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Essays on Two Implications of the Affordable Care Act (aca)

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ESSAYS ON TWO IMPLICATIONS OF THE AFFORDABLE CARE ACT (ACA)

by

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ABSTRACT

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The main objective of my dissertation is to investigate some of the causal effects of the Affordable Care Act (ACA) on U.S. healthcare system. After an overview about some of the new provisions enacted by the ACA and their components and timelines, effects of the ACA on immunization coverage for children under age of three and its impact on retention of the insureds receiving newly established rebates would be assessed.

Chapter 2 evaluates changes in the up-to-date status of the nine vaccines recommended by the Advisory Committee on Immunization Practices (ACIP) for children aged 19 to 35 months following new provisions of the ACA that aimed to boost the coverage of the preventive services. By using a Difference-in-Difference identification strategy, I found that the ACA has significantly increased the number of up-to-date vaccines. In particular, it has boosted the up-to-date status for DTaP, Hepatitis A, and Varicella vaccines, especially for the states without universal or universal select programs prior to the ACA. While results show that families with lower income have utilized more benefits in the case of Varicella vaccine, parents with higher income and education have utilized more benefits in the case of the Hepatitis A vaccine. Also, the ACA has been more successful for Varicella vaccine in the states where non-religious exemptions were not permitted by law. Although the ACA might have been effective for these

immunizations through eliminating the financial doubts for delays or refusals, since concerns about effectiveness and side effects of the vaccination have been reported as more important barriers, significant increases in the coverage should be seen as indirect information perceived by parents to address some of their non-price concerns.

Chapter 3 investigates the changes in insureds' retention rates once new rebate regulations under the ACA came into effect in 2011. By new provisions, if insurer does not spend at least 80% of the premiums collected from Individual Medical (IM) policies on claims and healthcare quality improvements, the left over should be returned to the insureds in form of the rebate. According to the literature, subjective value of the windfalls –like this rebate– could be different depending on how the recipient looks at the windfall. A rebate or returned wealth has a higher subjective value for the recipient than a bonus, and will more likely work as forced savings that convinces insured to stay with current insurer. Private insurance data used in the study shows that rebates have actually worked as a conventional rebate rather than a bonus, and have significantly discouraged insureds from lapsing, especially in the markets with fewer rivals. Also, larger amounts of the rebate show stronger impact on retention.

This dissertation is lovingly dedicated to my wife, Ensieh, and my children, Ehsan and Saba. I give Ensieh deepest expression of love and appreciation for the encouragement that she gave and the sacrifices she made during this graduate program.

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Chapter 1

Introduction

Patient Protection and Affordable Care Act (PPACA) or the so-called Affordable Care Act (ACA) or Obama-Care is a United States federal statute signed into law by President Barack Obama on March 23, 2010. As described by many of the professionals in the US healthcare system, it represented a significant change in the US healthcare system, encouraging or even forcing all US citizens to be involved in the health insurance market.

The main purposes of the ACA are to increase the rate of health care coverage for Americans and reduce the overall costs of health care.[1] In order to combat the market failure of adverse selection in a partially voluntary system of healthcare insurance, the ACA provides a number of mechanisms including mandates, subsidies, and tax credits to insurers, employers and individuals to increase the coverage rate. The ACA included numerous provisions to take effect over several years beginning in 2010.

One of the ACA's changes was to require insurance companies to deliver preventive services for free, starting from September 23, 2010. This includes all recommended vaccinations for children and adults. Section 2713 under section 1001, says:

" . . . a health insurance issuer offering group or individual health insurance coverage shall, at a minimum *provide coverage for* and *shall not impose any cost sharing requirements for*: . .

[1] For example, see 111th Congress Public Law 148, from the U.S. Government Printing Office

. (2) *immunizations* that have in effect a *recommendation from the Advisory Committee on Immunization Practices (ACIP)* of the Centers for Disease Control and Prevention . . . ." [2]

In chapter 2, I estimate the effect of this new provision by measuring changes in vaccination coverage for children under three years old. Since some states have had provisions similar to what the ACA provided but prior to 2010, I will employ a difference-in-difference (DiD) approach to identify the effect of the ACA on coverage.

Also, all private health insurance companies have faced a regulatory change by the ACA since end of 2010. Starting in 2011, the ACA requires insurance companies to submit data on the proportion of premium revenues spent on clinical services and health quality improvements, which is called the Medical Loss Ratio (MLR). Then, the ACA requires insurers to issue rebates to enrollees if this percentage does not meet minimum standards. Minimum MLR requirement for insurance companies is 85% for the Large Group (LG) plans and 80% for the Individual Medical (IM) and Small Group (SG) plans. If an issuer fails to meet these minimum MLR, they are required to send rebates to their insureds based on contributions to the premium pool.

In chapter 3, I investigate causal effects of the rebate, which works as a windfall gain that may send some signals towards insured regarding their expected payoff in next period, on their retention rate and lapse behavior. Since an exogenous policy determines rebate eligibility – independently from insured's characteristics, and only part of the population were eligible for the rebates, conditional independence assumption holds and therefore, causal effect would be measurable by gauging the difference in the lapse behavior of the eligible and non-eligible

[2] U.S. Government Printing Office

insureds, after taking into the account the effects of the other covariates. So, a Difference-in-Difference (DiD) identification strategy will be employed in this quasi-experimental analysis.

Chapter 2

How Effective was ACA for Childhood Immunization Coverage in the United States?

2.1. Introduction

Since September 23, 2010, all health plans and insurance policies were required to provide coverage without cost sharing, such as deductibles, copayments, or coinsurance for childhood immunizations recommended by ACIP. In this chapter, I will use households (children) in states with ACA-type provisions prior to 2010 as a control group to estimate the causal effect of this provision on immunization coverage for children three years old or less in treated states, where parents experience a change in vaccination costs.

From a social planner's perspective, vaccination is one of the best examples of a positive externality leading to market failure, where the free market ends up in a lower than socially optimal level of production. Thus, the ACA is policy tool to combat the market failure.

Although one avenue for this goal is through price reduction, vaccine demand is likely inelastic due to having no close substitute and a negligible share in household budget. Gust et al. (2008) have shown that cost of the vaccine was of importance for only delays in vaccination for Diphtheria, Tetanus and acellular Pertussis (DTaP) vaccine, which is among most expensive vaccines to complete. In order to explain significant increase in the coverage rate for the 4:3:1

vaccine series[3] after state insurance mandates, Chang (2015) suggests that part of the population is still responsive to the immunization's price which includes vaccine prices and time costs. More importantly, different copayment and deductible structures for different insurance plans hides the actual costs of vaccination for different families; therefore, results found in this work cannot be solely interpreted as the ACA's impact through price elasticity.

More to the point, the ACA likely makes coverage of vaccines more routine and may provide parents with more confidence against the side effects, too. Thus, I test the empirical hypothesis on whether the ACA's new legislations about immunization affect overall coverage for childhood vaccines.

This chapter continues as follows. Trends in childhood immunization will be discussed in section 2, followed by modeling and identification strategy in section 3. Data sources will be explained in section 4, and section 5 presents the results. I will try to find out the rationale behind the ACA effect on immunization in section 6.

[3] 4 doses diphtheria–tetanus toxoids–pertussis, 3 doses of polio, and 1 dose measles-mumps-rubella

2.2. Childhood immunizations in practice

Immunization and childhood immunization in particular, have proven the idiom that "An ounce of prevention is worth a pound of cure". A review published in 1999, for example, indicated that coverage of childhood vaccinations has resulted in more than a 95 percent decline in vaccine-preventable childhood diseases (Shefer et al. 1999). Also, Zhou et al. (2009) performed a cost-benefit analysis for full coverage of the same nine vaccines covered in this study – called routine immunization, and showed that the direct and societal benefit-cost ratios were 3.0 and 10.1, respectively. While their calculated incremental societal benefit-cost ratios for Hepatitis A (Hep A) and Rotavirus (ROT) vaccines were below one, meaning that they were not cost-saving, they indicate that these two vaccines are still cost-effective from the societal perspective.

As of February 2011, ACIP recommended ten vaccines to be covered within the first three years of age for each child, nine of which are studied here.[4] The timeline and schedule have not changed since 2008.

During the past few decades, almost all countries have experienced vast success in bringing childhood immunizations to the forefront of preventive and public health policies. The Healthy People 2010 report, which covers the main objectives and goals related to public health in the United States for a 10-year horizon, indicates that substantial progress has been achieved for childhood immunization during the past decade in this country. Zhou et al. (2009) estimated that recommended immunizations by members of the 2009 US birth cohort prevented about 42,000

[4] Morbidity and Mortality Weekly Report, February 11, 2011.

early deaths and also 20 million cases of disease. Their estimation suggested net savings of $13.5 and $68.8 billion in direct and indirect savings, respectively.

Despite the seemingly obvious importance of childhood immunizations, take-up is still far from universal, and the reasons are diverse. Elementary, middle, and high school registration processes often mandate entrants to complete recommended vaccines or give explanation for exemptions, but there are no similar provisions for infants or preschool children. Doubts and concerns about vaccines' necessity and safety, side effects, child illness, and not enough information about vaccination benefits have been the most important reasons for delay or refusal for childhood vaccinations (Gust et al. 2008, Benin et al. 2006). Gust et al. (2008) estimated that in about 5.9% of the cases, parents refuse to get all the vaccines due to different reasons while 13.4% will delay. Varicella (VRC; chickenpox) vaccine has had the largest share of refusal in their study.

Financial constraints are another possible explanation for delay or refusal. Kenyon et al. (1998) and Orenstein et al. (1990) found lower-than-average coverage within inner cities and public housings. In particular, Kenyon et al. (1998) found that African-American children throughout Chicago, and particularly those in public housing, are less likely to be vaccinated. Also, Gust et al. (2008) show that while very few parents had doubt or refused vaccines due to the cost, it was a little bit more important for delay in vaccination, especially for DTaP. Chang (2015) investigates the effects of the state mandates on insurance companies for infants 4:3:1 vaccine series and found mandates as significant boosters for the coverage. She takes the proportion of the children vaccinated at hospitals or doctors' offices into the account and since this portion increases significantly after the mandates, it suggests that some parents are responding to the lower overall costs of vaccination which include the implicit time costs.

Even with low price-elasticity, it is plausible to have some families foregoing to get shots on-time due to the budget limitations, especially for vaccines with more concerns about side effects like VRC. A study by Joyce and Racine (2003) about the effectiveness of State Children's Health Insurance Programs (SCHIP) in getting kids up-to-date for two series of vaccines and two individual vaccines showed that the program had been significantly effective only in the case of VRC. So, they argue that insurance coverage is of importance to adopt new vaccines like VRC. While their study did not cover PCV (Pneumococcal Containing Vaccine), they predicted that the importance of the insurance coverage is likely for more coverage for more expensive vaccines like PCV, too. A study by Abrevaya and Mulligan (2011) shows that state mandates on VRC vaccination prior to enter the school or daycare have significantly increased the vaccination rates among preschool ages of 19-35 months. They argue that these mandates could be affecting the coverage by raising public awareness about the vaccination benefits.

As mentioned before, although new legislation is explicitly reducing the costs of vaccination, its indirect effects could be even more through providing more information and awareness to parents (Abrevaya and Mulligan, 2011) and reducing their concerns about the side effects.

2.3. Identification strategy

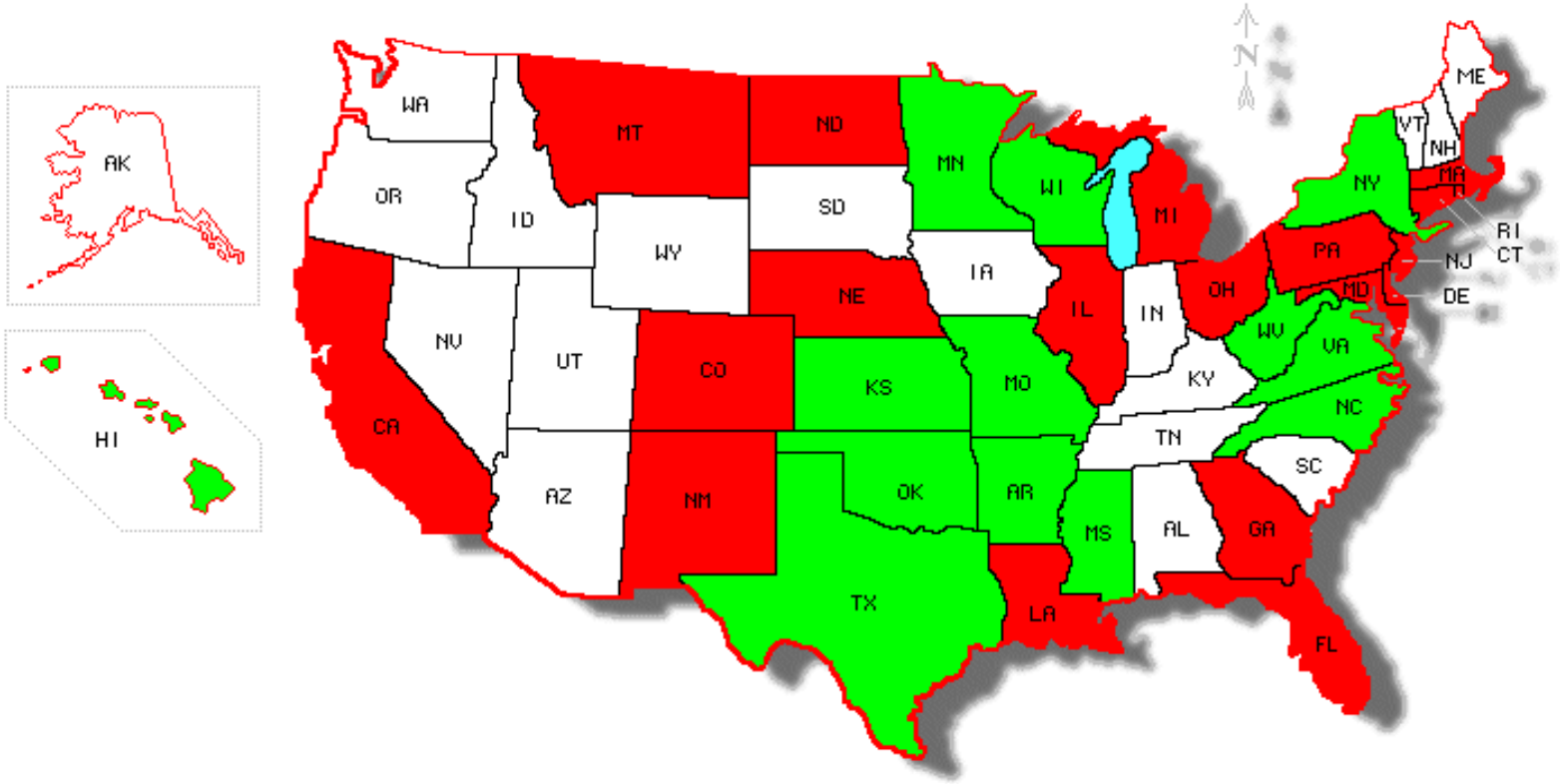
2.3.1. States Affected and not affected by the ACA

Immunization provision in the ACA has not affected all states equally. Some of the states have had similar regulations in effect even prior to the ACA. Figure 1 shows how 50 states and the District of Columbia were dealing with childhood immunizations as of September 2010. I updated the study of Rosenbaum et al. (2003) about legal framework for childhood immunization and found that prior to the ACA, 13 states had coverage mandate and cost-sharing prohibition for childhood immunizations, similar to the ACA's regulations. These states play the role of control states in identification strategy. On the other hand, 18 states did not have any mandate for insurance companies to cover immunizations as of 2010, and 20 jurisdictions did not have the cost-sharing prohibition with their coverage mandates. The ACA therefore has changed the regulation environment for these 38 jurisdictions, including D.C. which form treated—affected—states.

Figure 2 depicts trends in the up-to-date status of the nine childhood vaccines for affected and non-affected states from 2008 through 2012, based on National Immunization Survey (NIS) data. As I will explain later, only children with private insurance coverage through their parents' employer are included. Along with trends for affected and non-affected states, I have included trend approximation for treated states if they would not had been affected by the ACA since 2010 but had grown with same trend as control states. This is shown by dotted-dash lines. Indeed, I try to estimate the size and significance of the deviation from the trend (deltas on Figure 2). With a valid identification strategy, this deviation might be interpreted as a causal effect of the ACA.

One critique for this analysis about the ACA's impact on vaccination coverage could be that is this really a change in vaccination coverage or a change in private insurance coverage? One can argue that effects shown here could come from changes in private insurance coverage –due to the ACA or anything else– and not changes in behavior of the parents. As Figure 3 shows, there have been parallel downward trends in private insurance coverage since 2008 in either treated and control states. Thus, the change in vaccination coverage could be attributed to causal effect of the ACA on parents' behavior regarding vaccination, and not change in demand for private insurances.

Figure 1- Affected (Treated) States versus Non-affected (Control) States by the ACA Provision about Cost-sharing for Childhood Immunization[5]



States with coverage mandate and prohibition for both deductible and copayment in 2010: "Control Group" (13)
States with coverage mandate, but silent about deductible/copayment or permitted deductible/copayment in 2010 "Treated Group" [20 including D.C.])
States without any immunization coverage mandate for insurance companies in 2010 "Treated Group" (18)

[5] For a comprehensive discussion, see Rosenbaum et al., 2003.

Figure 2- Trend of the coverage rates for nine of ACIP recommended vaccines in affected and non-affected states by the ACA, for children with ONLY private insurance coverage through parents' employer

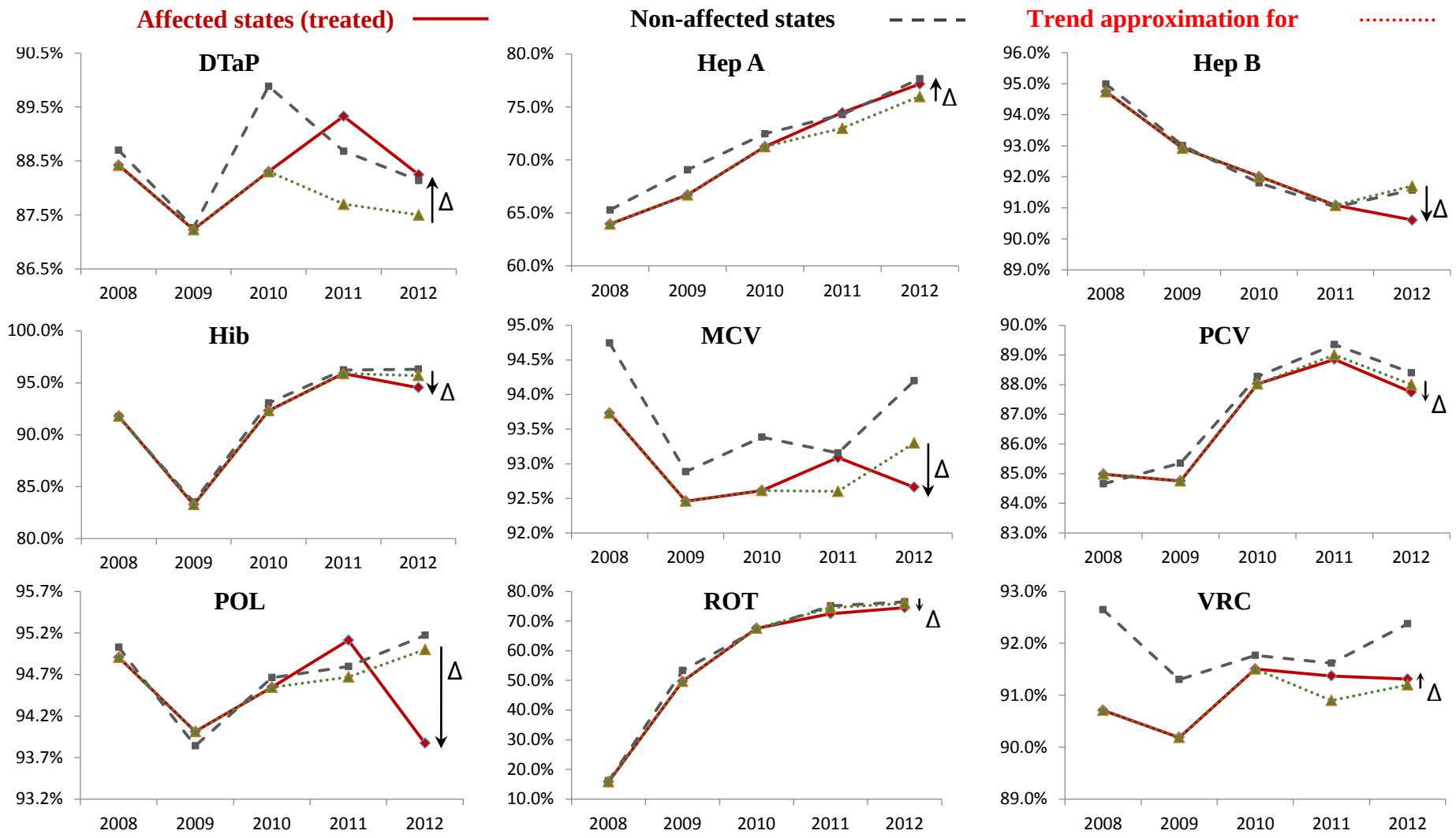
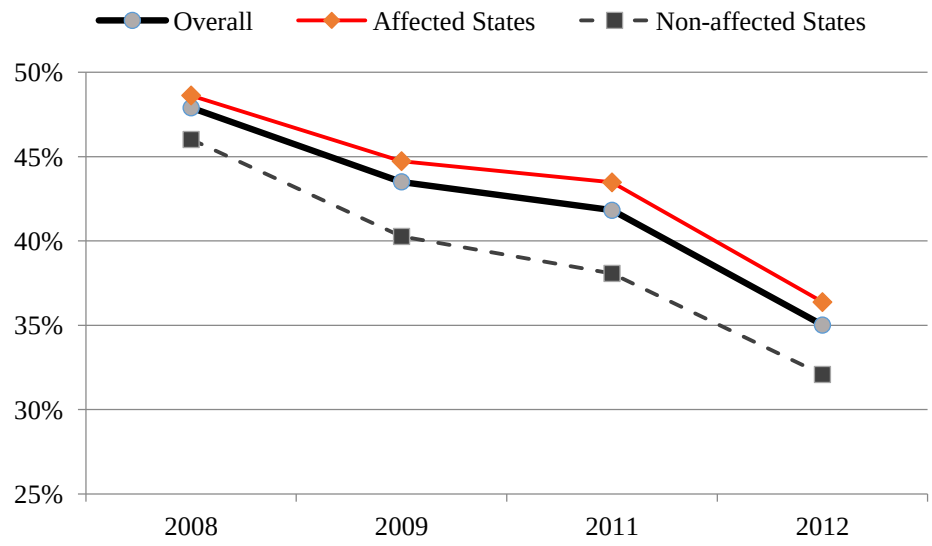


Figure 3- Trends of the private insurance coverage: overall, Affected states, and Non-affected states



2.3.2. Estimation model

I employ the following two estimation equations:

$$\mathbf{N\_UTD_{it} = \mu + \varphi_1 X_{it} + \delta_1 S_i + \delta_2 \tau_t + \delta_3 Tr_i + \delta_4 Af_t + \beta_1 (Tr_i . Af_t) + \varepsilon_{it}} \quad (1)$$

$$\mathbf{Vac_{it} = \alpha + \varphi_2 X_{it} + \lambda_1 S_i + \lambda_2 \tau_t + \lambda_3 Tr_i + \lambda_4 Af_t + \beta_2 (Tr_i . Af_t) + \upsilon_{it}} \quad (2)$$

While I estimate number of up-to-date vaccines for a child (N_UTD_{it}) in Equation (1), I will estimate up-to-date status (Vac_{it}) for each of the nine vaccines in Equation (2). Vac_{it} shows whether or not (1 or 0) the child *i* is up-to-date in receiving sufficient doses of shots for any of the 9 recommended vaccines at time t. X_{it} is the matrix of the child's demographics and background such as parents' education, parents' age, parents' income, child's gender, and marital status of mother. S_i is the state dummy variable, τ_t is the time dummy, Tr_i is the dummy for treated group (equals one for children in treated states), and Af_t is the dummy for the after-

change period (equals one for years after 2010). Finally, since the policy change is expected to be *exogenous* to households (parents), the estimated β_1 and β_2 can be interpreted as the effect of the ACA on changing the probability of being up-to-date in terms of the number of vaccines or specific vaccine, respectively.

Poisson and *Logit* models would be the most sensible approaches to fit the data for count and binary dependent variables in Equation (1) and (2), respectively. However, interpretation regarding magnitude and significance of the interaction terms is controversial in non-linear models, as has been shown in Ai and Norton (2003), Norton et al. (2004), and Karaca-Mandic et al. (2012). Indeed, magnitude and significance are both conditional on the independent variables, and sign of the effect could vary, even to be of different sign from the conventional estimated effect. There are two ways to deal with this. First, is to employ linear regressions whose coefficients are simply the marginal effects. This is what Abrevaya and Hamermesh (2012) called as Linear Probability Model (LPM) whose marginal effects are easily inferred. Currie and Gruber (1996) have used the same linear approach to evaluate effects of the public health insurance – like Medicaid – on children's medical utilization and health outcomes. Since goal of this study is to investigate the causal effects of the ACA and not predictive modeling, this approach sounds easier and straightforward to make inferences.

Second and more time consuming way, is to employ more complicated calculations to take the values of the independent variables into the account (Norton et al., 2004).[6]

In this study, I will present Ordinary Least Squares (OLS) regression results, whose details are presented in appendix A′. Results from *Poisson* and *Logit* models can be found in appendix

[6] *inteff* command in Stata is one of the technical solutions to get marginal effects for interaction terms.

A. Almost all coefficients from OLS regressions and significant coefficients in particular, have consistent significance with non-linear models presented in appendix A. In order to make sure about the accuracy of the size and significance of the effects found in *Logit* and OLS models, I have then revised all the estimated interaction terms in main models, model (2) and model (4) of Table 1, by employing more complicated calculations recommended by Norton et al. (2004). Results which are consistent with all significant results from OLS and *Logit* models are presented in Appendix B. Only these two models were chosen since running the command is so time consuming.

2.4. Data

I use the NIS data from the 2008-2012 period, excluding 2010. NIS is a comprehensive dataset of the CDC that documents the immunization for all age cohorts. Each year phone interviews with parents of about 25,000 children are conducted. I include only children with private insurance through their parents' employer, since families with complete or partial coverage of Medicaid or other federal and state insurance programs were less likely subject to the copayments and out-of-pocket payments even prior to the ACA. Of 104,230 children covered by these interviews in 2008, 2009, 2011, and 2012, 43,833 (42%) had been covered only under private insurances and, thus, were subject to the ACA. Of those, 38,295 (87%) have complete information provided by their health providers. I excluded children without health provider data due to significant number of invalid or missing information.

NIS covers all 10 vaccines recommended by the ACIP.[7] The NIS dataset includes up-to-date status for eight vaccines studied here, with only Hep A and influenza excluded. Up-to-date status for Hep A is calculated by author, using the recommendation schedule.

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[7] National Immunization Survey: A User's Guide for the 2009 Public-Use Data File, December 2010, has counted the following for the up-to-date status of different vaccines:

1. Diphtheria and Tetanus toxoids and acellular Pertussis vaccine (***DTaP***) – 4 doses;
2. Poliovirus vaccine (***Polio***) – 3 doses;
3. Measles/mumps/rubella vaccine (*MMR*-***MCV*** (Measles-Containing-Vaccine)) – 1 dose;
4. Haemophilus influenzae type b vaccine (***Hib***) – 3 or 4 doses depending on product type;
5. Hepatitis B vaccine (***Hep B***) – 3 doses;
6. Varicella zoster (*chicken pox*) vaccine (***VRC***) –1 dose;
7. Pneumococcal vaccine (***PCV*** (Pneumococcal-Containing-Vaccine))– 4 doses;
8. Hepatitis A vaccine (***Hep A***), 2 doses;

2.5. Results

2.5.1. Base model

Model (1) in Table 1 summarizes the causal effect of the ACA – interaction of *after* and *treated* variables – after taking into account the effects of the covariates and adjusted for possible intra-correlations or clustered standard errors for observations within same state. The first row of Table 1 (and a more detailed Table A1′ in the Appendix) contains the regression results for the count of up-to-date vaccines. The ACA seems to have had a positive but marginally significant effect on number of up-to-date vaccines with a coefficient of 0.074 and p-value of 0.13. Results from *Logit* regression in appendix A1 show almost similar significance in outcomes.

The next rows in Table 1 (and more detailed Tables A2′-A10′ in the Appendix) show the results for each of the nine vaccines studied. A glance shows that ACA has significantly boosted up-to-date status for DTaP and VRC vaccines. For other vaccines, ACA's effect is usually positive but always statistically insignificant.

2.5.2. Alternative models and robustness checks

I next put the basic results of Table 1 through a series of robustness checks to account for potential non-similarity of treated and control groups, different legal and regulatory regimes, and causal effect among different segments of the sample.

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9. Influenza vaccine.

10. Rotavirus vaccine (***ROT***) – 2 or 3 doses depending on product type.

2.5.2.1. Non-similarity of treated and control groups

Any difference-in-difference estimation must establish the similarity of treated and control observations prior to the experiment to ensure the post treatment treated and control observations are not comparing apples with oranges. Propensity Score Matching (PSM) is done here to minimize the possible differences between treated and control observations' characteristics. Several studies suggest that combining PSM and DiD for repeated cross-section data results in even better performance than using either one (Blundell and Costa Dias, 2000- Smith and Todd, 2005). Blundell and Costa Dias (2000) suggests three times matching for each treated individual in after-change period: first, finding the matched treated in before-change period; second, finding matched control in before-change period, and finally, finding matched control in after-change period.

Model (2) in Table 1 uses the closest neighbor without replacement method of propensity score matching to address this issue. For matching, I first estimate the relationship between child (family) characteristics and whether the child is within the treated or control group. This ensures me that families in the treated states will be compared to peer families in control states, in terms of the income, education, etc. Then, I try to match any child from the treated group with one in the control group with the closest propensity score. I assume that NIS surveys are representative for the population—treated and control—over time, so I skip the first step mentioned above. Therefore, I do the matching procedure once for observations before 2010 and once for observations after 2010.

Results of model (2) in Table 1 show that matching makes ACA's effect on DTaP less significant but still positive while its effect on VRC becomes stronger. The overall effect of the ACA on the number of vaccines is still marginally significant and effects on other vaccines are still insignificant.

2.5.2.2. Regulatory issues: Grandfathered status of insurance policies

There is a grandfather clause on policies issued before March 23, 2010 that exempts them from many of the provisions in the ACA, including preventive services. The insured may retain their grandfathered status until a significant change in coverage happens. According to the to the Kaiser Family Foundation's Employer Health Benefits Survey in 2013, the percentage of covered workers enrolled in grandfathered plans have been 56%, 48%, and 36% in 2011, 2012, and 2013, respectively.

Since grandfathered status can interrupt the effectiveness of the ACA provisions and makes problems for identification strategy, I argue that the effects found in this chapter are the lower-bound effect of the ACA on immunization, as some portion of the policies has not experienced a real change by the ACA due to their grandfathered status.

One possible way to address this issue is to use only 2012 as the after-change period. Model (3) in Table 1 shows the estimations using only 2012 for the after-change period along with the matching procedure. However, since the diminishing rate of the grandfathered status was very slow and we lose half of the data for the after-policy change period, i.e. 2011, standard errors doubled in most cases and all effects became insignificant, except of the effect of the ACA on the VRC vaccine which is still statistically significant.

2.5.2.3. Legal differences: Universal Purchase and Universal Select Programs[8]

The majority of states depend primarily on federal resources to purchase vaccines. However, some states add their own programs to supplement these funds. As of 2010, six states (NH, NM, RI, VT, WI, and WY) had *Universal Purchase* programs where the state purchased all recommended vaccines for all children, including those who are fully insured. Six other states (AK, HI, ME, MA, SD, and WA) had *Universal Select* programs that purchase all recommended vaccines for all children with the exception of one or more vaccines. Once the vaccines are purchased, they are distributed to all public and private providers, who may charge an *administration fee*. Although cost is not considered as main driver of the results, having only an administration fee instead of the full price of a vaccine means lower effective price of the vaccine in the above-mentioned states, which may have impacts on identification strategy.

Excluding Hawaii and Wisconsin which were in control states, 10 out of 12 states mentioned above were in the treated group. I exclude those 10 from the treated group to observe more clear effect of the ACA in model (4) of Table 1. As expected, it shows a stronger and more significant effect of the ACA on the number of up-to-date vaccines, and in particular on the DTaP and VRC vaccines.

To check the results against non-linear estimation recommended by Norton et al. (2004), I estimate interaction term based on the independent variables which is shown in set of figures B2 in the appendix B. These figures show that magnitude and z-stat of the interaction effect vary by

[8] Information regarding these programs has been obtained from: National Conference of State Legislatures, *"Immunizations Policy Issues Overview"*, April 2011.

the values of covariates, which finally enables me to calculate average magnitude and average significance of interaction effect (term *"ie"* on the graphs) for whole population. Using this method, for example, results in average marginal effect of 0.0313 for interaction term (the ACA's impact) for DTaP in model (4) of Table 1, where estimated coefficient was 0.019 for OLS. I prefer to refer to OLS results as marginal effects since they come from model and not simply taking average of the effects found for all individuals. Based on model (4), therefore, the ACA has increased the up-to-date status of both DTaP and VRC by about 2 percentage point.

2.5.2.4. Legal differences: Vaccination Exemption Permissions[9]

States have different approaches regarding exemptions from vaccination requirements for schools and preschool daycares which could be another source of variation among different states.

As of 2012, all states were permitting medical exemptions which are essentially difficult to achieve. Also, all states except Missouri and West Virginia were permitting religious exemptions for immunizations. Both Missouri and West Virginia are among our control states, so they do not have any corresponding treated state. On the other hand, there were 19 states who were accepting philosophical, conscientious, and personal belief exemptions in addition to religious exemptions: AZ, AR, CA, CO, ID, LA, ME, MI, MN, MO, ND, OH, OK, PA, TX, UT, VT, WA, and WI. Out of the 19 states mentioned above, 5 states (AR, MO, OK, TX, and WI) are in our control group and 14 are in treated states. In order to have more clear impact of the ACA on childhood

[9] Information regarding this topic has been obtained from: National Conference of State Legislatures, *"Immunizations Policy Issues Overview"*, December 2012.

vaccination, I have excluded all 19 states above from my sample in model (5) of Table 1. This assures me that nothing other than the ACA has caused a change in coverage rate. Since I have lost significant portion of the observations, standard errors become larger and significance of the estimates observed in model (4) goes away. The only exception is Varicella vaccine whose coefficient became larger while still significant at 95% confidence level.

Table 1- Estimation of the ACA effectiveness to boost the coverage of nine recommended vaccines for children aged 19-35 months under private insurance coverage in OLS regressions (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Effect of the ACA on number of up-to-date vaccines	**0.074** (0.048)	**0.079** (0.058)	**0.032** (0.082)	**0.107*** (0.059)	**0.027** (0.066)
Effect of the ACA on *DTaP* vaccine	**0.013*** (0.007)	**0.016*** (0.009)	**0.008** (0.014)	**0.019**** (0.009)	**0.014** (0.013)
Effect of the ACA on *Hep A* vaccine	**0.018** (0.018)	**0.019** (0.019)	**0.013** (0.024)	**0.022** (0.020)	**-0.011** (0.027)
Effect of the ACA on *Hep B* vaccine	**0.001** (0.006)	**-0.002** (0.007)	**-0.001** (0.008)	**-0.001** (0.007)	**-0.008** (0.010)
Effect of the ACA on *Hib* vaccine	**0.003** (0.020)	**-0.005** (0.020)	**-0.017** (0.021)	**0.011** (0.020)	**-0.004** (0.024)
Effect of the ACA on *MCV* vaccine	**0.005** (0.007)	**0.005** (0.008)	**-0.001** (0.010)	**0.009** (0.008)	**0.007** (0.013)
Effect of the ACA on *PCV* vaccine	**0.003** (0.006)	**0.007** (0.009)	**0.003** (0.013)	**0.001** (0.010)	**-0.016** (0.014)
Effect of the ACA on *Polio* vaccine	**-0.001** (0.005)	**0.004** (0.006)	**-0.008** (0.008)	**0.006** (0.006)	**0.001** (0.010)
Effect of the ACA on *ROT* vaccine	**0.017** (0.019)	**0.017** (0.020)	**0.016** (0.023)	**0.019** (0.020)	**0.009** (0.029)
Effect of the ACA on *VRC* vaccine	**0.015**** (0.007)	**0.020**** (0.008)	**0.018*** (0.011)	**0.019**** (0.008)	**0.036***** (0.011)

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models. For list of variables, see tables A1-A10 in the appendix.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.5.3. Differences among different segments of the sample

Almost all the regressions in Tables A1-A10 and A1′-A10′ in the Appendix show that a family's income-to-poverty ratio and the education of the mother were quite important and strongly significant in explaining the behavior of parents. From another perspective, the ACA might affect parental choice about vaccination through change in cost-sharing mechanism, or by addressing some their concerns about vaccines. For example, vaccines with more safety concerns might gain increased coverage because parents who were not potentially choosing to vaccinate their children prior to the ACA are now receiving more information and incentive to follow the recommendations. Thus, I split the sample into two subsamples to study the effectiveness of the ACA in detailed subsamples.

Model (1) in Table 2 replicates results of model (1) in Table 1. I picked model (1) as base because there are few number of observations in the segment with below 300% poverty line income which results in even fewer observations and higher standard errors after matching process. Then, I split the sample into two subsamples: families with income of 300% or more of the poverty line and families with income of 300% or less of the poverty line. On average, 30% of the families have had less than 300% of the poverty line. Models (1-1) and (1-2) in Table 2 show the results for each segment. They indicate that while the ACA has shown more success with families with lower incomes for VRC, it has been more effective for higher income families in the case of the Hep A vaccine.

Due to positive correlation between parents' education and income, once I split the sample based on the mother's education, similar trends in the utilization of this new provision is observed, as expected.

Table 2- Estimation of the ACA effect on nine recommended vaccines, for subsamples including children in families with income below/above 300% of poverty line in OLS regressions (SE in parentheses)

		Subsamples depending on family's income	
		Subsample of children in families with income < 300% PL	**Subsample of children in families with income >= 300% PL**
Model #	**(1)**	**(1-1)**	**(1-2)**
Effect of the ACA on *DTaP* vaccine	**0.013*** (0.007)	**0.013** (0.010)	**0.008** (0.008)
Effect of the ACA on *Hep A* vaccine	**0.018** (0.018)	**-0.028** (0.023)	**0.037*** (0.022)
Effect of the ACA on *Hep B* vaccine	**0.001** (0.006)	**-0.004** (0.008)	**0.001** (0.002)
Effect of the ACA on *Hib* vaccine	**0.003** (0.020)	**0.014** (0.016)	**-0.007** (0.016)
Effect of the ACA on *MCV* vaccine	**0.005** (0.007)	**0.009** (0.010)	**0.002** (0.005)
Effect of the ACA on *PCV* vaccine	**0.003** (0.006)	**-0.015** (0.016)	**0.008** (0.008)
Effect of the ACA on *Polio* vaccine	**-0.001** (0.005)	**-0.001** (0.007)	**-0.002** (0.003)
Effect of the ACA on *ROT* vaccine	**0.017** (0.019)	**-0.001** (0.016)	**0.024** (0.025)
Effect of the ACA on *VRC* vaccine	**0.015**** (0.007)	**0.030***** (0.008)	**0.006** (0.006)

Note: a. All models with clustered standard errors for states.

b. There are other variables in the models. For list of variables, see tables A1-A10 in the appendix.

c. * p < 0.10 , ** p < 0.05 , *** p < 0.01

2.6. Discussion

Number of up-to-date vaccines: The ACA has been successful in boosting immunization status for children aged 19-35 months, especially for families living in states without universal/universal select programs prior to the ACA. Although the effects typically fall just below conventional levels of statistical significance, the consistent positive impact across models suggests a positive impact is likely.

DTaP and VRC: The ACA appears to be successful for these two vaccines. All estimates in Table 1 are statistically significant for VRC, and in the case of the DTaP, only estimates in model (3) and (5) are statistically insignificant. The ACA's impact has almost been doubled on VRC in states without non-religious exemption permissions in model (5) of Table 1. To check this, I limit the treated sample to those states with non-religious permissions and positive impact of the ACA on VRC disappeared. This means that the ACA has not been successful to boost VRC's coverage in the states where non-religious exemptions are allowed. In terms of policy implications, the ACA has not provided enough informational support to address the concerns regarding VRC vaccine. However, wherever vaccination had more restrictive regulations, the ACA has shown more successful to close the gap.

In summary, OLS results show that the ACA has increased the up-to-date status of DTaP by 1.6-1.9 percentage point while the increase has been about 1.9-3.6 percentage point for VRC.

Furthermore, the segmented models in Table 2 verify that VRC has experienced higher coverage among families with lower incomes, who reacted significantly to the policy change where the ACA has raised the up-to-date status by 3 Percentage point. For DTaP, on the other hand, both subsamples have shown positive and marginally significant response to the ACA.

Hep B, Hib, MCV, PCV, POL, ROT: The ACA has no observable effect for these vaccines. Other than a few negative but insignificant coefficients, the majority of the estimated effects were insignificantly positive. The marginally significant effect of the ACA on PCV vaccine for families with higher incomes in Table 2 is not enough to conclude a successful outcome, as it is not stable and changes with minimal change in the segmentation procedure.

Hep A: At first glance, Table 1 showed that ACA was not statistically successful for this vaccine. Once I split the sample into segments in Table 2, however, it reveals a significant effect of the ACA on families with higher income and higher education. OLS model in Table 2, for example, shows that the ACA has increased the up-to-date status of Hep A by 3.7 percentage point for the high-income segment while it was insignificantly negative for low-income segment.

2.6.1. Possible explanations

As I mentioned earlier, demand theory suggests very low price elasticity for vaccines. From empirical perspective, on the other hand, we do not have clear information about incurred vaccination cost for each family. So, I just know that cost could be one of the potential barriers for up-to-date coverage of vaccines (Orenstein et al., 1990). Cutts et al. (1992) and Shefer et al. (1999) noticed that cost effectiveness is one important element of "perceived barriers" of children's immunizations which may result in delay or refusing childhood vaccination.

Gust et al. (2008) show that few cases of delays or refusals were contributable to costs. In the study, vaccination refusal due to the costs was only observed in the case of VRC while DTaP has had the highest number of delays due to the costs. This suggests that cost of vaccination is still a minor concern. Looking at the costs of different vaccines shows that DTaP is the third most

expensive vaccine to complete, after PCV and ROT vaccines.[10] Therefore, among three successful vaccines mentioned above, highest price reduction due to the ACA has occurred for DTaP vaccine.

On the other hand, opportunity costs of not getting the vaccine could be even more important than its direct and explicit costs. There are several studies focused on indirect individual and societal savings of vaccinations. The probability of hospitalization and its costs in case of illness by vaccine-preventable diseases are different across vaccines. In a study, Zhou et al. (2009) have estimated costs related to vaccine-preventable diseases. According to their study, DTaP has the highest probability of hospitalization (almost 100%) and highest costs per hospitalization, too. These facts suggest that the ACA could potentially reduce costs of vaccinations for DTaP such that no delay happens due to the costs as was reported in Gust et al. (2008).

Cost is not the only barrier against full coverage. As mentioned before, there are concerns about necessity, and also about safety and side effects of vaccination which play much more important role than costs. According to Gust et al. (2008), ranking the total number of children with vaccination delay or refusal due to safety and side effect concerns, VRC with 69 cases would be in first place, followed by DTaP (32). Ranking with respect to the total number of delays and refusals due to doubts about vaccination effectiveness puts VRC with 55 cases at the top, followed by Hep B with 10 cases. Unfortunately, Hep A and ROT have not been covered in their work.

[10] Based on CDC vaccine prices per dose, for private sector, and for single vaccines list, updated on June 3, 2014: http://www.cdc.gov/vaccines/programs/vfc/awardees/vaccine-management/price-list/index.html

More importantly than cost reduction, the ACA could arguably be successful by spreading the information and support regarding safety and effectiveness of the recommended vaccines. From another perspective, combining cost reduction and indirect information perceived by parents regarding their non-price concerns might have affected parents; especially those who were undecided but just on the border. My results show that in states where non-religious exemptions was not allowed, effect is stronger, suggesting that the ACA has helped to convince concerned or undecided parents to get immunized.

In the case of Hep A, low coverage rate could be the answer to the reason of the success. We saw vaccination coverage rates for both control and treated groups in Figure 2. Although trends for both groups look similar, Hep A and ROT have had substantially less than an 84% coverage rate, which is the floor coverage rate for other vaccines. This relates to the fact that CDC recommended these two vaccines only recently.[11] Hence, I may consider ACA as a catalyst to expand the coverage for Hep A for all families, rich or poor, but more educated parents took more advantage from the ACA. But why ROT does not follow the same way? According to Zhou et al. (2009), while probability of hospitalization is 0–100 percent for Hep A with $11000–$33000 cost of hospitalization, these values are 0.5–3.8 percent and $3000–$4000 for ROT, respectively. However, vaccine's cost is less than half for Hep A. Therefore, one could argue that the ACA did not still magnify the benefits and reduce non-price concerns so much in case of the ROT vaccine to be significantly observable in data.

Another explanation for effectiveness of the ACA on coverage rate could be combination of the vaccines. The possible combinations of the vaccines include DTaP-POL (Kinrix), DTaP-Hep

[11] CDC schedules of recommended vaccines show that Hep A was added in 2006, and ROT was added in 2007.

B-POL (Pediarix), DTaP-POL-Hib (Pentacel), Hep A-Hep B (Twinrix), Hib-Hep B (Comvax), and MCV-VRC (Proquad). Since DTaP and Hep A vaccines are combined with other vaccines whose coverage rates did not change due to the ACA, this explanation does not sound relevant for their boosted coverage after the ACA. However, in the case of VRC, we know that it is almost always offered individually. While combinations of MMR (MCV) and VRC vaccines are possible as indicated above, but some studies, such as Klein et al. (2010), have concluded that this combination may increase safety risks. Therefore, vaccines combinations cannot provide additional explanation for the impact of the ACA on coverage rate.

Chapter 3

Effect of the Rebate on Insured's Retention Rate

An Applied Study

3.1. Introduction

Insurance industry in general and health insurance in particular, used to be categorized as a screening game in economics literature. Due to asymmetric information between insured and insurer, with less information about health conditions for the latter, the insurer tries to provide different options so that the insured self-select themselves into most appropriate product for their health conditions. Deductible, copayment, coinsurance, maximum out of pocket payment, and maximum lifetime payment are all examples of the options insurers use to enforce insured to self-select themselves according to their better-than-insurer known health conditions.

Since 2011, the ACA has added minimum MLR provision to the health insurance regulations. If an insurance company does not meet the minimum MLR of say 80% for Individual Medical (IM) policies, the left over should be sent back to the insureds, based on insured's contribution to the total premiums collected.

To calculate MLR, an insurer takes the sum of claims plus payments for healthcare quality improvement activities, divided by premiums less taxes and other regulatory fees. Of course, MLR calculation is done in aggregate and on a state-by-state basis for each line of business – large group/small group/individual medical insurances.[12] Therefore, eligibility to receive the rebate is determined based on all the premiums and claims submitted in a state and market combination, and not by an individual policy. Therefore, rebate eligibility and amount are not controlled by insured. Once the MLR calculated for each of the lines of businesses in a state[13], the rule of 80 or 85 percent may result in rebate eligibility for some of the insureds.

The goal of this study is to investigate the causal effects of the rebate on the lapse behavior of the insurdes. Therefore, in this case, investigation is around the insured's reselection among different alternative insurers in the market rather than self-selection according to different available insurance options. So, the decision turned to be whether or not to stay with current insurer for another year or month, i.e. retention, after rebate distribution.

Apparently, insurance rebate puts some windfall gains in the pocket of the insured. According to several studies, windfall gains could be used for consumption or saving, depending on whether the recipient views the windfall as a bonus or returned wealth. This study, however, aims to find out causal effects of the rebate on insureds' lapse behavior. While price and quality

[12] For a good and comprehensive discussion about MLR regulations under PPACA, see "Early Effects of Medical Loss Ratio Requirements and Rebates on Insurers and Enrollees", United States Government Accountability Office (GAO), July 2014

[13] There are more details and some exceptions for MLR calculation. For example, insurers with small number of enrollees can make certain adjustments to their MLR. For complete details see GAO report of July 2014 as indicated above.

of the services are still the most important determinants of the insured's demand for insurer's services, rebate may play an important role in insured's renewal decision based on her perception of the received windfall gains, like what was mentioned for consumption/saving decision. If the windfall gains are viewed as a rebate or returned wealth, it will more likely work as forced savings where the insured might be more inclined to stay with current insurer as opposed to look at that as a one-time bonus which looks like a noise and does not make any change in her lapse behavior. Thus, the question would be whether or not the inured views the received rebate as a rebate or bonus.

Lapse/stay decision is, however, different than consumption/saving behavior in some aspects. First, insurance rebate is less determined by insured's self actions, and is basically based on the overall risk of the insurance pool. Also, some other important factors like number of alternative insurers in the market may impact the insured's perception and decision, too.

From empirical perspective, since eligible insureds have been subject to an exogenous policy change (rebate eligibility) and due to the independence of the rebate eligibility and characteristics of the insured, any change in the behavior of policyholders could be attributed to the causal effect of the policy change. Since only part of the population became eligible for the rebates, the causal effect could be measured by gauging the difference in the lapse behavior between eligible and non-eligible insureds, after taking into the account the effects of the other covariates. Thus, I employ a Difference-in-Difference (DiD) identification strategy by using rebate recipients as treated group and non-recipients as control group in this quasi-experiment.

This chapter continues as follows: Section 2 includes literature review and expected outcomes. In section 3, I will explain about data and feasible identification strategy. Results are presented in section 4 and section 5 concludes the discussion.

3.2. Backgrounds

3.2.1. Income timing/unexpected income

There are several studies about the effect of the change in timing of the income or unexpected income on behavior of the consumers. Shapiro and Slemrod (1993), for example, have studied the effect of reduction in tax withholding in 1992 on consumers' behavior. They found that timing of the income did really change the consumption behavior, in contradiction to permanent-income/life-cycle hypotheses, but substantially less than one hundred percent aimed by policy-makers. Also, Jones (2010) explains why taxpayers do not utilize gains from the change in federal tax withholding in 1992. He adds inertia as another explanation for negligible change in withholding in response to the change in regulations, in addition to precautionary behavior and forced saving motive explanations.

On the other side, effects of the unexpected incomes –windfalls– on consumption path have been subject of numerous theoretical and applied studies. In an experimental study, Arkes et al. (1994) provide psychological explanation for consumers' behavior regarding windfall gains. They propose two different curves for objective dollar amount of the income and subjective value of that. Conclusion is that subjective curve changes when income is perceived as windfall gain instead of regular income, resulting in higher spending rate out of windfall gains. In an effort to explain different consumption patterns for bonuses versus rebates, some studies have compared additional income with someone's current wealth, called difference in income framing or description of the income. They distinguish between objective and subjective gains from income and argue that consumers see the rebate as a returned loss from *previous* wealth state in

contrast to a bonus as a gain from *current* wealth state. Therefore, they predict larger spending from bonuses in comparison to the rebates (Epley et al. (2006) - Epley and Gneezy (2007)).

Billions of dollars tax rebates related to the federal government fiscal policy in 2001 provided a good real experiment for effect of the windfall incomes on consumption. Several studies have focused on this change and most found that the change in consumption was much lower than expected, like what Shapiro and Slemrod (2003, 2009) found, which can be explained by above theories.

Although all mentioned theories and experiments focused on the consumption path out of the windfall gains received by a person, this study focuses on testing whether insured views the rebate as a returned wealth or forced saving which creates a tendency towards staying with current insurer and likely receive another rebate next year, or as a one-time bonus without a significant impact on the lapse rate. Basically, if the insured sees the rebate as a gain from previous wealth state, the subjective value of this forced saving would be higher than if it was seen as a gain from current wealth state, i.e. a bonus. Therefore, higher value of the perceived change in wealth in first case may incentivize the insured enough to affect her stay/lapse decision.

In addition to the insured's perception regarding the rebate, there are some other important variables that may affect insured's expected rebate in next period once she receives the rebate in current period, and thus, encourage/discourage her to keep the policy. In next section, I will propose some of the expected changes in the behavior of the insured, after receiving the rebate.

3.2.2. Expected outcomes

In order to predict the behavior of the insured, I consider all parameters with possible impacts on insured's decision regarding stay/lapse. Rebate eligibility, health insurance market concentration, rebate frequency in the market, and rebate amount are the ones I can imagine with potential impacts on the lapse behavior.

Rebate eligibility: Obviously, this is the most important and most relevant variable affecting insured's lapsing behavior. While the argument that rebate may keep the insured hopeful for the next rebates is still in place, it might be argued that rebate sends information that insurer services were originally overpriced. However, since the services –including doctors' network, drug coverage, etc– was selected by the insured in the past, the rebate is more to incentivize one to stay with the company to continue to receive the financial return. Therefore, I expect to see lower lapse rate for those received the rebate.

Insurance market concentration: Count and availability of alternative insurers in the market may impact decision of the insured for next period's enrollment as well. In more concentrated markets where searching and switching cost goes up due to less available alternatives, the policy is more likely to be kept. On the other side, more alternative available in more competitive markets might still convince insureds to switch, even after receiving new information –rebate; especially if I consider the positive relationship between market competition and rebate frequency which comes later.

Rebate frequency in the market: Once insured receives the rebate, s/he looks around to see how popular rebates are. Less frequent rebates results in stronger incentive for the insured to stay with current insurer. For example, if rebate receiver sees that almost everyone else has received

the rebate too, there would be the same likelihood of the rebate for the next year regardless of the insurer. Therefore, I expect to see positive relationship between lapse rate and rebate frequency in the market.

Rebate amount: Beside the rebate eligibility, its amount indicates the worthiness of taking this new information into the account once revising stay/lapse decision. Apparently, negligible rebate should have no impact on decision while larger rebates –especially if it can be expressed as proportional to the paid premium, increase the chance of staying with the same insurer.

There are other variables which generally have influence on the retention which should be included in the model like policy duration, age of the head of the policy, number of lives under the policy, etc. Next, I will discuss about availability of the variables described so far and how to prepare the data for analysis.

3.3. Data and Methodology

3.3.1. Data

I have used data on Individual Medical (IM) health insurance policies from one of the private health insurance companies in the US that were subject to the minimum 80% MLR regulation.[14] Data contains information about demographics (gender, age, number of lives under policy, and state), insurance policy (channel, product type, deductible, policy class, preferred, rating, and adverse action indicators, duration, rebate amount, and exposure[15]), and lapse indicator for any month between January and October, 2012. I will explain later that I add an indicator for competition level in IM insurance market within different states to measure the market power of the insurer and concentration in the market.

Although I have 140,702 policies during this 10 month time window with lapse exposure, only 118,243 of those were initiated on or before January 2012, i.e. policies that were potentially eligible for the rebate, based on 2011 performance. Since goal is to figure out effect of rebate on lapse rate, I only include policies. About 30% of these policies received the rebate in 2012. Table 3 shows summary statistics for this sample.

[14] Insurer has only provided a subset of the data for research purposes. Disclosure of the name or data of the insurer is prohibited. Data provided by insurer is not representative for the complete dataset and thus, results are neither necessarily in the line with true results, nor under approval of the insurer.

[15] Exposure indicator shows whether or not the policy is eligible to lapse within that month. For example, partial month enrollment is not possible at the beginning of policy initiation so that policy cannot be terminated within the starting month for partial enrollment.

Table 3- Summary statistics of the sample

Number of policies initiated on or before January 2012	118,243
Average number of lives under policy	1.77
Average age of primary policyholder	45.02
Average duration (month)	43.19

I also include information about the rate of competition in the market and rebate frequency in the market, both calculated at the state level, which will be explained later.

3.3.2. Identification Strategy

Although a linear or non-linear regression of the binary choice variable of Lapse (0/1) as a function of the explanatory variables explained so far is an option, there are two main concerns about this approach: First, the coefficients, including the one related to rebate eligibility, just show the correlation between lapse and explanatory variables, without any logical causality inference. Secondly, although rebate eligibility is based on the market's risk pool in state level, it is not completely independent from policyholder's characteristics. For example, correlation coefficient between rebate eligibility and renewal month of the policy (*rn_on_ind*) or duration are 0.3 and 0.2, respectively. This means that there could be some unobserved characteristics of the policyholder –which are accumulated in error term– that may have correlation with rebate eligibility. This does not comply with the assumption for independence between covariates and error term, resulting in biased coefficients.

In order to deal with above mentioned concerns, I attempt to simulate a semi-randomized experimental analysis based on available information. Then, I address the second issue by matching observations based on their backgrounds.

Since rebate is an exogenous change to a policyholder, I divide the sample into two groups: the *treated* group which includes rebate-eligible insureds, and a *control* group including non-eligible ones. Rebates were actually determined at the end of the 2011, based on medical claims; however it was not determined until processing all the accounts which usually occurs about end of June of the following year. Therefore, even insureds that were eligible for 2011 rebates might lapse before rebate determination and distribution in July 2012. Thus, I can argue that the rebate is determined out of policyholder's control and regardless of their characteristics[16]. Hence, the conditional independence assumption holds and I can consider differential changes in the lapse rate between treated and control groups as a causal effect of the rebate once I take into the account the effects of other covariates. Therefore, I pursue a Difference-in-Difference (DiD) strategy to identify the causal effect of the rebate eligibility on lapse rate by estimating Equation below:

$$\mathbf{Lapse_{it}} = \boldsymbol{\mu} + \boldsymbol{\phi_1 X_{it}} + \boldsymbol{\delta_1 S_i} + \boldsymbol{\delta_2 Tr_i} + \boldsymbol{\delta_3 Af_t} + \boldsymbol{\beta_1}(\mathbf{Tr_i}.\mathbf{Af_t}) + \boldsymbol{\varepsilon_{it}} \qquad (3)$$

Here, I estimate the likelihood of the lapse (0 or 1) for a policy *i* in a time period of *t* that is simply before or after the policy implementation ($Lapse_{it}$) as a function of a matrix of the demographics X_{it}, state fixed effects of S_i, a dummy variable of Tr_i for whether or not the policyholder is in treated group (equals one for policies eligible for the rebate), and a dummy variable Af_t for *after-change period* (equals one for months after rebate distribution, i.e. on or after July 2012). Finally, interaction term of Tr_i and Af_t will show the policy effect since it would be one only for treated policies (subject to the rebate) and after the change period. Hence,

[16] Like any semi-randomized experiment, however, there might be some correlations between treatment assignment and covariates. I will try to minimize selection biases by using propensity score matching later.

β_1 could be interpreted as the effect of the rebate eligibility on changing the probability of lapse in the treated group, or more explicitly, the causal effect of the rebate eligibility on the lapse behavior.

Equation (3) above is a binary estimation – $Lapse_{it}$ is zero or one, so I can use a non-linear binary estimation model like a *Probit* or *Logit* model. However, interpreting the magnitude and significance of the interaction terms in non-linear models is controversial (Ai and Norton, 2003, Norton et al., 2004, and Karaca-Mandic et al., 2012) and requires more complicated approaches (Norton et al., 2004). Instead, I employ an OLS approach, like what has been done by Abrevaya and Hamermesh (2012) and Currie and Gruber (1996), in parallel with the *Logit* model. Only results from the OLS model are presented here but they are all consistent with *Logit* outputs in terms of the sign and significance.

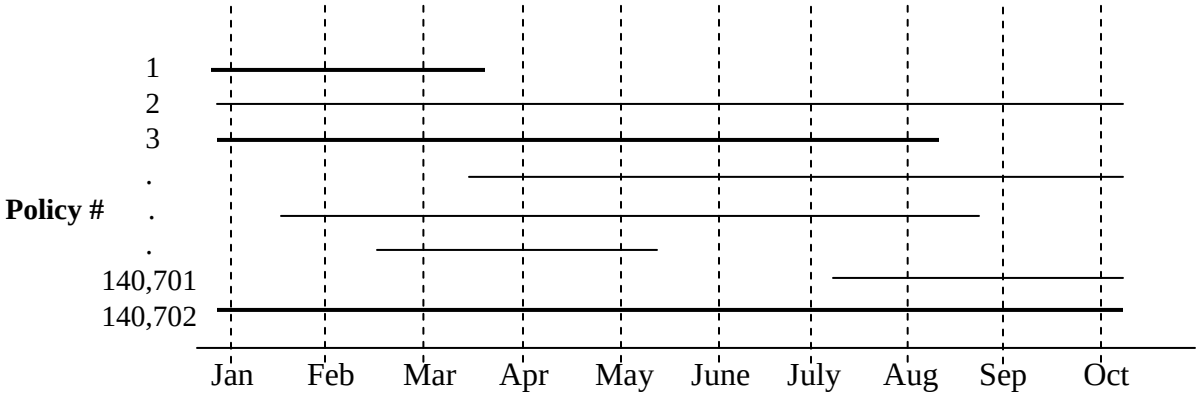
3.3.3. Data preparation

From January through October 2012, there were 140,702 policies in the sample, some of which were active in only part of the time frame. This is shown in Figure 4 below. Even if a policy was initiated after January 2012, I do not exclude it from sample as long as it meets the criteria for the *after-change* period. I will explain about this later when I group observations into before and after change samples.

July 2012 was the date that rebate-eligible policies were aware of their eligibility and received their checks in the mail. So, I divided the time frame into "*Before"* (before July) and "*After"* (on or after July) time periods as follows: Any policy initiated on or before January 2012 would be in "*Before"* group. I keep looking at those policies in the January-April window to see

who lapses within these four months. Any policy initiated on or before July 2012 would be in "*After"* group. I follow them as well in the July-October window of to see lapses. I chose same time window for both groups to make them as much as comparable. Therefore, it is possible for a policy to be in both groups since some policies like #2, #3, and #140,702 have been active at the starting time of either period. On the other hand, some policies are only in one time window, and some are in neither.

Figure 4- Available information for Jan-Oct of 2012; treated (rebate eligible) policies are shown in bold



In Figure 5, I have 4 policies in "*Before"* period (#1, #2, #3, and #140,702), 3 of whom are treated (rebate eligible, or bold); one of those, #1, has lapsed before end of the period. On the other hand, there are 5 policies in *after* period (#2, #3, #4, #5, and #140,702), 2 of whom were rebate eligible (treated or bold). Among those 5, one treated policy and one control policy have lapsed within the four months time window, #3 and #5. Note that some policies are in neither before nor after time windows, like #140,701.

Once I remove policies initiated and ended in February-June window, and policies initiated after July, 8,716 policies disappear, leaving 131,986 policies in the original dataset. Now, once I

use the criteria to split the sample into *"Before"* and *"After"* groups, 124,782 policies sit in the *"Before"* subsample, while 108,121 policies are located in *after* subsample, with 100,917 policies shared between two subsamples. I stacked up these two subsamples to end up with 232,903 observations in the sample for DiD regressions.

Figure 5- Dividing observations to *before/after* periods, *treated/control*

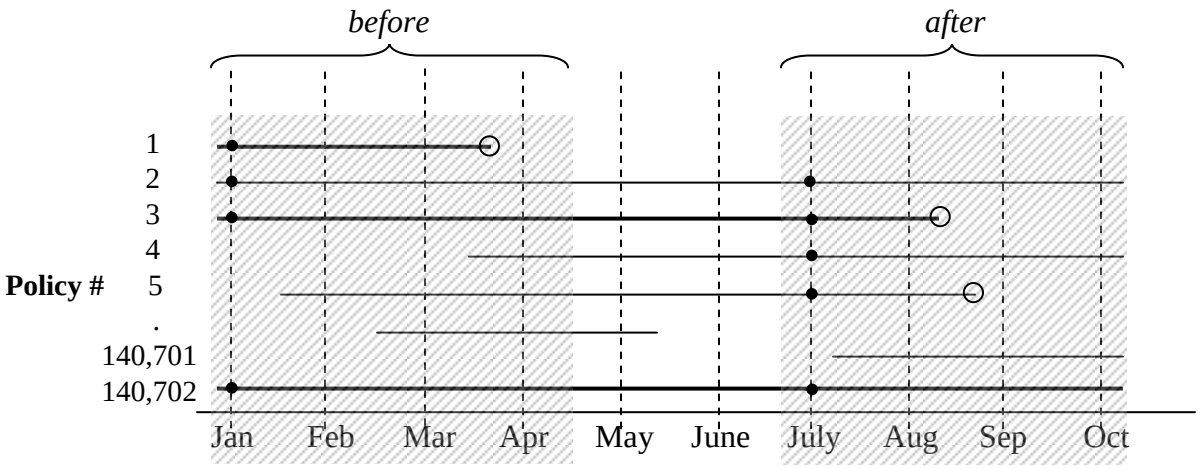


Table 4 shows the summary statistics for *"Before"* and *"After"* subsamples, for both *treated* and *control* groups.

Obviously, *treated* group shows lower lapse rate after policy implementation while it had a little higher lapse rate before change. This is a clue for positive effect of the rebate policy on retention. However, *treated* group has longer duration (over 13 months), is more likely subject to price and underwriting ratings, and it includes older people. These all suggest that treated people that were eligible for rebate are more likely older and unhealthier insureds on average, which means that they are subject to higher premiums and more expensive plans. One necessary condition for DiD strategy is to have common trend before the treatment. Panel (a) of Figure 6 shows the lapse trend for both treated and control groups for before/after change period.

Although trends before rebate distribution look very similar, I will try to make groups even more similar later when I try to match observations from control group to ones from treated group, based on propensity scores, in order to reduce possible selection biases.

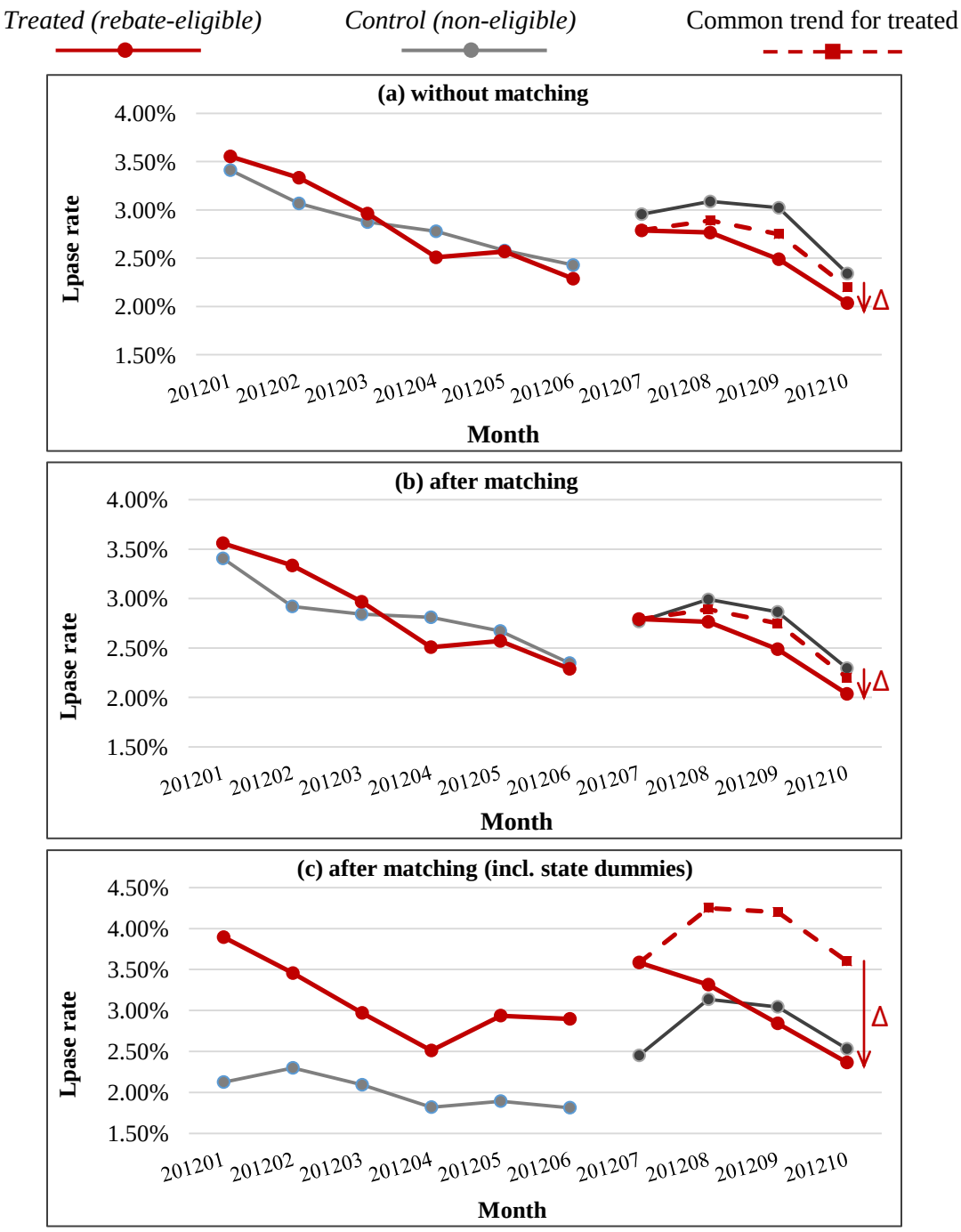
Table 4- Summary statistics of the sample for DiD regressions, for *before/after* and *treated/control* groups

	Counts and Averages for *Before (Jan - Apr)*		**Counts and Averages for *After (Jul - Oct)***	
Characteristic	***Treated*** ***(Rebate elig.)***	***Control*** ***(Rebate non-elig.)***	***Treated*** ***(Rebate elig.)***	***Control*** ***(Rebate non-elig.)***
# Policies	44,756	80,026	35,867	72,254
Lapse Rate (%)	12.36	12.13	10.08	11.41
FY_ind (% with one year or less duration)	19.24	26.58	9.05	29.21
Rn_on_ind (% with renewal took place)	44.69	36.78	49.34	31.50
Preferred_ind (% with Preferred Rating)	35.64	35.41	35.29	35.88
Rating_ind (% with Price Rating)	19.17	12.69	18.91	12.43
Adverse_action_ind (% with Underwriting Rating)	29.84	21.37	29.91	21.23
Integrated_ind (% with Integration Indicator)	5.79	7.43	5.56	10.14
# Lives under policy	1.81	1.74	1.80	1.74
Age of primary policyholder	45.66	44.41	46.38	44.63
Final rebate amount ($'000)	0.299	0	0.303	0
Duration @ beginning (months)	49.63	36.18	57.65	38.34

Indeed, I am trying to evaluate the significance of the delta (Δ) shown in panel (a) of Figure 6, which shows deviation from common trend we have observed in before-change period. Of course, delta shown in Figure 6 is just for last month of *after* period while regressions reveal overall deviation from common trend for *treated* group during *after* period. A simple calculation from Table 4 reveals that while *control* group has shown about 0.7 percentage point lower lapse rate after policy implementation, *treated* group has shown about 2.3 percentage point decrease in lapse rate, meaning that rebate has decreased the lapse rate by about 1.6 percentage point, which

could be interpreted roughly as causal effect of the rebate. However, I should capture the effects of other covariates first, to end up with more reliable causal effect of the rebate.

Figure 6- Lapse ratio trend for *treated* versus *control* group for *before/after* rebate distribution



3.3.4. Matching based on propensity scores

Now, I go back to the question that how comparable are treated and control groups in terms of backgrounds prior to the experiment? Table 4 showed that although treated and control groups are very similar to each other in many aspects, there might be concerns about selection biases raised from the fact that less healthy people are more likely rebate eligible; but we know that they lapse less, too. In order to verify common trend for treated and control groups, I employ a propensity score matching based on most important characteristics of the treated group, like age, duration, channel, product type, policy class, and rating determinants, in order to address this concern.

In the first step, to match observations, I regress the likelihood of the treatment (rebate *indicator* as dependent variable) on above independent variables. Since I do not have a panel data set, I do this separately for before and after periods. Then, I pick the observation in the control group with nearest propensity score for any one of the observations in the treated group and delete that observation from the potential control group. I then continue matching for rest of the treated observations.

Table 5 shows the summary statistics for the matched couples. Now treated and control groups sound more similar for either before and after periods. However, as panel (b) of Figure 6 shows, the common trend has not changed in comparison to the base case in panel (a). Once I include state dummies in the matching procedure, however, similar trend is clearly visible, as shown in panel (c) of Figure 6. The only issue for this last matching is that since no rebate was paid in several states, I will lose more than half of the observations.

Table 5 shows that while reduction in lapse rate in treated group is about 2.29 percentage point after the policy change, lapse has decreased by about 1.05 percentage point for matched control. Therefore, rebate shows effective to keep policyholders by about 1.24 percentage point lower lapse rate. I should take into the account the effects of other covariates and trends, however. Thus, in second step, I do OLS regressions using only matched observations from control and treated groups.

Table 5- Summary statistics of the sample for DiD regressions after 1:1 matching, for *before/after* and *treated/control* groups

	Counts and Averages for *Before (Jan - Apr)*		**Counts and Averages for *After (Jul - Oct)***	
Characteristic	***Treated*** ***(Rebate elig.)***	***Control*** ***(Rebate non-elig.)***	***Treated*** ***(Rebate elig.)***	***Control*** ***(Rebate non-elig.)***
# Policies	44,688	44,688	35,810	35,810
Lapse Rate (%)	12.37	11.97	10.08	10.92
FY_ind (% with one year or less duration)	19.14	27.04	8.93	21.44
Rn_on_ind (% with renewal took place)	44.74	43.60	49.29	46.90
Preferred_ind (% with Preferred Rating)	35.65	38.76	35.30	38.02
Rating_ind (% with Price Rating)	19.18	17.17	18.92	17.58
Adverse_action_ind (% with Underwriting Rating)	29.84	27.50	29.92	28.19
Integrated_ind (% with Integration Indicator)	5.79	5.56	5.57	4.65
# Lives under policy	1.81	1.84	1.80	1.82
Age of primary policyholder	45.66	44.94	46.37	45.79
Final rebate amount ($'000)	0.299	0	0.303	0
Duration @ beginning (months)	49.64	41.73	57.69	48.70

3.4. Results and discussion

3.4.1. Effect of the Rebate

Table 6 below shows the estimated effect of the rebate eligibility on the lapse rate, or one minus the retention rate, using OLS regression. Model (1) shows estimated coefficients for DiD regressions, which reveals the positive effect of the rebate on the retention after taking into the account the similar trends shared between eligible and non-eligible insureds for the rebate.

While the indicator for rebate (or treatment indicator) shows positive effect on lapse, coefficient for after treatment is insignificantly negative, meaning that lapse in July-October period was insignificantly lower than lapse in January-April period. Most importantly, *DiD* interaction coefficient –which shows the coincidence of rebate eligibility and after period– has s significant negative impact on the lapse. The size of the coefficient is -0.017, which means that rebate has decreased the likelihood of the lapse by 1.7 percentage points for those eligible for rebate, after capturing the effects of covariates and taking the common trends of treated and control groups into account. This estimated impact is close to the rough estimation of 1.6 percentage point from Table 4.

Model (2) in Table 6 shows estimated coefficients for DiD regressions after matching. Rebate's impact is now smaller in magnitude than what was in model (1), and it becomes marginally significant, with p-values of 0.12. Like before, estimated marginal effect of -1.3 percentage point is very close to drop of 1.24 percentage point observed in Table 5.

Again, note that no insured was aware of the rebate eligibility prior to rebate distribution in June and July of 2012; hence, this differential in the lapse could be interpreted as the negative causal effect of the rebate on lapse.

In model (2), indeed, clustered standard errors within states makes the DiD coefficient marginally significant, at 88-89% confidence interval. In other words, if I use simple or even robust standard errors instead of clustered standard errors, the coefficient becomes significant at 99% confidence interval while it has the same magnitude. There might be several reasons behind this. Correlations between regressors and errors within cluster, or same sign regressor–error correlations within clusters are the important ones, as described by Cameron and Miller (2013). They indicated that the last one is the usual reason for bigger clustered standard errors for DiD regressions.[17] In our case, since observations in a cluster –state– have very close backgrounds, at least in terms of the type of product, policy class, and channel, it is expected to see same sign correlations when we do the regression over all observations regardless of cluster. As I mentioned, however, the impact is still negative and marginally significant.

[17] For detailed discussion and proofs see Cameron and Miller "A Practitioner's Guide to Cluster-Robust Inference", October 15, 2013.

Table 6- Results of OLS regressions for lapse (0 or 1) on demographics, state dummies, and DiD variable (SE in parentheses)

	DiD model	*DiD model after 1:1 matching*	*DiD model after 1:1 matching (incl. state dummies)*
Model #	**(1)**	**(2)**	**(3)**
Intercept	0.173 (5.875)	0.153 (6.523)	0.441*** (0.055)
Gender (= F)	-0.001 (0.003)	-0.003 (0.003)	0.002 (0.005)
# of lives	0.002 (0.002)	0.002 (0.002)	0.003 (0.002)
Age	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Duration @ beginning (month)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001 (0.000)
Rebate Amount ($'000)	0.002 (0.005)	0.006 (0.004)	-0.006 (0.006)
Rebate eligibility (= 1)	0.029*** (0.008)	0.036*** (0.009)	0.055*** (0.011)
After period (Jul-Oct = 1)	-0.003 (0.004)	-0.006 (0.005)	0.031*** (0.006)
DiD (Treated and After Change = 1)	**-0.017**** (0.007)	**-0.013** (0.008)	**-0.035***** (0.011)
No. of observations	232,328	160,996	64,860
R^2	0.011	0.012	0.012

Note: a. Model 1: Naïve regression using all policies initiated on or before January 2012, clustered for observations within same states.

Model 2: Difference-in-Difference (DiD) model, with clustered standard errors within states. Jan-Apr window is considered as *before* change period, and Jul-Oct as *after* change period. So, any policy initiated on or before January 2012 is considered in *before* period, and any policy initiated on or before July 2012 is considered in *after* period.

Model 3-4: DiD regression after matching one eligible policy to the nearest control policy (non-eligible) in terms of characteristics, with clustered standard errors within states. Matching is done for before and after periods separately. In model 4, state dummies are included in propensity score matching process.

b. There are other variables in the models including: state dummy variables, channel, product type, policy class, different rating factors, and frequency of the rebate in the state of residence.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Another way to approach the matching procedure is to consider state dummies in matching. However, since rebate eligibility strongly depends on the state dummies – such that rebate was paid to all insureds in some states or no one received the rebate in some other states – this removes insureds of many states from the sample. In fact, less than half of the states here include

both eligible and non-eligible insureds for rebate. Thus, I lose about 60% of observations by including state dummies within matching variables. On the other hand, I can argue that this reveals the impact of the rebate on retention much better due to closer similarity of the treated and control insureds. Model (3) in Table 6 shows results of this approach. Now, the impact is much stronger than what observed in model (1) and (2). Model (3) shows that while lapse had been more frequent in after period (July – October), those eligible for rebate have shown much less lapses.[18]

3.4.2. Effect of market concentration

It was mentioned in section 3.2 that due to fewer numbers of alternatives in more concentrated health insurance markets, rebate would be expected to show a stronger impact on lapse rate. However, major obstacle to estimate the effect of the market concentration on lapse behavior is the data limitation. There is almost no data about market concentration for detailed segments of the market like counties or zip codes. Also, since health insurance, like many other services, is a network service which might be delivered in close but not exactly same neighborhood, a wider geographical region should be considered to measure the rate of competition in the market. Here, I gather the data regarding competition rate in the individual medical insurance market for each of 50 states and District of Columbia from Kaiser Family Foundation in the first three columns of the Table 7 below.

[18] Summary statistics which are not shown here indicate that while treated group lowered the lapse rate from 12.83% to 12.10%, matched control group has experienced increase in lapse rate from 8.3% to 11.16%, a rough causal effect of about 3.6 percentage point decrease in lapse for rebate, but very close to -3.5% found in Table 6.

While it is possible to use either of the three concentration measures in Table 7, I chose the Herfindahl Hirschman Index (HHI) since it contains information about both number of insurers and their market shares. Also, the remained of Table 7 shows how I have estimated the rebate frequency for IM insurance policies in each of 50 states and District of Columbia, based on information from the Center for Consumer Information & Insurance Oversight (CCIIO) report. Similar to the concentration in the market, I have one number – rebate frequency – for each state. I will use this measure in the next section. Before talking about results, however, I should note that there are some important issues that should be considered before statistical analyses: First, market concentration and rebate frequency are measured at the state level, and therefore, have correlations with state dummies. Also, these two measures are correlated with each other which calls for more cautious in interpreting the results. I show and address both issues below.

Having state-level measures of market concentration and rebate frequency results in impossible estimation for effect of these covariates in presence of the state dummies due to collinearity. Therefore, at least one state dummy should be dropped in order to include each of these measures in the results.

On the other hand, most of the states with high rebate frequency are more competitive as well; for example, they have 4 or more insurers with over 5% market share or lower HHI, as Table 7 indicates. This means that states with less frequently paid rebate were on average less competitive, too. This sounds reasonable since health insurance markets are regulated such that market regulation and competition are negatively correlated. Figure 7 shows scatter plot of the number of insurers with 5+ percent market share and rebate frequency derived in Table 7. I found a correlation coefficient of 0.55 between the dummy variable for 20% or more rebate frequency (*high_freq* = 1 if rebate frequency >= 20%) and dummy variable for 4 or more

insurers with 5+ percent market share (*comp* = 1 if N >= 4). There is similar negative relationship between HHI and rebate frequency.

Due to mentioned issues, especially the interactions with state dummies, I did not include market concentration and rebate frequency variables in the regressions of Table 6. Instead, I split the sample to subsamples according to the variable of interest –market concentration or rebate frequency, to see how rebate has influenced the lapse behavior among different subsamples. This is the way to observe difference-in-difference-in-difference (DiDiD) effect of the rebate, i.e. difference in the causal effect among different segments of population affected in comparison to their peers. If I had not had these issues, I might be able to include dummies like *comp* and *high_freq* mentioned above, and their interaction with DiD variable in order to have DiDiD coefficients.

Table 8 shows the different causal effects among different subsamples, after matching observations and with clustered standard errors –similar to model (2) in Table 6– using OLS regressions. Comparing models (5-1) and (5-2) reveals that causal effect of the rebate on lapse is significantly negative in less competitive markets, while it is insignificantly positive for more competitive markets. This is exactly what was predicted. Model (5-2) in Table 8 shows 3 percentage point reduction in lapse rate in less competitive markets while lapse rate has not changed significantly in more competitive markets. In splitting the sample, I included markets with HHI of 2500 or under in more competitive subsample and those with HHI of greater than 2500 in less competitive subsample. This is partially due to sample size considerations, but also in the line with Federal Trade Commission (FTC) guidelines.

Figure 7- Positive Correlation between market competition and rebate frequency

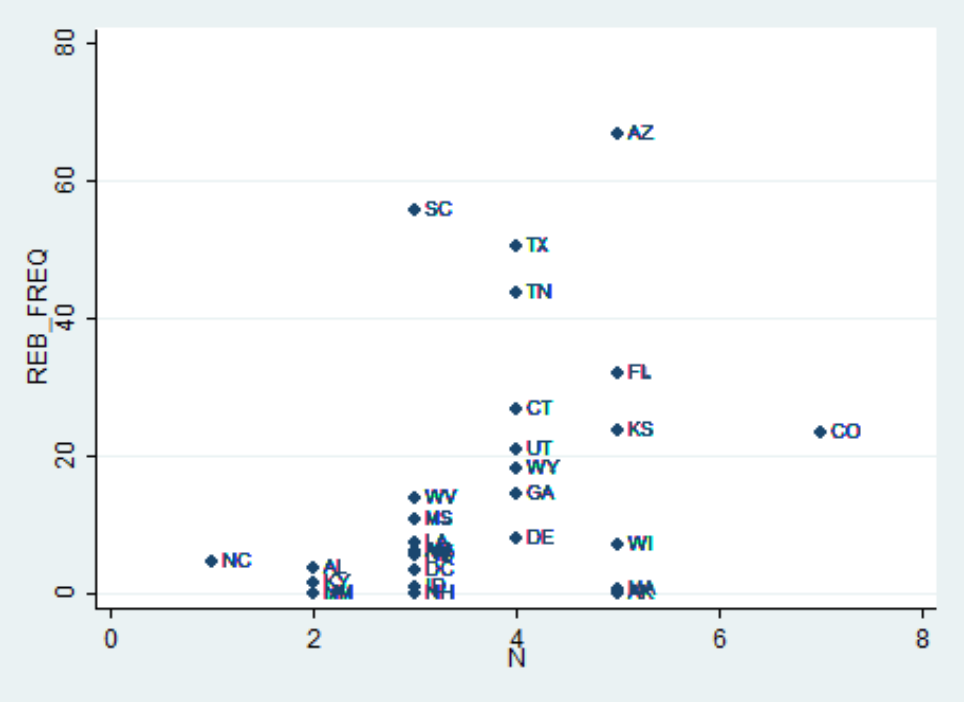


Table 7- Individual Medical (IM) insurance market concentration, and rebate frequency in the U.S. and in different states

Market/ State	Individual Medical Insurance Concentration, 2012*			Individual Medical Insurance characteristics and Rebate Status					
	Herfindahl Hirschman Index (HHI)	Market share of the largest insurer	# of insurers with market share >= 5 %	Total IM Rebate ($ million)**	# IM Enrollees Got Rebates**	Avg Rebate per Family**	Population, July 01, 2012 (million)***	Privately Insured, other than through employer****	Rebate Frequency in IM market
US	N/A	N/A	N/A	393.88	4,122,682	$152	313.91	6%	21.89%
Alabama	8213	90%	2	3.19	8,718	$582	4.82	5%	3.62%
Alaska	3810	60%	5	0.00	0	$0	0.73	6%	0.00%
Arizona	3169	50%	5	12.69	218,153	$97	6.55	5%	66.58%
Arkansas	6499	80%	3	0.53	12,406	$75	2.95	7%	6.01%
California	3052	47%	3	20.51	956,514	$30	38.04	7%	35.92%
Colorado	1731	34%	7	3.06	109,460	$44	5.19	9%	23.44%
Connecticut	2962	45%	4	3.99	47,990	$124	3.59	5%	26.73%
Delaware	3068	49%	4	0.96	2,948	$461	0.92	4%	8.04%
D.C.	5170	70%	3	0.15	1,908	$103	0.63	9%	3.35%
Florida	2771	48%	5	47.26	308,944	$240	19.32	5%	31.99%
Georgia	2334	40%	4	2.89	85,442	$51	9.92	6%	14.36%
Hawaii	4805	52%	2	0.00	0	$0	1.39	4%	0.00%
Idaho	3401	45%	3	0.14	1,083	$323	1.60	9%	0.75%
Illinois	4637	67%	3	7.79	60,787	$199	12.88	8%	5.90%
Indiana	4077	61%	3	2.84	42,320	$128	6.54	6%	10.79%
Iowa	4712	64%	2	0.00	0	$0	3.07	7%	0.00%
Kansas	2511	42%	5	3.54	54,763	$101	2.89	8%	23.72%
Kentucky	6643	80%	2	0.23	2,830	$150	4.38	4%	1.62%
Louisiana	5564	74%	3	2.86	23,866	$193	4.60	7%	7.41%
Maine	3854	49%	3	0.00	0	$0	1.33	5%	0.00%
Maryland	4727	66%	2	12.10	38,696	$496	5.88	7%	9.39%
Massachusetts	2454	40%	5	0.23	2,487	$116	6.65	7%	0.53%
Michigan	3178	52%	3	11.87	99,919	$205	9.88	5%	20.22%
Minnesota	3999	59%	4	0.49	30,512	$38	5.38	9%	6.30%
Mississippi	3981	59%	3	6.13	15,789	$651	2.98	5%	10.58%
Missouri	2014	33%	5	16.33	181,007	$139	6.02	7%	42.94%
Montana	4407	62%	3	1.69	16,825	$203	1.01	7%	23.91%
Nebraska	4907	68%	3	3.70	29,827	$267	1.86	10%	16.07%
Nevada	3198	44%	3	0.72	9,744	$115	2.76	6%	5.89%
New	6596	80%	3	0.00	0	$0	1.32	6%	0.00%

New Jersey	5381	71%	3	0.11	4,430	$25	8.86	4%	1.25%
New Mexico	3873	49%	2	0.00	0	$0	2.09	5%	0.00%
New York	1641	28%	5	6.05	83,541	$90	19.57	6%	7.11%
North	7217	85%	1	3.11	26,185	$218	9.75	6%	4.48%
North Dakota	5800	75%	3	0.01	4,229	$5	0.70	11%	5.50%
Ohio	2677	36%	3	8.20	130,898	$106	11.54	6%	18.90%
Oklahoma	4279	63%	3	6.60	104,568	$110	3.81	6%	45.68%
Oregon	1841	33%	6	2.63	13,528	$360	3.90	6%	5.78%
Pennsylvania	1949	35%	6	20.68	133,264	$238	12.76	6%	17.40%
Rhode Island	8824	94%	1	0.00	0	$0	1.05	9%	0.00%
South	3678	57%	3	15.28	105,043	$227	4.72	4%	55.59%
South Dakota	5582	74%	3	0.05	1,370	$68	0.83	11%	1.49%
Tennessee	2793	38%	4	18.45	140,962	$207	6.46	5%	43.67%
Texas	3682	59%	4	134.48	657,993	$356	26.06	5%	50.50%
Utah	2927	41%	4	2.74	47,358	$145	2.86	8%	20.73%
Vermont	8114	90%	2	0.00	0	$0	0.63	6%	0.00%
Virginia	5816	76%	3	5.01	265,149	$32	8.19	6%	53.99%
Washington	3230	40%	3	0.43	4,939	$161	6.90	7%	1.02%
West Virginia	4207	61%	3	2.27	10,305	$383	1.86	4%	13.89%
Wisconsin	1524	25%	5	0.65	19,759	$63	5.73	5%	6.90%
Wyoming	2141	38%	4	0.93	5,201	$356	0.58	5%	18.05%

Sources: * Kaiser Family Foundation: State Health Facts: Individual Insurance Market Competition. KFF has disclosed that this table took from analysis of Public Use File of Submissions of 2012 Medical Loss Ratio Annual Reporting Data (as of August 1, 2013) Available from the Center for Consumer Information & Insurance Oversight (CCIIO).

** The Center for Consumer Information & Insurance Oversight (CCIIO), "The 80/20 Rule: Providing Value and Rebates to Millions of Consumers", June 21, 2012; except other sources mentioned below. Last column is calculated by (# rebate precipitants) / (Privately Insured × Population), in percentage.

*** US Census Bureau, Annual Population Estimates, July 1, 2012.

**** Kaiser Family Foundation, State Health Facts, Health Insurance Coverage of the Total Population.

Table 8- Results of OLS regressions for lapse (0 or 1) for different subsamples based on market concentration, rebate frequency, and rebate amount (SE in parentheses)

	DiD model after 1:1 matching	*DiDiD for Market Concentration*		*DiDiD for Rebate Frequency in market*		*DiDiD with HHI and Reb_Freq*	*DiDiD for Rebate Amount*	
Model #	**Model (2) (from Table 6)**	**HHI<=2500 (5-1)**	**HHI>2500 (5-2)**	**RF >= 25% (6-1)**	**RF < 25% (6-2)**	**(7)**	**RA >= $300 (8-1)**	**RA < $300 (8-2)**
Intercept	0.153 (6.523)	0.039 (0.088)	0.176 (10.208)	0.254*** (0.060)	0.234*** (0.111)	0.145 (5.358)	0.130 (23.531)	0.088*** (0.028)
Gender (= F)	-0.003 (0.003)	-0.003 (0.004)	-0.001 (0.003)	0.005* (0.002)	-0.001 (0.004)	-0.003 (0.003)	0.001 (0.004)	-0.003 (0.004)
# of lives	0.002 (0.002)	-0.005 (0.006)	0.003 (0.002)	0.004 (0.002)	-0.003 (0.003)	0.002 (0.002)	0.005** (0.002)	-0.001 (0.003)
Age	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Duration @ beginning (month)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Rebate Amount ($'000)	0.006 (0.004)	0.257*** (0.036)	0.006 (0.004)	0.006 (0.006)	0.004 (0.007)	0.006 (0.004)	0.003 (0.005)	-0.007 (0.025)
Rebate eligibility (= 1)	0.036*** (0.009)	0.033*** (0.004)	0.043*** (0.011)	0.052** (0.016)	0.025*** (0.004)	0.034*** (0.009)	0.023 (0.015)	0.035*** (0.011)
After period (Jul-Oct = 1)	-0.006 (0.005)	-0.004 (0.007)	0.007 (0.008)	0.014 (0.011)	-0.011*** (0.003)	-0.006 (0.005)	-0.014*** (0.004)	-0.002 (0.006)
DiD (Treated and After Change = 1)	**-0.013** (0.008)	**0.002** (0.007)	**-0.030**** (0.011)	**-0.030** (0.016)	**0.001** (0.005)	**0.002** (0.005)	**-0.021**** (0.010)	**-0.010** (0.007)
Concentrated (=1 if HHI > 2500)						0.011*** (0.001)		
High Reb_Freq (=1 if RF >=25%)						-0.018*** (0.006)		
Conc. × DiD						**-0.014***** (0.004)		
HRF × DiD						**-0.005** (0.007)		
No. of observations	160,996	26,669	134,120	62,164	58,682	160,996	51,796	91,097
R^2	0.012	0.017	0.012	0.010	0.015	0.013	0.015	0.012

Note: a. Models 3 and 7: DiD regression after matching one eligible policy to the nearest control policy (non-eligible) in terms of characteristics, with clustered standard errors within states. Matching is done for before and after periods separately. **Other models:** DiD regression after splitting the main sample into subsamples based on variable of interest, matching, and clustered standard errors within states. Matching is done for before and after periods separately for subsamples.

b. There are other variables in the models including: state dummy variables, channel, product type, policy class, different rating factors, and frequency of the rebate in the state of residence.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.4.3. Effect of rebate frequency

A positive relationship between lapse and rebate frequency in the market was another expected results in section 3.2. Intuition was that once insured gets the rebate while a lot of similar policies did not, next rebate would be more likely from the same insurer than the other alternatives in coming year.

Models (6-1) and (6-2) in Table 8 show that rebate has had marginally significant – with *p-value* of 0.13 – to discourage insureds from lapse in markets with higher than 25% rebate frequency, while its impact was positive but insignificant in markets with lower than 25% rebate frequency; opposite of what expected. My explanation for this unexpected result is that it could be due to the correlation between rebate frequency and market competition. I showed positive correlation between market competition and rebate frequency. So, how is this possible to see lower lapse rates in both more concentrated markets (model (5-2) in Table 8) and markets with more rebates (model (6-1) in Table 8)?

A closer look at Figure 7 reveals that some states are just on the border of market competition but with high lapse frequencies. This includes AZ, SC, TN, and TX, where HHIs are between 2,500 and 3,700 (means concentrated markets but very close to competitive ones) where more than 40% of the insureds received rebate. These outliers which include significant portion of the observations may drive the results in model (6-1) of Table 8 such that more rebates explain fewer lapses significantly.

One possible way to see effect of each variable, keeping the other one constant, is to use both simultaneously in a regression. I avoided this approach at the beginning to avoid losing information inherent in state dummies.

Once two new dummy variables are included in the OLS model, one for states with HHI of greater than 2500 (*Conc* = 1 if HHI > 2500) and one for states with rebate frequency of greater than 25% (*HRF* = 1 if rebate frequency > 25%) and also their interactions with DiD variable, I get the results of model (7) in Table 8. They show that more concentration in the market has significantly negative impact on lapse while rebate frequency's impact is negative but insignificant.

3.4.4. Effect of rebate amount

I expected to see lapse to decrease with bigger rebates. The better way to consider the effect of the rebate amount is to consider the ratio of the rebate to premium which is not available in our data set. Also, other variables like policy class or product type contain information regarding premiums, thus I have had included some components of the premium in the models. Most of models in tables above indicate insignificant effect for rebate amount. Once I divided the sample into two subsamples based on rebate amount, in models (8-1) and (8-2) of Table 8, rebate amount effectively drives the results. Where rebate was greater than $300 –model (8-1), it has significantly reduced the lapse, while its impact is negative but insignificant for rebates below $300.

3.5. Concluding Remarks

The purpose of this study is to figure out the causal effect of the rebates paid by health insurance companies on lapse behavior of the insureds. New regulations under ACA requiring insurers to achieve a minimum MLR or send a rebate to insured may change the retention behavior of the insureds.

Using data from an insurance company for individual medical insurance policies, a DiD identification strategy showed that lapse rate was effectively reduced by the rebates. In order to obtain a valid causal inference from this methodology, I chased all policies issued on or before January 2012 for 4 months. Then, I did the same for policies issued on or before July 2012. Since rebates were determined and distributed in July 2012 and no one was aware of rebate eligibility prior to that date, any differential change in retention rate for after policy change period between eligible and non-eligible policies can be attributed as causal effect of the rebate, after capturing the effects of other covariates like state fixed effects or insured backgrounds.

Matching observations based on their backgrounds, in order to check the robustness of the results, showed that rebate is still positively effective for retention purposes.

In order to examine the impacts of some other covariates whose effects are expected to influence rebate impact, I estimated the causal effect of the rebate on subsamples based on rebate amount, market competitiveness, and rebate frequency in the markets. First of all, rebate amount sounds important for retention; bigger rebates end up in lower lapse rates, as predicted. On the other hand, less competition in the market has reduced the lapse rate significantly.

With respect to the rebate frequency, there was no significant causal effect attributable to that. Main reason could be insufficient time to observe rebate popularity in the market.

Based on the results, I can propose following policy implications:

- Peace of mind in pricing: Assuming that market is not too much sensitive to change in prices and quality of the services –like network, discounts, etc. – are of importance as well, we may choose to overprice the products. Possible rebates in the future will show some kind of reputation.
- State specific pricing: In states with fewer competitors, rebate seems more successful. The bad news, however, is that these states are generally more regulated and pricing is therefore less flexible.

Once rebate policy becomes a usual practice for insurers over time, there would be a need to employ more complex theories to explain insureds' behavior. For example, rebate could be seen as a signal from insurer –sender– towards insured –receiver– in a signaling game, like the role of the guaranties to transfer some information from sender –seller– to receiver –buyer. This is one possible extension of this current study where all or most insurers may choose to send rebates, even though that is not required by law, to show their reputation.

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Appendix A

Regression results for effect of the ACA on childhood immunization coverage in the U.S.

Table A1 – Poisson regression results for Count of Up-To-Date vaccines among 9 recommended vaccines for children aged 19-35 months covered by private insurances

(SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	1.905***	1.904***	1.898***	1.908***	1.930***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
First Born Child (= No)	-0.007*	-0.012**	-0.009	-0.009*	-0.013*
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Child's Age Group (= 19-23 m)	0.003	0.003	0.005	0.004	0.002
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
(= 24-29 m)	-0.002	0	-0.001	-0.001	-0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Breast-fed Child (= Don't Know)	-0.336***	-0.279***	-0.319***	-0.218**	-0.221**
	(0.06)	(0.08)	(0.07)	(0.09)	(0.07)
(= No)	0	0	-0.004	0.001	0.003
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Mother's Educ. (< 12 Years)	-0.018	-0.022	-0.018	-0.028*	-0.012
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
(= 12 Years)	-0.020***	-0.020***	-0.033***	-0.019***	-0.020***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(> 12, No College Grad.)	-0.011***	-0.015***	-0.014**	-0.014***	-0.013***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)
Income-to-Poverty Ratio	0.017***	0.020***	0.022***	0.017***	0.022***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mother's Age (<= 19 Years)	-0.023	-0.058	-0.043	-0.067	-0.048
	(0.02)	(0.04)	(0.04)	(0.05)	(0.04)
(= 20-29 Years)	-0.012***	-0.016***	-0.015***	-0.014***	-0.009*
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Mother's Marital Stat. (= Married)	0.015***	0.011*	0.01	0.014**	0.012*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	0.004	0.008	0.006	0.004	0.003
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
(= Non-His,Black Only)	-0.002	0.01	-0.004	0.019	0.018
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(= Non-His,Other,Mult.)	-0.006	-0.003	-0.004	-0.006	-0.006
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.007***	0.010***	0.005	0.010***	0.008***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treated Dummy	-0.097***	-0.134***	-0.153***	-0.041***	-0.089***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
After Change Dummy	0.058***	0.061***	0.073***	0.060***	0.064***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treated and After Change Dummy	**0.01**	**0.012**	**0.006**	**0.014***	**0.004**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of observations	36,896	20,122	14,610	20,122	9,236
BIC	159510	88197	63813	87596	40603
Goodness of Fit Chi-Sqr	19484	11732	8449	10730	5401

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2 – Logistic regression results for Up-To-Date flag (0 or 1) for *DTaP* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	2.488***	2.411***	2.432***	2.495***	2.025***
	(0.21)	(0.18)	(0.23)	(0.19)	(0.38)
First Born Child (= No)	-0.152***	-0.244***	-0.195***	-0.162***	-0.273***
	(0.04)	(0.05)	(0.06)	(0.06)	(0.04)
Child's Age Group (= 19-23 m)	-0.819***	-0.776***	-0.806***	-0.804***	-0.787***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
(= 24-29 m)	-0.210***	-0.134**	-0.181***	-0.185***	-0.162***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Breast-fed Child (= Don't Know)	-1.353***	-0.571	-0.686	-0.619	-0.428
	(0.36)	(0.68)	(0.68)	(0.83)	(0.68)
(= No)	-0.039	0.043	0.002	0.023	0.024
	(0.06)	(0.08)	(0.09)	(0.08)	(0.09)
Mother's Educ. (< 12 Years)	-0.274**	-0.308**	-0.306**	-0.275**	-0.254*
	(0.11)	(0.14)	(0.16)	(0.13)	(0.14)
(= 12 Years)	-0.241***	-0.262***	-0.294***	-0.219***	-0.259***
	(0.05)	(0.06)	(0.08)	(0.05)	(0.07)
(> 12, No College Grad.)	-0.150***	-0.192***	-0.203***	-0.186***	-0.151**
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Income-to-Poverty Ratio	0.142***	0.196***	0.210***	0.177***	0.189***
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Mother's Age (<= 19 Years)	-0.107	-0.084	0.034	-0.171	-0.206
	(0.21)	(0.30)	(0.33)	(0.32)	(0.26)
(= 20-29 Years)	-0.217***	-0.209***	-0.183**	-0.237***	-0.237***
	(0.04)	(0.05)	(0.07)	(0.05)	(0.05)
Mother's Marital Stat. (= Married)	0.174**	0.190**	0.218**	0.215***	0.181**
	(0.08)	(0.08)	(0.09)	(0.07)	(0.08)
Race/Ethn. of Child (=Hispanic)	-0.001	0.095	0.117	0.025	0.004
	(0.06)	(0.08)	(0.09)	(0.08)	(0.08)
(= Non-His,Black Only)	0.013	0.265	0.202	0.335	0.247
	(0.08)	(0.16)	(0.17)	(0.23)	(0.18)
(= Non-His,Other,Mult.)	-0.06	0.029	0.041	-0.044	-0.063
	(0.07)	(0.08)	(0.09)	(0.08)	(0.08)
Child's Gender (= Female)	0.023	0.054	0.004	0.066*	0.039
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)
Treated Dummy	-1.017***	-1.140***	-1.319***	-0.892***	-0.586***
	(0.04)	(0.05)	(0.05)	(0.06)	(0.08)
After Change Dummy	0.103*	0.142**	0.185**	0.129**	0.227**
	(0.06)	(0.07)	(0.09)	(0.06)	(0.08)
Treated and After Change Dummy	**0.152****	**0.155***	**0.072**	**0.202****	**0.140**
	(0.08)	(0.09)	(0.14)	(0.10)	(0.13)
No. of observations	36,646	20,058	14,563	20,064	9,216
BIC	24391	13930	10331	13411	6723
McFadden_Adj_R2	0.058	0.051	0.053	0.05	0.046
Chi-Sqr	1672.66	909.271	729.106	843.439	443.811
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3 – Logistic regression results for Up-To-Date flag (0 or 1) for *Hep A* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.532***	0.348**	0.352**	0.223	0.180
	(0.19)	(0.14)	(0.17)	(0.14)	(0.19)
First Born Child (= No)	-0.143***	-0.186***	-0.150***	-0.201***	-0.200***
	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)
Child's Age Group (= 19-23 m)	0.563***	0.517***	0.557***	0.554***	0.481***
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
(= 24-29 m)	-0.162***	-0.175***	-0.156***	-0.164***	-0.174***
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
Breast-fed Child (= Don't Know)	-0.993**	-1.622**	-2.037***	-0.925	-0.955*
	(0.40)	(0.67)	(0.77)	(0.62)	(0.56)
(= No)	0.022	0.04	0.009	0.039	0.006
	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)
Mother's Educ. (< 12 Years)	0.088	0.066	0.027	0.072	0.126
	(0.10)	(0.11)	(0.14)	(0.11)	(0.12)
(= 12 Years)	-0.191***	-0.180***	-0.322***	-0.152**	-0.102*
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
(> 12, No College Grad.)	-0.144***	-0.174***	-0.201***	-0.196***	-0.123***
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)
Income-to-Poverty Ratio	0.060**	0.052	0.051	0.073*	0.074**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Mother's Age (<= 19 Years)	-0.115	-0.362	-0.397	-0.446*	-0.282
	(0.24)	(0.25)	(0.26)	(0.23)	(0.23)
(= 20-29 Years)	-0.094**	-0.107**	-0.082	-0.095**	-0.094**
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Mother's Marital Stat. (= Married)	0.157***	0.168***	0.110*	0.184***	0.194***
	(0.05)	(0.06)	(0.07)	(0.06)	(0.06)
Race/Ethn. of Child (=Hispanic)	0.06	0.114*	0.05	0.095	0.024
	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)
(= Non-His,Black Only)	0.038	0.111	0.082	0.126	0.120
	(0.06)	(0.11)	(0.11)	(0.14)	(0.13)
(= Non-His,Other,Mult.)	0.138***	0.168***	0.126**	0.139***	0.158***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Child's Gender (= Female)	0.043*	0.056*	0.045	0.025	0.032
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Treated Dummy	-0.840***	-1.217***	-1.401***	0.106**	-1.014***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)
After Change Dummy	0.324***	0.374***	0.560***	0.355***	0.458***
	(0.06)	(0.06)	(0.08)	(0.06)	(0.10)
Treated and After Change Dummy	**0.084**	**0.087**	**0.058**	**0.109**	**-0.035**
	(0.07)	(0.08)	(0.11)	(0.08)	(0.12)
No. of observations	36,648	20,058	14,563	20,064	9,216
BIC	45478	25126	18457	24847	11989
McFadden_Adj_R2	0.068	0.059	0.061	0.058	0.049
Chi-Sqr	3489.039	1728.534	1343.67	1650.121	735.423
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4 – Logistic regression results for Up-To-Date flag (0 or 1) for *Hep B* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	3.861***	3.636***	3.728***	3.824***	2.839***
	(0.41)	(0.39)	(0.43)	(0.45)	(0.58)
First Born Child (= No)	0.117	0.158*	0.144	0.165*	0.180*
	(0.08)	(0.09)	(0.10)	(0.09)	(0.10)
Child's Age Group (= 19-23 m)	-0.325***	-0.298***	-0.380***	-0.263***	-0.303***
	(0.06)	(0.08)	(0.10)	(0.07)	(0.08)
(= 24-29 m)	-0.090*	-0.039	-0.074	-0.053	-0.037
	(0.05)	(0.08)	(0.09)	(0.07)	(0.08)
Breast-fed Child (= Don't Know)	-1.577***	-0.811	-0.913	-0.22	-0.769
	(0.42)	(0.78)	(0.79)	(1.12)	(0.80)
(= No)	0.124	0.116	0.15	0.152	0.072
	(0.08)	(0.10)	(0.11)	(0.10)	(0.12)
Mother's Educ. (< 12 Years)	-0.052	0.007	0.078	-0.163	-0.029
	(0.18)	(0.18)	(0.27)	(0.17)	(0.20)
(= 12 Years)	0.08	-0.012	-0.049	-0.005	-0.035
	(0.07)	(0.07)	(0.11)	(0.09)	(0.08)
(> 12, No College Grad.)	0.06	-0.012	-0.003	0.048	0.000
	(0.06)	(0.08)	(0.09)	(0.09)	(0.07)
Income-to-Poverty Ratio	0.146***	0.225***	0.234***	0.152***	0.257***
	(0.03)	(0.05)	(0.06)	(0.05)	(0.05)
Mother's Age (<= 19 Years)	0.109	-0.131	0.078	0.015	0.520
	(0.30)	(0.49)	(0.57)	(0.56)	(0.52)
(= 20-29 Years)	0.073	0.085	0.029	0.006	0.206*
	(0.07)	(0.09)	(0.10)	(0.08)	(0.11)
Mother's Marital Stat. (= Married)	0.073	-0.006	-0.035	0.006	0.026
	(0.08)	(0.10)	(0.14)	(0.10)	(0.12)
Race/Ethn. of Child (=Hispanic)	0.223***	0.226***	0.225**	0.142**	0.207**
	(0.07)	(0.07)	(0.10)	(0.06)	(0.09)
(= Non-His,Black Only)	0.243**	0.135	-0.037	0.203	0.222*
	(0.12)	(0.13)	(0.18)	(0.13)	(0.13)
(= Non-His,Other,Mult.)	0.133*	0.118	0.076	0.055	0.037
	(0.08)	(0.09)	(0.11)	(0.08)	(0.08)
Child's Gender (= Female)	0.05	0.062	0.03	0.094*	0.124**
	(0.05)	(0.06)	(0.07)	(0.06)	(0.05)
Treated Dummy	-0.834***	-0.960***	-1.173***	-0.853***	-0.436***
	(0.07)	(0.07)	(0.08)	(0.08)	(0.11)
After Change Dummy	-0.328***	-0.309***	-0.168***	-0.294***	-0.159
	(0.09)	(0.09)	(0.11)	(0.09)	(0.13)
Treated and After Change Dummy	**-0.003**	**0.039**	**0.052**	**-0.001**	**-0.111**
	(0.10)	(0.11)	(0.12)	(0.12)	(0.16)
No. of observations	36,646	20,058	14,563	20,064	9,216
BIC	17600	10136	6902	9878	4645
McFadden_Adj_R2	0.053	0.048	0.046	0.042	0.038
Chi-Sqr	1138.883	669.341	484.737	564.292	304.117
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5 – Logistic regression results for Up-To-Date flag (0 or 1) for *Hib* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	1.625***	1.635***	1.570***	1.759***	2.778***
	(0.43)	(0.35)	(0.36)	(0.35)	(0.47)
First Born Child (= No)	-0.066	-0.034	-0.028	0.021	0.024
	(0.05)	(0.08)	(0.08)	(0.07)	(0.08)
Child's Age Group (= 19-23 m)	-0.590***	-0.517***	-0.636***	-0.568***	-0.511***
	(0.05)	(0.06)	(0.07)	(0.07)	(0.06)
(= 24-29 m)	-0.283***	-0.198***	-0.276***	-0.230***	-0.235***
	(0.04)	(0.06)	(0.07)	(0.07)	(0.06)
Breast-fed Child (= Don't Know)	-2.855***	-2.675***	-2.914***	-2.759***	-2.654***
	(0.36)	(0.46)	(0.48)	(0.50)	(0.45)
(= No)	-0.101	-0.140*	-0.176*	-0.121	-0.098
	(0.07)	(0.08)	(0.09)	(0.09)	(0.09)
Mother's Educ. (< 12 Years)	-0.016	-0.042	0.118	0.011	0.139
	(0.15)	(0.19)	(0.22)	(0.21)	(0.23)
(= 12 Years)	0.045	0.132	0.108	0.129	0.124
	(0.08)	(0.11)	(0.12)	(0.12)	(0.12)
(> 12, No College Grad.)	0.038	0.036	0.052	-0.001	0.040
	(0.05)	(0.08)	(0.08)	(0.08)	(0.08)
Income-to-Poverty Ratio	0.095***	0.107**	0.132**	0.108**	0.166**
	(0.04)	(0.05)	(0.06)	(0.05)	(0.05)
Mother's Age (<= 19 Years)	-0.313	-0.656*	-0.531	-0.467	-0.475
	(0.31)	(0.35)	(0.37)	(0.41)	(0.42)
(= 20-29 Years)	-0.162***	-0.205***	-0.185**	-0.121	-0.109
	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)
Mother's Marital Stat. (= Married)	0.097	0.02	-0.004	0.038	0.075
	(0.08)	(0.09)	(0.10)	(0.10)	(0.10)
Race/Ethn. of Child (=Hispanic)	0.055	0.092	0.131	0.11	0.068
	(0.07)	(0.08)	(0.09)	(0.09)	(0.10)
(= Non-His,Black Only)	-0.059	0.098	-0.113	0.338*	0.203
	(0.10)	(0.12)	(0.14)	(0.19)	(0.15)
(= Non-His,Other,Mult.)	-0.139*	-0.096	-0.058	-0.13	-0.144
	(0.08)	(0.09)	(0.10)	(0.10)	(0.10)
Child's Gender (= Female)	0.031	-0.008	-0.085	-0.001	0.006
	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)
Treated Dummy	-0.218***	-0.326***	-0.345***	0.377***	-0.285***
	(0.07)	(0.08)	(0.07)	(0.08)	(0.12)
After Change Dummy	1.766***	1.729***	1.715***	1.695***	1.617***
	(0.23)	(0.24)	(0.23)	(0.24)	(0.32)
Treated and After Change Dummy	**0.006**	**-0.229**	**-0.462**	**0.081**	**-0.201**
	(0.26)	(0.26)	(0.29)	(0.27)	(0.31)
No. of observations	36,646	20,058	14,563	20,064	9,216
BIC	17957	10053	8360	9918	4786
McFadden_Adj_R2	0.111	0.091	0.086	0.096	0.089
Chi-Sqr	2391.888	1153.252	933.181	1185.127	576.619
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6 – Logistic regression results for Up-To-Date flag (0 or 1) for *MCV* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	2.732***	2.594***	2.838***	2.811***	1.561***
	(0.33)	(0.25)	(0.35)	(0.27)	(0.46)
First Born Child (= No)	-0.068	-0.115	-0.038	-0.05	-0.160*
	(0.07)	(0.09)	(0.10)	(0.08)	(0.09)
Child's Age Group (= 19-23 m)	-0.604***	-0.544***	-0.539***	-0.567***	-0.604***
	(0.05)	(0.07)	(0.08)	(0.08)	(0.08)
(= 24-29 m)	-0.127**	-0.121	-0.093	-0.098	-0.118
	(0.05)	(0.08)	(0.09)	(0.08)	(0.08)
Breast-fed Child (= Don't Know)	-0.582	-0.092	-0.165	-0.151	---
	(0.55)	(1.11)	(1.09)	(1.10)	---
(= No)	0.223***	0.172*	0.155	0.192**	0.192**
	(0.08)	(0.10)	(0.11)	(0.10)	(0.10)
Mother's Educ. (< 12 Years)	-0.126	-0.327*	-0.365*	-0.505***	0.298
	(0.16)	(0.18)	(0.21)	(0.15)	(0.20)
(= 12 Years)	-0.203***	-0.183*	-0.310**	-0.225**	-0.110
	(0.08)	(0.10)	(0.13)	(0.11)	(0.11)
(> 12, No College Grad.)	-0.116*	-0.148*	-0.117	-0.174**	-0.126*
	(0.06)	(0.08)	(0.09)	(0.09)	(0.07)
Income-to-Poverty Ratio	0.119***	0.114**	0.177***	0.062	0.143***
	(0.04)	(0.05)	(0.07)	(0.05)	(0.05)
Mother's Age (<= 19 Years)	-0.467*	-0.38	-0.078	-0.330	-0.191
	(0.24)	(0.40)	(0.49)	(0.42)	(0.36)
(= 20-29 Years)	-0.001	-0.122*	-0.065	-0.125*	-0.048
	(0.06)	(0.07)	(0.08)	(0.07)	(0.08)
Mother's Marital Stat. (= Married)	0.04	-0.025	-0.124	-0.002	-0.035
	(0.09)	(0.11)	(0.11)	(0.11)	(0.12)
Race/Ethn. of Child (=Hispanic)	0.036	0.075	0.057	-0.001	-0.030
	(0.09)	(0.10)	(0.12)	(0.10)	(0.09)
(= Non-His,Black Only)	0.194	0.141	-0.094	0.313	0.251
	(0.14)	(0.21)	(0.24)	(0.27)	(0.25)
(= Non-His,Other,Mult.)	0.075	0.117	0.066	0.061	0.063
	(0.09)	(0.11)	(0.12)	(0.11)	(0.13)
Child's Gender (= Female)	0.224***	0.288***	0.250***	0.328***	0.284***
	(0.05)	(0.06)	(0.08)	(0.07)	(0.06)
Treated Dummy	-0.941***	-1.143***	-1.366***	-0.867***	0.099
	(0.06)	(0.07)	(0.06)	(0.09)	(0.13)
After Change Dummy	0.001	0.017	-0.095	-0.008	-0.143
	(0.11)	(0.11)	(0.14)	(0.11)	(0.15)
Treated and After Change Dummy	**0.109**	**0.103**	**0.003**	**0.170**	**0.113**
	(0.12)	(0.14)	(0.18)	(0.14)	(0.21)
No. of observations	36,646	20,058	14,563	20,064	9,198
BIC	16292	9370	6714	8972	4556
McFadden_Adj_R2	0.047	0.042	0.043	0.036	0.038
Chi-Sqr	969.62	571.614	460.147	473.547	298.581
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7 – Logistic regression results for Up-To-Date flag (0 or 1) for *PCV* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	1.825***	1.778***	1.801***	1.927***	0.814***
	(0.23)	(0.20)	(0.25)	(0.23)	(0.29)
First Born Child (= No)	-0.006	-0.029	0.014	-0.050	-0.077
	(0.05)	(0.06)	(0.07)	(0.06)	(0.06)
Child's Age Group (= 19-23 m)	-0.313***	-0.306***	-0.309***	-0.320***	-0.321***
	(0.04)	(0.05)	(0.07)	(0.05)	(0.05)
(= 24-29 m)	-0.071*	-0.051	-0.054	-0.061	-0.063
	(0.04)	(0.05)	(0.06)	(0.05)	(0.05)
Breast-fed Child (= Don't Know)	-2.731***	-3.066***	-3.413***	-2.389***	-2.406***
	(0.34)	(0.67)	(0.64)	(0.63)	(0.53)
(= No)	-0.132***	-0.158***	-0.205***	-0.180***	-0.192***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Mother's Educ. (< 12 Years)	-0.292***	-0.275**	-0.288	-0.247*	-0.206
	(0.10)	(0.13)	(0.19)	(0.13)	(0.13)
(= 12 Years)	-0.296***	-0.265***	-0.304***	-0.243***	-0.347***
	(0.05)	(0.07)	(0.08)	(0.08)	(0.08)
(> 12, No College Grad.)	-0.172***	-0.164***	-0.111**	-0.144***	-0.143***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Income-to-Poverty Ratio	0.144***	0.180***	0.173***	0.164***	0.191***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
Mother's Age (<= 19 Years)	0.099	-0.368	-0.291	-0.376	-0.442
	(0.22)	(0.31)	(0.33)	(0.34)	(0.28)
(= 20-29 Years)	-0.117***	-0.142***	-0.148**	-0.114**	-0.080
	(0.04)	(0.05)	(0.06)	(0.05)	(0.05)
Mother's Marital Stat. (= Married)	0.075	0.047	0.041	0.089	0.053
	(0.06)	(0.07)	(0.09)	(0.09)	(0.08)
Race/Ethn. of Child (=Hispanic)	-0.172***	-0.131*	-0.171*	-0.175**	-0.180**
	(0.06)	(0.08)	(0.09)	(0.08)	(0.07)
(= Non-His,Black Only)	-0.277***	-0.11	-0.161	-0.135	-0.109
	(0.06)	(0.14)	(0.13)	(0.16)	(0.15)
(= Non-His,Other,Mult.)	-0.382***	-0.330***	-0.323***	-0.356***	-0.393***
	(0.05)	(0.07)	(0.08)	(0.08)	(0.06)
Child's Gender (= Female)	0.061**	0.074*	0.014	0.079*	0.029
	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)
Treated Dummy	-1.174***	-1.169***	-1.228***	-0.983***	-0.066
	(0.04)	(0.04)	(0.05)	(0.06)	(0.08)
After Change Dummy	0.339***	0.378***	0.273***	0.375***	0.323***
	(0.05)	(0.06)	(0.08)	(0.06)	(0.07)
Treated and After Change Dummy	**0.045**	**0.038**	**0.011**	**0.019**	**-0.135**
	(0.06)	(0.08)	(0.12)	(0.09)	(0.11)
No. of observations	36,646	20,058	14,563	20,064	9,216
BIC	26417	15215	11671	14643	7386
McFadden_Adj_R2	0.057	0.046	0.043	0.045	0.041
Chi-Sqr	1744.51	897.571	681.716	829.225	435.745
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8 – Logistic regression results for Up-To-Date flag (0 or 1) for *Polio* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	3.476***	3.244***	2.874***	3.381***	2.613***
	(0.45)	(0.35)	(0.36)	(0.40)	(0.56)
First Born Child (= No)	0.077	0.056	0.031	0.023	-0.016
	(0.08)	(0.10)	(0.11)	(0.09)	(0.09)
Child's Age Group (= 19-23 m)	-0.457***	-0.404***	-0.509***	-0.426***	-0.467***
	(0.06)	(0.08)	(0.10)	(0.08)	(0.07)
(= 24-29 m)	-0.087	-0.046	-0.146	-0.094	-0.137*
	(0.06)	(0.09)	(0.10)	(0.09)	(0.08)
Breast-fed Child (= Don't Know)	-1.237**	-0.948	-1.042	-0.272	-0.909
	(0.49)	(0.81)	(0.84)	(1.14)	(0.83)
(= No)	0.076	0.104	0.072	0.188	0.106
	(0.08)	(0.12)	(0.12)	(0.13)	(0.14)
Mother's Educ. (< 12 Years)	-0.237	-0.152	-0.11	-0.174	-0.144
	(0.16)	(0.18)	(0.22)	(0.18)	(0.19)
(= 12 Years)	-0.096	-0.136	-0.319***	-0.088	-0.153
	(0.07)	(0.10)	(0.11)	(0.10)	(0.10)
(> 12, No College Grad.)	-0.02	-0.063	-0.092	-0.033	-0.077
	(0.07)	(0.10)	(0.11)	(0.11)	(0.09)
Income-to-Poverty Ratio	0.136***	0.172***	0.212***	0.134**	0.216**
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Mother's Age (<= 19 Years)	-0.275	-0.478	-0.205	-0.734	-0.156
	(0.30)	(0.43)	(0.51)	(0.45)	(0.52)
(= 20-29 Years)	-0.191***	-0.191***	-0.172**	-0.205***	-0.219***
	(0.06)	(0.07)	(0.08)	(0.07)	(0.06)
Mother's Marital Stat. (= Married)	0.167*	0.116	0.131	0.145	0.170
	(0.10)	(0.12)	(0.13)	(0.12)	(0.13)
Race/Ethn. of Child (=Hispanic)	0.053	0.056	0.095	-0.008	0.053
	(0.08)	(0.11)	(0.12)	(0.09)	(0.10)
(= Non-His,Black Only)	0.263***	0.394***	0.127	0.556**	0.610**
	(0.10)	(0.13)	(0.15)	(0.22)	(0.17)
(= Non-His,Other,Mult.)	-0.014	-0.003	0.033	-0.088	-0.051
	(0.10)	(0.11)	(0.13)	(0.11)	(0.12)
Child's Gender (= Female)	0.123***	0.142**	0.051	0.192***	0.154***
	(0.05)	(0.06)	(0.07)	(0.07)	(0.06)
Treated Dummy	-1.075***	-1.478***	-1.457***	-0.790***	-0.837***
	(0.06)	(0.08)	(0.08)	(0.08)	(0.12)
After Change Dummy	0.209**	0.236***	0.321***	0.192**	0.279*
	(0.09)	(0.09)	(0.12)	(0.09)	(0.15)
Treated and After Change Dummy	**-0.002**	**0.088**	**-0.133**	**0.139**	**0.042**
	(0.10)	(0.13)	(0.16)	(0.14)	(0.20)
No. of observations	36,646	20,058	14,563	20,064	9,216
BIC	13349	8033	6018	7597	3758
McFadden_Adj_R2	0.058	0.048	0.051	0.043	0.047
Chi-Sqr	984.125	562.073	476.177	477.041	301.574
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9 – Logistic regression results for Up-To-Date flag (0 or 1) for *ROT* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	-1.236***	-1.220***	-1.307***	-1.417***	-1.413***
	(0.22)	(0.19)	(0.19)	(0.20)	(0.30)
First Born Child (= No)	-0.069*	-0.075	-0.049	-0.050	-0.075
	(0.04)	(0.05)	(0.06)	(0.04)	(0.05)
Child's Age Group (= 19-23 m)	1.026***	0.990***	1.235***	1.011***	0.995***
	(0.04)	(0.05)	(0.06)	(0.05)	(0.06)
(= 24-29 m)	0.450***	0.434***	0.517***	0.424***	0.453***
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)
Breast-fed Child (= Don't Know)	-4.187***	-2.511**		-2.382**	-2.633**
	(1.07)	(1.03)		(1.04)	(1.03)
(= No)	-0.110***	-0.111***	-0.102**	-0.122***	-0.150***
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)
Mother's Educ. (< 12 Years)	-0.290***	-0.238*	-0.251*	-0.273*	-0.113
	(0.11)	(0.13)	(0.14)	(0.14)	(0.11)
(= 12 Years)	-0.201***	-0.228***	-0.385***	-0.256***	-0.251***
	(0.05)	(0.07)	(0.08)	(0.08)	(0.08)
(> 12, No College Grad.)	-0.103***	-0.138***	-0.129**	-0.103**	-0.192***
	(0.04)	(0.05)	(0.06)	(0.05)	(0.05)
Income-to-Poverty Ratio	0.148***	0.178***	0.173***	0.191***	0.185***
	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
Mother's Age (<= 19 Years)	-0.038	-0.153	-0.179	-0.303	-0.085
	(0.28)	(0.47)	(0.51)	(0.50)	(0.43)
(= 20-29 Years)	-0.126***	-0.138***	-0.091*	-0.133***	-0.067
	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)
Mother's Marital Stat. (= Married)	0.166***	0.116*	0.167**	0.137**	0.089
	(0.05)	(0.06)	(0.07)	(0.07)	(0.06)
Race/Ethn. of Child (=Hispanic)	-0.024	0.002	-0.005	-0.001	0.036
	(0.05)	(0.04)	(0.06)	(0.05)	(0.04)
(= Non-His,Black Only)	-0.193***	-0.004	-0.163	0.111	0.048
	(0.07)	(0.11)	(0.12)	(0.11)	(0.10)
(= Non-His,Other,Mult.)	-0.200***	-0.192***	-0.183***	-0.178***	-0.179***
	(0.06)	(0.06)	(0.07)	(0.06)	(0.07)
Child's Gender (= Female)	0.019	0.048	0.011	0.045	0.021
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
Treated Dummy	-0.533***	-0.641***	-0.796***	-0.189***	-0.401***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.10)
After Change Dummy	1.066***	1.066***	1.146***	1.074***	1.012***
	(0.10)	(0.09)	(0.11)	(0.09)	(0.11)
Treated and After Change Dummy	**0.137**	**0.109**	**0.126**	**0.103**	**0.059**
	(0.12)	(0.12)	(0.15)	(0.12)	(0.17)
No. of observations	36,646	20,058	14,546	20,064	9,216
BIC	37872	21045	14917	21067	10114
McFadden_Adj_R2	0.257	0.247	0.26	0.244	0.219
Chi-Sqr	13215.272	7000.67	5356.299	6898.574	2921.829
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10 – Logistic regression results for Up-To-Date flag (0 or 1) for *VRC* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	1.852***	2.495***	2.380***	2.534***	1.553***
	(0.28)	(0.24)	(0.28)	(0.27)	(0.35)
First Born Child (= No)	-0.076	-0.151*	-0.16	-0.091	-0.174*
	(0.06)	(0.09)	(0.11)	(0.09)	(0.09)
Child's Age Group (= 19-23 m)	-0.386***	-0.383***	-0.428***	-0.444***	-0.403***
	(0.05)	(0.07)	(0.06)	(0.07)	(0.07)
(= 24-29 m)	-0.058	0.009	-0.015	-0.061	0.008
	(0.05)	(0.07)	(0.08)	(0.08)	(0.07)
Breast-fed Child (= Don't Know)	-0.485	---	---	---	---
	(0.39)	---	---	---	---
(= No)	0.209***	0.173**	0.117	0.243***	0.193**
	(0.06)	(0.07)	(0.08)	(0.07)	(0.08)
Mother's Educ. (< 12 Years)	0.052	-0.047	-0.072	-0.251	-0.105
	(0.17)	(0.21)	(0.24)	(0.19)	(0.23)
(= 12 Years)	-0.068	-0.031	-0.068	-0.068	-0.080
	(0.07)	(0.09)	(0.10)	(0.09)	(0.11)
(> 12, No College Grad.)	-0.032	-0.048	-0.004	-0.097	-0.102
	(0.05)	(0.06)	(0.07)	(0.07)	(0.06)
Income-to-Poverty Ratio	0.155***	0.141***	0.138**	0.086	0.123**
	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)
Mother's Age (<= 19 Years)	-0.467**	-0.698*	-0.571	-0.764*	-0.512
	(0.20)	(0.38)	(0.44)	(0.40)	(0.35)
(= 20-29 Years)	-0.065	-0.169***	-0.221***	-0.129*	-0.106
	(0.05)	(0.06)	(0.08)	(0.07)	(0.07)
Mother's Marital Stat. (= Married)	-0.043	-0.114	-0.151	-0.057	-0.098
	(0.08)	(0.10)	(0.11)	(0.10)	(0.12)
Race/Ethn. of Child (=Hispanic)	0.161**	0.157**	0.13	0.104	0.120*
	(0.07)	(0.07)	(0.08)	(0.08)	(0.07)
(= Non-His,Black Only)	0.088	-0.034	-0.172	0.133	0.110
	(0.10)	(0.17)	(0.17)	(0.23)	(0.20)
(= Non-His,Other,Mult.)	0.05	0.072	0.017	0.025	0.074
	(0.07)	(0.08)	(0.09)	(0.08)	(0.10)
Child's Gender (= Female)	0.092**	0.095*	0.065	0.148***	0.134***
	(0.04)	(0.05)	(0.06)	(0.06)	(0.04)
Treated Dummy	-0.833***	-1.189***	-1.179***	-0.549***	-0.709***
	(0.05)	(0.05)	(0.06)	(0.06)	(0.09)
After Change Dummy	-0.035	0.019	0.192	0.006	-0.071
	(0.08)	(0.08)	(0.14)	(0.08)	(0.10)
Treated and After Change Dummy	**0.199****	**0.257****	**0.235**	**0.262****	**0.468*****
	(0.09)	(0.10)	(0.15)	(0.11)	(0.14)
No. of observations	36,646	20,040	14,546	20,047	9,198
BIC	20888	11388	8301	10887	5490
McFadden_Adj_R2	0.039	0.033	0.033	0.027	0.034
Chi-Sqr	1022.387	547.882	439.746	440.65	309.159
Chi-Sqr's p-value	0.00	0.00	0.00	0.00	0.00

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A1′ – OLS regression results for Count of Up-To-Date vaccines among 9 recommended vaccines for children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	6.987***	-0.443***	-0.390***	7.054***	-0.764***
	(0.14)	(0.11)	(0.12)	(0.10)	(0.17)
First Born Child (= No)	-0.056*	-0.087**	-0.067	-0.068*	-0.108*
	(0.03)	(0.04)	(0.05)	(0.04)	(0.06)
Child's Age Group (= 19-23 m)	0.025	0.02	0.032	0.029	0.001
	(0.02)	(0.03)	(0.04)	(0.03)	(0.05)
(= 24-29 m)	-0.018	-0.001	-0.007	-0.01	-0.001
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
Breast-fed Child (= Don't Know)	-2.095***	-1.768***	-1.979***	-1.414**	-1.690***
	(0.36)	(0.48)	(0.42)	(0.53)	(0.54)
(= No)	-0.002	0.001	-0.029	0.004	-0.029
	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)
Mother's Educ. (< 12 Years)	-0.127	-0.158	-0.131	-0.198*	-0.457**
	(0.10)	(0.10)	(0.13)	(0.10)	(0.21)
(= 12 Years)	-0.148***	-0.148***	-0.237***	-0.143***	-0.102
	(0.04)	(0.04)	(0.05)	(0.04)	(0.07)
(> 12, No College Grad.)	-0.084***	-0.110***	-0.102**	-0.106**	-0.095**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Income-to-Poverty Ratio	0.123***	0.145***	0.157***	0.129***	0.159***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
Mother's Age (<= 19 Years)	-0.16	-0.398	-0.289	-0.461	-0.281
	(0.16)	(0.24)	(0.24)	(0.30)	(0.45)
(= 20-29 Years)	-0.093***	-0.120***	-0.110***	-0.108***	-0.099**
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)
Mother's Marital Stat. (= Married)	0.110***	0.078*	0.068	0.101**	0.04
	(0.04)	(0.04)	(0.05)	(0.04)	(0.07)
Race/Ethn. of Child (=Hispanic)	0.026	0.063	0.046	0.028	0.069
	(0.03)	(0.04)	(0.05)	(0.04)	(0.09)
(= Non-His,Black Only)	-0.015	0.079	-0.03	0.144	0.097
	(0.05)	(0.08)	(0.10)	(0.10)	(0.17)
(= Non-His,Other,Mult.)	-0.047	-0.022	-0.032	-0.051	0.001
	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)
Child's Gender (= Female)	0.052***	0.071***	0.034	0.074***	0.051
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Treated Dummy	-0.722***	-0.962***	-1.094***	-0.300***	-0.613***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)
After Change Dummy	0.446***	0.470***	0.560***	0.677***	0.660***
	(0.04)	(0.04)	(0.06)	(0.05)	(0.05)
Treated and After Change Dummy	**0.074**	**0.079**	**0.032**	**0.107***	**0.027**
	(0.05)	(0.06)	(0.08)	(0.06)	(0.07)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.21	0.15	0.15	0.14	0.13

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2′ – OLS regression results for Up-To-Date flag (0 or 1) for *DTaP* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.864***	0.013	-0.005	0.928***	0.013
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
First Born Child (= No)	-0.012***	-0.023***	-0.020***	-0.015***	-0.034***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Age Group (= 19-23 m)	-0.082***	-0.082***	-0.086***	-0.080***	-0.079***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(= 24-29 m)	-0.017***	-0.011**	-0.016***	-0.015***	-0.009
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Breast-fed Child (= Don't Know)	-0.188***	-0.058	-0.072	-0.064	-0.107
	(0.07)	(0.09)	(0.10)	(0.12)	(0.12)
(= No)	-0.003	0.005	0.001	0.003	0.008
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Educ. (< 12 Years)	-0.034**	-0.042**	-0.040*	-0.037*	-0.106**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
(= 12 Years)	-0.026***	-0.028***	-0.032***	-0.023***	-0.029***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(> 12, No College Grad.)	-0.014***	-0.019***	-0.020***	-0.018***	-0.020**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.018***	0.024***	0.027***	0.021***	0.022***
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Mother's Age (<= 19 Years)	-0.015	-0.013	0.004	-0.026	-0.014
	(0.03)	(0.04)	(0.04)	(0.04)	(0.07)
(= 20-29 Years)	-0.021***	-0.021***	-0.019**	-0.022***	-0.022***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Marital Stat. (= Married)	0.020**	0.022**	0.026**	0.024**	0.009
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	0.001	0.01	0.012	0.003	0.012
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
(= Non-His,Black Only)	0.001	0.028	0.022	0.032	0.009
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
(= Non-His,Other,Mult.)	-0.005	0.003	0.005	-0.004	0.007
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.002	0.005	-0.001	0.007*	0.003
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Treated Dummy	-0.097***	-0.120***	-0.142***	-0.322***	-0.078***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
After Change Dummy	0.010*	-0.004	0.020**	-0.007	-0.001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treated and After Change Dummy	**0.013***	**0.016***	**0.008**	**0.019****	**0.014**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.1	0.07	0.07	0.07	0.07

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3′ – OLS regression results for Up-To-Date flag (0 or 1) for *Hep A* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.625***	-0.094**	-0.031	0.504***	-0.259***
	(0.04)	(0.04)	(0.05)	(0.04)	(0.06)
First Born Child (= No)	-0.031***	-0.040***	-0.033***	-0.043***	-0.039**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Age Group (= 19-23 m)	0.115***	0.108***	0.118***	0.113***	0.108***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(= 24-29 m)	-0.036***	-0.039***	-0.036***	-0.037***	-0.026**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Breast-fed Child (= Don't Know)	-0.215**	-0.333***	-0.393***	-0.202	-0.226*
	(0.08)	(0.12)	(0.11)	(0.13)	(0.12)
(= No)	0.004	0.008	0.0001	0.008	0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Educ. (< 12 Years)	0.018	0.015	0.007	0.016	0.059
	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)
(= 12 Years)	-0.041***	-0.039***	-0.071***	-0.032**	-0.014
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
(> 12, No College Grad.)	-0.031***	-0.038***	-0.044***	-0.042***	-0.017
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.013*	0.011	0.011	0.016*	0.018*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Age (<= 19 Years)	-0.027	-0.078	-0.086	-0.099*	-0.032
	(0.05)	(0.06)	(0.06)	(0.05)	(0.07)
(= 20-29 Years)	-0.020**	-0.023**	-0.018	-0.020**	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Marital Stat. (= Married)	0.034***	0.036***	0.024	0.039***	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Race/Ethn. of Child (=Hispanic)	0.012	0.023*	0.01	0.02	0.002
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
(= Non-His,Black Only)	0.008	0.026	0.02	0.029	0.059
	(0.01)	(0.02)	(0.02)	(0.03)	(0.04)
(= Non-His,Other,Mult.)	0.029***	0.034***	0.025**	0.028***	0.039**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Child's Gender (= Female)	0.009*	0.013**	0.01	0.005	0.013*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Treated Dummy	-0.193***	-0.280***	-0.320***	-0.038***	-0.053***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
After Change Dummy	0.069***	0.115***	0.118***	0.116***	0.135***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
Treated and After Change Dummy	**0.018**	**0.019**	**0.013**	**0.022**	**-0.011**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
No. of observations	36,896	20,122	14,610	20,122	9,236
$\mathbf{R^2}$	0.1	0.09	0.09	0.08	0.08

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4′ – OLS regression results for Up-To-Date flag (0 or 1) for *Hep B* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.925***	-0.001	-0.027	0.992***	-0.001
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
First Born Child (= No)	0.006	0.010*	0.008	0.010*	0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Age Group (= 19-23 m)	-0.021***	-0.019***	-0.023***	-0.017***	-0.026***
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
(= 24-29 m)	-0.005*	-0.002	-0.004	-0.003	-0.010*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Breast-fed Child (= Don't Know)	-0.170**	-0.054	-0.063	-0.008	-0.064
	(0.07)	(0.08)	(0.08)	(0.06)	(0.08)
(= No)	0.007	0.007	0.008	0.008	0.004
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Educ. (< 12 Years)	-0.004	0.001	0.009	-0.012	-0.039
	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)
(= 12 Years)	0.005	0.001	-0.002	0.001	-0.003
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
(> 12, No College Grad.)	0.004	-0.001	-0.001	0.004	-0.001
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.010***	0.017***	0.016***	0.011***	0.017***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Mother's Age (<= 19 Years)	0.004	-0.016	-0.001	-0.009	0.008
	(0.02)	(0.04)	(0.03)	(0.04)	(0.04)
(= 20-29 Years)	0.005	0.006	0.002	0.001	0.008
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Mother's Marital Stat. (= Married)	0.004	-0.001	-0.003	-0.001	-0.002
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	0.014***	0.015***	0.014**	0.010**	0.027***
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
(= Non-His,Black Only)	0.014**	0.007	-0.005	0.012	0.014
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(= Non-His,Other,Mult.)	0.008	0.008	0.004	0.004	0.008
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.003	0.005	0.003	0.006	0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Treated Dummy	-0.045***	-0.052***	-0.057***	-0.128***	-0.041***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
After Change Dummy	-0.021***	-0.048***	-0.011*	-0.047***	-0.040***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Treated and After Change Dummy	**0.001**	**-0.002**	**-0.001**	**-0.001**	**-0.008**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.11	0.07	0.08	0.06	0.06

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5′ – OLS regression results for Up-To-Date flag (0 or 1) for *Hib* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.805***	-0.092***	-0.088***	0.952***	0.014
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)
First Born Child (= No)	-0.002	-0.001	-0.001	0.002	-0.003
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Age Group (= 19-23 m)	-0.039***	-0.035***	-0.050***	-0.038***	-0.032***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(= 24-29 m)	-0.017***	-0.011***	-0.019***	-0.013***	-0.011
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Breast-fed Child (= Don't Know)	-0.435***	-0.456***	-0.494***	-0.484***	-0.501***
	(0.07)	(0.10)	(0.10)	(0.12)	(0.11)
(= No)	-0.008	-0.010*	-0.016**	-0.009	-0.017*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Educ. (< 12 Years)	-0.004	-0.008	0.011	-0.004	-0.069**
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
(= 12 Years)	0.002	0.008	0.009	0.007	0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(> 12, No College Grad.)	0.002	0.002	0.004	0.001	0.007
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.007**	0.008*	0.011**	0.007*	0.007
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Mother's Age (<= 19 Years)	-0.029	-0.073*	-0.057	-0.053	-0.068
	(0.03)	(0.04)	(0.04)	(0.05)	(0.07)
(= 20-29 Years)	-0.011**	-0.014***	-0.014**	-0.007	-0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Mother's Marital Stat. (= Married)	0.008	0.002	0.002	0.003	0.006
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	0.005	0.007	0.011*	0.007	0.011
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
(= Non-His,Black Only)	-0.003	0.008	-0.007	0.020**	-0.009
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(= Non-His,Other,Mult.)	-0.009*	-0.006	-0.004	-0.009	-0.007
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.002	0.001	-0.006	0.001	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treated Dummy	-0.019*	-0.030***	-0.036***	0.039***	-0.072***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
After Change Dummy	0.121***	0.119***	0.121***	0.036***	0.038**
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Treated and After Change Dummy	**0.003**	**-0.005**	**-0.017**	**0.011**	**-0.004**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.14	0.1	0.09	0.09	0.09

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6′ – OLS regression results for Up-To-Date flag (0 or 1) for *MCV* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.903***	0.023	-0.005	0.980***	0.018
	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)
First Born Child (= No)	-0.003	-0.007	-0.003	-0.003	-0.012
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Child's Age Group (= 19-23 m)	-0.036***	-0.034***	-0.033***	-0.033***	-0.040***
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
(= 24-29 m)	-0.006**	-0.006	-0.005	-0.005	-0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Breast-fed Child (= Don't Know)	-0.04	-0.001	-0.006	-0.002	0.055***
	(0.04)	(0.06)	(0.06)	(0.06)	(0.01)
(= No)	0.012***	0.010*	0.009	0.011**	0.013
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Mother's Educ. (< 12 Years)	-0.008	-0.025	-0.026	-0.040***	-0.071**
	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)
(= 12 Years)	-0.012**	-0.01	-0.019**	-0.013*	-0.004
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(> 12, No College Grad.)	-0.006*	-0.008	-0.006	-0.009*	-0.009
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.009***	0.008**	0.013**	0.004	0.010*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Mother's Age (<= 19 Years)	-0.039*	-0.033	-0.011	-0.029	-0.039
	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
(= 20-29 Years)	0.001	-0.007	-0.003	-0.007	-0.008
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Mother's Marital Stat. (= Married)	0.002	-0.002	-0.008	-0.001	-0.008
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	0.002	0.005	0.004	0.001	0.011
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(= Non-His,Black Only)	0.011	0.008	-0.006	0.016	0.005
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
(= Non-His,Other,Mult.)	0.004	0.006	0.004	0.003	0.007
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.012***	0.017***	0.014***	0.018***	0.019***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Treated Dummy	-0.049***	-0.067***	-0.077***	-0.126***	-0.019***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
After Change Dummy	-0.001	-0.020***	0.018**	-0.020***	-0.031***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treated and After Change Dummy	**0.005**	**0.005**	**-0.001**	**0.009**	**0.007**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.12	0.07	0.08	0.07	0.06

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7′ – OLS regression results for Up-To-Date flag (0 or 1) for *PCV* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.808***	-0.042*	-0.037	0.858***	-0.094***
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
First Born Child (= No)	0.001	-0.003	0.002	-0.005	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Age Group (= 19-23 m)	-0.033***	-0.035***	-0.037***	-0.034***	-0.044***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(= 24-29 m)	-0.007*	-0.005	-0.006	-0.006	-0.016**
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Breast-fed Child (= Don't Know)	-0.541***	-0.622***	-0.670***	-0.489***	-0.576***
	(0.07)	(0.11)	(0.09)	(0.13)	(0.12)
(= No)	-0.014***	-0.018***	-0.026***	-0.019***	-0.032***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Educ. (< 12 Years)	-0.041***	-0.043**	-0.047	-0.039*	-0.06
	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)
(= 12 Years)	-0.035***	-0.033***	-0.041***	-0.030***	-0.028**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(> 12, No College Grad.)	-0.018***	-0.018***	-0.012**	-0.015**	-0.013*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.020***	0.025***	0.025***	0.021***	0.034***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Age (<= 19 Years)	0.01	-0.064	-0.051	-0.065	-0.091
	(0.03)	(0.05)	(0.05)	(0.06)	(0.09)
(= 20-29 Years)	-0.012***	-0.016***	-0.018**	-0.012**	-0.016**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Marital Stat. (= Married)	0.01	0.005	0.005	0.01	-0.014
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Race/Ethn. of Child (=Hispanic)	-0.018***	-0.014	-0.019*	-0.018**	-0.015
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
(= Non-His,Black Only)	-0.030***	-0.011	-0.017	-0.014	-0.017
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
(= Non-His,Other,Mult.)	-0.042***	-0.037***	-0.038***	-0.038***	-0.042***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.006**	0.008*	0.002	0.009*	0.001
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Treated Dummy	-0.113***	-0.119***	-0.142***	-0.389***	-0.039***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
After Change Dummy	0.035***	0.039***	0.030***	0.036***	0.038***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treated and After Change Dummy	**0.003**	**0.007**	**0.003**	**0.001**	**-0.016**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.1	0.07	0.07	0.06	0.06

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8′ – OLS regression results for Up-To-Date flag (0 or 1) for *Polio* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.919***	-0.001	-0.023	0.979***	0.013
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
First Born Child (= No)	0.003	0.002	0.001	0.001	-0.001
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Child's Age Group (= 19-23 m)	-0.021***	-0.021***	-0.027***	-0.020***	-0.022***
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
(= 24-29 m)	-0.003	-0.002	-0.006	-0.004	-0.007
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Breast-fed Child (= Don't Know)	-0.090*	-0.061	-0.07	-0.008	-0.062
	(0.05)	(0.08)	(0.08)	(0.06)	(0.08)
(= No)	0.003	0.005	0.004	0.008	0.007
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Mother's Educ. (< 12 Years)	-0.017	-0.012	-0.006	-0.014	-0.064**
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
(= 12 Years)	-0.005	-0.007	-0.018**	-0.005	-0.005
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
(> 12, No College Grad.)	-0.001	-0.003	-0.004	-0.001	-0.008
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Income-to-Poverty Ratio	0.008***	0.010***	0.014***	0.008**	0.007
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mother's Age (<= 19 Years)	-0.02	-0.041	-0.02	-0.063	-0.033
	(0.02)	(0.04)	(0.04)	(0.04)	(0.06)
(= 20-29 Years)	-0.008***	-0.009**	-0.008*	-0.009**	-0.014**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Mother's Marital Stat. (= Married)	0.009*	0.006	0.008	0.007	0.022**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	0.003	0.004	0.006	0.001	0.020*
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
(= Non-His,Black Only)	0.012***	0.018**	0.005	0.022***	0.024*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(= Non-His,Other,Mult.)	0.001	0.001	0.003	-0.004	-0.003
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.005***	0.007**	0.003	0.009***	0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treated Dummy	-0.043***	-0.071***	-0.070***	-0.236***	-0.059***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
After Change Dummy	0.009**	-0.004	0.016***	-0.005	-0.006
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Treated and After Change Dummy	**-0.001**	**0.004**	**-0.008**	**0.006**	**0.001**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.15	0.08	0.09	0.08	0.08

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9′ – OLS regression results for Up-To-Date flag (0 or 1) for *ROT* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.303***	-0.554***	-0.167***	-0.080**	-0.521***
	(0.04)	(0.04)	(0.04)	(0.03)	(0.08)
First Born Child (= No)	-0.012*	-0.013*	-0.009	-0.009	-0.005
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Age Group (= 19-23 m)	0.171***	0.167***	0.205***	0.171***	0.169***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(= 24-29 m)	0.078***	0.076***	0.085***	0.074***	0.084***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Breast-fed Child (= Don't Know)	-0.367***	-0.263***	-0.284***	-0.236**	-0.290***
	(0.05)	(0.09)	(0.08)	(0.10)	(0.09)
(= No)	-0.019***	-0.019***	-0.018**	-0.021***	-0.032***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Educ. (< 12 Years)	-0.042**	-0.040*	-0.035	-0.046*	-0.059*
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
(= 12 Years)	-0.033***	-0.037***	-0.058***	-0.042***	-0.018
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
(> 12, No College Grad.)	-0.016***	-0.022**	-0.019*	-0.016*	-0.024**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.025***	0.030***	0.028***	0.033***	0.036***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Age (<= 19 Years)	0.002	-0.013	-0.014	-0.044	0.001
	(0.04)	(0.07)	(0.08)	(0.08)	(0.12)
(= 20-29 Years)	-0.021***	-0.023***	-0.015*	-0.022***	-0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Marital Stat. (= Married)	0.027***	0.019*	0.027**	0.023**	-0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	-0.004	-0.001	-0.002	-0.001	-0.007
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
(= Non-His,Black Only)	-0.033**	-0.002	-0.028	0.019	0.015
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
(= Non-His,Other,Mult.)	-0.035***	-0.035***	-0.033***	-0.033***	-0.016
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.004	0.009*	0.002	0.008	-0.006
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Treated Dummy	-0.093***	-0.113***	-0.135***	-0.026**	-0.122***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
After Change Dummy	0.225***	0.583***	0.234***	0.583***	0.552***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Treated and After Change Dummy	**0.017**	**0.017**	**0.016**	**0.019**	**0.009**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.32	0.31	0.32	0.31	0.28

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10′ – OLS regression results for Up-To-Date flag (0 or 1) for *VRC* vaccine among children aged 19-35 months covered by private insurances (SE in parentheses)

	Base model	*Alternative/Robustness check models*			
Model #	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Intercept	0.835***	0.008	-0.008	0.945***	0.054
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
First Born Child (= No)	-0.005	-0.011*	-0.012	-0.006	-0.013
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Age Group (= 19-23 m)	-0.031***	-0.030***	-0.034***	-0.033***	-0.033***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(= 24-29 m)	-0.004	0.001	-0.001	-0.004	-0.002
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Breast-fed Child (= Don't Know)	-0.048	0.081***	0.076***	0.080***	0.082***
	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
(= No)	0.015***	0.013**	0.009	0.016***	0.018**
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Mother's Educ. (< 12 Years)	0.006	-0.003	-0.004	-0.022	-0.048
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
(= 12 Years)	-0.006	-0.002	-0.005	-0.005	-0.006
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(> 12, No College Grad.)	-0.003	-0.003	-0.001	-0.007	-0.011
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Income-to-Poverty Ratio	0.014***	0.012***	0.012**	0.007	0.009
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Mother's Age (<= 19 Years)	-0.045**	-0.068	-0.054	-0.073	-0.012
	(0.02)	(0.04)	(0.05)	(0.05)	(0.05)
(= 20-29 Years)	-0.005	-0.013**	-0.017**	-0.009*	-0.011
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's Marital Stat. (= Married)	-0.004	-0.009	-0.012	-0.005	-0.001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Race/Ethn. of Child (=Hispanic)	0.012**	0.012**	0.010*	0.007	0.007
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
(= Non-His,Black Only)	0.006	-0.004	-0.013	0.008	-0.001
	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)
(= Non-His,Other,Mult.)	0.004	0.005	0.001	0.002	0.009
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Child's Gender (= Female)	0.007**	0.007*	0.005	0.011**	0.013***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treated Dummy	-0.070***	-0.114***	-0.116***	-0.036***	-0.129***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
After Change Dummy	-0.002	-0.014***	0.015	-0.013**	-0.026***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treated and After Change Dummy	**0.015****	**0.020****	**0.018***	**0.019****	**0.036*****
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of observations	36,896	20,122	14,610	20,122	9,236
R^2	0.1	0.06	0.07	0.06	0.06

Note: a. Models: (1) with fixed effect and all covariates, clustered standard errors for states. **(2)** same as (1) but after propensity score matching between treated and control groups, clustered for states. **(3)** same as (2) but only 2012 for after change period (reducing grandfathered status impact). **(4)** same as (2) but after eliminating 10 states with Universal/Universal Select programs from treated group. **(5)** same as (2) but after eliminating 19 states with legal exception permissions from treated group.

b. There are other variables in the models including: state dummy variables, number of providers responding the questionnaires, shot card usage, number of children, mobility of family, vaccine ordered from state, number of household members, and time dummy variables.

c. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Revision of the regression results

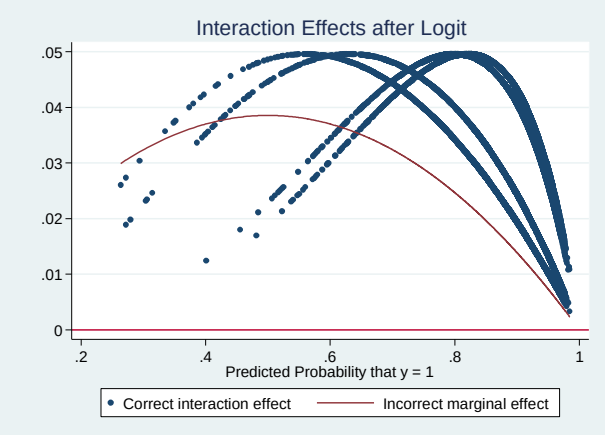
for interaction terms in *Logit* models (2) and (4)

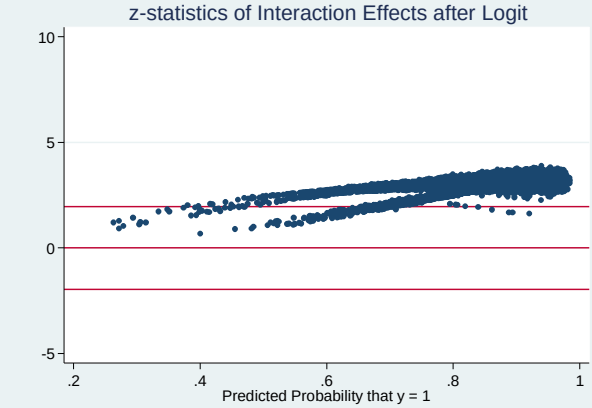
of Appendix A

Figure B1 – Revising magnitude and significance of the interaction term in model (2) of *Logit* regressions in Appendix A, by using *inteff* command in Stata

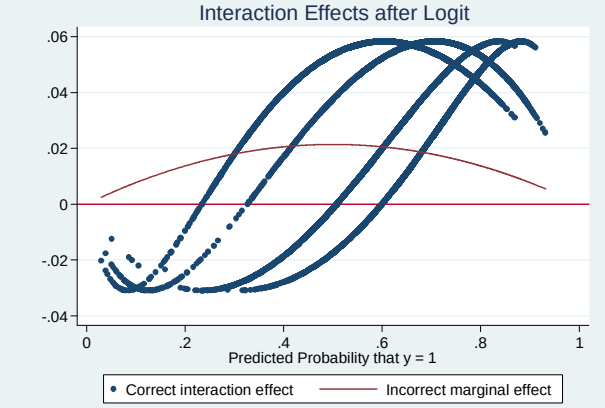
(ie ≡ interaction effect, se ≡ standard errors for interaction effect, z ≡ z-stat for interaction effect)

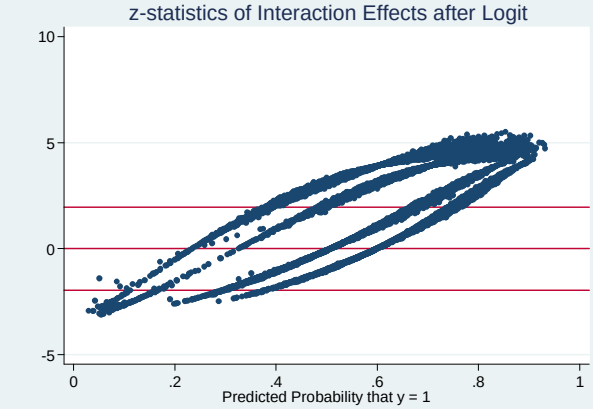
DTaP Avg(ie) = 0.032** Avg(se) = 0.011 Avg(z) = 3.01



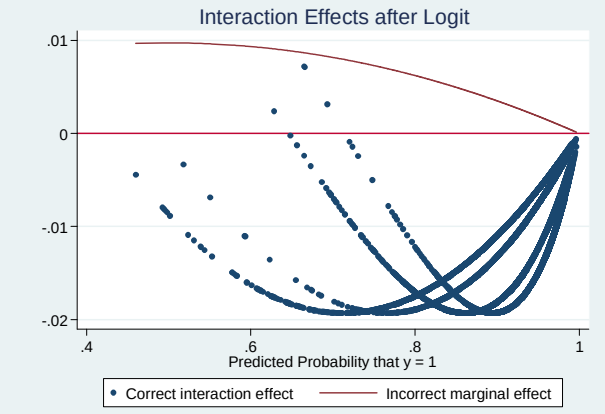


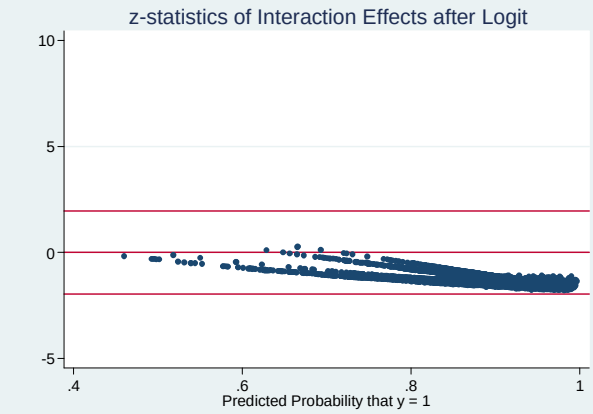
Hep A Avg(ie) = 0.034* Avg(se) = 0.017 Avg(z) = 2.24



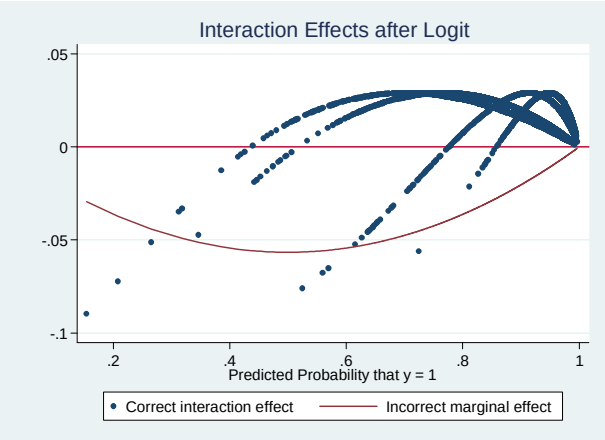


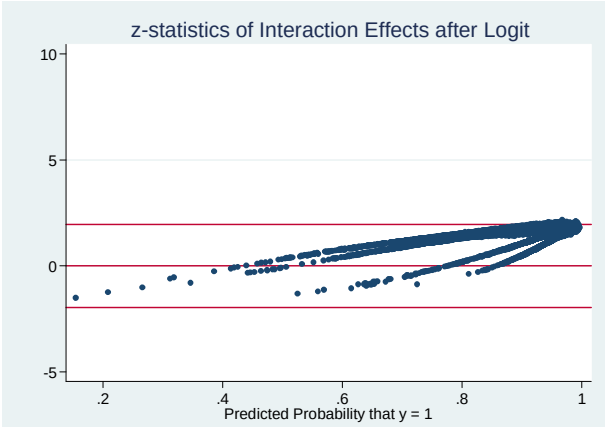
Hep B Avg(ie) = -0.012 Avg(se) = 0.009 Avg(z) = -1.43



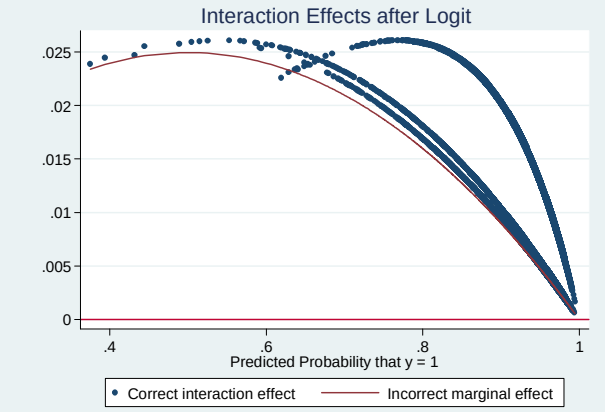


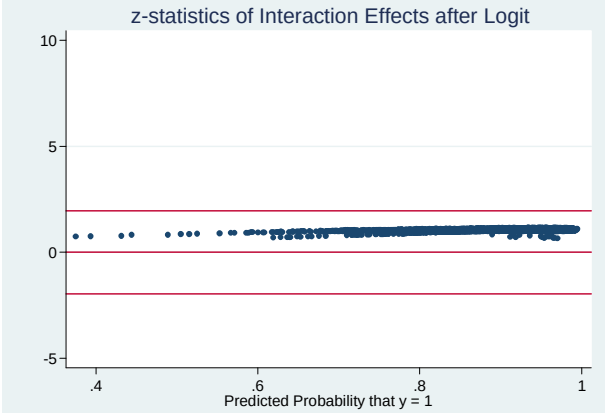
Hib Avg(ie) = 0.019 Avg(se) = 0.014 Avg(z) = 1.60



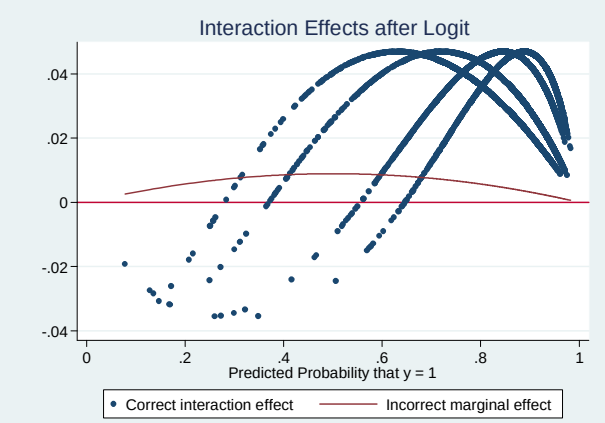


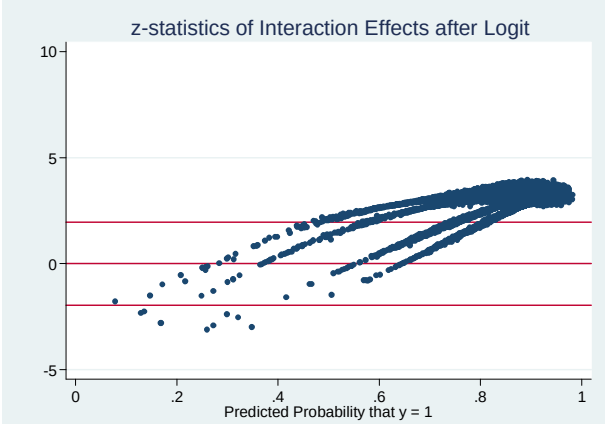
MCV Avg(ie) = 0.010 Avg(se) = 0.010 Avg(z) = 1.07



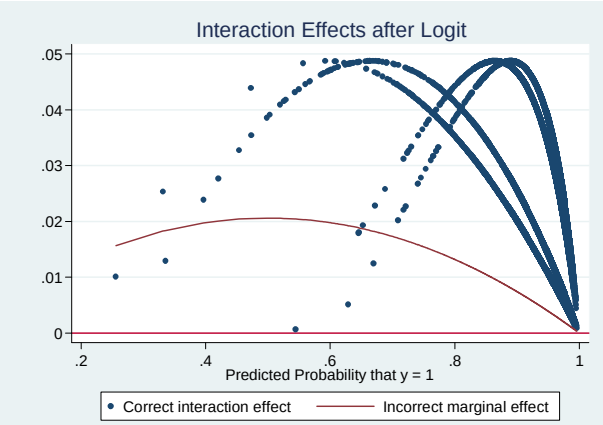


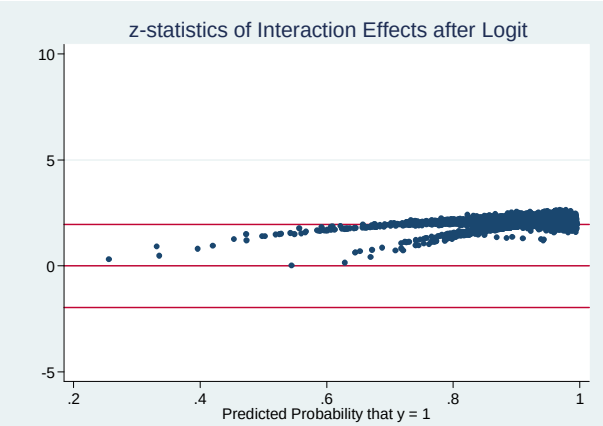
PCV Avg(ie) = 0.035* Avg(se) = 0.012 Avg(z) = 2.98



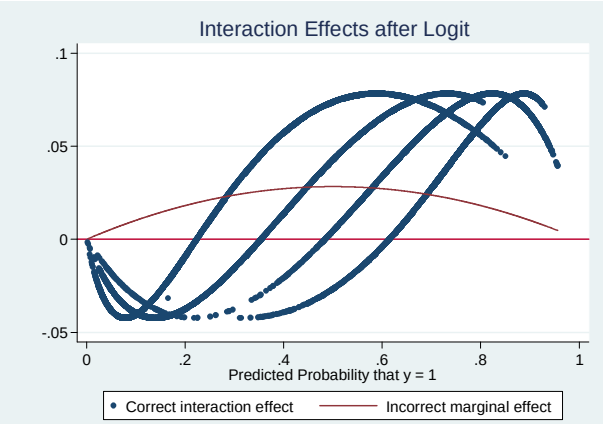


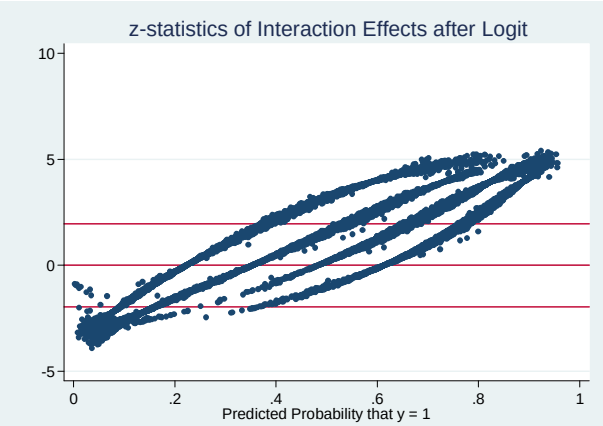
POL Avg(ie) = 0.022* Avg(se) = 0.011 Avg(z) = 2.11



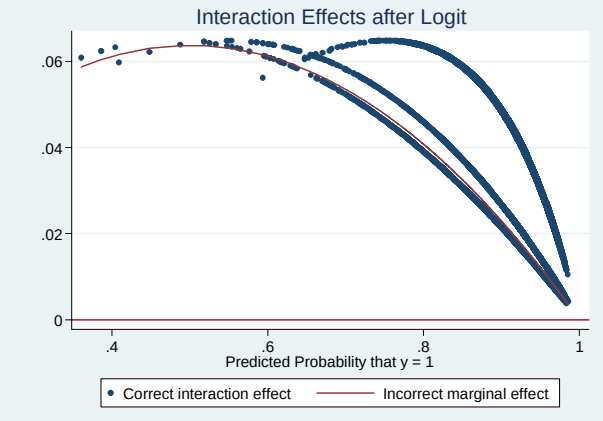


ROT Avg(ie) = 0.030 Avg(se) = 0.023 Avg(z) = 1.29





VRC Avg(ie) = 0.030** Avg(se) = 0.010 Avg(z) = 3.10



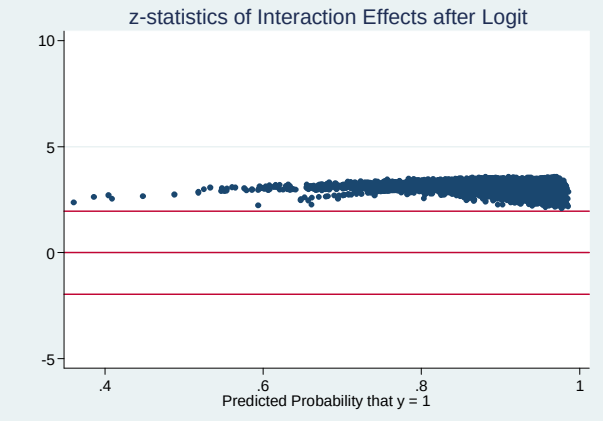
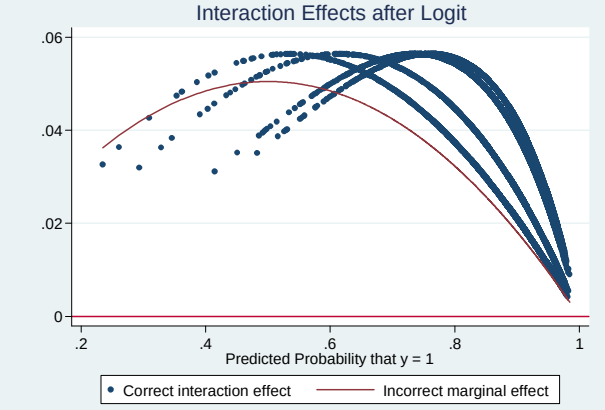
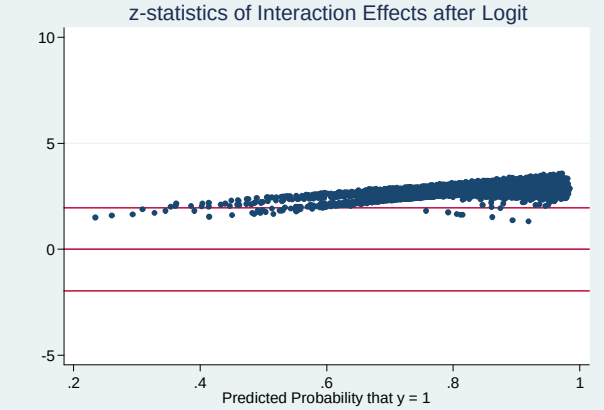


Figure B2 – Revising magnitude and significance of the interaction term in model (4) of *Logit* regressions in Appendix A, by using *inteff* command in Stata

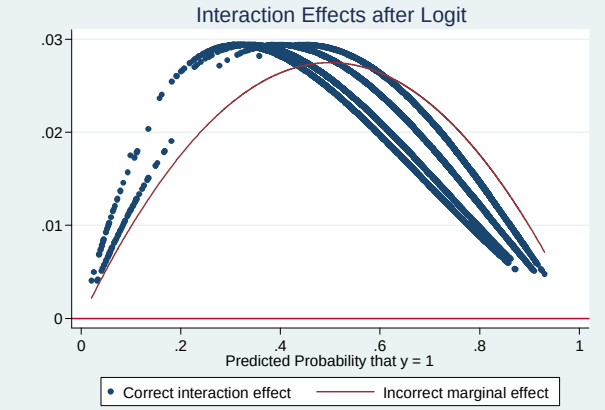
(ie ≡ interaction effect, se ≡ standard errors for interaction effect, z ≡ z-stat for interaction effect)

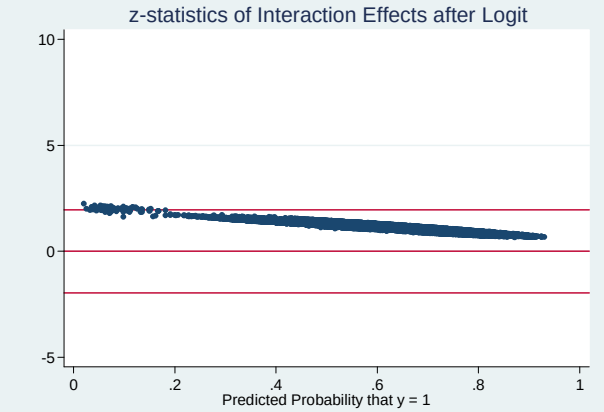
DTaP Avg(ie) = 0.032** Avg(se) = 0.011 Avg(z) = 3.01



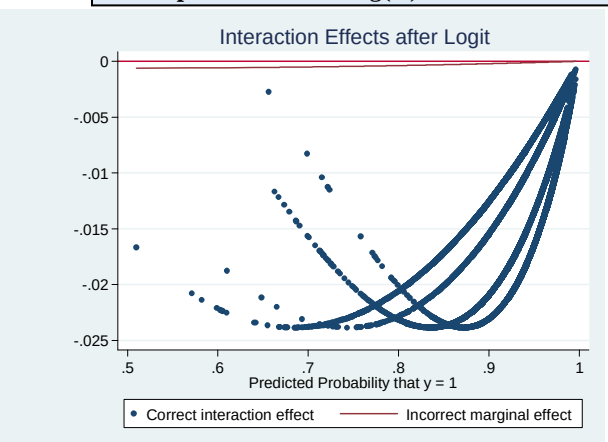


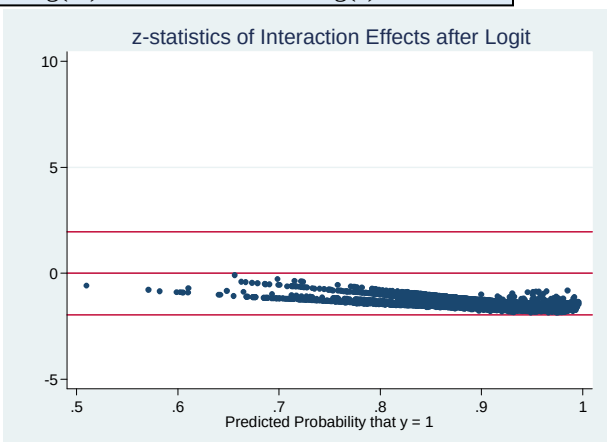
Hep A Avg(ie) = 0.020 Avg(se) = 0.018 Avg(z) = 1.12



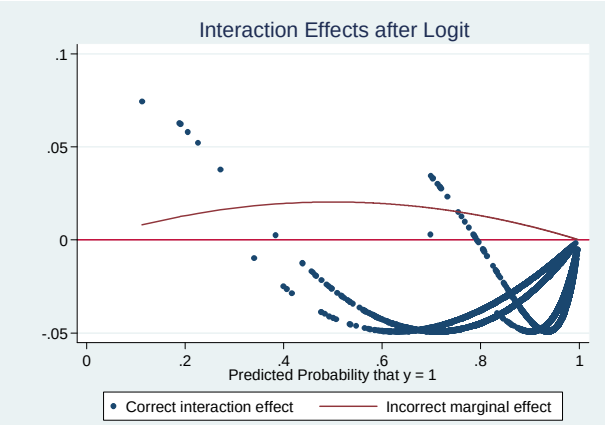


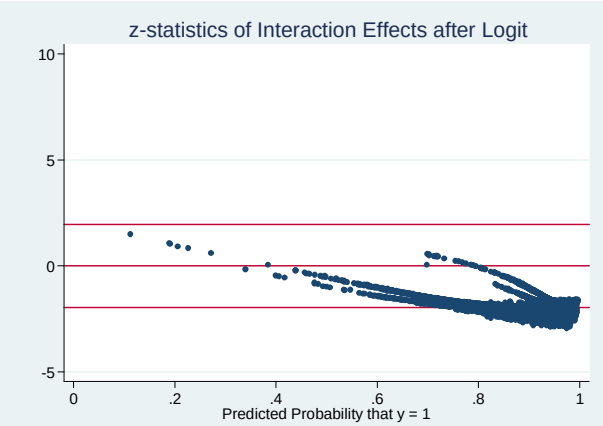
Hep B Avg(ie) = -0.013 Avg(se) = 0.009 Avg(z) = -1.53



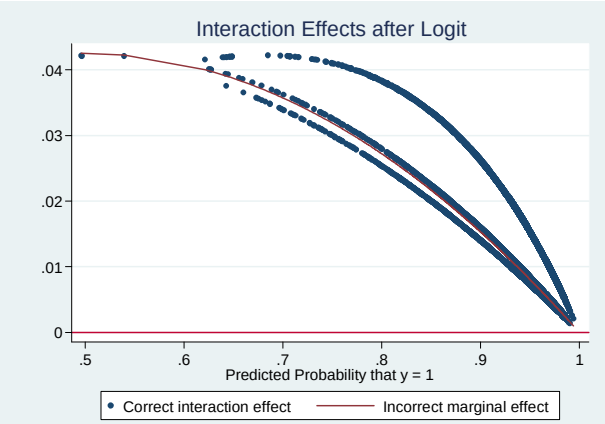


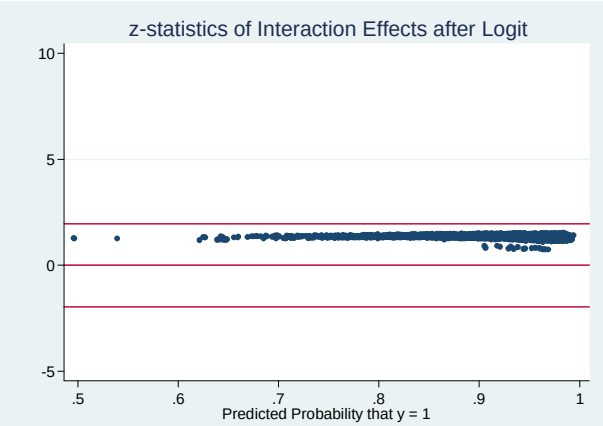
Hib Avg(ie) = -0.029 Avg(se) = 0.016 Avg(z) = -1.96



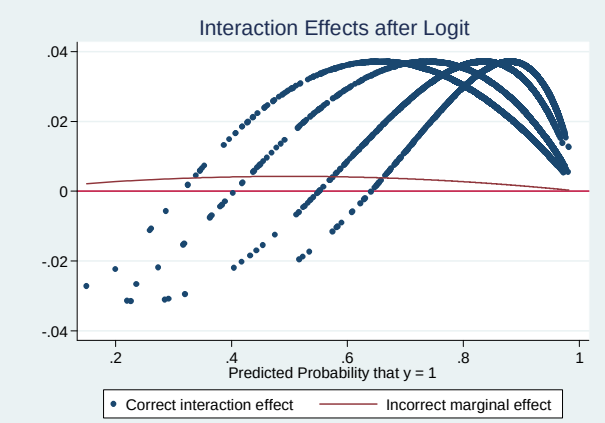


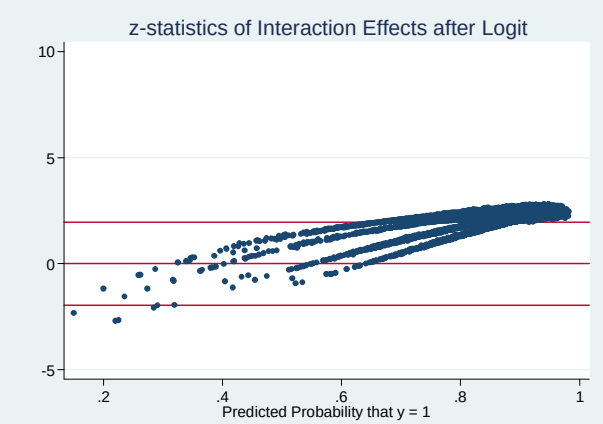
MCV Avg(ie) = 0.013 Avg(se) = 0.010 Avg(z) = 1.36



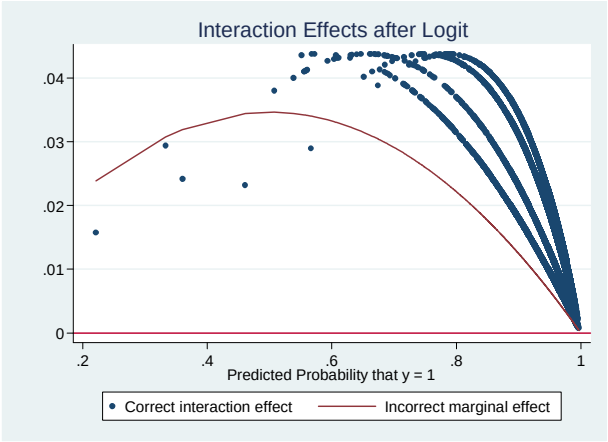


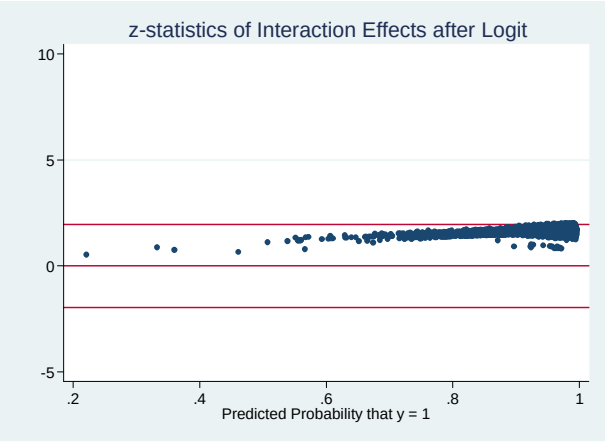
PCV Avg(ie) = 0.028* Avg(se) = 0.013 Avg(z) = 2.18



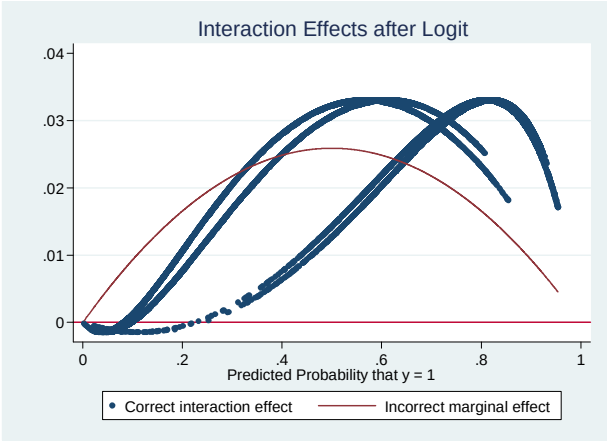


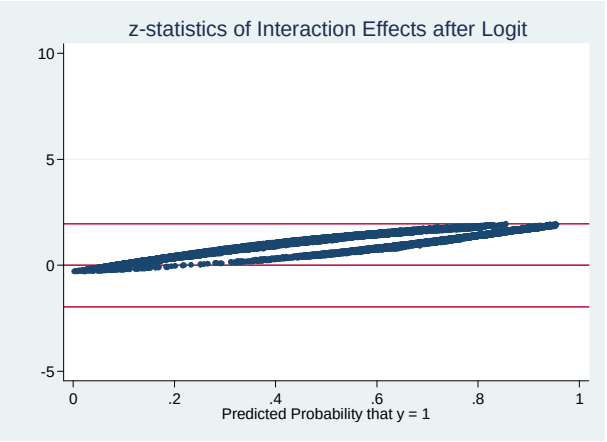
POL Avg(ie) = 0.014 Avg(se) = 0.009 Avg(z) = 1.69



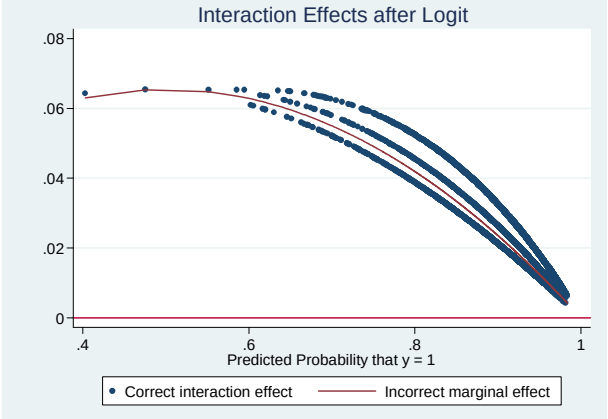


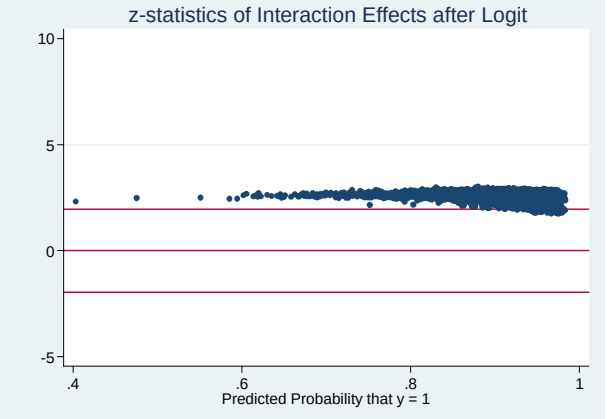
ROT Avg(ie) = 0.022 Avg(se) = 0.022 Avg(z) = 0.99





VRC Avg(ie) = 0.022* Avg(se) = 0.002 Avg(z) = 2.59





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January 2016

Education

January 2016 Ph.D., Economics, University of Wisconsin-Milwaukee, Milwaukee, WI
Dissertation title: "Essays on Two Implications of the Affordable Care Act (ACA)"

"High Pass" in PhD preliminary examinations in Microeconomics and Labor Economics

May 2011 M.A., Economics, University of Wisconsin-Milwaukee, Milwaukee, WI
Thesis title: "Market efficiency in stock exchanges: Case of Tehran Stock Exchange"

July 2006 M.Sc., Socio-Economic Systems Engineering, Institute for Management and Planning Studies (IMPS), Tehran, Iran
Thesis title: "Saving Behavior of Iranian Households"

September 2001 B.Sc., Civil Engineering, Amirkabir University of Technology-Tehran Polytechnic, Tehran, Iran

Fields of Interest

Primary: Applied Microeconomics, Health Economics, Labor Economics, Industrial Organization

Secondary: Econometrics

Working Papers

Esmaeil Salem, "How Effective was ACA for Childhood Immunization Coverage in the United States?"

Esmaeil Salem, "Effect of the Rebate on Insured's Retention Rate: An Applied Study."

Research and Professional Experience

Jul 2015-Jan 2016 ***Healthcare Statistician*** at Assurant Health, Milwaukee
Risk adjustment simulation and modeling. Cost-benefit analysis for prospective and retrospective interventions. Transfer payments simulations based on Health conditions (HCCs) and ACA's Risk Adjustment and Reinsurance mechanisms. CMS' Edge Server RA/RI data reconciliation and simulations.

Sep 2014-June 2015 ***Business Analyst II*** at Assurant Health, Milwaukee
Predictive modeling using medical and pharmacy claims and other data sources like TRUVEN Marketscan, Acxiom, and BLS. Risk scoring and simulations based on HHS risk scoring and risk adjustment models. Cost trend for major health conditions like diabetes, cancers, etc.

Feb 2013-Jul 2014 ***Data Modeling/Statistics Intern*** at Assurant Health, Milwaukee
Predictive modeling and analysis of the behavior of policyholders using statistical models like Logistic and GLM regressions. Involved in various projects such as modeling for retention analysis, ranking predictive variables in models, risk scoring, simulations, etc.

Jul 2008-Jan 2010 ***Director of Financial and Investment studies*** at Amin Investment Bank, Tehran, Iran

May 2007-Jul 2008 ***Economic Expert & Investment Analyst*** at Iran Foreign Investment Company (IFIC), Tehran, Iran

Oct 2005-Apr 2007 ***Economic Expert & Investment Analyst*** at AryaPangan Co., Tehran, Iran

Dec 2003-Oct 2005 ***Economic Planning Expert*** at Ministry of Energy, Tehran, Iran

Apr 2002-Nov 2006 ***Research Assistant*** at Sharif University of Technology, Tehran, Iran

Teaching Experience

Aug 2010-Dec 2014 ***Instructor and Teacher Assistant*** at Department of Economics, UW-Milwaukee, Milwaukee, WI
Introductory Economics (Fall 2011-Fall 2014).
Introduction to Microeconomics (Summer 2013).
Teaching Assistant for Introductory Economics (Fall 2010-Fall 2012).

Jan 2013-May 2013 ***Adjunct Lecturer*** at Lubar School of Business, UW-Milwaukee, Milwaukee
Teaching Managerial Economics to MBA students.

Summer 2009 ***COMFAR Lecturer*** at Ghods Niroo, Tehran, Iran

Software Skills

SAS, Stata, Eviews, Maple, Gauss, C++, M.S. Package, MATLAB, R, SQL

Certificates

Jan 2014 SAS Base Programmer

Jun 2011 Passed Chartered Financial Analyst (CFA) Exam, Level I

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