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BY THE COMMITTEE CONSISTING OF

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DEDICATION

to:

My son Reed for always making me love life and on days that are hard putting a smile on my face.

My parents for always believing in me, encouraging me, and teaching me how to be a great father.

My very loving and supportive dissertation chair Greg Burge for always knowing what to say to keep me going, being supportive no matter what, and pushing me to be the best father, economists, and teacher he knew I could be.

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Abstracts

In the first chapter, we investigate the relationship between student test scores and discipline outcomes in Texas public schools and whether or not schools participated in the Universal Free Breakfast Program (UFB). Eating a routine breakfast leads to increased physical and mental performance, as well as test scores. Surprisingly, there has been little focus on how eating a routine breakfast affects disruptive behaviors. We compile a panel data set from two administrative sources in Texas, spanning school years 2011/2012-2016/2107. Using fixed effects models, a staggered difference in differences model, and a fuzzy regression discontinuity design, we find that schools that offer UFB experience higher test scores and have reduced conflict outcomes such as fights, substance abuse, and truancy. These results suggest that the benefit schools receive from taking part in UFB are significant, help their students achieve better outcomes in schooling, behavior, general well-being, and increase funding from lower truancy rates.

In the second chapter, we investigate how the technique of hydraulic fracturing or ”fracking”, has made it possible to produce vast new quantities of oil and natural gas. States like Colorado, Texas, and Oklahoma have seen a dramatic increase in the number of wells for both oil and natural gas. In this study, the main source of exogenous variation to be explored is the location of oil and natural gas well sites over time, relative to home locations. We estimate the effect of hydraulically fractured natural gas and oil well sites on both urban and rural residential home prices between 2000 to 2018. The data stems from the U.S. Department of Homeland Security that lists locations of all oil and natural gas wells, and from Zillow's ZTRAX data base which contains home transaction and administrative data. ArcGis is used to create varying buffer zones sizes around well sites,

exploring how average home prices changed before and after a well opens. First, a zip code level fixed effects model is used. Second, household level fixed effects models and repeat sales models are implemented. Lastly, a spatial differences in differences (SDID) approach is used. Our results show that homes within .5 mile of a well have a 2.9% increase in selling price and homes that are .5-1 mile from a well site see a 1.2% increase compared to homes that are more than 2 miles away. In the third chapter, we are interested in flat rate tuition and how it has effected student registration behaviors and academic performance. The cost to attend college has risen drastically over the past decade. This sharp increase has caused universities to reevaluate tuition pricing schemes how they charge tuition in efforts to keep enrollments and revenue's high. There is growing interest in flat rate tuition (FRT) where tuition is based on 15 credit hours per term for students enrolled in 12-19 hours. Thus, the marginal cost for over 15 credit hours is effectively zero. This new tuition pricing system has two big impacts on the student body. First, it can alter the academic performance of students. Second it can alter their registered and attempted semester course loads. Using a linear probability model and fixed effects regression models, find that under FRT, students register for more classes, attempt more credit hours, and have higher semester, yearly, and graduation GPA's compared to students that paid per credit hour. Using a rich data set from the University of Oklahoma, we compare cohorts of students facing different tuition schemes: no FRT (or per credit hour tuition), 1, 2, or 3 years of FRT, and all years of FRT, from the Fall of 2008 to Spring of 2018.

Chapter 1

Breakfast of Champions: Universal Free Breakfast, Student Conflict, and Test Scores in Texas Schools

Breakfast has been called the most important meal of the day. According to the USDA dietary guidelines, we should all be aiming to consume around 15 percent to 25 percent of our daily energy intake at breakfast. Studies have proven that after our bodies have been at rest overnight, individuals who wake up and fuel their bodies generally have better vitamin and nutrient consumption, enjoy healthier diets, and are less prone to being overweight or obese (Betts et al. [2014]). Unfortunately, food insecurity plagues millions of children in the United States. According to the No Kid Hungry project run by the USDA, roughly 13 million children (one out of every six) live in houses without sufficient access to food. In Texas, one in five students is at risk of going hungry. With a lack of proper nutrition, specifically in the morning and at lunch time, these children are at a huge disadvantage compared to their peers. Looking past the benefits school nutrition programs may offer by helping to obtain a basic physiological need (Maslow [1943]), there is a growing body of literature on the effects of offering school-wide free meals having impacts on a child's academic performance, body weights, and behavior. While a considerable amount of research has focused on the effects of eating a routine breakfast on physical, mental, and academic abilities, very few studies have focused on the impact that eating a routine breakfast has on the behavior of school-aged children in academic settings.

We add to the literature by presenting clear evidence that schools that participate in universal free breakfast (UFB) do indeed show an increased likelihood to meet or exceed state average test scores and have lower rates of discipline infractions among their student population when compared to schools that offer a standard breakfast program (SBP). Using data from the Texas Education Agency (TEA) that tracks schools meets or exceeds state test scores and discipline reports for each school, we are able to compare schools that adopt UFB to those that do not to estimate the causal impact of free school meals on discipline and academic performance. We find modest increases in schools' percentage of students that meet or exceed the passing rate for the Texas state tests and a reduction in a schools discipline counts. Specifically, these reductions are meaningful for truancy, code of conduct violations (in classroom disruptions), and substance abuse.

We explore the potential for UFB to impact student test scores and disciplinary actions through at least two reasonable channels. While this paper is not able to clearly distinguish between them, like previous literature, both channels contribute to the results. First, UFB could potentially reduce the stigma associated with free or reduced meals, most noticeably in schools where a considerable fraction of students do not qualify individually for free or reduced meals, thus improving the social climate of the school. Marples and Spillman [1995] and Poppendieck [2010] both have conducted surveys and student interviews suggesting that stigma discourages free meal consumption conditional on eligibility, thus students who are eligible for free meals may be more willing to consume those meals when offered school-wide via UFB. The second channel is UFB may improve nutritional intake by increasing the share of students eating school meals.

The capability for UFB to increase the number of students eating free meals comes from two sources. The first from students who would not be individually eligible but now have access to free meals. Secondly, from students who would be eligible, but failed to return the proper parental-reported family income forms needed to qualify. Furthermore, Leos-Urbel et al. [2013]

investigated the impact of the implementation of a universal free school breakfast policy on meals program participation, attendance, and academic achievement in New York City schools. They found that post implementation participation increased among all students, including those previously eligible. Additionally, data from TEA shows raw number of breakfasts and lunches served between UFB participating schools and those that do not (Figure 1), as well as yearly number of schools that participate in each of the two breakfast type programs (Figure 2). Figure 1 shows that both breakfast and lunch UFB participating schools serve more meals compared to schools that do not participate. Lastly, the data shows that more disadvantaged or lower socioeconomic students rely on schools for their primary source of meals. Schools' districts are also aware of the need to offer meals to school-age children during times when school is not in session. This can be seen over the summer/winter breaks, or most recently during school closures for the 2020 COVID-19 pandemic. During this time, school districts had to shut down due to social distancing requirements (USDA [2020], VOX [2020]). After schools had to shut down, they quickly came up with ways to keep providing both breakfast and lunches, not only to their student body, but in most places to any child that was under the age of 18. (Recommended to insert Figure 1 and 2 about here)

The main goal of this paper is to investigate how UFB influences the school environment, specifically student behaviors and overall school test scores on state benchmark exams. Given the acquired data and the constraints of the research design, we exploit variation across demographically similar schools based on participation in the UFB program. We investigate the link between provision of universal free breakfast and disciplinary outcomes, which are available at the school level. We focus on the total school level number of infractions, as well as the total number of code of conduct, truancy, and substance abuse infractions in a school in a given year. Our assumption is that within schools, changes in these infraction rates are correlated with changes in student behavior, as recognized by school facility and staff, but we do not assume that they are perfectly correlated.

The data was acquired from the TEA and covers over 50 different conflict outcomes, spanning a course of 6 academic school years, and a schools "meets or exceeds" rate on certain state benchmark exams. We group similar discipline outcomes into categories[1]. The groups range from the common code of conduct violation (daily disruptions), to substance abuse, truancy, and more serious actions such as assault and vandalism. We are able to use fixed-effects and a difference in differences model to estimate the average treatment effect between academic performance and discipline reports at the school level. Additionally, in 2013, the Texas Senate Bill 376 (83rd Texas Legislative Session) was passed requiring all schools (contracting entities [CEs]) who's free and reduced eligibility rate was equal to or grater than 80 percent to participate in UFB and provide breakfast to all children at no charge. The passage of this Bill allows us to use a fuzzy regression discontinuity design (FRD) to estimates the local average treatment effect.

The remainder of the paper is organized as follows: Section 2 details the history of school feeding programs in the US, as well as reviews the recent relevant literature. Section 3 describes the data examined in this study. Section 4 outlines the methodical approaches adopted herein. Finally, sections 5 and 6 present the main results and conclusion.

1.1 US School Feeding Programs and Student Outcomes

Initially established in 1966 as a 2-year pilot project designed to provide categorical grants to assist schools serving breakfasts to "nutritionally needy" children, the school feeding program expanded and was made permanent through Congressional Legislation in 1975 (USDA [2019]). Student participation in the lunch program is high, with about 60 percent of the nation's elementary school participating. However, from the children that participate in the lunch program, less than one-third of these children participate in the school breakfast program. Among poor and low-income children, most of whom are entitled to free school meals, participation is less than 25 percent (Burghardt et al. [1993]). One of the leading reasons behind the low participation rate in subsidized meals is the negative stigma that can be associated with it as McGlinchy [1992] found. Outside of helping to reduce negative stigma with subsidized meals, school feeding programs can also help decrease food insecurity and other negative outcomes for children. These include physical and mental health and social and interpersonal development (eg. Howard [2011]; Gundersen and Kreider [2009]). Poorer health and increased food insecurity in a child's early school-age years has also been linked to worse long-term outcomes such as poorer health in adolescents and lower educational attainment resulting in lower labor force participation in adulthood (Case et al. [2005]). If these relationships from these correlational studies show any causal impact of higher food insecurity, then school meals programs could lead to improvements in student academic outcomes and behaviors.

The federal government has implemented numerous programs whose goals are to reduce food insecurity and improve nutrition in children and adults. Most notable of these programs include the Supplemental Nutritional Assistance Program (SNAP, formerly known as Food Stamps) and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). Both are aimed to provide assistance targeted to low-income families. In Texas, the law requires all schools that have at least 10 percent of their students qualifying for the free/reduced program to offer a Standard breakfast program (SBP) or Universal free breakfast (UFB), but after 2013, if a school has at least 80 percent that fall under the free/reduced requirement, it is required to offer UFB.[2] The major advantage of offering UFB over SBP is that more students that qualify for free/reduced breakfast actually eat breakfast under UFB as opposed to SBP and students that were just above the qualifying point for free or reduced meals now have access to free meals (Priorities [2015]).

As Gordon and Ruffini [2018] mentioned in their analysis of school meal programs and student discipline, all analyses of income assistance programs, such as school meals, encounter two specific challenges. In the case of the school feeding programs, at the national level they have remained relatively unchanged since implementation. This limits the time and geographic variation available to study. Secondly, participation is non-random, since being eligible is limited to those with low family incomes (eg.Bitler and Currie [2005]). There is a high probability that children and adolescents who eat breakfast differ from those who do not eat breakfast in ways that also influence educational outcomes. We know that socioeconomic status (SES) is associated with breakfast eating, where children from higher SES backgrounds are more likely to regularly eat breakfast than children from lower SES backgrounds, and this finding is consistent across gender and age (eg. Doku et al. [2013]; Hall et al. [2012]). Likewise, many researchers have shown that SES is a central determinant of academic performance and cognitive ability (eg. McCulloch and Joshi [2001]; Machin and Vignoles [2005]). With this negative relationship between family income and health outcomes, students who participate in school meal programs and other nutritional assistance programs, are more inclined to have poorer health outcomes without the program compared to other ineligible individuals. Because of this, a simple comparison between these 2 groups of students will tend to understate the benefits of school feeding programs. To overcome these potential issues in the main analysis, we drop never participating schools' to mitigate the selection bias, along with accounting for the school-level characteristics of each school.

Previous literature finds that school meals program increases food consumption and nutritional intake, with mixed effects on academic performance and overall health. There are several papers that compare students in schools that offer SBP to similar students in schools that do not and they generally find that participation in SBP generally improves nutritional intake during breakfast and increases reading academic performance, but has mixed results on students overall nutritional intake (Bhattacharya et al. [2005]; Frisvold [2015]). Wodon et al. [2002] evaluated the impact of government programs on social welfare and found that school breakfast programs have a targeting performance that is very efficient and there are few differences in the allocation of benefits between program participants. Additionally, research from Gurley-Calvez and Higginbotham [2010] showed that obesity negatively affects reading proficiency in high poverty districts. Complementing that, List and Samek [2017] showed that school lunch and breakfast programs can help to serve as a nudge to improve food choice and consumption in its student body. Studies such as Gundersen and Kreider [2009] find that students who receive free school meals are associated with a lower likelihood of poor health outcomes, including obesity and food insecurity. The positive effects of breakfast are more demonstrable in children who are considered undernourished, typically defined as one standard deviation below normal height or weight for age using the US National Center for Health Statistics (NCHS) (eg. Pollitt et al. [2009]). In contrast, Dunifon [2002] do not find that

NSLP significantly affects math or reading achievement or increase positive behaviors. Additionally, Hinrichs [2010] found no effect of school lunches on short-term performance, but does find that in the long-term school meals increase educational attainment. Competing against this, Imberman and Kugler [2014] and Frisvold [2015] both find that school breakfasts improve both reading and math performances. Ruffini [2018] finds that math test score do improve and improvements are concentrated among Hispanic and white students-groups with relatively low eligibility rates under the traditional school meals program.

Most importantly for this study, a few pieces of newer research have come out that specifically looks at schools' access to universal free meals and academic performance. First, Dotter [2013] found that universal breakfasts in the classroom increases math and reading test scores by 15 percent and 10 percent of a standard deviation on average, respectively. They found that gains were higher in schools where fewer students were previously participating in school breakfasts, specifically among students with lower achievement levels. Furthermore, these effects extended in later years of treatment. They also found that moving breakfast into the classroom does not significantly impact academic achievements in schools that had already implemented universal free breakfast programs. These results suggest that offering UFB increases participation possibly by reducing the associated negative social stigmas. It also suggests that the resulting positive impacts on academic achievement are at least in part driven by the year round benefits rather than only eating breakfast at the time of testing.

Following that, Schwartz and Rothbart [2020] investigates the impact of offering free school lunch to all students on academic performance and lunch participation in New York City middle schools. They found that free meals increases academic performance by as much as 0.083 standard deviations in math and 0.059 in english/language arts for non-poor students, with smaller, statistically significant effects of 0.032 and 0.027 standard deviations in math and english/language arts for poor students. Additionally, access to free meals increases participation in school lunch by roughly 11 percent for non-poor students and 5.4 percent for poor students.

Multiple papers use the Community Eligibility Provision (CEP) and a schools choice to participate in it and offer their student body universal free meals. Comperatore and Fuller [2018] use school data from North Carolina and estimate a difference-in-differences model between eligible participating and non-participating schools using administrative student-level data. They find universal meals reduce absences, improve test scores, and do not affect disciplinary outcomes. Kho [2018] looked at how CEP effects suspensions, attendance, and expulsions in Tennessee. These findings showed that CEP reduced suspensions about 10 percent and increased access to free school meals by 50 percent. In addition, they were able to examine a year-by-year analysis that showed that the effects of CEP on student educational outcomes grew over time, with more positive effects on on-time grade progression and behavioral outcomes in future years of implementation. Davis and Musaddiq [2018] looked at k-12 schools in Georgia and used CEP eligibility as an instrument for CEP participation and find that CEP participation increases the percentage of a schools students who fall within a healthy weight range and reduces school-level average BMI scores.

Lastly, (Gordon and Ruffini [2018] and Altindag et al. [2020]) specifically explore behavioral outcomes among schools that offer free meals. Both papers conclude that universal school meal programs reduce student conflict. Gordon and Ruffini [2018] focus on suspension rates in schools but, as mentioned in their research, not the reasons attributed to these suspensions. Their study relies on the timing of pilot implementation of CEP across states to examine how disciplinary infractions evolve within a school as it adopts CEP. They find modest reductions in suspension rates among elementary and middle but not high school students. Altindag et al. [2020] looked at bullying and fights in South Korean schools over a four year time span. They find that the provision of universal school lunches reduces the number of behavioral incidents, particularly physical fights between students, by about 35 percent. They attest that the reduction could be observed because universal free meal programs reduces the chances that a students socioeconomic status can be identified, and therefore fights and bullying that are motivated by a bias towards the wealthy or poor students are reduced.

We add to the existing literature because we have access to student-level data on a broader range of outcomes. These outcomes include total school discipline reports, truancy, substance abuse, in-class disruptions, and weapon abuse. We also investigate both the average treatment effect, through the use of a difference in differences approach, as well as the local average treatment effect, through the use of a regression discontinuity design. We also tie together both academic performance and behavior into one combined analysis when schools participate in free meal programs. Specifically, we are able to determine whether there exists a strong link between the overall impact of free meal programs on cognitive, behavioral, and academic performance.

1.1.1 School Feeding in Texas

Currently, the reimbursement rates for the 2019-2020 school year in Texas are \$0.31 for paid breakfasts, \$1.54 for reduced breakfasts, and \$1.84 for free breakfasts. Additionally, a school may qualify and apply for Severe Need Breakfast reimbursement, i.e., an additional \$0.36 on top of the regular breakfast reimbursement for reduced and free meals served. To be granted the extra funding, at least 40 percent of the total lunches served in the school year must have been free or reduced. To receive reimbursements for meals, schools are required to send home paperwork with the students that parents can fill out to determine whether they qualify for reduced-price or free meals. Once the forms are returned, they are then verified by a certified person in the district or school. Additionally, schools can file under the Community Eligibility Provision (CEP) and Special Assistance Provision 2 (P2). These are two alternate ways to the standard requirements for determining the eligibility and claiming reimbursement for the National School Lunch Program (NLSP) and School Breakfast Program (SBP). The rationale behind using these alternative approaches to determine how many students qualify for subsidized meals is that these approaches involve less paperwork than the standard approach. Both options reduce the paperwork sent home and eliminate the verification process. They are able to achieve this by utilizing the school's claiming percentages based on the number of students identified for reimbursement in the school. An identified student is one that participates in other need-based programs such as Supplemental Nutritional Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and Food Distribution Program on Indian Reservations (FDPIR) or is categorically eligible (including Foster, Migrant, Head Start, and Runaway children) (USDA [2019]). Under these alternative provisions, schools are reimbursed based on the percentage of identified students multiplied by 1.6 to determine the total percentage of meals reimbursed at the Federal free rate. One restriction is that the percentage derived from the calculation cannot exceed 100 percent. The remaining percentage of meals is reimbursed at the Federal paid rate. Once schools have their claiming percentages, they multiply them by the total number of breakfasts and lunches served to determine the number of meals claimed at the Federal free, reduced, and paid rates (USDA).

The main difference between UFB and a standard breakfast program (SBP) is that UFB offers breakfast at no charge to all students, irrespective of their household income. Breakfast is given free to any student who wants it that day and the school files for the Federal reimbursement at the correct income category for that student. In terms of nutrition standards, there is no difference in nutrition standards between UFB and SBP. There is also very little within district variation in the breakfast offered at the different schools, eliminating the possible bias of UFB or SBP schools offering different nutritional quality via different breakfast. The data also shows schools that always participate in UFB or schools that ever participate served more lunches and breakfast than schools that never participate in UFB.[3] Tables 1.1 and 1.2 show the summary statistics for the number of free, reduced, and full paid lunches and breakfast for the three types of schools. As they both show, always and ever-participating UFB schools give out almost two and three times the number of breakfast as schools that never participated in UFB, respectively. One contributing factor is that UFB helps reduce the stigma attached to eating breakfast at school and provides breakfast for those students who cannot afford the cost of breakfast. It also now allowed students that were just above to cutoff the have breakfast cost free (Schwartz and Rothbart [2020]). Lastly, Tables 1.1 and 1.2 show the free and reduced served rates and eligible rates. The eligible rate is the total number of free and reduced price eligible students divided by the student population for that school, where the served rate is the number of free and reduced priced lunches/breakfasts served divided by the total number of lunches/breakfasts served. The served rate is higher than the eligible rate for all three types of schools (never, always, and ever-participating schools), indicating that more students from low-income families rely on the school for food than those from higher-income families. More striking is the difference in breakfast served and eligible rates. Again, all three types of schools show a higher served rate than eligible rate, but both ever and never participating schools had served rates of over 84 percent. This helps support the notion that low-income students primarily depend on school breakfast as their main meal in the morning, while higher-income students do not. Most recently, during the social distancing and stay-at-home orders implemented by most US states, the USDA came out with the SFSP/SSO COVID-19 waiver. This waiver stated that schools with a free and reduced rate of 50 percent or higher may develop meal distribution methods in which meals are available to all families with children enrolled in that school, with a focus on serving low-income children. For dismissed schools with less than 50 percent free or reduced price enrollment, meal distribution methods must more directly target the households of enrolled children who are eligible for free or reduced price meals (Porter [2020], USDA [2020], VOX [2020]). This swift waiver and support shows that there is a great need to provide meals to students, specifically students of low-income families. (recommended to insert Tables 1.1 and 1.2 here).

Some individuals might have a concern about sample selection, certain types of schools choosing to opt for UFB, and omitted variable bias, such as other programs that were implemented during the sample years that also targeted conflict in schools. With regard to sample selection, one would think that richer schools would be more likely to participate because they can afford to, but the competing force that pushes against them is that richer schools are less likely to rely on the school for student meals and would not want any negative stigma that might be associated with UFB participation. Referring back to Tables 1.1 and 1.2, the free and reduced meal served rates for all three types of UFB schools was higher than that of the actual eligible rate, showing that lower-income students depend more on schools for their meals than do higher-income students. In addition to that, Table 1.4 shows school-level demographics and discipline averages for never-participating, ever-participating, and always-participating schools. Both the ever-participating and always-participating have averages for all variables that are close to each other. The never-participating averages are slightly lower for all the discipline groups except for truancy and substance abuse. They also have slightly lower demographic averages for the schools' free and reduced lunch rate and bilingual rate, but a much higher Asian rate and graduation rate. To mitigate any possible bias, we follow the same approach as Gordon and Ruffini [2018] and in the main analysis excluded schools that never participate from the regression analysis. We instead focus on schools that always participate and schools that ever participate in UFB by school year 2016/2017. Lastly, we also contacted TEA's Department of Programs and Reporting, and asked them to check if there were any state-wide programs that were also being rolled out during the sample years that specifically targeted conflict. They confirmed there were not.

1.2 Data

This paper uses two unique data sets. The first comes from the Texas Department of Agriculture. It tracks all schools in the state of Texas that participate in UFB and schools that participate in an SBP. The data spans from school years 2011/2012-2016/2107. The UFB to non-UFB participation ratio of schools averaged 27.3 percent throughout the sample period. The second data set, acquired from the Texas Education Administration (TEA), contains annual school-level disciplinary reports, school-level meets or exceeds passing rates, and school-level characteristics and demographics. The discipline data reports detailed numbers over total counts of students and actions (incidences) by discipline action groups and discipline action reasons. This data represents the highest level of detail the state records. In total, there are over 50 different categories that a discipline action can fall into. Because of the large number of different categories, we grouped similar action reasons into subgroups.[4] Action reason 21, Code of Conduct violation, accounted for 31 percent of the total conflict reports. Due to federal and state privacy regulations, some of the data came masked. If data was masked, it was because the reported actions for that school fell between 1 and 9.[5]

To overcome the masked data problem, we looked at four different options. First, replace all the masked data with the lowest bound of 1; second, replace masked data with 5 the middle number on the scale; third, replace it with 9 the highest bound of the masked data; lastly, drop all masked data. To pick the best option, we ran equation 1 on all 4 different options and the point estimates for the low parameter and dropping all masked data were almost identical, -0.182 and -0.189, respectively. The middle and high parameter were both also nearly identical with each being -0.166 and -0.161, respectively. Because the point estimates were all close, the models are all run with the middle bound, replacing the masked data with 5.[6] This would allow for the analysis to not omit the masked data and represent the middle bound of the possibilities that the masked data could be. The complete summary statistics of conflict, test, and school demographics data is presented in Table 1.3. Lastly, the discipline data was reported in a manner such that if a school reports 0 for a type of discipline it meant that there were no reported incidents of that discipline action. Therefore, taking the log of zero will cause it to be dropped in the regression. To overcome this, we used the inverse hyperbolic sine (IHS) of the reported conflict rather than the more traditional log of conflict. Estimated coefficients can be interpreted in the same way as with a log transformed dependent variable but, unlike with the log of conflict, IHS is defined for zero data points.[7] The IHS is defined as $log(Y_{it}(1+Y_{it}^2)^{0.5}$, where Y_{it} is the conflict report for school i during time t. (See Burbidge et al. [1988]).

Testing data was acquired from each school's Texas Academic Performance Report (TAPR). This report contains each schools TAKS or STAAR results which reported the percentage of students that meets or exceeds the states passing standard. Before spring of 2012 the states standardized test was the TAKS test (Texas Assessment of Knowledge and Skills). The STAAR test (State of Texas Assessments of Academic Readiness) replaced the TAKS test and each year post implementation the meets or exceeds standard increased. The performance section of the TAPR shows STAAR/TAKS performance by grade, subject, and performance level.[8] Following past literature, we standardized the passing rates by taking the standard score, also know as the z-score ((x-mean)/SD) ,for each school per year and used it as the outcome variable of interest.[9] The advantage of standardizing the scores, especially when the passing rate was increasing from year to year, is it allows comparison of scores on different kinds of variables by standardizing the distribution. A positive z-score means the data value is larger than the mean. If a data value has a z-score of 2, that tells us that this data value is 2 standard deviations larger than the mean. The same goes from if the value is negative.

Yearly school level demographics and characteristics were also acquired from TEA. The data included student body composition, total enrollment, student to teacher ratio, and the graduation rate for the high schools. Table 1.3 shows the summary statistics for the school-level demographics, conflict reports, and STAAR/TAKS passing rates for all schools in the sample, as well as by school type (high school, middle school, and elementary school). Table 1.4 shows the summary statistics when we break the schools into UFB groups (never-participating, always-participating, and ever-participating). This additional table helps address concerns over school sample selection into the program, but as the table shows, there is little difference between the three types. Additionally, the never-participating schools are omitted from the main specification results. Lastly, in the appendix there is a visual representation of the location of each school in the state, colored by the schools UFB group.[10] Lastly, county-level wages and employment data was acquired from the Bureau of Labor Statics. (Recommended to insert Tables 1.3 and 1.4 about here)

1.3 Methodology

1.3.1 Test Scores

The preferred estimation strategy compares changes in academic performance and various discipline reports within ever-participating and always-participating universal free breakfast schools. Ever-participating schools are schools that have at one point adopted UFB in the sample period. Always-participating schools are schools that always participate. By focusing on just the two types of schools in the main analysis, we are able to estimate the effect of universal free breakfast without concern for eligibility with the decision to participate.

For comparative purposes, we start off by first exploring the following fixed-effects model to explore the relationship between UFB and a schools' meets or exceeds passing rate and discipline reports:

$$Y_{it}{=}\beta_1 UFB_{it} + \beta_2 AWC_{it} + \beta_3 AEC_{it} + \beta_4 X_{it} + \delta_d + \sigma_t + \epsilon_{it} \tag{1.1}$$

Where Y_{it} is the z-score of students that meet or exceed standards on the state test, for school i during year t, β_1 is the coefficient for UFB Indicator which equals one if the school in year t participated in the UFB program, β_2 is the coefficient for county annual wage, β_3 is the coefficient the county annual employee, β_4 is the coefficient for the set of control variables for school i at time t,[11] δ_d is the district fixed effects, and σ_t is year fixed effects. Analyzing academic performance and UFB participation is useful but is not the main specification or analysis of this paper. Numerous past research has already contributed to this topic, but it is useful in our analysis to better link how participation in UFB effects both academic progress and discipline actions.

1.3.2 Conflict in Schools

Equation 1 is also used as the initial analysis on UFB participation and discipline actions. Each one of the discipline groups was used as the outcome variable to see how overall discipline was affected and how individual discipline actions were affected. The FE model is useful for causal inference because it controls for all fixed characteristics, both observed and unobserved, that may confound the estimate of the effect of discipline on UFB participation.

Fixed effects models use within-unit changes over time to estimate the causal effect, with units serving as their own controls. If time-varying confounding remains a concern, an external control group may help provide a counterfactual for what would have happened to the units with exposure changes in the absence of that change. To overcome this limitation the use of a DID design utilizes policy changes rather than time-invariant policies that differ across jurisdictions. By controlling for all fixed differences between schools and shared changes over time, the DID model focuses on changes in the exposure of interest that occur in some schools but not others and can thereby estimate the unbiased causal effect of the exposure. Specifically, we estimate equation (2) as:

$$Y_{it}{=}\beta_1(UFB_i * Participate_{it}) + \beta_2 AWC_{it} + \beta_3 AEC_{it} + \beta_4 X_{it} + \delta_d + \sigma_t + \epsilon_{it} \tag{1.2}$$

Where Y_{it} is one of the discipline outcomes for school i during year t, β_1 is the coefficient for UFB non-time varying variable that equals one if a school participates in UFB by 2017. $Participate_{it}$ equals one each year a school participates in UFB. The measure of interest is the interaction of these terms, which equals one if a school that participates in UFB at any point through 2017 participates in year t. β_2 is the coefficient for county annual wage, β_3 is the coefficient the county annual employee, β_4 is the coefficient for the set of school level control variables for school i at time t,[12] we include district fixed effects, δ_d, to account for time-invariant district factors, as well as year fixed effects, σ_t, to account for temporal trends in discipline, and temporal changes due to and state advocacy, media, and any policy that is affecting all schools at the same time. This approach is most similar to that used in papers that analyze changes within a state stemming from universal meal program (Comperatore and Fuller [2018], Kho [2018], Gordon and Ruffini [2018]).

Lastly, effective September 1, 2013, the Senate Bill 376 (83rd Texas Legislative Session) was passed that requires all schools (contracting entities (CEs)) with 80 percent or more free/reduced-price meal eligible students to participate in UFB (Legislature Of The State Of Texas [2013]). The passage of this Bill allowed for the use of a regression discontinuity design. Specifically, a fuzzy regression discontinuity (FRD) because there are schools that are below the 80% cutoff that also participate in UFB. FRD estimates local average treatment effects around the cutoff point, where treatment schools and control schools are most similar and provides useful evidence on whether a program, such as UFB, should be cut or expanded at the margin. The FRD is not used as the main specification of choice because the FRD estimates the local average treatment effects around the cutoff point, the estimate does not necessarily apply to schools with free and reduced rates further away from the cutoff point. Combining the FRD and DID results is key for the goal of this paper, to see not only if schools should adopt UFB, the average treatment effect, but also if it should be expanded, the local average treatment effect. Figure 3 graphically shows the discontinuity in UFB participation between the schools above and below the cutoff both before and after the bill was passed.[13] Both before and after the Bill was passed, one can see the same general trend below the cutoff. Before the Bill was passed, UFB participation peaked at about the 80 percent free/reduced mark and then gradually fell. This is in sharp contrast to the situation observed after the bill was passed, with 100 percent UFB participation after the cutoff point. The bottom graph shows the discontinuity around the 80 percent free/reduced mark, but now with total school conflict as the outcome variable. The window was restricted to just schools that had a free/reduced rate of 60 percent and higher, but as it shows because schools are forced to opt into UFB after 80 percent they also have lower conflict than schools that are right before the cutoff. Because of this, we are able to identify the causal effect through an FRD design, where the threshold serves as the instrument for participation in the program (Lehmann and Matarazzo [2019]). The first and second stage regressions are as follows:

$$\widehat{UFBI}_{it} = \beta_1 FreeReducedRate_{it} + \beta_2 AWC_{it} + \beta_3 AEC_{it} + \beta_4 X_{it} + \delta_d + \sigma_t + \epsilon_{it} \tag{1.3}$$

and

$$Y_{it} = \alpha + \beta_1 \widehat{UFBI}_{it} + \beta_2 AWC_{it} + \beta_3 AEC_{it} + \beta_4 X_{it} + \delta_d + \sigma_t + \epsilon_{it} \tag{1.4}$$

Where in the first stage of the regression, UFB is the outcome and the school's free and reduced rate is the instrument. All the same controls are used in equations 4 and 5 as were used and described in equation 1. In the second stage the results for $\widehat{UFBI}_{it}$ are used and the outcome variable is total school conflict. (Recommended to insert figure 3 here)

We closely follow the empirical approaches that Calonico et al. [2014, 2015] have detailed in their research. We followed Calonico, Cattaneo and Titiunik (2015) to find the optimal data-driven RD bandwidth selection by calculating the number of bins based on the mimicking-variance quantile-spaced method using polynomial regression. This approach allowed for the variability of the local sample means to change across bins only due to non-constant conditional variances (because of to the presence of heteroscedasticity), but not due to different sample sizes in each bin, like in other bin selection methods. The mimicking variability picks the number of bins so that the binned sample means have an integrated (asymptotic) variability closely equal to the amount of variability of the raw data. Specifically, we can allow V_{-} and V_{+} represent the variance of the outcome variables (Total School Conflict) for control (schools with free/reduced meals under 80 percent and not participating in UFB) and treatment units (schools over 80 percent). Thus, the optimal number of bins would come from the following:

$$var_{QS,-}(J_{-,n}) = V_{-} \tag{1.5}$$

and

$$var_{QS,+}(J_{+,n}) = V_{+} \tag{1.6}$$

This allows for the number of bins for each group's mean to have approximately the same asymptotic variability in the QS-based local sample, equal to the overall variability of the data.

1.4 Results

1.4.1 Test Scores

As a first pass, we look at all students' combined and math test z-scores. The z-score for each school was used as the outcome variable of choice because it allows comparison of scores on different kinds of variables by standardizing the distribution. Results for the estimation of Equation 1 are presented in Table 1.5. Columns 1 and 2 show the z-score for all tests and math test, respectively. Schools that participated in UFB show higher z-scores by 0.049 and 0.059, respectively. The value of the z-score tells you how many standard deviations you are away from the mean. This means if the z-score is equal to 0, it is on the mean. Whereas, a positive z-score indicates the raw score is higher than the mean average. In the case for schools that participated in

UFB they have a z-score that is 0.049 standard deviations above the mean for all tests and 0.059 standard deviations above the mean for math tests.

Next, columns 3, 4 show the same results, but focusing only on high school. The results show that high schools actually experience a higher return from participation in UFB. For high school, the combined test z-scores was by $.135$ standard deviations above the mean, and math only test z-scores were by $.16$ standard deviations above the mean.

These results are very promising, especially the high school results that show a large return on participation. They also align with the results of other researchers such as Dotter [2013], Ruffini [2018], and Frisvold [2015]. As promising as these results are, the main analysis and purpose of this paper was to examine discipline outcomes and link those results to the already abundant literature over academic outcomes. This will have the potential for policy makers and school leaders to have a more robust picture of the overall benefits of UFB for their and student body. (Recommended to insert Table 1.5 here)

1.4.2 School Discipline Fixed Effects Results

As discussed previously, we know that cognitive, behavioral, and academic outcomes are interdependent. An increase in a schools' meets or exceeds percentage does not solve the puzzle. It instead suggests a deeper probe that looks at school-level discipline would help further round out the analysis. As previously discussed, some of the schools might have reported 0 for a specific discipline outcome, such as truancy or substance abuse. Using the natural log of the data would omit those data points from the analysis. To overcome this, we used the inverse hyperbolic sine which can be interpreted in exactly the same way as a standard logarithmic dependent variable[14] (Burbidge et al. [1988]). As a double check, we also ran the model using both the natural log and natural log $+1$ and compared all the outcome. These results are presented in the appendix and the point estimates for all three were almost identical.

Table 1.6 shows the results for equation 1 for total conflict, as well as the other discipline groups for all grades and school. Controlling for both school and county level demographics, the model shows that there is a 16.6 percent decrease in total discipline reports at a school that participates in UFB compared to schools that do not. At the mean, an always participating school has on average 269 total discipline reports a year, and this would be reduced by 45 reports. Once the outcome variable is switched to one of the more specific discipline groups, we see the same general reduction for each one, except the placebo test of weapon violation. Under Texas Education Code of conduct 37.002 (b) "A teacher may remove from class a student who has been documented by the teacher to repeatedly interfere with the teacher's ability to communicate effectively with the students in the class or with the ability of the student's classmates to learn, or whose behavior the teacher determines is so unruly, disruptive, or abusive that it seriously interferes with the teacher's ability to communicate effectively with the students in the class or with the ability of the student's classmates to learn." (Agency [2019]). Using equation 1, code of conduct reports are reduced by 16.1 percent. This reduction in common classroom disruptions is not only economically significant and highly meaningful, but also meaningful in direct and indirect ways. Directly, participating schools on average will see yearly total code of conduct violations decrease from 241 to 202. Indirectly participating schools will see positive spillovers through an overall enhancement in the classroom environment, leading to a more positive learning environment for all students. This can help us link back to the increase in test results that we saw in the previous section.

The substance abuse categories includes any conflict report for alcohol, tobacco, or any controlled substance. Column 3 of Table 1.6 shows that participating schools experience a reduction in substance abuse by 12.2 percent. The driving force behind this result can come from many different directions. One might think that being hungry is the driving factor for kids to substitute food with tobacco/drugs. Mineur et al. [2011] from Yale and the Baylor College of medicine, found that smoking decreases appetite, and smokers often report that they smoke to control their weight. Singh [2014] showed how there is a complex relationship between food and mood; that, with a lack of proper nutrition your mood is negatively impacted. Research from a number of organizations, including APA and Alcoholics Anonymous, have recognized that issues with irritation and the expression of more negative/depressed moods and trying to managing anger can lead to an exacerbation of substance abuse. Given this linkage, another plausible explanation is that students now are getting better/more proper nutrition in the mornings, which results in them being less irritated, moody, or on edge and not reaching out to tobacco/drugs.

In Texas, truancy is reported for students that are in the age range of 12-18 years with 10 or more unexcused absences per semester or failure to attend school because they failed to enroll. As column 4 of Table 1.6 shows, participating schools saw a reduction in truancy of 22.1 percent. The question arising here is why are these truant students coming to school more? First, as noted earlier, UFB reduces the negative stigma that is associated with being on the free/reduced meal plan. There is no longer the negative social aspect related to breakfast and students are more willing to come to school and eat. As they have better access to regular nutrition, they are gaining more classroom learning time, as well as not acting out as much, in turn leading to an increase in test scores. Another benefit from an increase in student attendance is the increase in school funding that participating schools receive because most schools receive some part of their funding based on the number of students that attend school daily. The average student misses about 2-3 days a semester (García and Weiss [2018]), but truancy kicks in on the 10^{th} absent days per semester. The state of Texas pays each school $\$45$ a day for each student they have in attendance. Also, the mean truancy rate across all high schools that do not participate in UFB is 62. When these schools participate in UFB, truancy drops by 22.1 percent; in other words, a reduction by 13-14 reports. This decrease in truancy could generate a school an additional $\$5,000$ to $\$8,000$ in attendance revenue.[15] Lastly, the students that are constantly absent for unexcused reasons are highly likely to be the same students that care the least about school attendance, cause classroom disruptions, or fall on the lower end of the socioeconomic ladder and would benefit the most from more focus on their education. A final implicit spillover from increasing attendance would be, with all other factors kept constant, that the conflict rates per student would decrease. This is a common statistic that schools are evaluated on.

Fighting in schools never ends well for either party involved. It is one of the more serious conflicts a student can engaged in. Some fighting might be premeditated, but for many fights, teachers report that they come out of nowhere and the people involved seemed to be off that day. This can be circled back to the fact that being hungry causes an individual's mood to be highly sensitive to swings, resulting in students acting on aggression for insignificant reasons. As column 5 of Table 1.6 shows, participating schools saw a reduction in fighting by 13.5 percent, or on the mean, a reduction from 32 reported fights per year to 28. These results show the cognitive, behavioral, and academic benefits of regular access to nutrition for our youth.

There were some actions that did not fit into well-defined categories such as substance abuse, fighting, or truancy, so we grouped them under the "Other" sub-category. Examples that fall under this category include terrorism threats, arson, title 5 felony off-campus, and sexual assault. All of these conflict outcomes are very serious actions. Individually, each one of these conflict reports account for a small proportion of the total conflict number, but once they are all grouped together, there was enough data points for the model to work. As Table 1.6 shows, there was a significant decrease in the return to UFB participation, a decrease of 10.5 percent.

So far, we have seen significant reductions in all the conflict categories, but the one that showed no significant change was the category of weapon violations. These conflict reports were written for students that brought knifes, firearms, and other prohibited weapons to school. Generally, these violations require more planning. If a student is going to bring a weapon to school, they need to make that choice before they come, so simply eating breakfast should not have an effect here. That is exactly what the results show. This observation was not needed, but it was a good robustness check to determine whether the link between food and academic/behavioral was clean.

(Recommended to insert Table 1.6 about here)

1.4.3 Difference in Differences Results

Equation 2, the DID model, is the specification of choice because it focuses on changes in the exposure of interest that occur in some schools but not others and can thereby estimate the unbiased causal effect of the exposure. Following Gordon and Ruffini [2018], the never-participating schools were omitted and only schools that would participate in UFB by the end of school year 2016/2017 were kept. Table 1.7 presents the results of equation 2 and the same trend from the fixed effects model are present. Here, we see that total yearly school level discipline reports are reduced by 17.1 percent for schools that participate in UFB compared to schools that had not yet participated. This indicates at the mean, total discipline reports would be reduced from 268 to 222, a reduction of 46 reports. Code of Conduct violations decrease by 16.6 percent, and substance abuse reports decrease by 12.1 percent. Truancy reports decrease by 23.5 percent; which is close to the same point estimates that Leos-Urbel et al. [2013] found when they looked at schools in New York City. Fighting reports also decreased by 14 percent, an increase in point estimate from the fix effects model and is similar to what Altindag et al. [2020] found in their study.

This subsection specifically looks at the results of Equation (2) and discipline at the high school level. High school analysis and behavioral analysis is an area that is missing in the past research that typically focuses on elementary and middle school students. In addition, Sepe [2009] in her report to the Maryland Department of Education found that skipping breakfast increased with age and high school students are more likely to skip breakfast compared to middle and elementary students. Sweeney and Horishita [2005] found that 57 percent of inner-city high school students reported skipping breakfast because of a lack of time or the negative stigma associated with meal programs at school. High school students may also by more involved in extra-curricular activities, such as sports, bands, choirs, and other that require more fuel for the body to perform efficiently. Given these facts, schools that offer UFB have the potential to increase student meal participation and at the same time possibly reduce discipline reports. Table 1.8 reports the results of this analysis. Schools that adopt UFB see total discipline reports decrease by 12.8 percent and code of conduct reports by 13.5 percent. We believe all of the possible channels that were discussed in the previous sections are evident here, as well as when focusing on just high schools. Additionally, column 3 (Substance Abuse), 4 (Truancy), and 5 (Fighting) also see a decrease at the high school level and the point estimates are closely matched with the ones in Table 1.6. Lastly, a reduction in both overall and classroom disruptions can only positively benefit the learning environment for the same reasons as discussed in the past section. This benefit comes at a critical time when high school students are preparing for post-secondary choices or to join the labor market.

1.4.4 Fuzzy Regression Discontinuity Design Results

The passage of Senate Bill 376 allowed for an FRD to be estimated. The FRD design was used to help supplement the DID results, as well as to provide insight into the local average treatment effect. The running variable is the school's free and reduced meal rates. If a school's free and reduced meal rate is 80 percent or more, then they are required to implement UFB. As mentioned earlier, because schools can opt for UFB without being at or above the 80 percent cutoff, this restricts the use of a sharp RD and for the use of the fuzzy RD.[16] Following past research, the sample was restricted to just schools that were 20 percent above and below the cutoff point. Figure 3 shows that there is discontinuity at the cut off once the bill was passed when compared to before. Before the bill was passed, UFB participation peaked around the 80 percent free/reduced mark. After the bill was passed, you see a clear discontinuity and jump to 100 percent UFB participation after the cutoff. This results in the bottom graph, where it shows again the 80 percent free/reduced cutoff. Now, with total school conflict as the outcome variable, schools right after the cutoff and up to about 95 percent have lower total school conflict than one that are just before the cutoff. Empirically, results from equations 3 and 4 are presented in Table 1.9. The first column presents the first-stage regression results when UFB is the outcome variable and the variable of interest on the right-hand side is the school's free and reduced rate. This is highly significant with the first stage F-stat is over 900 with a particle R-squared of 0.325. Both of these support the notion that it is highly unlikely that the results are driven by a weak instrument. Column 2 shows the second-stage regression results. Once UFB is instrumented, we see that there is a significant reduction in total conflict by 13.1 percent. This shows that the local average treatment effect is 13.1 percent lower because schools after the cutoff are forced to offer UFB when compared to schools just before the cutoff. As mentioned previously, the DID shows the average treatment effect; if schools should adopt UFB. The FRD shows the local average treatment effect, indicating if UFB should be expanded. Given that both models showed reductions in total conflict helps give clear evidence that there is a positive effect to adopting UFB. Lastly, according to Nathaniel Hendren and Ben Sprung-Keyser from Harvard University, investment in low-income children's health and education have historically had the highest return and have paid for themselves as governments have recouped the initial cost of their expenses through additional tax revenue collected and reduced transfers and transfer payments. (Hendren et al. [2019])

1.5 Conclusion

Food insecurity plagues roughly 13 million children around the world and in the US. Roughly 1 out of every 6 live in houses without sufficient access to food. In Texas, this ratio is 1 in 5 (USDA [2019]). In light of this trend, rates of food insecurity are noticeably higher than the national average among households whose incomes are near or below the Federal poverty line. In particular, among Black, Hispanic, and single-parent households. The children of these households would benefit the most from having regular access to nutrition coming in a manner that is free of stigma. This research gives policy-makers and politicians a new set of results that highlight a cost effective policy option to help increase cognitive, behavioral, and academic performance as well as bridge the education attainment gap. This is critical, as the gap is a large driver of the wage gap.

We investigated the relationship between a schools' "meets or exceeds" standards, numerous discipline outcomes, and whether or not schools participated in a standard breakfast program or universal free breakfast program. We analyzed both the average treatment effect and the local average average treatment effect. Analyzing both effects allowed us to strongly contribute to the literature and build a better analysis over free meal programs. The results from both the DID model (average treatment effect) and the FRD model (local average treatment affect), both show that when schools adopt UFB, they can have both direct and indirect positive benefits. One direct benefit includes students having increased access to meals. This is most notable for students that were just above the qualifying level for meal assistance, resulting in overall increases in their mental and physical health. Other direct benefits come from the reduction in discipline reports and increases in academic performance. Specifically, with the reduction in truancy, schools will see an increase in state funding from the increase in attendance. With an increase in attendance, schools will receive funding they can use for other programs throughout their school. Indirectly, students that are already consuming a regular breakfast gain from positive spillovers. Benefits come in the form of a better classroom environment, given that code of conduct violations are reduced by 16.6 percent. Better classroom environments can help lead to more quality instruction time. Outside of the classroom, the overall environment of the school is better with a reduction in total discipline reports. Less truancy, substance abuse, or code of conduct violations can help make the school a safer and better place for all if its students, teachers, and staff.

Authors' Notes

Opinions expressed in this essay are solely our own responsibility and do not reflect the view of any agency and any errors are ours.

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Notes

[1]A complete list the conflict outcomes is reported in the appendix.

[2] We performed a regression discontinuity and used each school's free/reduced rate as the running variable. The RD plot and results are presented in section 5.

[3]Ever-participating schools are schools that switch out and in to the UFB program throughout the sample years, always-participating schools are schools that always participate in the UFB program, and never-participating schools are schools that never participate in the UFB program.

[4] The complete breakdown of what action reason fell into each group is presented in Figure A1 in the appendix section of the paper.

[5] A majority of the masked data was from smaller schools or fell into the serious violations group that happen very infrequently. This group includes reports such as terrorism threat, arson, title 5 felony off campus, and aggravated kidnapping.

[6]Table A1 with regression results for the fixed effect model are presented in the appendix that shows the complete results for each bound.

[7]Table A3 in the appendix shows the comparison for the IHS, natural log, and natural log plus 1, and all points estimates are nearly identical.

[8] Included in the report was the passing rates for grade 3 (reading and mathematics), grade 4 (reading, mathematics, and writing), grade 5 (reading, mathematics, and science), grade 6 (reading and mathematics), grade 7 (reading, mathematics, and writing), grade 8 (reading, mathematics, science, and social studies). It also included End-of-Course (EOC) test for the following high school classes: English I, English II, Algebra I, Biology, and U.S. History.

[9] The STAAR test was first introduced in the 2011/2012 school year. Before STARR, the state of Texas was under the TAKS test. Because of the changing passing rate and switch in test, the ranking system and z-score were used instead of the raw passing rate to account for the changes.

[10] On average, there were about 8,744 schools per year in the sample.

[11] Control variables include Total Students, Black, Asian, Hispanic rates, and the Free lunch rate.

[12] Control variables in the DiD model are the same that were used in the FE model.

[13]Table A2 in the appendix shows the UFB participation rate for every 10th free/reduced percentile and what share of the total schools they account for.

[14]The inverse hyperbolic sine transformation is defined as $\log(\mathrm{Y_i} + (\mathrm{Y_i^2} + 1)^{1/2}$

[15] A complete payoff matrix is included in Table A4 in the appendix.

[16] Refer to Figure 3 to see the discontinuity at the cutoff point.

Tables

Table 1.1: Average Lunch Counts By UFB Group

	Lunch Total	Lunch Served Free	Lunch Served Reduced	Lunch Served Paid	F-R Served Rate	F-R Eligible Rate
Never UFB						
mean	43,983.97	23,613.1	2,876.831	17,494.04	.598	.427
sd	40,638.18	33,212.19	3,593.853	20,084.84	.324	.320
Always UFB						
mean	63,923.12	46,493.6	5,824.945	11,604.57	.810	.723
sd	48,743.72	38,067.84	5,342.588	11,542.37	.151	.193
Mixed UFB						
mean	70,504.55	48,935.46	4,980.639	16,588.45	.735	.642
sd	47,836.8	40,433.77	4,751.928	17,758.13	.216	.258

Summary table shows the average lunches served per year for each for each of the 3 types of student categories by a schools UFB group. Never UFB are schools that never implemented UFB throughout the sample. Always UFB are schools that were always on UFB, and Mixed are schools that opt in/out of UFB throughout the sample. F-R Served Rate stands for the ratio of lunches served Free-Reduced to total lunches served. This is different than the F-R Eligible Rate which is the ratio of the total number of students eligible for free and reduced meals compared to the total student body for a school. The difference between the two rates shows that more students that come from higher income families bring lunch to school. If a school is over 80% they are required to participate in UFB, but this law was not passed till 2015.

Table 1.2: Average Breakfast Counts By UFB Group

	Breakfast Total	Breakfast Served Free	Breakfast Served Reduced	Breakfast Served Paid	F-R Served Rate	F-R Eligible Rate
Never UFB						
mean	17,332.08	13,636.23	984.8665	2,710.986	.843	.472
sd	28,188.8	25,685.3	1,430.194	4,052.773	.324	.320
Always UFB						
mean	48,773.28	33,248.22	4,310.84	11,214.23	.770	.723
sd	35,076.74	25,531.98	3,926.12	11,917.25	.151	.193
Mixed UFB						
mean	37,963.05	30,812.25	2,230.025	4,920.778	.870	.642
sd	35,986.56	32,241.23	2,499.212	5,890.489	.216	.258

Summary table shows the average breakfast's served per year for each for each of the 3 types of student categories by a schools UFB group. Never UFB are schools that never implemented UFB throughout the sample. Always UFB are schools that were always on UFB, and Mixed are schools that opt in/out of UFB throughout the sample. F-R Served Rate stands for the ratio of breakfasts served Free-Reduced to total breakfasts served. This is different than the F-R Eligible Rate which is the ratio of the total number of students eligible for free and reduced meals compared to the total student body for a school. The difference between the two rates shows that more students that come from higher income families bring lunch to school. It also shows that more low income students depend on school breakfasts as their morning meal and higher income students eat at home. If a school is over 80% they are required to participate in UFB, but this law was not passed till 2015.

Table 1.3: School Demographics and STAAR Rates

	All Schools	High School	Middle School	Elementry School
	Mean	Mean	Mean	Mean
	SD	SD	SD	SD
UFB Rate	0.535	0.513	0.505	0.551
	(0.382)	(0.394)	(0.349)	(0.386)
Observations	78,695	19,203	14,277	45,215
STAAR Passing Rates	Mean	Mean	Mean	Mean
	SD	SD	SD	SD
	N	N	N	N
Combined Tests	55.60	50.35	54.89	57.82
	(14.25)	(13.96)	(13.65)	(14.25)
	(44,275)	(9,605)	(9,029)	(25,641)
Math	56.76	46.83	58.87	59.82
	(17.25)	(18.62)	(17.35)	(15.65)
	(38,126)	(8,358)	(8,471)	(21,297)
Reading	56.06	53.87	54.58	57.15
	(13.62)	(13.92)	(12.82)	(13.69)
	(41,541)	(7,275)	(8,935)	(25,331)
Writing	44.93	50.74	46.50	43.26
	(16.14)	(17.22)	(16.93)	(15.23)
	(9,360)	(671)	(3,265)	(5,424)
School Demographics	Mean	Mean	Mean	Mean
	SD	SD	SD	SD
Total Students	588.98	634.04	632.64	557.98
	(464.82)	(783.72)	(373.96)	(268.52)
FTE Total	40.39	48.81	43.78	36.18
	(28.46)	(49.25)	(21.80)	(15.32)
Teacher Student Ratio	14.44	12.43	14.24	15.24
	(12.35)	(18.03)	(2.85)	(10.83)
Black Rate	13.50	14.629	12.61	13.25
	(17.55)	(18.88)	(16.39)	(17.30)
Hispanic Rate	50.87	48.31	47.90	52.60
	(29.84)	(29.88)	(29.80)	(29.76)
White Rate	30.93	33.0	34.82	29.19
	(27.35)	(28.58)	(28.16)	(26.52)
Asian Rate	2.79	2.39	2.78	2.96
	(6.06)	(5.81)	(5.38)	(6.36)
Free Lunch Rate	63.43	58.95	59.48	66.19
	(28.52)	(25.15)	(25.20)	(25.84)
Bilingual Rate	17.43	9.36	10.50	22.23
	(19.26)	(14.34)	(12.77)	(20.61)
Observations	78,695	19,203	14,277	45,215
School Discipline Reports	Mean	Mean	Mean	Mean
	SD	SD	SD	SD
Total	70.68	132.89	214.34	31.45
	(306.30)	(476.49)	(464.60)	(172.71)
Code of Conduct	9.40	5.33	22.14	9.08
	(96.61)	(23.86)	(178.22)	(95.64)
Substance Abuse	5.50	7.44	5.12	2.80
	(16.63)	(21.51)	(10.84)	(9.13)
Fighting	2.36	2.63	3.66	2.17
	(6.91)	(10.52)	(10.25)	(5.45)
Weapon Violation	1.46	1.51	2.23	1.36
	(1.76)	(1.51)	(2.99)	(1.60)
Truancy	2.27	4.70	2.87	1.87
	(7.25)	(17.70)	(8.20)	(3.43)
Assault	2.27	3.49	3.76	1.97
	(4.48)	(6.06)	(7.41)	(2.11)
Serous Violation	3.16	4.95	2.97	1.86
	(23.70)	(38.75)	(8.72)	(4.07)
Removal	1.88	1.09	2.58	1.97
	(2.67)	(1.09)	(2.95)	(2.88)

UFB Rate is the number of schools in the sample that participate in UFB. The discipline reports are total school reports. Passing rates are for grades 3 (reading and mathematics), grade 4(reading, mathematics, and writing), grade 5(reading, mathematics, and science), grade 6(reading and mathematics), grade 7(reading, mathematics, and writing), grade 8(reading, mathematics,science, and social studies). It also included End-of-Course (EOC) test for the following high school classes, English I, English II, Algebra I, Biology, and U.S. History.

Table 1.4: Summary Stats By UFB Group

Conflict	Always-Participating Mean SD	Never-Participating Mean SD	Ever-Participating Mean SD
Campus Total	268.31	204.40	265.58
	(580.51)	(533.25)	(541.20)
Conflict Per Person	0.675	0.566	0.451
	(2.14)	(1.79)	(1.29))
Code of Conduct PP	0.517	0.397	0.367
	(1.91)	(1.56)	(1.04)
Fighting PP	0.051	0.050	0.040
	(0.145)	(0.112)	(0.081)
Substance Abuse PP	0.125	0.190	0.061
	(0.592)	(0.937)	(0.287)
Truancy PP	0.069	0.104	0.040
	(0.358)	(0.274)	(0.106)
Observations	12,493	2,726	63,476
School Demographics	Mean SD	Mean SD	Mean SD
Bilingual Rate	16.66	14.79	16.85
	(18.79)	(18.11)	(19.47)
Black Rate	14.56	14.37	12.42
	(18.31)	(18.22)	(16.14))
Hispanic Rate	53.88	45.10	48.89
	(27.28)	(29.72)	(30.04)
Asian Rate	1.98	5.09	2.85
	(4.51)	(9.99)	(6.18)
American Indian Rate	0.418	0.447	0.435
	(1.06)	(1.41)	(0.861)
Teacher Ratio	14.58	15.16	14.87
	(12.22)	(9.50)	(9.52)
Free Lunch Rate	54.99	48.43	49.19
	(22.45)	(27.42)	(21.59)
Graduation Rate	51.08	72.10	54.98
	(49.61)	(44.64)	(49.37)
Observations	12,493	2,726	63,476

Summary table shows the conflict per person for each sub group and total conflict as well as the average demographics for each school type. Always-Participating schools are schools that always participate in UFB over the sample period, never-participating are schools that never do, while the ever-participating school group are schools that go off and on at least once throughout the sample period.

Table 1.5: STAAR/TAKS Meets of Exceeds Standards Results

	(All Schools) All Tests	(All Schools) Math Test	(High Sschol) All Tests	(High School) Math Test
UFB	0.049***	0.059***	0.135***	0.160***
	(0.012)	(0.012)	(0.033)	(0.036)
Black	-0.019***	-0.014***	-0.019***	-0.014***
	(0.001)	(0.001)	(0.002)	(0.002)
Asian	0.024***	0.020***	0.026***	0.019***
	(0.001)	(0.001)	(0.003)	(0.003)
Hispanic	-0.007***	-0.003***	-0.004***	0.001
	(0.001)	(0.001)	(0.001)	(0.002)
Free Lunch Rate	-0.014***	-0.011***	-0.025***	-0.019***
	(0.000)	(0.000)	(0.001)	(0.001)
Total Students	0.000***	0.001***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Annual Wage	-0.028	-0.188	-0.177	-0.129
	(0.169)	(0.191)	(0.270)	(0.336)
Annual Employees	0.752***	0.547**	0.713**	0.353
	(0.190)	(0.217)	(0.320)	(0.396)
Sample Years	2012-2017	2012-2017	2012-2017	2012-2017
School Demographics	Yes	Yes	Yes	Yes
County Wage and Employment	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	38,908	38,908	9,159	9,159
R-squared	0.411	0.382	0.628	0.546

Results are the z-score for each schools meets or exceeds rate. Each of the control variables are rates. Year 2012 represents school year 2011/2012, and year 2017 represents school year 2016/2017. So for all the schools the demographics, including free lunch, rates are calculated and used in the regression. School level demographics such as total students and ethnicity are controlled for. County level wage and employment is also included on the estimation. Passing rates are for grades 3 (reading and mathematics), grade 4(reading, mathematics, and writing), grade 5(reading, mathematics, and science), grade 6(reading and mathematics), grade 7(reading, mathematics, and writing), grade 8(reading, mathematics,science, and social studies). It also included End-of-Course (EOC) test for the following high school classes, English I, English II, Algebra I, Biology, and U.S. History Robust standard errors in parenthesis. $***p < 0.01, **p < 0.05, *p < 0.1$

Table 1.6: Fixed Effects Results

	(Total Discipline)	(Code of Conduct)	(Substance)	(Truancy)	(Fighting)	(Weapon)	(Other)
UFB	-0.166***	-0.161***	-0.122***	-0.221***	-0.135***	0.007	-0.105***
	(0.023)	(0.027)	(0.024)	(0.076)	(0.027)	(0.015)	(0.018)
Total Students	0.002***	0.002***	0.001***	0.001***	0.001***	0.000***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black Rate	0.021***	0.021***	0.001	0.012***	0.018***	-0.001*	0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)
Asian Rate	-0.027***	-0.026***	-0.018***	-0.016***	-0.011***	0.001	-0.010***
	(0.001)	(0.001)	(0.002)	(0.004)	(0.002)	(0.001)	(0.001)
Hispanic Rate	0.003***	0.003***	0.001	0.000	0.009***	-0.001***	0.001
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Free/Reduced Lunch Rate	0.004***	0.003***	0.011***	0.008***	-0.000	0.002***	0.006***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Annual County Wage	0.000***	0.000***	0.000***	0.000***	0.000***	0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Annual County Employees	-0.000***	-0.000***	-0.000***	-0.000	-0.000***	0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Sample Years	12-17	12-17	12-17	12-17	12-17	12-17	12-17
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53,074	46,536	15,463	3,156	19,095	7,693	19,830
R-squared	0.384	0.356	0.577	0.437	0.360	0.161	0.376

Results are in the IHS (Inverse Hyperbolic Sine), and total students per school is controlled for. Each of the control variables is a rate. For all the schools the demographics, including free lunch, rates are calculated and used in the regression. School level demographics such as total students and ethnicity are controlled for. Year 2012 represents school year 2011/2012, and year 2017 represents school year 2016/2017. Robust standard errors in parenthesis. $***p < 0.01, **p < 0.05, *p < 0.1$

Table 1.7: All Schools Difference in Differences Results

	(Total Discipline)	(Code of Conduct)	(Substance)	(Truancy)	(Fighting)	(Weapon)	(Other)
UFB Participation	-0.171***	-0.166***	-0.121***	-0.235***	-0.140***	0.007	-0.108***
	(0.023)	(0.027)	(0.024)	(0.077)	(0.027)	(0.015)	(0.018)
Total Students	0.002***	0.002***	0.001***	0.001***	0.001***	0.000***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black Rate	0.022***	0.021***	0.001	0.012***	0.018***	-0.001*	0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)
Asian Rate	-0.027***	-0.027***	-0.020***	-0.016***	-0.011***	0.001	-0.010***
	(0.001)	(0.001)	(0.002)	(0.004)	(0.002)	(0.001)	(0.001)
Hispanic Rate	0.003***	0.003***	0.001	0.001	0.009***	-0.001***	0.000
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.000)	(0.000)
Free/Reduced Lunch Rate	0.004***	0.003***	0.011***	0.008***	-0.000	0.002***	0.006***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Annual County Wage	0.000***	0.000***	0.000***	0.000***	0.000***	0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Annual County Employees	-0.000***	-0.000***	-0.000***	-0.000	-0.000***	0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Sample Years	12-17	12-17	12-17	12-17	12-17	12-17	12-17
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,703	44,567	14,902	3,040	18,417	7,474	19,127
R-squared	0.381	0.354	0.580	0.432	0.359	0.164	0.374

Results are in the IHS (Inverse Hyperbolic Sine), and total students per school is controlled for. Each of the control variables is a rate. For all the schools the demographics, including free lunch, rates are calculated and used in the regression. School level demographics such as total students and ethnicity are controlled for. Never-Participating schools are excluded from the analysis following the methods of Gordon and Ruffini [2018]. Year 2012 represents school year 2011/2012, and year 2017 represents school year 2016/2017. Robust standard errors in parenthesis. $***p < 0.01, **p < 0.05, *p < 0.1$

Table 1.8: High School DID Results

	(Total Discipline)	(Code of Conduct)	(Substance)	(Truancy)	(Fighting)	(Weapon)	(Other)
UFB Participation	-0.128**	-0.135**	-0.133***	-0.232	-0.066	0.011	-0.075*
	(0.050)	(0.061)	(0.042)	(0.233)	(0.051)	(0.042)	(0.039)
Total Students	0.001***	0.001***	0.001***	0.001***	0.001***	0.000***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black Rate	0.025***	0.028***	0.006***	0.028***	0.027***	0.000	0.012***
	(0.001)	(0.002)	(0.001)	(0.006)	(0.002)	(0.001)	(0.001)
Asian Rate	-0.029***	-0.030***	-0.020***	-0.003	-0.012***	-0.003	-0.011***
	(0.003)	(0.004)	(0.003)	(0.015)	(0.004)	(0.003)	(0.003)
Hispanic Rate	0.011***	0.011***	0.007***	0.026***	0.013***	-0.000	0.007***
	(0.001)	(0.001)	(0.001)	(0.006)	(0.001)	(0.001)	(0.001)
Free/Reduced Lunch Rate	0.004***	0.002	0.004***	0.004	0.002**	0.002***	0.005***
	(0.001)	(0.001)	(0.001)	(0.006)	(0.001)	(0.001)	(0.001)
Annual County Wage	0.000***	0.000*	0.000***	0.000***	0.000***	0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Annual County Employees	-0.000***	-0.000***	-0.000***	-0.000**	-0.000***	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Sample Years	12-17	12-17	12-17	12-17	12-17	12-17	12-17
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,886	10,260	6,877	812	5,303	2,499	6,577
R-squared	0.716	0.697	0.763	0.689	0.705	0.274	0.602

Results are in the IHS (Inverse Hyperbolic Sine), and total students per school is controlled for. Each of the control variables is a rate. For all the schools the demographics, including free lunch, rates are calculated and used in the regression. School level demographics such as total students and ethnicity are controlled for. The results are just for High School discipline reports and Never-Participating schools are excluded from the analysis following the methods of Gordon and Ruffini [2018]. Year 2012 represents school year 2011/2012, and year 2017 represents school year 2016/2017. Robust standard errors in parenthesis. $***p < 0.01, **p < 0.05, *p < 0.1$

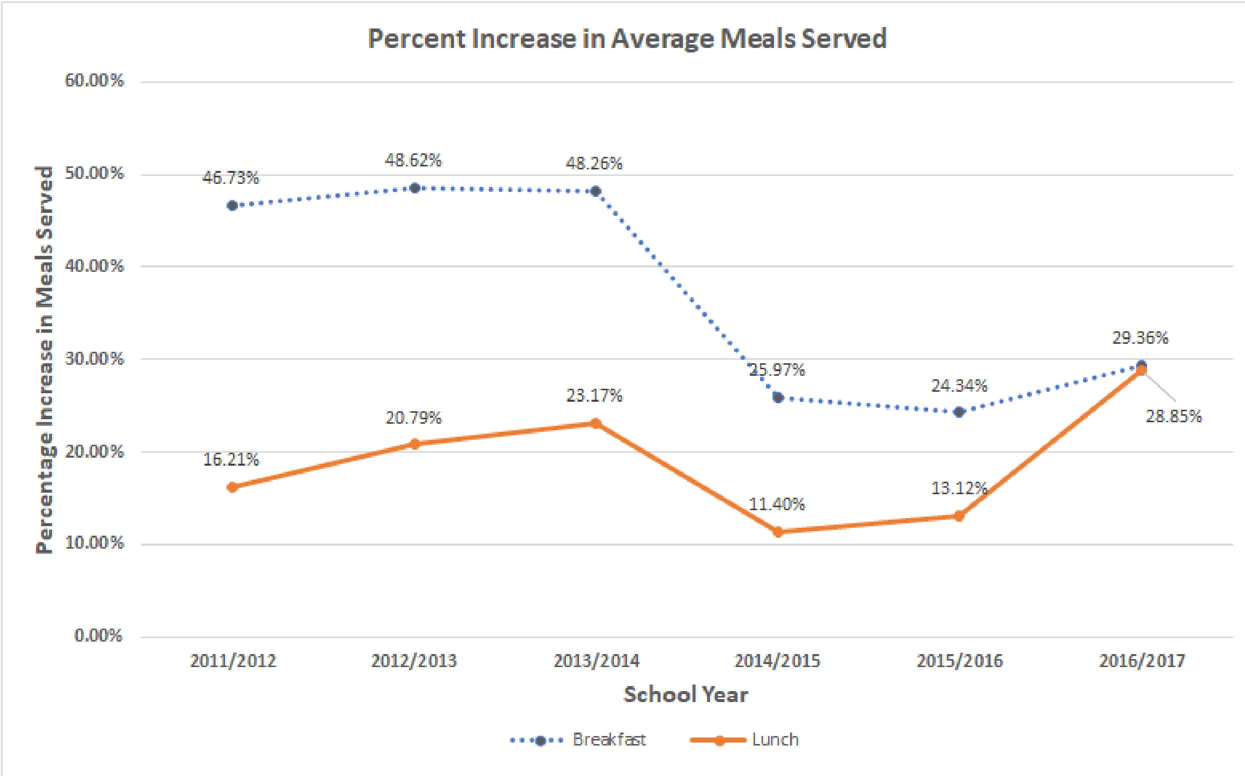
Table 1.9: 2SLS Fuzzy RD Results

	(1)	(2)
VARIABLES	UFB	Total Campus Conflict
Free and Reduced Rate	2.462***	
	(0.026)	
UFB		-0.131***
		(0.046)
Total Students	0.000***	0.002***
	(0.000)	(0.000)
Black Rate	0.002***	0.018***
	(0.000)	(0.001)
Asian Rate	-0.001***	-0.009***
	(0.000)	(0.003)
Hispanic Rate	0.002***	0.007
	(0.000)	(0.001)
At Risk Rate	0.000***	-0.013***
	(0.000)	(0.001)
Bilingual Rate	0.000***	-0.027
	(0.000)	(0.001)
Annual County Wage	-0.000	0.000
	(0.000)	(0.000)
Annual County Employees	-0.000	-0.000***
	(0.000)	(0.000)
Observations	16,093	16,093
R-squared	0.3252	0.348
Robust S.E.	YES	YES
Sample Years	2012-2017	2012-2017
F-stat	1434.94	

The cutoff point is 80% Free and Reduced rate, so the sample was restricted to schools that were both 20 points above and below the cutoff. The first column is the first stage regression results when UFB is the outcome variable and the variable of interest on the right hand side is the schools free and reduced rate, which is highly significant and the first stage F-stat is over 900 with an R-squared of .3252 which all points that it is highly unlikely that the results are driven by a weak instrument. Column 2 shows the second stage regression results, and once UFB is instrumented in we see that there is a reduction in total conflict of 13.1%, which is consistent with the graphs in figure 5 that showed that after the cut off there was a general reduction in total school conflict, and also with the results from the Fixed effects and DID models. $***p < 0.01, **p < 0.05, *p < 0.1$

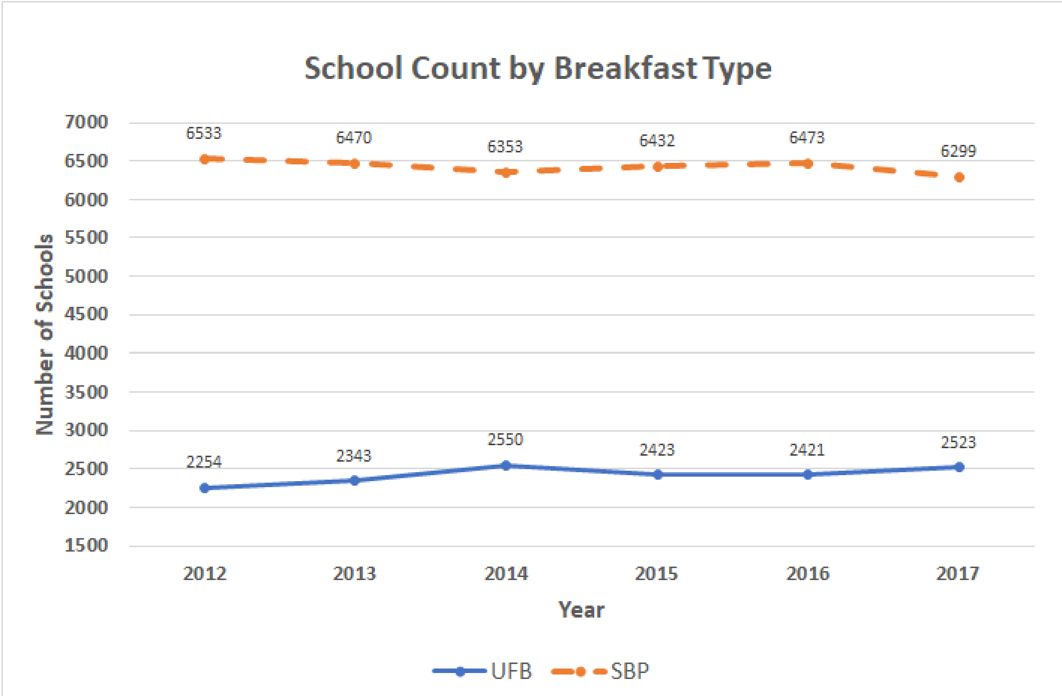
Figures

Figure 1.1: Percentage increase in served meals



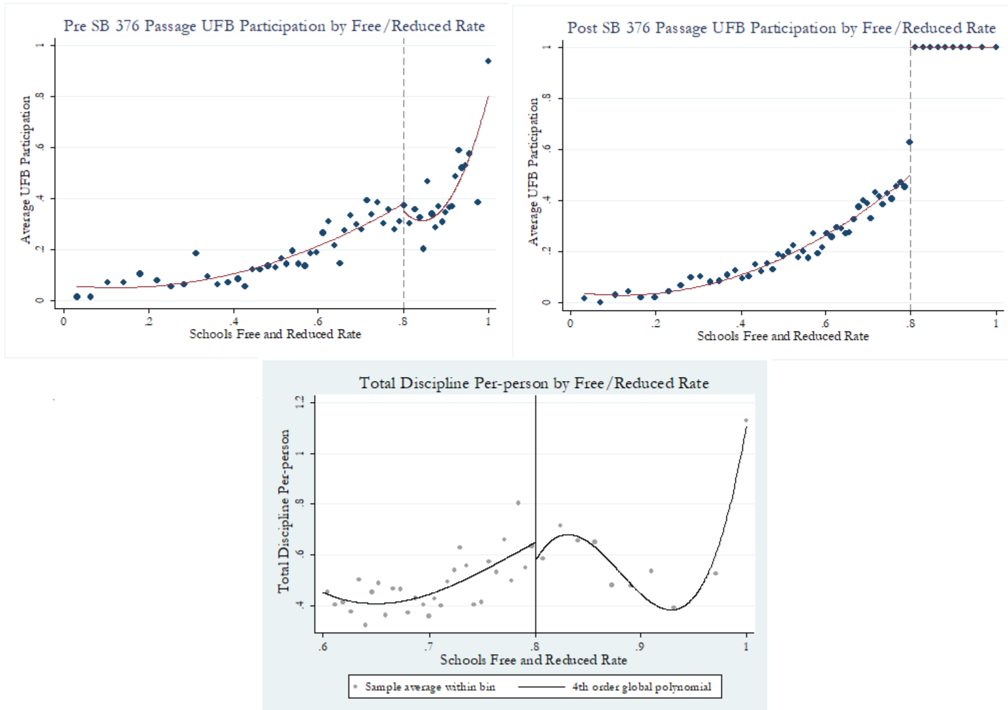
The figure shows the percentage increase in the total number of breakfasts and lunches served at UFB participating schools compared to schools that do not participate.The increase possibly comes from not only the reduction in negative stigma associated with free meals (Marples and Spillman [1995], Poppendieck [2010]), but also from students who would not be individually eligible, but now have access to free meals.

Figure 1.2: School Breakfast Count Graph



Source: Texas Education Agency In school year 2012/2013 there are 8,787 schools and at the end of the panel in school year 2016/2017 there are 8,822 school. Growth in school throughout the panel was minimal.

Figure 1.3: UFB Participation before and After SB 376



The Top left shows the average UFB participation by the schools Free and Reduced rate, which were school years 2011/2012 and 2012/2013. The top right graph shows the same but after SB 376 was passed requiring all schools with a free and reduced rate of 80% or more to participate in UFB. Quadratic fit lines are used in all three plots and the cutoff line is included in the Pre SB 376 plot just to show where it would be if the bill was already a law. Both before and after the bill was passed you see the same general trend below the cutoff. Before the bill was passed UFB participation peaked at about the 80% free and reduced mark then gradually staying the same then slightly increasing. This is in sharp contrast to after the bill was passed there was 100% UFB participation after a schools free and reduced rate was over 80%. The bottom plot shows the relationship with the outcome of interest, total school conflict. As it shows, once the bill was passed, and UFB was required at schools who's free and reduced rate was 80% or more, those schools after the cutoff and up until the 95% free and reduced rate see lower discipline reports.

Chapter 2

Fracking and Tracking: The Effects of Oil and Natural Gas Well locations on the housing market.

An individual's home is often their greatest source of wealth. Home ownership is also a part of the great "American Dream", where throughout generations ones home is viewed as a symbol of pride. Given these facts, it is not surprising that home owners care so deeply about maintaining their homes value.

Over the past 25 years the US has seen a huge increase in the construction and development of oil/gas well injection sites. According to the US Energy Information Administration, hydraulically fractured and horizontally drilled wells are now the majority of all new wells drilled, and by 2016, about 670,000 of the 977,000 producing wells were hydraulically fractured and horizontally drilled. Hydraulic fracturing has led to a mining boom across the US. Fracking technology in combination with horizontal drilling has made shale deposits that were previously not economically exploitable now profitable. Consequently, employment in the mining sector has reached levels not seen since the early 1990s (Fetzer et al. [2014]). This boom in the oil and gas industry helped make the United States the top oil producing country in the world, accounting for 18% of world oil production (U.S. Energy Information Administration). Energy prices and their connection to other economic variables, such as employment, wages, output, and urban development, has been an area of interest for academic economist since the oil price shocks in the 1970s (Berndt and Wood [1975]). Previous literature has shown that local communities around shale deposits can see an increase in employment in the mining community, as well as have significant spill-over effects into different sectors at the locations where resource extraction is taking place (Fetzer et al. [2014], Weber [2012, 2014], Weber et al. [2014]). Aside from creating thousands of jobs, fracking has also helped secure US energy security and can make the US economy less carbon intensive, as we have relied less on coal and more on natural gas production (U.S. Energy Information Administration).

However, this progress is not without new challenges and risks. Over the past decade, hydraulic fracturing has also been negatively highlighted in news reports and politics.[17] Hydraulic fracturing causes earthquakes, specifically in Oklahoma and Texas (Frohlich [2012]); wells that have leaking cases can contaminate air and groundwater (Darrah et al. [2014], Muehlenbachs et al. [2015]), and some have asserted the general presence of a well may be a disaminities for some individuals. In 2013, The Wall Street Journal analyzed well location and census data for more than 700 counties in 11 major energy-producing states and found that nearly 15.3 million Americans lived within one mile of a well that has been drilled since 2000 (Gold and McGinty).

One way to simultaneously measure the positive and negative impacts of the recent boom, is through the housing market, focusing on how individual home owners are affected through changes in their homes value. This paper focuses on this question. Using data from 11 different energy producing states, we focus on the variation in home sale prices and spatial differences in new well locations to analyze the impacts of hydraulic fracturing on homeowners.

So far, the literature on hydraulic fracturing and home prices has been focused on small populations, rural areas, small sample sizes, or how ground water treatment affects homes next to disposal sites (Boslett et al. [2016], Gopalakrishan and Klaiber [2014], Muehlenbachs et al. [2015]). The results are mixed with some of the research pointing to a decrease in home values when they are close to wells or when the home uses private ground water. On the other hand, homes that are on piped water see a slight increase. Balthrop and Hawley [2017] look at a specific county, Tarrant County, TX, and the Barnett Shale region, to see how homes are affected when they are within one half of a mile of a well. On the other hand, Weber et al. [2014] looked at the same shale region and found a net positive affect on home prices over the span of a decade. Our paper seeks to add clarity to the literature by using a large data set of wells and home across 11 different states and over a span of 18 years to analyze the longer run impacts new wells have on home prices. We also add to the literature by focusing on all sales, repeat sales, and utilizing a spatial difference-in-differences model that exploits quasi-experimental variation from homes that maintain their distance from the nearest well site throughout the sample period, compared with a treatment group that initially displays the same distance to wells as the control group, but then has this distance reduced through the construction of a closer well site.

The remaining portions of the paper proceed as follows. Section 2 describes briefly the background of hydraulic fracturing and some of the past literature. Section 3 describes the data. Section 4 describes the methodology. Section 5 present the results. Robustness checks and various extensions are presented in Section 6 and 7. Finally, sections concludes.

2.1 Background Literature

There have been two advances in well drilling technology that have made it economically meaningful to drill in shale deposit regions, hydraulic fracturing and horizontal drilling. Many areas across the country have shale oil that are of low pressure permeability, which means conventional drilling technology extraction would not be profitable. Hydraulic fracturing and horizontal drilling have alleviated these constraints. This new combination, from an economic and financial perspective, makes Fracking extremely cost effective. Oil and gas companies can drill one well production pad that acts as multiple wells, making the above ground footprint less invasive. Finally, if the well is connected directly to a pipeline, there is no need for above ground storage tanks to hold the oil/gas.

The process of fracking involves getting roughly a 0.5 acre of land to establish a pad foundation. Next, a company would come in with a drilling derrick about 150 ft tall, to bore the well. From the vertical well, multiple horizontal wells are drilled in any direction into the desired target area(s) below. Horizontal wells come in three varieties depending on the horizontal distance it takes the well to go from ground zero to the depth of the targeted area. Long radius wells, which require more than a thousand feet to reach 0 degree level (targeted area), and short and medium radius horizontal wells, that have sharper turns. According to Halliburton, once you start fracking it takes about 3-10 days. The process starts with a hole that is drilled 1,000 to 4,000 feet deep. Next, steel casings are placed into the well and the space between the casing and the hole are filled with cement. This process is needed to ensure groundwater is protected and to prevent gas leaks. This process is replicated numerous times, each time size of the casings gets smaller and smaller. Eventually, they eventually reach a total depth of 6,000 to 10,000 feet, where the gas can be accessed.[18] After the depth and casings are reached and complete, a perforated pipe gun is inserted into the horizontal part of the hole that produces explosions that create fractures in the shale. Fracking fluids, (3 to 5 million gallons of water) are mixed with chemicals and sand, and is then pumped at high pressure into fractures.

After fracking is complete and the drilling equipment is removed, the only visible structure above ground are 5-6 feet of surface valving left behind. Additionally, the average well can go two years before it needs to be extensively serviced, and can be re-fracked many times over. These new approaches to fracking have reduced the cost of drilling new wells, as well as, making it easier to place well sites in denser urban areas. Well location is also not random. Specifically, they are going to be placed in areas where there is oil or natural gas to be extracted and near major roads/highways for ease of access. As mentioned before, with horizontal drilling they can locate the well head closer to highways/roads, and still drill horizontally in multiple directions to access the target areas. In residential areas, this means companies will be vying for the same land as home builders. In our data we analyzed this relationship between home sales and new wells, finding that there is such a relationship. Specifically, homes and wells both target similar intersections and access to highways and roads. To mitigate the expected bias associated with this non-random placement we use a spacial difference-in-differences.[19]

Our paper fits into the broader recent research that has looked at the housing market and the amenities or dis-amenities that oil and gas wells have on home value. Currently there is no clear consensus on the short or long term affects, This is because some studies focus on more rural areas in the Marcellus Shale of Pennsylvania and New York (Boslett et al. [2016], Delgado et al. [2016], Gopalakrishan and Klaiber [2014], Muehlenbachs et al. [2015]), or specifically on the Barnett shale of Texas by the greater Dallas metro area (Balthrop and Hawley [2017], Weber [2014]). Our paper instead focuses on the broader cross-state effects of oil and gas well locations and the effects it has on the housing industry.

Hydraulic fracturing has the potential to provide many benefits, as well as costs, to local economies. Homeowners that have mineral rights can get royalty payments from the drilling and extraction of oil and gas.[20] Broader benefits come in the form of a boost to the local economy that hydraulic fracturing has been shown to bring. These impacts include increases in employment both within and outside of the mining sector. Fetzer et al. [2014] found that each oil and gas sector job creates about 2.17 other jobs. Maniloff and Mastromonaco [2015] found that counties with shale development had 6% higher wages.In other studies, findings suggest wages in the oil and gas sector increased by approximately 30%, wages in retail increased by 6%, and in the hotel sector wages increased by 17% (Marchand [2012], Mason et al. [2015], Weber [2012, 2014]). These wage increases were not limited to short term gains. Feyrer et al. [2017] find that roughly two-thirds of the wage income increase persists for two years. Ooms and Tracewski [2011] analyzed the rural Marcellus Shale region and found that counties that had an increase in wells also saw an increase in housing demand. Lastly, Raimi et al. [2020] used voting data from precinct-level results of a 2018 election in Colorado to analyze Proposition 112, a measure that would have made about 80 percent of Colorado's surface area off limits for drillers. The proposition failed. They found partisan affiliation correlates very strongly with support for oil and gas development, and that voters in precincts with higher levels of oil and gas activity are more supportive of the industry and drilling.

Hydraulic fracturing has the potential to negatively impact homeowners as well. When wells are being drilled and fractured there can be an increase in noise and air pollution, traffic, potentially spills and other environmental hazards (Lipscomb et al. [2012]). The greatest danger can come from a well leaking and potentially contaminating ground water.This phenomenon has been shown to have a negative impact on home values (Zabel and Guignet [2012]). Outside of fear of spills, the oil and gas industry equipment might bring other dis-amenities which may affect a homes sale price. The newer well heads being drilled leave a smaller above ground footprint, but during the drilling process itself has a large above ground footprint and may be unsightly (Lipscomb et al. [2012]). Gopalakrishnan and Klaiber (2014) show increased truck traffic and noise during construction and fracturing may be an annoyance for homeowners. Fear of pipeline explosions may drive some households to not want to live near well heads, but Boxall et al. [2005] found no evidence that pipelines negatively affect sales prices.

Previous studies on the effect of hydraulic fracturing on home values have been largely concentrated in two areas; the Marcellus Shale of Pennsylvania and New York and the Barnett shale of Texas by the greater Dallas metro area. In two papers by Muehlenbachs et al. [2012, 2015] they use a triple difference-in-difference approach to estimate the effect of fracking on home prices. The authors find negative effects of hydraulic fracturing are mostly felt by rural homes dependent on groundwater, while the effect for homes with piped water is slightly positive. In another study, focused on Washington County, PA, Gopalakrishnan and Klaiber (2014) find that negative effects of fracking are felt by homes on groundwater during the well construction phase, but the effects vastly decrease once well construction has finished and also as the distance from the well increases. Conversely, Delgado et al. [2015] focused on two counties in northeastern Pennsylvania, failing to find strong evidence that fracking significantly reduces home values. Following that, Boslett et al. [2016] utilize a drilling suspension in New York and find the likelihood of shale oil and gas drilling increases the value of properties.

A second wave of papers focus on the Barnett shale of Texas. Weber et al. [2014], analyzed this area and found that property values have appreciated in shale producing ZIP codes relative to non-shale ZIP codes over the course of a decade. Specifically, they show the increase in home prices is attributable to the effect shale gas development has had on public finances. The increase in revenues has allowed for more public spending, while still maintaining lower property taxes. Mineral rights are also important. If a homeowner has mineral rights, they are entitled to royalty payments from the production of a well. Vissing [2015] looks at factors driving tract-level heterogeneity in the caliber of leases executed by property owners who transfer their mineral rights to firms that drill and extract natural gas. Timmins and Vissing [2015] use a dual-gradient hedonic model to measure the capitalization of lease clauses into housing values. That they are able to analyze the effect of spacial proximity to hydraulically fractured wells and lease quality on a houses appraisal values. Lastly, Balthrop and Hawley [2017] looked at the Barnett shale of Texas and exploit variation in distance to nearby gas wells in home sale prices to estimate the effect. They looked at an urban area and found that wells within 3,500 feet reduces property values by 1.5%-3.0%. Our study covers 11 different states containing both rural and urban areas. Similer to Balthrop and Hawley [2017], we are able to estimate the net effect of wells on higher density settlements. Currently, the net effects on higher density areas is still unclear. In these areas more homeowners are being exposed to the full cost and the spillovers of being in close spatial location to hydraulic fracturing. The case could also be that in higher density possibly makes homeowners less sensitive to nearby drilling, like Raimi et al. [2020] found. Here the attest that because there are numerous alternative roads that can be used during construction, homeowners that are are around wells do not see any increase in wells as a negative. Lastly, this study is not confined to a single region but looks across multiple states. This will also allow for a more aggregate analysis that the long run impacts of the fracking boom has had on the housing market as a whole.

Finally, this study uses multiple empirical approaches including spatial variation with zip code and household fixed effects to measure the treatment effect of nearby hydraulically fractured wells on property values, a repeat sales estimator that looks at just repeat sales to examine how they change before and after a well is located next to them, and lastly a spatial difference-in-differences to help account for the non-random location of wells. Our data shows that new wells and new homes are both competing for desirable locations near the same roads, highways, interstates, and access points. By using a spatial difference-in-differences, we are able to mitigate location bias, and see how a homes price changes when it was once 2 miles away from a well, a new well comes in decreasing the distance to the nearest well, compared to homes that are always 2 miles away. Beyond that, the use of a repeat sales estimator with household level fixed effects allows us to better control for property level fixed effects. Others have controlled for property level fixed effects, but they were either in rural settings, or in a single county. Past papers that have controlled for spatial variation, such as Timmins and Vissing [2015] and Delgado et al. [2015], employ identification strategies that compare observations within the same time period and geographic boundary, that are near and far from a well. Balthrop and Hawley [2017] did have a panel analysis that spanned multiple years, but it does not cover as many years and was restricted to a single county in Texas. Finally, our paper contributes to the existing research by having a spatial analysis and a repeat sales model, both with ZIP and household fixed effects. This study also covers 18 years and analyzes homes across 11 high oil and gas producing stats. This allows us to examine both the larger cross-state macroeconomic effect. Additionally, we look individually at each state to see the microeconomic effect that the fracking boom has had on the housing market.

2.2 Data

2.2.1 Housing Data

We gather data from two main sources. The first comes from Zillow, a popular tool used by researchers and the public to search for properties available for sale in a given area. The company provides a centralized source of property transactions through its Zillow Transaction and Assessment Dataset (ZTRAX). [21] This dataset compiles multiple listing services (MLS) from all eleven oil producing states being used in my sample, between years 2000-2018, containing just over 16.4 million observations. The information includes details of a given housing market transaction, such as the sales price and date, as well as, large number of home characteristics, such as the number of rooms, bedrooms, bathrooms, square footage of the property, and any structures on it.

We consider all homes in each of the 11 states that are used in the sample, if and only if, the Zillow housing data is representative of a states' housing market. Unfortunately, MLS reporting standards are not uniform across all states. For example, Utah has only a few counties which consistently reports transactions to the states' MLS, so we exclude those observations from the main analysis[22]. Additionally, since we are focused on locations of wells and the affects on the housing market, I only consider homes which Zillow documents as residential properties. Some states do report business, government, and other non-residential properties, but these observations are excluded.

The data is also filtered for situations where it is likely a non-market transaction. We only included observations that are categorized as a deed transfer, which means the exchange of a property's title from one party to another. To ensure that results are not being driven by incorrect or improbable observations, transactions which had sales prices below $10,000 and above $10,000,000 are excluded, similar to Cheng et al. [2018]. On the lower end it is not likely that transactions who's prices fall below $10,000 occurred on the market. Such transactions may have slipped through the DataClassStndCode filter. Transactions with prices above $10,000,000 are extraordinary, and in some cases are possibly the result of data entry errors, as well as, not being representative of a states housing market. House characteristics are filtered as well to exclude observations that are in the top thousandth or top ten-thousandth percentile. This filtering, for example, eliminated any observation with over 1,000 bedrooms. This filtering process also removed a large number of observations in states which do not require counties to report the home characteristics, such as Utah and Wyoming. We provide a more comprehensive examination of our data cleaning process for the Zillow data in the Appendix. Summary statistics are presented in Table 2.1.

We also our data into sub-samples to check for potential sources of bias. Table 2.2 shows the total number of treatment homes for each state and the total number of treatment homes in one of the three buffer zones per state. States such as Utah and Virginia have low treatment to control ratios, but are included in the initial analysis. A robustness check we drop states with a low treatment to control ratio and re-estimate. Table 2.3 looks at the summary statistics for houses in ZIP codes that have wells present, relative to houses in ZIP codes that do not. Ideally, the means of these two samples should be similar. In fact, Table 2.4 shows they are very similar.

2.2.2 Oil and Natural Gas Well Data

The second data set contains the well information, it was acquired from the U.S. Department of Homeland Security. It provides a mostly complete listing of most of the oil and natural gas wells locations across the USA.[23] The data set contains just over 1 million well locations, and has detailed information such the wells operators, location by longitude and latitude, approval date, status, and type of well. Companies are not required to give a public notice about their intent to drill, but permit records are public information and individuals can access them. They are located in a sate level database on the web site of the agency that is in charge of oil and natural gas production.[24] This means that unless individual specifically is looking for this information they would not know if/when a well could be placed near their home. This suggests that home prices should not be affected before a well is actually built, since their is little foreknowledge of the drilling. Also, most of the wells can be turned on and off over night, and once it is in off mode, or if they become an orphaned well, most of the physical structure is still there, where homeowners can still see it. Table 2.4 shows how many wells were drilled between 2000 and 2018, and that years average crude oil closing price. Between 2000 and 2008 the yearly wells drilled gradually increases and spikes between 2006-2008. This is expected because during those years oil prices were hitting record highs. Drilling slowed down after oil prices went from a yearly closing average of $99.67 in 2008 to $61.95 in 2009. The biggest one year jump in wells drilled came in 2014 when 33,285 wells were drilled, which corresponds to a spike in oil prices when the yearly closing average went back up to $93.17. The following years, well drilling slowed drastically partially due to OPEC and Russia flooding the oil markets near the end of 2014 resulting in the average yearly closing price of oil in 2015 dropping to $48.72. Table 2.4 also shows how fast the oil and gas sector can respond and quickly ramp up production, or just as easily slow it down.

2.2.3 ArcGIS

Using each house and well longitude and latitude, we are able to use ArcGIS to spatially plot the wells on a map of the United State and create a various sized buffers around each well. For each year, to identify what wells were established, we selected wells that were in use from that year and all the years before. This allowed us to isolate the correct number of new wells and their location for each year. Once layers were created for each year, we then imported the housing transactions. Again, to isolate the homes that were sold each year to create a layer that just contained each years housing transactions. Once each yearly layer was created we were able to match corresponding well and housing years together to separate out the homes that fell inside each wells $.5$ mile buffer. The same process was used to identify homes that fell between .5 miles and 1 mile , and again for homes that fell between 1 mile and 2 miles of a well. Figure 1 shows a map of all wells used in the sample.

2.3 Methodology

This project mainly aims to find the average effect of oil and natural gas well locations on the housing market, and to test and see if consumers are driven away from homes close to wells, if they are indifferent, or if the oil/gas sites increase local economic activity thus increasing housing demand.

To test the hypothesis we use three approaches. First, we will start with a panel data fixed effects model, then a repeat sales model, and finally a spatial Difference-in-Differences. First consider the following fixed effects model:

$$\log(Price_{ijt}) = \alpha_1 Treat_{it} + \alpha_1 Treat1_{it} + \alpha_2 Treat2_{it} + \alpha_3 Treat3_{it} + \gamma X_{ijt} + \delta_j + \rho_t + \epsilon_{ijt} \quad (2.1)$$

Household level observations are used in the equation, and the primary dependent variable $\log(Price_{ijt})$, is the logged house price of home i, in ZIP code j, at time t. Independent variable of interest are $Treat_{it}$, $Treat1_{it}$, $Treat2_{it}$, $Treat3_{it}$ where $Treat = 1$ if the home is located within 2 miles of a well, $Treat1 = 1$ if the home is located 0-.5 miles of a well, $Treat2 = 1$ if the home is located .5-1 mile of a well, and $Treat3 = 1$ if the home is located 1-2 miles of a well. We use various ring boundaries in order to examine the sensitivity of the threshold definition for the treatment group to the measured treatment effect. My boundaries also align with the thresholds used by Balthrop and Hawley [2017] and Muehlenbachs et al. [2015] who used 3500, 5000, or 6500 ft (in miles, .5, .94, and 1.23) and 1 km, 1.5 km, and 2 km. (in miles, .62, .93, and 1.24). Gopalakrishnan and Klaiber (2014) used boundaries between 0.75 mile and 2 miles, while Delgado et al. [2016] used between 1 and 4 miles. I stop my boundaries at 2 miles for 2 reasons. First, Gopalakrishnan and Klaiber found significant fading of the treatment effect by 2 miles; Delgado et al. [2016] estimate that the treatment effect reduces to zero around 4 miles. Secondly, our sample area covers much more dense housing markets where there are more properties and they have access to a greater number or alternative routs to get around remote drilling.

In addition, X_{ijt} is a vector containing traditional housing characteristic controls such as number of rooms, bedrooms, square feet, age of property, state GDP, state population, state land area, as well as county poverty counts and county median household income to control for state and county-level time-varying demographic factors. Finally δ_j is zip code level fixed effects and ρ_t is a month-year fixed effect. Table 2.2 shows a breakdown of how many total treatment homes and how many homes fell into each treatment buffer zone by state. We also cluster the standard errors by zip code to allow for arbitrary heteroskedasticity and serial correlation within the same zip code over time.

One possible concern with equation 1, when using a ZIP code level fixed effect, is the possible existence of un-observable characteristics of the properties that might be correlated with observed variables, including the three boundary zones. Hence, we estimate equation one again this time using a household level fixed effects. After that I restrict my sample to only repeat sales. Using a household level fixed effect allows us to control for some of the possible bias with ZIP code fixed effects. Additionally, there are two advantages of a repeat sales model. First we are able to run the model with both ZIP code and household level fixed effects. Second, we are able to difference out the unobservables effect assuming they are constant at both sale points. Balthrop and Hawley [2017] also used a repeat sales model, but when they restrict their observations to only repeat sales their sample is reduced by 64%. Since our panel is so long, our sample is only reduced by 26.6%, still with just over 9.5 million transactions. We follow a repeat sales model similar to Balthrop and Hawley [2017] with the estimating equation:

$$\log(Price_{ijt+\theta}/Price_{ijt}) = (\alpha_1 Treat_{it+\theta} - \alpha_1 Treat_{it}) + \gamma X_{ijt} + \lambda_i + \rho_t + \epsilon_{ijt} \tag{2.2}$$

Each property included in the repeat sales model has at least two transactions: the original transaction price at time t is given by $Price_{ijt}$, and the second price at time $t + \theta$ is denoted $Price_{ijt+\theta}$. Now, the percentage change in price can be explained by the change in proximity to a well. In this model I use λ_i first with ZIP code level fixed effects then again with household level fixed effects.[25] WE also cluster the standard errors in this model as we did in equation 1.

As stated earlier, a source of possible Endogeneity in equations 1 and 2 is the non-random location of a oil/gas well sites. Both home builders/buyers and oil and gas companies are looking for places to build/buy/drill that have easy access to roads, interstates, and access points. Because of this the selling price of a home might be affected by the location of well heads. To account for this possible endogeneity concern, we use a spatial Difference-in-differences (SDiD) approach such as the one used in Dronyk-Trosper [2017]. This study examined the local government's construction of public service facilities, such as fire departments and police stations, to see if they impacted the local housing market. In that application, control homes were homes which maintained their distance from the closest facility throughout the sample period. Treatment homes are ones which at period t_0 have the same distance as the control group but at some future period t_s

where $s > 0$, a new public service facility that reduces the distance to the nearest option. We follow the same intuitive logic, but focusing on the placement of new wells that led to quasi-experimental differences in proximity to wells. See Figure 2 for an illustration of these two groups. Homes listed as the control are shown in the red 1 mile zone with no change in their distance to the construction of a new oil/gas well. These are compared to the treatment homes that were in the original 1 mile zone, but move closer to the .5 mile zone when the new oil/gas well is constructed. This method provides additional clarity as to the probable pathway of housing capitalization effects. Significant results here would help strengthen the idea that other significant findings are not likely from a spurious effect relating to oil/gas well location choices. The SDiD model is represented by:

$$\log(Price_i) = \beta_1 Treatment_i + \beta_2 BaseTreat_i + \beta_3 (Treatment_i * BaseTreat_i) + \gamma X_i + \epsilon_i \quad (2.3)$$

with $Treatment_i$ is an indicator variable which reflects whether a home is in one of the treatment groups. $BaseTreat_i$ is a dummy interaction term for whether a home sale occurred before or after the construction of a new oil/gas well, and X_i is a vector of home characteristic controls. β_3 is our variable of interest, which represents the change in home values for treated units following the opening of a new well site. A significant value here would demonstrate that the construction of a new well site altered local housing values. Again, the standard errors are clustered at the ZIP code level. Lastly, to further round out the analysis we run equations one and three individually for each state to analyze results at a more micro/state level.

In order to run the models at the per state level, each state needed to have not only enough observations, but also the treatment to control ratio could not be skewed to far to the control side. This restriction did not allow some of the states, like Kansas, Kentucky, or Tennessee to be analyzed in the main models. States that were able to be analyzed included California, Colorado, Oklahoma, Pennsylvania, and West Virginia. Looking more into individual states is also important because state regulation may differ and affect measured amenities and/or dis-amenities from hydraulic fracturing. These estimated amenities and/or dis-amenities may not be the same from state to state.

Each state may have different policies with respect to hydraulic fracturing operations, although it is not clear which state has more overall stringent regulations. Pennsylvania, for example, has longer setback restrictions on wells from buildings and water sources, whereas California has no rule regulating set backs. Oklahoma has more restrictive venting and flaring regulations, whereas Colorado requires more stringent pre-drilling water testing requirements (Richardson et al. [2013]). Different states can also have different local zoning and noise regulation that may have an effect on the amenities and/or dis-amenities homeowners are subject to. State and local finances are also different across states where Oklahoma has a hybrid system that consists of a fixed amount and a percentage amount, but Colorado is just a percentage of extracted gas. Given all of these differences, we also conduct a state by state analysis to help give more insight into how regulatory differences that affect the oil and gas industry translate into housing market impacts.

2.4 Results

Table 2.5 shows the estimated results for Equations 1 and 3. Columns 1 and 2 show the results for the ZIP code level fixed effect model, when all states are included, and clustering at the ZIP code. Columns 3 and 4 show the SDiD results. Because some states had extremely low treatment to control ratios; we drop those states for the preliminary results, but don't include them in the refined sample and those results are presented in Table 2.6.[26] When we include the states that have a imbalance of treatment to control observations the fixed effects model has positive results but they are not significant. But when we turn to columns 3 and 4, the SDID we gain significant and positive results. When comparing homes that started off two miles from a well site, and then a new well is drilled decreasing the distance to between one-half to one mile there is an increase in selling price of 1.9%. Also, homes that are now within one-half mile from a well see an increase of 6%. Column 4 shows homes that were once between one-half to one mile of a well, and then become within one-half mile of a well, increased their selling price by 4.5%.

Table 2.6 shows the results when we take out states with low treatment counts and balance the overall sample. Now both the ZIP code level fixed effects model and the SDiD both show positive and significant results. Columns 1 and 2, the ZIP code fixed effects models, show an increase in the point estimates and gain significance. Column one now shows that homes within two miles of a well get an increase in selling price of 1.6%, or evaluated at the mean transaction price roughly a $5,393.73 increase. Homes that are within one-half mile of a well experience a 4.5% increase, homes that are one-half mile to one mile from a well see a 2.5% increase, and homes that are one to two miles from a well increase their selling price by $.9\%$. The point estimates decreasing as the distance from the well increase follows the assumption that both well sites and homes sites/owners are competing for the same ease of access to roads, highways, and interstates, and is what past research has also documented. The SDiD results using the restricted sample shows about the same as the full sample result. The point estimates are slightly lower but none of the signifiance is lost. Using the restricted sample, homes that started off two miles from a well and then a new well is drilled decreasing the distance to be between one-half to one mile, show a $.7\%$ increase. Similarly, homes that moved to be within one-half mile have a 2.9% increase. Finally, column 4 shows results for homes that were once one mile from a well, and transitioned to within one-half mile of a well, receiving a 2.8% increase in selling price.[27] These results align directly with what Fetzer et al. [2014], Weber [2012], Weber et al. [2014] and Muehlenbachs et al. [2012] all pointed to, and help support the hypothesis that the boom in hydraulic fracturing has increased the local economy through increases in wages, employment, and local tax revenue. This in-turn helps increase the demand for housing. Some would be concerned that hydraulic fracturing effects of home values differs during the life-cycle of the well. But Balthrop and Hawley [2017] looked at the impact of drilling on sales that occur within six months of well completion, and sales that were made shortly after well drilling. They found that well construction does not seem to be the primary driver of any amenity or dis-amenity on a homes selling price. The data also shows that both new well sites and homes are attracted to the same plots of land where they have ease of access to roads, highways, interstates which are both convenient for homeowners to be able to get around quickly but also to well operators to have ease of access to the well heads. This also hints at the possibility that potential home buyers are not ”scared” away by oil/gas well sites and they are indifferent because as fracking continues and grows it is more and more a part of our normal reality.

2.5 Household Fixed effect and Repeat Sales Results

Table 2.7 shows the results for Equation 1 using household level fixed effects and Equation 2, analyzing repeat sales with ZIP and household level fixed effects. Both of these approaches have the advantage of being better able to control for constant and unobserved neighborhood quality than the previous estimates. Additionally, they should produce the same, or nearly the same, point estimates because when we analyze all transactions, but use a household level fixed effect it should only look at homes that have been sold more than once. For these reasons are the preferred specifications.

Columns 1 and 2 show the results of using household level fixed effects. Looking at the entire sample, homes that are within two miles of a well still experience a positive and significant increase in their selling price by 3.9%. Balancing the sample and taking out low treatment states the results still hold, with the point estimate increasing to 4.9%, now with significance. Repeat sales estimates are identified as a home that goes from being greater than two miles from a well to being within two miles of a well. This is different than Equation 1 because it does not rely on spatial differences in well exposure, which requires comparing houses that can likely be in different neighborhoods. When our sample is restricted to just repeat sales and excluding the states that have a low treatment count, we are left with a sample of $9,594,780$ observations. The richness of our data allows for a more in depth examination than previous repeat sales models have been able to do[28]. We use two different levels of fixed effects, ZIP code level and Household level, along with clustering the standard errors at the ZIP code level. Columns 3 and 4 show the results for both specifications. The point estimates for both repeat sales models are largely the same as when we looked at the entire sample. This was expected because as mentioned above making use of household level fixed effects in equation one will automatically look at only repeat sales in the full sample. On top of that, restricting the sample to just repeat sales allowed for the use of two different layers of fixed effects. The results presented in Table 2.7 are reassuring in that they confirm the results from our outer estimations.

2.6 Individual States Analysis

The results presented in Table 2.5 are across 11 states and results in Table 2.6 and 2.7 are across 7 states. However, state and local policies regarding hydraulic fracturing vary from state to state. It is additionally not clear which states have the most stringent regulations. Some states might be tougher on certain aspects, but simultaneously more relaxed on others. Because of this, estimating Equations 1 and 2 for each state will provide a deeper view, to see if there are any heterogeneous effects from state to state. Because some states have low treatment counts, this was not possible for all states in the sample.[29] Tables 2.8 through 2.12 show the results for California, Colorado, Oklahoma, Pennsylvania, and West Virginia. Richardson et al. [2013] did a state by state analysis that analyzed numerous different regulations per state and created a ranking for individual regulations but, not a clear overall state wide regulation ranking. Some of the regulations included general well spacing, venting regulations, building setback requirements, flaring regulations, pre-drilling water well testing, severance taxes, and accident reporting requirements. For some of the regulations they are able to determine how stringently states regulate. This includes setback restrictions, casing/cementing depth requirements, and command-and-control regulations are quantitative reported. There analysis shows how widely the states vary in regulations that are beyond federal regulations. The results from the state by state analysis shows positive, and significant results except West Virginia, which showed negative and significant impacts. Starting with California, and equation 1, the results show positive and significant point estimates for all three buffer zones. the .5 mile zones has a 5.1% increase, the .5-1 mile zone has a 2.2% increase and lastly the 1-2 mile zone has a 3.8%. When we look at the spatial model there is no significance.

Where in Oklahoma and Colorado, which have a higher treatment to control ratio than California there are positive and significant point estimates for both the Zip code fixed effect model and the spatial difference in differences. Pennsylvania, a sate that much past literature has focused on, shows positive point estimates only in the SDID model. It is hard to draw a clear reason why some states show significant point estimates in just one or both of the models but I believe that is has a lot to do with the states regulation and with its past history with hydraulic fracturing. Pennsylvania encountered numerous ground water contamination reports in the early 2000's, which might contribute to a negative overall impression with fracking, but because in recent years they have not had as many, homeowners that are around wells or homes that do not rely on ground water might not see then as a dis-amenity anymore. This could explain why the SDiD shows significant point estimates but not with the overall fixed effect model. According to the US Energy Information Agency, Oklahoma accounted for 9% and 4.5% of the US natural gas and oil respectively. Colorado accounted for 5.6% and 4.2% of the US natural gas and oil respectively. These two states had a balance of both oil and natural gas, where states like California and Pennsylvania did not. In California they accounted for 3.3% of the total US oil, but only 0.8% towards natural gas. Pennsylvania was the countries second largest gas producing state accounting for 16%, but only 0.1% towards oil. Instead Pennsylvania has a larger coal production.

The same also goes for West Virginia but they rely more heavily on coal, accounting for 12.6% of total US production, and 5.5% and 0.4% towards gas and oil respectively. Looking at the total states regulations towards shale production West Virginia is the only state that regulates all 20 elements, where in Oklahoma they regulate 16. But on the other side states like Colorado and Oklahoma have more quantitatively regulated elements than states like California and West Virginia. In fact, West Virginia is the state with the most non-quantitatively regulated elements. Looking at specific regulations the situation becomes more varied. West Virginia have cement type regulations but Oklahoma and Colorado do not, and in California it is addressed in the drilling permit. All of the states require at least a permit to withdraw water over the threshold of 1,000 gal/day, but Pennsylvania and the Susquehanna RBC require permits for any water withdrawals for hydraulic fracturing and operate ecosystem models that provide the basis for rejecting applications for water withdrawals that would put stress on ecosystems. Where West Virginia requires a similar water management plan. For withdrawals of more than 210,000 gallons per month. To withdraw more than 210,000 they need to document the source of the water withdrawal and shows its impact will be minimal. Lastly, we see differences in states like Oklahoma and Colorado when it comes to how fracturing fluids can be stored. In Oklahoma they require sealed tanks for some fluids, but in Colorado pits are allowed and regulated for all fluids.

Because of these difference in how heavy the state relies on the oil and gas industry for employment and income and in regulatory standards, helps contributes to the reason we see mixed results when we look at individual states and the affects hydraulic fracturing has on the housing market. Out of the five, Colorado and Oklahoma seem to have the most in common. They both share a similar balance when it comes to the extraction of oil and natural gas, as well as, many, but not all state regulations. Both states show a positive and and significant point estimates for both the Zip code fixed effect model and the spatial difference in differences. On average though, four out of the five states examined showed positive and significant point estimates for either the Zip code fixed effect model, the spatial difference in differences, or both. West Virginia had negative and significant point estimates for both the Zip code fixed effect model and the spatial difference in differences, but these estimates might be driven by the states heavy reliance on coil, which from an industry perspective has been declining over the past decade and being replaced by cheaper natural gas.

2.7 Conclusion

This paper uses data from over 15 million house transactions across 11 different states to show that houses within two miles of a hydraulically fractured natural gas wells sell at higher prices compared to homes that are not. The increase in price is largest in states that have a more balanced treatment to control ratio, and for homes within one-half mile of a well pad. This is approximately

4.5%, or when the sample house price is $337,004.40$ and increase of $15,165.20$. We controlled for constant and unobserved neighborhood quality by controlling for both household level fixed effects and repeat sales. Doing this, homes within two miles of a well see an increase in selling price by 4.9%. These estimates extend previous studies because they not only show how the broader housing market is affected, but the estimates depict the benefit to property net of any lease payments, and reflect the overall costs and benefits to homeowners.

These findings, while potentially controversial, show that at least in the short run ignoring any negative externalities related to global pollution, that any local disamenities are offset by the gains in local economic opportunities. This does not mean potentially adverse long run impacts are not important, but it dose help us understand the better socially relevant costs and benefits associated with fracking. Local public finance will also be affected, through tax revenue generated from both an increase in economic activity, directly through well permits, and indirectly through higher property tax assessments

Lastly, this paper was able to look across five different states to be able to see how differences in state regulation on the oil and gas industries impacted the housing market. Four of the five showed positive results. But due to the complected nature and the wide variance in state regulations, it is difficult to pin point exactly what regulations hinder the oil and gas sector the most, and what impacts those have for the housing market. While we have done our best in trying to undercover how the fracking boom has impacted the housing market across states, these and other questions actively remain.

2.8 Tables

Table 2.1: State Housing Summary Statistics

	count	HP	HP_sqft	rooms	bedrooms	bathrooms	sqfeet	yearbuilt	wells
CA	8,503,009	434,726.30	251.03	2.97	3.12	2.21	1,747.96	1977.32	12,201
		(445,818.60)	(189.58)	(3.544)	(0.95)	(0.84)	(5,307.71)	(24)	
CO	1,771,029	290,086.40	171.45	3.70	2.96	2.41	1,882.01	1981.	47,374
		(289,518.60)	(398.42)	(3.17)	(0.98)	(1.07)	(1,113.15)	(26)	
KS	45,683	114,765.80	77.42	6.04	3.02	1.90	1,427.10	1960	86,790
		(149,634.10)	(100.15)	(1.53)	(0.85)	(0.91)	(628.88)	(28)	
KY	359,049	174,939.90	93.41	2.39	2.93	2.11	1,840.95	1980	12,297
		(20,0437.80)	(107.12)	(3.14)	(0.90)	(0.85)	(1,160.86)	31	
OK	555,914	133,791.80	71.94	5.71	3.07	1.86	1,809.81	1979	25,605
		(168,148.50)	(95.29)	(2.30)	(0.69)	(0.64)	(824.50)	26	
PA	2,183,286	196,639.60	105.46	6.36	3.13	1.79	1,804.77	1960	10,6073
		(200,441.00)	(88.88)	(1.96)	(.80)	(0.85)	(941.01)	(36)	
TN	874,251	170,397.20	74.99	6.33	3.08	2.10	2,256.33	1978	13,072
		(194,195.00)	(72.81)	(2.05)	(0.80)	(0.89)	(1,322.68)	(26)	
UT	39,461	168,552.10	118.36	8.75	3.69	2.02	1,421.36	1974	11,949
		(145,809.10)	(65.03)	(2.78)	(1.18)	(0.87)	(711.17)	(27)	
VA	1,967,558	295,113.90	151.22	5.02	3.33	2.55	2,024.50	1984	10,132
		(233,333.70)	(96.39)	(3.33)	(0.89)	(0.97)	(1,035.81)	(23)	
WV	127,345	142,562	77.53	6.28	3.02	1.94	1,829.23	1969	113,463
		(166,406.80)	(98.04)	(1.62)	(0.76)	(0.84)	(911.45)	(33)	
WY	3,529	171,798.60	117.86	4.98	3.09	1.99	1,478.70	1970	51,325
		(262,969.70)	(144.82)	(3.10)	(0.94)	(0.80)	(1,591.91)	(29)	
Total	16,430,114	337,004.40	190.11	4.05	3.12	2.20	1,833.14	1976	29,720
		(374,030.30)	(208.45)	(3.47)	(0.92)	(0.92)	(3,890.13)	(27)	(33,155)

Variable means are presented with standard deviations in parenthesis below. Observations are not aggregated so house price is ist level where i is household, s is the state and t is the exact date of house contracts. Years 2000-2018 are included in the sample. Column 8 lists the number of oil and gas wells that are located in the state according to the data provided by the U.S. Department of Homeland Security.

Table 2.2: State Treatment Summary Statistics

	Treat	Treat1	Treat2	Treat3	Non-Treat
CA	64,100	1,678	11,585	50,837	8,438,766
CO	80,430	14,144	240,10	42,276	1,690,358
KS	1,932	44	187	1,701	45,651
KY	676	49	138	489	356,959
OK	12,057	1,218	2,194	8,645	541,462
PA	60,674	5,474	19,067	36,133	2,122,528
TN	367	0	21	346	870,738
UT	11	0	0	11	39,802
VA	921	84	244	593	1,966,052
WV	15,226	1,635	3,500	10,091	111,244
WY	262	251	1	10	3974
Total	236,656	24,577	60,947	151,132	16,187,534

Treat 1 are any home in the sample that is within 1/2 of a mile of a well. Treat 2 and 3 are the same but Treat 2 are any homes that are between .5 to 1 mile of a well Treat 3 are homes that are 1-2 miles from a well. Non-Treat are home that are farther than 2 miles from a well.

Table 2.3: State Base Treatment Summary Statistics

	hp	hp_sqft	rooms	bedrooms	bathrooms	sqfeet	yearbuilt
Homes in Control ZIP's							
mean	339,859.60	189.95	4.65	3.14	2.19	1,856.55	1977
(sd)	(360,985.20)	(167.61)	(3.43)	(0.93)	(0.90)	(5,168.10)	(28.45)
Homes in Treatment ZIP's							
mean	333,727.20	190.39	3.31	3.09	2.21	1,804.63	1975
(sd)	(389,580.90)	(249.65)	(3.37)	(0.90)	(0.93)	(957.32)	(26.32)
Total Sample							
mean	337,108.40	190.15	4.05	3.12	2.20	1,833.25	1976
(sd)	(374,097)	(208.45)	(3.47)	(0.92)	(0.92)	(3,890.81)	(27.53)
Observations	16,424,190						

Table 2.3 looks at the summary statistics between houses in ZIP codes that have wells present in them to houses in ZIP codes that do not. Ideally, the means of these two samples should be similar, and as Table 2.4 shows they in-fact are very similar. If the treatment and control samples would have been different in the observed variables, then the samples might also vary in correlated unobserved variables, which would bias the estimates. The table shows that both have means that are close.

Table 2.4: Well and Crude Oil Price Analysis

Year	Number of Wells Drilled	Average Crude Oil Closing Price
2000	11,870	$30.38
2001	13,561	$25.98
2002	10,771	$26.19
2003	13,410	$31.08
2004	16,207	$41.51
2005	19,205	$56.64
2006	22,627	$66.05
2007	22,836	$72.34
2008	22,848	$99.67
2009	15,140	$61.95
2010	15,563	$79.48
2011	16,558	$94.88
2012	16,272	$94.05
2013	14,478	$97.98
2014	33,285	$93.17
2015	6,228	$48.72
2016	4,165	$43.58
2017	10,114	$50.84
2018	10,890	$64.90

These are the number of new wells drilled per year during the sample period. The huge drop off after 2014 was when the oil and gas market bottomed out with a decrease in demand and OPEC deciding not to cut production.

Table 2.5: Ln HP Results

VARIABLES	LnHP	LnHP	LnHP	LnHP
Within 2 Miles	0.006			
	(0.009)			
.5 Mile Zone		0.029	0.060***	0.045***
		(0.018)	(0.016)	(0.014)
.5-1 Miles Zone		0.012	0.019*	
		(0.010)	(0.010)	
1-2 Mile Zone		0.000		
		(0.009)		
Rooms	0.005***	0.005***	0.015***	0.015***
	(0.001)	(0.001)	(0.003)	(0.004)
Bedrooms	0.131***	0.131***	0.177***	0.154***
	(0.006)	(0.006)	(0.022)	(0.030)
Ln SqFeet	0.716***	0.716***	0.622***	0.554***
	(0.008)	(0.008)	(0.036)	(0.052)
Year Built	0.002***	0.002***	0.004***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
Ln State GDP	1.883***	1.883***	1.851***	1.766***
	(0.079)	(0.079)	(0.156)	(0.214)
Ln State Population	0.360***	0.356***	-0.240	-0.097
	(0.115)	(0.115)	(0.271)	(0.280)
Ln State Area	-8.522***	-8.512***		
	(0.366)	(0.367)		
Ln County Poverty Count	0.017***	0.017***	-0.001	-0.012**
	(0.002)	(0.002)	(0.005)	(0.006)
County Median House Income	0.000***	0.000***	0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Sample	Full	Full	Base Treat 3	Base Treat 2
Observations	15,875,600	15,875,600	986,540	478,250
R-squared	0.734	0.734	0.704	0.663
Zip FE	YES	YES	YES	YES
Year-Month FE	YES	YES	YES	YES
Clustered S.E.	YES	YES	YES	YES

Results are in the Logged value of the sales price and the standard errors are clustered at the ZIP code level. Columns 3 and 4 are the spatial DID results. Column 3 the sample is restricted to just Base Treat 3 homes and it looks at the affect when a home is initially in the 2 mile zone and then moves to the .5-1 or 0-.5 mile zones because a new well was drilled and the distance to the nearest well decreased. Column 4 is the same but looking at home that were once in the .5-1 mile zone and now are in the 0-.5 mile zone.

Table 2.6: Ln HP Results dropping low Treatment States

VARIABLES	LnHP	LnHP	LnHP	LnHP
Within 2 Miles	0.016*			
	(0.009)			
.5 Mile Zone		0.045**	0.029*	0.028**
		(0.018)	(0.015)	(0.014)
.5-1 Mile Zone		0.025**	0.007	
		(0.011)	(0.009)	
1-2 Mile Zone		0.009		
		(0.009)		
Rooms	0.008***	0.008***	0.016***	0.017***
	(0.001)	(0.001)	(0.003)	(0.003)
Bedrooms	0.132***	0.132***	0.097***	0.091***
	(0.007)	(0.007)	(0.012)	(0.013)
Ln SqFeet	0.725***	0.725***	0.669***	0.662***
	(0.009)	(0.009)	(0.016)	(0.017)
Year Built	0.002***	0.002***	0.003***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)
Ln State GDP	2.387***	2.388***	2.156***	2.050***
	(0.074)	(0.074)	(0.130)	(0.147)
Ln State Population	-0.481***	-0.487***	-0.295**	-0.218*
	(0.112)	(0.112)	(0.115)	(0.118)
Ln State Area	-8.013***	-8.000***	-7.492***	-7.303***
	(0.437)	(0.437)	(0.416)	(0.434)
Ln County Poverty Count	0.024***	0.024***	-0.006**	-0.007***
	(0.003)	(0.003)	(0.003)	(0.003)
County Median House Income	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Sample	Restricted	Restricted	Base Treat 3	Base Treat 2
Observations	13,082,632	13,082,632	5,000,861	4,712,508
R-squared	0.743	0.743	0.663	0.651
Zip FE	YES	YES	YES	YES
Year-Month FE	YES	YES	YES	YES
Clustered S.E.	YES	YES	YES	YES

Results are in the Logged value of the sales price and the standard errors are clustered at the ZIP code level. Homes with low treatment counts have been restricted from the sample. These include KY, TN, UT, VA, and WY. Column 1 and 2 are the fixed effects model results. Columns 3 and 4 are the spatial DID results. Column 3 the sample is restricted to just Base Treat 3 homes and it looks at the affect when a home is initially in the 2 mile zone and then moves to the .5-1 or 0-.5 mile zones because a new well was drilled and the distance to the nearest well decreased. Column 4 is the same but looking at home that were once in the .5-1 mile zone and now are in the 0-.5 mile zone.

Table 2.7: Household ID and Repeat Sales Model Results

VARIABLES	LnHP	LnHP	LnHP	LnHP
Within 2 Miles	0.039*	0.049**	0.016*	0.049**
	(0.020)	(0.022)	(0.010)	(0.022)
Rooms	0.006	0.006	0.008***	0.006
	(0.005)	(0.005)	(0.001)	(0.005)
Bedrooms	0.131	0.135	0.142***	0.135
	(0.142)	(0.145)	(0.007)	(0.145)
Ln SqFeet	0.084	0.082	0.718***	0.082
	(0.151)	(0.155)	(0.010)	(0.155)
Year Built	0.000	0.000	0.000	0.000
	(0.002)	(0.003)	(0.000)	(0.003)
Ln State GDP	3.213***	3.278***	2.486***	3.278***
	(0.069)	(0.084)	(0.089)	(0.084)
Ln State Population	-2.852***	-2.866***	-0.513***	-2.866***
	(0.160)	(0.201)	(0.128)	(0.201)
Ln County Poverty Count	-0.163***	-0.196***	0.032***	-0.196***
	(0.007)	(0.009)	(0.003)	(0.009)
Ln County Median House Income	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Sample	Full	Restricted	Repeat Sales	Repeat Sales
Observations	15,875,600	13,082,632	9,594,780	9,594,780
R-squared	0.893	0.894	0.725	0.845
Household FE	YES	YES	-	YES
Zip Code FE	-	-	YES	-
Clustered S.E.	YES	YES	YES	YES

Results are in the Logged value of the sales price and the standard errors are clustered at the ZIP code level. These results are when we use the household level fixed effects to capture how repeat sales of homes are affected when a well comes within 2 miles or closer. Column 1 is the full sample, where in column 2 I drop low treatment to control states. These states include KY, TN, UT, VA, and WY. The results here help support the hypothesis that housing demand is stimulated positively when hydraulically fractures wells are present and the boost to the local economy helps boost the demand for housing.

Table 2.8: California Results

VARIABLES	LnHP	LnHP	LnHP
Within 2 Miles	0.035***		
	(0.002)		
.5 Mile Zone	0.051***	0.013	0.010
	(0.010)	(0.010)	(0.011)
.5-1 Mile Zone	0.022***	-0.017***	
	(0.004)	(0.004)	
1-2 Mile Zone	0.038***		
	(0.002)		
Rooms	0.002***	0.003***	0.022***
	(0.000)	(0.001)	(0.003)
Bedrooms	0.156***	0.218***	0.246***
	(0.002)	(0.006)	(0.008)
Bathrooms	0.005***	0.010***	0.006***
	(0.000)	(0.001)	(0.001)
Ln Sqfeet	0.784***	0.855***	0.865***
	(0.001)	(0.003)	(0.007)
Year Built	-0.001***	0.000***	0.001***
	(0.000)	(0.000)	(0.000)
Ln County Poverty Count	-0.002	0.002	0.029
	(0.002)	(0.008)	(0.019)
County Median House Income	0.000*	0.000**	0.000
	(0.000)	(0.000)	(0.000)
Observations	8,502,866	421,095	132,742
Sample	CA	CA Base Treat 3	CA Base Treat 2
R-squared	0.750	0.765	0.758
Zip FE	YES	YES	YES
Year-Month FE	YES	YES	YES
Robust S.E.	YES	YES	YES

Results are in the Logged value of the sales price. Column 1 is the fixed effects model results. Columns 2 and 3 are the spatial DID results. Column 2 the sample is restricted to just Base Treat 3 homes and it looks at the affect when a home is initially in the 2 mile zone and then moves to the .5-1 or 0-.5 mile zones because a new well was drilled and the distance to the nearest well decreased. Column 3 is the same but looking at home that were once in the .5-1 mile zone and now are in the 0-.5 mile zone.

Table 2.9: Colorado Results

VARIABLES	LnHP	LnHP	LnHP
Within 2 Miles	0.013***		
	(0.002)		
.5 Mile Zone	0.066***	0.035***	0.011**
	(0.004)	(0.005)	(0.005)
.5-1 Mile Zone	0.040***	0.026***	
	(0.003)	(0.004)	
1-2 Miles Zone	0.018***		
	(0.003)		
Rooms	0.016***	-0.030***	-0.024***
	(0.000)	(0.002)	(0.003)
Bedrooms	0.091***	0.118***	0.092***
	(0.003)	(0.007)	(0.006)
Bathrooms	0.092***	0.072***	0.061***
	(0.002)	(0.005)	(0.007)
Ln Sqfeet	0.534***	0.278***	0.275***
	(0.002)	(0.005)	(0.005)
Year Built	0.000***	0.001***	0.002***
	(0.000)	(0.000)	(0.000)
Ln County Poverty Count	-0.005**	-0.011	-0.008
	(0.002)	(0.009)	(0.010)
County Median House Income	0.000*	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Observations	1,668,992	122,901	94,305
Sample	CO	CO Base Treat 3	CO Base Treat 2
R-squared	0.621	0.456	0.427
Zip FE	YES	YES	YES
Year-Month FE	YES	YES	YES
Robust S.E.	YES	YES	YES

Results are in the Logged value of the sales price. Column 1 is the fixed effects model results. Columns 2 and 3 are the spatial DID results. Column 2 the sample is restricted to just Base Treat 3 homes and it looks at the affect when a home is initially in the 2 mile zone and then moves to the .5-1 or 0-.5 mile zones because a new well was drilled and the distance to the nearest well decreased. Column 3 is the same but looking at home that were once in the .5-1 mile zone and now are in the 0-.5 mile zone.

Table 2.10: Oklahoma Results

VARIABLES	LnHP	LnHP	LnHP
Within 2 Miles	0.031***		
	(0.006)		
.5 Mile Zone	0.060***	0.040**	0.024
	(0.016)	(0.017)	(0.019)
.5-1 Mile Zone	0.070***	0.057***	
	(0.012)	(0.012)	
1-2 Mile Zone	0.018**		
	(0.007)		
Rooms	0.020***	0.021***	0.017***
	(0.001)	(0.002)	(0.003)
Bedrooms	0.160***	0.141***	0.132***
	(0.014)	(0.021)	(0.027)
Ln Sqfeet	0.826***	0.719***	0.698***
	(0.004)	(0.008)	(0.012)
Year Built	0.003***	0.004***	0.005***
	(0.000)	(0.000)	(0.000)
Ln County Poverty Count	-0.005	0.021***	0.025**
	(0.004)	(0.008)	(0.012)
County Median House Income	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Observations	553,519	133,522	52,323
Sample	OK	OK Base Treat 3	OK Base Treat 2
R-squared	0.401	0.482	0.508
Zip FE	YES	YES	YES
Year-Month FE	YES	YES	YES
Robust S.E.	YES	YES	YES

Results are in the Logged value of the sales price. Column 1 is the fixed effects model results. Columns 2 and 3 are the spatial DID results. Column 2 the sample is restricted to just Base Treat 3 homes and it looks at the affect when a home is initially in the 2 mile zone and then moves to the .5-1 or 0-.5 mile zones because a new well was drilled and the distance to the nearest well decreased. Column 3 is the same but looking at home that were once in the .5-1 mile zone and now are in the 0-.5 mile zone.

Table 2.11: Pennsylvania Results

VARIABLES	LnHP	LnHP	LnHP
Within 2 Miles	0.015***		
	(0.003)		
.5 Mile Zone	0.011	0.028***	0.019**
	(0.008)	(0.009)	(0.009)
.5-1 Mile Zone	0.043***	0.016***	
	(0.005)	(0.005)	
1-2 Mile Zone	0.003		
	(0.003)		
Rooms	-0.002*	0.101***	0.114***
	(0.001)	(0.013)	(0.018)
Bedrooms	0.071***	0.148***	0.095***
	(0.003)	(0.028)	(0.030)
Bathrooms	0.100***	0.268***	0.240***
	(0.002)	(0.016)	(0.019)
Ln Sqfeet	0.613***	0.444***	0.440***
	(0.002)	(0.007)	(0.008)
Year Built	0.004***	0.006***	0.007***
	(0.000)	(0.000)	(0.000)
Ln County Poverty Count	-0.006*	-0.011	-0.006
	(0.004)	(0.013)	(0.016)
County Median House Income	0.000**	0.000	0.000
	(0.000)	(0.000)	(0.000)
Observations	2,183,202	171,198	109,586
Sample	PA	PA Base Treat 3	PA Base Treat 2
R-squared	0.645	0.453	0.427
Zip FE	YES	YES	YES
Year-Month FE	YES	YES	YES
Robust S.E.	YES	YES	YES

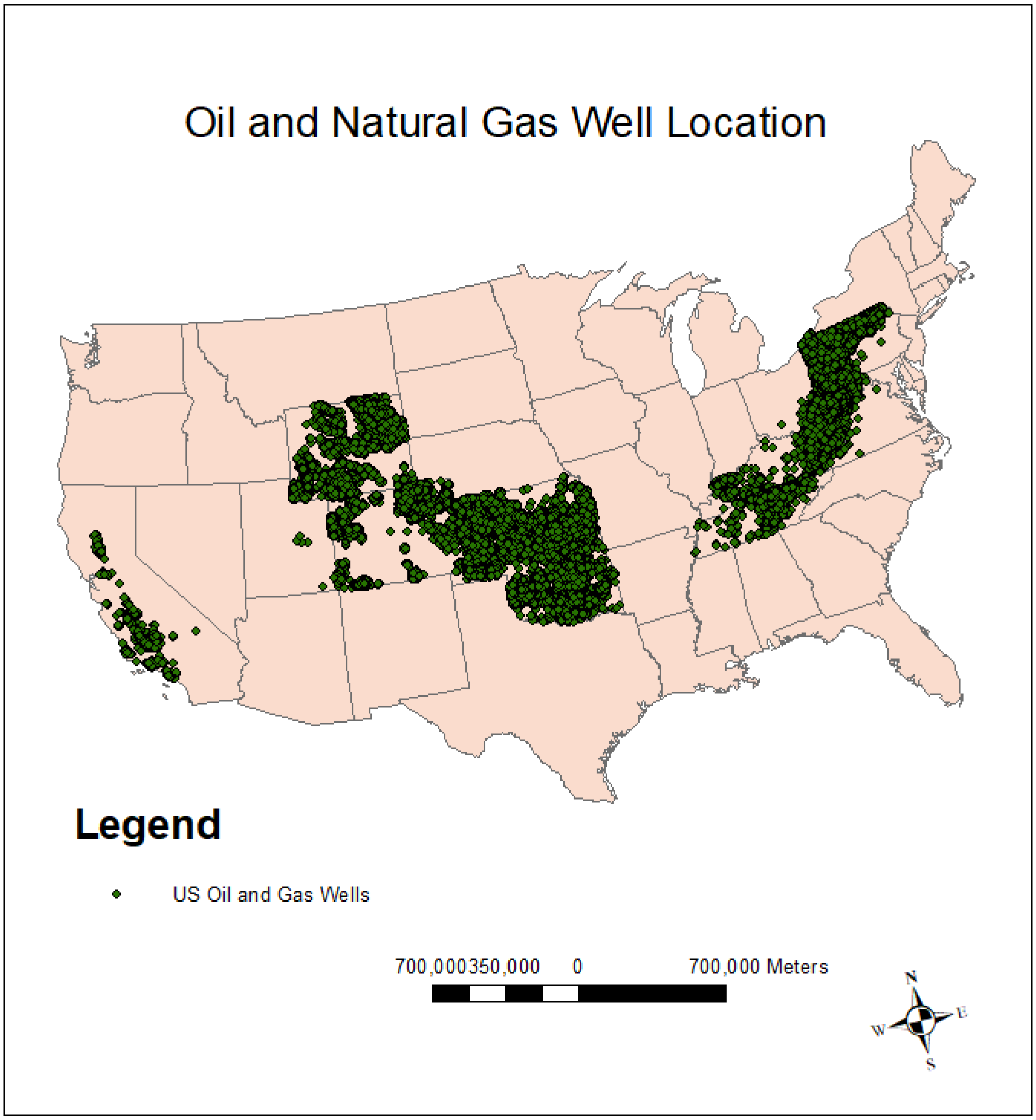
Results are in the Logged value of the sales price. Column 1 is the fixed effects model results. Columns 2 and 3 are the spatial DID results. Column 2 the sample is restricted to just Base Treat 3 homes and it looks at the affect when a home is initially in the 2 mile zone and then moves to the .5-1 or 0-.5 mile zones because a new well was drilled and the distance to the nearest well decreased. Column 3 is the same but looking at home that were once in the .5-1 mile zone and now are in the 0-.5 mile zone.

Table 2.12: West Virginia Results

VARIABLES	LnHP	LnHP	LnHP
Within 2 Miles	-0.016**		
	(0.007)		
.5 Mile Zone	-0.049***	-0.041**	-0.028
	(0.018)	(0.018)	(0.018)
.5-1 Mile Zone	-0.033***	-0.026**	
	(0.012)	(0.012)	
1-2 Mile Zone	-0.006		
	(0.007)		
Rooms	0.012**	0.016***	0.018***
	(0.005)	(0.005)	(0.005)
Bedrooms	0.086***	0.096***	0.115***
	(0.013)	(0.015)	(0.017)
Bathrooms	0.290***	0.314***	0.308***
	(0.016)	(0.014)	(0.016)
Ln Sqfeet	0.558***	0.560***	0.565***
	(0.007)	(0.009)	(0.010)
Year Built	0.003***	0.004***	0.004***
	(0.000)	(0.000)	(0.000)
Ln County Poverty Count	-0.001	0.001	-0.001
	(0.006)	(0.007)	(0.008)
County Median House Income	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Observations	126,470	89,475	67,611
Sample	WV	WV Base Treat 3	WV Base Treat 2
R-squared	0.485	0.472	0.467
Zip FE	YES	YES	YES
Year-Month FE	YES	YES	YES
Robust S.E.	YES	YES	YES

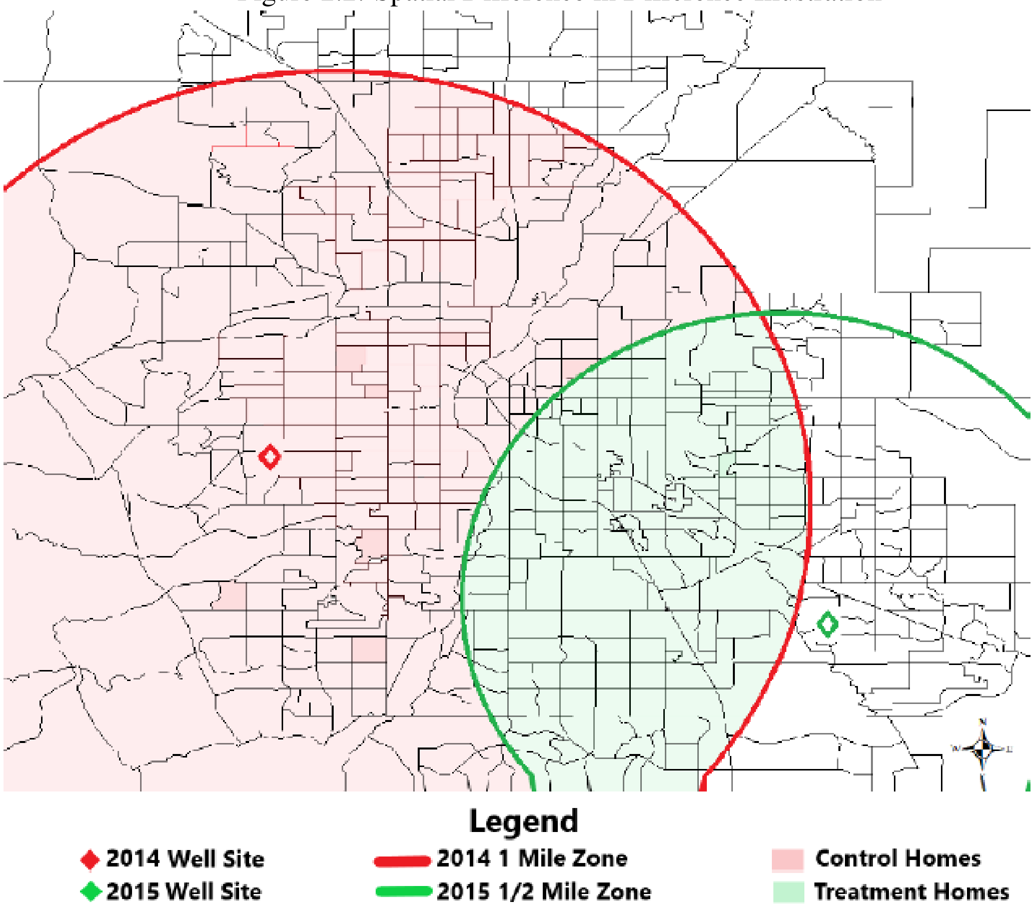
Results are in the Logged value of the sales price. Column 1 is the fixed effects model results. Columns 2 and 3 are the spatial DID results. Column 2 the sample is restricted to just Base Treat 3 homes and it looks at the affect when a home is initially in the 2 mile zone and then moves to the .5-1 or 0-.5 mile zones because a new well was drilled and the distance to the nearest well decreased. Column 3 is the same but looking at home that were once in the .5-1 mile zone and now are in the 0-.5 mile zone.

Figure 2.1: US well map



The map was created using ArcGIS and shows the location of all the wells that were used in the project.

Figure 2.2: Spatial Difference in Difference Illustration



The red diamond marks a well that was constructed in 2014 and the red ring is the 1 mile buffer zone that captures all homes. the green diamond is the location of a well that was built the following year and has a .5 mile buffer zone. The intersection of the 2, the green shaded area, are the new treatment homes, and the red shaded area are the control homes.

Chapter 3

More Bang for your Buck: Flat Rate Tuition and its effect on Educational Investment and School Performance

Economists and other education policy makers have long been interested in understanding the demand for higher education. Recently, barely more than half of college students are graduating within six years (Shapiro et al. [2013]). Additionally, Bound et al. [2012] found the time-to-degree has increased, particularly for students from low-income families. As tuition prices are rising, student's are taking longer to graduate which means the overall (or cumulative) cost of attending college is increasing. Low income families will be the ones that are hit hardest, thus possibly keeping them from attending.

Given these startling facts, many federal, state, and local policy institutions are looking at ways to help reverse the above mentioned static's to find ways to help make college more affordable. Examples include those focused on quantifying price elasticities for various student populations(Crouse [2015]) and estimating student sensitivity to changes in financial aid packages(Bryan and Whipple [1995]).

Much of the early work on the demand for higher education was reviewed by Jackson and

Weathersby [1975]. Using the parameters estimated in a number of studies, they concluded that the net behavioral response to changes in tuition is modest suggesting: a decrease of between 0.05% and 1.46% in enrollment ratio per each $\$100$ increase (in 1974 dollars) in student cost. Additionally, they found a decrease in absolute magnitude of price responsiveness to decrease with increasing income. In a meta-analysis of studies completed between 1967 and 1982, Leslie and Brinkman [1987] concluded a $\$100$ tuition price (in 1982 dollars) increase to be associated with a 0.6 to 0.8 percentage point decline in college enrollments.

Heller [1997] provided an update to Leslie and Brinkman (1987). He concluded that a $\$100$ increase results in a 0.5% to 1.0% decline in enrollments. But he pointed out that the empirical work he examined used data from the 1970s and 1980s, so the effect might not generalize to the higher tuition levels at the time of his analysis.

More recent research has analyzed net college price and students enrollment, persistence, and college choices show that a $1,000$ change in college price (1990 dollars) is associated with a 3 to 5 percentage point difference in enrollment rates (Dynarski [2013]). Evidence on the effect of college price on persistence and degree completion is not as well investigated, but most studies suggest that persistence and completion are modestly responsive to prices for at least some groups (Bettinger [2004]; Castleman et al. [2013]). Price also appears to be a strong predictor of the specific college students choose to attend (Hemelt and Marcotte [2016]; Long [2004]), institution level enrollment (Hemelt and Marcotte [2011]), and choice of major (Stange [2015]). While some works have looked at price response in educational investment, there is little that has been done that ew studies compare the impact of FRP versus PCH schemes on student educational and labor market outcomes.

A number of studies look at outside interventions, such as grants or scholarships tied to performance, that can alter a individual students choice in course load and performance. These include, the Promise Scholarship in West Virginia which explicitly tied aid to the number of credits (and GPA), and resulted in more students taking 15 credits rather than the full-time minimum of 12 (Scott-Clayton [2011]). A similar result was found for a scholarship program at the University of

New Mexico (Miller [2011]). Yet, work on Georgias HOPE scholarship, which tied eligibility and retention of funds to maintaining a 3.0 GPA, found that HOPE reduced the likelihood students took full course loads and increased their propensity to withdraw from classes and to divert credits to the summer (Cornwell et al. [2005]). Barrow et al. [2014] found that a performance-based scholarship at community colleges in New Orleans increased credit loads, as did an intervention that combined financial incentives and academic support services at a Canadian university (Angrist et al. [2009]). At a large Italian university, Garibaldi et al. [2012] found that charging students extra for taking too long to graduate speeds up time-to-degree.

Helmet and Strange (2016) is the only study (to my knowledge) that investigates flat rate tuition compared to per credit hour tuition. They examine Michigan public high school graduates in the classes of 2008 through 2011, and who attended one of the states public universities. They looked at students who attended one of the state schools that charged per credit taken compared to those on flat rate tuition. They found that exposure to zero marginal pricing tuition influences 7 percent of students to attempt up to one additional course, or three credits hours. Along with that they concluded that there was little evidence that these additional attempted credits resulted in more earned credits in a semester. Secondly, they found that students facing flat rate tuition are more likely to withdraw from at least one course and the likelihood of meeting the on-time benchmarks for graduation was unchanged. A possible bias they pointed out with the study was the selection bias of students picking what schools to go to, and that each schools pricing scheme would have been know upfront by the incoming students. Our research extends Helmet and Strange (2016) by looking not only at the academic impacts of FRT, but also how its effects the class registration and credits attempted behavior of the student body. We evade the selection bias by looking at different cohorts at the University of Oklahoma.We also look at behavioral changes of the mixed cohorts of students who experienced FRT for 1, 2, or 3 years. With this data we can see how as the number of years on FRT a student experiences impacts their course registration and academic performance change.

3.1 Background on Flat Rate Tuition

The University of Oklahoma switched from per credit hour tuition pricing to a flat rate tuition (FRT) in the fall of 2013. According to the university, FRT was implemented to reduce the cost of completing an OU degree. The rate is based on OU's current 15 credit hour rate of tuition. Students registered in fewer than 12 hours will continue to pay on a per credit hour basis. OU also claimed that the FRT would allow students to graduate and enter the labor market sooner. They claim that FRT would also save a student one or two years worth of room/board, transportation, and other college-related expenses. The university may also be motivated the switch to FRT. In particular, college rankings consider 4 year and 5 year graduation rates. Thus, if FRT could help to improve the 4 and 5-year graduate rate it will also inprove the overall college ranking.

According to the university, part-time students, or students enrolled in 1-11 hours, are charged a per credit hour basis. All undergraduate student registered in 12 or more hours will be charged a flat rate based on the current 15 credit hour rate for tuition. College program and technology, additional academic excellence, and course fees will continue to be charged on a per credit hour basis. The FRT does not apply to Graduate, Law, Advanced Program student, or students enrolled in only Liberal Studies courses. These students will be charged on a per credit hour basis for tuition and fees. Summer courses are still continued to be charged on a per credit hour basis if a undergraduate student registered for 15 hours in the fall and spring and did not "bank" any hours.

One of the surprising additions to FRT cohorts is they register for 12 and not 15 or more, they can bank their 3 unused hours and if they want they can apply them for summer classes. In this case a student that registers for 12 hours in the fall and spring could take tuition free 2 classes in the summer. They would still have to pay the associated fees for the summer classes. The university did set some restrictions with the banked hours. They cannot be converted into cash or held for a future academic year. Also they do not go towards fees such as Academic Excellence Fees, College Program and Technology Fees, Mandatory Semester Fees, and Course Specific Fees.

Some students might have situations that restrict them from enrolling in 15 credit hours per semester. Because of this the university created an appeals process. Examples of students that might be eligible for an appeal are students with disabilities. Students' that have fewer than 15 hours remaining per semester until graduation. Or students' participating in a study abroad program, have a temporary medical condition, or are participating in an internship.

3.2 Data

The data set comes from the University of Oklahoma, and a key characteristic is it consists of student level data across 10 school years. The student level data is de-identified and each student is given a unique number that we use to match them across all for reporting files. Our data ranges from fall 2008 thorough May 2018. Flat Rate tuition was implemented in the fall of 2013. This gives us five different types of student groups we can compare and analyze. First, one that were never on FRT. The next group is our Mixed group, student that were on FRT for 1, 2, or 3 years and pay by the hour on the remaining. Then our Always FRT students, ones that enrolled fall of 2013 or later. The main data panel is at the semester level, but stationary in time. It shows the end of semester each students semester GAP, freshman start semester, hours they completed that semester, major, full/part time, residency information, demographics, financial aid/Pell grant, citizenship, and graduation date. What this data set does not show us is the number of hours they registered for the following semester and then kept/dropped after the ORD date (the date they can drop any class and get a full refund for all fees and hours). To get this data, the university is providing us with three transaction style data sets that shows as the semester went on, if the student made any changes.

This first transactional data panel contains the number of hours a student registered for each semester, and then the number of hours they had after the ORD date. An interesting question we are going to be able to answer is if with the new flat rate tuition if the students are "hording" class during registration periods. We want to see is if under FRT students are optimize by registering for one more class than they want to actually complete, and "test driving" all the classes during the first two weeks of the following semester. This optimizing behavior would allow the students to pick the classes they believe they will do the best in that specific semester. Table 3.1 shows cohort mean graduation GPA. The always FRT cohort has a mean graduation GDP of 3.23% compared to 3.19% for the pay per credit hour cohort. This would be beneficial for older classmates as OU allows seniors to register first, followed by junior, sophomores, and finally freshman. On the inverse of this positive effect, younger classmates are crowded out. As older classmates register for more classes than they want then there is one less seat for a younger classmate.

The second transactional data panel covers tuition costs. This data panel will be a complete breakdown of all applied fees, tuition costs, and how it changed as the semester went on and if the student added or dropped a class how their final charged amount changes. Lastly, the third transactional data set will cover all financial aid data. This includes if they had any federal subsidized or unsubsidized loans, scholarship's, grants (such as the PELL grant), and awards. This will allow us to get a clearer picture of how the financial burden has changed since the new tuition policy was started. In Table 3.1 we present the summary statistics for each group. Table 3.2 we present just the freshman mean actual hours attempted with other key statics. A key finding is that at the mean, all three groups register for just about the same number of classes. In fact all of the statics are almost identical. Table 3.3 shows us the "Hording" by each of the groups, breaking out the mixed group to how many years they were on FRT. Each group horded on average one class, but the percentage of students that horded in the Always FRT group is 7.54% compared to only 3.64% in the per credit hour. This shows as the number of years on FRT goes down, the percent of students that horde classes decreases. We believe here that when the marginal cost after 15 credit hours was zero, more of the student population are willing to sign up for more class and then in the following semester drop one after they were able to "test" out the classes and see which ones they believed they could do the best in that semester.

Table 3.1: Freshman Cohort Summary Statistics

Cohort	Always Flat Rate	Mixed Tuition	Per Credit Hour
Cohort Freshman Observations	25,298	11,987	7,610
Cohort Mean Graduation GPA	3.23	3.16	3.19
	(Percentage)	(Percentage)	(Percentage))
In State Students	58.70	61.12	64.39
Out of State Students	38.72	35.79	33.71
International Students	2.57	3.08	1.91
Not First Generation	76.01	83.82	99.92
First Generation Student	23.99	16.18	0.08
Citizen	96.62	96.74	98.24
Not a Citizen	3.38	3.26	1.76
Male	47.40	47.22	47.11
Female	52.60	52.78	52.89
Full Time	90.30	98.91	98.90
Part Time	9.70	1.09	1.10
White	78.29	75.15	72.56
African American	6.61	6.24	5.85
Asian	9.51	7.07	6.28
American Indian	10.35	9.11	7.36
Hispanic	9.91	9.29	3.61
Pacific Islander	0.49	0.68	0.34
Pell Grant	21.29	24.70	N/A
Sub Stafford Loan	27.80	29.74	N/A

Other than Cohort Observations, all of the statistics are percentages. Column 1 is the cohort of students that were always under flat rate tuition. Column 2 are students that were under a mix of flat rate and pay per credit hour. Column 3 is the cohort of students that were always under pay per credit hour. For this cohort the University of Oklahoma did not provide data for First Generation status, Pell Grant or Sub-Stafford Loan. Out of the entire sample there were 1,292 that did not wish to report their race and 30 non citizens that did not report race.

Table 3.2: Freshman Credit Hours By Cohort

	Mean	Standard Deviation	Median	Minimum	Maximum
Always Flat Rate	14.01	1.96	14	3	18
Mix Tuition	14.14	1.51	14	3	18
Per Credit Hour	14.26	1.35	14	3	18
Entire Sample	14.09	1.75	14	3	18
Observations	44,553				

Row 1 shows summary statistics for incoming freshman credit hours at enrollment for students that were always under flat rate tuition. This consisted of students that enrolled on and after Fall 2013. Row 2 shows summary statistics for incoming freshman credit hours at enrollment for students that were under both pay per credit hour and flat rate tuition. This consisted of students that enrolled during Fall 2010 through Fall 2012. Students that enrolled in Fall 2010 would have been under pay per credit hour for 3 years and flat rate for 1 year. Those that enrolled in Fall 2011 would have been under each payment scheme for 2 years and those that enrolled Fall 2012 would have been pay per credit hour for 1 year and flat rate for 3 years. Row 3 shows summary statistics for incoming freshman credit hours at enrollment for students that were always under pay per credit hour. This consisted of students that enrolled Fall 2008 and Fall 2009.

Table 3.3: Enrollment and Drop Day Credit Hours

Cohort	Always Flat Rate	3 Years Flat Rate	2 Years Flat Rate	1 Year Flat Rate	Per Credit Hour
Cohort Observations	127,429	29,562	29,562	26,446	46,696
Number of Students that Horded	9,609	1,712	1,512	1,273	1,701
Percentage Horded	7.54%	5.8%	5.11%	4.8%	3.64%
Mean Credit Hours Horded	2.93	2.94	2.87	2.97	2.71
Max Credit Hours Horded	16	12	12	11	12

Hording is when a student registers for more credit hours during registration and then during the first 2 weeks of the semester they drop the classes they don't want and still get a full refund. The table shows that students under flat rate or cohorts that experience more years under flat rate then pay per credit tend to register for more classes and then drop them once the school year starts, thus potentially blocking other students during the registration window, during the previous semester, from being able to register for some classes that are full during registration but then seats free up when the hording students drop them.

3.3 Theoretical Framework

3.3.1 Basic Model and Prediction

Following Hemelt and Stange (2016), we use the same static single period basic model predictor to help better understand and analyze how the tuition-pricing schedule effects post-secondary investment. In this framework, a students utility depends positively on the students lifetime consumption c as well as, on time spent not in school, n. This implies attendance will incur effort costs which will increase with the level of intensity. Students' choose time spent in school, z, in order to maximize utility $u(c, n)$, which is subject to a students' budget constraint and standard time constraint. In this model, the time constraint is total time spent in (z) and out (n) of school equals total time available, $n + z = H$. Accordingly, the students' number of credits taken can be thought of as one measure of z. The budget constraint states that consumption equals the sum of endowed income (I) and lifetime earnings minus tuition: $c = I + E(z) - T(z)$. In Hemelt and Stange's static (single-period) model, they simplify things by assuming that each addition of schooling increases earning potential by some fixed amount w, thus $E(z) = wz$. This simplification allows both Hemelt and Stange (2016) and our research to abstract from effects of non-linearities in the returns to college education and focus on the students decision about the number of credits they take in a single period. From this, tuition is a nonlinear function of credit load, changing discretely as an individuals credit load exceeds a threshold, z^*:[30]

3.4 Methodology

The objective of the paper is to compare cohorts that experienced per-credit hour pricing strategy to those that experienced a mix of flat rate and per-credit, and to those that only experienced flat rate tuition pricing. Our data starts in 2008 giving us 2 cohorts that go from freshman year to graduation under the per credit hour payment system. Freshman cohorts that entered from 2010-2012 will have one to three years under the pay per credit system, and one to three years under flat rate tuition. All incoming freshman from fall 2013 till the end of the data set will only experience FRT. Because of these differences in cohorts we will be able to compare aggregate and individual level choices. This type of set up will allow us to estimate a liner probability model with ordinary lease squares (OLS) with the following form:

$$Y_{ict} = \alpha + \beta_1 FlatRate_{ict} + \gamma_{ict} + \delta_c + \epsilon_{ict} \tag{3.1}$$

Where Y_{ict} is the outcome variable of choice, registered credits per semester, for student i in cohort c at time t. Our three primary outcome variables are indicators if a student registers for a credit load greater and within certain thresholds. These include if a student attempted over 15 credit hours, attempted between 12-15, and if a student attempted between 16-18. This follows and extends the analysis from Hemelt and Stange [2016]. β_1 is the coefficient for flat rate tuition, which is a dummy for if student i in cohort c at time t is under flat rate tuition, γ_{ict} is a vector of student level measures, background characteristics and demographics, δ_c represents major fixed effects, and ϵ_{ict} is stochastic error term.

The primary coefficient of interest is β_1, the effect of flat pricing on our outcome of interests. We only include full time students because they are the only ones that would be effected by FRT. To account for correlation in the errors among students at the same college or major, the standard errors will be clustered. Usually, cluster-robust standard errors perform poorly in settings with few clusters, but our data set contains a large enough sample size with a wide range of majors, so clustering should be achievable.

Some of the student differences might effect the outcome variables differently. One such difference might be the student age and if they are a traditional student or a non-traditional student. Older or non-traditional students might be more/less willing to take more hours to save money or because they have outside families, full time jobs, or be the sole source of income for the family. To distinguish these students a cohort average age variable will be created using the ages of all the students per cohort. Then we will set a restriction that for any age that is two standard deviations above the average cohort age these will be classified as non-traditional students. This will be incorporated into the estimation equation to see the effects flat rate tuition systems have with a students age. Gender, race, major, and family income qualities will be analyzed to see how the change to a flat rate tuition system affects each one. Here we are thinking that for students that come from a higher income quantile the decision to take a simple 12 hour or benefit with flat rate tuition and take 15 or more would not matter as much as a student coming from a lower income quantile. For accuracy the models will be run with just major fixed effects, to account for the differences in course load between majors. The data also allows us to look at differences between specific groups such as gender, race, and family income levels. This type of analysis will allow us to see how different groups of individuals are affected and how they change their behavior in response to the new pricing strategy.

3.4.1 Student GPA Analysis

The model we use for examining the effect FRT has on a students semester, yearly, and graduation GPA will be a advanced Fixed Effects model that account for semester fixed effects and major fixed effects. In this analysis we are trying to see how students on full FRT, mix FRT and per credit hour perform academically. The hypothesis is students on FRT are more likely to "horde" classes and then self select the best ones to take before the free drop period is up, thus optimizing their class selection based on how well they believe they can perform. If this is true they should in theory be able to "test drive" the classes and then pick the ones they believe they individually will perform the best in. Generally, seniors are the first to be allowed to register, followed by junior, then sophomores, and finally freshman. So the negative unintended consequence from FRT would be that there are higher classmates that are "over" registering for classes and that is taking seats away from the under classmates. That is why we believe a GPA analysis at the freshman level and then the over all level is needed. We intend to do this with the following model:

$$log(Y_{ict}) = \beta_1 FlatRate_{ict} + \gamma_{ict} + \delta_c + \sigma_t + \epsilon_{ict} \tag{3.2}$$

Where $log(Y_{ict})$ is the outcome variable of choice, GPA of student i in cohort c at time t. Our primary outcome variables are semester, yearly, and graduation GPA. β_1 is the coefficient for flat rate tuition, which is a dummy for student i in cohort c at time t. We also look at students under 1, 2, 3, or always FRT compared to those that were not. γ_{ict} is a vector of student level measures, majors, background characteristics and demographics, δ_c represents freshman major fixed effects, σ_t is a set of semester fixed effects, and ϵ_{ict} is stochastic error term.

The primary coefficient of interest is β_1, the effect of flat pricing on our outcomes of interests. As with equation 1 we are only including full time students that would be effected by FRT. Also like in equation 1, we account for correlation in the errors among students at the same college or major and cluster the standard errors will be clustered at the major level.

Hemelt and Stange [2016] identified three possible sources of bias that might come about from the basic model. One being they used samples of students from 2 universities with possible different student level demographics and characteristics. Our sample uses only students from the University of Oklahoma, where pre selection into what school and pricing system is constant. Further, we control for a wide array of student-level characteristics including Financial-aid received, major, Stafford Loan, Pell Grant, Race, sex In State/Out of State, and International status.

Second, more financial aid might offset the increase tuition and fees associated with an increase in credits, thus, diminishing the treatment. The counter to this is that the fact that the maximum PELL amount increases discretely at students that are attending school quarter-time, half-time, three-quarters-time, and full-time, but after a student is full time, taking 12 or more credit hours it does not increase in value. Like Hemelt and Stange [2016], we are also not aware of any institutional, state, or federal programs that explicitly allows or give increases in aid for students that are taking more than 12 credit hours. In our data, most students who receive and use the Pell grant at OU are receiving the maximum amount. Thus, any increases in their cost of attendance due to taking more than 12 hours will not increase the amount of grant aid for which they are eligible.

Lastly, as Hemelt and Stange [2016] point out, there is a possibility that OU's schools pricing schemes coincide with other college level attributes or policies that can potentially influence student course selection behavior. These include resources or advising for students. Because the primary focus in this research is on the public four-year sector in one state and at one institution it eliminates any institutional differences that correlate with pricing structure nationally

3.5 Results

3.5.1 Credit Per Semester

The results for equation 1 are presented in Table 3.4. Row 1 shows the prediction a student on FRT will register for more than 15 credit hours, between 12-15 credit hours, or between 16-18 credit hours compared to pay per hour. When analyzing all students that were on FRT, we see the same increase in likelihood as Hemelt and Stange [2016]. Equation 1 predicts the likelihood that students under FRT register for more than 15 credit hours is 2.8%. Equation 1 also shows FRT students are 8.7% less likely to register between 12-15 credit hours and 1.3% more likely to register for 16-18 hours.

Rows 2-4 show the results for the mixed cohorts when each cohort was on FRT for a different amount of years. Students that were on FRT for just one year do not show any big changes in registration behavior. The model predicts that students will register for over 15 hours by 0.5%, but it is not significant. Students in this cohort are also 1.1% less likely to register between 12-15 hours 1.3% more likely to register between 16-18 hours. This is expected because they were the first cohort to be able to take advantage of FRT and might have not been as aware, or for many already close to graduation and not needing to go over a 15 credit hour semester.

Row 3 shows the results for students that were a 50/50 split between pay per credit and FRT. The model predicts that these students are 1.9% more likely to register for over 15 credit hours. The model also shows that they are -2.9% likely to register for between 12-15 credit hours and 2.1% more likely to register for between 16-18. Lastly, students that only experienced per credit hour for 1 year and FRT for 3 years show a predicted increase in registering for over 15 credit hours by 3.3%. They are also 3.9% less likely to register between 12-15, and 2.8% more likely to register between 16-18.

All of these result follow each other. In general students under FRT will register for more than 15 credit hours and not between 12-15. As the number of years a student is on FRT increases, they are more likely to register for more than 15 credit hours compared to students that had less experience with FRT.

3.5.2 GPA Analysis

Turning to equation 2, and focusing on a students semester, yearly, and graduation GPA (Tables 3.5-3.7) we are able to see that students under FRT are possibly optimizing because they all have higher GPA's compared to non-FRT students. Hemelt and Stange [2016]'s basic model predicts that students would try and register for an additional hour or hours if they thought it would maximize their future outcome. One concern with the model was it was "lumpy", meaning that if it was optimal for a student to register for just one credit hour this would be difficult or not possible because most classes are 3-4 credit hours. But from the data, on average, students on FRT were registering for 3 hours. When the marginal cost to register for over 15 hours is 0, we believe that they are able to get around the uncertainty problem, "test drive" classes, and within the 2 week window drop with no penalty. Thus, they are able to pick the classes they think they will do best in that semester. The results from equation 2 show just that. Again, in this model the standard errors are clustered to account for correlation in the errors among students at the same college or major. Tables 3.5-3.7 show the results for the semester, yearly, and graduation GPA's for students that were under FRT and Mixed years compared to those that were not or on a different level of mixed years.

All the results back up the findings from the credit hours linear probability model, and show that in general students on FRT register for more classes and have higher semester, yearly, and graduation GPA. Specifically, students on FRT have a semester GPA that is 3.5% higher, a yearly that is 3.1% higher, and a graduation GPA that is 2.6% higher. Breaking it down among the mixed FRT cohorts we see that the longer duration on FRT the higher the percentage increase in semester, year, and graduation GPA . A student that is on FRT for one year has a semester increase of 1.8% compared to a student that is on FRT for 3 years has a semester increase of 2.5%. The same is true with yearly, 1.5% compared to 2.0%. Graduation GPA was the closest margin in difference with one year students receiving a 1.4%, where as 3 year FRT students see a 1.8% increase. This is ideal, because your graduation GPA is a key component in labor market job placements. Higher GPA's means a better possible job placement and higher future lifetime income.

The most interesting result is how close the point estimates are between the 2 year and 3 year FRT cohorts. All the point estimates are nearly identical, leading us to believe that the change in behavior was quick. This could reflect marketing from the university or the academic advisors promoting FRT.

Breaking it down, the mean GPA for per credit hour cohorts was 3.19. If they would have received the 2.6% graduation boost FRT students had, their GPA's would have been 3.28 This might have opened more doors for students in the labor market. We can also do the same with the mixed group that have a mean GPA of 3.16, under FRT it would have been 3.25. These increases sound small but in the labor market when possible employers are looking at undergraduates they might be that small edge that separates them from others.

3.6 Conclusion

This study used a rich administrative data on all University of Oklahoma incoming freshman cohorts from the fall of 2009 through spring of 2018. our paper further extends the initial analysis that Hemelt and Stange [2016] and further provides evidence on whether students educational investments respond to marginal price incentives. We find that a zero marginal price system (taking more than 15 credit hours) students are 2.6% more likely to take more than 15 hours, -7.9% less likely to attempt between 12-15 and 1.8% percent more likely to attempt between 16-18 credit hours. We also are able to see how students that experienced FRT for a different number of years (i.e. 1, 2, or 3) compare not only against each other but also to students that never experienced FRT. This analysis showed that the longer a student was under FRT, the more likely they were to attempt more credit hours. Case in point, students that were on FRT for only 1 year were only 1.7 percent more likely to attempt betwee 16-18 credit hours, where as students that experienced FRT for 3 years were 3.3 percent more likely.

The data also suggest that additional attempted credits hours does not appear to mean students are earning more credits in a semester or cumulatively, or reducing the time-to-degree completion. We do want to note that these estimates of these outcomes are admittedly less precise and more variable across specifications. We believe here students have learned to optimize by registering for a higher course load than they intend to take the following semester, then within the 2 week drop period "test driving" each class to see which ones they would do best in, then dropping the the one class they think would be the hardest for their current situation. This hypothesis aligns with the findings we see in increases in semester, year, and graduation GPA's for students under FRT. Specifically, students under FRT saw a 2.6% increase in graduation GPS's when compared to those that were on per credit hour pricing. Again, our data allows us to look at different cohorts that experienced different number of years on FRT. Students that had one year of FRT saw a slight increase of 1.8% compared to students that had 3 years of FRT who saw a semester GPA increase of 2.5%. This does come at a cost, because when higher rank classmates (i.e. seniors and juniors) are allowed to register before lower level classmates (i.e. freshman and sophomore's), then they can possibly register for a class that they will eventually drop where a lower level classmate could have fill the seat.

Universities across the country have many different views on flat rate tuition, and its effectiveness. They are looking at hard choices that they know will highly impact not only their student body but also revenue and recruitment. The main selling point for universities with FRT is that it allows a student to take more classes for free and to Finish in Four, as advertised on the University of Oklahoma's web page. It also increase the ranking for the university if they can show that more students are graduating with better GPA's and/or sooner because those are two key metrics a school is graded on.

Many universities across the country are switching to a Four-Year Locked Rate which is a compounded four-year average, allowing for a per semester fixed rate of required tuition and fees for twelve consecutive full semesters, or four consecutive years, from the time of first enrollment in an institution of higher education after high school. The only draw back with these pricing systems is either the university will widely benefit or the student will. If a students chooses the locked rate and during the time they are in college interest rates go down they are paying at a higher interest locked rate. The same is true for the university, if the interest rate goes up are charging at a lower locked rate then they otherwise could have received. With these systems there is more uncertainty as compared to FRT, where per year the university can adjust the flat 15 costs based on the current market rates.

Our finding show that flat rate tuition, at the University of Oklahoma, has impacts its student body in regards to registered credit-taking and achievement stands (GPA) in contrast to students that were not on FRT. Our theoretical extensions describe how students would increase enrollment even if the optimal level was 1 credit hour. In reality most classes are 3 or 4 hours, and the data we have shows that on average students on FRT "horde" 3 hours allowing them to register for more class in their current semester and then drop one class the following. This partially allows them to get around the uncertainty of guessing the class difficulty and pick the classes they believe they will performer best, during that given semester. Our results are also consistent with the presence of marginal adjustment frictions, large or uncertain marginal effort costs, or large nonlinear returns to degrees.

Another explanation for the effects of marginal price on college outcomes is that marginal pricing policies may help explain student choice in credits based on the overall (average) price, which determines enrollment and college choice. At the University of Oklahoma they have seen tuition prices increased due to less state and local funding and students may be optimizing to try and lessen the cost to GPA ratio by hording and optimizing the classes they take.

Our study covered several limitations that Hemelt and Stange [2016] pointed out. Our data allowed to get around the differences in institutional characteristics as a source of bias. We were able to exam the experience of an institution that recently changed it's marginal price. Our results also were able to dig deeper into the choices students make after entering college allowing us to better understand the mechanisms at work. We look at the mix cohort group and examine students that were on FRT for 1, 2, and 3 years and non-FRT students. We showed that the more years a student was on FRT the better semester GPA they had and the more likely they were to register for more classes. We also were able to control for major choice among freshman and able to track if the major choice changed by graduation date. Having FASFA and Pell grant data also allowed us to control for financial burden. Future analysis should look at a comparison of schools on FRT and those like Texas AM, that offer a locked or flexible rate payment plan to see what option provides the best options for their student body. Future work should examine student registration and GPA's between FRT and Locked Rate Tuition. In general though, with ever rising costs to attend college, any option policymakers create should be closely analyzed to make sure there are no unintended negative consequences that would put the student body and university in a worse situation than the current system.

3.7 Tables and Figures

Table 3.4: Linerar Probability Model

VARIABLES	(1) Over 15 Hours	(2) 12-15 Hours	(3) 16-18	(4) Over 15 Hours	(5) 12-15 Hours	(6) 16-18 Hours	(7) Over 15 Hours	(8) 12-15 Hours	(9) 16-18 Hours	(10) Over 15 Hours	(11) 12-15 Hours	(12) 16-18 Hours
FRT	0.028***	-0.087***	0.013***									
	(0.006)	(0.004)	(0.003)									
1 Year FRT				0.005	-0.011**	0.013***						
				(0.005)	(0.006)	(0.004)						
2 Year FRT							0.019***	-0.029***	0.021***			
							(0.006)	(0.006)	(0.005)			
2 Year FRT										0.033***	-0.039***	0.028***
										(0.007)	(0.005)	(0.004)
Financial Aid	0.054***	0.006	0.031***	0.036***	0.021***	0.021***	0.041***	0.015**	0.025***	0.041***	0.015**	0.023***
	(0.007)	(0.004)	(0.004)	(0.006)	(0.006)	(0.004)	(0.007)	(0.007)	(0.005)	(0.008)	(0.006)	(0.005)
Stafford Loan	0.013***	0.012***	0.010***	-0.002	0.025***	-0.006	-0.008	0.016*	-0.004	0.014	0.001	0.014**
	(0.005)	(0.004)	(0.003)	(0.010)	(0.007)	(0.006)	(0.011)	(0.009)	(0.008)	(0.009)	(0.009)	(0.006)
Pell Grant	-0.019***	0.025***	-0.011***	0.007	0.002	0.003	-0.007	0.023***	-0.006	-0.035***	0.038***	-0.028***
	(0.006)	(0.004)	(0.004)	(0.011)	(0.010)	(0.008)	(0.010)	(0.009)	(0.008)	(0.011)	(0.009)	(0.007)
African American	-0.004	-0.020***	-0.002	-0.031**	-0.003	-0.020**	0.003	-0.005	-0.004	-0.020*	-0.008	-0.014
	(0.007)	(0.007)	(0.005)	(0.012)	(0.013)	(0.010)	(0.011)	(0.012)	(0.009)	(0.011)	(0.011)	(0.008)
Asian	-0.012**	-0.017***	-0.003	-0.018*	-0.024***	-0.008	-0.028***	-0.017**	-0.010*	-0.024***	-0.021***	-0.011*
	(0.006)	(0.005)	(0.004)	(0.009)	(0.008)	(0.006)	(0.008)	(0.008)	(0.006)	(0.008)	(0.008)	(0.006)
Pacific Islander	-0.018	-0.017	-0.004	-0.013	-0.014	-0.011	-0.016	-0.008	-0.029	0.006	-0.057*	0.015
	(0.021)	(0.020)	(0.015)	(0.036)	(0.034)	(0.020)	(0.033)	(0.030)	(0.019)	(0.026)	(0.033)	(0.023)
American Indian	-0.000	0.009**	-0.004	0.002	-0.008	0.007	0.003	0.012	0.001	-0.002	0.011	-0.010
	(0.006)	(0.005)	(0.003)	(0.009)	(0.009)	(0.007)	(0.009)	(0.007)	(0.006)	(0.011)	(0.007)	(0.006)
Hispanic	-0.013**	0.006	-0.005	-0.027***	0.002	-0.008	-0.017	-0.002	0.005	-0.024**	-0.003	-0.006
	(0.005)	(0.004)	(0.004)	(0.010)	(0.007)	(0.006)	(0.011)	(0.010)	(0.009)	(0.012)	(0.009)	(0.008)
Male	-0.019***	0.021***	-0.014***	-0.023***	0.019***	-0.015***	-0.020***	0.012**	-0.010***	-0.015**	0.018***	-0.010**
	(0.004)	(0.003)	(0.003)	(0.006)	(0.005)	(0.004)	(0.006)	(0.005)	(0.004)	(0.006)	(0.005)	(0.004)
In State	-0.047***	0.001	-0.025***	-0.042***	-0.007	-0.021***	-0.046***	-0.007	-0.022***	-0.045***	-0.005	-0.021***
	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)	(0.005)	(0.007)	(0.005)	(0.004)
International	0.003	-0.081***	0.019*	0.016	-0.097***	-0.002	-0.004	-0.085***	0.003	-0.011	-0.097***	0.014
	(0.014)	(0.008)	(0.010)	(0.018)	(0.017)	(0.014)	(0.016)	(0.020)	(0.015)	(0.015)	(0.013)	(0.014)
Observations	193,160	193,160	193,160	70,188	70,188	70,188	72,328	72,328	72,328	72,497	72,497	72,497
R-squared	0.024	0.025	0.021	0.025	0.020	0.024	0.026	0.020	0.024	0.026	0.022	0.025
Major FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Clustered S.E.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Results are the probability that a FRT, student will or will not register for a certain course load compared to those that were not. Row 1 shos FRT to non FRT, including the mixed years FRT students. Rows 2-4 break out the differences in students that experienced different years under FRT. Standard errors are clustered at the major level, and the sample only includes full time students that would be subjected to FRT.

Table 3.5: Semester GPA Analysis

VARIABLES	(1) Ln GPA	(2) Ln GPA	(3) Ln GPA	(4) Ln GPA
Flat Rate	0.035***			
	(0.005)			
1 Year FRT		0.018***		
		(0.005)		
2 Years FRT			0.025***	
			(0.007)	
3 Years FRT				0.025***
				(0.008)
Financial Aid Received	0.075***	0.069***	0.066***	0.065***
	(0.005)	(0.006)	(0.005)	(0.007)
Stafford Loan	-0.042***	-0.050***	-0.057***	-0.047***
	(0.004)	(0.007)	(0.008)	(0.007)
Pell Grant	-0.021***	-0.021**	-0.003	-0.014*
	(0.004)	(0.008)	(0.007)	(0.007)
African American	-0.087***	-0.108***	-0.100***	-0.091***
	(0.008)	(0.011)	(0.011)	(0.009)
Asian	-0.003	-0.017*	-0.003	-0.011
	(0.004)	(0.009)	(0.007)	(0.008)
Pacific Islander	-0.020	0.013	0.016	-0.027
	(0.013)	(0.020)	(0.018)	(0.024)
American Indian	-0.016***	-0.021***	-0.012*	-0.019***
	(0.004)	(0.008)	(0.007)	(0.006)
Hispanic	-0.030***	-0.034***	-0.018**	-0.037***
	(0.004)	(0.008)	(0.008)	(0.007)
Male	-0.038***	-0.040***	-0.040***	-0.040***
	(0.004)	(0.007)	(0.006)	(0.005)
In State	-0.012***	-0.010**	-0.014***	-0.014***
	(0.003)	(0.005)	(0.005)	(0.004)
International	-0.008	-0.001	-0.001	-0.015
	(0.013)	(0.022)	(0.018)	(0.017)
Observations	187,677	67,536	69,726	69,975
R-squared	0.067	0.064	0.062	0.061
Major FE	YES	YES	YES	Yes
Semester FE	YES	YES	YES	Yes
Clustered S.E.	YES	YES	YES	Yes

Results are the log of a students semester GPA under FRT compared to those that were not. Column 1 is all FRT compared to no FRT. Columns 2-4 show how as the number of years a student was on FRT they had a higher percentage increase between the one year and two year FRT cohorts, but 2 year and 3 year FRT cohorts had the same benefit in their semester GPA.

Table 3.6: Yearly GPA Analysis

VARIABLES	(1) Ln GPA	(2) Ln GPA	(3) Ln GPA	(4) Ln GPA
Flat Rate	0.031**			
	(0.005)			
1 Year FRT		0.015***		
		(0.005)		
2 Years FRT			0.019***	
			(0.007)	
3 Years FRT				0.020***
				(0.008)
Financial Aid Received	0.072***	0.064***	0.062***	0.062***
	(0.005)	(0.006)	(0.005)	(0.006)
Stafford Loan	-0.042***	-0.048***	-0.054***	-0.046***
	(0.004)	(0.007)	(0.007)	(0.007)
Pell Grant	-0.020***	-0.021***	-0.005	-0.012*
	(0.004)	(0.007)	(0.007)	(0.007)
African American	-0.082***	-0.101***	-0.096***	-0.087***
	(0.007)	(0.010)	(0.010)	(0.008)
Asian	-0.003	-0.018*	-0.003	-0.009
	(0.004)	(0.009)	(0.006)	(0.007)
Pacific Islander	-0.020	0.017	0.021	-0.028
	(0.013)	(0.019)	(0.016)	(0.024)
American Indian	-0.015***	-0.020***	-0.011	-0.019***
	(0.004)	(0.007)	(0.007)	(0.006)
Hispanic	-0.029***	-0.031***	-0.018**	-0.039***
	(0.004)	(0.008)	(0.008)	(0.007)
Male	-0.039***	-0.039***	-0.040***	-0.040***
	(0.004)	(0.006)	(0.006)	(0.005)
In State	-0.011***	-0.008*	-0.013***	-0.012***
	(0.003)	(0.005)	(0.005)	(0.004)
International	-0.006	0.004	0.003	-0.012
	(0.012)	(0.021)	(0.017)	(0.017)
Observations	187,937	67,655	69,850	70,088
R-squared	0.091	0.088	0.083	0.084
Major FE	YES	YES	YES	Yes
Semester FE	YES	YES	YES	Yes
Clustered S.E.	YES	YES	YES	Yes

Results are the log of a students yearly GPA under FRT compared to those that were not. Column 1 is all FRT compared to no FRT. Columns 2-4 show how as the number of years a student was on FRT they had a higher percentage increase in their yearly GPA. Results show that between the two year FRT cohort and 3 year FRT cohort they receive almost the same benefit.

Table 3.7: Graduation GPA Analysis

VARIABLES	(1) Ln GPA	(2) Ln GPA	(3) Ln GPA	(4) Ln GPA
Flat Rate	0.026***			
	(0.004)			
1 Year FRT		0.014***		
		(0.005)		
2 Years FRT			0.018***	
			(0.006)	
3 Years FRT				0.018***
				(0.007)
Financial Aid Received	0.067***	0.060***	0.058***	0.058***
	(0.004)	(0.005)	(0.004)	(0.005)
Stafford Loan	-0.038***	-0.043***	-0.048***	-0.043***
	(0.003)	(0.006)	(0.006)	(0.006)
Pell Grant	-0.018***	-0.017***	-0.004	-0.013*
	(0.004)	(0.006)	(0.006)	(0.006)
African American	-0.075***	-0.093***	-0.087***	-0.078***
	(0.006)	(0.009)	(0.008)	(0.007)
Asian	0.003	-0.009	0.004	-0.001
	(0.004)	(0.008)	(0.005)	(0.006)
Pacific Islander	-0.016	0.014	0.019	-0.020
	(0.012)	(0.018)	(0.014)	(0.023)
American Indian	-0.013***	-0.017**	-0.010	-0.016***
	(0.004)	(0.007)	(0.006)	(0.005)
Hispanic	-0.026***	-0.029***	-0.016**	-0.036***
	(0.004)	(0.008)	(0.007)	(0.006)
Male	-0.035***	-0.035***	-0.035***	-0.037***
	(0.004)	(0.006)	(0.005)	(0.005)
In State	-0.007**	-0.005	-0.009**	-0.008**
	(0.003)	(0.004)	(0.004)	(0.004)
International	-0.005	0.005	0.006	-0.009
	(0.012)	(0.021)	(0.018)	(0.017)
Observations	187,981	67,680	69,879	70,116
R-squared	0.173	0.168	0.161	0.162
Major FE	YES	YES	YES	Yes
Semester FE	YES	YES	YES	Yes
Clustered S.E.	YES	YES	YES	Yes

Results are the log of a students graduation GPA under FRT compared to those that were not. Column 1 is all FRT compared to no FRT. Columns 2-4 show how as the number of years a student was on FRT they had a higher percentage increase between the one year and two year FRT cohorts, but 2 year and 3 year FRT cohorts had the same benefit in their graduation GPA.

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Appendices

Table A1: Masked Data Comparison

	(FE) Low	(FE) Middle	(FE) High	(FE) Dropped
UFB	-0.182***	-0.166***	-0.161***	-0.189***
	(0.031)	(0.023)	(0.020)	(0.023)
Total Students	0.002***	0.002***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Black Rate	0.027***	0.021***	0.019***	0.018***
	(0.001)	(0.001)	(0.001)	(0.001)
Asian Rate	-0.032***	-0.027***	-0.024***	-0.021***
	(0.002)	(0.001)	(0.001)	(0.001)
Hispanic Rate	0.005***	0.003***	0.003***	0.004***
	(0.001)	(0.000)	(0.000)	(0.001)
Free and Reduced Lunch Rate	0.003***	0.004***	0.005***	0.003***
	(0.001)	(0.000)	(0.000)	(0.001)
Annual County Wage	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Annual County Employees	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Sample Years	12-17	12-17	12-17	12-17
Time FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	53,074	53,074	53,074	30,620
R-squared	0.326	0.384	0.405	0.458

Results are in the IHS (Inverse Hyperbolic Sine), and total students per school is controlled for. Each of the control variables is a rate. For all the schools the demographics, including free lunch, rates are calculated and used in the regression. School level demographics such as total students and ethnicity are controlled for. Year 2012 represents school year 2011/2012, and year 2017 represents school year 2016/2017. Columns 1 is when the low parameter (all masked data is replaced with 1) is used for the masked data, columns 2 is when the middle parameter (masked data is replaced with 5), 3 is when the high parameter is used (masked data replaced with 9), and column 4 is when the masked data is dropped. Robust standard errors in parenthesis. $***p < 0.01, **p < 0.05, *p < 0.1$

Table A2: UFB Participation by Free-Reduced Eligible Rates

	2015/2016-2017/2018		2011/2012-2014/2015	
	UFB Rate	% of Total Schools	UFB Rate	% of Total Schools
90% +	1	5.00	.524	15.8
80% - 89%	1	10.1	.356	17.8
70% - 79%	.471	17.0	.360	13.6
60% - 69%	.345	15.9	.276	12.7
50% - 59%	.223	14.6	.179	11.7
40% - 49%	.155	12.3	.120	9.21
30% - 39%	.10	8.51	.101	6.72
20% - 29%	.071	6.41	.063	4.89
10% - 19%	.026	5.56	.058	4.43
1% - 9%	.014	4.10	.011	3.24

The schools are separated by their Free and Reduced eligible rates. The Texas Senate passed bill 376 in 2013 that required all public schools with a Free and Reduced eligible rate of 80% or higher to participate in UFB. Before this bill was passed it was the schools choice. 85% of the schools that were required after 2013 to take part in UFB had already been in UFB prior to the bill becoming law. Texas Department of Agriculture was only able to provide school level meals and snacks served for school years 2011/2012 to present. UFB Rate is the number of schools in the sample years that participate in UFB.

Table A3: IHS, LN, and LN+1 Comparison

	(1)	(2)	(3)
	IHS Campus Total	LN Campus Total	LN Plus One Campus Total
UFB	-0.166***	-0.166***	-0.166***
	(0.023)	(0.023)	(0.023)
Observations	53,074	53,074	53,074
R-squared	0.384	0.384	0.384
	IHS CC Total	LN CC Total	LN Plus One CC Total
UFB	-0.161***	-0.161***	-0.161***
	(0.027)	(0.027)	(0.027)
Observations	46,536	46,536	46,536
R-squared	0.356	0.356	0.356
	IHS Substance Total	LN Substance Total	LN Plus One Substance Total
UFB	-0.122***	-0.123***	-0.123***
	(0.024)	(0.024)	(0.024)
Observations	15,463	15,463	15,463
R-squared	0.577	0.577	0.577
	IHS Truancy Total	LN Truancy Total	LN Plus One Truancy Total
UFB	-0.221***	-0.223***	-0.223***
	(0.076)	(0.076)	(0.076)
Observations	3,156	3,156	3,156
R-squared	0.437	0.437	0.437
	IHS Fighting Total	LN Fighting Total	LN Plus One Fighting Total
UFB	-0.135***	-0.135***	-0.135***
	(0.027)	(0.027)	(0.027)
Observations	19,095	19,095	19,095
R-squared	0.360	0.360	0.360
District FE	YES	YES	YES
Year FE	YES	YES	YES
Robust S.E.	YES	YES	YES
Sample Years	2012/2013-2017/2018	2012/2013-2017/2018	2012/2013-2017/2018

Point estimates were rounded to the third decimal point. District fixed effects and robust standard errors were used on all the estimates for all three types of variables. IHS was the primary variable used in the main estimations because the sub groups do have 0s and we did not want to exclude them from the analysis. The inverse sine is approximately equal to $log(Y_{it}(1+Y_{it}^2)^{0.5}$ where Y_{it} is the conflict report for school i during time t, thus it can be interpreted in exactly the same way as a standard logarithmic dependent variable.

Table A4: Income from Additional Attendance

		Truancy		
		20	15	10
Non-Truancy	9	$\$6,930$	$\$3,780$	$\$630$
	5	$\$9,450$	$\$6,300$	$\$3,150$
	2	$\$11,340$	$\$8,190$	$\$5,040$

The above payoff matrix show the possible different combinations that high schools can see as increased attendance revenues when they participate in UFB and it reduces their truancy rates by 20%. 20,15,10 represent days that a truant student would miss. 9,5,2 represent the number of days a non truant student would miss. Looking at row 1 column 1 the $\$6,930$ would come from a truant student missing 20 days to now being non-truant and missing 9 days. The mean truancy rate across all high schools that do not participate in UFB is 62. When these schools participate in UFB truancy drops by 20%, or a reduction of 14 reports. The state pays each school $\$45$ a day for each student in attendance. Truancy kicks in when a student has missed 10 or more days in one semester, but the average student misses only 2-4 days a semester. We estimate that schools will not see the extremes but some placement near the middle of the matrix.

Figure .0A1: Discipline Groups Breakdown

Code of Conduct
21-VIOLATED LOCAL CODE OF CONDUCT

Fighting
17-MURDER/ATTEMPTED MURDER
34-SCHOOL-RELATED GANG VIOLENCE
41-FIGHTING/MUTUAL COMBAT
46-AGGRAVATED ROBBERY
47-MANSLAUGHTER
48-CRIMINALLY NEGLIGENT HOMICIDE
49-ENGAGES IN DEADLY CONDUCT

Truancy
42-TRUANCY - PARENT CONTRIBUTE TO
43-TRUANCY - 3 UNEXCUSED ABSENCES
44-TRUANCY - 10 UNEXCUSED ABSENCE
45-TRUANCY - FAILURE TO ENROLL

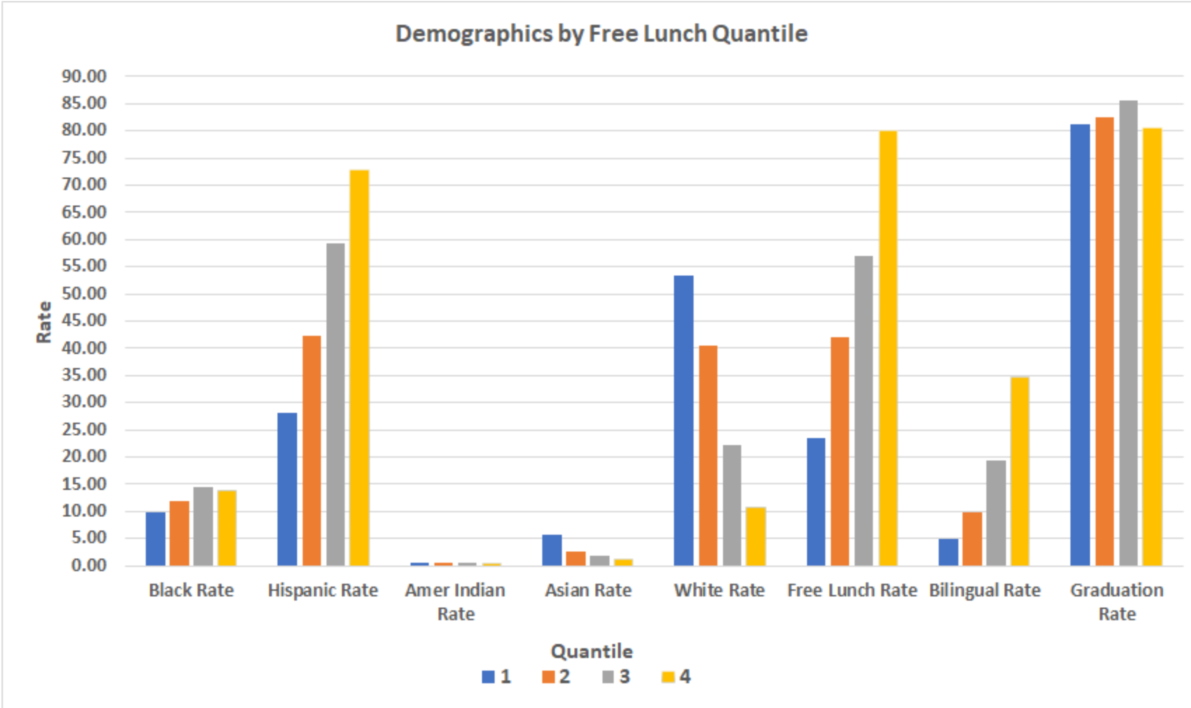
Substance
04-CONTROLLED SUBSTANCE/DRUGS
05-ALCOHOL VIOLATION
33-TOBACCO
36-FELONY CONTROLLED SUBS VIOLAT
37-FELONY ALCOHOL VIOLATION
06-ABUSE OF A VOLATILE CHEMICAL

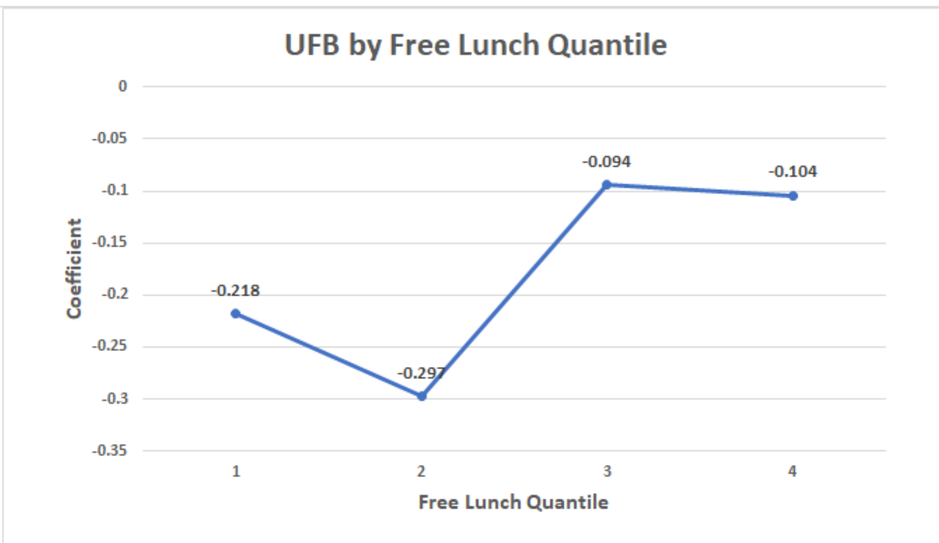
Weapon
11-FIREARM VIOLATION
12-ILLEGAL KNIFE
13-CLUB
14-PROHIBITED WEAPON
50-NON-ILLEGAL KNIFE

Other
01-PERMANENT REMOVAL BY TEACHER
23-EMERGENCY PLACEMENT/EXPULSION
08-RETALIATION AGAINST DIST EMPL
27-ASSAULT-DISTRICT EMPLOYEE
28-ASSAULT-NONDISTRICT EMPLOYEE
29-AGG ASSAULT-DISTRICT EMPLOYEE
30-AGG ASSAULT-NONDIST EMPLOYEE
31-SEXUAL ASSAULT-DIST EMPLOYEE
32-SEXUAL ASSAULT-NONDIST EMPLOYE
02-CONDUCT PUNISHABLE AS A FELONY
07-PUBLIC LEWDNESS/INDCT EXPOSURE
09-TITLE 5 FELONY - OFF CAMPUS
10-NON-TITLE 5 FELONY-OFF CAMPUS
16-ARSON
18-INDECENCY WITH A CHILD
19-AGGRAVATED KIDNAPPING
20-SERIOUS/PERSISTENT MISCONDUCT
22-CRIMINAL MISCHIEF
35-FALSE ALARM/FALSE REPORT
26-TERRORISTIC THREAT

There were over 50 total individual discipline categories that a school can report. Categories that were similar in nature were grouped together. Code of Conduct was the most reported and accounted for 31% of all the actions reported.

Figure .0A2: Free Lunch Quantile Demographics and Results





Free lunch quantiles are broken up into Q1 the bottom 25%, with a rate less than 34.4%, Q2 $(25\% - 50\%)$ with a rate between 34.5-49.1, Q3 $(50\% - 75\%)$ with a rate between 49.2-65.9, and Q4 with a rate that is $> 66\%$. Once each quantile was isolated the estimation was run with district and time fixed effects and county and school demographics. Robust standard errors were used, and results are significant to the 99% level. These results help show that UFB helps reduce the stigma associated with Free/Reduced lunches. The mechanism here are schools with lower Free lunch rates see lower participation, but when that is removed, they not only serve more meals but also have lower conflicts in schools.