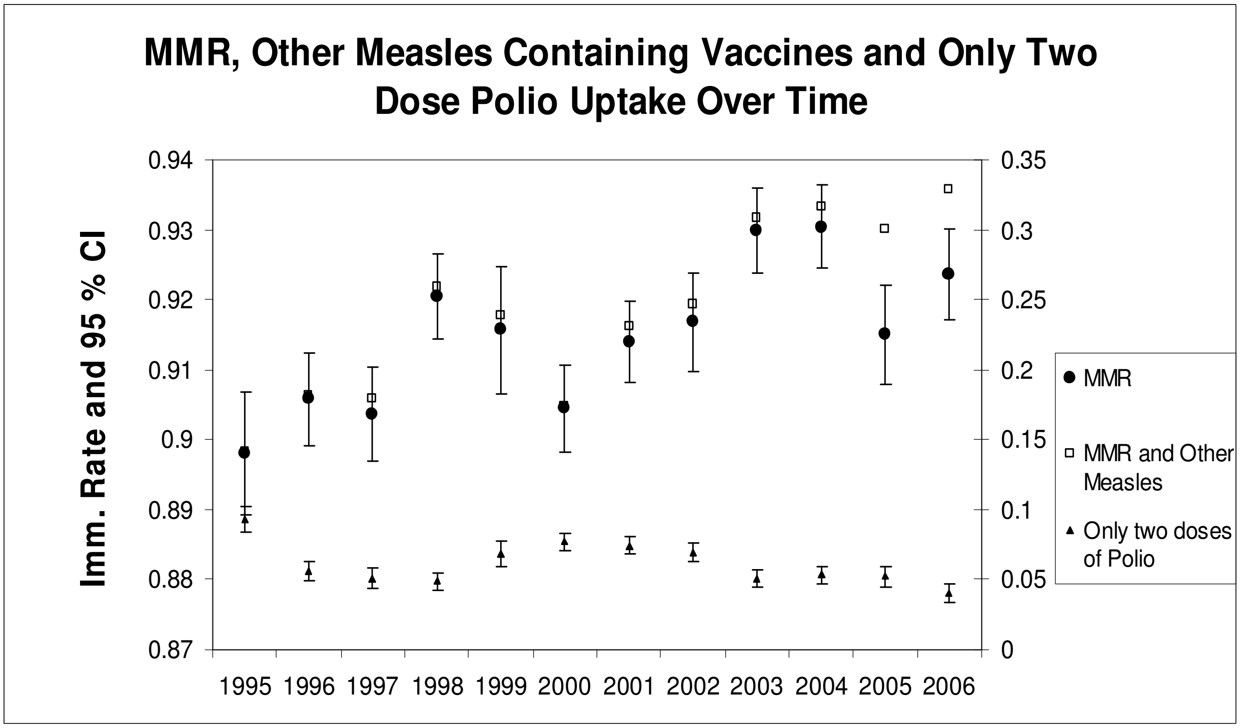
stratification weights within each stratum to adjust for the characteristics of households with non-provider data. I use these weights in my analysis.[29]

Although the NIS asks detailed questions about the immunization status of a child, it does not ask any questions about insurance status. Moreover, only in 2005 did the NIS start asking providers whether they participated in the VFC. This lack of information does not allow using insurance information or VFC participation directly in the analysis and thus will need to resort to indirect ways of controlling for trends in insurance coverage and access to the VFC program.

Throughout its duration NIS has collected information about the MMR vaccine take up and other measles containing vaccines. The take up for other measles containing vaccines excluding MMR became more predominant after 2001, likely as a response to the increased demand by parents on taking the measles, mumps and rubella shots separately. Figure 2.2 shows the take up for the MMR and other measles containing vaccines indicating an increase from 0.02 percent in 1995 to 0.15 percent in 2001 for non MMR measles vaccine. However, MMR is still the most commonly used vaccine to immunize children for measles. The supply of the separate measles shot is limited and many doctors do not have them available in their offices.[30]

[29] Smith et al. (2001) describe the NIS dataset in greater detail.
[30] http://www.immunize.org/askexperts/experts_mmr.asp

Figure 2.2 Fraction of Infants Up to Date with MMR, All Measles Containing Vaccines and Only Two Doses of Polio



Notes: Data comes from the National Immunization Survey. Sampling weights and strata used in the calculations and confidence intervals shown. Years refer to the interview years from NIS. Measles containing vaccines include the combined MMR vaccine and measles only shots. The immunization rates are shown with the axis on the left. The take up for the only two doses of polio is shown with the axis on the right.

2.3.2 Media and Newspaper Data

Public coverage of an autism vaccine link was almost nonexistent in the national or local media prior to the publication of the Wakefield et al. study, as shown in Figure 2.1 (Smith et. al., 2008). After the journal publication and the press conferences that followed, the main newspapers in the UK and the BBC covered the story substantially almost immediately but in the US there was a lag until almost a year later when stories abounded in the press as well as on TV and radio with shows from *60 Minutes*, *20/20*, *Nightline* and features on the National Public Radio stations.

The media during this time period played an important role in diffusing information that parents used to make decisions on vaccines for their children. Gellin et al. (2000) for instance found newspapers and magazines (18.1%) to be the second most used source of information after the doctor (84.2%) in the US in 1999. Internet was mentioned as a source of information by only 7% of the parents at the time, but that number is believed to have grown substantially since then (Smith et al., 2008; Sugerman et al., 2010).

As a measure of the spread of information regarding the possible link between the MMR vaccine and autism, I count how many times the words "vaccine" and "autism" were mentioned in the same article in a newspaper, wire service and transcripts of TV shows from the NewsLibrary database from 1995 until 2006. I review the outcomes of these searches in five different newspapers and the results are discussed in Appendix A. NewsLibrary archives information from all these sources, and most importantly identifies newspapers based on the state of publication.[31]

[31] The reason for choosing the more general keywords of "vaccine" and "autism" versus the more specific terms "MMR" and "autism" or a mentioning of the Wakefield study, is that many articles especially in the local media did not explicitly use these terms even though they were

For national data I surveyed eight newspapers on which there is archive information available since 1995, six wire and transcript sources for the whole time period that include Associated Press, National Public Radio and also all available transcripts which include CNN, CBS, Fox News and MSNBC starting from 2003. Figure 2.3 shows the number of search results for the different national and state sources. Appendix A provides additional details on the mechanics of these searches, the effectiveness of the keyword terms and the sources of information. The findings on the national level MMR media coverage until 2004 resemble those by Smith et al. (2008) who use as a source of their news data the LexisNexis news database.

2.3.3 BRFSS

The NIS does not ask about parents' attitudes towards vaccines or their perceptions for their safety, although an exception was a special module asked in 2003 and 2004 to a subsample of the parents interviewed for NIS (Gust et. al., 2008). This data is not available publicly, so instead I examine a publicly available dataset that has information on parents' attitudes towards vaccines – a 2006 special module of the Behavioral Risk Factor Surveillance System (BRFSS).

The BRFSS is an annual telephone-based survey commissioned by the CDC and is designed to generate state-level data on the prevalence of important health behaviors. A children immunization module is conducted by a few states every year and the respondents are asked whether the child has received the flu vaccine. In 2006 in an attempt to assess the attitudes and reasons behind the decision not to vaccinate, some states included a question on why the flu shot was not received by young children with one of the possible responses being its safety.

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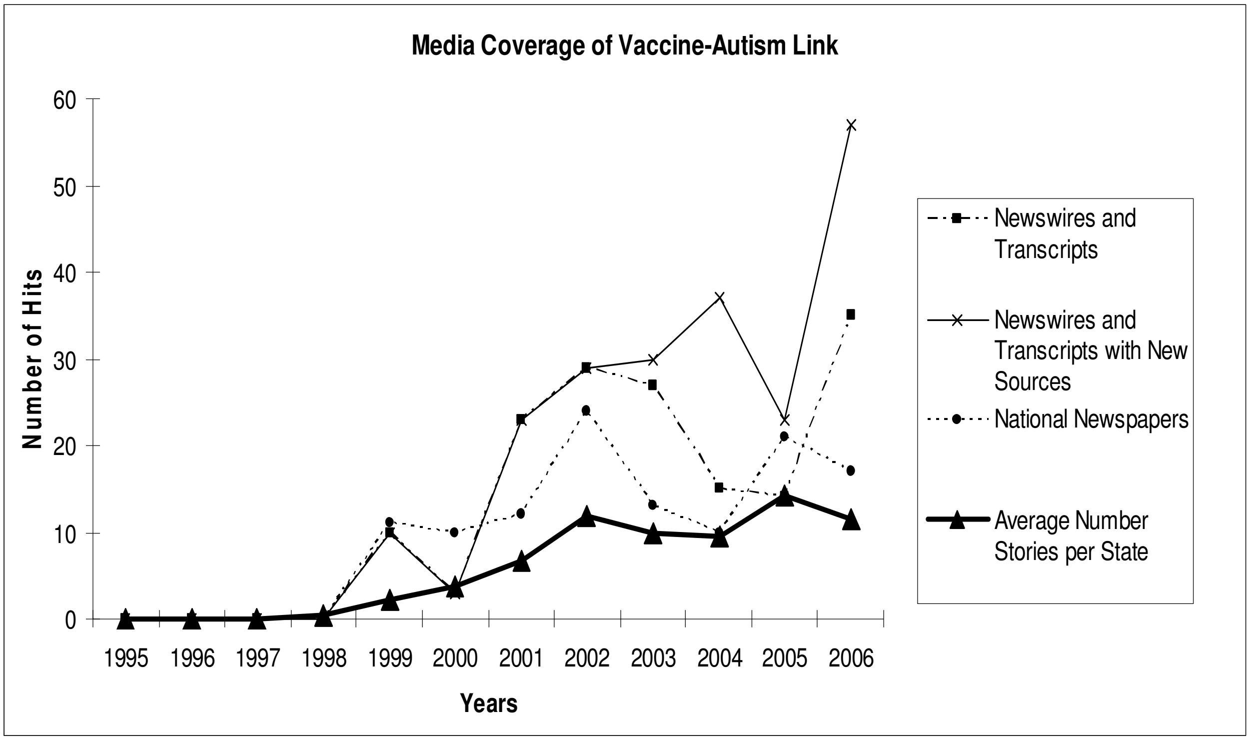
making reference to the MMR – autism link. Identifying individually which article is referring to the autism-MMR link indirectly would be an insurmountable task. Moreover, in light of the fact that it is hard to establish whether the response in the MMR take up was unrelated to the

Figure 2.3 Number of Stories Mentioning the Vaccine-Autism Link in the National Media



Notes: News Sources refer to hits on media outlets that were not present in the NewsLibrary database prior to 2003 such as CNN, CBS, Fox News and MSNBC.

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thimerosal or multiple vaccines hypotheses, I will consider the response in MMR take up to the overall vaccine safety concerns expressed in the media.

2.4 Did the Vaccine-Autism Controversy Affect MMR Take Up?

Figure 2.2 shows the take up over time for the MMR vaccine and other measles containing vaccines including MMR and the take up of only two doses of polio out of the recommended three. The MMR take up reflects an increasing trend absent the dip around the year 2000, while the change in the take up of only two doses of polio around the time of the controversy indicates the spillover effect that the controversy had on the take up of other vaccines. Figure 2.2 also indicates the take up of measles only vaccine relative to the combined MMR and shows that the two series follow each other pretty closely suggesting that most of the measles containing vaccines were received as MMR. A slight discrepancy appears after 2002 that grows over time and become statistically significant in the later years, suggesting a substitution away from MMR and into other types of measles vaccines.[32]

Figure 2.4 shows the uninsurance rates for children based on reports from the Current Population Survey publications between 1995 and 2006 indicating an increase in access to health insurance that likely affected vaccine take up. While I cannot account for insurance coverage in the NIS, using eligibility rules by state and time and income category information I estimate eligibility for public insurance and show in Figure 2.4 that my sample was also affected by changes in access to health insurance.[33]

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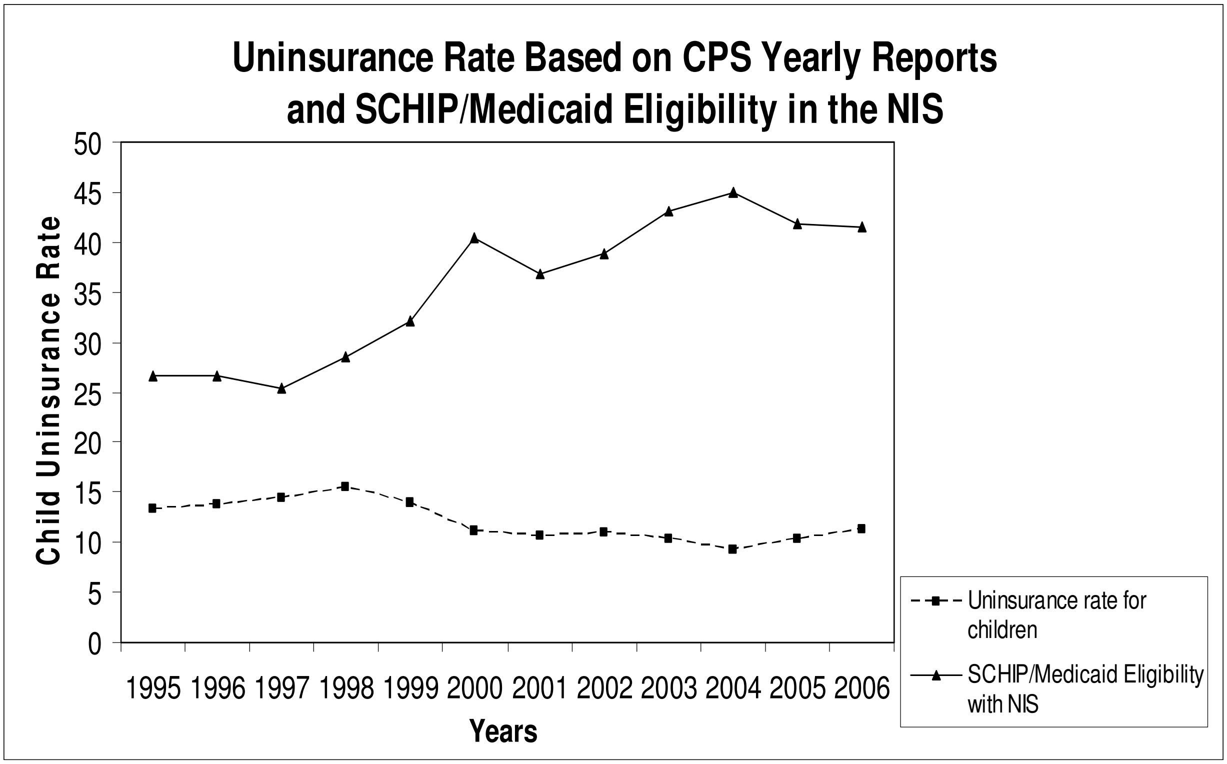
[32] While other measles containing vaccines aside from MMR have been available from pharmaceutical companies for the whole time period, their availability at most doctors' offices is rare since doctors usually obtain the recommended vaccines that appear in the official schedule which only mentions MMR as a multivalent vaccine.

[33] I estimate eligibility for public insurance using income category information from NIS and state's eligibility rules by year for children less than five years old available in LoSasso and Buchmueller (2004). I assign to each household the median income in their respective categories

Figure 2.4 Fraction of Uninsured Children in the US based on CPS data and Public Insurance Eligibility based on NIS data



Notes: Uninsurance rate is measured for children less than 6 years old based on historical estimates from the Current Population Survey publications. Public insurance eligibility is estimated using income information categories from the NIS and Medicaid and SCHIP eligibility rules by state and year for children between 1 and 5 years old from LoSasso and Buchmueller (2004). I report here only the results based on allocating each household, the 75 percentile of the income in it's income category, the overall trends and jump in eligibility in 2000 is similar when I assign the median of the income category as household income.

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and the overall trends are the same even if I assign the corresponding 75th percentile of the category.

2.4.1 Time Trend Analysis

The results of a formal analysis on the effect of the controversy are presented in Column (I) of Table 2.2 shows the results from estimating the following regression:

$$MMR_{ist} = \beta_1 After_t + \delta X_{ist} + \gamma state_s + \varepsilon_{ist}, \tag{1}$$

where MMR_{ist} indicates whether child *i* in state *s* in year *t* received the MMR vaccine. $After_t$ refers to the period on or after the year 2000, X_{ist} is a vector of individual control variables available from the NIS such as child's gender, race, indicator variables on the child's age group, mother's college degree, marital status, household's income and number of children categories, poverty status, whether the child moved from state of birth, whether the household lives in an urban area and the number and type of providers that respond with the child's immunization information. $State_s$ is a set of state fixed effects controlling for state level unobservables. In the first specification the sample includes only four years, two years before and after the beginning of the controversy so as to extrapolate from overall trends.

Column (I) shows that there was a significant decline of about 1.6 percentage points in the likelihood of an infant's MMR take up right after the controversy. I define the NIS interview year 2000 to be the year when we should expect any differences since there was no major news in the US until 1999 and since depending on a child's age and the interview time, the parents who likely made decisions about their child's MMR status in 1999 appear in the NIS data in 2000 at the earliest. This comparison can partially account for the overall increases in access to insurance and vaccines, but unfortunately is not ideal since as shown in Figure 2.4 the largest increase in eligibility for public insurance and the largest decrease in the uninsurance rate for

Table 2.2 Time Trend Analysis of MMR Takeup

	(I) Two Years Before/After 1998-2001	(II) Full Sample Linear Trend	(III) Separate Years Linear Trend	(IV) Full Sample Quadratic Trend	(V) Separate Years Quadratic
After	-0.0156 ***	-0.0144 ***	---	-0.0229 ***	---
	[0.0036]	[0.0043]		[0.0049]	
Before 1 Year	---	---	0.0073	---	-0.0019
			[0.0052]		[0.0056]
After Year 0	---	---	-0.0091 ***	---	-0.0190***
			[0.0037]		[0.0054]
After Year 1	---	---	-0.0074 **	---	-0.0170***
			[0.0037]		[0.0057]
After Year 2	---	---	-0.004	---	-0.0136**
			[0.0043]		[0.0053]
After Year 3	---	---	0.0136 ***	---	0.0049
			[0.0035]		[0.0042]
Trend	---	0.0039 ***	0.0019 ***	0.0101 ***	0.0011***
		[0.0006]	[0.0037]	[0.0021]	[0.0024]
Trend Square	---	---	---	-0.0004 ***	-0.0006***
				[0.0001]	[0.0002]
Controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
R Square	0.0235	0.0218	0.022	0.022	0.023
Sample Size	59,382	187,756	187,756	187,756	187,756

Notes: NIS provider weights and strata information used. Controls include demographic information available in the NIS like child's gender, race, age categories, mother's education level, marital status, households income categories, number of children in household, poverty status, whether the child moved from state of birth, urban area, number and type of providers. * denotes significance at 10%, ** 5% and *** at 1%.

children happened in 2000 and 2001. My results are then likely to be underestimates of the actual effect. Controlling for state fixed effects in every specification is important as an indirect way of accounting for the variation at the state level in the establishment and the way SCHIP/Medicaid and VFC are run. In fact, adding to the list of controls estimates of individual SCHIP/Medicaid eligibility based on NIS data, state's uninsurance rate and unemployment level, I estimate the decline in MMR take up to be 1.7 percentage points with a standard error of 0.0038, statistically different from zero at the 1 percent level. In the full sample results, I allow for different effects in MMR take up for the first four individual years in the "after" period and control for a linear and quadratic trend in take up. The results are presented in Columns (II) through (IV) and we see that the negative take up of MMR died out in the overall population by the year 2003.

Allowing for trends in vaccine take up by parameterizing the behavior overtime with a linear and quadratic trend addresses the increased access to insurance and the missing information on insurance status. Under this specification absent the MMR-autism controversy the positive trends in take-up that we observed before and after the controversy would have continued even in the very few years after 2000. These positive trends would have likely been sustained by the increased eligibility and enrollment in SCHIP/Medicaid and the spread of VFC. Using a quadratic trend makes sense as the increase in MMR take up would likely be at a decreasing rate especially as MMR take up approaches 100%. We see with the analysis at the individual years that by the third year of the controversy the take up approached the pre - controversy trend line. I include the year right before as a test of assignment for the "after" period. The results suggest that the year before the controversy would fit in the overall trend in MMR take up.

I find a positive and statistically significant trend in the likelihood of children being up to date with the MMR vaccine by the time of interview with the full sample, even after controlling for state varying controls such as lag unemployment rate, percentage of children under 18 who are uninsured and those who might be eligible for Medicaid and SCHIP, although as expected the magnitude of the trend coefficients decline after the inclusion of these control that account more directly for the changes in access.

Two additional controls mothers' age and whether the child is the firstborn are not available in 1995 and 1996 but even after using these variables known to affect vaccine take up we do not find substantially different effects with the remaining years for which this information is available. I also estimate all the regressions in Table 2.2 using a logit functional form specification to take into account the binary nature of the dependent variable. The findings are not presented here for brevity, but they are consistent with the OLS results reported here.

In the analysis presented, I have used the old strata framework available in the NIS and redefined the observation in the new strata introduced in 2005 and 2006 as parts of the old strata and sampling framework in the years prior. However, the findings are similar when I conduct the analysis by eliminating all the observations from the new strata or restricting the sample to the years with the old sampling framework and drop from the sample the years 2005 and 2006.

2.4.2 Differencing Strategies

General time trends cannot account for specific factors such as the thimerosal controversy that coincided with the MMR-autism controversy. One possible way to address underlying time trends and the thimerosal issue is to consider MMR uptake relative to other vaccines that would be similarly affected by the thimerosal-autism controversy. The problem with such an analysis would be to find a vaccine that would not suffer from spillover effects of the MMR-autism controversy on its own take up. Anderberg, Chevalier, and Wadsworth (2008) found that the MMR controversy spilled over on the take up of other vaccines in the UK where there was no contemporaneous thimerosal issue. This might be because parents doubt the safety of vaccines in general and are being cautious or because the parents probably miss the doctors' visit altogether as they delay or do not take the MMR vaccine for their children. Figure 2.2 shows evidence of the timing spillover on the take up of the third dose of polio in the US. The third dose of polio is usually administered at the same well-child visit to the pediatrician as the MMR vaccine. This is unfortunate as the take up for the third dose of polio would have been a good candidate to compare the MMR take up since both vaccines never had thimerosal and even if some parents were not aware of this both vaccines would likely have been affected the same way by their beliefs on thimerosal.

So there are three potential sources of spillover of the controversy on the take up of other vaccines, the timing spillover to the vaccines administered in the same visit as the MMR, the thimerosal controversy, and the overall concern about childhood vaccine safety that the MMR-autism controversy ignited. I address these concerns in my estimation in two ways, although none of the approaches is ideal and one of them would largely underestimate the effect of the MMR controversy.

An alternative vaccine that has the advantage of not being recommended to be administered at the same time as MMR, but has also been available for a long time so would be affected by increased access in a similar way as the MMR is the DTaP vaccine. More specifically, the DTaP vaccine is administered in four doses, the first on the second month of life, the second on the fourth, the third dose on the sixth month of life and the fourth dose between the 15th and 18th month of life. The MMR vaccine is recommended to be administered between the 12th and 15th month of life so even if parents are avoiding the recommended doctor's visit on the 12th month of life because they have concerns about MMR, this should not affect very much the take up of the fourth dose of the DTaP vaccine. The fourth dose of DTaP however has a slight disadvantage as far as thimerosal is concerned as some of the manufacturers used thimerosal while others didn't: of the six different DTaP vaccines manufactured by different pharmaceutical companies, only two contained thimerosal prior to 1999. To test the relative difference in take up between the MMR vaccine and the fourth dose of DTaP, I estimate regressions of the form:

$$MMR_{ist} = \beta_1 After_t + \beta_2 DTaP4_{ist} + \beta_3 After_t \cdot DTaP4_{ist} + \delta X_{ist} + \gamma state_s + \varepsilon_{ist}, \qquad (2)$$

where $DTaP4_{ist}$ indicates whether child i in state s in year t received the 4th dose of the DTaP vaccine. If the controversy had a negative impact on MMR take up relative to DTaP we should expect a negative coefficient on the interaction term. Children now have access to the medical system and are able to get 4 doses of DTaP, so they should have gotten the MMR vaccine which is usually administered before or at the same time as the fourth dose of DTaP. If some parents refused the MMR but received all four doses of DTaP they probably did not suffer from lack of access or had no general concern about the safety of vaccines, but did so most likely due to the MMR controversy. Results are presented in Table 2.3. I consider the immediate timing before and after the controversy with observations from four years, full sample results as well as separate results by each individual year. The evidence seem to suggest that after the controversy the MMR take up fell substantially relative to the full uptake of the DTaP vaccine and persisted for at least the first four years after. As expected, this approach finds a bigger effect of the controversy on the MMR uptake than the time trend analysis as it better controls for changes in

Table 2.3 MMR Take up relative to the Take up of the Fourth Dose of DTaP

	(I) Two Years Before/After 1998-2001	(II) Full Sample 1995-2006	(III) Full Sample Separate Years
4 Doses of DTaP	0.384*** [0.0129]	0.393*** [0.0079]	0.381*** [0.0069]
After	0.0347* [0.0182]	0.0421*** [0.0102]	---
After*4 Doses DTaP	-0.0481*** [0.0183]	-0.0495*** [0.0102]	---
Before 1 Year	---	---	0.0071 [0.0236]
After Year 0	---	---	0.0470** [0.0195]
After Year 1	---	---	0.0264 [0.0192]
After Year 2	---	---	0.0531*** [0.0200]
After Year 3	---	---	0.0690*** [0.0188]
Before 1 Year * 4 Doses DTaP	---	---	-0.0053 [0.0238]
After Year 0 * 4 Doses DTaP	---	---	-0.0609*** [0.0198]
After Year 1 * 4 Doses DTaP	---	---	-0.0328* [0.0193]
After Year 2 * 4 Doses DTaP	---	---	-0.0612*** [0.0202]
After Year 3 * 4 Doses DTaP	---	---	-0.0679*** [0.0189]
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Sample Size	59,382	187,756	187,756
R- Square	0.264	0.253	0.257

Notes: The dependent variable is whether the child has taken the MMR vaccine. NIS provider weights and strata information used. Controls include demographic information available in the NIS like child's gender, race, age categories, mother's education level, marital status, households income categories, number of children in household, poverty status, whether the child moved from state of birth, urban area, number and type of providers. After Year 0 refers to the year 2000, after year 1 to 2001, after year 2 to 2002 and after year 3 to 2003. * denotes significance at 10%, ** 5% and *** at 1%.

access. According to these results, the MMR-autism controversy led to a 5 percentage points decline in the likelihood of infants getting the MMR shot.

Surprisingly, the separate analysis by each individual year indicates that this effect actually was the largest in 2003, which is different from what we find in the time trend analysis. By the year 2003, there were already a few studies that did not support the MMR autism link and they were already being publicized in the media, so we should expect the gap between the take-up in MMR and DTaP to start closing up and not increase up to almost 6.8% percentage points.

One approach that addresses increased access, the thimerosal spillover as well as safety concerns is considering how did the MMR-autism controversy affect the number of children that obtained all three polio vaccines and all four DTaP vaccines, but not the MMR vaccine. The children that missed the MMR but not the other doses most likely did not have an access issue and were unlikely to have missed the vaccine due to thimerosal since they obtained the DTaP which was more likely than MMR to have thimerosal. This strategy however is likely to be a n underestimate due to the "timing" spillover since we are eliminating from the sample those children who missed the third dose of polio and the MMR vaccine.

I estimate regressions of the following form where the dependent variable is 1 if the child got all 4 doses of DTP, 3 doses of polio and 1 MMR and 0 if the child got all the other vaccines except for the MMR:

$$431_{ist} = \beta_1 After_t + \delta X_{ist} + \gamma state_s + \varepsilon_{ist}. \tag{3}$$

In this specification the sample is restricted to those children who obtained all four doses of DTaP, three doses of polio and one of dose of MMR and those who only missed the last one. Children who missed one or more of the DTaP or polio shots are eliminated from the sample. If there was a response to the MMR-autism controversy we should expect β_1 to be negative. For completion I report the results using only the first few years before and after the controversy as well as looking at the results by individual year right after the controversy spread. These findings are reported in Table 2.4 and seem to support the evidence presented so far indicating that despite the increases in overall access for the population of children as well as the separate effect of thimerosal on vaccine take up, the MMR-autism controversy had an effect on the take up of the MMR vaccine. In the two years after the controversy, the likelihood that a child got the four doses of DTaP and three doses of polio but missed MMR increased by 1.3 percentage points. This effect is still prevalent after expanding the sample to the twelve years available. The individual year analysis shows that the largest effect was in the year 2000 with the likelihood to avoid MMR increasing to as much as 1.33 percent and the effect being smaller for the rest of the years. These estimates however are smaller than what we found after differencing relative to the fourth dose of the DTaP vaccine. This is expected as we are now not taking into consideration the spillover from the timing of the MMR and third dose of polio. This estimate is also lower than that from the time trend analysis suggesting that the timing spillover effect might be quite large.

2.4.3 Number of News Stories in the Media and MMR Take up

As a final test of the response to the controversy I try to establish whether the spread of the news via local media had an effect on the take up rate of MMR and selective avoidance of MMR. The variation used in the news data is state-year and unfortunately I am not able to model separately the negative news referring to the link of MMR with autism and the positive information that disclaimed this link, but given that most of the research against this association was published by 2004, we should expect that after this time the majority of news stories would

Table 2.4 Selective avoidance of MMR – Completing the 431 Series Except for the MMR Vaccine

	(I) Two Years Before/After 1998-2001	(II) Full Sample 1995-2006	(II) Full Sample Separate Years
After	-0.0130***	-0.0059***	---
	[0.0026]	[0.0013]	
Before 1 Year	---	---	0.0016
			[0.0030]
After Year 0	---	---	-0.0133***
			[0.0027]
After Year 1	---	---	-0.0045**
			[0.0020]
After Year 2	---	---	-0.0056**
			[0.0042]
After Year 3	---	---	0.0022
			[0.0019]
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Sample Size	49,580	158,668	158,668
R- Square	0.05	0.021	0.025

Notes: NIS provider weights and strata information used. Controls include demographic information available in the NIS like child's gender, race, age categories, mother's education level, marital status, households income categories, number of children in household, poverty status, whether the child moved from state of birth, urban area, number and type of providers. * denotes significance at 10%, ** 5% and *** at 1%.

mention the studies disclaiming the link and might no longer affect MMR take up negatively. Secondly, the overall media attention might have been higher to the initial negative stories than to the corrected information afterwards, even though as Figure 2.3 suggests, there was an abundance of articles referring to this link even in 2004, 2005 and 2006.

I estimate the following regression to identify how the number of news stories in the state papers during the previous year affected MMR take up after allowing for a quadratic trend:

$$MMR_{ist} = \beta_1 Numstor_{st} + \beta_2 \cdot t + \beta_3 \cdot t^2 + \delta X_{ist} + \gamma state_s + \varepsilon_{ist}, \tag{4}$$

where $Numstor_{st}$ refers to the number of stories in the local newspapers and TV stations in the state during the previous year that used the keywords "vaccine" and "autism". Using 2 lags as well as state/year averages of the previous two years yields similar results. In a second specification, I estimate equation (4) using the selective avoidance differencing strategy and present the findings in Table 2.5.

I first estimate equation (4) with only two years of data before and after the controversy and without explicitly accounting the trend in MMR take up. These results suggest that the number of stories in the state negatively affected the take up of the MMR vaccine. For every news story mentioning the vaccine-autism link the take up of the MMR vaccine fell by almost 0.2 percentage points. This is a rather large effect, but as Figure 2.3 suggests during this time period there were not many articles in the media so the marginal effect of another article could be substantial. Using the same sample and using the selective avoidance of MMR as a differencing strategy, I find that the effect persists although it is much smaller.

Using the full sample results and explicitly allowing for a trend in MMR take up, I find that the number of news stories did not have an effect on the MMR take up or on the likelihood that children avoided it. These findings seem to suggest that the effect of media on parents

Table 2.5 State Level News Stories Effect on MMR Uptake and Completing the 431 Series

	Time Trend Analysis			Differencing Strategy		
	(I) MMR Two Years Before/After 1998-2001	**(II) MMR Full Sample 1995-2006**	**(III) MMR Full Sample**	**(IV) Complete 431 Series Two Years Before/After**	**(V) Complete 431 Series Full Sample**	**(VI) Complete 431 Series Full Sample**
Number of Stories	-0.0022**	0.0002	0.00004	-0.0010**	0.00006	-0.00016*
	[0.0009]	[0.0001]	[0.0001]	[0.0005]	[0.00007]	[0.0009]
Trend	---	0.008***	---	---	---	---
		[0.002]				
Trend Square	---	-0.0005***	---	---	---	---
		[0.0001]				
Year FE	No	No	Yes	No	No	Yes
Sample Size	59,382	187,755	187,755	49,580	131,962	131,962

Notes: NIS provider weights and strata information used. Controls include demographic information available in the NIS like child's gender, race, age categories, mother's education level, marital status, households income categories, number of children in household, poverty status, whether the child moved from state of birth, urban area, number and type of providers. I also control for Medicaid/SCHIP eligibility, unemployment and uninsurance rate in the state and state mandates on immunizations. Every specification includes state fixed effects. * denotes significance at 10%, ** 5% and *** at 1%.

decisions to vaccinate their children with MMR or not was more pronounced in the very few years right after the spread of the controversy.

2.5 Who was most Affected by the Controversy? The Role of Parental Education

In this section I try to establish whether there was a differential effect in the response to the MMR controversy based on the mothers' education level and whether the amount of information available in the press affected their response disproportionally relative to other education groups. Anderberg, Chevalier and Wadsworth (2008) found that in the UK the more-educated parents responded more quickly to the information on the possible MMR-autism link, even after controlling for household income and additional variables. However, most of their analysis relies on aggregate measures of MMR uptake by geographical regions, and their available data starts only one year prior to the Wakefield study. Instead, I use individual level data from a nationally representative US dataset that spans a twelve year period from 1995 (three years before the publication of the Wakefield study) to 2006.

2.5.1 Results on the Differential Effect of the Controversy across Education Groups

Table 2.6 shows the results of a formal analysis on the differential responses of college educated mothers. Their children are less likely after the controversy to get vaccinated for the MMR even after controlling for the differences and changes in access to vaccines, and the results persist after indirectly accounting for the differential impact of the changes in access to immunizations. Accounting for a quadratic trends in the MMR take up I find that after the controversy college educated mothers were 1.75 percentage points less likely to immunize their children with the MMR vaccine relative to non-college educated mothers. The next specification looks at the changes by individual years and we observe that the coefficients on the estimates of the difference by education level are negative, however for most of the years they are not statistically significant. An important finding from the analysis on individual years is that the

Table 2.6 Effect of College Education on MMR Take up and Completing the 431 Series

	Time Trend Analysis				Differencing Strategy	
	(I) MMR	**(II) MMR**	**(III) MMR**	**(IV) MMR**	**(V) Complete the 431 Series**	**(VI) Complete the 431 Series**
Mother with College	0.0293***	0.0292***	0.0196***	0.0189***	0.0032 *	0.0016
	[0.0035]	[0.0035]	[0.0028]	[0.0028]	[0.0018]	[0.0014]
After	-0.0149***	-0.0177***	---	---	-0.0046***	---
	[0.0057]	[0.0052]			[0.002]	
After*College	-0.0180***	-0.0175***	---	---	-0.0042 *	---
	[0.0042]	[0.0042]			[0.0024]	
Before 1 Year	---	---	-0.0102	-0.0084	---	0.002
			[0.0069]	[0.0073]		[0.003]
After Year 0	---	---	-0.0101	-0.0194***	---	-0.0148***
			[0.0063]	[0.0061]		[0.004]
After Year 1	---	---	-0.0036	-0.0147**	---	-0.0024
			[0.0063]	[0.0066]		[0.0033]
After Year 2	---	---	-0.0039	-0.0131**	---	-0.0058**
			[0.0065]	[0.0067]		[0.0032]
After Year 3	---	---	0.0227***	0.0096 *	---	0.0034
			[0.0047]	[0.0050]		[0.0029]
Before Year 1 * College	---	---	0.0111***	0.0236***	---	-0.0008
			[0.0034]	[0.0083]		[0.0035]
After Year 0 * College	---	---	-0.0070	0.0041	---	0.0068
			[0.0078]	[0.0075]		[0.0076]
After Year 1 * College	---	---	-0.0116**	-0.0047	---	-0.0043*
			[0.0060]	[0.0060]		[0.0035]
After Year 2 * College	---	---	-0.0037	0.0001	---	-0.0037
			[0.0047]	[0.0074]		[0.0046]
After Year 3 * College	---	---	0.0051	-0.0161***	---	-0.0054
			[0.0044]	[0.006]		[0.0038]
Quadratic Trend	Yes	Yes	No	Yes	No	No
Sample Size	187,756	187,756	187,756	187,756	131,963	131,963

Notes: NIS provider weights and strata information used. Completing the 431 series denotes those children who obtained most of the shots of the 431 series, all except for MMR, relative to those who obtained the complete 431 series, including MMR. Before 1 year, refers to the year 1999, after year 0 to 2000, after year 1 to 2001, after year 2 to 2002 and after year 3 to 2003. Controls include demographic information available in the NIS like child's gender, race, age categories, mother's education level, marital status, households income categories, number of children in household, poverty status, whether the child moved from state of birth, urban area, number and type of providers. In Columns (I) and (II) I also control for Medicaid/SCHIP eligibility, unemployment and uninsurance rate in the state and state mandates on immunizations. Every specification includes state fixed effects. * denotes significance at 10%, ** 5% and *** at 1%.

negative education gradient was the greatest in 2001 and disappeared by 2003 as more studies were published refuting the claim of the link between MMR and autism. However after accounting for the overall trend, the take up of college educated mothers relative to non-college educated was negative and statistically different only in 2003. An individual year by year analysis confirms this and finds that the major difference in take up between college educated mothers and others happened after 2003, even though at this point many studies were published and disclaimed the findings in Wakefield et al.

Using one of the differencing strategies, the completion of the 431 series, I find that the difference across education groups becomes much smaller, only 0.4 percentage points. This method accounts better for changes in access, which affected disproportionally more the less educated, however it also ignores the spillover effect which we actually might expect to be higher for college educated mothers. I do not present results with the fourth dose of DTaP for brevity and because as shown in the individual year results in Table 2.3 these are dependent on the exact specification.

In the US part of the difference in MMR take up between the children of mothers with and without college degree, would be due to the different changes in access over this time period across the two groups. In my sample only 18% of children of mothers with college degrees are eligible for public insurance and that figure is almost 39% for the non-college educated. The increases in eligibility were also much smaller over this time period for the college educated mothers, so not accounting for these changes would probably overestimate the differential responses by education group. I dealt with this confounding factor in my analysis in two ways. In the specifications with MMR as dependent variable I control for the estimated eligibility for public insurance, the unemployment and children's uninsurance rate in the state during the previous year and whether the child lived in an a state that had enacted mandates on insurance coverage and also allow for overall trends in take up. Second, I look at the complete take up of the 431 series relative to those who only missed MMR by mothers' education level. This second approach most likely underestimates the effect of the controversy if the timing spillover on the uptake of the third dose of polio also varies by education level.

If the difference in take up rates by parents' education level was in response to the MMR controversy and if it can be explained through their absorption of information as some evidence suggest (Cutler and Lleras-Muney, 2010; Price and Simon, 2009) then we would expect more educated parents to be disproportionally affected by the number of stories in their local media. I find some weak evidence of this as shown in Table 2.7 where in the full sample analysis shown in Columns (II) and (III), in states where there were more media stories it appears that the take up of MMR for college educated parents was lower. Each additional news story in the press lowered the MMR take up for college educated parents by 0.04 percentage points whether I explicitly control for the quadratic trend in MMR take up or year fixed effects. These results do not persist when I try to indirectly account for the differential access in the complete 431 series results. The sign of the coefficient is still negative although no longer statistically significant, but at least in the overall sample the estimated coefficient is negative.

2.5.2 Parents Attitudes and Variations across Education Groups

If the differential response to the MMR autism controversy by education level is partly due to the difference in the information set and beliefs about the safety of childhood vaccines and MMR more specifically rather than different changes in access to vaccines, we would expect more educated parents as a result of the MMR or thimerosal autism controversy to express more

Table 2.7 Effect of News Stories by Education on MMR Take up and Completed 431

	Time Trend Analysis			Differencing Strategy	
	(I) MMR Two Years Before/After 1998-2001	**(II) MMR Full Sample 1995-2001**	**(III) MMR Full Sample 1995-2001**	**(IV) Complete 431 Series Two Years Before/After**	**(V) Complete 431 Series Full Sample 1995-2001**
Mother with College	0.0243***	0.0225***	0.0219***	-0.0028	0.0002 *
	[0.0043]	[0.0025]	[0.0025]	[0.0024]	[0.0011]
Number of Stories	-0.001	0.0002	0.0003 **	0.0006	0.0002
	[0.0007]	[0.00014]	[0.00013]	[0.0004]	[0.0008]
College*Number of Stories	-0.0006	-0.0004 ***	-0.0004 ***	0.00007	-0.00007
	[0.0009]	[0.00014]	[0.00014]	[0.0001]	[0.0001]
Quadratic Trend	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes
Sample Size	59,382	187,756	187,756	49,580	131,963

Notes: NIS provider weights and strata information used. State news refers to the number of stories on state newspapers and local TV and/or radio stations the previous year. Completing the 431 series denotes those children who obtained most of the shots of the 431 series, all except for MMR, relative to those who obtained the complete 431 series, including MMR. Controls include demographic information available in the NIS like child's gender, race, age categories, mother's education level, marital status, households income categories, number of children in household, poverty status, whether the child moved from state of birth, urban area, number and type of providers. In Columns (I) and (II), I also control for Medicaid/SCHIP eligibility, unemployment and uninsurance rate in the state and state mandates on immunizations. Every specification includes state fixed effects. * denotes significance at 10%, ** 5% and *** at 1%.

concern about childhood vaccines safety relative to those with lower education. Ideally we would like to see the progression of such attitudes during and after the controversy, but unfortunately this kind of detailed information over time is not available to my knowledge. There have been however different polls and smaller surveys since the early 2000s that have asked questions about parents' attitudes towards vaccines. According to all these sources, more educated parents have expressed larger concerns about vaccine safety. Surprisingly, these differences seem to persist even after the publication of studies refuting the MMR-autism claim and after the removal of thimerosal from childhood vaccines. This could be explained by parents giving higher weight to high risk information and lower weights to low risk information in their decision whether to vaccinate their children in the face of contradictory information (Viscusi, 1997).

Gust et al. (2008) using a special module of NIS find that white mothers older than 30 years old, with at least a college degree, were more likely to have refused recommended immunizations for their young children. Additional small size surveys find college educated parents to be more doubtful about vaccine safety and regard immunizations less important to the health of their child than the general population and are also more likely to exempt their children from the required vaccinations before school entry (Gellin et al., 2000; Salmon et al. 2005). Using the NIS, Smith, Chu and Barker (2004) found evidence that children of more educated mothers were more likely to never get a vaccine, and Smith et al. (2008) found that they were less likely to receive the MMR vaccine. In the Smith, Chu and Barker study, unvaccinated children were more likely to be male than female, indicating that probably the highest safety concern was autism since male children are much more likely to be diagnosed with autism than female children (Rutter, 2005).

Table 2.8 summarizes the data from BRFSS and shows the number and percentage of children getting the flu vaccine during the season by respondent's education level. Table 2.9 shows the reason for not getting the vaccine, by education level. The respondents are 62% male, so they are likely to be the fathers of the children in question, while in the NIS we only have information about mother's education level. These tables indicate that conditional on not getting the flu shots, more educated parents of the younger children especially those under 3 years old are more likely to refuse the vaccine due to safety concerns.

Table 2.8 Number and Percentage of Children Getting the Flu Vaccine by Respondent's Education Level

	N	(I) Child got flu shot	(II) Child without flu shot	(III) Don't know/Not Sure/Refused
Panel A. All Children				
No College Degree	6823	1159	5383	281
		16.99%	78.89%	4.12%
College Degree	3666	767	2805	94
		20.92%	76.51%	2.57%
Panel B: Children younger than 36 months				
No College Degree	629	229	366	20
		36.41%	58.19%	3.18%
College Degree	463	212	241	10
		45.79%	52.05%	2.16%

Notes: Author's tabulation from BRFSS in 2006. The module was asked to respondents from 7 states: Missouri, Nevada, New Hampshire, Ohio, Vermont, West Virginia and Wisconsin. The analysis corresponds to any random child selected in the module for Panel A and only those 36 months or younger in Panel B.

Table 2.9 Reasons for not getting the Flu Vaccine by Respondent's Education Level

	N	(I) No need/ Not recommended	(II) Concern about vaccine	(III) Access / Cost / Convenience	(IV) Not eligible/ Other/Don't know/Refused
Panel A. All Children					
No College Degree	5826	3007	274	207	2338
		51.61%	4.70%	3.55%	40.13%
College Degree	2938	1704	143	73	1005
		58.00%	4.87%	2.48%	34.21%
Panel B: Children Younger than 36 months					
No College Degree	423	153	20	11	239
		36.17%	4.73%	2.60%	56.50%
College Degree	267	94	18	7	148
		35.21%	6.74%	2.62%	55.44%

Notes: Author's tabulation from BRFSS in 2006. The module was asked to respondents from 7 states: Missouri, Nevada, New Hampshire, Ohio, Vermont, West Virginia and Wisconsin. The analysis corresponds to any random child selected in the module for Panel A and only those 36 months or younger in Panel B.

2.6 Conclusion

From an analysis of the National Immunization Survey (NIS), I find that in response to the MMR-autism link the take up of the MMR and other childhood vaccines fell by almost 2 percentage points, departing from an otherwise increasing trend. Even after controlling for household income, race, Medicaid/SCHIP eligibility, and other demographic variables, and indirectly accounting for the increased access due to expansions in access to health care for children and programs aimed at increasing immunization rates, the uptake of the MMR vaccine declined after the controversy became widespread in the media. The number of stories in the local media did not seem to affect the behavior of parents in the overall population, but seem to affect take up in the years immediate after the controversy. These results are only suggestive since my variation is limited at the state level and it is hard to make the case that local newspapers defined at the state level provide additional information to the national media and are a significant source of information for parents deciding on their children's vaccine take up.

Considering the separate responses to the controversy by mother's education level, I find that college educated lowered the MMR take up of their children by more than those mothers without a college degree. Allowing for a separate effect of the number of news stories in the national and local media by mother's education level, I find some suggestive evidence of a negative education gradient in the uptake of the MMR vaccine indicating that at least partially higher education is likely to affect health outcomes by the quicker absorption of advances in medicine. These findings disappear, with the estimated coefficients still negative but no longer significant, as I try to indirectly account for the different changes in access to medical services using a differencing strategy. This approach however likely underestimates the effect of the controversy, as observed in the overall population results probably because more educated mothers might also be more likely to decline the take up of the polio vaccine as well.

My findings about the differential response in take up based on parents' education are different from those in the Anderberg, Chevalier and Wadsworth study in UK who find that the differences in education disappeared after new research refuting the MMR-autism link became publicly available. In the US these differences persisted and became more pronounced in 2003, 2004 and 2006 suggesting that the negative information received more weight in the decision process than positive information. Using data from the BRFSS, years after the controversy I still find a difference in the attitudes of the highly educated parents towards vaccine safety suggesting that these attitudes affected the differential take up of MMR when the controversy first spread, rather than changes in access. Moreover, the persistence of such attitudes in 2006 explains the persistence in disparity in the future years.

Although the interpretation of the differences between college and non-college educated parents at least in the beginning of the controversy are ascribed to the differences in absorption of information, I cannot rule out the possibility that other factors that are correlated with education level might be in play. A possible candidate is risk aversion and/or risk perception. There is some evidence in the literature suggesting that education is positively correlated with risk-aversion. Harrison, Lau and Rustrom, (2007) for example find that higher educated Danes are more risk averse than others and Shroeder et al., (2007) find positive correlation between education and risk perception. Unfortunately, I do not have any available data that could provide information on the risk aversion of parents, however, Anderberg, Chevalier and Wadsworth who had proxies of risk aversion available to them depending on parents' smoking and drinking behavior, found that in the UK these measures could account only for a small portion of the differential responses by education level to the MMR-autism controversy.

APPENDIX

APPENDIX A

FURTHER DETAILS ON MEDIA DATA

The search terms used in identifying the articles referring to the MMR autism controversy are the very general combination of “vaccine” and “autism”. In the public press and for the majority of the public opinion it is hard to make the distinction between the separate possible effects of the MMR vaccine, thimerosal, and the general safety of vaccines on the likelihood that a child manifests symptoms of autism, so I do not make this distinction either. Conducting a separate search with national newspapers and magazines for “MMR vaccine” and “autism”, “MMR” and “autism” and “measles” and “autism” resulted in fewer hits for each case, all of which were included in the results from the more general keywords.

I tested the quality of the search keywords by searching five different newspapers and reading through the articles that included these terms. The five newspapers are Omaha World Herald, Los Angeles Times, St. Louis Post Dispatch, New York Times and Washington Times. With the exception of the St. Louis Dispatch, I checked directly the archives on the websites of the newspapers which were available since at least the early 1980s. New York Times is not available in the NewsLibrary database and so the articles in the New York Times are not included in the data used here. However, there are 157 news sources from the state of New York represented in NewsLibrary and these include The New York Observer, New York Post, The New York Sun, New York Daily News etc. In those instances when I have information available from the newspapers’ own websites and the NewsLibrary database, the count of hits matched between the two sources in every year.

Table A.1 shows the number of right and wrong hits for each year from the five different newspapers. Most of the wrong hits from Washington Times are due to announcements in their Health Calendar when they were referring to health clinics providing vaccines and separate announcements for meetings with parents of autistic children. Some of the wrong hits from the Los Angeles times refer to articles about the need to have movies with autistic children as characters. Another hit that was considered a “right” hit mentioned an episode of the lawyer TV show “Eli Stone” which depicts the battle of the parents of an autistic child against a pharmaceutical company.

Almost all of the hits appear after 1998. The one instance of an article discussing the link between autism and vaccines before the publication of the Wakefield et. al. study was in the LA Times in 1997. This piece focused on the creation of the National Vaccine Information Center, a non-profit watchdog which was founded by vaccine critics in the early 1980s and originally focused on vaccine injury compensation for those who suffered from vaccine side effects especially the DTP.

Table A.1 Testing the Quality of the Keywords Used

		'95	'96	'97	'98	'99	'00	'01	'02	'03	'04	'05	'06
Omaha World Herald	Right	0	0	0	0	0	0	2	1	2	4	2	2
Archive available since 1984	Wrong	0	0	0	0	0	0	0	2	0	3	0	0
LA Times	Right	0	0	1	0	3	9	9	8	7	11	13	4
Archive available since 1985	Wrong	0	0	0	0	0	0	2	1	1	3	0	0
St. Louis Post Dispatch	Right	0	0	0	0	2	0	1	5	5	7	2	1
Via NewsLibrary since 1988	Wrong	0	0	0	0	0	0	1	3	0	3	1	2
New York Times	Right	0	0	0	0	1	3	6	17	14	6	11	3
Archive available since 1981	Wrong	0	0	0	1	0	0	0	0	0	0	0	0
Washington Times	Right	0	0	0	0	0	7	6	21	7	10	10	7
Archive available since 1987	Wrong	0	0	0	0	0	0	2	4	2	6	6	6

The national newspapers and magazines sources available in the NewsLibrary database used to obtain the news count for each year include The Christian Science Monitor, U.S. News & World Report, USA TODAY, Slate, Education Week, Foreign Affairs, Newstex blogs, Newsweek and Pro Football weekly. The archives of these sources have been available in the NewsLibrary database since at least the beginning of the autism – vaccine controversy in 1998.

The original sources for newswires and transcripts include Associated Press, BBC Monitoring International Reports, Harris News Service, McClatchy-Tribune Regional News, National Public Radio and Scripps-Howard News Service. All these sources have been available in NewsLibrary since at least January 1st, 1997 before we would expect any news to be reported on the autism – vaccine link. The additional sources CBS, CNN, Fox News Channel and MSNBC were added to the database in March or April, 2003.

The news on the controversy peeked in 2002, and then made a comeback in 2005 and 2006. The stories in 2000, 2001 and 2002 focused partially on the Congressional House Committee reform hearings of April 2000 where Dr. Andrew Wakefield testified, The Institutes of Medicine MMR-Autism Report of April 2001, and additional House Committee Reform hearings of June 2002. During this time, the press also provided some coverage of specific studies and their results. The spike in news hits in 2005 was driven by a few different events related to the controversy that together added to the hype. One such event was the reorganization of the US vaccine program within the CDC that separated the program which promotes higher immunization rates from its safety branch which monitors the potential risks of vaccines.[34] Two

[34] New York Times article “Health Agency Splits Program Amid Vaccination Dispute” February, 25, 2005.
http://www.nytimes.com/2005/02/25/politics/25vaccine.html?pagewanted=1&_r=1&sq=vaccine autism&st=nyt&scp=9

other events were the news coverage of two studies that provided evidence against the MMR-autism link, the study on Japan from Honda, Shimizu and Rutter (2005) and a major review of the literature by an international panel of researchers (Demicheli et. al., 2005). However, the story that probably received the biggest media coverage in 2005 was a legal provision that was added by the majority leader of the time, Senator Frist that shielded companies making vaccines from lawsuits, even if they were negligent or reckless.[35]

[35] Sample NY Times article from December, 2005 describes the story : http://query.nytimes.com/gst/fullpage.html?res=9F0DE3DA1730F933A15751C1A9639C8B63&scp=2&sq=frist+AND+vaccine&st=nyt

Table A.2 Number of News Articles in the State Media on the Vaccine – Autism Link from 1995 to 2006

Year	(I) Total	(II) Mean	(III) St. Deviation	(IV) Median
1995	0	0	0	0
1996	2	0.04	0.2	0
1997	5	0.1	0.3	0
1998	21	0.42	0.8	0
1999	114	2.24	3.22	1
2000	189	3.71	4.91	1
2001	340	6.67	7.79	4
2002	605	11.86	15.19	7
2003	508	9.96	12.29	5
2004	480	9.41	12.47	7
2005	723	14.17	18.33	7
2006	589	11.55	13.02	6
Total	**3576**	**5.84**	**10.94**	**1**

Notes: Author's findings from the NewsLibrary archive with information from state newspapers and wiretranscripts from local TV stations accessed in April 2010.

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Chapter 3

AN EVALUATION OF A BREASTFEEDING SUPPORT PROGRAM FOR LOW INCOME WOMEN[36]

3.1 Introduction

Substantial evidence exists that breastfeeding imparts numerous health benefits to both the child and mother (Dewey, Heinig and Nommsen-Rivers, 1995; US Department of Health and Human Services, 2000a). Based on this evidence, the American Academy of Pediatrics and the American Dietetic Association recommend exclusive breastfeeding of almost all infants until 6 months of age and complementary breastfeeding for the rest of the infant's first year of life (American Academy of Pediatrics Work Group on Breastfeeding, 2005; American Dietetic Association, 2001), while the World Health Organization recommends breastfeeding until at least 2 years of age (World Health Organization Fifty-fifth World Health Assembly, 2002). Despite these clear recommendations, breastfeeding initiation and duration among women in the United States are low, with 74% initiating breastfeeding and only 42% continuing until their infants are 6 months of age. These rates are even lower for low income women; for example, only 67% of women enrolled in the Supplemental Nutrition Program for Women, Infants and Children initiate breastfeeding and only 33% continue to 6 months (Centers for Disease Control and Prevention Breastfeeding Web site). In Michigan, the breastfeeding rates for WIC mothers are lower still, with 49% initiating and 15% continuing until 6 months (Polhamus et al., 2006).

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[36] This chapter draws heavily from "A Quasi-Experimental Evaluation of a Breastfeeding Support Program for Low Income Women in Michigan", co-authored with Beth H. Olson, Steven J. Haider, Tracie A. Bolton and Jonathan G. Gold that was published in the *Maternal and Child Health Journal* (2010), 14: 86-93

Increasing breastfeeding, particularly among the low income population, is listed as one of the national public health objectives in Healthy People 2010 (US Department of Health and Human Services, 2000b).

Support programs to increase breastfeeding among low income women have been implemented by many US public health departments. These programs are often based on the United States Department of Agriculture's (USDA) Food and Nutrition Service's "*Loving Support*™" model (Guise et al., 2003), which uses mothers from the community with previous breastfeeding experience and who have received training as peer counselors. Although many studies have concluded that such peer counselor (PC) support programs improve breastfeeding outcomes, recent reviews have noted that most of the studies do not use convincing analytic methods to uncover the causal effect of the programs (Protheroe et al., 2003; Renfrew et al., 2007; Chapman et al., 2004). The only randomized controlled trial for a disadvantaged U.S. population is for urban, low-income, Hispanic women (USDA, Food and Nutrition Service, WIC Works Resource System Web page). Findings from the study show that the program increased breastfeeding rates at initiation (22.7 vs. 8.9% for the control group), 1 month, and 3 months, but not at 6 months. Despite the lack of empirical evidence, $14.9 million in fiscal year (FY) 2004 was appropriated in the USDA budget breastfeeding support programs for WIC participants and another $14.8 million was appropriated in FY 2005 and FY 2006 (Guise et al., 2003).

The purpose of this study is to estimate the effectiveness of a PC breastfeeding program for low income women in Michigan. The effectiveness is assessed by a quasi-experimental design in which we exploit the fact that because there was substantially more demand for program services than what could be provided, many expectant mothers who requested service were not subsequently contacted by a PC. The use of this quasi-experimental design has the potential to eliminate the bias that typically exists when instead one compares participants to non-participants. The bias would likely overestimate the effect of the program as participants in the program are more likely to want to breastfeed their children in the first place and that is why they would want to participate in a program that provides assistance with breastfeeding. We first examine whether the contact process was consistent with our quasi-experimental interpretation. Given an affirmative finding, we estimated the causal effect of the support program on breastfeeding initiation and duration. The analysis uses data from several programmatic and state administrative data sources for women from five Michigan counties who requested services from the PC program during the years 2002-04 and were enrolled in WIC. Our final sample included 336 women who requested services prenatally and were contacted by a PC (the treatment group) and 654 women who requested services prenatally but were not contacted by a PC (the control group).

3.2 Methods

3.2.1 Background on the Breastfeeding Initiative

The Breastfeeding Initiative (BFI) is collaboration between the Michigan's Women, Children and Infant (WIC) Division and Michigan State University Extension. The program operated in 17 Michigan counties in 2002 and then expanded to 22 counties in 2004, the period we analyzed (P. Benton, BFI program leader, oral communication, February 2008). The program provides breastfeeding education and support to low income women through peer counselors. The PCs are recruited from the community; must have obtained a high school education or equivalent, have access to a means of transportation, and express a positive breastfeeding experience with their baby; and are provided training on how to provide breastfeeding support.

Potential program participants are recruited by personnel at WIC clinics and are asked to fill out a referral form. The PCs then use the referral forms to contact women to provide program services. For women who are served, the PCs provide at least one contact to mothers in person, with subsequent contacts in person or by telephone based on the type of support needed. The subsequent contacts are at least monthly. Program data from this time period indicate that participants enrolling prenatally received on average 3 home visits, 2 personal contacts in locations other than the home, and 6 telephone contacts during their participation in the program. Mothers remain in the program until they discontinue breastfeeding, the baby is 1 year old, or support services are no longer desired. Prenatally enrolled women participated in the program for an average of 24 weeks. A detailed description of the program and a qualitative evaluation from the perspective of PCs and participants have been previously published (Meier et al., 2007).

3.2.2 Estimation Strategy

A naïve estimate of the effect of the BFI program on breastfeeding could be obtained by comparing a breastfeeding outcome for participants in the program to non-participants. However, to the extent that mothers who would have breastfed in the absence of the program were more likely to participate in the program, then such a naïve estimate could overstate the effectiveness of the program; the difference in breastfeeding rates between participants and non-participants would reflect both the effectiveness of the program and the higher motivation of the participants. In such a situation, the naïve estimate is plagued by endogeneity bias. A common solution to the endogeneity bias is to rely on an experimental design, where a group of individuals are randomly assigned to be either in a treatment group or a control group.

Although a true experimental design was not built into the BFI program, a feature of the program existed that closely approximated an experiment. Specifically, there was substantially more demand for the services of the program than could be provided, so PCs contacted only some of the individuals who filled out a referral form. Any selective contact, at most, could have been based on the limited amount of information available on the referral forms. Under the assumption that who PCs contacted is independent of underlying breastfeeding propensities (the unobservable component of breastfeeding conditional on referral form information), we could compare those who requested service and were contacted, the treatment group, to those who requested service and were not contacted, our control group, to obtain the causal effect of the BFI program. Fortunately, we were able to explore the validity of this key assumption with our data. We provide these results below.

We test for differences in pre-program characteristics between the treatment and control groups using a multiple linear regression framework. To isolate treatment-control differences within county, the level at which the programs are administered, we include county indicator variables in all regressions. We test for differences in outcomes between the treatment and control groups also using multiple linear regressions, including other explanatory variables to adjust for any differences that existed between the two groups and to increase the precision of our statistical tests. All reported p-values are based on two-tailed *t*-tests, with significance denoted at $p<0.10$, $p<0.05$, and $p<0.01$. For dichotomous outcome variables, we re-estimated the models using logistic regression; because none of our substantive findings were different from these models, we reported multiple linear regression results for all outcomes for consistency and simplicity.

3.2.3 Data Sources

Our analysis relies on several data sources. The first data source was an initial referral form through which expectant mothers requested services from the BFI. This form contains the name of the mother and infant due date, contact address, WIC identification number, previous breastfeeding experience, whether the expectant mother was subsequently contacted, and, in some cases, race/ethnicity. Although the BFI program operated in about 20 Michigan counties, referral forms were sufficiently completed and retained in only five counties: Lenawee, Monroe, Newaygo, Sanilac, and Wayne. The second data source was forms completed by PCs for all women who eventually participated in the BFI program. These forms were completed at program enrollment, infant birth, and program exit and included WIC identification number, name of mother, mother's birth date and address, and infant name and birth date. The final data source was state administrative data contained in the Michigan Department of Community

Health Data Warehouse, including data from WIC administrative records, Medicaid administrative records, and Vital Records.

The two BFI data sources, the referral forms and the program forms, provided us information about who belonged in the treatment and control groups. In addition, the identifying information on both forms was used to match these women to the state administrative data. For the treatment group, we first matched on WIC ID and infant date of birth and progressed through other identifiers such as mother and infant last name, county of residence, and infant first name (infant last name was not always recorded and may differ from the mother's). Because only referral form information was available for women in the control group, the matching algorithms focused on WIC ID, mother's last and first name, and mother's due date. Matches of BFI treatment and controls with state data were 78.3% and 68.0%, respectively. The lower match rate of controls was to be expected given that less identifying information was available for them.

Once the data were matched, we obtained breastfeeding information, household income, gestational age, and head circumference from WIC administrative data. Our key dependent variables on breastfeeding were constructed from the WIC administrative data. We obtained race/ethnicity information from the Medicaid data. We obtained various pregnancy and birth characteristics from Vital Records (e.g., Apgar score, tobacco use, adequacy of prenatal care, birth weight, etc.). The information in these latter two data sets allowed us to assess the validity of our estimation strategy and to adjust our findings for pre-programmatic group differences.

Our analysis sample contains all women who were successfully matched to the state administrative data and for whom the WIC administrative data contained breastfeeding information. For women who were missing other data elements, we defined an indicator variable for each data element and included this indicator variable in our regression models. This analysis strategy allowed us to retain the observations in our regressions and allow the observations with missing data to be systematically different than the observations without missing data. We provide sample size information in Table 3.1 to point out the extent of the missing data problem in our analysis.

Table 3.1 Summary Statistics

	Treatment		Control	
	N	Mean	N	Mean
BFI				
County	336		654	
Lenawee	48	14.3%	176	26.9%
Monroe	110	32.7%	108	16.5%
Newaygo	17	5.1%	105	16.1%
Sanilac	64	19.0%	23	3.5%
Wayne	97	28.9%	242	37.0%
Previous breastfeeding experience	309	26.9%	139	31.7%
Medicaid				
Mother's race/ethnicity	336		654	
African American	83	24.7%	222	33.9%
Hispanic	22	6.5%	36	5.5%
White	229	68.2%	388	59.3%
Female infant	336	50.9%	654	50.2%
Vital Records				
Birth weight (g)	288	3291.7	611	3259.3
Pregnancy within 18 months [a]	276	22.8%	587	23.3%
Early prenatal care [b]	287	90.2%	596	89.4%
Adequate prenatal care [c]	282	77%	582	77.1%
Tobacco use in pregnancy	286	23.1%	607	24.9%
Prior pregnancies	288	49.7%	606	61.4%
Apgar score (1-10)	288	9.04	606	8.95
Mother's education (years)	286	11.72	590	11.76
Mother's age (years)	288	23.24	607	23.79
Drinks per week in pregnancy	288	0.066	607	0.068
Admitted to the NICU	288	3.8%	607	2.6%
WIC				
Gestational Age (weeks)	336	36.42	654	36.89
Head Circumference (cm)	184	34.07	408	34.00
Household Monthly Income	282	14,952	578	14,196

Notes: [a] This variable takes on a value of 1 for those women who have had a prior pregnancy within 18 months, 0 if there was a prior pregnancy more than 18 months ago, and missing for everyone else.

[b] Early prenatal care is an indicator variable for whether the woman sought care before the 5th month of pregnancy.

[c] Adequate prenatal care is an indicator variable for whether the amount of prenatal visits is deemed adequate or better as measured by the Kotelchuck index, which compares the actual prenatal visits to the expected prenatal visits from the start of pregnancy care.

3.3 Results

3.3.1 Verifying the Quasi-Experimental Estimation Strategy

Our key identifying assumption is that the provision of services among those who requested service is independent of underlying breastfeeding propensities within each county. We examined whether the data were consistent with this assumption by comparing various pre-program characteristics of the treatment group and the control group. These comparisons are provided in Table 3.2. To assess whether there were statistical differences between the treatment and control groups within a county, we estimated a linear regression for each of the listed characteristics in which we included county indicator variables; the p-values in the final column of Table 3.2 are from these regressions.

We divided the pre-program characteristics into three categories: background characteristics on the mother, pregnancy characteristics, and birth characteristics. With respect to the background characteristics, there are significant differences only between the treatment and control group based on whether the mother had a prior pregnancy. The participants in the treatment group were about 10% less likely to have had a prior pregnancy (49.7% vs. 61.4%; p-value = .001). There is weaker evidence of differences for whether the mother is Hispanic (treatment 6.5% vs. control 5.5%; p-value = .068). There are no other significant differences at the .10 level for the other background characteristics: mother's race, mother's age, household monthly income, whether there was a prior pregnancy within 18 months, and whether the mother had any previous breastfeeding experience.

We also examined whether there were differences in several pregnancy characteristics (tobacco use during pregnancy, drinks per week during pregnancy, early prenatal care, and adequate prenatal care) and birth characteristics (whether infant was female, birth weight,

Table 3.2 Comparing Pre-Program Characteristics for Treatment and Control Groups

	Treatment N=336	Control N=654	P-value
Background characteristics			
Mother's race/ethnicity			
African American	24.7%	33.9%	.208
Hispanic	6.5%	5.5%	.068
White	68.2%	59.3%	.943
Mother's education (years)	11.7	11.8	.910
Mother's age (years)	23.2	23.8	.196
Household monthly income	$14,952	$14,196	.343
Prior pregnancies	49.7%	61.4%	.001
Pregnancy within 18 months [a]	22.8%	23.3%	.327
Previous breastfeeding experience	26.9%	31.7%	.364
Pregnancy characteristics			
Tobacco use in pregnancy	23.1%	24.9%	.043
Drinks per week in pregnancy	0.07	0.07	.757
Early prenatal care [b]	90.2%	89.4%	.915
Adequate prenatal care [c]	77.0%	77.1%	.239
Birth characteristics			
Female infant	50.9%	50.2%	.393
Birth weight (g)	3291.7	3259.3	.070
Gestational age (weeks)	36.4	36.9	.104
Head circumference (cm)	34.1	34.0	.575
Apgar score (0-10)	9.04	8.95	.100
Admitted to the NICU	3.8%	2.6%	.616

Notes: The data come from Medicaid, WIC and Vital Records administrative records, with the exception of previous breastfeeding experience that comes from BFI program information. The p-values are from two-tailed *t*-tests from a simple linear regression with the pre-birth characteristic as the dependent variable and county indicator variables as the control variables.

[a] This variable takes on a value of 1 for those women who have had a prior pregnancy within 18 months, 0 if there was a prior pregnancy more than 18 months ago, and missing for everyone else.

[b] Early prenatal care is an indicator variable for whether the woman sought care before the 5th month of pregnancy.

[c] Adequate prenatal care is an indicator variable for whether the amount of prenatal visits is deemed adequate or better as measured by the Kotelchuck index, which compares the actual prenatal visits to the expected prenatal visits from the start of pregnancy care.

gestational age, head circumference, Apgar score, and whether the infant was admitted to the neo-natal intensive care unit (NICU)). There is evidence that the treatment group mothers were less likely to smoke during pregnancy (23.1% vs. 24.9%; p-value = .043) and weaker evidence that the treatment group's infants weighed more (3291.7g vs. 3259.3, p-value = .070) and had higher Apgar scores (9.04 vs. 8.95; p-value = .100). For all of the other characteristics, the characteristics are statistically indistinguishable between the treatment and control groups.

We consider these results largely supportive of our study design for several reasons. First, the strongest difference we observed was for whether there was a prior pregnancy, with the treatment group being less likely to have had a prior pregnancy than the control group. This characteristic was one of the few characteristics that could be identified from the referral form. We incorporated the possibility that counselors systematically chose whom to call back based on these characteristics, as well as other observable characteristics, in our analysis below. Second, at the standard significance level of .05, there were differences between the treatment and control group on only one other characteristic (tobacco use during pregnancy). We expected to find pre-program differences on a characteristic or two given the number of characteristics we examined.

3.3.2 The Effect of the BFI on Breastfeeding

We present our results of the effects of the BFI on breastfeeding in Figure 3.1 and Table 3.3. Our results indicate that the BFI was very effective in increasing breastfeeding among the treatment group. To provide an initial indication of the effectiveness of the program, we present the unadjusted breastfeeding duration and rates for both groups. Mean total duration for the treatment group was 7.8 weeks, whereas the similar duration was only 5.7 weeks in the control

Figure 3.1 Percent Breastfeeding over Time by Group

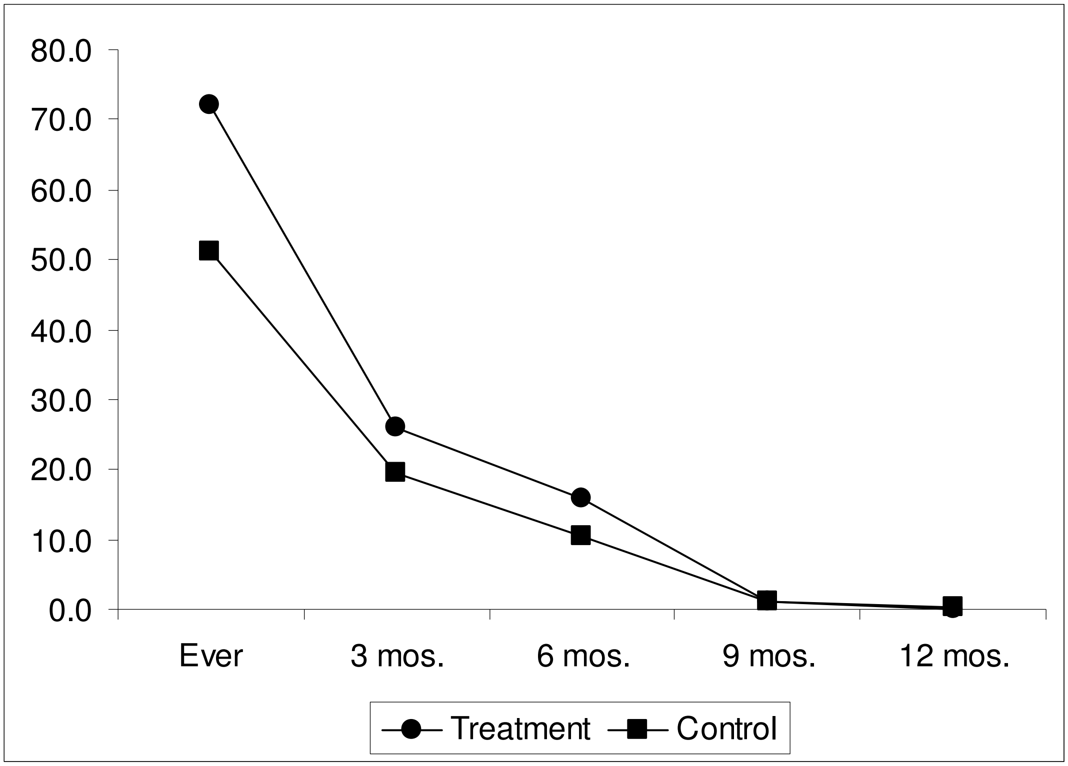

Table 3.3 Comparing Breastfeeding Outcomes for the Treatment and Control Groups

	Treatment N=336	Control N=654	Estimated Differences by Regressor Set: Set 1	Set 2	Set 3
Total weeks breastfeeding	7.83	5.66	2.62 ***	3.49 ***	3.61 ***
			[1.2, 4.0]	[1.7, 5.3]	[1.8 , 5.4]
Breastfeeding rate at					
0 months	.720	.511	.223 ***	.274 ***	.270 ***
			[.16, .29]	[.19, .36]	[.18 , .36]
3 months	.259	.196	.090 ***	.119 ***	.120 ***
			[.03, .15]	[.05, .19]	[.04 , .19]
6 months	.158	.104	.062 ***	.074 **	.076 **
			[.02, .11]	[.01, .13]	[.02, .14]
9 months	.012	.011	.001	.008	.009
			[-.01, .03]	[-.01, .03]	[-.01, .03]
12 months	.000	.002	-.001	-.000	.001
			[-.00, .00]	[-.01, .01]	[-.01 , .01]

Notes: Regressor set 1 includes indicator variables for county. Regressor set 2 includes regressor set 1 and indicator variables for race/ethnicity of mother, a quadratic in age of mother, an indicator variable for whether the mother previously breastfed a child, indicator whether there was a previous pregnancy and an indicator for whether previous pregnancy was within 18 months. Regressor set 3 includes regressor set 2 and the gender of the infant, a quadratic for infant's birth weight, infant's head circumference at birth, a quadratic for gestational age in weeks, an indicator variable for early prenatal care, an indicator variable for adequate prenatal care, Apgar score at birth, mother's education, an indicator for prenatal tobacco use, drinks per week, an indicator for whether infant was admitted to NICU, and a quadratic in household income. Significance levels are denoted as * for .10, ** for .05 and *** for .01 p-values. In brackets we report 95% confidence intervals.

group. As is made clear by examining breastfeeding duration at different time points (see Table 3.3 and Figure 3.1), the longer duration was due to the treatment group being more likely to initiate breastfeeding and then continuing at 3 and 6 months.

We assess the statistical significance of our results by estimating multiple linear regression models. The first regression (regressor set 1) includes only county indicator variables, allowing us to isolate within county treatment-control differences. The results suggest that the treatment group breastfed 2.6 weeks longer than the control group and is strongly statistically significant (p-value < .001). We also examined the breastfeeding differences for the duration at various time points. The results suggest that the treatment group was 22.3% more likely to initiate breastfeeding (p-value < .001), 9.0% more likely to breastfeed at 3 months (p-value = .002), and 6.2% more likely to breastfeed at 6 months (p-value = .008). The treatment-control differences at the other duration time points (9 and 12 months) are not statistically significant at the 10% level.

We estimate two additional sets of treatment-control differences to further probe the validity of our quasi-experimental strategy. One additional set of treatment-control differences was estimated by including those characteristics that were potentially observable by peer counselors in the BFI program, including race/ethnicity of the mother, age of the mother, whether the mother had a prior pregnancy, and whether the mother previously breastfed a child; this set of regressors is referred to as regressor set 2. The second additional set of treatment-control differences was estimated by including all pre-program characteristics that were listed in Table 3.1; this larger set of regressors is referred to as regressor set 3. Results for these two additional regressor sets are also presented in Table 3.3.

There are small systematic changes when we compare the estimated differences with regressor set 1 to those with regressor set 2, but basically no change when we compared the estimated differences with regressor set 2 to regressor set 3. For example, the estimated difference in total weeks of breastfeeding increased from 2.62 weeks with regressor set 1 to 3.49 weeks with regressor set 2. This large estimated difference with regressor set 2 is still significantly different from zero (p-value < .001) and remains within the 95% confidence interval of the estimated difference with regressor set 1. When we include the exhaustive set of pre-programmatic characteristics as regressor set 3, the estimated difference increased slightly to 3.61 weeks. A similar pattern is observed when comparing the results for the other breastfeeding outcomes.

These findings are consistent with their being some systematic contact of referred women based on the information contained on the referral form, but then no systematic contact based on the numerous other pre-program characteristics contained in our data but that were not observable by peer counselors. These results provide further evidence of the validity of our key identifying assumption about the process of PCs contacting referred women. It is worth noting the nature of the systematic contacts implies that counselors contacted women who were less likely to breastfeed, implying that the systematic recruitment makes the program look less beneficial that it actually was. Based on this interpretation, our preferred set of estimates is those with regressor set 3 and we interpret the estimates to be the causal effect of the BFI program on breastfeeding behavior.

3.4 Discussion and Conclusion

We examined the effectiveness of a peer counseling breastfeeding support program for low income women in Michigan who also participated in WIC. Using a quasi-experimental methodology that stems from the program having excess demand for its services and data derived from administrative and survey-based sources, we estimated the causal effect of the support program on several breastfeeding outcomes.

We first presented results to examine the validity of our key identifying assumptions. Specifically, we compared the treatment and control group along a range of pre-programmatic characteristics. Although there is evidence of the systematic contact of referrals on some of the characteristics on the referral reform (whether the mother had a prior pregnancy most notably), there is little evidence of systematic recruitment based on characteristics that were not known to the BFI program at the time of recruitment. These results supported our assumption that the process of contacts could be used as a quasi-experiment to identify the causal effect of the BFI program.

We then estimated the causal effect of the peer counseling program on breastfeeding outcomes. Our preferred estimates, which take into account the possibility of systematic recruitment on the characteristics that were observable by peer counselors and other pre-program characteristics to adjust for any remaining differences, indicated that the program was very effective at increasing breastfeeding among women in the treatment group. We found that the support program led to 3.6 additional weeks of breastfeeding for the treatment group, a very large effect when compared to the 5.7 weeks of average breastfeeding among the control group. Our results also indicated that this longer duration was due to more breastfeeding in the treatment group initially and at 3 and 6 months.

Our findings of programmatic effects through 6 months were more sustained when compared to the only U.S.-based study for low income women that used a true experimental design (USDA, Food and Nutrition Service, WIC Works Resource System Web page). The previous study, which only found significant programmatic effects initially and at 3 months, reported the support program as understaffed, with less than 10% of women in their treatment group reporting a peer counselor contact past 1 month postpartum. By contrast, the BFI program requires monthly peer counselor contacts for all participants until they exit the program. Continued support past the initiation of breastfeeding may be critical for extending breastfeeding duration as women encounter challenges such as returning to work and the issues of breastfeeding older infants, such as the introduction of solid foods and teething.

There are several limitations to our study. The first rests with the validity of our key assumption regarding how women were contacted. Although we found little evidence that was inconsistent with our key assumption, the assumption itself cannot be tested. Thus, a true randomized control trial of such a PC program would be useful to corroborate the results we report here. The second and more important limitation rests with the external validity of our findings. Strictly speaking, even if our key assumption is valid, our study has identified the average program effect for women who requested PC services. It may be that women who request service make better use of the assistance provided by PCs, and thus, the average effectiveness would be larger for women in our study than it would be for the more general population of low income women.

Many studies have documented the low breastfeeding rates among low income mothers, a population whose children are at relatively high risk for poor health outcomes and who often receive government-supported medical care through Medicaid. Given the substantial evidence that breastfeeding imparts health benefits to both the child and mother, a program that increases breastfeeding among low income women could improve the health of an important, vulnerable population and generate large cost-savings for the Medicaid program. Our estimates suggest that the benefits of the BFI program could be substantial: it increased the breastfeeding initiation rate by about 27 percentage points, increased breastfeeding duration by 63% (or more than three weeks), and had lasting effects on breastfeeding rates through the sixth month. Moreover, such PC support programs are relatively inexpensive to administer because of their reliance on peer counselors rather than health care professionals. In light of these encouraging results, the BFI program should be subjected to a rigorous cost-benefit analysis to establish its cost-effectiveness and evaluated in other settings to establish whether its substantial effects are replicable.

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