Essays in Applied Microeconomics

By

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Abstract

This dissertation consists of three chapters that use applied microeconomic methods to investigate the impact of public policies. In Chapter 1, I study the relationship between food insecurity among U.S. households and state sales taxes on food. Households that experience food insecurity are either unable or uncertain that they can acquire enough food to meet their nutritional needs. On average an estimated 10.5 percent (13.7 million) of U.S. households were food-insecure at least some time during the year 2019. Food insecurity is also larger for more vulnerable groups, such as low-income households, and minorities. Few studies explore how grocery food sales taxes affect food insecurity prevalence in the US. This study contributes to the literature by drawing upon newly constructed data from various sources on changes in state food sales taxes policies over time, and implementing a difference-in-differences and fixed-effects ordered logit models to estimate impact of food tax changes on self-reported food insecurity. I find that decreases in the food sales tax decreases the likelihood of being a food insecure household by 4.93 percentage points and increasing the food sales tax increases the likelihood of being a food insecure household by 9.77 percentage points. Decreasing food sales taxes contributes to an improvement in a household's food security levels. Conversely, food sales tax increases result in a decrease in self-reported food security.

In Chapter 2, I study housing displacement and evictions in the context of third-party policing nuisance property ordinances. Third-party policing laws are policies in which police attempt to persuade or force third parties, such as landlords, to take some level of responsibility in preventing criminal activities. The most prevalent of which are nuisance property ordinances. These require landlords to regulate tenant behavior to eliminate behaviors which are classified as a "nuisance" generally through enacting some form of an abatement program. The adverse effects of such laws

have yet to be examined from an economic perspective. This study examines how these laws may affect the levels of both legal and extralegal evictions in cities and counties in which they are used. Using data from a variety of sources a difference-and-difference model is used to discover the impact of these nuisance ordinances. A selection of large US cities which have initiated the use of these ordinances will be examined within the time frame available with the data. It is the aim of this study to discover what effect if any these laws have on individuals housing displacement. It finds that legal evictions decrease sometime within two years following the treatment of the nuisance ordinances, and that renter tenure exhibits a positive spike in the levels of tenants who had recently moved into their current rental residences within that period. This supports the hypothesis of some kind of extra-legal displacement of renters following the enactment of these laws.

In Chapter 3, I review the EPA's Priority Program using data from chemical manufacturing to examine the impact of the program on regulatory compliance of discharge limits. The EPA's Priority Program increased monitoring, enforcement actions, and incentive programs for certain industrial manufacturing sectors in an attempt to abate water pollution. Using difference-in-differences, and synthetic control methods the study addresses three questions: First did facilities included in the significant sector perform better relative to no status? Second did facilities included in the priority sector perform even better than the significant sector? Finally did facilities continue to perform better after the Priority Program ended? I find that inclusion as a significant sector was effective at reducing discharges and improving compliance, whereas inclusion as a priority sector did not significantly improve compliance. Once the Priority program ended there were no statistically significant effects on compliance.

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Chapter 1

Effects of State Grocery Tax Changes on Household Food Insecurity

1.1 Introduction

Most households in the United States are food secure, and have consistent access to dependable sources of food, but this is not true of all households. An estimated 10.5 percent (13.7 million) of households in the US are classified as food insecure—they experienced difficulty providing adequate food for members as a result of inadequate financial resources. The prevalence of food insecurity is also larger for vulnerable groups such as low-income families, minorities, and households with children, particularly those headed by single parents (Coleman-Jensen et al., 2019). Previous research has focused on the determinants, consequences, and health implications, as well as the efficacy of public program mitigation for food insecurity. Most of this research examines the effect of the Supplemental Nutrition Assistance Program (SNAP) on food insecurity. This study will examine the causal effect of food sales tax on food insecurity. I find that as the food sales tax decreases, the proportion of food insecure household falls as well. In addition, increasing food sales taxes decreases a household's relative food security.

Previous studies focusing on factors contributing to food insecurity have not examined the impact of food sales taxes directly. Household income and various types of assets, such as human capital through education levels, and physical capital in the form of home ownership have been found to be significant factors affecting food insecurity levels (Gundersen, et al, 2018). Similarly, given the higher levels of real income, areas with relatively lower food prices are found to be less likely to be classified as food insecure (Gregory & Coleman-Jensen, 2013). They find the effect of prices on food insecurity to be substantial. Gregory and Coleman-Jensen (2013) find that a less than one standard deviation change in food prices "amounts to 5.0%, 5.1% and 12.4% increase in the prevalence of household, adult, and child food insecurity levels, respectively." The hypothesis examined in this study is that a relatively higher food tax leads to an increase in food insecurity because it increases the price of food for consumers.

Demographics and household composition also have significant influence on food insecurity. For example, families with children have higher rates of food insecurity when compared to those without children (Gundersen et al, 2018). Likewise, marital status, the presence of elderly or disabled family members, and households that are headed by single parents are all significant determinants in food insecurity (Ziliak & Gundersen, 2016; Huang et al., 2010).

A substantial branch of food security literature deals with the potential health outcomes which have been found to be consistently negative. Gundersen et al. (2015) found that food-insecure children are at least twice as likely to be in fair or poor health. Food insecurity among children has been found to be associated with an increased risk of anemia, cognitive problems, anxiety, asthma, and depression. Among adults, depression, diabetes, and hypertension have also been found to be related to the inaccessibility of food (Gundersen, et al, 2018).

In response to these concerns many recent studies of food security target alleviation via SNAP. Although the primary goal of SNAP is to alleviate food insecurity, researchers have found that food insecurity rates are found to be much higher among SNAP participants (Coleman-Jensen et al. 2019). This is not surprising as SNAP participants are those who are most at risk of being food insecure. In order to get a clearer picture of the alleviating effects of SNAP, many studies have controlled for this selection issue. It has been found that participants are somewhere between 5 and 20 percentage points less likely to suffer from food insecurity than non-participants (Gundersen et al., 2017). Due to this shielding effect of SNAP additional research has delved deeper into the structure of the SNAP program, its underlying construction and possibility of its expansion (Ziliak, 2016; Gundersen, Kreider, & Pepper, 2018).

In related research, researchers have examined whether specific food taxes influence consumer choices between healthy and non-healthy food and have an impact on obesity and weight. For instance, soda consumption has been thought to be a large factor in the growing rate of obesity in the US. This has resulted in many states implementing an excise sales tax on soda. Fletcher et al. (2010) found only modest effects from soda sales taxes on weight, and have shown to increase the cost of those beverages to consumers (Willage & Frisvold, 2018). High energy food taxes have also been studied, finding that effects of such special taxes are unclear due to no obvious list of food which should be taxed (Freebairn, 2010). Okrent and Alston (2012) find that such caloric based taxes would be regressive, falling disproportionately on the poor. Lacking in this particular branch of food tax literature are the considerations of sales tax effects on all food such as those found in state grocery sales taxes.

Researchers have argued that food sales taxes are regressive, are poor sources of revenue for states and demonstrate the complicated nature of tax credits in mitigation of their effects (Lav & Johnson, 1998). Additionally, examining border counties and grocery taxes reveals an inverse relationship between food tax and food sales (Walsh & Jones, 1988). More specifically, Mehmet and Skidmore (2007) found that for "every one-percentage point increase in the county relative price ratio due to sales tax change, the per capita food sales decreases by about 1.38 percent." Nevertheless, these studies do not consider food taxes in relation to accessibility, and therefore food security.

A growing body of literature examines tax salience, or how taxes are displayed to consumers and impact consumer responses. Researchers have found that relative to excise taxes, sales taxes have a smaller effect on consumer behavior. (Chetty et al., 2009). Researchers have found that sales tax does not change demand as much as excise taxes and conclude that sales taxes have a lower salience in regard to consumer behavior (Chetty et al., 2015; Zheng et al., 2013). Researchers also find that only low-income consumers respond to taxes which are levied at the register (Goldin & Homonoff, 2013). Such low-income consumers would also be those who are most likely to experience food insecurity in their households. However, notwithstanding these findings, this literature again does not examine the way in which grocery taxes may influence food purchases and therefore food security. Additionally, researchers have found that a small negative incentive, such as a $0.05 tax on disposable bag use, can have large effects in absolute terms decreasing the use of disposable bags by 40 percentage points (Homonoff, 2018).

Although the effects of grocery taxes on food security levels seems to be a consistent gap in these various literatures some researchers are starting to examine this issue. Wilson et al (2017) use probit models to estimate the effect of food sales taxes on SNAP recipients and non-recipients. They find that the likelihood of food insecurity increases for non-SNAP recipients, and that SNAP shields participants to the effects of grocery taxes. They note as well that their study does not consider tax credits, and therefore their results represent lower or upper bound of the true impact depending on the impact a tax credit would have on food accessibility. Their study does not identify the causal effect of food sales taxes on food insecurity.

My analysis uses difference-in-difference design exploiting a newly created data set of time-varying food tax rates to examine the effect on household food security levels. By examining both increases and decreases in food tax rates relative to non-changing states, I am able to identify the causal effect of food taxes on food security. Additionally, this analysis considers policy changes in Kansas, a state that taxes food at the same rate as other commodities and that eliminated the state food tax rebate in 2012. Food tax increases in Kansas with and without refundable tax credits allows me to look at how rebates can affect food security levels. I find that decreases in food sales tax lead to lower levels of food insecurity and that policies increasing such taxes have higher levels of food insecurity. Furthermore, these effects are seen in the marginal effects, with the probability of households being classified at lower food security levels rising with a food sales tax increase and falling with a food sales tax decrease.

1.2 Data

The sample for this study is generated from merging two supplements from the Current Population Survey (CPS) at the household level. These supplements are the Annual Social and Economic Supplement (ASEC) and the Food Security Supplement (CPS-FSS) for the years 2007-2018. The CPS is a large nationally representative survey of the non-institutionalized civilian population conducted each month containing data on demographics and labor-market information at the household level. The ASEC and CPS-FSS are annual subsamples which are conducted in March and December, respectively. The ASEC subsample covers social and economic characteristics of individuals in the household, and it measures family income, household composition, poverty, and welfare status. The CPS-FSS elicits information regarding household-level issues relating to food security and access.

These supplements are administered in different survey months and merging them is challenging. Households enter the CPS and are surveyed for four months, and then they have a break for eight months after which they are subsequently surveyed for an additional four months. For example, a household may start being surveyed in December of 2016 for four months until March 2017. After this point, the household will start being surveyed again in December 2017. The CPS-FSS sample for Dec 2016 are then matched to the next closest ASEC Subsample, which would be March 2017. (See Figure 1.1 for a visual illustration of this example). The observation is then set for 2016 the year for the CPS-FSS. Therefore, households in this study have been surveyed in both the ASEC and the CPS-FSS to facilitate this matching across the years 2008–-2018 resulting in 70,731 households. Of these households 28,514 appear for the full two years expected from 8 months of survey questioning with the remaining 42,217 households appearing only in one year of the data. These 42,217 households appear in only one survey year because of the CPS sampling structure.

ASEC data are used to calculate percentage levels of household income relative to the federal poverty line as well as to determine welfare program participation. The CPS-FSS provides the data necessary for analysis on food security status. I use the food security status provided in the supplement, which is based on a 12-month food security scale derived from a series of 18 questions around food security. Questions ask about the ability to afford adequate levels of balanced meals and resulting scores separate individuals into four categories: High, Marginal, Low, and Very Low Food Security. Households falling under Low and Very Low Food Security are classified as “Food Insecure Households.” Changes in household status over time have been noted, and also used in this analysis. For example, a household which moves from Low to Marginal food security status during the two years in which they appear in the panel are relatively more food secure. These movements are considered in the marginal analysis.

Aggregate state level panel data for minimum wages, population, and EITC refundability are taken from the University of Kentucky Center for Poverty Research’s National Welfare Data (2020). An additional aggregate state level index of regional food prices from the BEA’s regional price parities by state is also included in the analysis.

State-level grocery food tax data and corporate tax data across time are compiled from the Tax Policy Center, The Tax Foundation, The Federation of Tax Administrators, The Council of State Governments' Books of State, and my own searches of various state departments of revenue. By using these sources, I am able to construct historical grocery tax data that captures changes in food tax rates where possible. Summary statistics can be found in Table 1.1 for the full sample and by SNAP participation, and Table 1.2 by the percent of the federal poverty line. These summary statistics report the mean and standard deviations for the primary variables used in the analysis. The sample shows that 12.6% of households in the sample are classified as food-insecure households. SNAP participants account for 9.63% of the sample and 43.8% of those households are food insecure. Of the households in the full sample 59.5% of the head of households are employed, and 15.9% of the head of households in the sample report having some sort of cognitive or physical disability. The proportion of the sample with the head of the household being employed decreases as the sample is limited to the 300% and 185% of the federal poverty line to 43.2% to 34.7% respectively. Likewise, households which are food insecure increases to 22.1% and 27.6% respectively in these lower income groups.

For this study, the primary dependent variables of interest are derived from food security ranges in the CPS-FSS. First, the individual household whose survey responses lead to a food security classification of low or very low food security are then defined as a "Food Insecure Household." This measurement is straightforward, following the definition of food insecurity. The second dependent variable is a categorical variable which encompasses all of the food security ranges in the CPS-FSS. These categories are ordered to align with the convention of order logit models where larger categorical numbers represent improvement or higher levels of food security. The food security status variable is defined as: 1=Very low food security, 2=Low food security, 3=Marginal food security, 4=High food security. This categorical dependent variable will be used to capture movements in household classification during the two-year period of survey participation. For example, consider a household who in the first year of participating in the CPS-FSS is classified as a low food-secure household. In the second year of participation the household is then classified as a marginally food-secure household. This would constitute a positive movement in their relative food security status. Figure 1.2 shows these categories and movements for households who appear twice in the sample. The rows correspond to the first period and the top columns correspond to the second period. The bottom triangle (in red) of the figure shows the number of households that moved to lower food security levels. The top triangle (in green) shows the number of households that moved to higher food security levels. The diagonal band (in yellow) shows those households that did not change status. By using this categorical measure, together with the food insecurity measure, I am able to consider both total food insecurity levels and marginal effects resulting from food sales taxes.

1.3 Empirical Model

Using household level panel data, I estimate fixed effect difference-in-differences (DID) models using the two household food security measures. I will begin this section discussing the determinants of food security, and the covariates used across the different DID models. Next, I will discuss the control and treatment groups for the state grocery tax fluctuations used in the analysis. I conclude with the various model specifications, and robustness checks.

1.3.1 Determinants of Food Security and Covariates

I control for many determinants of food security that have been used in the literature. Food security as a measure is derived as function of household demographics and composition. For this analysis household level demographics include things such as owning the house and, log of total household income, which is the combined income of all adult household members from all sources (including welfare programs), the number of family members, and number of children in the household and whether the household receives SNAP or Temporary Assistance for Needy Families (TANF) benefits. Remaining demographics included are those of the head of the household. These include age, gender, race, marital and employment status, US citizenship, education level, and the presence of any physical or cognitive disabilities. As shown in previous studies I expect these demographics to play a role in the likelihood of food security. For instance, households headed by older, married, or more educated individuals would typically experience lower levels of food insecurity. Similarly, I would expect those headed by females or non-white individuals to experience higher instances of food insecurity. In regard to household composition, the presence of non-working members, children, or someone with a disability may put pressure on household resources affecting food insecurity levels. These household composition factors are captured by variables on the number of children in a household as well as the household family size. Additionally, I include state-level economic conditions included as controls. Specifically, refundability of the state earned income tax credit (EITC), availability of a state refundable food tax credit, a regional food price index, and the log of the state minimum wages.

1.3.2 Treatment and Control Groups

The specifications used in this analysis are subject to the same treatment and control groups depending on the food tax decreasing, or increasing. In general, the treatment groups consist of those states with food taxes which experience some change in their food tax rate after 2008 (the start of the sample period). Control groups are then all other states, except the states that have any tax variation in the opposite direction. This is done in order to eliminate any confounding effects.

For instance, the state of Arkansas experienced the first food tax decrease after 2008 in the year 2010, so from 2010–2018 Arkansas would be in the treatment group for a food tax decrease along with all other food-taxing states who had a decrease in food taxes. The control group would then be all other states with no change in food sales taxes. For food tax increases the treatment and control groups are defined in a similar manner with one exception being Kansas. Kansas is unique in that it has both tax increases and decreases, and is omitted from the food tax decrease sample, but not the tax increase sample. The general treatment groups by tax variation and year of treatment are summarized in Table 1.3. Table 1.4 shows all the states in the sample with food sales taxes and the years for any changes in those rates.

1.3.3 Model Specifications

Before conducting the various DID specifications, I ran a simple pooled OLS model to evaluate the correlations between the control variables and three separate levels of food security status as defined by the CPS-FSS. For this specification standard errors are clustered at the household level and given by the following equation.

$$(1)\ Y_{status,it} = \beta_1 + \sum_{j=2}^{k} \beta_j X_{jit} + u_i + \varepsilon_i$$

Y_{it} represents the food security status, either marginal low or very low food security, for household *i* at time *t*. β are the coefficients for observed explanatory variables *j* for *k* number of included variables. u_i are unobservable characteristics, and ε_i is the error term.

The main identification strategy for this study is a fixed effect DID model. The treatment is identified by an increase, or decrease of the food tax rate at the state level. These primary specifications are conducted for both measures of food security over the full sample as well as by subsamples of 300% or below and of 185% or below the federal poverty line based on total household income. Additionally, subsamples by SNAP participation for the full sample were estimated separately to examine whether SNAP benefits would mitigate any effect of food sales taxes since food purchased using SNAP is not taxed. All of the DID models run in my analysis have clustered standard errors at the state level which is the level of treatment. The first specification is for a food tax decrease and is given by the following equation.

$$(2)\ FI_{it}=\alpha+\beta_{it}DID_C+\gamma_{it}X_{it}+State_t+Time_t+\varepsilon_{itg}$$

Where FI_{it} is a dummy variable for the food insecurity measure for household i and time t, equaling to one for households which are classified as food insecure, and zero otherwise. The DID estimation coefficient is represented by β_{it}, which estimates the treatment effect as the difference between the pre-treatment and post-treatment values of their respective outcome variables. DID_C is a dummy variable which defines the treatment group and treatment period for a decrease of the food sales tax. Where DID_C takes a value of one for states being treated starting at the initial point of treatment until the end of the sample period. The only exception to this is Kansas, which experiences increases and decreases in its food tax variation in the sample. Kansas is considered treated for a tax increase staring in 2010, and again in 2015. It is not considered treated for an increase in the years 2013, and 2014, but rather it is treated as experiencing a food tax decrease in those years. The term X_{it} includes the covariates and determinates of food security as outlined previously. This is followed by state and time fixed effects and the error term ε_{itg} which is clustered at the state level indicated by grouping g.

The increase in a food tax treatment is driven almost completely by Kansas, and during the sample period Kansas also eliminated the refundability of their food tax credit in the year 2012.

To account for these factors, I conducted an additional specification which is similar to the primary model construction but includes two DIDs. One for the first tax increase in 2010 with a refundable tax credit and the other for the second food tax increase in 2015 without the refundable tax credit. This specification is used for both the general control and treatment groups as well as the more strictly defined control and treatment groups targeting Kansas. The equation is given below.

$$(3)\ FI_{it} = \alpha + \beta_{it} DID_{Increase\&credit} + \delta_{it} DID_{increase\&no\ credit}$$

$$+\gamma_{it} X_{it} + State_t + Time_t + \varepsilon_{itg}$$

The identification strategy for examining the marginal effects of food security is a fixed effects ordered logit model. This model uses the latent variable FSS_{it}^{*} which relates the observable characteristics of X to the ordered dependent variable FSS which can take values which raises sequentially crossing higher thresholds of food security. FSS_{it}^{*} for household i at time t depends linearly on X_{it} and the unobservable characteristics are α_i and ε_{itg}

$$(3)\ FSS_{it}{}^{*} = \alpha_i + \beta_{it} DID_C + \gamma_{it} X_{it} + \varepsilon_{itg}$$

The vector X_{it} does not include an intercept because α_i acts as household-specific intercepts. The time-invariant part of the unobservable α_i are the fixed effects and depend on X_{it}. The DID_C is defined as in the previous specifications with changes in tax treatment being an increase, or decrease of the food sales tax. Standard errors are similarly clustered at the state level. The following rule ties the latent variable FSS_{it}^{*} to the observed ordered variable FSS_{it} through thresholds τ_{ik}:

$$(4)\ FSS_{it}=k \quad \text{if} \quad \tau_{ik} < FSS_{it}^{*} \leq \tau_{ik+1} \quad k= \begin{cases} 1=\text{Very Low Food Security} \\ 2=\text{Low Food Security} \\ 3=\text{Marginal Food Security} \\ 4=\text{High Food Security} \end{cases}$$

For a very low FSS_{it}^* the food security status is categorized as very low. For $FSS_{it}^*>\tau_{i1}$ food security status improves to from very low to low food security. For $FSS_{it}^*>\tau_{i2}$ food security status improves to further from low to marginal and so on. The required assumptions for this model are that the lowest and highest threshold are minus and positive infinity, and that household specific thresholds are increasing for each household.

$$(5)\ \tau_{i1}=-\infty;\ -\infty<\tau_{ik}<\tau_{ik+1}<\infty\ ,\ k=2,3;\ \tau_{i5}=\infty$$

Moreover, fixed-effect order logit assumes that ε_{itg} is iid with standard normal logistic cumulative density function:

$$(6)\ F\left(\varepsilon_{itg}\middle|X_{it},\alpha_i\right)=F\left(\varepsilon_{itg}\right)=1/(1+\exp\left(-\varepsilon_{itg}\right))\equiv\varphi(\varepsilon_{itg})$$

So then in this given model the probability of observing outcome k for household *i* at time *t* is therefore

$$(7)\ \Pr(FSS_{it}=k)=\Pr\left(\tau_{ik}<FSS_{it}^*\leq\tau_{ik+1}\right)$$

$$=\Pr\left(\tau_{ik}<\alpha_i+\beta_{it}DID_C+\gamma_{it}X_{it}+\varepsilon_{itg}\leq\tau_{ik+1}\right)$$

$$=\Pr\left(\tau_{ik}-(\alpha_i+\beta_{it}DID_C+\gamma_{it}X_{it})<+\varepsilon_{itg}\leq\tau_{ik+1}-(\alpha_i+\beta_{it}DID_C+\gamma_{it}X_{it})\right)$$

$$=\varphi\left(\tau_{ik}-\left(\alpha_i+\beta_{it}DID_C+\gamma_{it}X_{it}\right)\right)-\varphi\left(\tau_{ik+1}-\left(\alpha_i+\beta_{it}DID_C+\gamma_{it}X_{it}\right)\right)$$

This logit estimation is done on the full sample for each food sales tax variation as well as on the SNAP subsamples.

Additionally, I apply the synthetic control method (SCM) for food insecure households in Kansas, the state that experiences the most variation in food sales taxes in the sample. I apply the SMC for Kansas twice using separate donor pools each time and use the final treatment in Kansas of a food sales tax increase in the year 2015. As a more comprehensive test I conduct placebo-in-space tests for all states which did not experience food sales tax variation, and another for states with no food sales tax in the sample. I estimate the same synthetic control model on each state, assuming that it was treated in 2015. This examines whether other states from either donor pool—states with no food sales tax variation, or states with no food sales tax—experience similar effects on food insecure households as the treated state of interest Kansas. If similar effects are found in these placebo states, then the treatment effects may be due to some other latent factors outside of the food sales tax variation. Conversely if similar effects are not found it will provide further empirical support for any relevant effects found in the analysis.

1.4 Results

Results for the pooled OLS can be found in Table 1.5. This table contains three columns corresponding to three levels of food security (marginal, low, and very low) the results show that higher food sales taxes are associated with greater levels of marginal low and very low food security. Table 1.5 includes the levels of these tax rates, though similar results were found for the log of this rates. SNAP participation had a positive impact on all of these levels as well. The food price parity index which is used as a proxy for food price levels has a negative significant effect on the food insecurity measures which is counterintuitive. The index is used as a comparative measure for food prices by state. In the primary model regressions, the coefficients for this measure are positive, this negative estimate in the pooled OLS may be due to capturing movements in both directions as it affects the food price levels. The number of children in a household increase the levels of low and very low food security, consistent with the idea that more children may lead to more pressure on resources resulting in higher levels of food insecurity. Likewise, the total number of family members for marginal and low food security classifications show a positive impact indicating as more individuals enter a household the food security levels increase. These estimates are consistent with previous literature and expectations.

I will first be reporting the DID results for the food insecure household measure, followed by the full sample fixed-effect order logit DID results and their marginal effects at the sample means. Next, I will discuss the SNAP subsamples for both types of model, concluding with the placebo results and synthetic controls.

1.4.1 Food Insecure Households

First, I consider the effects of food sales tax changes on the probability of households being classified as food-insecure. Table 1.6 shows the DID coefficient results for this measurement. Table 1.6 has three column: the full sample, subsample of households at 300% of the federal poverty line or below and the subsample of households at 185% of the federal poverty level or below. Each row corresponds to a different DID treatment effect.

For a food sales tax decrease, in the full sample the probability of being a food-insecure household significantly decreased by 4.97 percentage points, or 39.4% relative to the mean. For households at three hundred percent or below of the federal poverty line the probability decreased by 4.88 percentage points or 22.1%. The tax increase treatment results are found in row two and three of Table 1.6. The second row shows the effects for the increase with a refundable tax credit, for which the subsample of 300% of the FPL. There is an increase in the probability of being food-insecure of 9.77 percentage points (44.21%) and households at 185% of the FPL show a 15 percentage-point increase in the probability of being classified as a food insecure household, or 54.43% increase relative to the mean. The third row shows the effect of a tax increase when there is no refundable tax credit, finding for the 185% FPL subsample an increase of 3.67 percentage points (13.3%).

1.4.2 Fixed Effect Ordered Logit Model

Table 1.7 reports the FE ordered logit DID effects for a food sales tax decrease, and increase. The sign of the regression coefficients here can be interpreted as determining if the latent categorical dependent variable increases with the regressor. That is if it is positive it decreases the probability of being in the lowest category (very low food security) and increases the probability of being in the highest category (high food security). The opposite is true for a negative effects. I find a positive and significant effect resulting from a food sales tax decrease meaning their probability of being in a higher food security category increases when the food sales tax decreases. Likewise, I find a negative effect for a food sales tax increase, which increases the probability of being in the lowest category of food security. These results are difficult to interpret in more detail.

To examine these results more fully I have generated the marginal effects of this DID treatment for each category at the sample means. Table 1.8 shows these effects, where each row is a different category of food security and each column is a DID treatment. Using these marginal effects from the sample mean, I find that a decrease in food sales taxes causes a decrease in the probability of experiencing very low, low, and marginal food security levels by 6.94%, 8.13% and 0.26% respectively when considered at their sample means. A decrease in the food tax also shows an increase in the probability of being categorized as high food security by 15.33% relative to its sample mean. Conversely, I find an increase in the food sales taxes increase the probability of being very low, low, and marginally food secure by 7.36%, 8.61% and 0.28% respectively, and decreasing the probability of being categorized as having high food security by 16.25%. These marginal effects illustrate the movements of households treated with these difference tax variations showing that as food sales taxes decrease household's food security increases, and as food sales taxes rise household food security falls.

1.4.3 SNAP Participation Subsample

Now I will consider the impact of food sales taxes by SNAP participation subsamples for both models. Table 1.9 reports the results for the DID coefficients for each DID treatment on food insecure households: column one shows results for SNAP recipients and column two for non-SNAP recipients who are at or below 300% of the federal poverty line. Due the restrictions in the data from CPS, the subsamples which are covered by SNAP recipients are small relative to the full sample and subsample of non-SNAP recipients. For the food insecure households with no SNAP participation I find that a decrease in food sales taxes results in a reduction in the probability of being food insecure by 9.72 percentage points which is a decrease of 58.6% relative to the mean. No other results were found using the linear probability diff-in-diff model on food insecure households.

Table 1.10 shows results for the FE order logit model by SNAP participation subsamples. The first two columns show results for SNAP recipients and the last two columns show results for non-SNAP recipients. A food sales tax increase with SNAP shows a negative effect, increasing the probability of being in the lowest food security category. A food sales tax decrease shows a positive effect for non-SNAP recipients, showing an increase in the probability of being in a higher food security category. These results are consistent with the main full and income-based subsamples. Table 1.11 reports the marginal effects at the sample means for these results. For a food sales tax decrease with no SNAP, I find that the probability of being in either very low, low, or marginal food security category decrease by 18.3% , 20.46% and 0.96% respectively while the probability of having have food security increases by 39.54% relative to their sample means. The marginal effects found for a tax increase with SNAP are very large. For the categories relating to food insecurity (low and very low) the probabilities of being in those groups increases by 221.4% for very low and 171.4% for low food security relative to their respective means. The marginal food security categories show an 82.73% decrease in probability relative to its mean and the high food security group shows a decrease of 310.2% relative to its mean.

1.4.4 Placebo Regression and Synthetic Control

Due to the weakly balanced panel is it is difficult to directly measure the common trends assumption for the treatment. For this reason, I conduct a falsification placebo regression on utilizing state corporate tax increases and decreases to evaluate if there are any signification effects in the treatment on my food insecurity measure. For the placebo regressions I have followed the same specification as outlined previously for the food insecurity measure. This means that I use the same control and dependent variables; however, the policy change is the placebo treatment of state corporate tax rates. Treatment groups are defined by the first instances of changes the state corporate tax either increases or decreases with controls being those states which do not experience any change in the corporate tax. The structure of these placebo regressions and their control and treatment groups follows very closely to the primary specifications of this analysis.

Table 1.12 reports the DID coefficients for the various state corporate tax rate placebo regressions with their accompanying standard errors and sample observation sizes for both of the primary dependent variables of interest. Columns one, two, and three show the results for food insecure household measurement for the full sample, the subsample at 300% of the federal poverty line, and 185% of the poverty line, respectively. Considering first the decrease in corporate tax treatment I find no significant effects relating to this treatment for food insecure households. Similarly, all but one estimate for the increase in corporate tax rate treatment are found to be statistically insignificant. A marginally significant ($p<.10$) result is found in column two showing a decrease in the probability of being a food-insecure household for the subsample of households at 300% of the federal poverty line or below. This result is significant at the ten percent level and goes in the opposite direction as expected.

Given the many policy changes in Kansas, I use two synthetic control model to examine the effect of these food tax changes on food insecurity. The first synthetic Kansas was created drawing from the pool of states with no variation in food sales taxes during the sample period, the second drawing only from states where no food sales tax exists during the sample period. Resulting weights for each pool of donor states making up the two synthetic Kansas can be found in Table 1.13. In implementing the method, I incorporated aggregate data from the University of Kentucky's welfare data, along with collapsed household- level means of individual level CPS data of the control variables used in the primary specifications. These aggregate-level data allowed for the inclusion of state level controls of log of the state gross GDP, unemployment rates, log of SNAP recipients, log of state population and the percentage of low-income uninsured children. A complete list of included control variables used in the creation of both Synthetic Kansas can be found in Table 1.14, which also illustrates the relatively good fit when comparing these controls and the real state of Kansas.

Figure 1.3 and Figure 1.4 shows the initial models, illustrating either a significant or similar effect on food insecure households after treatment when comparing Kansas to its synthetic control. For a tax increase, food insecurity levels increase, and do so at a much faster rate. Tax decreases in Kansas in the year 2013 are shown in green and exhibit a decrease in the proportion of food-insecure households which then rises again with the tax increase in 2015. Figures 1.5 and 1.6 show the placebo-in-space synthetic controls for Kansas from the treatment relative to the synthetic

Kansas where treatment effects are slightly higher than those from the placebo group with Kansas representing a kind of outlier from other states in terms of their variation.

1.5 Discussion

This study has examined the effect of food sales taxes on the likelihood of being in a food insecure household. I found that decreases in the food sales tax reduced the likelihood of being in a food insecure household. This supports my hypothesis that these food taxes which in practice result in higher food prices can adversely affect household's food security. This is true particularly for these effects where found within the 300% of the federal poverty line subsample, suggesting that decreases in taxes reduce the likelihood of these lower income households to experience food insecurity. The effect of a food sales tax decrease is also larger for those who do not receive SNAP. This suggest that the SNAP program shields individuals from the effects of these taxes, as well as supporting findings in the literature of SNAP improving food security levels among low-income households.

Decreases in the food sales tax result in a lowering of the price of food paid by households, which mitigates one of the driving factors of food insecurity, the cost and availability of food to households to meet nutritional needs. This effect is the only one seen at the full sample as well as at lower income subsamples where expected. Additionally, the marginal effects for decreasing the food sales tax rate are very clear and straight forward. As food taxes decrease households are less likely to be categorized as experiencing lower levels of food security and more likely to move towards a higher level of food security. These effects are consistent as well for those households which have no SNAP benefit and would therefore be subject to these food taxes and the effect they would have on food prices for those households.

Increases in food sales taxes, however, result in higher effective food prices for households. An increase in food sales tax leads to a higher likelihood of a household being food-insecure, results which primarily appear among low-income subsamples. These tax increases also increase the likelihood of moving to a relatively worse level of food security. On the margin households experiencing these increases are more likely to fall into the two categories used to define food insecurity (low and very low food security) and decreases the likelihood of being categorized as having high food security. These effects are true as well for households participating in SNAP which shows very large effects on the probability of being in the food-insecure categories.

The empirical results of the study all point to the effect these food sales taxes have on household's food security. Decreases result in the more desirable outcome of lower food insecurity while increases lead to higher likelihood of food insecurity. SNAP appears to shield households from these effects, but not completely. Even SNAP participants when viewed from the marginal effects see increases in the two lowest food security categories when faced with higher food sales taxes. The SNAP program expects households to pay 30% of their net income on food and provides a benefit above that to meet the government set requirement for meal plans. SNAP benefits are received monthly and once they are used no other benefits are given for the month. If these benefits do not sustain households for the whole period between payments, these households too would be subject to these food sales taxes and therefore higher prices. Researchers have shown that by the end of the second week in a given month, SNAP households have redeemed over three-quarters of their benefits (Tiehen et al., 2017). This present additional research question into the role of these taxes in state funding, and the uses of SNAP in food insecurity mitigation. Using Kansas as an example for every 1% cut to food sales taxes the state fund would result in a decrease of around

$65 million (Shorman, 2018). Additional research could help to illuminate policy considerations in addressing these losses in revenue and gains to food security.

Kansas experiences both increases and decreases in the sample period and experiences changes to its food tax credit refundability policy, which is designed to protect lower-income households from food sales taxes. The removal of this tax rebate was associated with increases in food insecurity among the lowest-income groups. This highlights Kansas as a particularly interesting case study for additional analysis in this area, which could shed light on the influence of food tax rebate policies on lower income household food security levels.

1.6 Conclusion

This study analyzes the potential influences that grocery food sales taxes may have on the incidence of food insecurity, both in overall food insecurity levels and marginal changes in relative food security. Using data from two CPS supplements and a constructed data set of time varying state food sales taxes data, I find significant effects of food sales taxes on food insecurity. This analysis finds that as food sales taxes decrease, the probability of households being food-insecure also decreases, particularly for low-income groups and households that do not receive SNAP benefits. Similarly, the marginal effects of food tax decreases lower the probability of households being food-insecure with tax increases raising the probability of households being food-insecure.

These results suggest that food sales tax increases the price of food, and as a result affects levels of food security for lower income households. The study also sheds light on an area for additional research via a case study in Kansas during the period of 2008 to 2013 where many policy changes around sales tax and food credit rebates took place. Additional study into this particular case may offer an opportunity to better understand how these policies and food sales tax schemes influence food security levels for at-risk households.

1.7 Tables and Figures

Table 1.1: Summary Statistics

	Full Sample		SNAP Non-Participants		SNAP Participants	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Food Insecure Households (%)	12.6	(0.33)	16.6	(0.37)	43.8	(0.50)
State Grocery Tax Rate* (%)	3.543	(1.94)	3.666	(1.96)	3.699	(1.97)
Age	52.34	(16.89)	54.11	(16.83)	47.76	(16.82)
Family Size	2.402	(1.42)	2.256	(1.52)	2.767	(1.75)
Number of Children	0.697	(1.08)	0.663	(1.13)	1.121	(1.35)
TANF (%)	0.629	(0.08)	0.211	(0.05)	5.65	(0.23)
Refundable State EITC (%)	40.9	(0.49)	38.4	(0.49)	40.9	(0.49)
State Min Wage	7.593	(1.13)	7.515	(1.09)	7.598	(1.10)
Food Price Parity	0.994	(0.05)	0.99	(0.05)	0.99	(0.05)
Own House (%)	69.7	(0.46)	61.2	(0.49)	34.4	(0.48)
Female	0.498	(0.50)	0.551	(0.50)	0.657	(0.48)
White	0.837	(0.37)	0.82	(0.38)	0.694	(0.46)
Black	0.097	(0.30)	0.114	(0.32)	0.229	(0.42)
Hispanic	0.094	(0.29)	0.123	(0.33)	0.175	(0.38)
US Citizen (%)	94.7	(0.22)	92.7	(0.26)	91.5	(0.28)
Highschool Degree (%)	27.7	(0.45)	35.3	(0.48)	36.4	(0.48)
Some College (%)	18.6	(0.39)	20.5	(0.40)	20.2	(0.40)
Less than Highschool (%)	10.1	(0.30)	15.3	(0.36)	27.8	(0.45)
Any College Degrees (%)	43.6	(0.50)	28.9	(0.45)	15.6	(0.36)
Any physical or cognitive difficulty (%)	15.9	(0.37)	21.3	(0.41)	33	(0.47)
SNAP Participant (%)	9.63	(0.30)				
Marital Status (Married %)	51.9	(0.50)	39.3	(0.49)	26.2	(0.44)
Employed (%)	59.5	(0.49)	45.9	(0.50)	33.3	(0.47)
NILF (%)	36.8	(0.48)	49.8	(0.50)	57.6	(0.49)
Refundable Food Credit (%)	4.98	(0.22)	5.49	(0.23)	4.98	(0.22)
Sample Size	99,212		36,883		9,550	

* Means are based on non-zero tax rates.

Table 1.2: Summary Statistics by Percent of Federal Poverty Line

	Full Sample		300 % FPL		185% FPL	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Food Insecure Households (%)	12.6	(0.33)	22.1	(0.42)	27.6	(0.45)
State Grocery Tax Rate* (%)	3.543	(1.94)	3.674	(1.96)	3.717	(1.97)
Age	52.34	(16.89)	52.82	(18.83)	52.52	(19.23)
Family Size	2.402	(1.42)	2.355	(1.58)	2.3	(1.61)
Number of Children	0.697	(1.08)	0.756	(1.19)	0.773	(1.24)
TANF (%)	0.629	(0.08)	1.33	(0.12)	2.06	(0.14)
Refundable State EITC (%)	40.9	(0.49)	38.9	(0.49)	38.7	(0.49)
State Min Wage	7.593	(1.13)	7.528	(1.09)	7.527	(1.09)
Food Price Parity	0.994	(0.05)	0.99	(0.05)	0.99	(0.05)
Own House (%)	69.7	(0.46)	55.7	(0.50)	48.2	(0.50)
Female	0.498	(0.50)	0.573	(0.50)	0.61	(0.49)
White	0.837	(0.37)	0.795	(0.40)	0.765	(0.42)
Black	0.097	(0.30)	0.137	(0.34)	0.161	(0.37)
Hispanic	0.094	(0.29)	0.133	(0.34)	0.152	(0.36)
US Citizen (%)	94.7	(0.22)	92.5	(0.26)	91.1	(0.29)
Highschool Degree (%)	27.7	(0.45)	35.6	(0.48)	36.2	(0.48)
Some College (%)	18.6	(0.39)	20.4	(0.40)	19.6	(0.40)
Less than Highschool (%)	10.1	(0.30)	17.9	(0.38)	22.8	(0.42)
Any College Degrees (%)	43.6	(0.50)	26.1	(0.44)	21.5	(0.41)
Any physical or cognitive difficulty (%)	15.9	(0.37)	23.7	(0.43)	28.3	(0.45)
SNAP Participant (%)	9.63	(0.30)	19.7	(0.40)	29.5	(0.46)
Marital Status (Married %)	51.9	(0.50)	36.5	(0.48)	29.1	(0.45)
Employed (%)	59.5	(0.49)	43.2	(0.50)	34.7	(0.48)
NILF (%)	36.8	(0.48)	51.5	(0.50)	58.9	(0.49)
Refundable Food Credit (%)	4.98	(0.22)	5.37	(0.23)	5.3	(0.22)
Sample Size	99,212		45,958		27,462	

* Means are based on non-zero tax rates.

Table 1.3: Treatment Groups by Tax Variation

	Tax Decreased	Tax Increased
Arkansas	2010	
Kansas	2013	2010, 2015
South Carolina	2009	
South Dakota		2018
Tennessee	2013	
Utah	2008	
West Virginia	2012	

Control groups: decreased taxes omit KS, SD.

Increased taxes omit AR, SC, TN, WV.

Table 1.4: State Food Tax Rate

State	Food Sales Tax Rate				Years of Tax Change
Alabama	4.00%				
Arkansas	3.00%	2.00%	1.50%		2010,2014
Hawaii	4.00%				
Idaho	6.00%				
Illinois	1.00%				
Kansas	5.30%	6.30%	6.15%	6.50%	2010,2013,2015
Mississippi	7.00%				
Missouri	1.225%				
Oklahoma	4.50%				
South Carolina	3.00%	0.00%			2009
South Dakota	4.00%	4.50%			2018
Tennessee	5.50%	5.00%	4.00%		2008,2013
Utah	1.75%				
Virginia	2.50%				
West Virginia	3.00%	1.00%	0.00%		2012,2013

Table 1.5: Pooled OLS

VARIABLES	(1) Marginal Food Security	(2) Low Food Security	(3) Very Low Food Security
State grocery tax rate	0.00123*	0.00150**	0.00115*
	(0.000700)	(0.000676)	(0.000596)
Refundable food tax credit	-0.00140	-0.00172	0.00155
	(0.00536)	(0.00509)	(0.00461)
Food price parity	-0.0782***	-0.0457**	-0.0431**
	(0.0215)	(0.0210)	(0.0174)
Number of children	0.000981	0.00973***	0.00571***
	(0.00217)	(0.00230)	(0.00169)
Number of family members	0.0111***	0.00941***	-0.00267*
	(0.00189)	(0.00199)	(0.00145)
Any physical or cognitive difficulty	0.00957**	0.00526	0.0332***
	(0.00403)	(0.00385)	(0.00346)
Receives TANF	-0.00316	0.0125	-0.00440
	(0.0164)	(0.0192)	(0.0165)
Receives SNAP	0.0427***	0.0974***	0.0890***
	(0.00470)	(0.00503)	(0.00458)
Constant	0.267***	0.294***	0.216***
	(0.0241)	(0.0233)	(0.0195)
Observations	99,157	99,157	99,157
R-squared	0.040	0.085	0.091

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Std Errors Clustered on Household CPSID
Similar results found for Log (State grocery tax rate)

Table 1.6: DID results for Food Insecure Households By % of Poverty Line

VARIABLES	(1) Food Insecure Household	(2) Food Insecure Household	(3) Food Insecure Household
Tax Decrease DID	-0.0497***	-0.0488**	-0.0363
	(0.0118)	(0.0198)	(0.0493)
	[96,134]	[44,370]	[26,482]
DID Increase Yes Refundable Credit	0.0251	0.0977**	0.150**
	(0.0232)	(0.0427)	(0.0587)
	[93,032]	[42,437]	[25,256]
DID Increase NO Refundable Credit	-0.00252	-0.00887	0.0367***
	(0.00559)	(0.0186)	(0.00875)
	[93,032]	[42,437]	[25,256]
300 & Below FPL		YES	
185& Below FPL			YES

Robust standard errors in parentheses, Observations in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: FE Ordered Logit DID Results

VARIABLES	(1) Food Security Status	(3) Food Security Status
Tax Decrease DID	0.663***	
	(0.209)	
Tax Increased DID		-0.702**
		(0.283)
Observations	16,428	15,684

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.8: FE Ordered Logit Marginal Effects at the Sample Means

	(1) Tax Decrease DID	(3) Tax Increase DID
Very Low Food Security	-0.0694***	0.0736**
	(0.0219)	(0.0297)
Low Food Security	-0.0813***	0.0861**
	(0.0257)	(0.0347)
Marginal Food Security	-0.0026***	0.0028**
	(0.0008)	(0.0011)
High Food Security	0.1533***	-0.1625**
	(0.0484)	(0.0655)
Observations	11,262	10,736

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.9: SNAP Subsample DID Results

VARIABLES	(1) Food Insecure Household	(2) Food Insecure Household
Tax Decrease DID	-0.0136	-0.0972***
	(0.110)	(0.0349)
	[9,237]	[35,600]
DID Increase Yes Refundable Credit	0.103	0.0472
	(0.135)	(0.134)
	[8,719]	[34,164]
DID Increase NO Refundable Credit	-0.0268	0.0135
	(0.111)	(0.110)
	[8,719]	[34,164]
SNAP	YES	NO

Robust standard errors in parentheses. Observations in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.10: Fixed effect Ordered Logit SNAP Subsample DID Results

VARIABLES	(1) Food Security Status	(2) Food Security Status	(1) Food Security Status	(2) Food Security Status
Tax Decrease DID	0.118		1.697***	
	(0.593)		(0.356)	
Tax Increased DID		-15.78***		-0.0899
		(1.910)		(0.395)
Observations	2,358	2,198	5,880	5,610
SNAP	YES	YES	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.11: SNAP Subsamples Fixed effect Ordered Logit Marginal Effects at the Sample Mean

	Tax Decrease DID		Tax Increase DID	
	(1)	(2)	(5)	(6)
	SNAP	NO SNAP	SNAP	NO SNAP
Very Low Food Security	-0.0167	-0.1813***	2.214***	0.0096
	(0.0844)	(0.0380)	(0.2680)	(0.0421)
Low Food Security	-0.0125	-0.2046***	1.714***	0.0108
	(0.0632)	(0.0429)	(0.2075)	(0.0474)
Marginal Food Security	0.0063	-0.0096***	-0.8273***	0.0005
	(0.0320)	(0.0020)	(0.1001)	(0.0024)
High Food Security	0.0229	0.3954***	-3.102***	-0.0209
	(0.1156)	(0.0830)	(0.3754)	(0.0920)
Observations	1,664	3,940	1,540	3,770

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 1.12 State Corporate Tax Placebo DID Results

VARIABLES	(1) Food Insecure Household	(2) Food Insecure Household	(3) Food Insecure Household
Tax Decrease DID	0.00434	-0.00440	-0.0202
	(0.0120)	(0.0266)	(0.0304)
	[92,279]	[42,768]	[25,589]
Tax Increased DID	-0.00950	-0.0659*	-0.0262
	(0.0126)	(0.0346)	(0.0626)
	[99,157]	[45,903]	[27,407]
300 & Below FPL	-	YES	-
185& Below FPL	-	-	YES

Robust standard errors in parentheses. Observations in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.13: Synthetic Kansas Donor Sampling Weights

I. Synthetic KS (No Variation)			II. Synthetic KS (No Food Tax)		
FIPS ID	State	Weight	FIPS ID	State	Weight
13	Georgia	0.47	2	Alaska	0.268
16	Idaho	0.15	11	D.C.	0.159
32	Nevada	0.10	13	Georgia	0.417
40	Oklahoma	0.02	30	Montana	0.002
41	Oregon	0.18	32	Nevada	0.011
56	Wyoming	0.07	41	Oregon	0.002
			48	Texas	0.001
			56	Wyoming	0.141

Table 1.14: Treated vs Synthetic Kansas

	Treated	I. Synthetic KS	II. Synthetic KS
State Grocery Tax Rate	5.97	1.03	0.00
Refundable Food Credit	0.71	0.18	0.00
% Low Inc. Uninsured Children	4.80	6.85	5.66
Poverty Rate	13.47	15.37	14.90
Log Population	14.87	15.31	14.55
Log Gross State GDP	11.81	12.20	11.86
Log state SNAP Recipient	12.49	13.33	12.50
Unemployment Rate	5.80	8.32	7.85
Log State Min. Wage	1.69	1.82	1.82
Refundable State EITC	1.00	0.20	0.16
Food Price Parity	0.93	0.97	1.01
Own House	0.61	0.70	0.68
Employed	0.67	0.59	0.64
NILF	0.31	0.37	0.32
Log Household Income	10.54	10.54	10.65
Family Size	2.37	2.47	2.37
Female	0.49	0.50	0.50
White	0.86	0.78	0.71
Black	0.06	0.17	0.21
Hispanic	0.06	0.08	0.06
US Citizen	0.95	0.95	0.95
Less than High school	0.08	0.12	0.10
High school Degree	0.27	0.28	0.28
Some College	0.23	0.23	0.19
Any College Degrees	0.42	0.37	0.43
Marital Status	0.48	0.53	0.48
Work Limiting Disability	0.14	0.14	0.13
Any physical or cognitive difficulty	0.19	0.16	0.15
Elderly Present in HH	0.22	0.27	0.24
Difficulty Present in HH	0.26	0.27	0.24
Work Limiting Dis. HH	0.20	0.24	0.21
SNAP Recipient	0.11	0.12	0.09

Figure 1.1: Illustration of CPS Household Sampling

Starting Year and Month

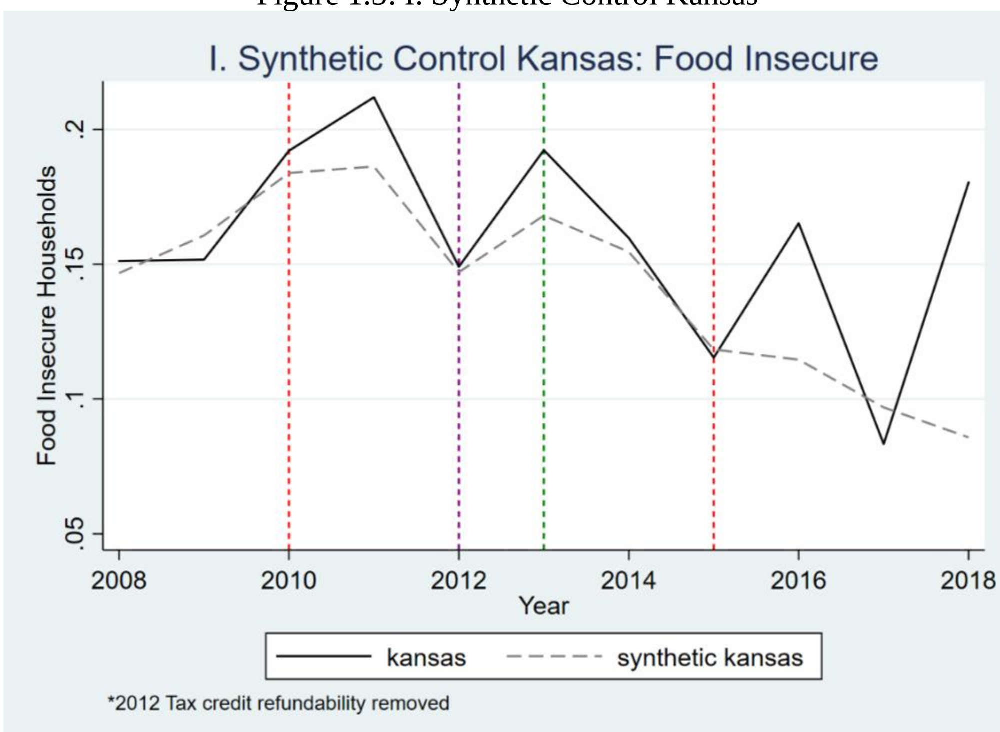
2016

Survey Year and Month		July	August	September	October	November	December
		Number indicates month in the sample					
2016	July	1					
2016	August	2	1				
2016	September	3	2	1			
2016	October	4	3	2	1		
2016	November		4	3	2	1	
2016	December			4	3	2	1
2017	January				4	3	2
2017	February					4	3
2017	March						4
2017	April						
2017	May						
2017	June						
2017	July	5					
2017	August	6	5				
2017	September	7	6	5			
2017	October	8	7	6	5		
2017	November		8	7	6	5	
2017	December			8	7	6	5
2018	January				8	7	6
2018	February					8	7
2018	March						8

Figure 1.2: Food Status Movements for Households
Observed Twice

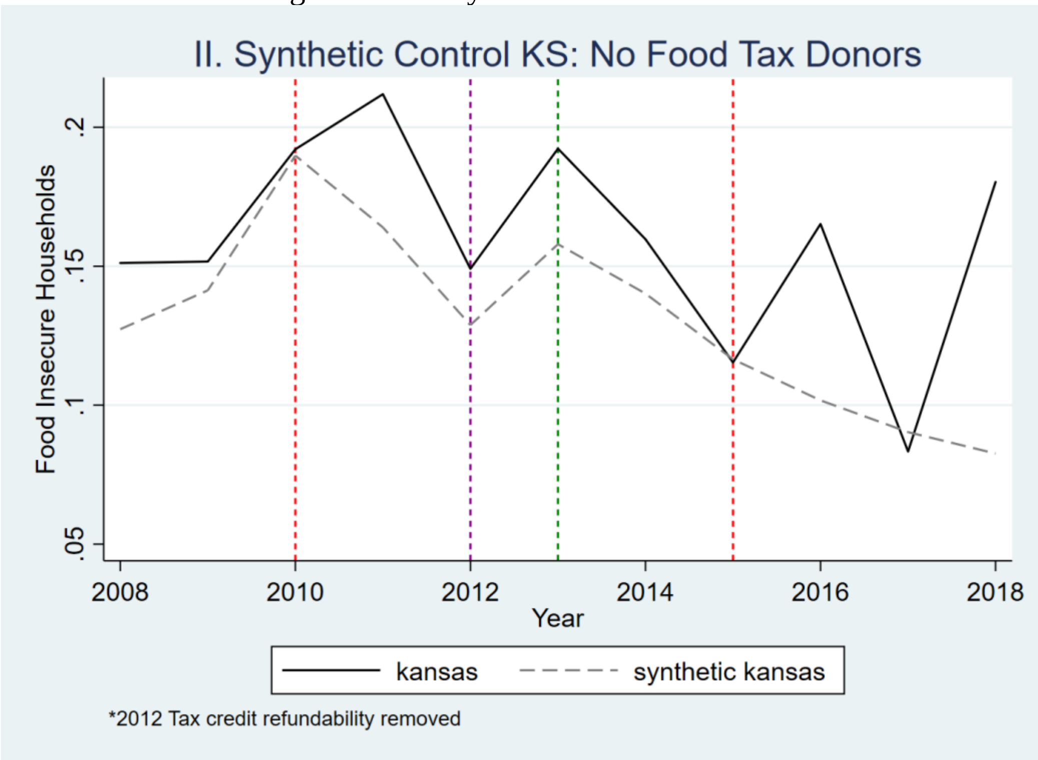
Food status: Period 1 \ Food Status: Period 2	VL	L	M	H
VL	515	331	148	312
L	270	574	421	776
M	109	318	460	1,305
H	216	578	1,041	21,118

Figure 1.3: I. Synthetic Control Kansas



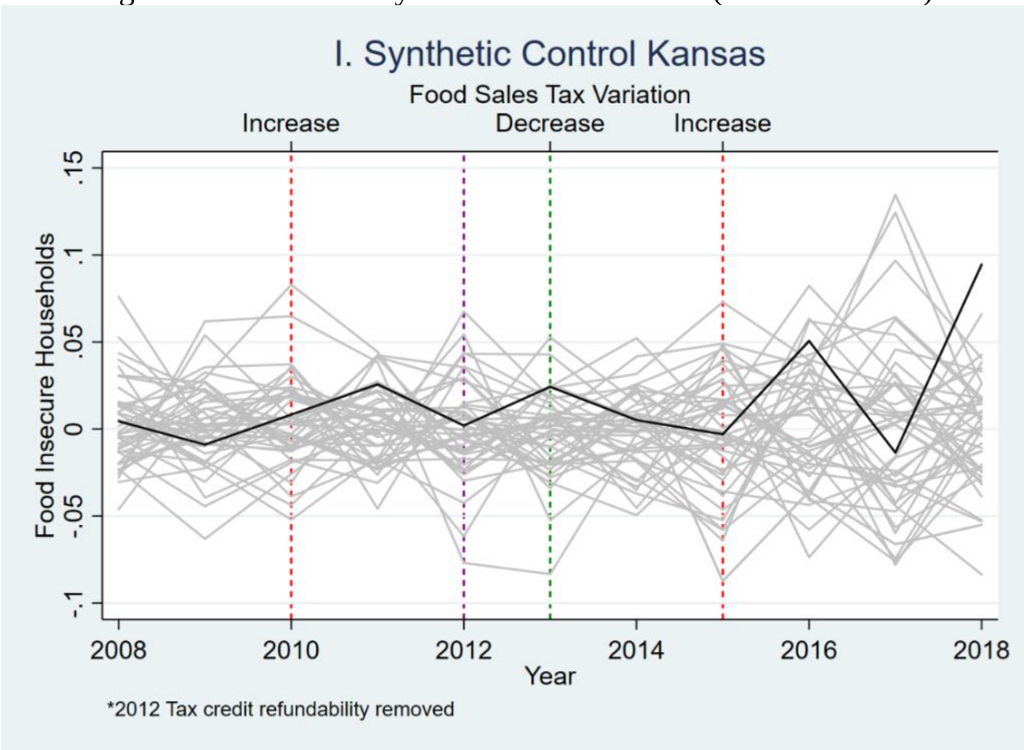
Red indicates an increase in sales tax, green a decrease.

Figure 1.4: II. Synthetic Control Kansas



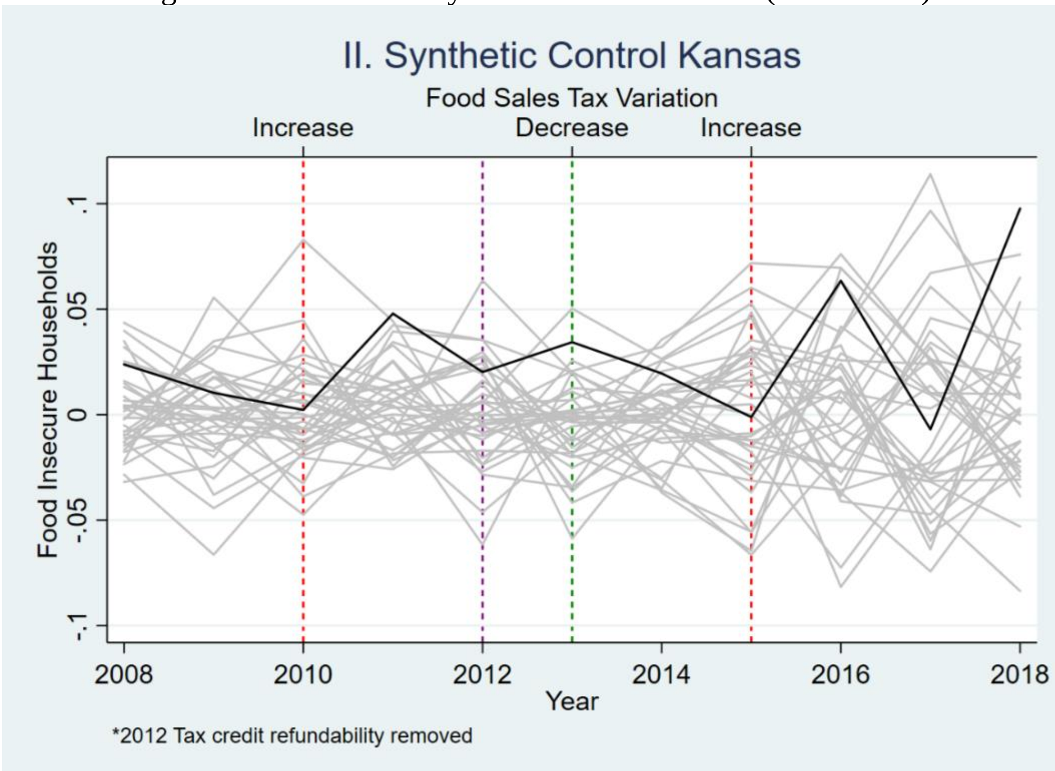
Red indicates an increase in sales tax, green a decrease.

Figure 1.5: Placebo I. Synthetic Control Kansas (no tax variation)



Red indicates an increase in sales tax, green a decrease.

Figure 1.6: Placebo II. Synthetic Control Kansas (no food tax)



Red indicates an increase in sales tax, green a decrease.

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Chapter 2

Nuisance Laws and their Impact on Legal and Extra-legal Evictions

2.1 Introduction

Third-party policing laws are a means of crime prevention which in the last decade or two have become more and more prevalent in many of the largest cities in the United States. Third-party policing laws refer to policies and city ordinances in which police attempt to persuade or force third parties, such as landlords, to take some level of responsibility in preventing criminal activities. The most wide spread example of third-party policing is the nuisance property ordinances. As of August 2017, nearly all of the top 40 most populous cities in America have some form of these laws on the books.

Nuisance property ordinances require landlords to regulate tenant behavior to eliminate what are defined as “nuisances”. What is determined to be a nuisance can vary by city ordinance. Nuisance ordinances are generally defined within a certain range of time for a number a police responses to a certain residence responding to a “nuisance activity.” These activities can range anywhere from simple police call to loud noises, and all the way up to explicit illegal activity. Some cities even define domestic violence calls as a nuisance activity. Although there are instances of cities going back and allowing exemptions to domestic violence via amendments. No matter how they may be defined, the ordinances typically have the goal of discouraging direct police involvement or responses to the property. Failure to do so results in a “nuisance citation” to the landlord. Landlords then in many cases are required to “abate” the unwanted behavior of tenants or face some form of punishment. These punishments could be in the form of fines, property foreclosure, or possible incarceration (Fais 2008).

In order to avoid these types of punishments some landlords take steps to limit the amount that tenants call the police, and in some instances, may even just evict the tenant as a form of abatement (Desmond and Valdez, 2013). This paper examines the impact of nuisance ordinances on evictions both legal and extra-legal finding that they decrease legal evictions, increases the number of individuals who moved in the last year, but have an insignificant decline in homelessness.

2.2 Literature Review

The United States is experiencing what many have called an “eviction crisis” which has spurred researchers in attempting to examine the cause for the rise and ultimately the consequences of these eviction. A prominent pioneer in this effort, a sociologist from Harvard University, Matthew Desmond and his collaborators have in recent years examined the many outcomes of evictions. They find evictions have a disproportionate effect on low-income families, particularly poor African American single mothers. Desmond, and his collaborators highlights the worsening impacts of eviction on both financial and physical wellbeing and mental hardship. They find that these individuals are more likely to report depression have worse health outcomes, and experience higher rates of material hardship when compared to their peers (2015).

Desmond also finds that various characteristics affect the likelihood of being evicted in general. These being family size, job loss, and crime and eviction rates in a neighborhood (2016). Linking evictions to these nuisance ordinances by looking specifically in a Milwaukee, his study found that properties in African American neighborhoods received a disproportionally high amount of these citations. In the case of Milwaukee, they found a third of the cases involved domestic violence disputes which then resulted in evicting battered women (2014). This nuisance law in Milwaukee has since been amended to include domestic violence exemptions which has also occurred in other cities around the US; however, there are still cities which do not have such exemptions.

The causes and outcomes of evictions have yet to see much attention from the economic perspective. A working paper headed by a group of economists entitled “Does eviction create poverty? Quasi-experimental evidence from Cook County, IL" is one of the few which attempts to tackle some of these related questions around evictions from an economic viewpoint (Humphries, Mader, Tannenbaum and van Dijk, 2018). This paper explores the idea posed in the sociology field that eviction is a cause, and not only a consequence of poverty. They look specifically at Cook County IL from 2000-2006 and their eviction cases. They linked credit bureau and payday loans data with county court cases, allowing them to link evictions to financial strain. They find evidence of significant financial distress and an increase in the demand for payday loans leading up to eviction courts regardless if they are eventually evicted or not. They also estimate the causal effects of eviction orders on individual financial strain, finding increases to financial strain and a reduction in consumption.

This paper on the connection between nuisance property ordinances and legal and extra-legal evictions seeks to contribute to this growing economic literature around evictions, particularly in relation to city ordinances and the possible outcomes on housing displacement. By using the variation in the effective dates for city nuisance ordinances enactment coupled with difference-and-difference models the paper examines how these cities’ ordinances impact their respective evictions levels. In order to better understand the impact of these laws outside of only legal eviction levels two additional factors will be explored—renter tenure and homelessness. Renter tenure measures the number of individuals who moved into their current rental property within a year, and homelessness being point and time estimates for related cities/counties. These two measures allow one to see how the various ordinances may have affected other extra-legal evictions via some form of housing displacement.

The paper proceeds as follows: Section 2.3, in which I discuss the primary sources of the data as well as the variables used in the analysis. Section 2.4 describes the econometric approach and Section 2.5 presents the results. The paper concludes in Section 2.6.

2.3 Data

The data used are from various sources which are then merged together into a single data set. The sources are the Policy Surveillance Program, The Princeton Eviction Lab, The American Community Survey (ACS), and The U.S. Department of Housing and Urban Development (HUD) Point-in-Time Homelessness estimates data.

The Policy Surveillance Program is a program which uses legal mapping in order to help policymakers, researchers and others to better understand what the laws are on a given topic and provide data to evaluate their impact. These data are used as a starting point and reference for the various city ordinances. They provide ordinances numbers and characteristics regarding the city property nuisance ordinances in the top 40 most populous cities in the US. They include data on the most recent iteration of these ordinances and their enactment as of August 2017. This information along with online supplements from Matthew Desmond's 2014 paper on nuisance laws serve as a launching point for scrubbing municipality codes to identify the initial enactment of the ordinances in each city, which are then later used as treatment dates.

The Eviction Lab at the University of Princeton is the first data set for evictions in America with data ranging from 2000-2016. The levels of evictions, which have been gathered from courts around the country, are pulled from these data as are many of the demographic and explanatory variables. Namely population, median household income, median rent, median property value, poverty rates, and the percentage of the population which are white, African American, and Hispanic. These descriptive variables while gathered via the eviction lab data are derived using Census data from 2010 as well as corresponding five-year estimates from the ACS. The logs of evictions, population, median household income, median rent, and median property value have also been computed and included. In the data cities are matched to counties for the purpose of merging with the remaining data sets. Cities are matched via the proportion of a city residing in a given county—e.g. 99.6% Portland OR, resides in Multnomah County. All such matches if not 100% are above 90%.

The ACS data is used to account for renter tenure by county as well as some additional demographic data such as citizenship status and number of mothers in a residence. Given the data restrictions from the ACS in sample size as well as granularity, the dates of these data range only from 2005-2016. The primary variable of interest from these data are the number of individuals responding in the affirmative to having moved into their current rental residency within the last 12 months. These counts are then collapsed down to the county level rather than the city level. Additionally demographic variables taken from the ACS are also collapsed down as either counts or averages by county level. This limits the scope of the study to the cities in which there is a treatment during the 2005-2016-time frame. These data are matched with cities by counties as outlined above.

The final data set comes from the HUD point-in-time homelessness estimates. These data range from 2007-2018 which again limits the sample of treated cities available. This data includes estimates for total homelessness as measured at a single point in time; the last week of January in any given year. These counts include individuals who are homeless both in sheltered and unsheltered populations according to HUD. The counts are done at a level defined by what are referred to as Communities of Care (CoC) which typically correspond to counties and city metropolitan areas. For this study all CoC correspond to the specific large cities and their corresponding primary counties in question. These counts are then matched by county to the other data. Due to this CoC matching process as well as some of the variation in collection across CoCs for these estimates, it is very likely that these homelessness estimates may underestimate the true level of homelessness in any given city. However more granular homelessness data outside of state levels are difficult to obtain, and are not readily or publicly available. Summary statistics for variables used in this analysis can be found in Table 2.1.

2.4 Empirical Model

The given data is a cross sectional panel by city or county, the specification used is difference-in-difference panel data fixed effects model. This model is used to exploit the quasi experimental nature of these city ordinances. In that there is one group of cities which has been exposed to the treatment of these city ordinances, and the other control group of cities which has not. This is used in an attempt to tease out any of the effects of the nuisance property ordinance on a treated city. The analysis is separated into three parts by sample and treatment groups, and each of the three parts implements the DID model specification using three different variables of interest: the log of evictions, log of renter tenure and the log of PIT homelessness. The first part of the analysis considers a smaller subsample of cities which align with data restrictions across the various sources. The second part expands the limited subsample by including more cities and counties into the analysis where possible.

The final part of the analysis attempts to address the staggered implementation bias of the treatment groups in the DID model. This is done by separating the various treated cities into four treatment groups. These groups are separated and grouped by treatment times for every 2-years, and those cities or counties are the only treated cities considered for that specification against the control group. Table 2.2 lists the control and treatment groups for the smaller subsample, showing treatment times, and for which set of data the cities have observations. Table 2.3 expands on this showing the complete expanded sample of cities as well as illustrating in which of the four treatment groups each city falls. The first DID specifications used in each part of the analysis is given by the following equation,

$$(1)\ Y_{it}=\alpha+\beta_{it}(\text{Treat*Time})_{l}+\gamma_{it}X_{it}+\text{City}_{t}+\text{Time}_{t}+\varepsilon_{it}$$

where Y_{it} is the dependent variable the log of one plus the eviction level for city i at time t. β_{it} represents the DID estimation coefficient and will estimate the treatment effect as the difference between the pre-treatment and post-treatment values of Y_{it}. The interaction term $(\text{Treat*Time})_{l}$ is the product of two dummy variables where treat is a one if a city at some time in the sample has a nuisance city ordinance, and time gives a value of one if during that year an ordinance was in effect. This interaction term defines the period of time for which each city was treated with effective nuisances' laws for any given year. To explore the relative time of the impact of the treatment effect a lag of this interaction will also be used where l= {0, 1, 2}. l=0 corresponds to the actual effective treatment year as found in the municipality. l=1 and l=2 corresponds to lagging this effective treatment time one or two years respectively.

For instance, if a city's law were effective in 2005 $(\text{Treat*Time})_{0}$ would be counted as being in effect and therefore the city would be in the treated group staring from 2005. For $(\text{Treat*Time})_{1}$ the period for the city in the treatment group would be starting in 2006 and so on. This lagging of the effective time of the ordinances allows to explore the possibility that results from the law do not become evident for some period of time have they come into effect.

The covariates of the model are given by X_{it}. They are the log of population, log of median household income, log median gross rent, log of median property value, the percentage of population that is White, African American, or Hispanic and the poverty rate log count of non us citizens and average number of mothers in the home. These are defined for i^{th} city at time t with γ_{it} reporting their corresponding coefficients. Followed finally by the city and time fixed effects and the residual error terms. Equation (2) follows this same specification construction and is given by:

$$(2)\ Z_{ct}=\alpha+\beta_{it}\ (Treat*Time)_{l}+\gamma_{it}X_{it}+City_{t}+\ Time_{t}+\varepsilon_{it}$$

where Z_{ct} is either the log of renter tenure or the log of the PIT homelessness for a given county c at time t. Renter tenure is defined here as the count from the ACS for total number of individuals having responded in the affirmative for to moving into their current rental residence within the last 12 months. This time frame is used as it reflects the shortest range of time asked by the ACS and coincides with the time frame most likely to be affected by a recent law change which may have led to renter displacement. PIT estimates are the logged estimates for CoC matched to counties as previously outlined.

2.5 Results

The results in presented in this section will be presented by parts outlined previously starting with the original smaller subsample followed by the extended sample and concluding with the treatment group analysis. Each part will present the results for fist evictions second renter tenure and conclude with the PIT homelessness results. Across each part of the analysis along with the DID coefficient reflecting the original treatment year, two further lagged DID coefficients were also considered. This was to investigate the possibility of effects taking one or two years after a law enactment to manifest.

2.5.1 Original Subsample

The first specification centers on the log of eviction levels as the dependent variable. The results for which are located in Table 2.4. The effect of treatment is found to be a large and statistically significant negative impact of on evictions. The effects grows in magnitude following one year after enactment before diminishing somewhat two years following the initial treatment. As the dependent variable is the log level of evictions the interpretation of the proportional change in the evictions variable when treatment occurs (i.e. $(\text{Treat*Time})_1=1$) is given by $((e^{\beta}-1)*100=\%\Delta Y$. Following this method of technical interpretation evictions fell 69.82% following treatment and fell by 73.29% one year following and fell 61.33% if considered two years after treatment. All of these decreases are statistically significant, but contradictory from expectations derived the literature. These unexpected result may be explained somewhat in part by the fact that reported evictions are via courts. That is to say that the data collection on evictions by the Eviction Lab gathers these data on legal evictions. Although care has been done to note periods of time which may result in lower estimates than normal, due to this data gathering process, a few small blips in low estimates at the census tract level may cause some amount of under reporting of eviction levels for a city in a given year.

For this sample of cities efforts were taken to ensure these low estimates where not numerous, but these limitations are present in the data. Notwithstanding these data restrictions, it may very well be that the level of legal evictions are decreasing following these types of laws and that other aspects of abatement plans are forcing tenants to leave locations before the legal need for a formal eviction is met. That is, that some forms of extra-legal evictions or pressures on tenants could be taking place resulting in tenants feeling the need to leave. This could be one explanation for these unexpected results. This in turn could lead to the overall renter environment in these areas being diminished, leading to individuals leaving the area of their own volition. These individuals would not be captured in this eviction data. To test this hypothesis the additional specifications on renter tenure and homelessness have been explored to examine these unforeseen outcomes, as well as expanding and segmenting the sample to account for possible biases in the data from the DID specification.

The estimations on renter tenure can be found in Table 2.5. The dependent variable here as mentioned before are the log count levels for individuals which have responded that they have moved into their current rental residence within the last 12 months. This is used to measure displacement and attempt to see how many people may have recently moved following the new law's enactment. Given that evictions have been shown to decline two years following treatment one would expect some small decline in the number of individuals who moved within the last year.

Here the initial treatment is not significant, and interestingly enough the two-year lagged estimates here are also insignificant, although both are positive. The effects of the renter tenure measure appear to be focused on one-year following the treatment. The one year lagged DID estimate is both positive and statistically significant. When comparing the only the magnitudes of the various DID estimates there is a significant spike in the level of individuals who report having moved in the last 12 months, one year following treatment. The magnitude then goes back down two years following this treatment. I note this as interesting due to the fact that the data does not differentiate the motivations for individuals to move. This increase in the magnitude of those moving one-year following treatment while remaining a positive effect may speak to the hypothesis of the treatment spurring an increase in the displacement of renters.

Focusing then on the one year following treatment results found in column two of Table 2.5, we see a positive DID coefficient of 0.105, or an increase of 11.07% indicating that a year following the enactment of property nuisance laws, there was an increase in the level of individuals which had recently moved into their rental residence during the previous 12-month period. This result supports the hypothesis of some kind of outside pressure for tenants to move outside of legal evictions. The one year after period also supports this idea as individuals responding the initial treatment date may have moved in prior to the law passing. Therefore, it would be expected to have most results of recent moves responding to treatment to be reported in the time period a year following the laws coming into effect.

Expanding on this extra-legal eviction displacement idea the final specification uses PIT homelessness estimates to attempt to see the effect the more extreme level of housing displacement homelessness. Results for which are found in Table 2.6. This third specification did not yield any significant results. A fact which I believe is caused by the potentially issues related to the quality of these data from HUD point-in-time estimates as outlined previously in the data section. The results are also somewhat confounding to the expectations. The results report decreases in homelessness initially of around 6.28% and a year following a decrease of 10.2%. Similar to the renter tenure there is no distinction between causes of homelessness in these data. To examine this result as well as the others the original sample was extended to include more cities for each specification where possible given data constraints.

2.5.2 Extended Subsample

In extending the sample of cities for the analysis, 9 cities were added to the original sample of 7 cities resulting in 16 total cities. Of the added 9 cities all were able to be applied to the eviction and homelessness analysis however only 4 of the 9 cities were able to be included in the renter tenure analysis due to constraints of the data and times of treatment. Results for the extended sample can be found in Table 2.7. None of these results were found to be statistically significant. For evictions and renter tenure the results maintained the same signs and held closely to a similar trend as before, where the results for PIT homelessness show similar magnitudes of effects but with a different sign now showing positive coefficients pointing to possible increased levels of homelessness.

The insignificant nature of the extended sample results is believed to be due to staggered implementation bias in the DID model specifications as many of the 16 cities considered have treatments coming into effect at different times. This particular type of bias has been brought to the forefront of DID analysis by recent papers by Andrew Goodman-Bacon (2021) and Brantly Callaway, Pedro H.C. Sant'Anna (2020). These papers show the potential for this bias in the DID specification when many treatments with varying timing are considered together, and offer insights on how to address these concerns

2.5.3 Treatment Group Analysis

To account for the staggered implementation bias, the treated cities were grouped together by 2-year increments in treatment. For example, those cities treated in 2005 and 2006 where grouped together for treatment in 2005, and those cities treated in 2007 and 2008 where similarly grouped and so on. This resulted in four different treatment groups which were then independently considered against the control group that had no nuisance laws to measure the impact of their respective treatment effects.

For evictions results are found in Table 2.8. For treatment groups 1 and 2, both showed similar significant and negative impacts on eviction levels following treatment. Group 1 had significant effects following treatment exhibiting a decrease in legal evictions of 14.87%. While group 2 saw no initial effects, and had significant effects one and two years following treatment. With a decline of 35.5% following one year and a fall of 61.13% following two years. These results may be driven by Portland and Atlanta who experienced later actual treatments in group 2. Group 4, which for evictions consists of only Fort Worth as a treated city had a strong statistical increase in legal evictions following treatment with an increase of 13.66% and an increase in the magnitude one year following treatment with an increase of 24.33%. The third treatment group exhibits no statistically significant results, but shows a negative impact on evictions following treatment.

Results for renter tenure treatment groups are found in Table 2.9, and due to data constraints has the fewer considered cities for certain treatment groups. Notwithstanding this limitation, the different treatment groups resulted in similar statistically significant positive impacts on renter tenure similar to that of the original subsample. Group 1 saw its impacts focused on the time of treatment and one year after treatment. With an approximate increase of 8% in the amount of renters who moved within the last 12 months following treatment and 3.56% increase one year after treatment. Group 2, which consisted of only St. Louis saw a statistically significant increase of 7.56% following treatment. Group 3 saw its impacts focused on one and two years following treatment showing an increase of 39.24% and 7.5% respectively. Group 4, which consisted only of Charlotte as a treated city for renter tenure, had an increase of 18.77% following treatment and an increase of 13.2% one year after treatment.

For the PIT homelessness as before nearly all the results are a not significant as seen in Table 2.10. The exception to these null results are for Group 4. Unlike with the other specifications, both cities in the group fall within the data constraints; however, the two cities do not fall into the 2-year period convention for groups and so as a result group 4 has been separated in order to just consider each of these treated cities. In both instances following treatment the PIT homelessness estimates fell by 30.79% for Fort Worth and fell by 12.89% for Charlotte. For Forth Worth, the PIT homelessness estimates increased by 11.85% for the first year after treatment, and 15.26% the second year after treatment. Charlotte had homelessness estimates fall 15.21% the first year after treatment, and a fall of 11.31% the second year after treatment. For these cities, Fort Worth has increasing legal evictions and homelessness a year after treatment, and Charlotte had increasing amount of renters who moved within the last 12 months and decreasing levels of homelessness.

2.6 Conclusion

This paper has sought to contribute to the nascent economic literature on the eviction epidemic currently sweeping the United States, as well as the impact of third-party policing nuisance ordinances enacted in some of its most populous cities. This was done by attempting to tie these two issues together and examine the possible unforeseen impact of these laws on evictions, both legal and otherwise. The estimations of this paper have found some interesting albeit confounding results.

Namely evictions have been found to be decreasing sometime within two years following the treatment of the nuisance ordinances. This has an interesting implication for the part of the relevant question of how these laws impact legal evictions. It was expected from previous literature in sociology that these laws would increase eviction levels, but the confounding results of these estimates shows the possibility of more at play than previously considered. Notwithstanding the decrease in legal evictions, it is possible renter displacement is still taking place. One possible explanation is an increase in extra-legal evictions e.g. landlord pressures which may cause a displacement of tenants.

This would constitute a decreased need for filing for evictions. Thereby leading to fewer evictions of “nuisance tenants”. This possible unforeseen outcome in evictions gives cause to the estimations of extra-legal evictions measured in the form of renter tenure and homelessness. In examining renter tenure, it was found that following the enactment of nuisance ordinances there was a positive spike in the levels of tenants who had recently moved into their current rental residences. This supports the hypothesis of some kind of increase in the displacement of renters following the treatment.

Additionally, as found by previous researchers, extralegal evictions, similar to legal evictions, may lead to increases in economic hardship and even homelessness. Which in turn would potentially eliminate some of the evicted individuals from the pool of responses for the ACS renter tenure data. Homelessness estimates where then also considered. The data for which being somewhat flawed, no significant impacts where found, except for in two cities in limited sample sizes; However, these estimates may not be very credible to do issues with the point-in-time estimates which also may be responsible for the insignificance of the estimations. Future research may be necessary in exploring these effects in the cities for which results seemed to be most prevalent.

2.7 Tables

Table 2.1: Summary Statistics

VARIABLES	(1) N	(2) Mean	(3) St. Dev
Population	272	1,331,000	1,104,000
Poverty-rate	272	13.43	4.113
Median Gross Rent	272	787.2	163.6
Median Household Income	272	48,113	9,128
median-property-value	272	167,870	72,809
% Population White	272	53.43	14.31
% Population Black	272	23.10	14.77
Evictions	261	7,853	5,120
Overall Homeless	160	3,326	2,558
Moved In 12 Months of Less	152	611.5	392.4
US Non-Citizen	152	501.4	506.3
Average Number Mothers	152	0.595	0.0960
Average Income from Welfare	152	76.39	33.20
Logged Rent Tenure 12mo or less	152	6.244	0.579

Table 2.2: Original Sample

	State	City	County (City in 90% or more)	Population	Treatment Date	Eviction Data (2002-2016)	ACS County Rent Tenure (2005-2016)	HUD Point-in-Time Homelessness (2007-2015)
Treated Group								
	IL	Chicago	Cook County	2,695,598	2010	✓	✓	✓
	IN	Indianapolis	Marion County	820,445	2009	✓	✓	✓
	NC	Charlotte	Mecklenburg County	731,424	2013		✓	✓
	OR	Portland	Multnomah County	583,776	2008	✓		✓
	OH	Cleveland	Cuyahoga County	431,369	2009	✓	✓	✓
Control Group								
	TX	Austin	Travis County	790,390	N/A	✓		✓
	TN	Memphis	Shelby County	646,889	N/A	✓	✓	✓

Table 2.3: Extended Sample and Treatment Groups

	State	City	County (City in 90% or more)	Population	Treatment Date	Eviction Data (2002-2016)	ACS County Rent Tenure (2005-2016)	HUD Point-in-Time Homelessness (2007-2015)
Treated Group								
1	OH	Columbus	Franklin County	787,033	2005	✓	✓	✓
	FL	Jacksonville	Duval County	821,784	2006	✓		✓
	NV	Las Vegas	Clark County	583,756	2006	✓	✓	✓
2	WI	Milwaukee	Milwaukee County	594,833	2007	✓		✓
	MO	ST Louis	St. Louis City County	319,294	2007	✓	✓	✓
	OR	Portland	Multnomah County	583,776	2008	✓		✓
	GA	Atlanta	Fulton County	420,003	2008	✓		✓
	IN	Indianapolis	Marion County	820,445	2009	✓	✓	✓
	WA	Seattle	King County	608,660	2009	✓	✓	✓
	OH	Cleveland	Cuyahoga County	431,369	2009	✓	✓	✓
	IL	Chicago	Cook County	2,695,598	2010	✓	✓	✓
4	TX	Fort Worth	Tarrant County	741,206	2011	✓		✓
	NC	Charlotte	Mecklenburg County	731,424	2013		✓	✓
Control Group	TX	Austin	Travis County	790,390	N/A	✓		✓
	TX	El Paso	El Paso County	649,121	N/A	✓	✓	✓
	TN	Memphis	Shelby County	646,889	N/A	✓	✓	✓

Table 2.4: Original Sample Evictions

VARIABLES	(3) Log Evictions	(4) Log Evictions	(5) Log Evictions
DID Estimator	-1.198**		
	(0.453)		
1-year After DID		-1.320***	
		(0.237)	
2-year After DID			-0.950**
			(0.350)
Observations	90	90	90
R-squared	0.794	0.794	0.767
Number of Cities/Counties	6	6	6

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Original Sample Rent Tenure

VARIABLES	(3) Logged Rent Tenure 12mo or less	(4) Logged Rent Tenure 12mo or less	(5) Logged Rent Tenure 12mo or less
DID Estimator	0.0661		
	(0.0639)		
1-year After DID		0.105*	
		(0.0434)	
2-year After DID			0.0301
			(0.0299)
Observations	60	60	60
R-squared	0.705	0.758	0.730
Number of Cities/Counties	5	5	5

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Original Sample PIT Homelessness

VARIABLES	(1) Log PIT Homelessness	(2) Log PIT Homelessness	(3) Log PIT Homelessness
DID Estimator	-0.0628		
	(0.128)		
1-year After DID		-0.102	
		(0.109)	
2-year After DID			-0.0490
			(0.0709)
Observations	70	70	70
R-squared	0.614	0.623	0.611
Number of Cities/Counties	7	7	7

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.7: Extended Sample

VARIABLES	Log Evictions			Logged Rent Tenure 12mo or less			Log PIT Homelessness		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
DID Estimator	-0.0309			0.035			0.0117		
	(0.328)			(0.0298)			(0.0605)		
1-year After DID		-0.134			0.0144			0.0503	
		(0.320)			(0.0261)			(0.0714)	
2-year After DID			-0.188			0.015			0.0781
			(0.264)			(0.022)			(0.0628)
Observations	225	225	225	120	120	120	160	160	160
R-squared	0.316	0.317	0.319	0.407	0.503	0.503	0.407	0.413	0.424
Number of Cities/Counties	15	15	15	10	10	10	16	16	16

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: Treatment Groups Evictions

VARIABLES	Treatment Group 1			Treatment Group 2			Treatment Group 3			Treatment Group 4: Fort Worth		
	Log Evictions			Log Evictions			Log Evictions			Log Evictions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DID	-0.161*			-0.129			-0.478			0.128***		
	(0.0687)			(0.329)			(0.273)			(0.000168)		
1-year After DID		-0.105			-0.440*			-0.37			0.217*	
		(0.0986)			(0.223)			(0.292)			(0.08230	
2-year After DID			-0.121			-0.945*			-0.0834			0.401
			(0.0999)			(0.416)			(0.212)			(0.174)
Observations	90	90	90	105	105	105	105	105	105	-0.535	-25.73	-60.01
R-squared	0.453	0.446	0.448	0.616	0.627	0.66	0.802	0.799	0.797	0.802	0.799	0.797
Number of Cities/Counties	6	6	6	7	7	7	7	7	7	60	60	60

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.9: Treatment Groups Renter Tenure

VARIABLES	Treatment Group 1			Treatment Group 2			Treatment Group 3			Treatment Group 4: Charlotte		
	Logged Rent Tenure 12mo or less			Logged Rent Tenure 12mo or less			Logged Rent Tenure 12mo or less			Logged Rent Tenure 12mo or less		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DID	0.0769*			0.0728**			-0.0993			0.172**		
	(0.033)			(0.0212)			(0.0557)			0.0433		
1-year After DID		0.0330*			0.0246			0.331***			0.124**	
		(0.0119)			(0.0423)			(0.0500)			0.0288	
2-year After DID			-0.0142			-0.081			0.0743*			0.0321
			(0.00751)			(0.127)			(0.0369)			0.0392
Observations	53	53	53	41	41	41	77	77	77	41	41	41
R-squared	0.62	0.613	0.609	0.632	0.616	0.623	0.591	0.66	0.611	0.713	0.677	0.634
Number of Cities/Counties	5	5	5	4	4	4	7	7	7	4	4	4

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.10: Treatment Groups PIT Homelessness

	Treatment Group 1			Treatment Group 2			Treatment Group 3			Treatment Group 4: Fort Worth			Treatment Group 4: Charlotte		
VARIABLES	Log PIT Homelessness			Log PIT Homelessness			Log PIT Homelessness			Log PIT Homelessness			Log PIT Homelessness		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
DID				0.066			0.125			-0.368***			-0.138**		
				(0.189)			(0.153)			(0.0251)			(0.0329)		
1-year After DID					0.264			-0.0272			0.112***			-0.165**	
					(0.169)			(0.155)			(0.0115)			(0.0332)	
2-year After DID			0.221			0.34			0.0131			0.142**			-0.120**
			(0.176)			(0.21)			(0.0445)			(0.0328)			(0.0248)
Observations	60	60	60	70	70	70	70	70	70	40	40	40	40	40	40
R-squared	0.614	0.614	0.64	0.613	0.665	0.674	0.628	0.613	0.613	0.819	0.825	0.835	0.762	0.771	0.759
Number of Cities/Counties	6	6	6	7	7	7	7	7	7	4	4	4	4	4	4

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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Chapter 3

EPA Priority Program Review using Evidence from the Chemical Manufacturing Industry

3.1 Introduction

Since the Clean Water Act was enacted in 1972, more than a trillion dollars has been spent by the EPA and private entities to fulfill its charge of improving water quality in the Nation's rivers, streams, and estuaries. Since that time clean water has remained one of the primary goals of the EPA and environmental regulatory agencies across the country. Hundreds of EPA regulatory programs have focused on improving water quality over the agency's 47-year history. One such initiative was the EPA's Priority Program. Begun in fiscal year 1996, the Priority Program increased monitoring, enforcement actions, and incentive programs for certain industrial manufacturing sectors in an attempt to abate water pollution.

Prior to the commencement of the Priority Program, the EPA's Office of Enforcement and Compliance Assurance ("OECA") identified a number of industrial sectors that required special attention to address compliance issues with regard to effluent discharges. The sectors identified as needing special attention from OECA to assure compliance with regulatory discharge limits were selected based on, inter alia, compliance history, regional and state concerns, and potential environmental risks posed by releases.

Initially for fiscal years starting in October 1996 and 1997, OECA identified three sectors to be included as "Priority" sectors, and an additional ten to be included as "Significant." This dichotomous hierarchy established for FY1996 and FY1997 increased monitoring, enforcement actions, and incentive programs for facilities in both priority and significant sectors, with Priority sectors receiving more stringent treatment than Significant sectors. Of note for the purposes of this paper, facilities designated by Standard Industrial Code ("SIC") Industrial Organics, Not Elsewhere Classified ("SIC 2869") were included in the Priority sector, and those identified by SIC as Chemical and Chemical Preparations, Not Elsewhere Classified ("SIC 2899") were included in the Significant sector during this time. For FY1998 and FY1999, the 2-tiered structure collapsed into a single echelon, Priority sectors. Again, noteworthy for this paper, SIC 2869 and SIC 2899 were included as Priority sectors. Figure 3.1 illustrates the timeline of treatments of the Priority Program for the analysis of this paper.

The purpose of this paper is to help understand the impact of the Priority Program by analyzing its effect on the chemical manufacturing industry as it pertains to improving compliance with discharge limits. By using difference-in-differences and synthetic control estimation, the impacts on the compliance of specific facility's inclusion in either the Significant or Priority echelon of the Priority Program can be assessed and the efficacy of the Priority Program, in the context of chemical manufacturing, measured. The questions specifically addressed in this paper are first did facilities included in the Significant sector perform better relative to no status, second did facilities included in the Priority sector perform even better than Significant sector, and finally did facilities that received either treatment continue to perform better after the Priority Program ended, i.e. were there any lingering or lasting effect of the Priority Program.

3.2 Literature Review

This review of the literature is not meant to be an exhaustive one; rather it is meant to give a brief background of some salient literature dealing with regulatory compliance in the field of environmental economics. Relevant for the purposes of this paper, deterrence through regulatory action plays an important role in incentivizing compliance. Two noteworthy publications on the topic of deterrence in the context of water quality and compliance are Shimshack and Ward (2008) and Cohen (2000). Shimshack and Ward find that, among other things, plants do in fact reduce discharges when regulators initiate enforcement actions, occasionally to the point of over-compliance. Cohen's paper is one of the first empirical studies focusing on how regulators' actions induce compliance via deterrent effects. Additional research has delved deeper into this idea of regulatory induced compliance. One such paper highlights the importance of community characteristics on compliance levels, showing they significantly affect regulatory interventions and facility performance both directly and indirectly (Earnhart, 2004c). Earnhart and Glicksman (2015a) examined the two different approaches to enforcement of regulatory law, coercive enforcement, which emphasis deterrence through inflexibly enforced sanctions, and co-operative enforcement, which emphasizes induced compliance through flexibility. They find that a more co-operative relationship between regulator, and regulated leads to better environmental management outcomes. Additionally, they conclude that enforcement strategies should not be viewed as unidimensional, but should be viewed as representing the multi-dimensional relationship which exists between the regulated and regulator. Whereby altering individual aspects may lead to greater, or lesser impacts on the regulated facilities behavior (2015b).

Many researchers also consider the type of enforcement action considered and the incentives around compliance and regulation. Shimshack and Ward (2005) find that non-monetary sanctions contribute no detected impacts on compliance, and that fines at the margin induces greater levels of compliance than inspections at the margin. Earnhart and Segerson (2012) consider the influence of financial status on the effectiveness of enforcement, and find that the financial dimension plays a key role in determining the incentives which are created by enforcement. They show empirically and theoretically that increased enforcement can actually lead to worse performance in some instances depending on the value of financial status factors, such as stock return rate, and the ratio of liquid to total assets among other things.

Examining another type of enforcement action, Earnhart and Harrington (2014) consider the effects of audits on the extent of compliance of multiple pollutants. They find that audits improve compliance for one but not both pollutants, and that facilities adjust their compliance dynamically for one pollutant, but not both. They conclude that audits may not improve compliance when facilities respond to lagged discharges, and regulators should consider that audits may not be the best policy when problematic pollutants are managed dynamically by the regulated firms. Wesley Brundell (2020) in a recent study finds that dynamic enforcements, which bases penalties and enforcement efforts on past compliance, plays a role in facility compliance decisions. He empirically finds that traditional enforcement measures, such as an increase in penalties applied to a facility, when considered in the dynamic setting lead to increased rates of regulatory compliance.

Several studies further suggest that compliance can be improved when the regulation is merely anticipated. The deterrent effect for firms can induce better environmental performance in the absence of actually imposed regulation as long as there exists a perception on the part the firms that regulation could be imposed. Segerson and Miceli (1998) find that polluters have an incentive to improve compliance and environmental performance if it can reduce the stringency of future regulations. This manner of self-regulation is supported through the perception that abatement costs incurred by better environmental performance will be less if the firms improve performance on their own than if the improvement is induced through regulation, introducing additional transaction costs. Lyon and Maxwell (2000) suggest that corporate social responsibility may preempt future legislation as a means of reducing the stringency of future regulation. In the context of this paper, this would translate to the “Significant” echelon in the EPA Priority Program incentivizing further compliance to avoid being moved to the more severe “Priority” echelon level of regulation within the program. Lana Friesen (2012) finds that compliance depends on the expected penalty and the probability of punishment. She finds that increasing the severity of the punishment is more effective than an equivalent increase in the probability of punishment.

Khanna and Anton (2004) empirically show that environmental liabilities and regulatory compliance costs create an incentive for firms to effectively self-regulate by voluntarily adopting Environmental Management Systems to improve environmental performance. In a similar vein Earnhart (2004b) considers both actual government interventions (i.e. inspections and enforcement actions) as well as the deterrence caused by the threat of receiving said government interventions, finding that the threat of intervention significantly induces better performance. In another study, Earnhart and Friesen (2017) examines regulated facilities perceptions of the effectiveness of monitoring, and enforcement efforts in inducing compliance. They find that for facilities who perceived enforcement as ineffective increased deterrence from inspections actually improved compliance whereas this was not the case for facilities which perceived enforcement as effective.

Taking into account these studies and this established literature, this paper seeks to contribute and build upon this literature on regulatory induced compliance. A review of the Priority Program has not yet been conducted, let alone one specifically for the chemical manufacturing industry. Examining the impacts on compliance of this older EPA Priority Program brings context and information relevant to any newer EPA initiative, or program implementations which may be conducted by the Office of Enforcement and Compliance Assurance (OECA). Additionally, preemption of future more stringent regulation by self-regulation provides an opportunity to improve compliance all the while reducing costs to firm and regulator alike. This older EPA

Priority Program offers an opportunity for gains in understanding in regards to the efficacy and application of future abatement programs.

3.3 Data

For this analysis, facility discharge data were gathered from the EPA Permit Compliance System Database, which is the EPA's discharge database as it applies to point source polluters under the Clean Water Act. Further data concerning local, state, and regional characteristics potentially impacting discharges were gathered from Census data, Bureau of Labor Statistics data, the Regional Economic Information System, and various public state databases.

These data include monthly observations across more than six years, starting January 1995 through June 2001, from 508 unique facilities in the chemical manufacturing industry located in 40 states. The observations include values from various regulatory, facility, community, and discharge characteristics of each facility. Regulatory characteristics include: state, local, and EPA regional budget per facility; federal and state inspection count; sanction amount in dollars; and informal and formal action counts. Facility characteristics cover: ownership structure of the facility and industrial sector by SIC code. Community characteristics include: voting data, private earnings from chemical manufacturing, housing data, population information, and demographic data. Finally, discharge data as they pertain to compliance include: total suspended solids ("TSS") limit, the nature of the TSS limit, and TSS composite ratio. The TSS composite ratio is the primary measure of interest in this analyses. It is a measure of TSS discharges against the permitted limit, and measures the extent to which a facility is complying with discharge limits. A lower value of this TSS composite ratio represents a greater level of compliance.

Several sample restriction were also imposed on the data. Facilities which were inactive during the observation period were removed from the sample as were outliers with particularly large discharges (TSS ratio greater than 10). Additionally, facilities that did not have full pretreatment observations were removed as well, so as to better establish a pretreatment trend of both treated and control groups. These restrictions accounted for the removal of less than ten facilities.

One, three, and six month lagged moving counts of enforcement actions and inspections were also created. Enforcement actions and inspections do not typically have immediate impacts, but rather effects are seen in subsequent months. Additionally, logarithm of the TSS ratio was generated to serve as the dependent variable to permit the interpretation of coefficients as percentage change. To prevent issues when taking logarithm, TSS ratios of zero were replaced with the smallest nonzero observed value. Dummy variables for State, year, EPA Administrative region, and quarter were generated. For the purposes of the analysis, various indicator variables were generated for treatment and control groups. Summary statistics of the primary variables of interest used in the analysis can be found in Table 3.1.

3.4 Empirical Model

The identification strategy for this analysis is a fixed effect difference-in-difference (DID) model. The treatment for DID is identified by the inclusion of a facility as a priority or significant sector. The control group consists of other facilities in the chemical manufacturing industry classified by the SIC 2800-series. The treated groups are facilities designated Industrial Organics, Not Elsewhere Classified (SIC 2869) and Chemical and Chemical Preparations, Not Elsewhere Classified (SIC 2899). There are four periods: first the pre-treatment period, then treatment period 1 which is the period from FY1996 to FY1997, followed by treatment period 2 ranging from

FY1998 to FY1999, and finally the post-treatment for the period after FY1999. In the pre-treatment period no groups received treatment. During treatment period 1, 2869 received the priority level treatment, and 2899 received the less stringent significant level treatment. During treatment period 2, both 2869 and 2899 received the priority treatment, and in the post-treatment period again no groups received treatment. The structure of these treatment groups and the timing of the treatment insures that DID models do not suffer from staggered implementation bias caused by multiple time periods, and variation in treatment timing identified by Goodman-Bacon (2021) and Callaway & Sant'Anna (2020).

Chemical and Chemical Preparations, Not Elsewhere Classified (SIC 2899) is the treated group that received two different treatments over two periods, and so a DID estimator is required for each of the treatment periods. A third DID estimator is used to assess lingering effects of treatment after the treatment ceased for both treated groups. To account for this, the functional equation uses different indicators specify the relevant treatment group and treatment period. The specification for the analysis is given by the following equation:

$$y_{it}=\alpha+\beta_1\mathbf{1}\{\text{period 1}\}+\beta_2\mathbf{1}\{\text{period 2}\}+\beta_3\mathbf{1}\{\text{period 3}\}+\beta_{69}\mathbf{1}\{\text{SIC 2869}\}+\beta_{99}\mathbf{1}\{\text{SIC 2899}\}$$

$$+\delta_P\mathbf{1}\{[(\text{period 2 or 3})\text{and SIC 2869}]\text{or [period 3 and SIC 2899]}\}$$

$$+\delta_S\mathbf{1}\{\text{period 2 and SIC 2899}\}$$

$$+\delta_A\mathbf{1}\{\text{period 4 and [SIC 2869 or SIC2899]}\} + \gamma Z_{it}+ \varepsilon_t$$

Where the logarithm of the TSS ratio of facility i at time *t* is y_{it} is the primary variable of interest. $\mathbf{1}\{\}$ are indicators of the relevant treatment period and treatment groups, and Z_{it} are the covariates in the analysis. The deltas are the three DID interactions and treatment-effect coefficients. The treatment effects are as follows: δ_P corresponds to the effect of receiving priority treatment, δ_S is the effect of receiving significant treatment, and δ_A is the lingering effect on compliance on the treated group after treatment has ceased. When measuring the impact of treatment with DID models the treatment and control groups must satisfy the parallel trends assumption. To test this assumption, pretreatment trends where estimated as average values of the logarithm TSS ratio for the three groups. These trends can be found in figure 3.2. They appear potentially periodic, but parallel thereby satisfying the parallel trends assumption.

3.5 Results

In assessing the treatment effects, many variations of the model where considered starting with a gradual increases in controls leading to a base or benchmark model against which additional robustness checks were conducted. In addition to these robustness checks a synthetic control approach considering the two treatment groups was done to further substantiate the results. The results for these different analyses will be presented in this section starting with the models leading to the primary benchmark model used in subsequent robustness checks and synthetic controls.

3.5.1 Parsimonious to Benchmark

In establishing a benchmark model to use as a base, many models where used with slowly increasing levels of controls. The results for these models as well as the benchmark model which is referred to as the "Base Model" in the paper can be found in Table 3.2. The first model employed was a parsimonious one—a simple two-way fixed effects linear regression model of logged TSS ratio regressed on treatment period indicators, treatment group indicators, and group and period interaction indicators with no covariates or controls. As previously stated, the effects of the treatments are captured by the interaction indicators, the variable of interest is the logarithm of TSS ratio, so the coefficient may be interpreted as a percentage change in the TSS ratio.

The results of this regression indicate a negative impact of significant treatment. That is to say, discharges were reduced and compliance improved, but a positive impact for priority treatment and for the period after treatment ceased. However, the impact of Priority treatment and the post-treatment effects were not statistically significant. Whereas, the effect of the Significant treatment were significant at the 5% level. In the parsimonious model, receiving significant status treatment caused a 25.2% reduction in TSS ratio during treatment, but these reductions ceased after SIC 2899 was transitioned to Priority treatment in Treatment period 2.

The results of the parsimonious model were supported when control variables were added. Controlling for facility-specific characteristics, regulatory characteristics, and community characteristics yielded similar results in terms of the magnitude and statistical significance of the treatment and post-treatment effects. Notably when SIC 2899 received Significant treatment, TSS ratio was reduced by 31.2%, statistically significant at the 1% level, while the Priority treatment and post-treatment effect were not statistically. Robust standard errors were employed and the statistical significance of the effect of receiving Significant treatment became even more pronounced. This final model is the base model, which includes the full suite of control variables and robust standard errors clustered on SIC.

3.5.2 Separation of SIC 2869 and SIC 2899 and Post-treatment effects

The decision to combine the effect of Priority treatment for the two treated groups (2869 and 2899) in treatment period 2 (FY1998 and FY 1999) was made to provide a single measure of the efficacy of Priority treatment. It seems reasonable that the facility fixed effects and covariates should account or any interindustry differences between SIC 2869 and SIC 2899. Further, the decision to separate the treatment periods and post-treatment period was made to identify and isolate what, if any, lingering or long-lasting effect of the Priority Program generally. Again, it seems reasonable to assume the time fixed effects should account for any discrepancies across treatment periods and the post treatment period.

Intuition behind these decisions notwithstanding, these assumptions were tested. Accordingly, several robustness checks were performed to ensure that the separations and inclusions mentioned above do not statistically meaningful impact on the results. Priority treatment indicators for each treated group were created to check for different effects across the two industries. The different effects were neither statistically significant nor statistically different from one another. Further, the Priority treatment effect and post treatment effect were folded into a single measure, and neither were the effects statistically significant nor were they statistically difference from those in the base model. These results are contained in Table 3.3.

3.5.3 Including Lagged Enforcement Actions

Given the data, a total of nine months of observations are available before treatment commences. In order to maximize the pre-treatment period so as to generate well developed pre-treatment trends, lagged enforcement actions and lagged inspections were not included. To check the appropriateness of this exclusion, the base model was run with 1-month, 3-month, and 6-month lagged moving enforcement actions and inspections counts. This reduced the pretreatment periods to 8 months, 6 months, and 3 months, respectively. The statistical significance and magnitude of the treatment effects were not meaningfully different from the base model having similar magnitudes. The 6-month lag having the largest difference in magnitude with a decrease of 36.7% as opposed to the 31.2% decrease from the base model. The results for all of these lagged controls can be found in Table 3.4.

3.5.4 Adjusting Control Group by Industrial Sector

Several different combinations of sectors in the Chemical Manufacturing industry were used as control groups. Using both SIC codes and by grouping similar manufacturing sectors into broader categories based on manufacturing output, denoted as SICO, different categories of sectors can be compared against the treated groups and used as different control groups. The goal of refining control groups was two-fold: control groups were chosen to have roughly the same number of observations as the treated group, which numbers around 7500 in total, and be more representative of the treated groups based on a univariate comparison of mean TSS ratios in the pretreatment period. Table 3.5 contains the means of the TSS ratios for the treatment period by SICO group and SIC respectively.

The SICO groupings allow for easier comparisons across the broad groups to see how similar or dissimilar they are when comparing the mean TSS ratio. The treated groups, which are organic fibers and adhesives (SICO2) and industrial organics (SICO8), for instance differ the most from alkalines, gases, and inorganic pigments (SICO1), toilet preparations and pharmaceuticals (SICO3), and Industrial Inorganic Chemicals, Not Elsewhere Classified (SICO4). Whereas, plastic materials and resins (SICO5) and cyclic crudes and intermediates (SICO6) were the most similar. Using the Base model as a benchmark, four additional models were constructed using different combinations of the aforementioned groups. How the groups and models were constructed, and which SIC are removed from each model are reported alongside their mean TSS ratios in Table 3.5. Regression results are reported in Table 3.6.

Use of these groups did not affect the statistical significance of Priority treatment effects and the post-treatment effects, both remained insignificant. The magnitude of effect of Significant treatment was altered, but the treatment effect remained negative and significant at the 1% level.

The first model uses the most similar SIC codes to the treated group as controls, removing all but four of the SIC codes, and sees a reduction in the TSS ratio of 20%. The second removes the eight most dissimilar facilities by SIC code from the control group and sees a reduction in the TSS ratio of 18.8%. The third model removes only the most dissimilar SICO group, resulting in the removal of three SIC codes, and has a reduction of 31.5%, and the fourth model removes the top three most dissimilar SICO group resulting in the removal of six SIC codes, and has a reduction in the TSS ratio of 34.3%.

3.5.5 Synthetic Control

As a final robustness check, a synthetic control model was implemented. Synthetic controls, pioneered in economic settings by Abadie, Diamond, and Hainmueller (2010), are a convex combination of individuals or groups with weights chosen to maximize the representativeness of the treated group during the pretreatment period for the variable of interest and covariates. In this way, synthetic control should afford a better depiction of the effects of a treatment. Two separate synthetic control models were run. One which considers only SIC 2869 as being treated, omitting SIC 2899 and the other vice-versa where SIC 2899 is treated and SIC 2869 is omitted. Resulting weights for each pool of donor SIC for these models can be found in Table 7. In implementing this method, all of the facility level data was collapsed down by averages to the SIC level. SIC codes for which there were too few observations to balance the panel were omitted. This resulted in two SIC codes being omitted, SIC 2891 and SIC 2844, for which they had half or fewer observations throughout the sample period. For all of the synthetic control models, the base model specification was used. A complete list of included control variables used in the creation of both models can be found in Tables 3.7 and 3.8, which also illustrates the relatively good fit when comparing these controls and true treated SIC.

Figure 3.3 and Figure 3.4 shows the initial models for SIC 2869 and SIC 2899 respectively. Examining these figures provides insight into the treatment effects of the two echelons on the Priority Program. The synthetic control for SIC 2869 shows the start of Priority treatment by green line commencing from FY1996 and treatment ending at the gray vertical line at the start of FY2000. Similar to the regression results found before, there seems to be no distinguishable effect of the Priority treatment. For the synthetic control for SIC 2899, the green line shows the start of the Significant level of treatment, where the red line represents the switch to Priority level treatment and then ending at the grey line for either treatment. For SIC 2899, the significant level of treatment shows a difference in the average logarithm TSS ratio starting in FY1996. After Significant treatment has ceased and transitioned to Priority treatment, the synthetic SIC 2899 levels drop lower showing worse compliance; however, the true treated SIC 2899 does not seem to change.

To examine this further, a placebo-in-space synthetic control model was also conducted for SIC 2899, as shown in Figure 3.5. Here the same results are substantiated for the early period immediately following the Significant sector's treatment, but the results for the troubling post Significant sector's treatment after FY1998 become less defined and more nebulous. This supports the finding in previous regressions that the Significant echelon exhibits statistically significant impacts on compliance where the Priority and Post treatment periods do not.

3.6 Conclusion

The results indicate that the treated group receiving the lesser punishment improved their compliance while the group receiving the more stringent treatment did not. Additionally, there were no significant post-treatment effects. This suggests that the SIC 2899 facilities anticipated the regulatory threat of potentially being subjected to Priority treatment and effectively selfregulated. In a sense, the threat of punishment was more effective than the punishment itself. This finding is congruent with the findings of Lyon and Maxwell (2000) in regards to preempting future legislation as a means of reducing the severity of future regulation.

The SIC 2899 facilities, while under Significant level treatment, attempted to preempt the treat of increased regulation by improving their environmental performance. With increased regulation comes increased abatement. In light of the increased cost burden of the Priority level treatment, it would be rational for a facility to improve compliance on its own and thus avoid the future potential costs altogether. It is noteworthy that it is, in essence, the threat of increased regulation that precipitates this preemptive self-regulation. Absent this threat, the notion of improving compliance preemptively evaporates. In the case of the SIC 2899 facilities, once they received the increased regulation of Priority treatment for FY1998, the incentive to self-regulate vanished, and compliance levels of those facilities reverted back to pre-Significant treatment levels. What this analysis indicates is exactly in line with fundamental firm objectives and the notion of preemptive self-regulation.

Therefore the overall effect of the Priority Program appears to have been somewhat of a mixed bag; The Priority Program was effective at improving facilities' compliance as long as there existed a more stringent regulatory framework—a punishment—to which facilities could be subjected. The treatment of inclusion as a Significant sector is found to be effective at reducing discharges by around 31.2%, and thereby improving compliance. Whereas inclusion as a priority sector did not significantly improve compliance, and there were no statistically significant effects on compliance after the program ended.

This result is interesting in that it seems to imply inclusion in the lower tier of treatment was a more effective incentive to improve compliance than actual increased regulatory efforts.

Perhaps it was the threat of receiving the more stringent priority treatment or these facilities felt they were on a sort of probation from which they could be removed if they cleaned up their act. If it was indeed the threat of Priority treatment that induced Significant facilities to improve performance, this preemptive self-regulation has powerful policy implications. Not only is it more cost effective for the firm to self-regulate rather than be subjected to the scrutiny, and therefore the associated costs of regulators, it is also cost effective for regulators if firms regulate themselves. All of this only occurs, of course, if the regulators have some sort of negative reinforcement available to facilities should they not perform to some standards defined by the regulators.

The Priority Program in the chemical manufacturing industry demonstrated that increased enforcement and monitoring like those observed here can not only achieve regulatory goals, but be cost-compatible for the polluter and regulator. It is a clear case of win-win-win—the public benefits from better environmental performance, the firm benefits from reduced transaction costs, and the regulators benefit from reduced regulatory costs. The lessons learned from the Priority Program in chemical manufacturing industries can help inform regulators implementing policy in the future in finding how to best incentivize facilities compliance.

3.7 Tables and Figures

Table 3.1: Summary Statistics

	Mean	Std. Dev.
TTS Limit Interim	0.016	(0.13)
TTS Permit Modification	0.087	(0.28)
Chemical Related Private Earnings	0.105	(0.11)
Male Residents	0.471	(0.03)
Family Households	0.686	(0.08)
Family Households with Children	0.482	(0.05)
Owner Occupied Housing	0.655	(0.12)
Democratic Voter	0.468	(0.09)
Voter Turnout	0.370	(0.06)
Non-white Residents	0.254	(0.20)
Ownership Structure	0.685	(0.47)
Unemployment	0.055	(0.02)
State and Local per Facility	49.090	(31.23)
EPA Regional Budget per Facility	0.665	(0.15)
Population Density	0.657	(1.10)
Per Capita Income	22.310	(4.66)
TTS Limit Level	1.261	(4.01)
Logged TSS Composite	-1.677	(1.53)
Sample Size	27,543	

Table 3.2: Regression Analysis Results Parsimonious to Benchmark.

VARIABLES	Parsimonious	Model 2	Model 3 TSS Ratio	Model 4	Base Model
Priority	0.00568	0.00604	0.00932	0.000973	0.000973
	(0.0431)	(0.0431)	(0.0432)	(0.0432)	(0.0469)
Post-treatment	0.0545	0.0550	0.0612	0.0439	0.0439
	(0.0480)	(0.0481)	(0.0485)	(0.0490)	(0.0557)
Significant	-0.252**	-0.250**	-0.278***	-0.312***	-0.312***
	(0.0887)	(0.259)	(0.285)	(1.297)	(4.376)
Time and Region Controls		X	X	X	X
Facility and Regulatory			X	X	X
Community Characteristics				X	X
Robust Errors					X
Observations	27,543	27,543	27,543	27,543	27,543
R-squared	0.010	0.012	0.013	0.016	0.016
Number of facilities	413	413	413	413	413

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Separating and Grouping Treatment Effects

	Base	Model 1	Model 2	Model 3
VARIABLES		TSS Ratio		
Significant	-0.312***	-0.255**	-0.162**	-0.252**
	(4.376)	(0.00757)	(6.22e-05)	(0.00994)
2869 Priority		-0.0301		
		(0.172)		
2899 Priority		-0.151		
		(0.100)		
2869 post-treatment		0.00076		
		(0.987)		
2899 post-treatment		-0.130		
		(0.323)		
Priority	0.000973			
	(0.0469)			
Post-treatment	0.0439			
	(0.0557)			
Priority and Post-treatment			-0.029	
			(0.301)	
2869 Priority and Post-treatment				-0.0225
				(0.366)
2899 Priority and Post-treatment				-0.144
				(0.170)
Observations	27,543	27,543	27,543	27,543
R-squared	0.016	0.022	0.022	0.022
Number of facilities	413	413	413	413

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Lagged Enforcement Variables

VARIABLES	(1) Base	(2) 1-mo Lag	(3) 3-mo Lag	(4) 6-mo Lag
Priority	0.000973	0.00175	0.00703	-0.0547
	(0.0469)	(0.0454)	(0.0484)	(0.0585)
Post-treatment	0.0439	0.0457	0.0523	-0.00878
	(0.0557)	(0.0555)	(0.0630)	(0.0768)
Significant	-0.312***	-0.313***	-0.306***	-0.367***
	(0.0687)	(0.0705)	(0.0773)	(0.0945)
Observations	27,543	27,184	26,468	25,385
R-squared	0.016	0.017	0.016	0.016
Number of facilities	413	413	413	411

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Adjusting Control Groups, Average Logarithm of TSS Ratio

SIC	Mean Logarithm of TSS Ratio pre-Treatment	Difference	Model 1: SIC Removed	Model 2: SIC Removed	Model 3: SIC Removed	Model 4: SIC Removed
Treated Group						
2899	-1.362	-0.01				
2869	-1.381	0.01				
Combined	-1.372	0.00				
Control Group						
SICO 1	-2.320	0.95				
2813	-3.164	1.79	2813	2813	2813	2813
2812	2.614	1.24	2812	2812	2812	2812
2816	-1.915	0.54	2816	2816	2816	2816
SICO 2	-1.514	0.14				
2841	-2.665	1.29	2841	2841		
2892	-2.421	1.05	2892	2892		
2823	-1.696	0.32	2823			
2843	-1.564	0.19	2843			
2822	-1.455	0.08				
2879	-1.346	-0.03				
2824	-1.299	-0.07				
2861	-1.073	-0.30	2861			
SICO 3	-1.856	0.48				
2833	-1.956	0.58	2833	2833		2833
2834	-1.754	0.38	2834			2834
SICO 4						
2819	-1.857	0.49	2819	2819		2819
SICO 5						
2821	-1.380	0.01				
SICO 6						
2865	-1.525	0.15	2865			
SICO 7	-1.30	-0.07				
2874	-1.833	0.46	2874	2874		
2873	-1.124	-0.25	2873			

Table 3.6: Control Group Refinements

	(0)	(1)	(2)	(3)	(4)
VARIABLES	Base Model	Model 1	Model 2	Model 3	Model 4
Priority	0.000973	-0.0222	0.00449	-0.00666	0.00793
	(0.0469)	(0.0365)	(0.0366)	(0.0515)	(0.0465)
Post-Treatment	0.0439	-0.000846	0.00695	0.0421	0.0513
	(0.0557)	(0.0519)	(0.0434)	(0.0558)	(0.0661)
Significant	-0.312***	-0.200***	-0.188***	-0.315***	-0.343**
	(4.376)	(0.0217)	(0.0238)	(0.0971)	(0.116)
Observations	27,543	16,237	20,470	25,412	20,391
R-squared	0.016	0.028	0.024	0.017	0.024
Number of facilities	413	239	301	380	303

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.7: Synthetic Control Donor Sampling Weights

Synthetic SIC 2869		Synthetic SIC 2899	
SIC	Weight	SIC	Weight
2816	0.033	2812	0.004
1821	0.109	2824	0.840
2822	0.332	2834	0.122
2834	0.024	2879	0.034
2861	0.023		
2865	0.399		
2879	0.081		

Table 3.8: Treated vs Synthetic SIC 2869

	Treated	Synthetic
Ownership Structure	0.75	0.581
TTS Limit Level	1.03	0.857
TTS Limit Interim	0.00	0.021
TTS Permit Modification	0.10	0.088
State and Local per Facility	49.57	48.654
EPA Regional Budget per Facility	0.62	0.627
Owner Occupied Housing	0.66	0.653
Chemical Related Private Earnings	0.16	0.102
Democratic Voters	0.84	0.483
Male Residents	0.47	0.472
Family Households	0.71	0.699
Family Households with Children	0.49	0.493
Unemployment	0.07	0.064
Voter Turnout	0.36	0.365
Non-white Residents	0.26	0.256
Per Capita Income	20.93	20.884
Population Density	0.58	0.585

Table 3.9: Treated vs Synthetic SIC 2899

	Treated	Synthetic
Ownership Structure	1.00	0.821
TTS Limit Level	0.50	0.750
TTS Limit Interim	0.00	0.000
TTS Permit Modification	0.00	0.049
State and Local per Facility	41.45	39.343
EPA Regional Budget per Facility	0.65	0.562
Owner Occupied Housing	0.67	0.625
Chemical Related Private Earnings	0.07	0.108
Democratic Voters	0.54	0.424
Male Residents	0.46	0.459
Family Households	0.69	0.688
Family Households with Children	0.49	0.488
Unemployment	0.07	0.063
Voter Turnout	0.41	0.330
Non-white Residents	0.21	0.302
Per Capita Income	20.28	18.932
Population Density	1.52	0.228

Figure 3.1: Timeline of Priority Program and Echelons of Treatment.

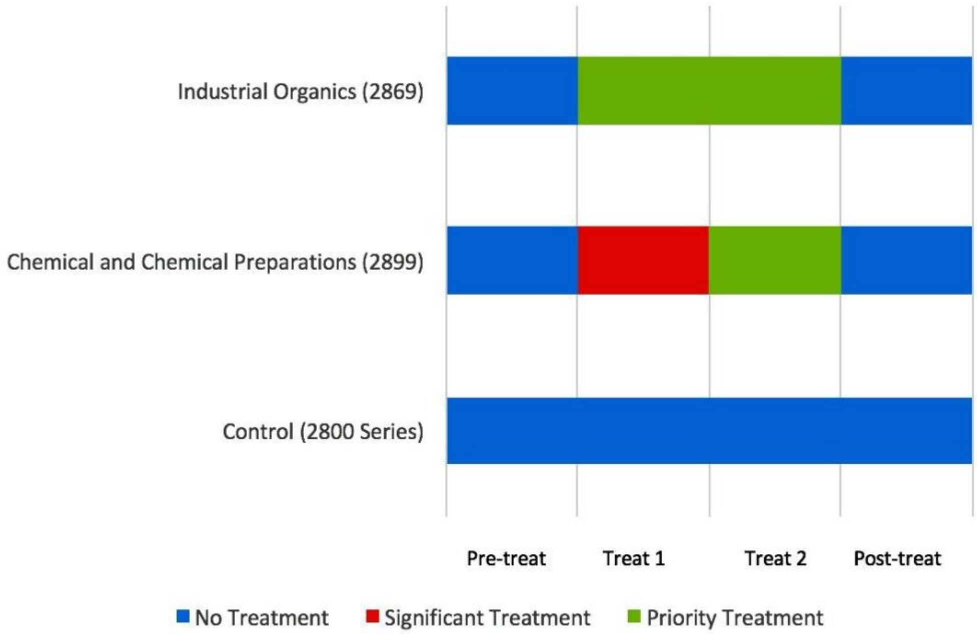


Figure 3.2: Pre-Treatment Trends Assumption

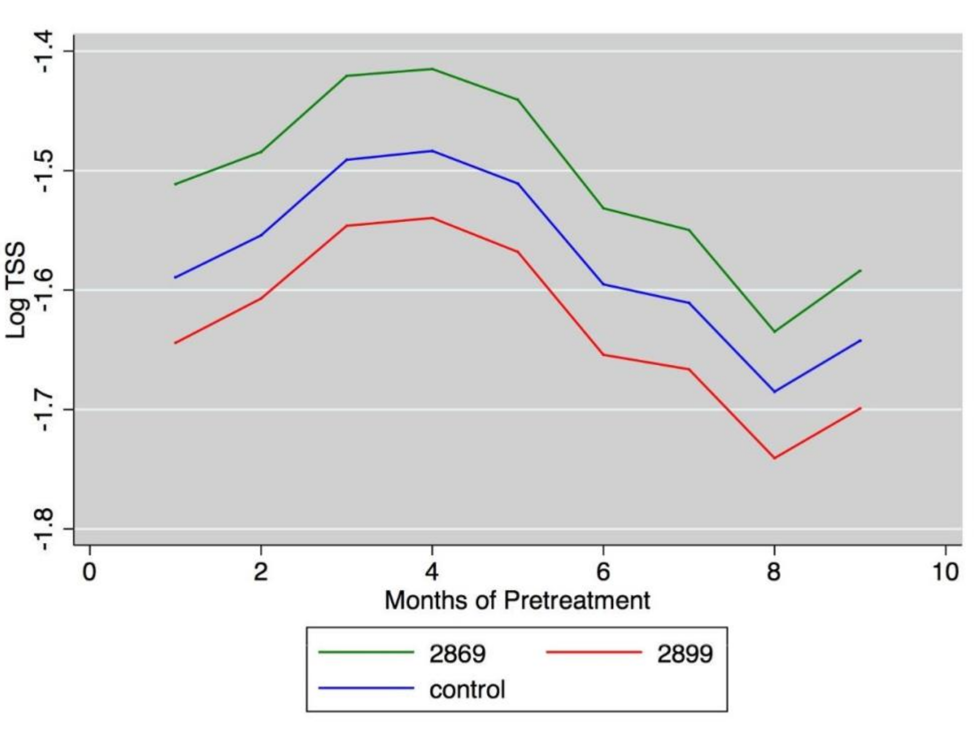
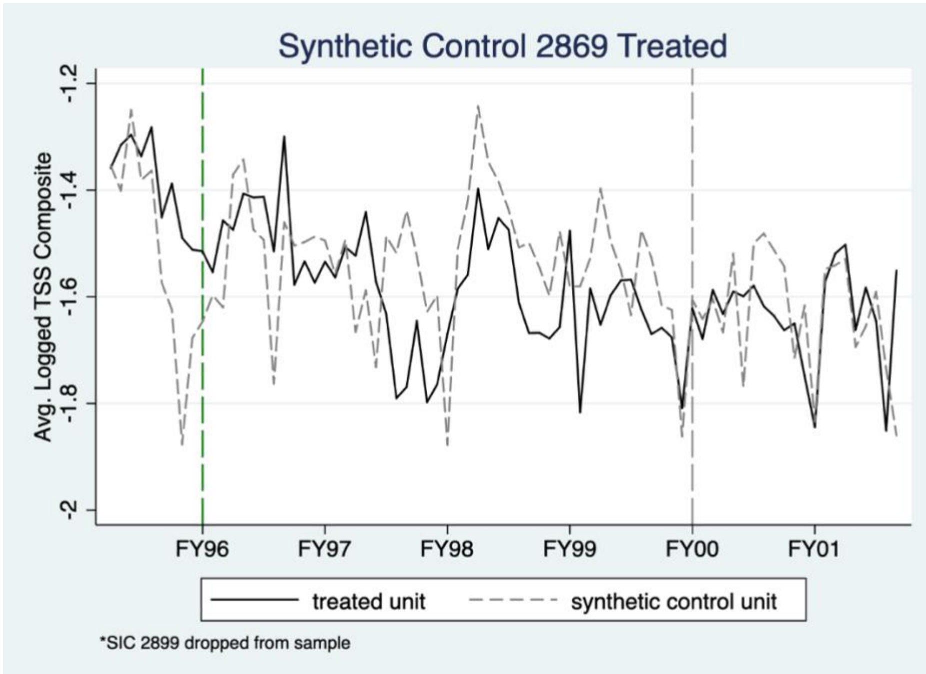
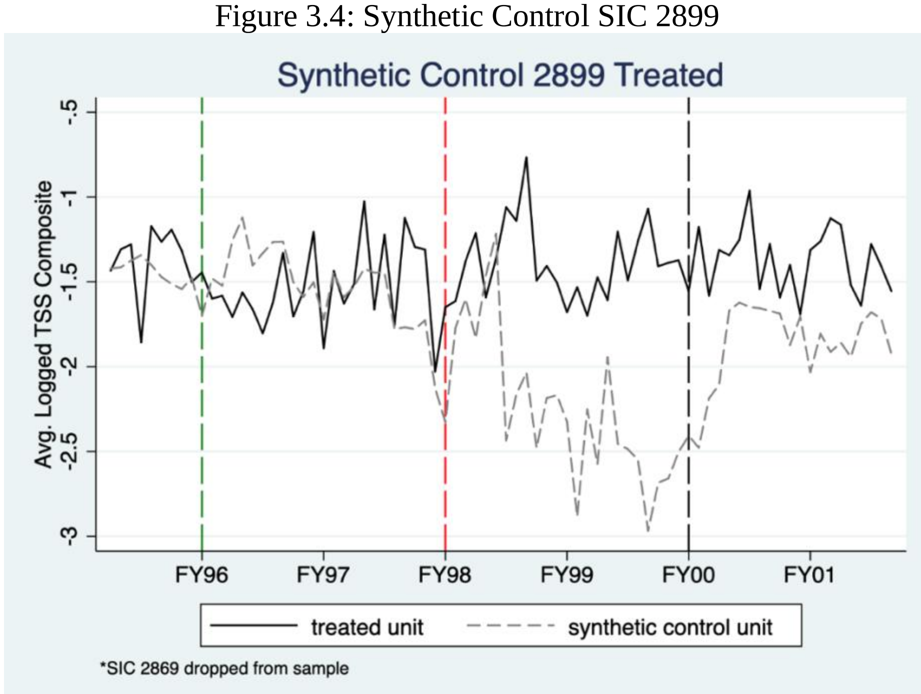


Figure 3.3: Synthetic Control SIC 2869



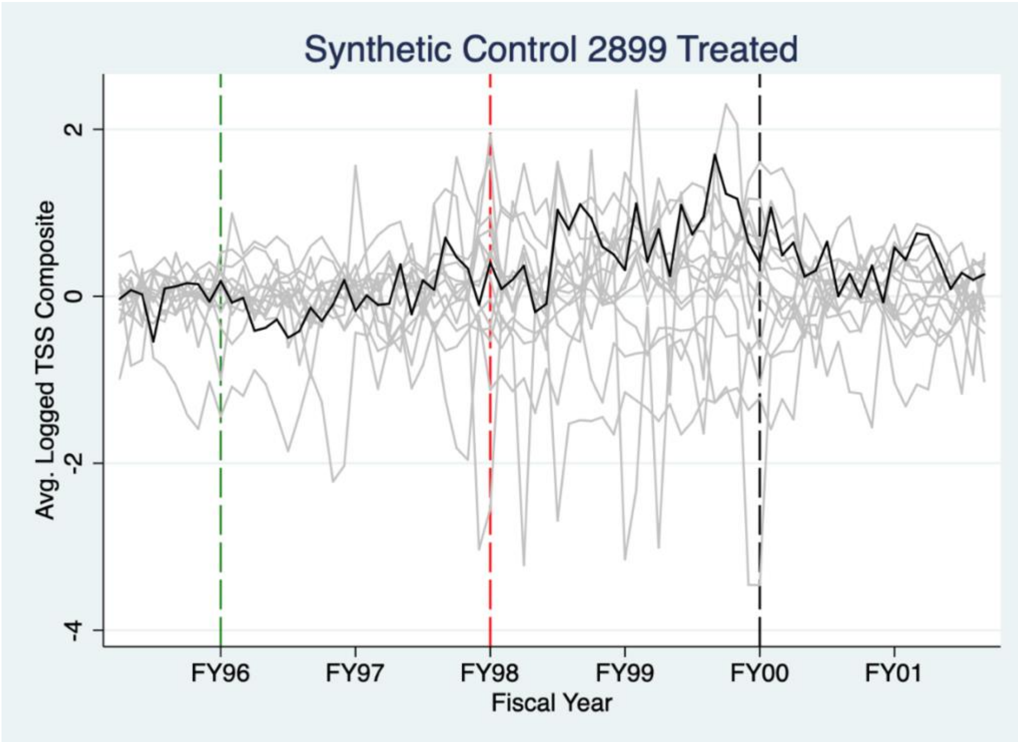
Treatment is shown by the green line and treatment ends at the gray line.

Figure 3.4: Synthetic Control SIC 2899



Treatment is shown by the green line and treatment ends at the gray line. The Red line indicates the switch to Priority treatment

Figure 3.5: Placebo Synthetic Control SIC 2899



Treatment is shown by the green line and treatment ends at the gray line. The Red line indicates the switch to Priority treatment

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