# Do Math Whizzes Save More? Long-term Effect of Mathemathical Education on Consumption Behavior

Felix Nguyen*

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**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 present how mathematical education during formative period of high school can shape later life consumption and spending habits. Exploiting the variation in required math courses triggered by "A Nation At Risk" report in the 1980s, combined with rich retail panel data, I show that additional math coursework can lead to roughly 20% improvement on coupon utilization rate, and this effect persists until much later in life. Evidence also suggests that the effect is strongest amongst minority consumers.

**Keywords:** Education, Consumption, Math, Coupon, Scanner Data

**JEL Codes:** I26, M38, J18

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

# 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 dynamic difference-in-difference framework at both aggregated state level and household level, I show a significant positive effect of additional mathematics coursework requirement on consumers' coupon utilization rate, and a modest positive effect of 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 effect becomes less pronounced with age, but still significant and positive.

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 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 annd 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 (2019) 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.

# 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 pointed out the failing state of secondary education across the nation, and recommended several changes to the curriculum, one of the main ones was to raise minimum number of math courses for graduation to at least three. This is my main focus for identification.

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

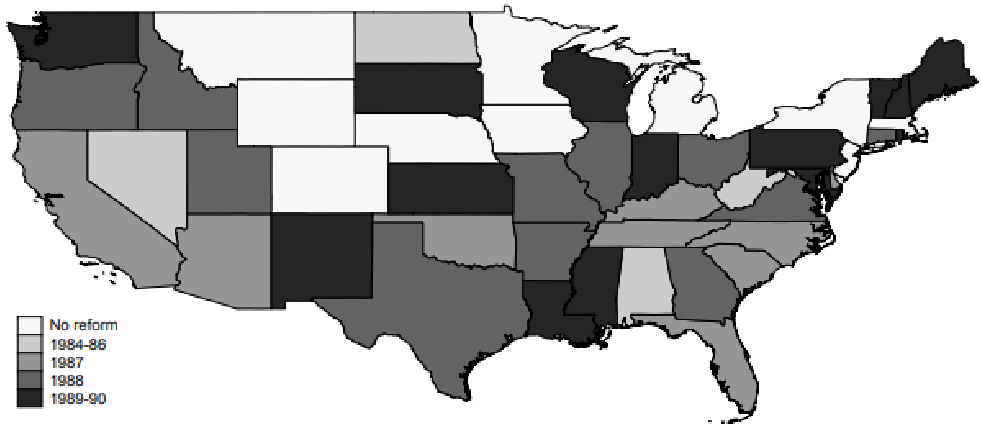


Figure 1: Timing of Math Curricular Reform by States

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. 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 - 2016, 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. One concern about this data is that many consumers do not stay in the same state as where they went to school in. 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 construct an accurate set of households and the head of household's location before 18 (i.e. normal high school graduation), to alleviate this concern (the panelist - household matching procedure can be found in Bronnenberg et al. (2012)'s appendix) . The final sample consists of 23,370 households and more than 1,900,000 observations. I narrow down the results to people born between 1945 and 1985, to avoid other education reform waves (Cole et al. (2016) identify a new wave of reform starting in mid 2000s), reducing the sample to around 1,600,000 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.

# 4 Identification Strategy

As mentioned above, the main identification strategy relies on the exogenous in timing of mathematics curricular reform across states. In an ideal experiment, consumers would be randomly assigned 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, in both logistical and ethical terms. 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 as an instrument. Unfortunately, however, I do not have access to such variable. Therefore, for this paper, I follow a staggered Difference-in-Difference approach. This approach relies of a parallel trend assumption, which I will provide further evidence below. First, the basic pooled state-level model is as follow:

$$Y_{sc} = \beta \mathbf{1}\{c \geq C_s\} + \alpha_s + \gamma_c + \varepsilon_{sc} \tag{1}$$

The main outcome of interest here are monthly coupon utilization rate and ratio of purchases made on sale ("Deal rate"). I calculate these outcomes using the following formulae:

$$CouponRate_{it} = \frac{\sum^s CouponValue_{sit}}{\sum^s TotalValue_{sit}} \qquad DealRate_{it} = \frac{\sum^s \mathbf{1}\{OnDeal_s\}}{N_{it}}$$

Namely, the coupon utilization is calculated as the ratio between total value of coupon used and total value of transaction made by a household $i$ in month $t$. Deal rate is, as mentioned above, the ratio between number of transactions made with deals and total number of transactions. For pooled state level models, I take the average of those measures across households and months, by each graduation cohort. Next, $\mathbf{1}\{c \geq C_s\}$ is the treatment indicator of whether cohort $s$ graduated after the passing of math reform at state $s$ (first cohort being $C_s$). $\alpha_s, \gamma_c$ are the set of state and cohort fixed effects, to account for unobserved variation between states and across time. This basic model, however, ignores the potential bias from differential time of treatment. Thus, I also estimate a second model:

$$Y_{sc} = \sum_g \beta_T \mathbf{1}\{c \geq C_s\} \times \mathbf{1}\{C_s = g\} + \alpha_s + \gamma_c + \varepsilon_{sc} \tag{2}$$

The above model interacts the treatment indicator with an indicator of the year of first treatment of each state, thus states where reforms started in 1987 and 1988 will have separate coefficients, for example. This is similar to the "stacked" estimator use by Cengiz et al. (2019). Next, I also estimate the "event-study"-like dynamic DiD model with distributed leads and lags, both as test for parallel trend assumption and to see if the effect is persistent for later cohorts. The specifications, include the classical event study model and the staggered treatment robust model proposed by Sun and Abraham (2021), are as follow:

$$Y_{sc} = \sum_{-k}^{k} \beta_k \mathbf{1}\{c - e = k\}\mathbf{1}\{c \geq C_s\} + \alpha_s + \gamma_c + \varepsilon_{sc} \tag{3}$$

$$Y_{sc} = \sum_g \sum_{-k}^{k} \beta_k \mathbf{1}\{c - e = k\}\mathbf{1}\{C_s = g\}\mathbf{1}\{c \geq C_s\} + \alpha_s + \gamma_c + \varepsilon_{sc} \tag{4}$$

As we can see, the two equations above are the dynamic equivalents of (1) and (2), with distributed coefficients so each event time $k$ (relative time between the current cohort and the first treated cohort in state $s$) has a unique coefficient. Aside from the state-cohort level analysis, I also estimate the model for household-month level data:

$$Y_{itsc} = \beta \mathbf{1}\{c_i \geq C_s\} + \mathbf{X}_{it}^{'}\gamma + \mathbf{Z}_{st}^{'}\mu + \alpha_t + \mu_{s,y} + \gamma_c + \varepsilon_{sc} \tag{5}$$

This household level model 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 fixed effects $\alpha_t$ control for monthly variation, and $\mu_{s,y}$, state-year fixed effects, control for unobserved temporal shocks to each state. In order to investiage treatment effect heterogeneity, I further interact the treatment indicator with other determinants of consumption habits, include: race, age, household income, education attainment, and marriage status. 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 large problem.

# 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 virtually similar between the two conditions. Both groups have an average age of around 54, average year of birth are both 1957 (treatment first started for cohort born 1966), roughly similar race distribution (Treated states have a slightly higher rate of minorities), similar education, income, household size, or marital status. These similarities show that the data satisfy covariates balancedness and there is unlikely any biases from the difference in socio-demographic characteristics.

Next, I also check the graphical evidence of the parallel trend assumption, as well as a

Table 1: Descriptive Statistics

| | | Control | | | Treat | |
|---|---|---|---|---|---|---|
| Statistic | N | Mean | Median | N | Mean | Median |
| Age | 530,162 | 53.962 | 55 | 1,172,674 | 53.817 | 54 |
| Cohort YOB | 530,162 | 57.179 | 57 | 1,172,674 | 57.322 | 57 |
| Race | 530,162 | 1.156 | 1 | 1,172,674 | 1.257 | 1 |
| Female Head Education | 530,162 | 3.778 | 4 | 1,172,674 | 3.724 | 4 |
| Male Head Education | 530,162 | 3.138 | 4 | 1,172,674 | 3.120 | 4 |
| HH Income | 530,162 | 20.500 | 21 | 1,172,674 | 20.222 | 21 |
| HH Size | 530,162 | 2.391 | 2 | 1,172,674 | 2.375 | 2 |
| Hispanic | 530,162 | 1.969 | 2 | 1,172,674 | 1.951 | 2 |
| Marital Status | 530,162 | 1.831 | 1 | 1,172,674 | 1.819 | 1 |
| Coupon Util | 530,157 | 0.036 | 0.012 | 1,172,671 | 0.030 | 0.008 |
| Deal Rate | 530,162 | 0.289 | 0.228 | 1,172,674 | 0.244 | 0.159 |

model free look as potential treatment effect. Figure 3 and 4 in the Appendix show the plots of Coupon utilization rate and Deal Rate 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. The Deal Rate plot, on the other hand, while also shows a parallel pre treatment trend, seems to indicate that both groups suffer a decline in deal rate for cohorts post treatment, with treated states see a smaller decline. This tells us we should intepret the Deal Rate results cautiously, as there may be some other factors (economic shock, or retailer response) that caused this change in trend.

## 5.2 Main Results

The main regression results of models (1), (2), and (5) are presented in Table 2 and Table 3 in the Appendix. As we can see, the effect of mathematics education on coupon utilization rate is significantly positive and robust across all specifications. In the pooled regression, the treatment effect is 0.7 percentage point increase, and in the household level regression, the effect is 0.5 percentage point increase. While these may appear small at first, when taking into account the mean coupon utilization of just over 3%, and median of 1 %, these present a 17-20 % increase over the mean, and 50-70 % increase over the median. Thus, we can say that mathemamatical curricular reform has a meaningful impact on coupon usage behavior of consumers later in life. From column (3) and (4), the effect holds up to the staggered treatment robust specification, with the coefficients of most waves being positive and statistically significant. The effects are negative for states where reforms came into effect in 1984 and 1986, however these are just two small and relatively idiosyncratic states (North Dakota in 1984 and Nevada in 1986), thus are unlikely to affect our overall conclusion.

| Dependent Variable: | Coupon Utilization M = .032 | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| Treated | 0.0069*** | 0.0047*** | | |
| | (0.0021) | (0.0009) | | |
| Treated × 1984 | | | -0.0057*** | -0.0003 |
| | | | ( 0.0013) | (0.0014) |
| Treated × 1985 | | | 0.0076** | 0.0049*** |
| | | | ( 0.0017) | (0.0022) |
| Treated × 1986 | | | -0.0073*** | -0.0150*** |
| | | | (0.0021) | (0.0022) |
| Treated × 1987 | | | 0.0098*** | 0.0068*** |
| | | | (0.0011) | (0.0020) |
| Treated × 1988 | | | 0.0097*** | 0.0042*** |
| | | | (0.00114) | (0.0025) |
| Treated × 1989 | | | 0.0047* | 0.0030* |
| | | | ( 0.0016) | (0.0042) |
| Treated × 1990 | | | 0.0064*** | 0.0076 |
| | | | (0.0053) | (0.0026) |
| State FE | Yes | | Yes | |
| Cohort FE | Yes | Yes | Yes | Yes |
| Month FE | | Yes | | Yes |
| State × Year FE | | Yes | | Yes |
| Control Vars | | Yes | | Yes |
| Observations | 1,762 | 1,731,920 | 1,750 | 1,730,949 |

Clustered (State) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2: Regression Results for Coupon Utilization

The results for Deal Rate, as presented in Table 3 in the Appendix, are mixed. The pooled regression shows a non-significant result, and while the household level result is statistically significant, its magnitude is relative small. The coefficient indicates an increase of 1.3 percentage point in deal rate, and given a mean rate of approximately 27 %, this translates to just a 5% change. The reform year-specific effects tell a similar story, with only a couple of reform years result in statistically significant changes.

The difference in effects between coupon utilization and deal rate is once again confirmed by the dynamic difference in difference results. From Figure 2, we can see that the effect of coupon utilization is relatively stable across time, and there seems to be no anticipatory effect. Since the pre-treatment coefficients are not different from 0, the parallel trend assumption is likely to be satisfied. As demonstrated in Figure 5 (in the Appendix), deal rate, on the hand, seems to be not affected by the math curricular reform. I also perform a Placebo Test by changing the treatment reference period (i.e., baseline period when indicator is 0) to -10, -5, and 5, and the plots (see Appendix) seem to indicate that the results are not spurious.

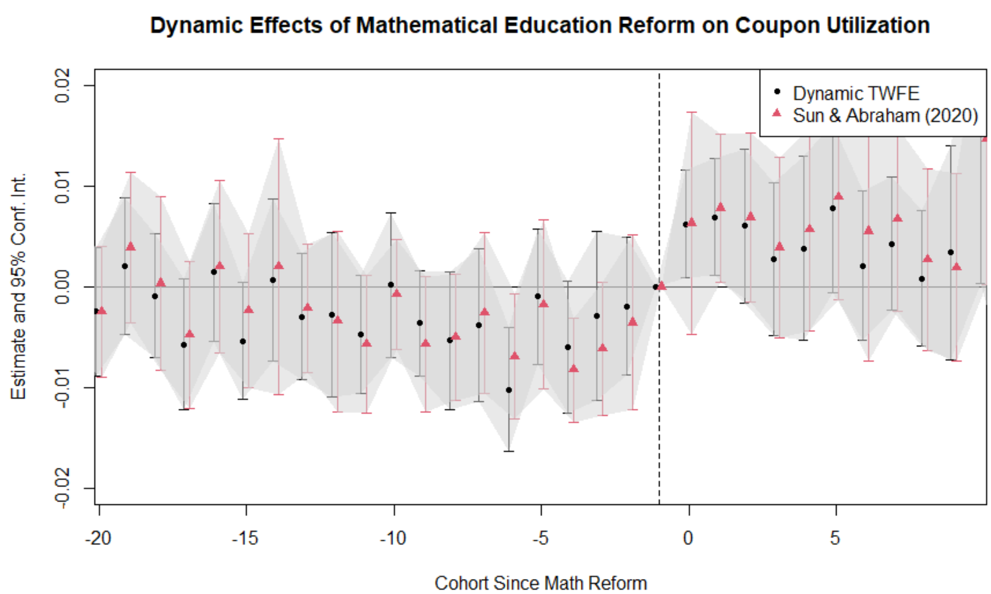


Figure 2: Dynamic Effect on Coupon Utilization

Finally, I examine the treatment effect heterogeneity across several variables through the triple difference model, as discussed above. These heterogeneities are illustrated through a series of plots in section 8.4. in the Appendix, and here I will discuss some interesting results. First of all, we can see that the treatment effect appears to be attenuated with age. The effect is strong around early to middle adulthood, and gradually declines toward zero as around 45. A possible explanation for this trend is other life experience gradually replace early life education as consumers get older. Mext, looking at socio-demographic factors, I found the effect to be much stronger for consumers belong to minorities groups, such as Asian or Black. This is similar to Goodman (2019)'s finding that math curricular reform mostly affects schools in minority neighborhoods in term of increased actual math courses in transcripts. Another interesting result is that the effect is strongest for middle income consumers with household income around US $50-80,000 per annum. Perhaps this group is the most ambivalent to coupon usage, as lower income households would be more budget conscious and are driven to use coupon by economic constraint, while higher income households have no constraints and can afford not to think about costs and savings, thus leaving the middle income group to be most affected by their childhood education.

# 6 Robustness Checks

The aforementioned Placebo test and the staggered treatment robust specification aside, I also performed several other robustness checks to probe the validity of the results. As discussed, spatial heterogeneity between states that decided to adopt the reform at different times, or not reform at all, can present a significant challenge to my results. 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 coupon utilization effects remain robust, while the the household level effect of deal usage becomes insignificant. The magnitude of the effects are relative similar to the original specifications.

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 eigendecomposition 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). The cross-validation procedure indicates a model without factors other than cohort and state FEs is the best fit, thus spatio-temporal correlation is unlikely. The estimated ATT is 0.0073, with a boostrapped SE of 0.0019 ($p < .001$), largely similar to the main result. Manually forcing the model to admit at least one additional factor also does not change the results in any non-trivial ways. The visualization of Generalized Synthetic Control results can be found in the Appendix. Overall, my main results appear robust to heterogeneities in selecting into treatment.

# 7 Conclusion

To conclude, the results of this paper illustrate how childhood mathematical education can lead to long lasting impacts on future consumption habits. Consumers in states that underwent math curricular reforms in the 1980s in the wake of "A Nation at Risk" report see a 0.5 - 0.7 percentage point effect on coupon utilization rate, which translates to roughly 20% higher rate than average and 50-70% higher than median. The effect on ratio of purchase made on sales is more muted and mostly statistically insignificant, yet still positive. 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.

My findings contribute to a better understanding of both the effect of financial and 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. However, this is just a preliminary step, and I welcome future work, especially with more granular data, to explore the exact mechanisms of how mathematics, and perhaps other subjects, affect consumption habits.

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# 8 Appendix

## 8.1 Model Free Evidence

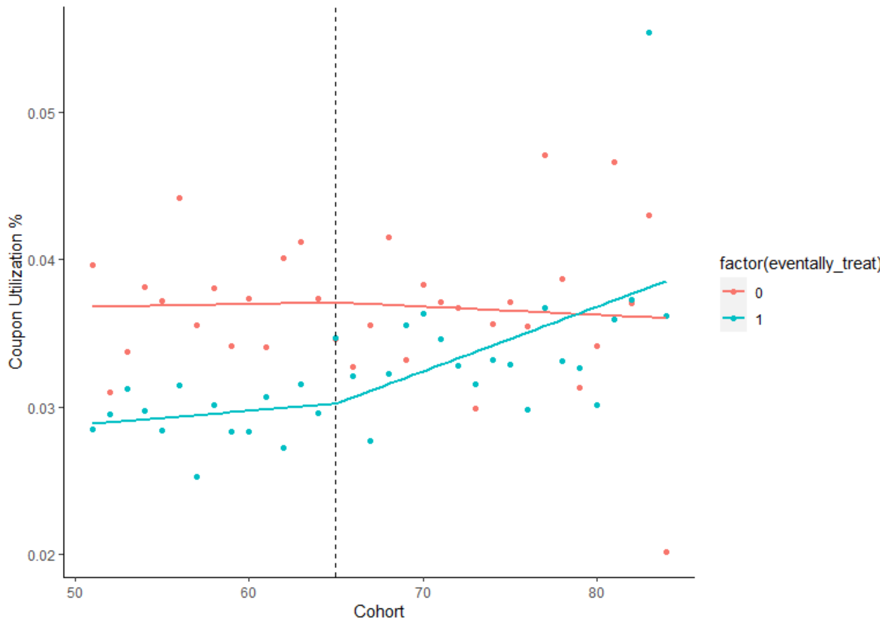



Figure 3: Coupon Plot

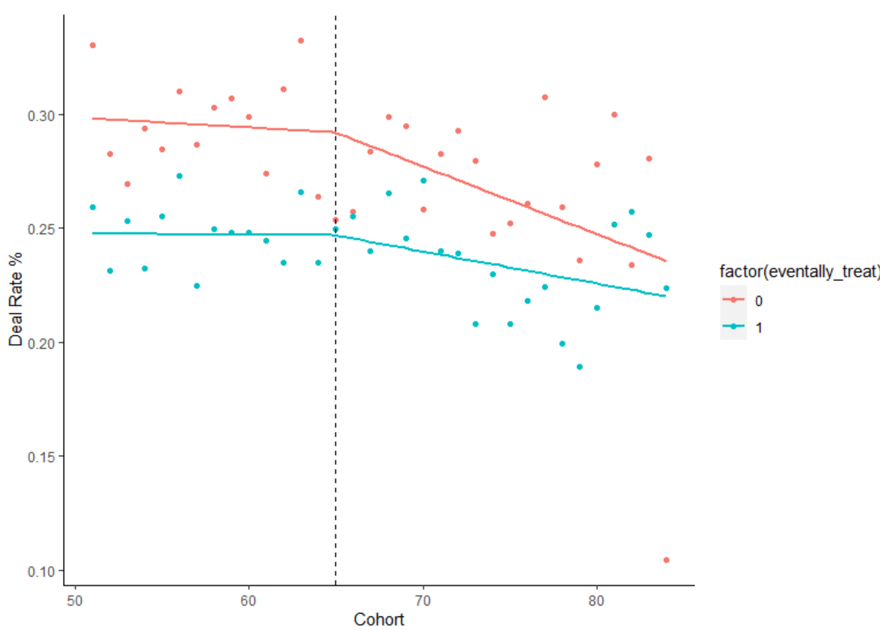



Figure 4: Sales Plot

## 8.2 Additional Results

| Dependent Variable: | Deal Rate M = .27 | | | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| Treated | 0.0108 | | 0.0128*** | |
| | (0.0100) | | (0.0046) | |
| Treated × 1984 | | -0.0523*** | | 0.0384** |
| | | (0.0135) | | (0.0184) |
| Treated × 1985 | | 0.0126 | | -0.0030 |
| | | (0.0155) | | (0.0055) |
| Treated × 1986 | | -0.0626*** | | -0.1016*** |
| | | (0.0188) | | (0.0113) |
| Treated × 1987 | | 0.0166 | | 0.0286*** |
| | | (0.0151) | | (0.0053) |
| Treated × 1988 | | 0.0209*** | | 0.0006 |
| | | (0.0060) | | (0.0046) |
| Treated × 1989 | | 0.0227 | | 0.0127 |
| | | (0.0167) | | (0.0079) |
| Treated × 1990 | | -0.0174 | | -0.0076 |
| | | (0.0177) | | (0.0210) |
| State FE | Yes | Yes | | |
| Cohort FE | Yes | Yes | Yes | Yes |
| Month FE | | | Yes | Yes |
| State × Year FE | | | Yes | Yes |
| Control Vars | | | Yes | Yes |
| Fit statistics | | | | |
| Observations | 1,762 | 1,750 | 1,557,919 | 1,730,957 |

Clustered (State) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3: Regression Results for Deal Rate

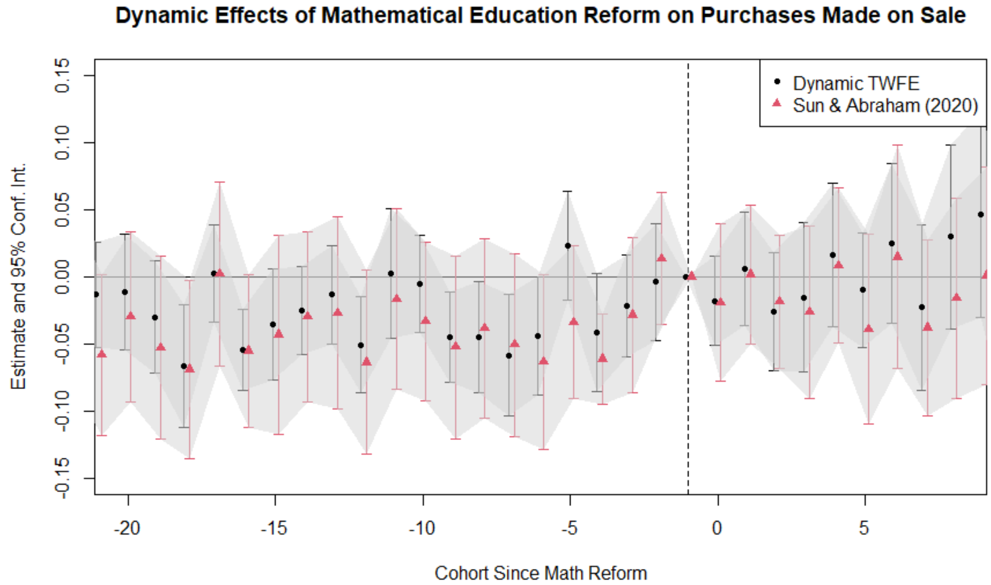



Figure 5: Dynamic Effect on Deal Rate

### 8.3 Placebo Test

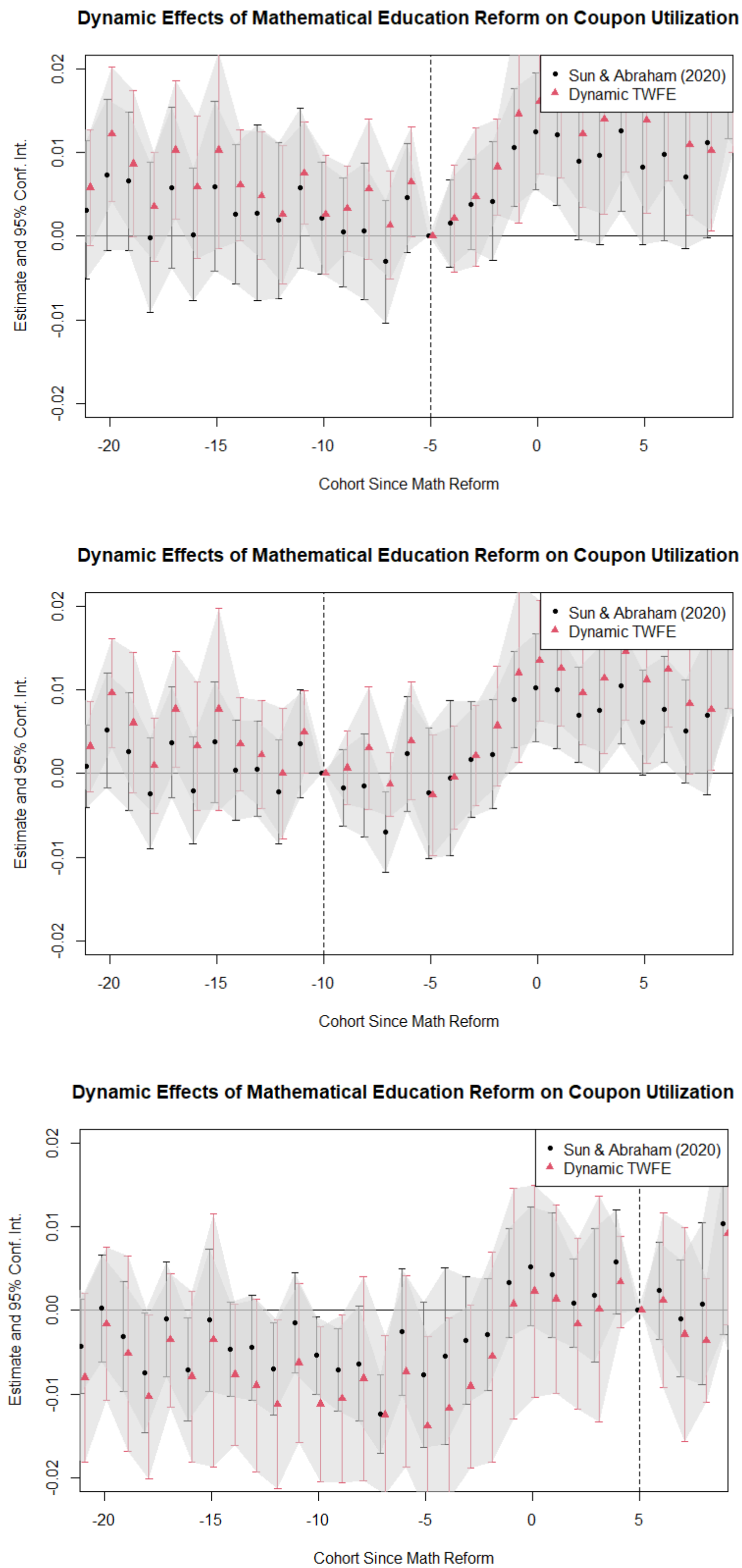



Figure 6: Placebo Test

## 8.4 Treatment Heterogeneity

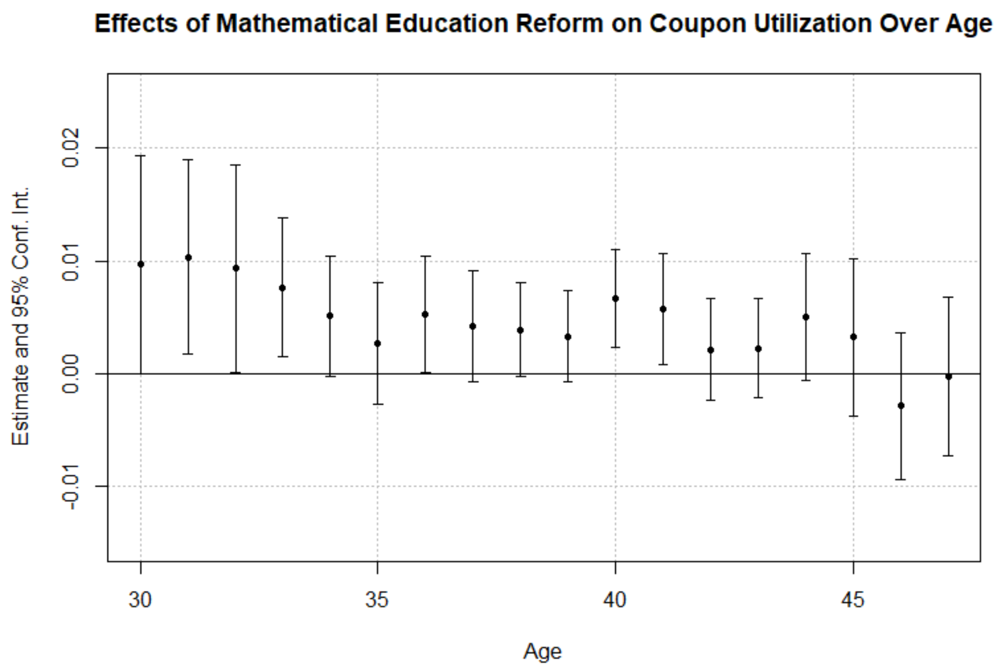



Figure 7: Effect by Age

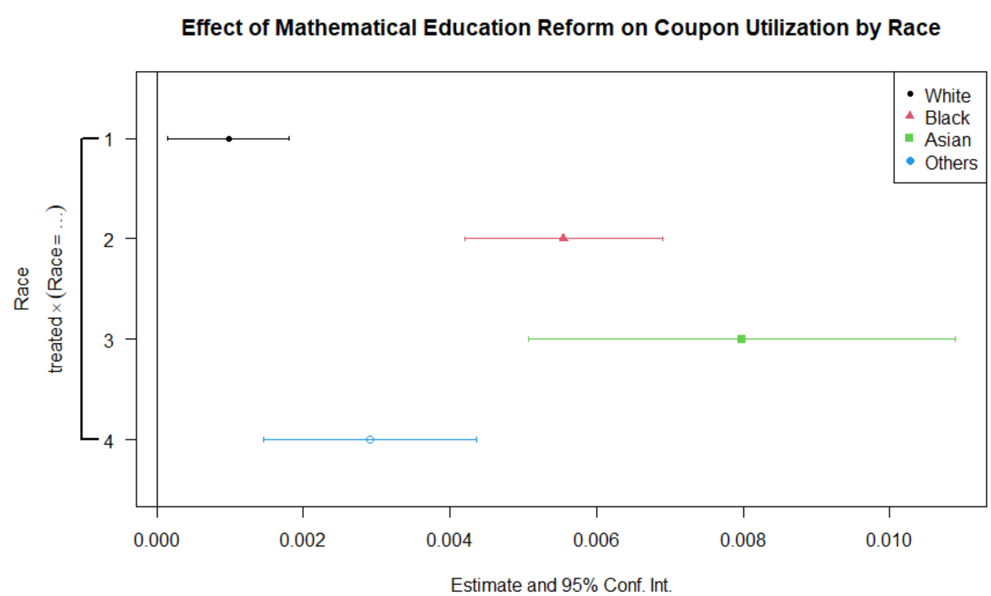



Figure 8: Effect by Race

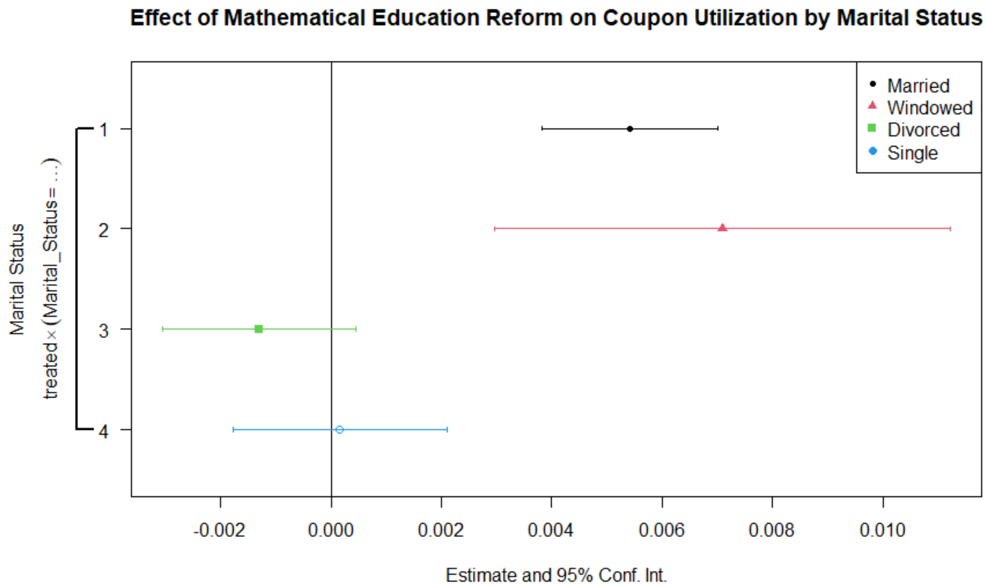
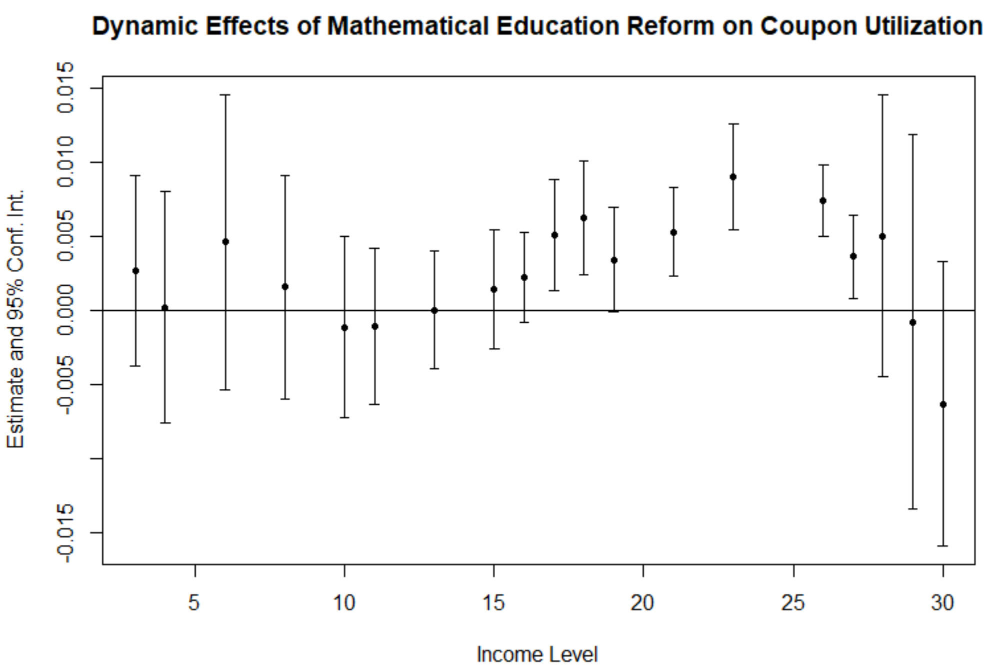
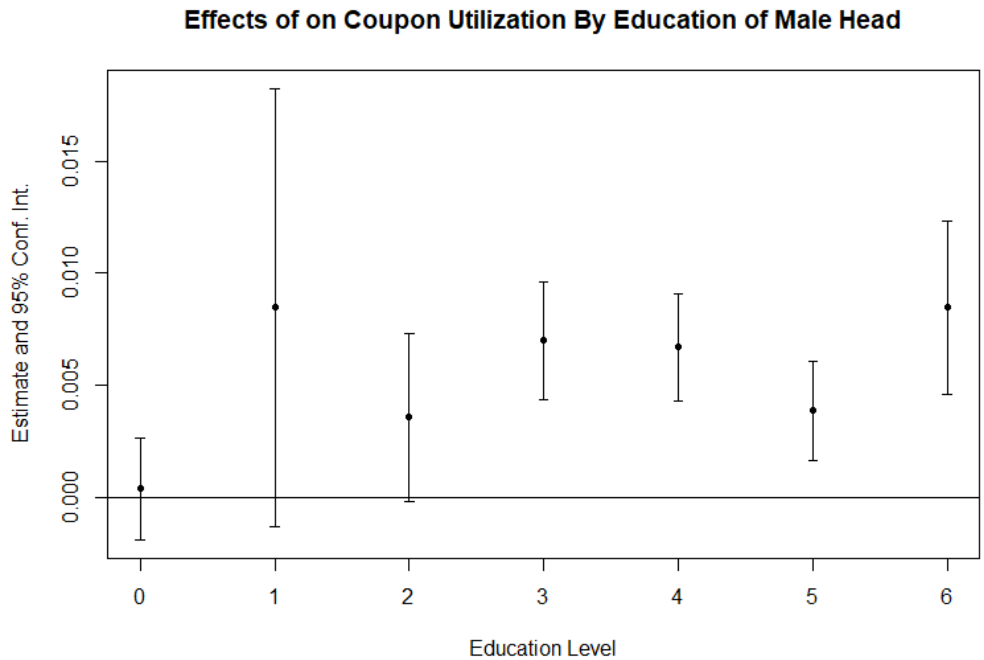
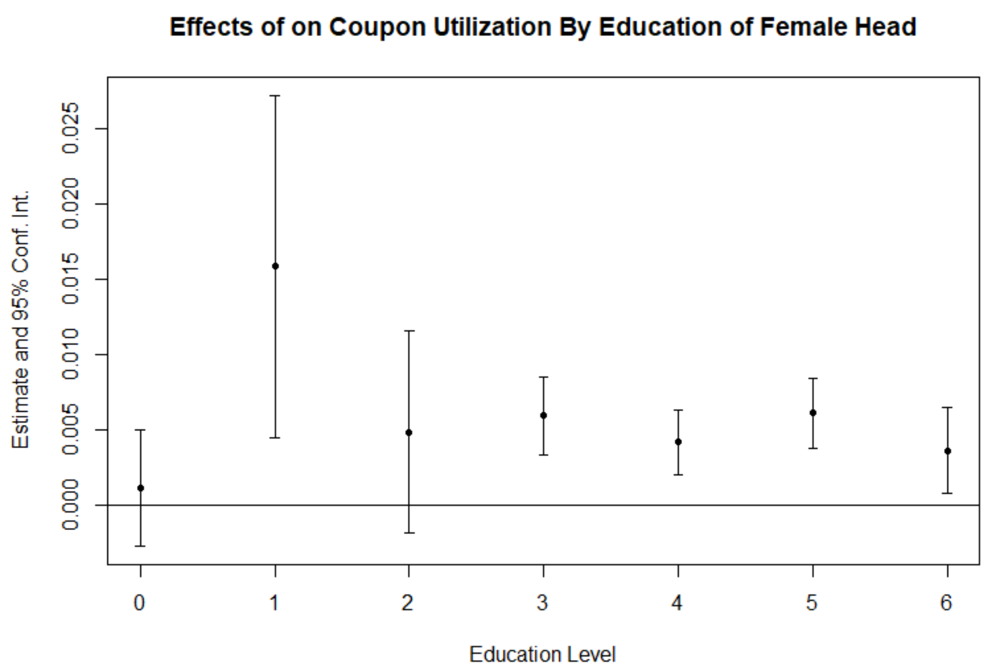



Figure 9: Effect by Marriage Status




Effects of on Coupon Utilization By Education of Male Head
Estimate and 95% Conf. Int.
0.000
0.005
0.010
0.015
0
1
2
3
4
5
6
Education Level


Effects of on Coupon Utilization By Education of Female Head
Estimate and 95% Conf. Int.
0.000
0.005
0.010
0.015
0.020
0.025
0
1
2
3
4
5
6
Education Level

## 8.5 Robustness Check

| Dependent Variables: | deal_rate | | coupon_util | |
|---|---|---|---|---|
| Model: | (1) | (2) | (3) | (4) |
| *Variables* | | | | |
| treated | 0.0081 | 0.0012 | 0.0070** | 0.0047*** |
| | (0.0149) | (0.0046) | (0.0027) | ( 0.0011) |
| State Time Trend | Yes | Yes | Yes | Yes |
| State FE | Yes | | Yes | |
| Cohort FE | Yes | Yes | Yes | Yes |
| Month FE | | Yes | | Yes |
| State × Year FE | | Yes | | Yes |
| Control Vars | | Yes | | Yes |
| *Fit statistics* | | | | |
| Observations | 1,762 | 1,731,928 | 1,715 | 1,731,920 |

Clustered (State) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 4: Results with Linear Time Trends

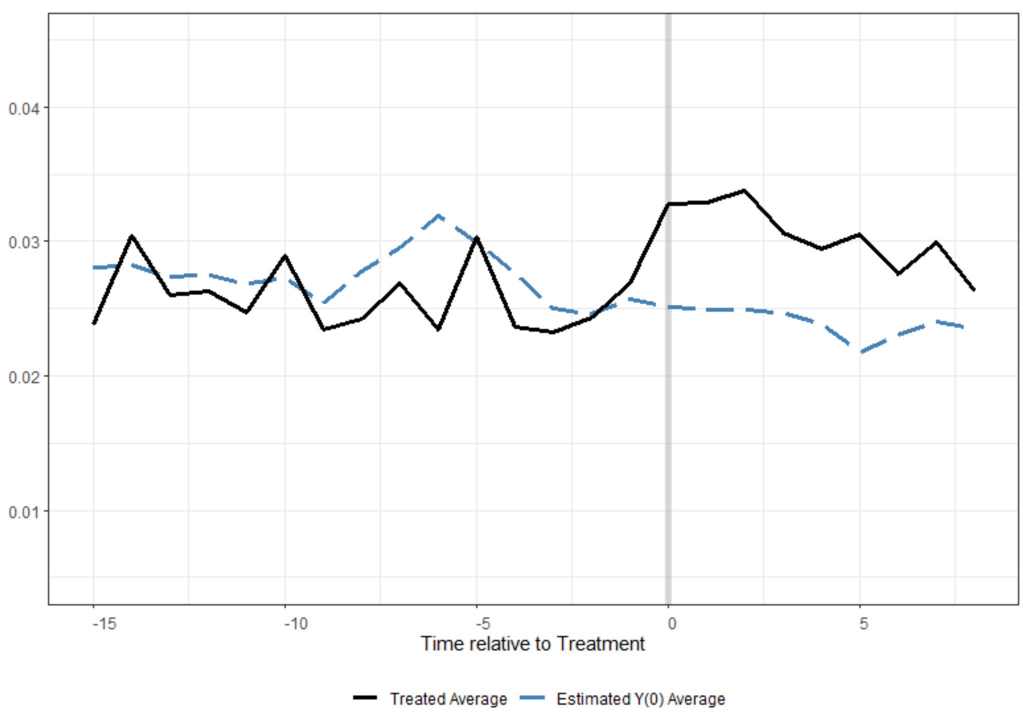



Figure 10: Real vs. Counterfactual of Generalized Synthetic Control Result

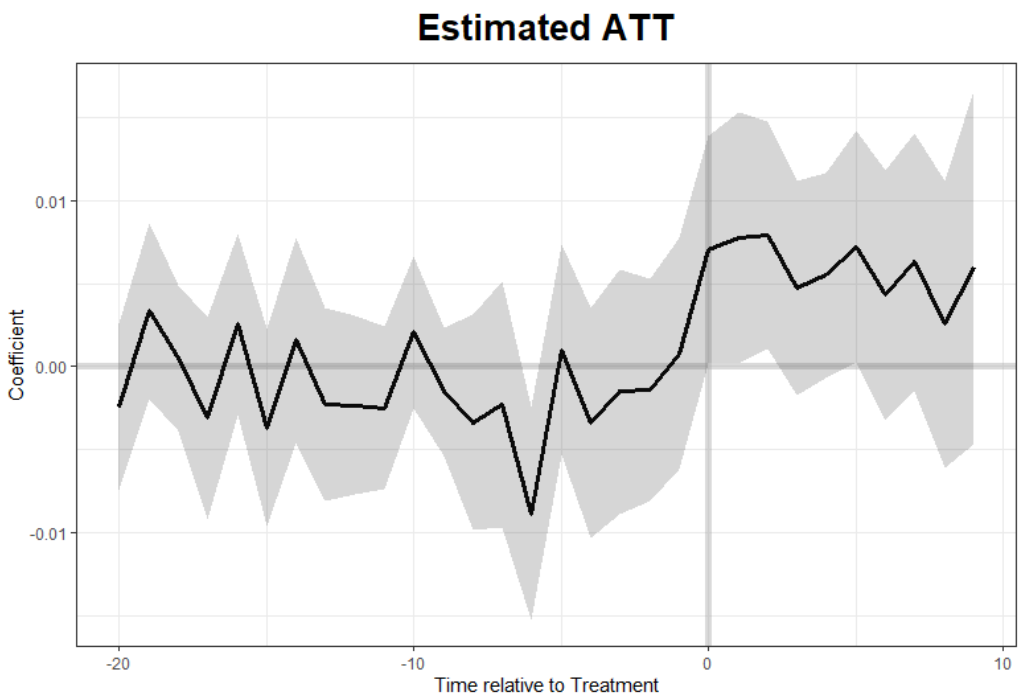



Figure 11: Gap Plot of Generalized Synthetic Control Result