

Long-Term Effect of Mathematical Literacy on Consumer Behavior: Evidence from Curricular Reforms

Felix Nguyen*

June 20, 2022

Abstract

Whether our formative experiences, including education, leave long lasting impacts on our later life consumption habits is an emerging question that has received more and more attention in recent literature. In this paper, I examine how mathematical education during formative period of high school can shape later life consumption and spending habits. Exploiting the variation in required math courses for high school graduation from a series of curricular reforms in the late 1980s, I show that increase in mathematical literacy can lead to significant increases in consumption-related saving behavior, up to 15.4% reduction in monthly packaged goods expenditure and increase coupon usage rate by nearly a quarter. These effects persist to much later in life, and are robust to potential biases from staggered adoption. Further analyses also show that the effects vary across demographic groups, are stronger for minorities, and differ between before and after the financial crisis of 2008.

*PhD Student in Quantitative Marketing - Emory University - pnguy38@emory.edu.

Disclaimer: Researcher's own analyses are calculated based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

1 Introduction

It goes without dispute that education has a strong and lasting effect on human behavior, including purchasing and consumption habits. What we learn, especially during our formative years during high school and early college, plays a pivotal role in shaping our way of thinking and interacting with the outside world, including the way we make purchases and what we consume. Through education, the consumer gains better long term planning ability, more accurate evaluation of costs and benefits, and more importantly, better self-regulation ability, amongst many other behavioral advantages. Prior research (e.g., [Sabol et al. 2021](#)) has shown that mathematical education, both of oneself and one's parents, is one of the most important determinant of future economic outcomes and financial behaviors. Following this, an emerging strand of literature in consumer finance and labor economics (e.g., [Lusardi and Tufano 2015](#), [Cole et al. 2016](#)) has been investigating the effect of math, economics, and personal finance courses during K-12 education on later life financial outcomes. Most of the existing studies in this literature have focused solely on traditional financial measures such as debt repayment, student loan, or credit card debt. Yet, there are other important and arguable as impactful manifestations of financial behaviors such as spending and consumption decisions that largely have not received much attention. This paper aims to address this gap in the literature.

In this study, I exploit the state level variation in high school mathematical education requirement due to a curricular reform wave swept across the United States after the damning report on the state of K-12 education in the country, "A Nation at Risk: The Imperative for Educational Reform" in 1983. Through a staggered adoption robust event study design and various robustness checks, I show that the reforms have strong effects on consumption-related saving behavior, such as monthly packaged goods expenditure, coupon utilization, and the rate of purchases made on sale. Taking advantage of the granularity of the household level data, I further investigate treatment heterogeneity across several demographic variables. Results indicate that the additional math requirement mainly affects minorities such as Black and Asian, and married consumers. Furthermore, the effects become less pronounced with age, but still significant and positive. Additionally, I further show that the effects are amplified by the financial crisis in 2008, provide evidence of how mathematical literacy can influence consumers responses to economic crises.

These findings have several implications. First of all, they provide a new perspective on how childhood education can influence later life economic outcome. Prior research has shown that people with more mathematical education tend to have larger investment assets and lower debt, and now we see that they do not only have increased savings on

a macro level, but also on a more microscopic, day-to-day level. As some studies have shown (e.g., [Dubé et al. 2018](#), [Nevo and Wong 2019](#)), retail consumption accounts for as much as 40-50% of total expense for many, and is highly sensitive to economic condition, thus my study showed another important benefit of math education that should be overlooked by policymakers. Secondly, the results also help inform retailers and brands of where and when to target consumers with price promotions and coupon strategy, and provide managers with a better understanding of consumers' preference for coupon and sale deals.

2 Literature Review

This research is related to two main research streams. First of all, it contributes to the extensive area of financial literacy and financial and mathematical education research ([Hastings et al. 2013](#), [Lusardi and Mitchell 2014](#)). Most of the work done in this literature stream has focused solely on how financial and mathematical education influences financial literacy and downstream financial outcomes, such as credit scores, wealth accumulation, debt and saving etc. Empirical results have been inconclusive about whether financial education is effective, with some ([Bernheim et al. 2001](#), [Skimmyhorn 2016](#), [Lusardi and Tufano 2015](#)) find positive effect of financial education on financial outcomes, while others ([Cole et al. 2016](#), [Fernandes et al. 2014](#)) find no statistically significant effects. The evidence of the effect of mathematical education is stronger, with [Cole et al. \(2016\)](#), [Brown et al. \(2016\)](#), and [Goodman \(2019\)](#) all found significant positive effect of additional math coursework on debt repayment, credit card usage, student loan, and labor outcomes. This research extends this literature stream by examining another type of outcome variables that has been so far overlooked: future consumption behavior.

The second strand of research similar to this paper is the literature on consumers' habit formation. A growing area in economics and marketing, research in this literature stipulates that experiences, during childhood, exerts a strong influence on preferences and consumption habit. For example, [Bronnenberg et al. \(2012\)](#) find that over 40 percent of geographic variation in market shares is explained by persistent childhood location, [Severen and Van Benthem \(2022\)](#) conclude that changes in gasoline price during formative years can shift later life travel behavior, [Binder and Makridis \(2020\)](#) show that consumers living through the oil crisis tend to be more pessimistic and frugal in consumption, and [Malmendier and Shen \(2018\)](#) find that consumers experiencing high unemployment rate during childhood tend to have a higher coupon utilization rate, purchase more products on sale, and buy more generic brand. My paper contributes to this literature by considering exposure to finance-related education as another important factor of preference formation.

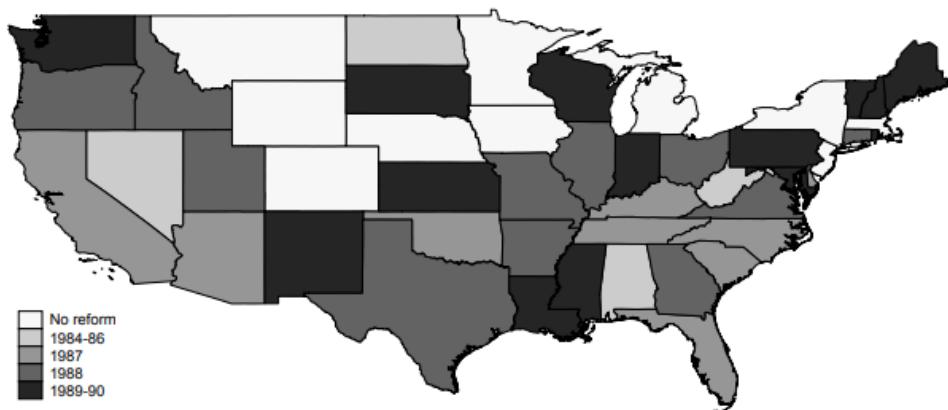


Figure 1: Timing of Math Curricular Reform by States

3 Context and Data

3.1 “A Nation at Risk” and Curricular Reform in the United States

The exogenous source of variation used in this research is the increased graduation requirements in terms of mathematics coursework, adopted by states throughout the United States in the wake of the April 1983’s publication “A Nation at Risk”, the final report of the National Commission on Excellence in Education (Gardner et al. 1983). This report opened with a stark warning, “Our Nation is at risk. Our once unchallenged preeminence in commerce, industry, science, and technological innovation is being overtaken by competitors throughout the world.” The report continued by mentioning Japan, South Korea, and Germany as countries making technological advances in industries where America had historically been dominant, concluding that “learning is the indispensable investment required for success in the ‘information age’ we are entering.”.

The commission identified as one of the primary causes of perceived educational decline the fact that “secondary school curricula have been homogenized, diluted, and diffused to the point that they no longer have a central purpose.” Noting that US high school students earned 25% of their credits in “physical and health education, work experience outside the school, remedial English and mathematics, and personal service and development courses,” the commission proposed that state and local graduation requirements be strengthened dramatically. Specifically, the commission recommended that at least 3 years of mathematics and 3 years of science have to be taken by high school students during their 4 years of high school.

Figure 1 plots the differential timing of math curricular reforms across states. We can see that only six states enacted reforms applying to cohorts graduating high school prior to 1987. The bulk of the reforms are roughly evenly split between cohorts graduating in 1987, 1988, and 1989, with only one state enacting reforms after that period (New Mexico). This timing stems from state policymakers immediate responses to “A Nation at Risk” by legislating increased graduation requirements in year T (generally 1983, 1984, or 1985) to apply to students entering high school that year and thus graduating in year $T + 4$. We can also see that variation in the timing of reforms is not closely related to geography with every region contains both late and early reforming states, as well as non-reforming ones, thus is plausibly exogenous. Variation in the timing of the math curricular reforms, the fact that some states did not carry out the reforms, in addition to the observation that such variation is not spatially concentrated, form my main strategy to identify the causal impact of such reforms on later life consumption related saving behavior. In the Robustness section, I discuss several methods to correct for any possible temporal spatial correlation that may exist.

3.2 Data

I use NielsenIQ’s Consumer Panel Data (provided through partnership with the Kilts Center for Marketing at the University of Chicago), a panel of 40,000-60,000 households from 2004 - 2018, who, through the use of in-home scanners, record all of their purchases (from any outlet) intended for personal, in-home use. These panelists provide information about their households and what products they buy, as well as when and where they make purchases. The transactions are recorded by households using an optical scanner in their homes, which members use to the barcodes on each of packaged goods items that they purchase during trips to supermarkets, convenience stores, mass merchandisers, and so on. Details on the stratified sampling methodology employed by Nielsen to promote representativeness of the Homescan panel can be found in Kilts Center for Marketing (2022). A key advantage of this dataset over retail scanner data is that it covers all retailers in the United States, including those without a contractual data sharing agreement with Nielsen. However, one concern about this data is that many consumers do not stay in the same state as where they went to school in, thus if we use their current state as is to determine treatment status, we will get imprecise estimates of the treatment.

Through a Supplemental Survey in 2008 that asked panelists about their state of origin and when they moved, conducted for [Bronnenberg et al. \(2012\)](#), I alleviate this concern by narrow down the dataset to only households where the head of household has stayed in the same state they were born in (“Stayers”). The survey provides information on when

and if a panelist move out of their home state, and how long have they stayed in their current state. A four-step matching procedure was performed to find which household members in the 2008 Survey are the heads of household (the detailed panelist - household matching procedure can be found in [Bronnenberg et al. \(2012\)](#)'s appendix).

The "Stayers" sample consists of 23,376 households and more than 2,100,000 monthly observations. I narrow down the results to people born between 1950 and 1980, to avoid other education reforms we are not observing ([Cole et al. \(2016\)](#) identify a new wave of reform starting in the early 2000s), reducing the sample to around 1,460,000 observations. I also construct the main outcomes of interest: coupon usage, percentage of purchases made on sale, share of private label, as well as control variables such as demographics, age, education, income, etc. I further supplement the household data with ZIP Code level home price index as a proxy measure of household's wealth, to use as a control variable, following [Dubé et al. \(2018\)](#), as even though we can observe household's income, their total asset is not reported.

To quantify spending behavior of households, I examine the following quantities: (1) How often they buy discounted products, (2) How often do they use coupons, (3) How often do they buy private label products, and (4) What is their monthly expenditure. The first measure is calculated as:

$$DealRate_{it} = \frac{\sum_s \mathbf{1}\{OnDeal_s\}}{N_{it}}$$

That is, the ratio of number of transactions where a price promotion is active over the total number of transactions within a month t of a household i . This is representative of the *short-term saving behavior* of households, as the information about price promotion is often not known well in advance, hence the decision to purchase "on sale" products is often a just-in-time decision if they are price-conscious. The second outcome is calculated as:

$$CouponRate_{it} = \frac{\sum_s CouponValue_{sit}}{\sum_s TotalValue_{sit}}$$

This is relatively similar to the first measure, however here we are looking at monetary values instead of just numbers of transactions. Specifically, it is the ratio between the total amount of coupon used in month t by household i , in dollar term, and the total value of their consumption (include coupon). In contrary to the previous measure, this is more representative of *long-term saving behavior* of households, as acquiring and redeeming coupons typically require longer term planning from the consumer side. Another difference is that coupon usage also signals higher variety seeking and lower brand loyalty ([Bawa](#)

and Shoemaker 1987), as it is often tied to specific brands or products. Another measure that may reflect the trade off between price and brand loyalty is *Private Label share*, i.e., the percentage of products purchased by a household i in month t that are private labeled products. This is calculated as:

$$PLShare_{it} = \frac{\sum^s \mathbf{1}\{PrivateLabel_s\}}{N_{it}}$$

Same measure is examined by Dubé et al. (2018), and in that study, the authors find that income has a negative relationship with private label share, and thus private label usage may be one of the cost-saving measure available to consumers who are price-conscious. Last but not least, I also examine the total monthly expenditure of each household, calculated as total value of transactions subtracted the total value of coupon, which gives us a general overview of how much they spend, and whether the treated ones are more frugal:

$$LogExpenditure_{it} = \log(\sum^s TotalValue_{sit} - \sum^s CouponValue_{sit})$$

4 Econometric Models

4.1 State Level Difference-in-Differences

My main identification strategy relies on the plausibly exogenous timing of mathematics curricular reform across different states. In an ideal experiment, we can identify causal treatment effect by randomly assign consumers into different level of mathematics education during childhood, and then their later life results would be observed over the following decades. This, however, is obviously unrealistic and infeasible to carry out. Alternatively, if I had knowledge of mathematical ability of each consumer in our panel, an Instrumental Variable strategy could be employed to causally identify the effect, perhaps using the curricular reform or local school district curriculum as an instrument. Unfortunately, I do not have access to such variable, thus in this paper I instead employ a Difference-in-Differences approach, examining the change in the differences between states with and without curricular reform for cohorts before and after the reforms. It should be noted that this set up differs slightly from a traditional Difference-in-Differences setting, with the two indices being Unit (i.e., States) and Cohort, i.e., the year of high school graduation, instead of Unit and Time, however the main assumptions and identification are still the same. The basic OLS regression equation has the form:

$$Y_{scy} = \beta Treat_s \times Post_{sc} + \gamma_s + \gamma_c + \gamma_{ys} + \varepsilon_{scy} \quad (1)$$

In the above equation Y_{scy} stands for the average outcome of interest (% Purchases made on sale, Coupon utilization, Private Label share, and Total Monthly Expenditure) of cohort c at state s in panel year y , weighted by the household projection factors provided by Nielsen. $Treat_s$ is an indicator variable of whether state s passed a mathematics curricular reform, and $Post_{sc}$ is an indicator of whether cohort c graduates from high school after the reform has passed at state s . Thus, coefficient β capture the main effect of interest, that is, how the mathematical curricular reform affects the outcomes of interest. Next, γ_s , γ_c , γ_{ys} are sets of State, Cohort, and Panel Year-by-State fixed effects respectively. The State fixed effects account for time invariant differences between states, such as their cultural and geographical characteristics that may affect cost-saving behaviors of consumers. The Cohort fixed effects account for cohort-specific unobserved factors, such as generational differences in taste, age difference and so on. For example, previous research has shown that younger generations are more open to private labeled products than older ones, being more familiar with high end private labeled products from retailers such as Whole Food or Trader Joe's. Lastly, Panel Year-by-State fixed effects account for time varying unobserved factors at state level, such as changing economic conditions, retailers entry and exit and so on. Additionally, following [Allegretto et al. \(2017\)](#), I also estimate an alternative specification of (1) with state-specific linear trends:

$$Y_{scy} = \beta Treat_s \times Post_{sc} + \sum_{i \in S} \alpha_i (\mathbf{1}\{s = i\} \times c) + \gamma_s + \gamma_c + \gamma_{ys} + \varepsilon_{scy} \quad (2)$$

The term $\sum_{i \in S} \alpha_i (\mathbf{1}\{s = i\} \times c)$ accounts for a state-specific factor that increases linearly with cohort number c (which is treated as a continuous variable in this case). This additional term can correct for some of the variation in cohort trends between states. This can include the differences in demographic shift, some states have a faster growing minority population than others, or differences in taste shift, as younger generations in more urban states may see more drastic changes in consumption behavior than in more rural states.

4.2 Staggered Treatment Robust Event Study Design

Recent econometric literature ([Goodman-Bacon 2021](#), [Imai and Kim 2021](#) and more) has demonstrated that in a staggered treatment roll-out setting, where different units are treated at different times, the classical two-way fixed effects model may lead to biased estimation of the treatment effect. This bias comes from the fact that when some units are treated before the others, the units treated in later periods are compared not only with the non-treated units, but also with already treated ones. Therefore, if the average treatment

effect is heterogeneous for different “treatment waves”, the estimated average treatment effect will be biased, and the direction of the bias depends on the different share of early and late treated units. This presents a real threat to identification in this case, as the math reforms were carried out on different years depend on the state, and the difference in pre-reform baseline math requirements as well as socio-demographic differences mean heterogeneity in treatment effect is almost unavoidable.

Several corrections have been proposed in recent literature to address this identification challenge, most notable of all are those of [De Chaisemartin and d'Haultfoeuille \(2020\)](#), [Callaway and Sant'Anna \(2021\)](#) and [Sun and Abraham \(2021\)](#). A common theme between these alternative estimators is that the Average Treatment on the Treated (ATT) estimation should be some form of aggregation of individual Treatment Wave-by-Period estimators, with the main difference being how to estimate those individual effects. In this paper, I follow the method proposed in [Sun and Abraham \(2021\)](#) for two reasons: First, this approach is straightforward to interpret, as it is an extension of classical “event study” or “dynamic Difference-in-Differences” design, and based off a simple OLS regression (in comparison for more semi- and non-parametric estimators in the other two approaches). Secondly, [Sun and Abraham \(2021\)](#) estimator can be readily applied to unbalanced panel settings, similar to this context, as there are gaps in high school graduation cohorts in smaller states where there are not enough households.

The Staggered Difference in Differences event study-based approach first starts with a modified event study regression:

$$Y_{scy} = \sum_g \sum_{k \in \{-T, T\}, k \neq -1} \delta_{gk} \mathbf{1}\{c - C_s = k\} \mathbf{1}\{C_s = g\} + \gamma_s + \gamma_c + \gamma_{sy} + \varepsilon_{scy} \quad (3)$$

In the above equation, $\mathbf{1}\{c - C_s = k\}$ is an indicator of whether the “relative treatment period”, that is, how many cohorts since the first treated one, is k . This is interacted with $\mathbf{1}\{C_s = g\}$, which is the “treatment wave” indicator of whether the first treated cohort at state s is g (which is always zero for never-treated states). Thus, δ_{gk} represents the treatment effect of observations in states where the first treated cohort is g , for cohort k -th after (or before if k is negative) the reform. Finally, $\gamma_s, \gamma_c, \gamma_{sy}$ are the same sets of fixed effects as in equation (1). Comparing this to the classical event study design:

$$Y_{scy} = \sum_{k \in \{-T, T\}, k \neq -1} \beta_k \mathbf{1}\{c - C_s = k\} + \gamma_s + \gamma_c + \gamma_{sy} + \varepsilon_{scy} \quad (4)$$

We can see that what the modified equation does is breaking the relative period-specific effects (often called “dynamic treatment effects”) down to each treatment wave through the

interaction term. Once we obtain the treatment wave-specific dynamic treatment effects δ_{gk} , the next step is to estimate the weights of each estimated coefficient, which are treatment wave shares among treatment wave that experience at least k cohorts since the first treated one. Specifically, let $N_g := \sum_s \mathbf{1}\{C_s = g\}$ be the number of states in the reform wave g . Additionally, let h_k be the set of reform waves that have at least k cohorts. Then, the corresponding weight of the estimator can be calculated as:

$$w_{gk} = \frac{N_g}{\sum_{j \in h_k} N_j}$$

With these weights in hand, the final step is simple aggregating the individual $\hat{\delta}_{gk}$ estimates obtained with (3) using the weighted average formula to achieve the relative period-specific effects as in the classical event study design:

$$\hat{\beta}_k = \sum_{g \in h_k} w_{gk} \hat{\delta}_{gk} \quad (5)$$

The total average treatment effect can then be estimated as the weighted average of $\hat{\beta}_k$:

$$ATT = \frac{\sum_{j \in h_k} N_j}{\sum_{k \in \{-T, T\}, k \neq -1} \sum_{j \in h_k} N_j} \hat{\beta}_k$$

4.3 Household Level Regression & Treatment Heterogeneity

Aside from the state-level aggregated regressions discussed above, I also estimate the Difference-in-Differences model with household level monthly data:

$$Y_{itsc} = \beta Treat_s \times Post_{sc} + \mathbf{X}'_{it} \Gamma + \gamma_t + \gamma_s + \gamma_c + \gamma_{sy} + \varepsilon_{itsc} \quad (6)$$

This household level model allow me to further controls for time varying covariates \mathbf{X}_{it} such as current age, education of the head of household, income, wealth, race, household size, marriage status, as well as state level covariates, including unemployment rate, population and GDP growth. The additional set of Month fixed effects γ_t control for monthly shocks such as seasonality, and the sets $\gamma_s, \gamma_c, \gamma_{sy}$ play the same roles as in previous model. I estimate two versions of equation (6), one with and one without the projection factors provided by Nielsen. It is worth noting that, even though the household level regressions allow richer controls and can possibly eliminate some biases in the estimation, not all households in treated states and cohorts are affected by the curricular reforms. Since the legislation can only control the *minimum* number of math classes needed for

graduation, some panelists may have gone to high schools in districts with the number of math courses already higher than the required minimum, thus not subjected to treatment. Because of this, the magnitude of effects estimated may be biased downward, and we should not interpret the results as causal effect of the reform on each specific household.

Another advantage afforded by the household level model is the ability to decompose the treatment effect heterogeneity, by a Triple Differences model, interacting the $Treat_s \times Post_{sc}$ interaction term with a demographic variable, to see how the effects vary across consumer groups. Specifically, the regression equation is as follow:

$$Y_{itsc} = \beta_1 Treat_s \times Post_{sc} + \beta_2 Treat_s \times Post_{sc} \times Demographic_{it} \\ + \mathbf{X}'_{it} \boldsymbol{\Gamma} + \gamma_t + \gamma_s + \gamma_c + \gamma_{sy} + \varepsilon_{itsc}$$

From the data, some variables that can potentially explain the heterogeneity in treatment effects are: race, age group, marriage status, education household income. One may concern that these variables are also causally affected by the treatment, for example, better math education may lead to higher income and overall education attainment, thus biases the results if we control for these variables. However, due to stratified random sampling nature of Nielsen household panel, which randomly sample households based on those socio-demographic factors, this should not be a substantial problem. Additionally, by using the projection factors as weights for the above Triple Differences regression, I ensure the balancedness of the demographic factors thus further remove potential indirect effects of the curricular reforms.

5 Results

5.1 Descriptive Statistics

The descriptive statistics of Nielsen household panel is summarized in Table 1, broken down by Treated and Control states. As we can see from the table, the control variables are largely similar between the two conditions. Both groups have an average age of around 51, average year of birth are both 1960 (treatment first started for cohort born 1966), roughly similar education, income, household size, or marital status. The main difference here is between the share of minority households, with the treated states having higher number of minority households (19.71% vs. 12.33%). This can be addressed through the Projection Factor provided by Nielsen, and I also rerun the analysis through a restricted sample of matched states through their demographic similarities and geographical closeness as a robustness check.

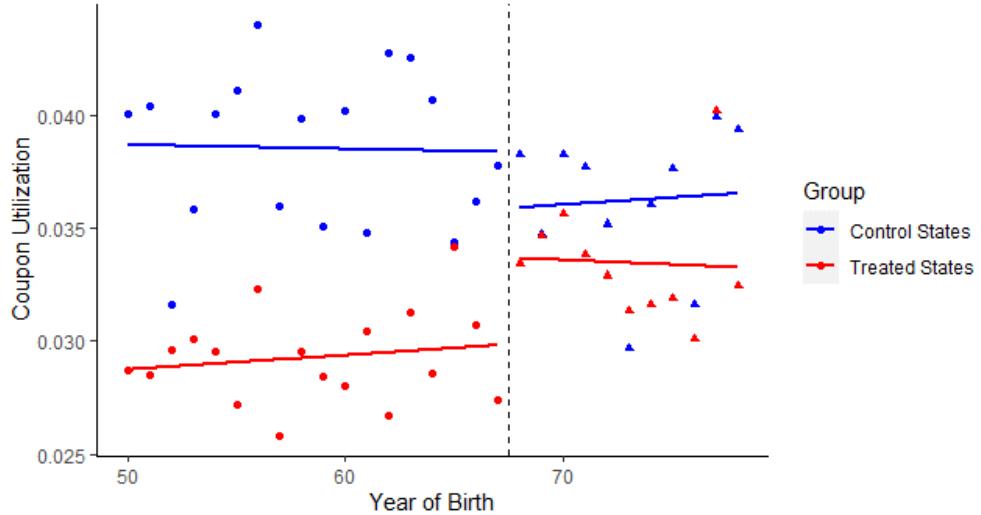


Figure 2: Model Free Evidence

Table 1: Descriptive Statistics

Statistic	N	Control		N	Treat	
		Mean	Median		Mean	Median
Age	381,105	51.408	52	1,087,745	50.985	52
Cohort YOB	381,105	60.472	59	1,087,745	60.914	60
% Minorities	381,105	12.33%		1,087,745	19.71%	
Female Head Education	381,105	3.849	4	1,087,745	3.758	4
Male Head Education	381,105	3.158	4	1,087,745	3.161	4
HH Income Level	381,105	21.000	23	1,087,745	20.517	21
HH Size	381,105	2.503	2	1,087,745	2.491	2
Marital Status	381,105	1.860	1	1,087,745	1.824	1

Next, I also check the graphical evidence of the parallel trend assumption, as well as a model free look as potential treatment effect. Figure 1 above, and additional figures in the Appendix show the plots of the average outcome variables for each cohort of the treatment and control groups, along with the linear spline models, break at cohort born 1966, when the first state started to roll out reform. From the plot of Coupon Utilization Rate, the parallel trend assumption appears to be satisfied, and the treatment group witnesses a rapid rise in coupon utilization rate for cohorts post treatment. As for other outcomes, the model free plots also show evidence of parallel pre-treatment trends between treated and control groups, however while there appear to be post-treatment gaps (indicator of an effect), the changes are not as clear as in Coupon Utilization. Naturally, we have to interpret these plots with caution, as the differential timing of treatment and differences

between states and across cohorts are not yet taken into account, but overall it appears that the Difference-in-Differences strategy is a valid one in this setting.

5.2 Main Results

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
A. Basic Difference-in-Differences				
Treat × Post	0.0121** (0.0047)	0.0074*** (0.0010)	0.0013 (0.0023)	-0.0083 (0.0143)
B. Sun and Abraham (2021) aggregated ATT				
ATT	0.0254** (0.0102)	0.0077*** (0.0021)	0.0012 (0.0037)	-0.1677*** (0.0301)
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	17,478	17,478	17,478	17,478
Clustered Robust Standard Errors in parentheses				
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1				

Table 2: State-Year Level Main Results

Table 2 presents the main state level regression results, both of the naive Difference-in-Differences regressions, in Panel A, and of the [Sun and Abraham \(2021\)](#)'s staggered adoption robust models in Panel B, with four main outcomes of interest: (1) Purchase-on-sale Rate, (2) Coupon Utilization Rate, (3) Private Label Share, and (4) log-transformed Monthly Expenditure. We can see that the classical Difference-in-Differences results indicate statistically significant and positive effects of mathematical curricular reforms on Purchase-on-sale and Coupon Utilization Rates. Specifically, from column (1), the reforms lead to an average 1.21 percentage points increase in the share of purchases made on sale. This is approximately a 5.2% increase over the average Purchase-on-sale rate of 23% of the whole sample. As for the Coupon Utilization Rate in column (2), the naive DiD result indicates a 0.74 percentage point effect. While this may appear modest at first glance, given the average coupon usage rate of just 2.8% in the whole sample, this represents a considerable 26.2% increase over the average. On the other hand, from column (3) and (4), we see that the estimated effects on Private Label Share and on (log-transformed) Monthly

Expenditure are negligible and cannot be accurately estimated.

As discussed above, however, these results may be subjected to biases due to the staggered adoption nature of the curricular reforms. To address this, in Panel B of **Table 2**, I present the aggregated Average Treatment effects on the Treated of [Sun and Abraham \(2021\)](#)'s staggered adoption robust design, using the procedure from **Section 4.2**. Furthermore, the effects aggregated by relative periods (i.e., number of cohorts since the first treated ones in each state) are illustrated in event study plots in Figure 3. Additionally, in the Appendix, I also report the treatment effects broken down by each treatment wave to illustrate the treatment heterogeneity that may have caused the differences.

From column (1) in Panel B, we see that after accounting for staggered adoption, the estimated effect of mathematical curricular reforms on Purchase-on-sale rate is more than doubled, from 1.21 to 2.54 percentage points. Taking a closer look at the effect-by-treatment wave estimates in the Appendix, the downward bias in the naive model, in comparison to the robust estimate, appears to be driven by a large negative estimate (-16.68 percentage points) in the 1986 Treatment Wave, which consists of only the state of Nevada, which can be idiosyncratic due to its small, rapidly changing, and highly transient population. On the other hand, the results for Coupon Utilization Rate and Private Label Share are very similar to what we observed with the naive model, indicating largely homogeneous effects between treatment waves, as confirmed by the treatment wave-specific results.

Perhaps the most remarkable divergence between the naive DiD results and the event study results is that between the estimates of curricular reforms effect on log-transformed Monthly Expenditure. While the naive DiD estimate in Panel A suggests a negligible effect on Month Expenditure, after applying correction for staggered adoption, the result in Panel B indicates a statistically and numerically significant relationship. Specifically, the curricular reforms lead to a 15.4 percentage point reduction in monthly packaged goods expenditure, an equivalence of a US\$ 62 decrease per month out of an approximately US\$ 400 mean monthly expenditure. Similar to the Purchase-on-sale rate case, the bias in the naive DiD estimate appears to be driven mostly by idiosyncratic outlier, here it is the last treatment wave of Class of 1990, which consists of only the state of New Mexico¹. This further illustrates the importance of accounting for staggered adoption in evaluating the effect of policies with a Difference-in-Differences design.

Figure 3 plots the treatment effects estimated using [Sun and Abraham \(2021\)](#)'s approach, aggregated by number of cohorts since first treated ones. The event study plots map out the "dynamic treatment effects", or "treatment paths", of curricular reform on each outcome

¹Center for Urban Research at the City University of New York and US Census put New Mexico as the most difficult state to get reliable data from. Source: [UNM Newsroom](#).

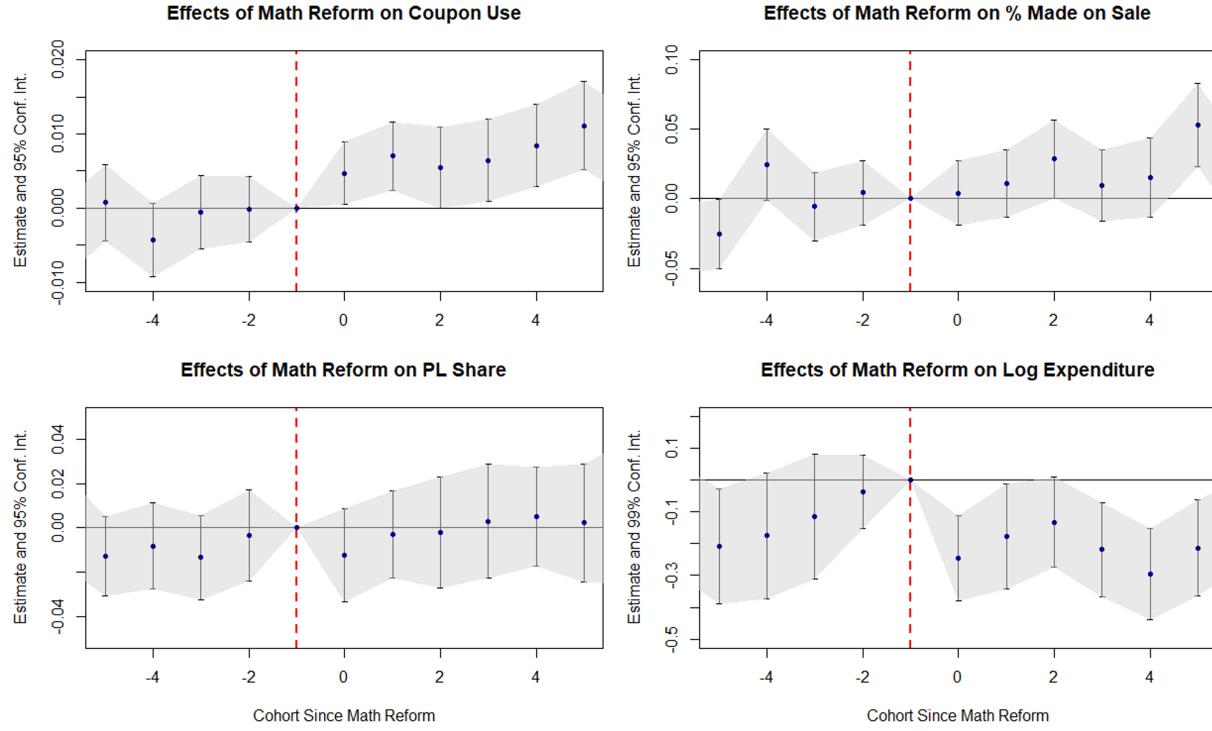


Figure 3: Sun and Abraham (2021) Staggered Treatment Robust Event Study Estimates

variable. From Figure 3, except for Private Label Share, the general trend appears to be an increase in effect magnitude the further away a cohort is from the first treated one. This is an expected observation, as policies, especially education policies, often take years before they can be implemented in full. The event study plots also serve as visual evidence of the parallel trends assumption, an important assumption for interpreting the Difference-in-Differences design results as valid causal quantities. As illustrated in the plots, the effects of pre-treatment cohorts are not statistically different from zero, thus there are no diverging or converging trends between the treated and control states in the pre-treatment periods.

Turning to household level models, the results are reported in Table 3 below. In both weighted and unweighted versions of the household-level regression, we observe qualitatively similar effects to the results of state-level models discussed above, with positive and statistically significant effects of math curricular reform on Purchase-on-sale and Coupon Utilization rates, and negative and significant effect on Monthly Expenditure. Additionally, the effects on Private Label Share is only marginally significant and quantitatively minuscule, in comparison to the average Private Label Share, so it is also similar to the previous results. In the household level estimations, the magnitudes of the effects are

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
A. Unweighted				
Treat × Post	0.0108** (0.0044)	0.0054*** (0.0009)	-0.0029* (0.0017)	-0.0539*** (0.0105)
B. Weighted				
Treat × Post	0.0090*** (0.0018)	0.0028*** (0.0004)	0.0014* (0.0008)	-0.0468*** (0.0059)
Control Variables	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Month-Year FEs	Yes	Yes	Yes	Yes
Observations	1,468,850	1,468,844	1,468,844	1,468,844

Clustered robust standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3: Household Level Fixed Effects Results

mostly smaller, with the effect on Purchase-on-sale ranging from 0.9 to 1.08 percentage points, on Coupon Utilization from 0.28 to 0.54 percentage point (this is however still an increase of 10-20% over the average rate), and on Monthly Expenditure from 4.8% to 5.8%, likely due to some variation being absorbed by the wide range of control variables and more granular fixed effects.

Overall, from the main results, we can conclude that the mathematical curricular reforms lead to strong and persistent effects on their overall monthly packaged goods expenditure, even after controlling for demographic factors such as income, education, geographical location and so on, indicating an impact on overall saving tendency. This effect is the strongest in long term saving action of coupon usage, which is not surprising, as mathematical literacy is an important factor in determining planning ability. As for short-term saving action, I also observe significant effects by the curricular reforms on the share of purchases made on sale, however this is less precisely estimated, perhaps due to a larger number of potential confounders. The effect on Private Label Share, however, is muted, so the increase in math literacy may not have an impact on this outcome.

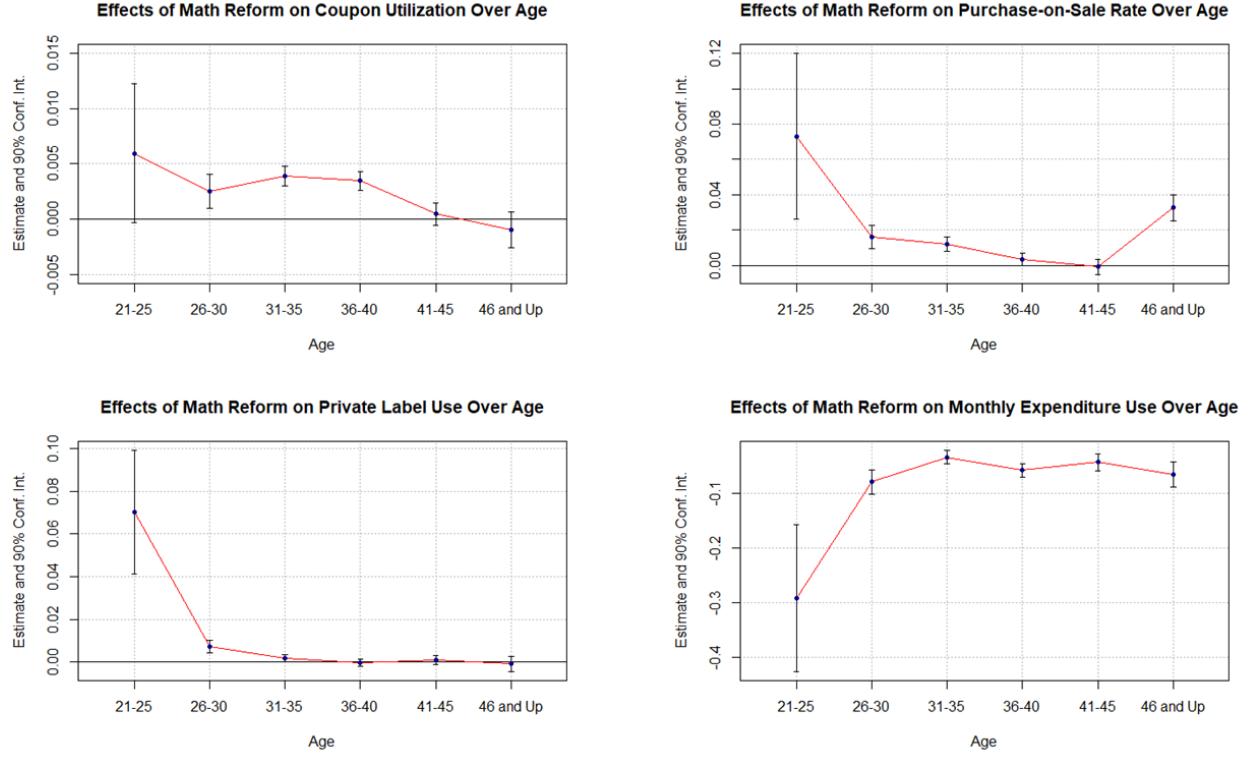


Figure 4: Treatment Effect Heterogeneity by Age

5.3 Treatment Heterogeneity

Finally, I examine the treatment effect heterogeneity across several variables through the Triple Differences model, as discussed above. These heterogeneities are illustrated through a series of plots as presented here and in the section 9.3 of the Appendix, along with the detailed regression tables. In this section, I will discuss the most interesting findings.

First of all, we can see in Figure 4 that the treatment effect appears to be attenuated with age across all outcomes. The effects are strong around early to middle adulthood, and gradually decline toward zero as around 45. This overall trend can be explained through the lens of “neural plasticity”, a framework in neuroscience which postulates that the way our brain functions is significantly shaped through experience during childhood and early adulthood, and gradually change over time as we get older. This means the saving tendencies and abilities from the increased mathematical literacy can be gradually replaced with age, or perhaps as not-treated consumers gained additional experience, their saving behavior catch up with the early boost from math education. Another explanation of this decreasing trend is that as consumers get older, their need for consumption-related saving

decrease, as they gain access to other channels of saving such as investment account, real estate, and retirement planning.

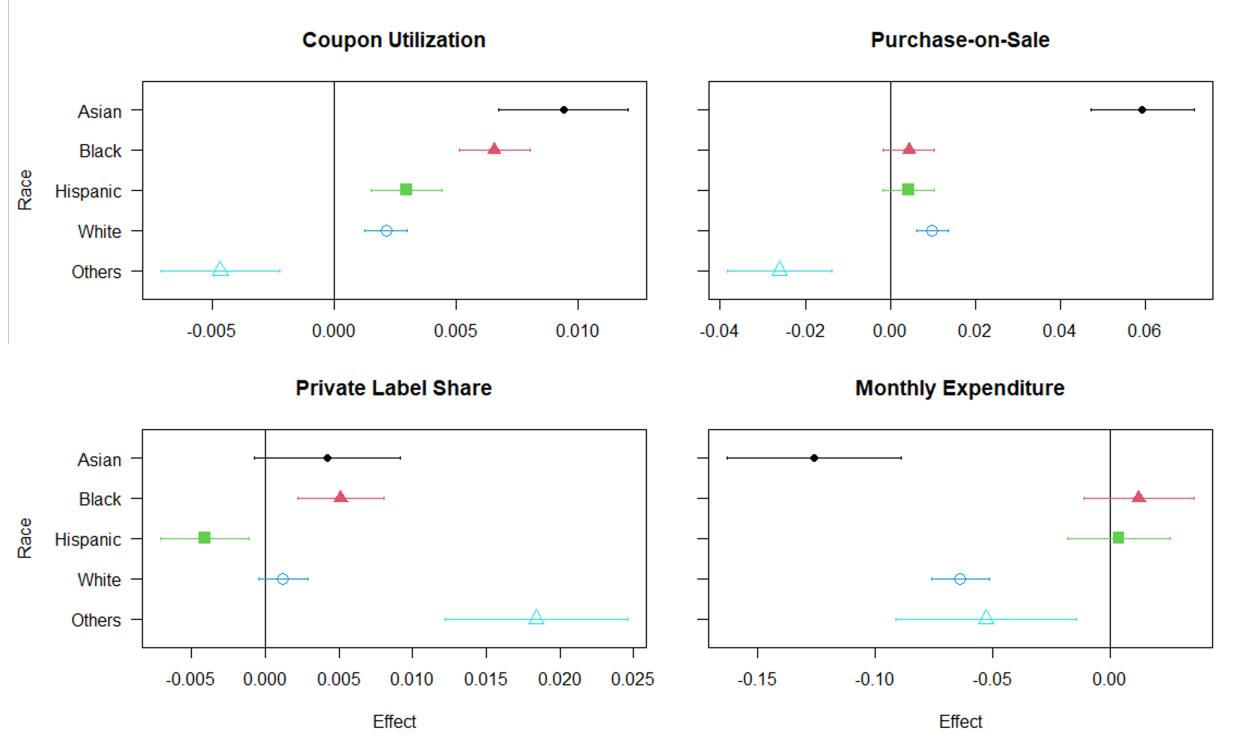


Figure 5: Treatment Effect Heterogeneity by Race

Next, looking at socio-demographic factors, I find that there are strong heterogeneities in treatment effects across races. The effect of math curricular reforms on Coupon Utilization is much stronger for minorities such as Asian or Black in comparison to White households. Additionally, the effect on Private Label Share, which is muted overall, also appears to be stronger in Asian and Black. These are in line with Goodman (2019)'s finding that the curricular reforms have stronger impact on minorities, as these tend to reside in school districts with lower or non-existent math requirements before the reforms. As for Purchase-on-sale and total Expenditure, however, while the effects remain highly substantial for Asian and notable for White households, they are negligible for Black and Hispanic ones. This may be due to differences in shopping habits or in access to promotional offers².

Another interesting result is that the effects on Coupon Utilization, Purchase-on-sale rate, and Monthly Expenditure appears to be strongest for the middle-class income house-

²Regressing the Purchase-on-sale rate on Race, with the same set of fixed effects and control variables and only *untreated* observations, shows that Black households on average have 32.36% lower rate than White households, while Asian households have 39% higher rate, pointing to a pre-existing disparity.

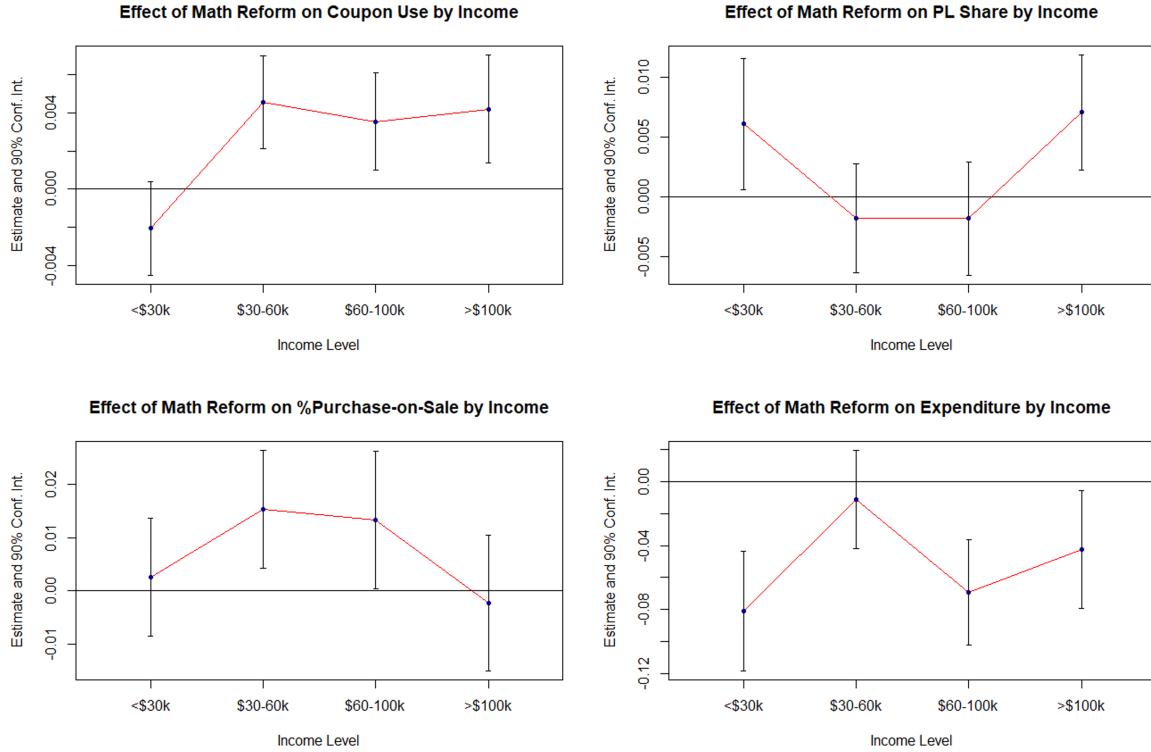


Figure 6: Treatment Effect Heterogeneity by Income Level

holds (household income of around \$60-100,000/year), against a natural assumption that the effects would be stronger for lower income households. It is possible that with lower income households even the untreated ones already utilize several cost-saving measures, so the incremental effect of mathematical literacy is not as substantial.

6 Robustness Checks

In order to ensure the validity of the results discussed in previous sections, I perform several robustness checks. First, even though the event study plots in Figure 3 demonstrate evidence of parallel trends, recent literature has pointed out that visually examining the plots alone may not be sufficient. In order to test the robustness of this assumption, as well as whether the treatment effects follow parallel paths post treatment, and ensuring the estimates are not spurious, I carry out two Placebo tests. The Placebo tests are done by shifting the treatment indicators forward and backward five relative periods, essentially testing if the observed results are not due to some other unobserved “placebo” treatments. Section 9.5 in the Appendix offers further detail of the procedure. The null results in tables 17 and 18 provide further evidence of the satisfaction of the parallel trends assumption, as well as attenuating the concern about other unobserved shocks in pre or post treatment

periods.

Another concern about the validity of the results is the potential spatial heterogeneity between states that decided to adopt the reform at different times, or not reform at all. I test this using two methods. First of all, following Allegretto et al. (2017), I allow for state level parametric time trend by adding an interaction term between state dummies and the (continuous) cohort variable. From Table 4 in the Appendix, we can see that the results are largely qualitatively similar in terms of direction and magnitude, and even though the Private Label effect becomes statistically significant, its overall magnitude is still small (0.9 percentage point increase on a 15% average Private Label Share). It could be the case that the differences in inter-cohort dynamic of Private Label preference lead to large variation and thus the null results in the main analysis, so when we control for the trends, the effect becomes more identifiable.

I further account for spatial correlation between states through the Generalized Synthetic Control method (Xu 2017). This is a generalization of the latent factor model with two way fixed effects, the main motivating model behind Synthetic Control. This adds a latent factor structure, or *interactive fixed effects* (Bai 2009) to the model error, which accounts for unobserved time varying heterogeneities. The Generalized Synthetic Control method calculates the individual factor loading (i.e., unobserved characteristics of the state) by eigen-decomposition of the squared residuals matrix in pre-treatment period, then use the full set of data to calculate the time varying factors (i.e., time varying effects of the said characteristics). Essentially, the model is as follow:

$$Y_{scy} = \beta Treat_s \times Post_{sc} + \sum_{n=1} f_c^n \lambda_s^n + \gamma_s + \gamma_c + \gamma_{ys} + \varepsilon_{scy} \quad (7)$$

With $\sum_{n=1} f_s^n \lambda_c^n$ being the interaction between some unobserved characteristics of the state λ_s^n and its time varying effect f_c^n . This could be some regional characteristics that differs by cohort, which will absorb potential spatial correlation. I manually force the model to admit at least one additional factor (a model with no additional factor is essentially the same as our naive DiD). As we can see in Table 4, the results are largely consistent with the main estimates, with statistically significant results for Purchase-on-sale rate and Coupon Utilization, and here the effect on Month Expenditure is also marginally significant, unlike in the naive DiD results. In section 9.4. in the Appendix, I present further robustness checks, include: (1) restricting the sample to a short period of 15 cohorts during the reform time, to further reduce the possibility of spurious results; (2) limiting the sample to only eventually treated states, making use of only the variation in the timing of the reform, to address the concern that never treated states may be inherently different

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
ATT	0.0151*** (0.0054)	0.0071*** (0.0009)	-0.0002 (0.0022)	-0.0288* (0.0153)
State-Year FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Interactive Factor	1	1	1	1
Observations	17,410	17,410	17,410	17,410

Block-Bootstrapped Robust Standard Errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 4: Generalized Synthetic Control Results

from the eventually treated one; and (3) a restricted sample with only one-to-one matched states based on their demographic variables and proximity. All of these robustness checks also lead to qualitatively similar results, indicating the robustness of the observed effects.

7 Extension: Response to 2008 Financial Crisis

In addition to the previous discussion on treatment heterogeneity, the wide temporal range of the panel, from 2004 to 2018 also allows us explore the dynamic of the treatment effects over panel years. One interesting question is how the effects change in response to the financial crisis of 2008, and in turn how mathematical literacy affects the ways households change their behavior to adapt to the period of uncertainty and economic downturn post 2008. This is done with a Trip Differences model as follow:

$$Y_{scy} = \beta_1 Treat_s \times Post_c + \beta_2 Treat_s \times Post_c \times Post2008_y + \gamma_s + \gamma_c + \gamma_y + \varepsilon_{scy} \quad (8)$$

Additionally, a household-level Triple Differences model, as well as the event study design as in the main analysis with the additional interaction term are also estimated. Figure 7 illustrates the Panel Year specific treatment effect by interaction the $Treat_s \times Post_c$ term with panel year dummies. As we can see, the effect on Coupon Utilization increases significant after 2008, and the effect on Purchase-on-sale rate also increases from negligible to substantial in the post 2008 period. On the other the hand, Private Label Share and Monthly Expenditure show a downward trend in magnitude. These visual observations are quantitatively confirmed by the regression results presented in Section 9.6 in the Appendix. The difference in treatment effects on Purchase-on-Sale between Pre and Post

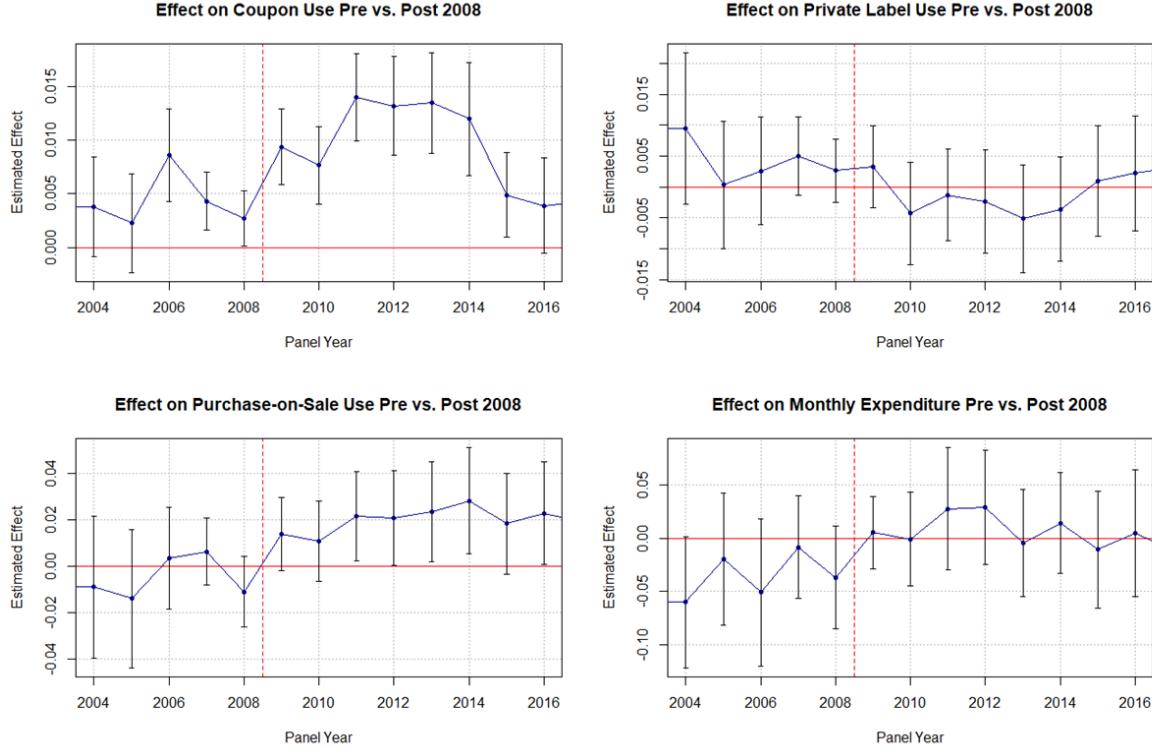


Figure 7: Treatment Effect Heterogeneity by Panel Year

2008 periods is up to 2.3 percentage point, and 0.46 percentage point for Coupon Utilization. Estimates of ATT using [Sun and Abraham \(2021\)](#)'s method separately for pre and post 2008 samples also exhibit similar trends. These differences tell us that households affected by the reforms (e.g. with potentially higher math literacy) increase their consumption-related short and long term saving behavior more than the untreated ones, shedding light on how mathematical literacy can influence the ways consumers respond to economic crisis.

8 Conclusion

To conclude, the results of this paper illustrate how childhood mathematical education can lead to long lasting impacts on future consumption habits. Consumers exposed to math curricular reforms in the 1980s in the wake of “A Nation at Risk” report see a 15% decrease in total monthly packaged goods expenditure, a equivalence of more than \$60 a month, 0.5 - 0.8 percentage point effect on coupon utilization rate, which translates to roughly 25% higher rate than average, and a more modest yet still substantial increase of 5% higher Purchase-on-sale rate. I also found large degree of heterogeneity in treatment effect across socio-demographic groups, with minorities and middle class consumers seeing stronger effects, and furthermore the effect appears to dissipate with age. Last but

not least, I also show that the effects on coupon use and purchase-on-sale intensify after the financial crisis of 2008, evidence of a more responsive reaction to adverse economic condition amongst the treated households.

My findings contribute to a better understanding of both the effect of mathematical education on later life outcomes, as well as the formation process of consumption habits. These results speak to the importance of considering the downstream effect on consumption behavior when evaluating the effect of education reform, as well as give business managers further insights into anticipating changing consumer behavior due to changes in education policies. The insights also help with targeting, with more mathematically savvy consumers may be more responsive to price-related marketing actions. Additionally, this also contributes to the consumer financial education literature by offering another channel that may affect consumer financial decisions other than short term financial literacy programs researched in previous studies.

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9 Appendix

9.1 Additional Illustrations

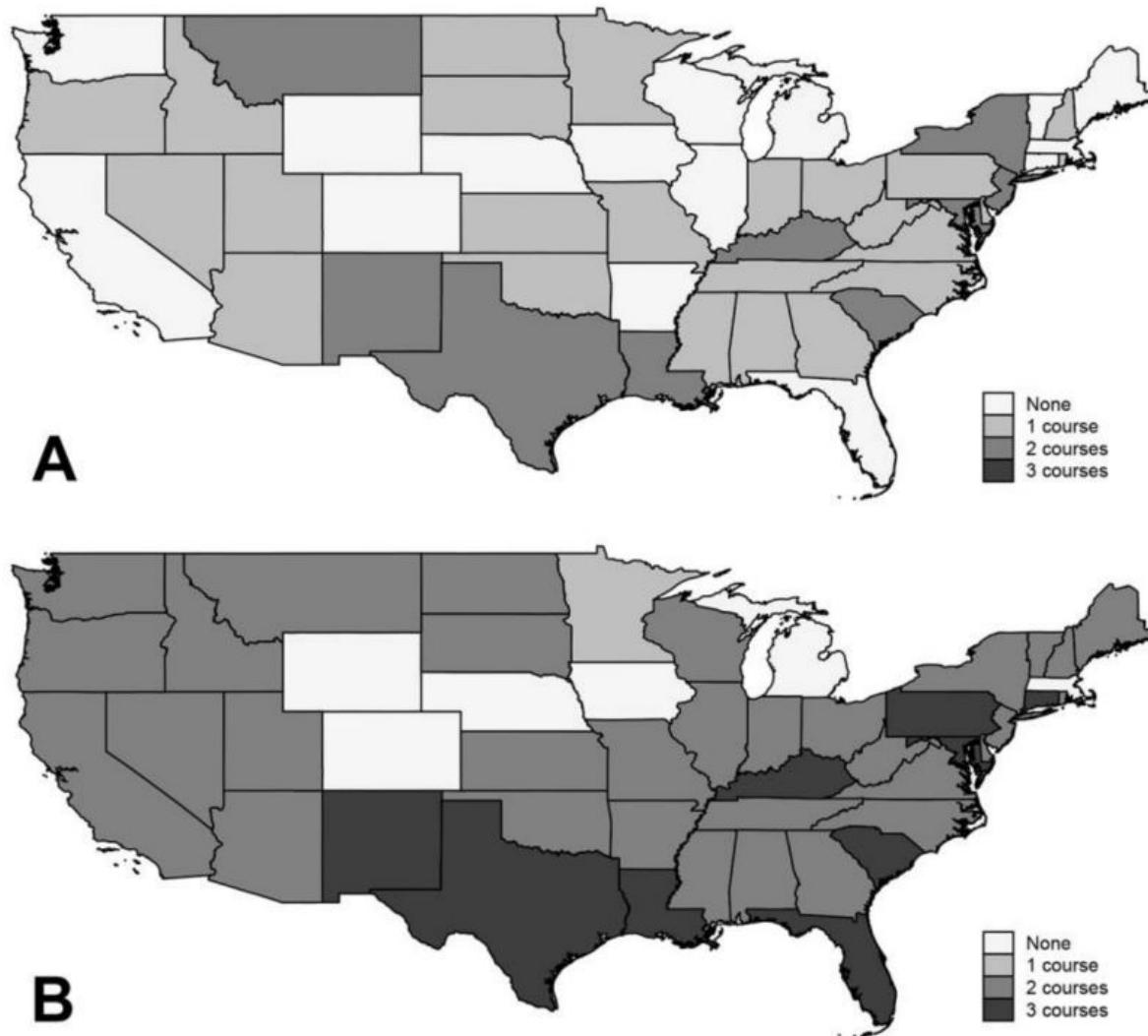


Figure 8: Changes in minimum math requirements Pre vs. Post Reform

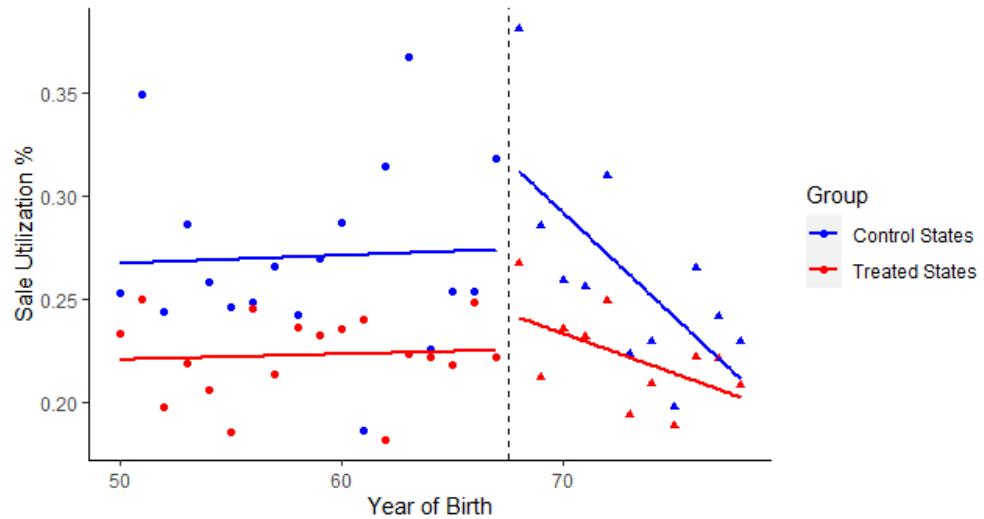


Figure 9: Model Free Evidence: Sale

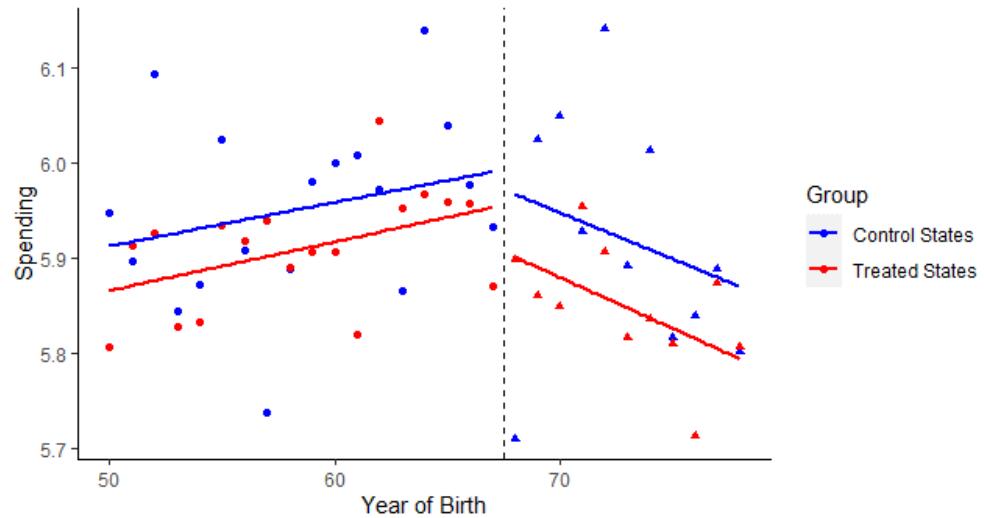


Figure 10: Model Free Evidence: Spending

9.2 Additional Results

Dependent Variables:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Model:				
Treat \times Post	0.0092 (0.0070)	0.0067*** (0.0014)	0.0088*** (0.0031)	-0.0086 (0.0217)
State linear trends	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	17,478	17,478	17,478	17,478

Clustered Robust Standard-Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 5: Aggregated State Level Fixed Effects Model With State-specific Linear Trends

9.3 Treatment Heterogeneity

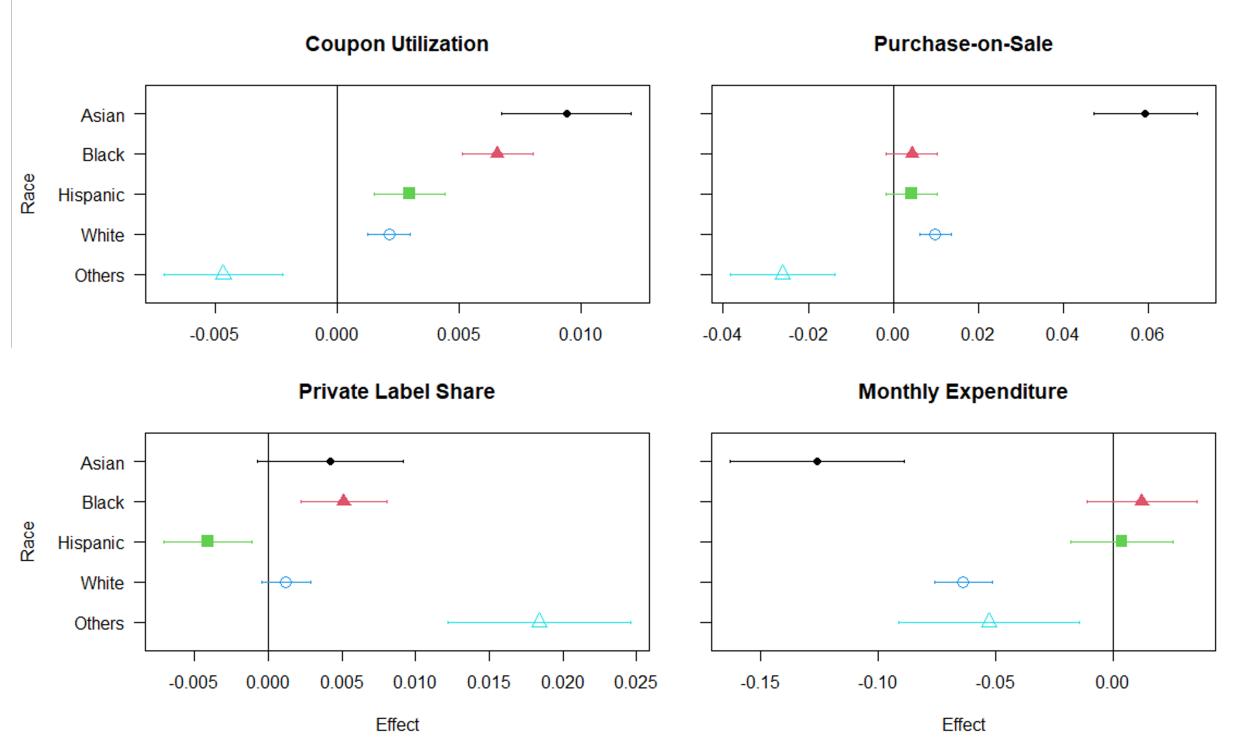


Figure 11: Treatment Effect Heterogeneity by Race

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
<i>Treated Cohort:</i>				
1984	-0.0684 (0.0587)	0.0201*** (0.0051)	0.0270** (0.0115)	0.2568* (0.1523)
1985	0.0654*** (0.0202)	0.0179*** (0.0037)	0.0006 (0.0188)	-0.0413 (0.0875)
1986	-0.1668*** (0.0510)	-0.0311*** (0.0113)	0.0530*** (0.0086)	-0.0809 (0.0839)
1987	-0.0155 (0.0223)	0.0035 (0.0054)	-0.0029 (0.0057)	-0.0287 (0.0682)
1988	0.0779*** (0.0203)	0.0083*** (0.0029)	-0.0067 (0.0069)	-0.3792*** (0.0619)
1989	0.0136 (0.0172)	0.0108** (0.0043)	0.0127* (0.0077)	-0.1453*** (0.0443)
1990	0.0513*** (0.0173)	0.0103* (0.0058)	-0.0599** (0.0263)	0.4050*** (0.1412)
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	17,410	17,410	17,410	17,410

Clustered Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 6: Sun and Abraham (2021)'s ATT-by-cohort Aggregated Results

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treat × Post × White	0.0098*** (0.0019)	0.0021*** (0.0004)	0.0012 (0.0008)	-0.0638*** (0.0062)
Treat × Post × Black	0.0044 (0.0031)	0.0066*** (0.0007)	0.0051*** (0.0015)	0.0120 (0.0119)
Treat × Post × Asian	0.0592*** (0.0062)	0.0094*** (0.0014)	0.0042* (0.0025)	-0.1258*** (0.0189)
Treat × Post × Hispanic	0.0043 (0.0031)	0.0030*** (0.0007)	-0.0041*** (0.0015)	0.0035 (0.0111)
Treat × Post × Other	-0.0261*** (0.0063)	-0.0047*** (0.0012)	0.0184*** (0.0032)	-0.0528*** (0.0196)
Control Variables	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Month-Year FEs	Yes	Yes	Yes	Yes
Observations	1,468,844	1,468,850	1,468,844	1,468,844

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 7: Treatment Heterogeneity by Race

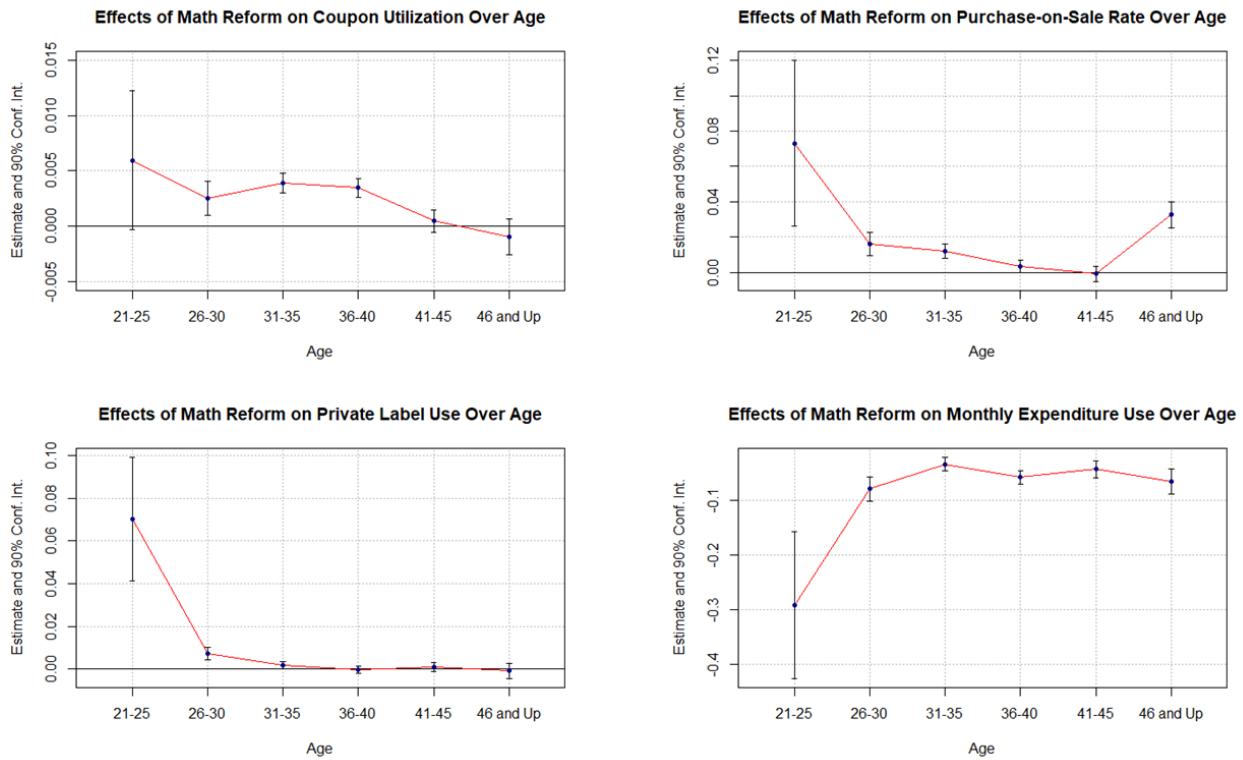


Figure 12: Treatment Effect Heterogeneity by Age

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treat \times Post \times 21-25	0.0731** (0.0285)	0.0060 (0.0038)	0.0703*** (0.0175)	-0.2913*** (0.0817)
Treat \times Post \times 26-30	0.0161*** (0.0040)	0.0025*** (0.0009)	0.0074*** (0.0018)	-0.0786*** (0.0132)
Treat \times Post \times 31-35	0.0121*** (0.0024)	0.0039*** (0.0005)	0.0020* (0.0010)	-0.0332*** (0.0076)
Treat \times Post \times 36-40	0.0035 (0.0022)	0.0035*** (0.0005)	-0.0004 (0.0010)	-0.0568*** (0.0074)
Treat \times Post \times 41-45	-0.0008 (0.0026)	0.0005 (0.0006)	0.0010 (0.0013)	-0.0425*** (0.0094)
Treat \times Post \times ≥ 46	0.0326***	-0.0010	-0.0008	-0.0648***
Control Variables	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Month-Year FEs	Yes	Yes	Yes	Yes
Observations	1,468,844	1,468,850	1,468,844	1,468,844

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 8: Treatment Heterogeneity by Age

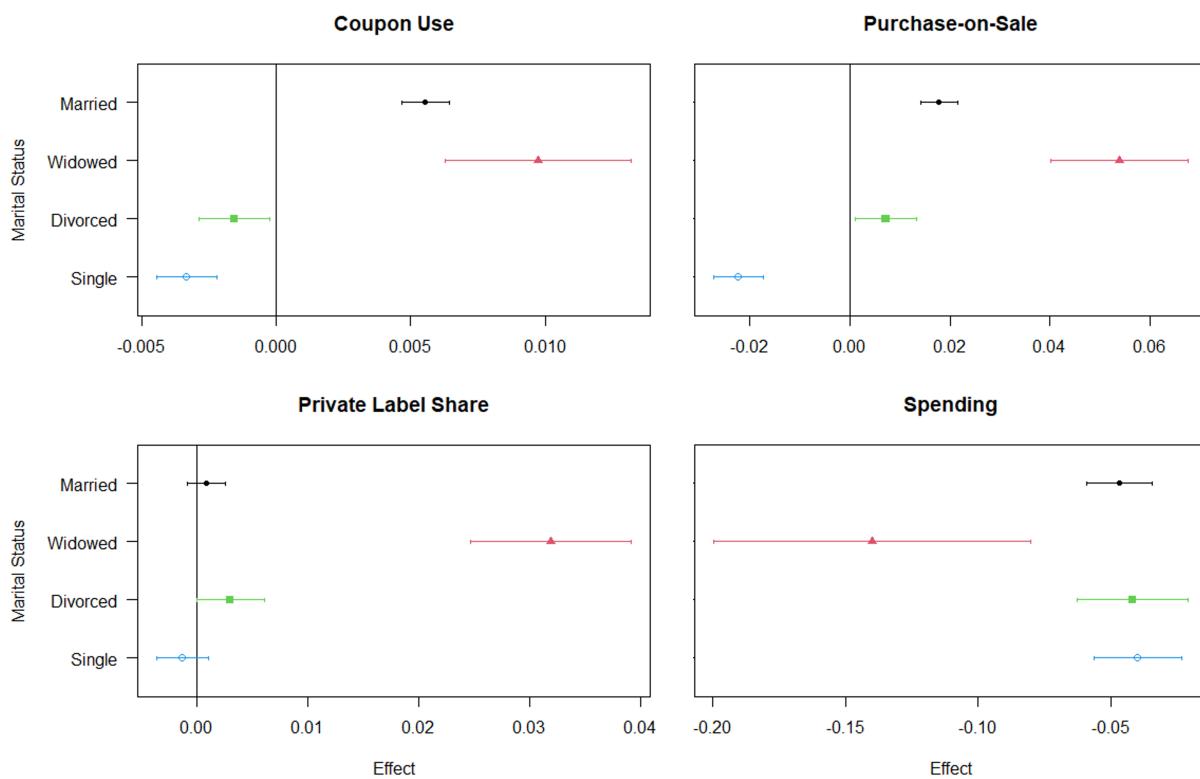


Figure 13: Treatment Effect Heterogeneity by Marital Status

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treat \times Post \times Married	0.0178*** (0.0019)	0.0055*** (0.0004)	0.0009 (0.0009)	-0.0467*** (0.0063)
Treat \times Post \times Widowed	0.0539*** (0.0070)	0.0098*** (0.0018)	0.0319*** (0.0037)	-0.1399*** (0.0305)
Treat \times Post \times Divorced	0.0072** (0.0031)	-0.0016** (0.0007)	0.0030* (0.0016)	-0.0417*** (0.0106)
Treat \times Post \times Single	-0.0223*** (0.0025)	-0.0033*** (0.0006)	-0.0013 (0.0012)	-0.0399*** (0.0085)
Control Variables	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Month-Year FEs	Yes	Yes	Yes	Yes
Observations	1,468,844	1,468,850	1,468,844	1,468,844

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 9: Treatment Heterogeneity by Marital Status

9.4 Robustness Check

9.4.1 Matched Sample

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
ATT	0.0296** (0.0128)	0.0131*** (0.0021)	0.0101* (0.0056)	-0.1681*** (0.0369)
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	8,543	8,543	8,543	8,543

Clustered Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 10: Robustness Check: ATT of State Matched Sample

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treat × Post	0.0086*** (0.0016)	0.0049*** (0.0004)	0.0005 (0.0007)	-0.0736*** (0.0050)
Control Variables	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Month-Year FEs	Yes	Yes	Yes	Yes
Observations	865,321	865,316	865,316	865,316

Clustered robust standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 11: Robustness Check: Matched States Sample

9.4.2 Restricted Sample Cohorts Born between 1960 and 1975

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
ATT	0.0231** (0.0106)	0.0072*** (0.0022)	-0.0003 (0.0039)	-0.1937*** (0.0296)
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	9,084	9,084	9,084	9,084

Clustered Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 12: Robustness Check: ATT of Restricted Sample of Cohorts Born between 1960 and 1975

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treat \times Post	0.0084* (0.0044)	0.0032*** (0.0009)	0.0002 (0.0018)	-0.0793*** (0.0112)
Control Variables	Yes	Yes	Yes	Yes
State-Year FEes	Yes	Yes	Yes	Yes
State FEes	Yes	Yes	Yes	Yes
Cohort FEes	Yes	Yes	Yes	Yes
Month-Year FEes	Yes	Yes	Yes	Yes
Observations	676,643	676,639	676,639	676,639

Clustered robust standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 13: Robustness Check: Restricted Sample of Cohorts Born between 1960 and 1975

9.4.3 Restricted Sample of Eventually Treated States Only

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
ATT	0.0295* (0.0170)	0.0091** (0.0044)	0.0200*** (0.0074)	-0.4177*** (0.0501)
State-Year FEes	Yes	Yes	Yes	Yes
State FEes	Yes	Yes	Yes	Yes
Cohort FEes	Yes	Yes	Yes	Yes
Year FEes	Yes	Yes	Yes	Yes
Observations	13,673	13,673	13,673	13,673

Clustered Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 14: Robustness Check: ATT of Restricted Sample of Eventually Treated States Only (Last Treatment Wave is the baseline)

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treat \times Post	0.0100*** (0.0021)	0.0019*** (0.0005)	-0.0028*** (0.0010)	-0.0574*** (0.0072)
Control Variables	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Month-Year FEs	Yes	Yes	Yes	Yes
Observations	1,087,745	1,087,741	1,087,741	1,087,741

Clustered robust standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 15: Robustness Check: Restricted Sample of Eventually Treated States Only

9.4.4 Generalized Synthetic Control

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
ATT	0.0151*** (0.0054)	0.0071*** (0.0009)	-0.0002 (0.0022)	-0.0288* (0.0153)
State-Year FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Interactive Factor	1	1	1	1
Observations	17,410	17,410	17,410	17,410

Block-Bootstrapped Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 16: Generalized Synthetic Control Results

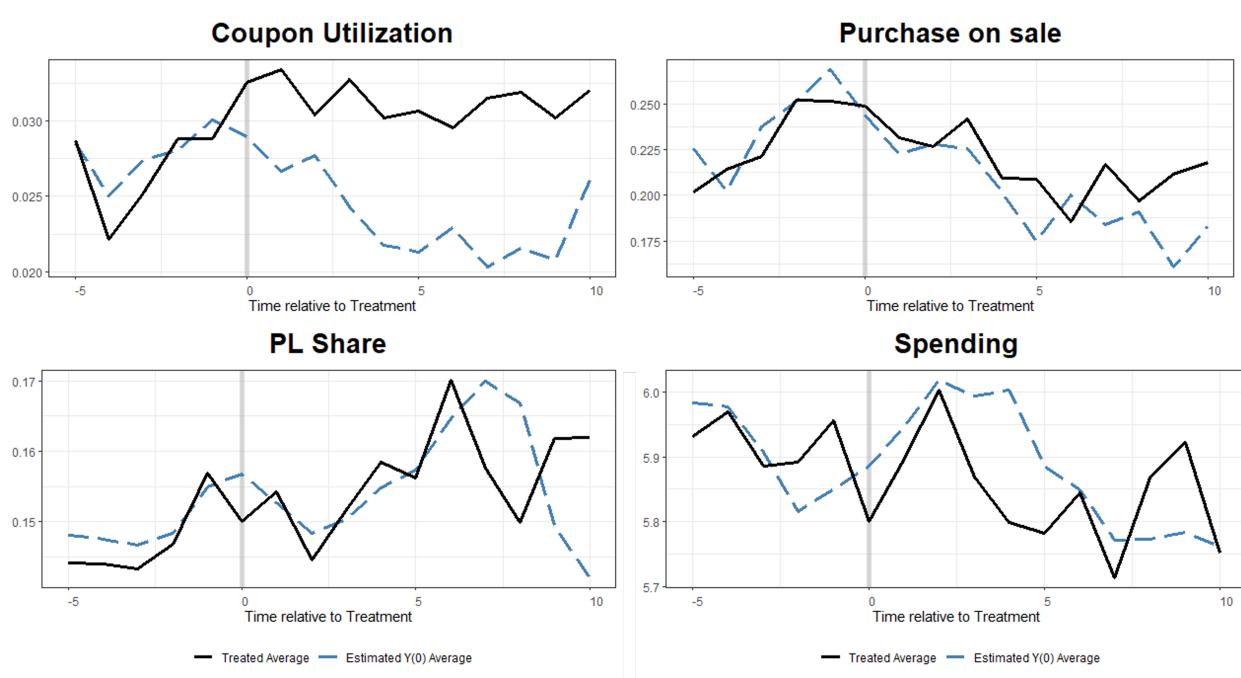


Figure 14: Real vs. Counterfactual of Generalized Synthetic Control Result

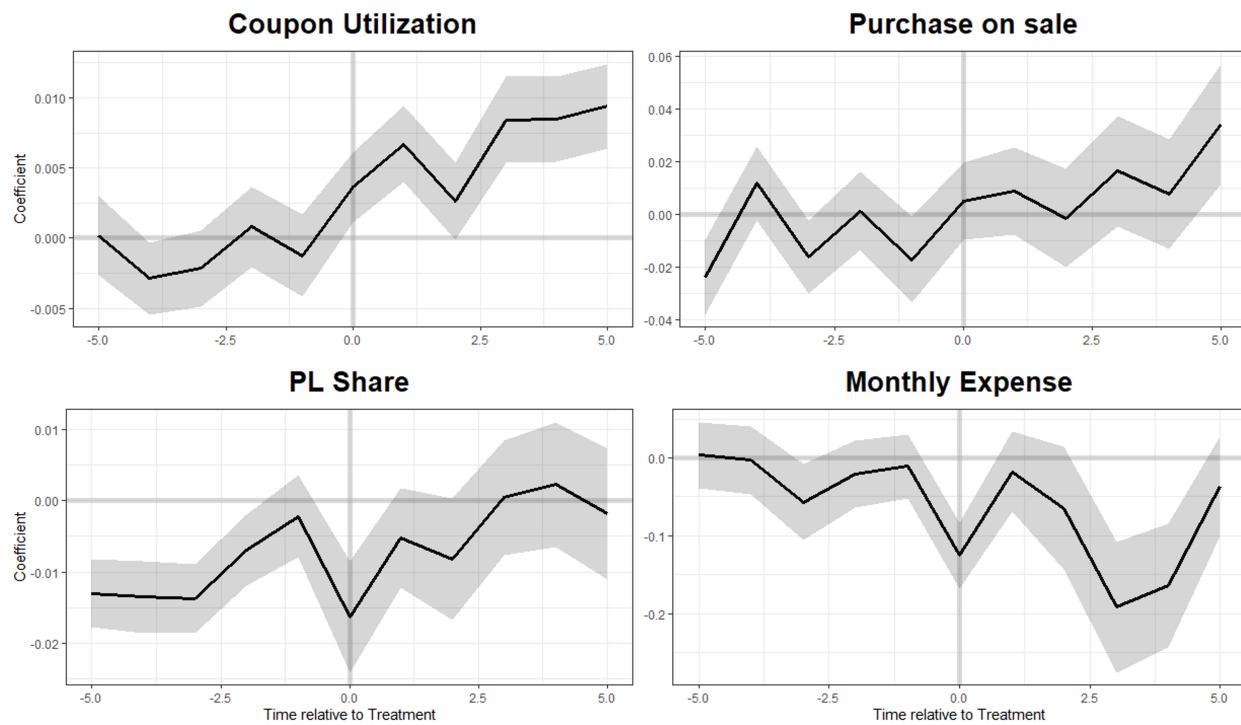


Figure 15: Gap Plot of Generalized Synthetic Control Result

9.5 Placebo Test

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
ATT	-0.0226* (0.0119)	-0.0025 (0.0024)	0.0028 (0.0043)	0.0428 (0.0327)
State-Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	17,410	17,410	17,410	17,410

Clustered Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 17: Placebo Test: Moving-forward (5 cohorts) Placebo Treatment

I perform two placebo tests for the [Sun and Abraham \(2021\)](#) event study estimations. In Table 17, I create new treatment indicators to denote a "Placebo" treatment that begins 5 relative periods in the future in comparison to the real treatments. For example, in states where the real first treated cohort is Class of 1985, the first treated cohort would be Class of 1990 for the placebo treatment. Specifically, the first step regression equation is now:

$$Y_{scy} = \sum_g \sum_{k \in \{-T, T\}, k \neq 4} \delta_{gk} \mathbf{1}\{c - C_s - 5 = k\} \mathbf{1}\{C_s + 5 = g\} + \gamma_s + \gamma_c + \gamma_{sy} + \varepsilon_{scy}$$

This placebo specification tests whether the observed results are truly the effects of the curricular reforms, or from another coincidental shock that happens after the curricular reforms. Additionally, this placebo procedure also provides evidence on whether the treatment effects of different treatment waves are on the same "treatment path" (i.e. if their post-treatment dynamic effects are roughly parallel). If this is satisfied, the analytical inference of the ATT is robust, otherwise, [Callaway and Sant'Anna \(2021\)](#) suggests that "simultaneous inference" may be necessary. The null results we get in Table 17 indicates that there are no coincidental shock after the reform that may contaminate my estimates, and further there is no evidence of diverging or converging treatment paths.

Similarly, in Table 18, I perform a different placebo test by shifting the treatment five relative periods in to the past (instead of the future). This also tests for pre-treatment parallel trends. The null results once again show that the parallel trends assumption is

reasonable, and there is no pre-treatment divergence or convergence in trends.

Dependent Variables:	% Sale	Coupon	Private Label	log(Spending)
Model:	(1)	(2)	(3)	(4)
ATT	0.0314 (24.08)	0.0045 (5.233)	-0.0143 (14.07)	-0.0586 (85.93)
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	13,357	13,357	13,357	13,357

Clustered Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 18: Placebo Test: Moving-backward (5 cohorts) Placebo Treatment

9.6 Pre vs. Post 2008

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
ATT (Pre-2008)	-0.0129 (0.0179)	-0.0013 (0.0036)	-0.0066 (0.0068)	-0.1667*** (0.0505)
ATT (Post-2008)	0.0432*** (0.0122)	0.0119*** (0.0026)	0.0052 (0.0044)	-0.1658*** (0.0376)
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	17,410	17,410	17,410	17,410

Clustered Robust Standard Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 19: ATT Triple Differences Model Pre vs. Post 2008

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treat \times Post	-0.0040 (0.0064)	0.0042*** (0.0012)	0.0039 (0.0027)	-0.0322* (0.0190)
Treat \times Post \times Post 2008	0.0233*** (0.0065)	0.0046*** (0.0012)	-0.0037 (0.0024)	0.0345** (0.0171)
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	17,478	17,478	17,478	17,478

Clustered Robust Standard-Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 20: State Level Triple Differences Model Pre vs. Post 2008

Dependent Variables: Model:	% Sale (1)	Coupon (2)	Private Label (3)	log(Spending) (4)
Treated \times Post	0.0015 (0.0055)	0.0027** (0.0011)	-0.0009 (0.0021)	-0.0636*** (0.0138)
Treated \times Post \times Post 2008	0.0142** (0.0055)	0.0041*** (0.0010)	-0.0031* (0.0018)	0.0162 (0.0120)
Control Variables	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes
Month-Year FEs	Yes	Yes	Yes	Yes
Observations	1,468,850	1,468,844	1,468,844	1,468,844

Clustered Robust Standard-Errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 21: Household Level Triple Differences Model Pre vs. Post 2008