ESSAYS ON THE SOCIAL IMPACT OF FIRMS

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This dissertation studies the behavior of firms and their interactions with society. Each essay analyzes this through a different lens; combining these independent and diversified applied works, I am able to depict a unique picture of the impact firms are having on our society. The first essay studies the relationship between crime and off-premise alcohol availability by looking at how fixed firm locations dictate the impact of an institutional change. Specifically looking at the repeal of Connecticut's blue law banning the sale of retail alcohol on Sundays, I find that allowing Sunday sales increased police incidents near retail alcohol stores in Hartford by approximately 14%. This increase was mostly driven by alcohol-related and less-serious crimes. The second essay examines the expansion of corporate dentistry, analyzing the impact of dental support organizations (DSOs), commonly referred to as dental chains, on the provision of dental care and their strategic entry and exit into markets. I find that the entry of DSO offices has a negligible but negative impact on the number of independent dentist offices. Moreover, I also measure the effects of rival offices and local chain networks for a heterogeneous sample of DSOs. The third essay studies how firms influence government, analyzing firms' strategic lobbying decisions in the context of unconventional upstream natural gas development in Pennsylvania. Lobbying seems to be the main strategy to influence the state government, and campaign contributions from these companies are relatively small and concentrated to legislative candidates running in districts that contain the majority of shale gas wells. Although previous campaign finance theories postulate that contributions aim to "buy policy" or "buy access" for lobbying, I find little evidence for either channel.

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PREFACE

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1.0 INTRODUCTION

Firms are ubiquitous in our modern world. A firm, often referred to as a company, business, enterprise, outfit, corporation, office or agency, is in essences the principal unit of economic activity. We interact, work with, work for, and shop with firms so often, that the concept of a firm, a concentrated organization of human activity for a specific economic purpose, is a natural as the trees and sky. But unlike the trees and the sky, firms are an aggregation of human decisions and actions, and thus have a fundamental influence on our lives and society. This dissertation seeks to understand the more nuanced and potentially unintended effects firms can have on our society.

Broadly, one can classify these effects into three categories: (i) indirect and unintentional effects from firms solely existing and operating. For example, technology firms did not intend to greatly alter our society with out everyday adoption of technology, but by operating and creating new technology they have definitely done so. In this dissertation, Chapter 2 presents an essay that looks at more subtle impact, how the locations of liquor stores determine the unintended impact of an alcohol policy change on criminal activity. (ii) indirect effects from the deliberate actions and decisions of firms; Chapter 3 looks at the deliberate decisions of dental chains, where to locate new offices, and how this potentially impacts consumers. (iii) direct and intentional effects from firms taking actions to with those intended goals; Chapter 4 studies how firms in a particular industry, shale gas, strategically take actions to intentionally influence state policies.

Each chapter is a self-contained and independent essay. Figures and tables for each essay can be found in the last two sections of each chapter, while additional information on data and technical aspects can be found in the respective chapter's section in the appendix of this dissertation.

2.0 BLUE LAWS, LIQUOR STORES, AND CRIME

2.1 INTRODUCTION

From the initial fermentation to drinking the last drop, alcohol is thoroughly regulated in most countries. The regulation of alcohol varies dramatically across states and municipalities in the U.S., and recently, many state governments have liberalized their laws concerning the sales of alcohol. Policy makers and supporters of alcohol regulation argue the widespread notion that alcohol consumption increases crime. Proponents of deregulating alcohol sales claim the regulations unnecessarily burden consumers and businesses that sell alcohol, in particular lowering businesses' ability to compete with other businesses in nearby less-regulated states. While facing fierce public debates in state politics, there is a lack of relevant research studying the potential costs of deregulating alcohol sales.

These regulatory issues have become ever more relevant as the trend of alcohol consumption has been increasing. Dwyer-Lindgren et al. (2015) document that from 2005 to 2012 the U.S. experienced a 17% national increase in heavy and binge drinking. Using data from the Behavioral Risk Factor Surveillance System (BRFSS), they also calculate county-level estimates and note that some counties experienced up to a 40% increase in aggregate drinking during this time. Similarly analyzing data from the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC), Dawson et al. (2015) find that from 2002 to 2013 the volume of consumed alcohol, the frequency of drinking, and the prevalence of monthly heavy-episodic drinking all increased. In addition to various public health issues, this trend raises serious concerns pertaining to public safety and crime.

Part of the uncertainty surrounding the potential costs of alcohol deregulation is from the lack of particular knowledge on the mechanisms linking alcohol consumption and crime.

Additional uncertainty is added from conflicting empirical results and a public discourse not grounded in research. This is further exacerbated by the fact that alcohol is regulated in numerous dimensions through multiple methods. States vary in restricting off-premises[1] alcohol sales spatially through limiting the density of stores and temporally through limiting the hours or days off-premises alcohol can be sold. Moreover, many states restrict the types, quantities, and prices of off-premises sales, and some only allow sales through state-run government agencies.[2]

One of the more common yet understudied types of alcohol regulation are blue laws: bans of off-premises alcohol sales on Sundays. In 2016, twelve states had existing Sunday retail alcohol bans[3] and eight states had partially or fully repealed their blue Sunday ban only within the last twenty years (Alcohol Policy Information System, 2016). Moreover, at the time of this writing two states, Minnesota and Indiana, had fully repealed their blue law within the last year (Jany, 2017; Cook and King, 2018). Yet despite their prevalence and current trend of being repealed, only a handful of studies have analyzed the effects of alcohol blue laws on crime in the United States. In addition to providing empirical results on a previously unstudied blue law, this essay is the first to look at temporal restrictions of retail alcohol at a city-block level while avoiding using potentially contaminated temporal controls. By doing so, these result contribute and sharpen the casual link between liquor store locations and crime.

Taking advantage of the municipal and state open data movement, I obtain police logs with precise temporal and spatial details as well as locations of all retail-alcohol stores to study the repeal of Connecticut's blue law. Employing a difference-in-differences analysis using concentric rings around stores with retail alcohol licenses, I find a 13-15% increase in overall urban crime near stores selling package alcohol. These results within Hartford use a treatment area defined by a ring with a robust radius of 0.1 miles and are mainly driven by an increase in less-serious and alcohol-related crimes. Then using Providence, Rhode Island

[1]The terms "off-premises alcohol," "package alcohol," and "retail alcohol" are used synonymously throughout this essay and all refer to sealed containers of alcohol purchased to be consumed outside the premises of the store selling them.

[2]See Carpenter and Dobkin (2011) for an excellent review of papers on various types of alcohol regulations and their impact on crime.

[3]Specifically Alabama, Arkansas, Georgia, Indiana, Kansas, Kentucky, Minnesota, Nebraska, Oklahoma, South Carolina, Tennessee, and Utah had blue laws with some states having exemptions for local options.

as a control city, I employ an additional difference-in-differences analysis to determine the repeal's citywide impact. These results, which I refer to as the "dual-city" results, show a 12% increase in overall crime. In both analyses, I find no evidence that allowing Sunday sales caused crime to change during any other day of the week, suggesting that the repeal did not simply redistribute crime across days of the week.

These results complement and expand the small but mixed empirical evidence on the effects of blue laws on crime. Consistent with Heaton (2012), Grönqvist and Niknami (2014), and Han, Branas, and MacDonald (2016), I find allowing Sunday sales causes an increase in Sunday crime. Using highly-detailed data to study temporal restrictions I am able to go further and provide evidence of the driving mechanisms as well as shine light on the spatial characteristics of this impact. These results provide crucial evidence in furthering our understanding of the relationship between crime and alcohol in urban settings. Additionally this essay has important contributions to urban planning and policy, allowing law enforcement to better understand how and where crime may increase due to changes in alcohol regulations.

The rest of this essay is organized as follows: Section 2.2 reviews related literature and provides relevant background information about the repeal of Connecticut's blue law; Section 2.3 describes the data; Section 2.4 presents the analysis within Hartford using concentric rings; Section 2.5 presents the dual-city analysis using both Hartford and Providence; Section 2.6 presents various specification checks and falsification tests; Section 2.7 discusses policy implications and concludes; lastly all figures and tables can be found in Appendices 2.8 and 2.9.

2.2 LITERATURE AND BACKGROUND

Blue laws are historically religious laws, banning commercial activity on the Christian holy day. The term "blue laws" was supposedly coined from the blue paper used by the New Haven colony[4] to mass print their code of law, which by 1665 was getting difficult for colonists to remember (Laband and Heinbuch, 1987). Historically, blue laws of some type were quite

[4]The New Haven colony coincidently made up part of present day Connecticut.

common across the United States. Gruber and Hungerman (2008) look at changes in church attendance in response to the repeal of various blue laws from the 1960s through 1980s, although none involve the sale of alcohol. These repeals caused a decrease in church attendance and donations. Moreover, they find evidence that the repeals caused a relative increase in drinking and drug use among, potentially former, church goers.[5] Similarly, Lee (2013) looks at the impact of these general commercial blue laws on educational attainment and finds that allowing Sunday commercial activity decreased the years of schooling and high school completion rate of youth in those states. Blue laws regulating alcohol seem to be more enduring than general commercial blue laws, although today the discussion on Sunday alcohol sales is typically about local economic development instead of religion and morality. By having an original and current motivation that is unrelated to crime and public safety, blue laws create an ideal natural experiment to study the relationship between alcohol and crime.

To date there has been one large-scale policy experiment on full-day restrictions of package alcohol, which took place in Sweden in 2000. Sweden relaxed their Saturday ban of retail alcohol in selected counties, as a trial policy.[6] Two social scientists, Thor Norström and Ole-Jrgen Skog, designed the policy themselves taking care to ensure the validity of the design, including locating treated jurisdictions far enough away from the control jurisdictions. Norström and Skog (2003) analyze the first stage of the policy, with six treated counties, and using various time series techniques find strong evidence that the repeal increased sales of retail alcohol but find mixed evidence on crime. They conclude that a significant increase they find in drunk driving incidents was a result of a coinciding increase in police efforts and surveillance around the capital. Norström and Skog (2005) confirm the authors' earlier results of no impact on crime while also studying the second stage of the policy, when the ban was lifted for the entire country. Recently however, Grönqvist and Niknami (2014) reevaluate the policy using additional data and different techniques and find that the first

[5] The last state to repeal a general commercial blue law was North Dakota in 1991 (Gruber and Hungerman, 2008). Thus this interesting results should not have an effect on the policy being studied in this essay.

[6] Unlike the long-standing blue laws in the U.S. and Canada, Sweden had only banned the sale of package alcohol on Saturdays in 1981. Olsson and Wikström (1982) evaluates this ban's effects and finds a decrease in crime but puzzlingly no net decrease in alcohol consumption.

stage of the policy increased crime by 18-20%. A consensus on the policy change has not been reached.

Taking advantage of rare county variation in Sunday blue laws in Virginia, Heaton (2012) finds that repealing Sunday bans increased lower-level crimes by 5% and serious alcohol-related crimes by 10%. Heaton uses other days of the week in a difference-in-differences framework and policy variation across counties for a triple-differences setup. Heaton's results are the first on blue laws in the United States, but the Virginia policy only involves the sale of hard liquor. Moreover, it is unclear how the Virginia Alcohol Beverage Control chose which independent cities and counties to be included and excluded in the policy change, which may affect the generalizability of the results and the design of the triple-differences specifications.[7]

Other papers have looked at policies similar to blue laws. Han, Branas, and MacDonald (2016) look at select Pennsylvania state-run liquor stores in Philadelphia allowing Sunday sales. They interestingly find an increase in crime near stores in low-socioeconomic neighborhoods but no impact in high-socioeconomic neighborhoods. Another related paper, Marcus and Siedler (2015) studies a German policy change restricting off-premise sales of alcohol during nighttime and find alcohol-related hospitalizations decreased 7%.

Numerous studies have looked at the relationship between crime and temporal alcohol restrictions in settings involving on-premises alcohol sales at bars and restaurants. Many of these studies, however, only provide observational results and do not attempt to address concerns of causality. A notable exception is Biderman, Mello, and Schneider (2010), who use variation in the adoption of municipal laws defining closing times of bars in Sao Paolo, Brazil. They find that restricting late-night on-premises alcohol sales had a 10% reduction in homicides and an 8% reduction in batteries. Another notable example is Anderson, Crost, and Rees (2017), who exploiting changes in Kansas' dry laws find that allowing more on-premises liquor licenses created a 5% increase in violent crime.

An aspect missing from the current literature is how off-premises alcohol sales affect crime. This of course has important policy concerns but also can provide insight into the

[7] Heaton (2012) uses both a difference-in-differences and a triple-differences design to study the Virginia policy. This nonrandom selection of jurisdictions does not affect the design of the difference-in-differences results in Heaton (2012) nor his numerous robustness checks.

mechanisms through which alcohol consumption may cause criminal activity. One potential channel is that the retail sale of alcohol can directly increase alcohol consumption. The mechanisms of alcohol consumption and crime have been studied extensively in the alcohol literature. Experimental studies, including Fromme, Katz, and D'Amico (1997) and Lane et al. (2004), have shown alcohol consumption can induce risk-taking behavior and decisions. Exum (2006) reviews a wide range of experimental studies examining alcohol consumption and violent behavior and concludes that there is sound experimental evidence that "alcohol [has] a causal influence on violent behavior." These can be problematic for off-premises alcohol since their sale allows individuals to consume alcohol at public locations in unmonitored doses.

A few papers have looked at understanding how changes in blue laws impact alcohol consumption. Carpenter and Eisenberg (2009) study the repeal of Ontario's blue law and its effects on alcohol consumption; they find that allowing state-owned liquor stores to operate on Sundays increased alcohol consumption from 7% to 15% but also decreased alcohol consumption in later days of the week, particularly on Saturdays. In contrast, Bernheim, Meer, and Novarro (2016) use variation in temporal restrictions to study commitment devices and find that extended Sunday hours increase on-premises alcohol consumption but not off-premises alcohol consumption. Yörük (2014) look at the repeal of five different blue laws between 1997-2007 and finds three of the states experienced an increase in per capita alcohol consumption. A major difference that may explain these conflicting results on off-premises consumption is in their data sources: Carpenter and Eisenberg (2009) use individual health survey data on consumption, while Bernheim, Meer, and Novarro (2016) and Yörük (2014) rely on state sales data. Hahn et al. (2010) review sixteen studies on temporal on-premise restrictions and conclude that policy changes of less than two hours often do not impact excessive alcohol consumption or alcohol related harms. This may suggest the importance of studying blue laws, full-day temporal restrictions, over other minor temporal policy changes.

Understanding how blue laws impact alcohol consumption is important when designing a study on blue laws and crime. All of the existing literature on blue laws with causal designs, Heaton (2012), Grönqvist and Niknami (2014), and Han, Branas, and MacDonald (2016), use a temporal control of other days of the week. This can be problematic, however, if allowing Sunday sales causes a decrease in consumption of alcohol during these other days, as Carpenter and Eisenberg (2009) finds. Thus, in this essay, I opt to choose various spatial controls instead of using other days of the week. This allows me to not only avoid a likely biased control group but also provides additional robustness of my control group selection.

Another channel through which alcohol retail can influence crime is by changing the travel patterns and activities of individuals purchasing alcohol. This can potentially concentrate intoxicated individuals, seeking more alcohol, and influence them to travel to particular locations they would not have traveled to otherwise. Much work in criminology has focused on the routine activities theory and crime hot spots. Liquor stores have been shown to be hot spots, e.g. Sherman, Gartin, and Buerger (1989) and Twinam (2017), and thus extending their hours of operation may significantly increase the opportunities for crime, regardless of where the package alcohol is consumed.

It is worth briefly reviewing the details and background of the retail alcohol laws in Connecticut and Rhode Island. Prior to May 2012, the state of Connecticut outlawed all sales of package alcohol on Sundays. Proponents of lifting the blue law, consisting of various consumer groups and liquor store owners near the state border, claimed the law needed "updated" to allow Connecticut businesses to be competitive. On the other side, critics of the blue law were afraid it would benefit chain liquor stores and grocery stores at the cost of small business owners (Maker, 2012; Phaneuf, 2012). Interestingly public safety and the impact on crime did not seem to be issues in the debate.

Connecticut Public Act No. 12-17 allowed Sunday retail sales for the entire state starting on Sunday May 20, 2012 (Connecticut General Assembly, 2012). Connecticut allows two types of package alcohol permits: "package store permits," i.e. liquor stores, and "grocery store beer permits," (Connecticut General Assembly, 2015). Liquor stores are permitted to sell beer, wine, and hard liquor in a variety of types but are limited in what type of non-alcohol products they can sell. Liquor stores are not permitted to operate during hours or days defined by the Connecticut Liquor Control Act. Stores with grocery beer permits can sell a variety of food products but are only allowed to sell package beer. They may not sell beer during hours or days defined by the Connecticut Liquor Control Act. The definition of what consists as a "grocery store" is quite broad, ranging from super markets to delicatessens and convenience stores (Connecticut General Assembly, 2015). Both types of permits were affected by the 2012 repeal of the blue law. Of course being private businesses, liquor stores and groceries did not have to open on Sundays if they chose not to. Not wanting to lose business to other stores, almost all stores started operating on Sundays immediately following the law change. A 2012 survey reports that across the state of Connecticut 85% of liquor stores chose to be open on Sundays following the repeal (Daley, 2012). In urban areas such as Hartford, facing stronger competition and higher demand, stores are even more likely to have chosen to be open. There do exist local and municipal ordinances in Connecticut that are permitted to be more restrictive in temporal regulations, but the city of Hartford does not have any and is thus governed by the state statue.

Located in central Connecticut, Hartford is the appropriate distance from any state border that driving to another state to simply buy package alcohol for that day seems unreasonable.[8] For reference Figure 1 shows a map of Connecticut and Rhode Island, with Hartford and Providence labeled with blue markers. The central location of Hartford, makes it an ideal setting to study the effects of changes in state retail-alcohol laws.

Rhode Island retail alcohol sales are dictated by Title 3 of the State of Rhode Island General Laws and are only permitted in licensed liquor stores (Rhode Island General Assembly, 2015b). Liquor stores have been permitted to sell alcohol on Sundays since 2004, when the Rhode Island 2004 Public Laws chapters 195 and 197 were passed (Rhode Island General Assembly, 2015a).

It should be noted that this essay uses police-incident data to measure crime, and thus while the terms "crime" and "police incidents" are used interchangeably, not all police incidents result in an arrest or conviction. Police incidents are often used in crime studies and provide a more comprehensive measure than convictions and closed criminal cases. Moreover, from the perspective of public safety and public costs looking at police incidents seem an appropriate measure, since it directly measures law enforcement activity. In the same light, it is important to look not only at "headline-grabbing" violent crimes but also at less serious crimes that occur more frequently and affect a larger share of the population. Carpenter and

[8] The northern edge of the city is approximately a 40 - 60 minute round-trip drive from the nearest point in Massachusetts, and the eastern edge of the city is approximately a 100 - 120 minute round trip from the nearest point in Rhode Island. Calculated using Google Maps: map data © 2016 Google.

Dobkin (2011) suggests that the current literature only focuses on limited subsets of crimes and that by not analyzing the effects on other types, such as property, drug-related, and social nuisance, the true social costs may be missed. Along these lines, in this essay I take a rather agnostic stand on the type of police incidents that the policy may have affected.[9]

2.3 DATA

One of the advantages of this essay over the previous literature is the high level of precision in the data. Having exact spatial and temporal information on reported police incidents, allows me to conduct a wide variety of analyses. Benefiting from the "open data revolution," I obtain data from various municipal and state government agencies. A complete list of the data sources including additional details and links to each of the following datasources is presented in Appendix A.

Hartford crime data is from the Hartford Open Data Portal's "Police Incidents" dataset. Reporting all types of police incidents, this dataset contains date, time, category classification of incident, and uncensored geocoded addresses. Providence crime data was compiled from daily police logs posted by the Providence Police Department on the city of Providence's official website, starting from June 2011. The date, time, basic category, and uncensored address for each incident are extracted from the daily logs and geocoded to obtain geographical coordinates.

Various aggregations of the Hartford crime dataset are used in the analysis throughout this essay, but the union of all subsets spans from January 2, 2011 to December 27, 2014. All of the analysis using Providence crime data uses the same dataset which spans from June 5, 2011 to December 27, 2014. Table 1 shows basic statistics of police incidents at various temporal levels for both datasets. Notably these statistics show that Hartford has more police incidents than Providence and correctly suggests a substantially higher crime rate. A more detailed comparison of the two cities is discussed later in Section 2.5.1. Since the focus

[9] In the dual-city analysis, Section 2.5, this is also a result of data limitations in matching reported categorical types between police departments.

of this essay is on the effects of a blue law, it is also worthwhile to look at the distribution of crime across days of the week. Figure 2 shows this distribution of police incidents for both cities. In both Hartford and Providence, Fridays appear to be the busiest days for the police, while Sundays seem to be the least busy.

Geographical information system (GIS) data for the city of Hartford was obtained from the GIS portal of the Hartford Open Data Portal. GIS data for the city of Providence was obtained from the Rhode Island Geographic Information System website. Using this geospatial vector data of municipal boundaries, neighborhoods, and 2010 Census tracts and block groups allows the wide scope of analysis preformed in this essay.

Store location data comes from liquor and grocery beer license records obtained from the Connecticut Open Data Portal. In Connecticut, off-premises alcohol sales are regulated at the state level through the Liquor Control Division of the state's Department of Consumer Protection. In Rhode Island, off-premises alcohol sales are regulated at the city or town level; in Providence, this falls under the jurisdiction of the Providence Board of Licenses. Unfortunately, due to limitations and systematic reporting errors in both agencies' records, it is infeasible to obtain a panel dataset of store openings and closings for either city. Instead, using the oldest snapshot of the current license list, I created a list of stores verified to have been open during the study window, 2010 to 2014. This was created by using correctly reported issue and expiration dates from licenses along with using Google Street View to verify continual operation and to check reporting inconsistencies. Stores that changed ownership but did not appear to be closed for an extended period of time were treated as continually operating.

This compilation and verification process resulted in 39 liquor stores and 107 stores with grocery beer permits in Hartford. Figure 3 shows a map of stores across the city of Hartford, with neighborhood boundaries and a base map for reference. A clear clustering of stores in business districts and along major roads can be seen throughout the city. This tight clustering creates technical difficulties when attempting to analyze the impact of stores from nearby observations; Section 2.4.1 discusses techniques used to remedy these difficulties. Additional summary statistics for each particular type of analysis is provided in the analysis "design" sections (Sections 2.4.1, 2.5.1).

2.4 WITHIN-CITY ANALYSIS: HARTFORD

2.4.1 Design and Estimation

The first type of analysis I use looks within Hartford, Connecticut, using police-incident data and the locations of stores selling off-premises alcohol to estimate the impact of Connecticut's repeal on crime. To analyze if there are any local increases in crime around liquor stores and beer-selling groceries, I employ a method of constructing concentric rings around each store creating a control and treatment area which are characteristically similar.[10] These rings are then used in a difference-in-differences framework to measure any changes in crime caused by the policy in the inner treatment ring relative to crime in the outer control ring.

While used in urban economics and criminology, this strategy can potentially have some major drawbacks and limitations. First, when being used to study changes in crime, it can be unclear if detected effects are true changes in crime or simply detecting the spatial displacement of crime within the study area. To address this concern, I employ a separate analysis in Section 2.5 which uses Providence, RI as a control city to determine the net citywide impact on crime.

Second, complications can arise if the events or locations the concentric rings are centered around occur too close to each other, since this causes overlapping observations. The spatial distribution of ring centers needs to be dispersed enough to prevent this. One potential remedy for overlapping rings is to drop any locations that contains rings that intersect with another location's rings. This changes the analysis to measure the effect of more isolated locations, which may or may not be generalizable to the true effect of all locations. If the number of potential intersections is small, the result is likely to generalize. Another potential remedy is to ignore such intersections. This however double counts any observations in intersecting areas and possibly contaminates control and treatment groups. Again if the number of potential intersections is small, this double counting probably has a minimal effect on the results.

[10] Throughout this essay I refer to these spatial areas as "rings," denoting the treatment area as the "inner ring" and the control area as the "outer ring." Mathematically though, the inner area formed by two concentric circles is a "disk" while the outer area is an "annulus."

I propose an alternative remedy that involves assigning each crime within an intersecting area to the nearest store. This results in bisecting the areas formed by any overlapping rings into equal parts and only assigning those parts to the ring(s) of the closer store. Figure 4 demonstrates this procedure for two intersecting control rings, in green. The left-most store is far enough away that its rings do not intersect, while as the right two stores have intersecting control rings, and thus I "divvy up" the intersecting areas. For each store, given a chosen radius of the inner ring, the radius of the outer ring was chosen such that outer and inner rings had equal area. This keeps the counts for treatment and control observations comparable for nonintersecting rings and also allows me to eliminate the outer ring radius as a tuning parameter.

To analyze the impact of allowing Sunday sales in Hartford, I employ a difference-in-differences framework using an ordinary least squares (OLS) panel fixed-effect model, defined by:

$$CRIME_{it} = \beta_1 + \gamma Post_t \cdot Treat_i + \delta_i + \theta_t + \epsilon_{it} \tag{2.1}$$

In this equation, $CRIME_{it}$ is the count of crimes in area i during week t. Gamma is the coefficient of interest measuring the treatment effect of the difference in differences, deltas are fixed effects for each concentric ring, and thetas are fixed effects for each week.[11] It is worth noting that due to the uniform timing and definition of treatment, all of Hartford is treated at the exact same time, the spatial and temporal fixed effects are "wiped" out with respect to the estimation of the treatment coefficient gamma.[12] The standard errors in this analysis and all subsequent regressions are calculated using robust estimates, estimated with the Huber-White sandwich variance-covariance estimator, and clustered at the corresponding spatial level.

An additional issue that any method using concentric rings must address is sensitivity to the selection of ring radii. If the rings are constructed too small the outer control area will contain treated observations, while if too large the control and treatment areas will be

[11] The difference-in-differences variables $Post_t$ and $Treat_i$ are collinear with the week and area fixed effects, respectively, and thus were not included in the regression computations.

[12] As thus while the fixed effects are estimated in Equation 2.1, the estimates for gamma are equivalent to those that could be obtained from a specification omitting both sets of fixed effects (i.e. $CRIME_{it} = \beta_1 + \beta_2 Treat_i + \beta_3 Post_t + \gamma Post_t \cdot Treat_i + \epsilon_{it}$).

too dissimilar for proper comparison. Various ring radii have been used to study the effects of crime in the literature. For example, Cui and Walsh (2015) use an inner radius of 250 feet ($\approx$ 0.05 miles) to study the effects home foreclosures and vacancies have on crime. This seems to be an ideal radius in an urban setting to create control and treatment rings that are similar in most characteristics. However, for off-premises alcohol consumption this distance may be too short. It is reasonable to think that residents may walk a block or two to buy package alcohol or that individuals wanting to drink the alcohol they recently purchased in public may walk a short distance from the store. Hence, for this analysis I choose inner ring radii varying from 0.08 to 0.1 miles. In addition, I check for sensitivity to ring size and find my results are robust for a wide variety of distances.

Looking at the data suggests this choice of inner ring radius is reasonable. Figure 5 shows pre-post plots of the average weekly crime counts for inner and outer rings for inner radii of 0.1 miles. The first plot show averages for crimes occurring on Sundays and early Monday mornings. A clear jump in crime is seen for inner rings immediately following the policy date. A very small increase is also present in the outer ring averages, which may suggest 0.1 miles is not a long enough inner radius. It is also worth noting that prior to allowing Sunday retail sales, the inner and outer rings seem to have the same general trends, although the inner rings do appear to have a higher variance. The second plot in Figure 5 shows the equivalent plot for crimes occurring between Tuesdays and Saturdays. The confidence regions for the local polynomial plot overlap substantially across the policy date for the inner ring averages; thus the small jump does not seem significant. Table 2 presents the same data collapsed in single pre-post means.

The substance of my analysis uses two samples, one comprising of weekly counts of crime on Sundays and the other of weekly counts of crimes that occurred during Tuesdays through Saturdays. This sample of other days of the week serves two purposes: first it examines if the policy redistributed crimes across days of the week; second it is used as a robustness check to verify that any effects found in the Sunday sample are not caused by some unknown source. This also brings up a major contribution of this essay to the blue law literature in relaxing the implicit assumption of temporal independence of crime across days of the week. Many of the blue law papers use counts of the other days of the week as a control group for counts on Sundays,[13] requiring that alcohol sales on Sundays will not impact crime during the other days. I choose not to use this approach since a priori it is not clear that allowing Sundays sales is independent of alcohol consumption and crime during the week. If Sunday retail sales displace crime from weekdays and Saturdays to Sundays, then using counts of these days as a control group would result in overestimating the treatment effect measured by gamma in Equation (2.1). This bias could work directly through displacing crime or indirectly though displacing alcohol consumption. Carpenter and Dobkin (2011) conclude after reviewing studies on temporal restrictions that "most important, few of the temporal availability studies provide compelling evidence on whether the policies simply shift the timing of alcohol consumption and crime or permanently reduce them." Instead I use two alternative control groups, spatial proximities in this within-city analysis and an out-of-state city in the dual-city analysis in Section 2.5.

2.4.2 Results

Table 2 and Table 3 present summary statistics and results for inner radius lengths 0.08, 0.09, and 0.1 miles. Allowing Sunday sales of retail alcohol caused a statistically significant increase in crime. Calculating the average treatment effect on the treated rings (ATT), these results vary in percent changes ranging from 13.1% (0.0249/0.189), for $r = 0.09$, to 15.4% (0.0330/0.214), for $r = 1.0$.[14] The results hold for a wide range of inner radii, as the first plot in Figure 6 demonstrates, as well as a wide range of outer radii, as Figure 7 demonstrates.

While he policy did not change the regular hours that package alcohol was permitted to be sold on the other days of the week, it could have potentially indirectly influenced crime these days of the week through redistributing alcohol consumption from other days of the week to Sundays. As mentioned earlier, Carpenter and Eisenberg (2009) found this result in the repeal of Ontario's blue law as an increase in Sunday alcohol consumption coincided with a decrease in consumption during other days of the week. While I am unable to tell what happens to alcohol consumption, I find no evidence that the policy had any temporal

[13] Heaton (2012), Grönqvist and Niknami (2014), and Han, Branas, and MacDonald (2016) all rely on this type of temporal comparison.

[14] The expressions in parentheses are my calculations of the ATT, $\hat{\gamma}$/(pre mean of treated).

displacement of crime across days of the week. Table 3 also presents results from counts of crimes occurring between Tuesdays and Saturdays, the days of the week the policy did not directly affect. Estimates from the treatment coefficient are not significantly statistically different from zero. Looking at the second plot of Figure 6, no values of inner radii seem to result in a significant effect for the other days of the week. Moreover, when looking at each day of the week individually in Figure 8 we see the same story.

This increase in crime can potentially be explained by individuals purchasing or consuming retail alcohol relatively nearby the stores they purchased them from. If the increase in crime was instead caused by a general increase in consumption of alcohol spread out across the city, one would expect to see increases in crime around population centers instead of around stores. Thus if the consumption of package alcohol is the driving mechanism, these results suggest that the consumption is by local residents or individuals consuming their purchases nearby. Exploring these ideas, Table 4 and Table 5 present summary statistics and results on samples of different types of crime: serious crimes, and less-serious crimes, and alcohol-related crimes.[15] Serious and less-serious classifications use the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) definitions.[16] In order to have mutually exclusive categories, I drop all alcohol-related crimes from the serious and less-serious categories in this analysis. It appears that the baseline increase is being driven by crimes that are less serious in nature or involving alcohol. Specifically, we see that for an inner radius of $r = 0.1$ miles, the ATT for alcohol-related crimes is approximately 135% (0.00476/0.00352) while only 15.9% (0.0202/0.127) for less-serious crimes and 14.8% (0.00912/0.0618) for serious crime. As seen in the various specifications of Table 5, these results for serious and less-serious crime appear to be sensitive to radius definitions. These results, however, does not rule out another possible explanation that the opening of stores on Sundays puts potential victims and criminals in contact with each other. This effect could even be exacerbated if individuals purchasing package liquor are already intoxicated.

[15] Alcohol-related crimes include any police incident that had one or more offense reported as: public drunkenness, open container, underage drinking, driving under the influence (DUI), and other drinking related violations. Results are robust to excluding DUIs.

[16] The FBI's UCR Program defines the following as Part I crime: criminal homicide, forcible rape, robbery, aggravated assault, burglary, theft, motor vehicle theft, and arson; while all other crimes excluding traffic offenses are considered Part II crimes (Federal Bureau of Investigation, 2011).

As with any analysis using treatment and control groups defined by concentric rings, one must be careful when attempting to extrapolate the results to broader spatial areas. This method does not take into account changes in crime outside of the concentric rings, and thus determining the citywide impact of the blue law repeal for these results can be dubious. For example, perhaps this increase in Sunday crime was at the same time offset by a decrease in crime in other areas of the city. Then the repeal would have spatially redistributed crime across the city, potentially concentrating it areas around liquor stores, instead of causing a net citywide increase. I am able to overcome this shortcoming and directly estimate citywide effects using a separate dual-city design, presented in the next section.

2.5 DUAL-CITY ANALYSIS: HARTFORD AND PROVIDENCE

2.5.1 Design and Estimation

The second type of analysis I use is also a difference-in-differences framework but uses Providence, Rhode Island as a control for the entire city of Hartford. Using police-incident data from Hartford and Providence, this analysis studies the citywide impact of the repeal as well as providing an additional check on the main results.

Connecticut was the last state in New England and the Mid-Atlantic to repeal a statewide blue law. Despite the fact that Rhode Island allows Sunday retail sales of alcohol throughout my study window, I use Providence as a control for the Connecticut policy since Hartford and Providence make an ideal comparison group for state-wide regulations potentially affecting criminal activities. Both cities are state capitals of similar size and economic influence in New England. Both are located at similar degrees of latitude and close enough to experience similar weather and regional environmental shocks. Located about 75 miles apart driving by car,[17] the two cities are close enough to experience the same general patterns and shocks in crime but far enough way to not cross-contaminate any effect caused by the policy change. There are some important differences between the cities worth noting. As of 2014, Providence

[17]Calculated using Google Maps: map data © 2016 Google.

has approximately 50,000 more residents and is part of a larger metro area.[18] As suggested in Table 1 and Table 6, Hartford has substantially higher crime rates than Providence, but Figure 9 shows both cities follow similar trends and that leading up to the policy change the patterns of Sunday police incidents seen in Providence seem to coincide with the patterns in Hartford.

To analyze the citywide impact of allowing Sunday sales in Hartford, I again employ an OLS difference in differences using Equation 2.1. Here the deltas fixed effects vary for different spatial levels as well as for the clustering of standard errors. To ensure that aggregation does not change the results, I present most results at three spatial levels: neighborhoods, Census tracts, Census block groups.

2.5.2 Results

Table 7 presents results of OLS regressions using weekly counts at various spatial levels, while Table 6 presents the corresponding summary statistics. When using weekly neighborhood observations, I find allowing Sunday sales caused a statistically significant 11.9% (0.486/4.101) increase in total crime. Looking at weekly Census tracts observations there is a significant 11.5% (0.200/1.737) increase, and looking at Census block groups there is a significant 11.9% (0.0861/0.724) increase. Comparing this citywide result to the literature, Heaton (2012) finds a 2.5% increase in total reported crime. Heaton's result is expectedly lower since he looks at a repeal of a blue law only affecting the retail sale of hard liquor and only uses police incidents that involved an arrest. On the other hand, Grönqvist and Niknami (2014) find a 20.8% increase in total crime from the repeal of the full-day ban on retail alcohol in Sweden.

Table 7 also presents results on the effects of the policy change on the other days of the week. In the second row of Table 7 estimates of the gamma coefficient from Equation (2.1) are reported using counts of crimes occurring between Tuesdays and Saturdays. These coefficients are not statistically different from zero at each spatial level. Moreover, Table 8 presents results using the counts for each individual day of the week; again none of these

[18]Source: Wolfram|Alpha, online database: [http://www.wolframalpha.com/input/?i=hartford+providence].

coefficients are statistically significant.[19]

Grouping crimes into serious, Part I, and less-serious, Part II, results in Table 10 indicate that the policy did not have a notable difference on this categorization of crime. Serious crime results are significant across all three spatial levels and vary from 12.1% (0.0213/0.176) increase for Census block groups to 16.7% (0.0704/0.421) increase for Census tracts. Less serious crimes vary from a 10.0% (0.119/1.194) increase for Census tracts to 11.9% (0.0616/0.519) increase for Census block groups. Unfortunately due to systematic reporting differences between the Hartford and Providence police departments, it is difficult to do any finer classifications.

Comparing these results with those presented in Section 2.4, we see a fuller story. Both set of results indicate that the repeal of Connecticut's blue law caused an increase in Sunday crime. Citywide estimates indicate the impact was driven by both serious and less-serious types of crimes, while the local impact around retail alcohol stores was driven by alcohol-related crimes. This suggests that there was additional crime caused by the repeal that did not occur in close proximity to liquor stores. A speculative explanation for this increase is that it may be driven by police incidents involving domestic violence and other incidents occurring at places of residence. Another potential explanation could be incidents of drunk driving, which would naturally occur in locations not necessarily near liquor stores.[20] The literature on off-premises alcohol regulation and drunk driving is mixed. McMillan and Lapham (2006) find New Mexico's repeal of their blue law increased alcohol-related crashes and fatalities, while Maloney and Rudbeck (2009) look at the same blue law and find no effect. Lovenheim and Steefel (2011) also concludes there is a minimal or zero impact from blue laws after studying multiple states' repeals and using data from the American Time Use Survey to measure changes in patterns of alcohol consumption.[21]

However, since the magnitude of the total ATT is similar for both the within-city and dual-city results, this also suggests there may indeed be some slight spatial redistribution or displacement of crime from the repeal. The consensus on how crime is displaced across

[19] For brevity, summary statistics for the regressions in Table 8 are not reported; available upon request.

[20] As mentioned, due to differences in Hartford's and Providence's classification systems of police incidents, it is unfortunately infeasible to empirically check if domestic violence or DUI incidents changed as a result of the policy.

[21] See Teutsch, Geller, and Negussie (2018) for the definitive and exhaustive source of drunk driving issues.

space is unclear. Guerette and Bowers (2009) review various crime-prevention evaluations and calculate displacement occurred 26% of the time. While looking a full-day temporal restriction on package wine and liquor, Han, Branas, and MacDonald (2016) concludes there is no evidence of displacement, but due to data limitations they are only able to check this within a relatively small radius around each store and not across the city as a whole.

2.6 SPECIFICATION CHECKS

Due to the timing of Connecticut's blue law repeal being on May 20th, a potential concern of both the within-city and dual-city designs is that the repeal coincided with the general summertime increase in crime. Figure 5 and Figure 9 both demonstrate the clear seasonal pattern of crime in Hartford and Providence. To check if results are being driven by seasonal patterns, I run a simple placebo test of having a false policy date on May 19th, 2013, one year after the actual policy date. Figure 10 and Table 11 present the results of this placebo test for the within-city and dual-city designs, respectively. In both designs, the false policy date results in positive but statistically insignificant point estimates.

Another potential concern for the within-city analysis can be seen in Figure 5; a handful of large single observation spikes can be seen in Sunday crimes after the repeal. Results indicate an increase in crime may be only be driven by a few large incidents and not caused by an overall increase near stores across the city. To check for this, I take x number of observations that are the largest outliers of Sunday crime counts and drop all stores that contain any of these outlier observations. This drops all observations, both inner and outer rings, for each of those stores for every week. Within-city results are robust to this procedure for values of x equalling five, ten, and fifteen. This concern is mitigated in the dual-city analysis since results hold for multiple levels of spatial aggregation.

Lastly, an additional plausible concern is that this increase may be caused by coinciding changes in law enforcement strategies and behaviors. Perhaps additional police officers were scheduled on Sundays in response to the policy change, or officers changed their patrol routes on Sundays to include the now open beer and package stores. If the observed increase in police incidents was a result of changes in police behavior, the actual number of crimes could have remained unchanged. To check this concern, I have extensively searched the City of Hartford's annual public safety reports and the Hartford Police Department's archives of news releases, none of which contained any mention of Sunday package sales. Furthermore, research looking at the deterrence of crime suggest these changes in police behavior would bias the measured treatment effect towards zero. Evans and Owens (2007) study the impact of a national policy adding additional officers to police forces and finds it is successful in decreasing crime. Chalfin and McCrary (2017) review studies on hot-spot policing and conclude that while there is not a consensus whether hot-spot policing spatial displaces crime to nearby areas there is a consensus that police focus on the hot spots decrease crime at the hot spots. Thus adding more officers to shifts on Sundays or increasing police presence near liquor stores in response to allowing Sunday sales seems it would likely deter and decrease crime if having any mechanical impact.

2.7 CONCLUSION

Looking at the repeal of Connecticut's Sunday ban on the retail sale of alcohol, I find an increase in crime around stores selling package alcohol in Hartford. In addition, I also find a citywide increase in crime in Hartford relative to crime in Providence, Rhode Island. Together the results tell a consistent story and suggest that the repeal of the Sunday ban increased crime in Hartford. The within-city results lend force to the selection of Providence as a control city in the dual-city analysis. While the dual-city results suggest that there exists a net citywide effect and that the effects I find within Hartford are not the result of simply concentrating crime around liquor stores. Moreover, allowing Sunday sales does not seem to temporally displace crimes across days of the week. Results suggest that alcohol is indeed involved in causing this increase in crime, but more research is needed to disentangle finer mechanisms at play.

Given the results from this essay, should we reinstate the repealed blue laws? This essay of course does not study the potential benefits allowing retail sales on Sundays, which can include additional convenience for consumers, increased competition in alcohol retail markets, spillovers to other local commercial activity, and increased tax revenues.[22] But we also need to consider the additional monetary costs of these crimes and any associated increase in law enforcement efforts. Cohen et al. (2004) use a contingent valuation method to estimate the public's annual willingness to pay for crime. Using their estimates, residents living near these stores would be willing to pay \$120 to \$180 per person per year to reduce the crime caused by the repeal. Using these estimates, the residents of Hartford would be willing to pay a total up to \$2.1 to \$3.2 million a year to mitigate this increase in Sunday crime. Similarly, Pope and Pope (2012) calculate elasticities of housing values that suggest Hartford suffered a 0.22-0.28% decrease in property values as a result of this increase in crime.[23] Lastly, using estimates from McCollister, French, and Fang (2010), the total social cost of the blue law repeal is between \$3.6 and \$14.7 million per year.

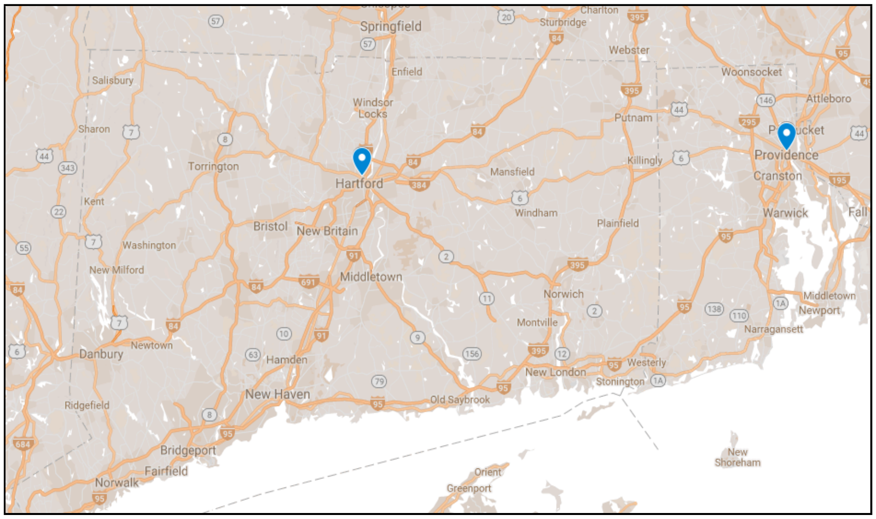
Policy makers should weigh both sides of the argument, seeking well-founded empirical evidence and ideas. Even if blue laws are no longer politically feasible, having other types of temporal restrictions of off-premises alcohol sales may be able to mitigate crime and have a net benefit to society. Regardless, knowing how crime will respond to changes in temporal restrictions can allow policy makers and law enforcement to better adapt and transition as laws are being "modernized." Relatively small increases in urban crime can likely be controlled with additional law enforcement efforts; thus for example, a bill allowing additional hours of sales could earmark any increases in tax revenue from alcohol sales to local law enforcement. At the very least, empirical evidence, such as that found in this essay, can help policy makers and law enforcement officials mitigate the costs of updating state alcohol laws.

[22] See Stehr (2007) for a study looking at the impact of Sundays sales and excise taxes on alcohol sales.

[23] Worth noting is that Ihlanfeldt and Mayock (2010) demonstrate that different types of crime impact housing values differently. They find that only robbery and aggravated assault have an negative impact on housing values, which may suggest the impact of the blue law on the Hartford housing market is minimal.

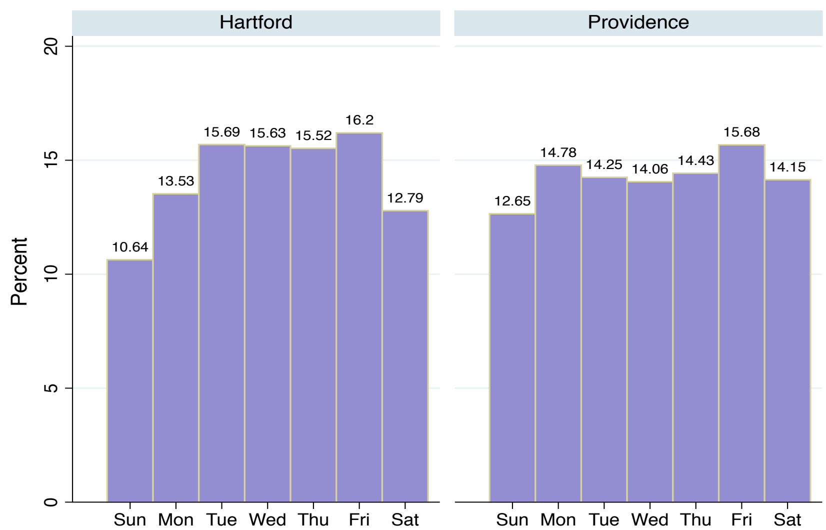
2.8 FIGURES

Figure 1: Map of Connecticut and Rhode Island



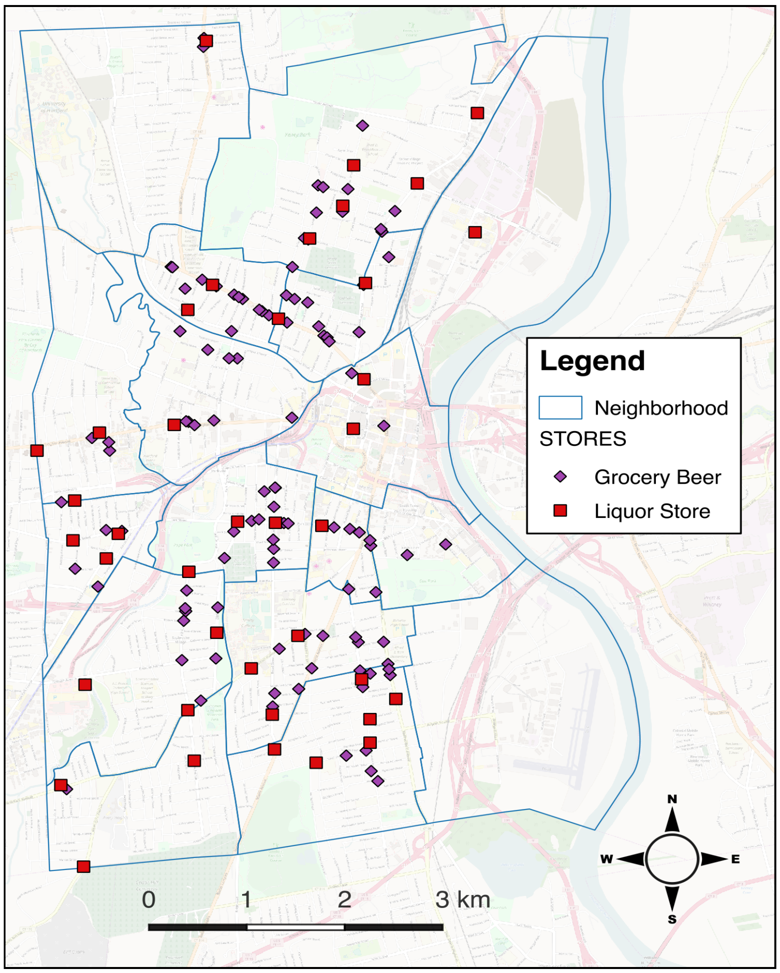
NOTE: Basemap is from Google Maps, © 2016 Google.

Figure 2: Histograms of Police Incidents across Days of the Week



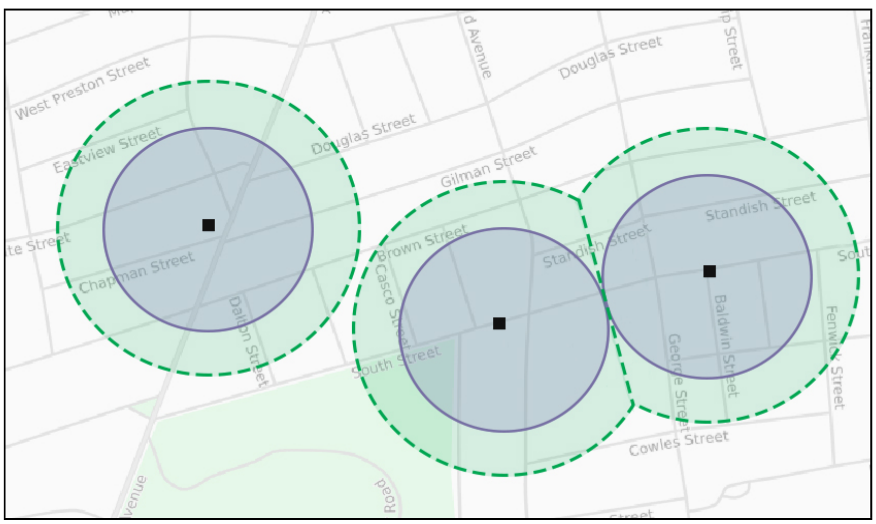
NOTE: Hartford data spans from 01/02/2011 to 12/27/2014; Providence data spans from 06/05/2011 to 12/27/2014.

Figure 3: Map of Liquor Stores and Groceries with Beer Permits in Hartford



NOTE: Basemap is ©OpenStreetMap contributors; CC BY-SA 2.0

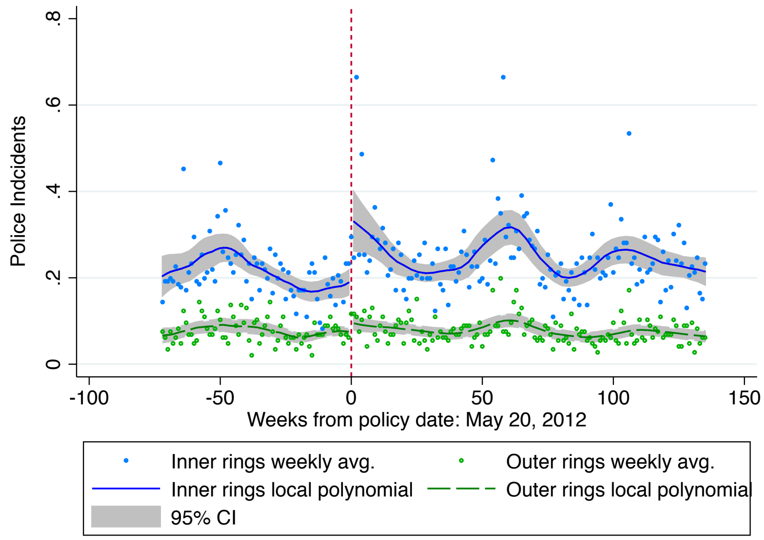
Figure 4: Diagram of Intersecting Control and Treatment Areas



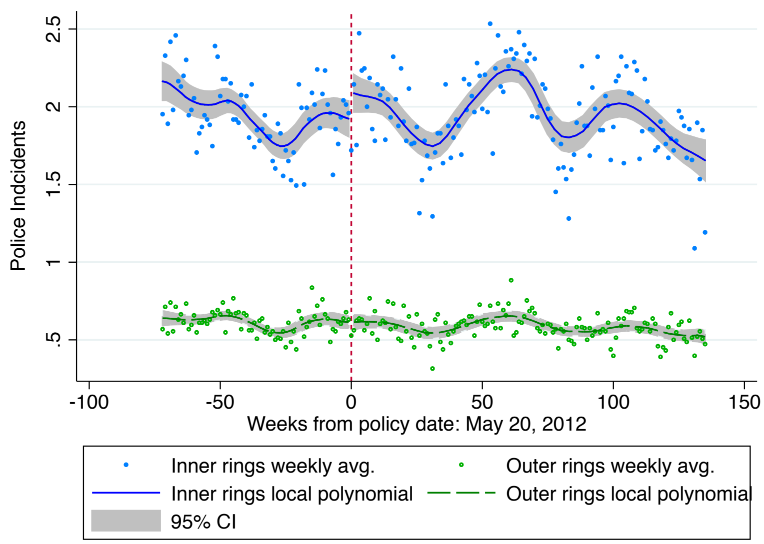
NOTE: Basemap is ©OpenStreetMap contributors; CC BY-SA 2.0

Figure 5: Scatter Plots of Weekly Averages Across Concentric Rings in Hartford

Counts of police incidents on Sundays (days affected by policy)



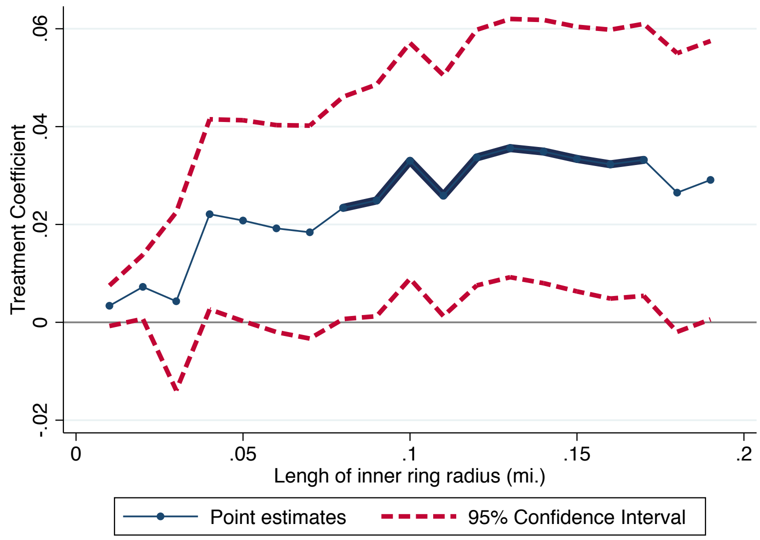
Counts of police incidents on Tuesdays thru Saturdays (days not affected by policy)



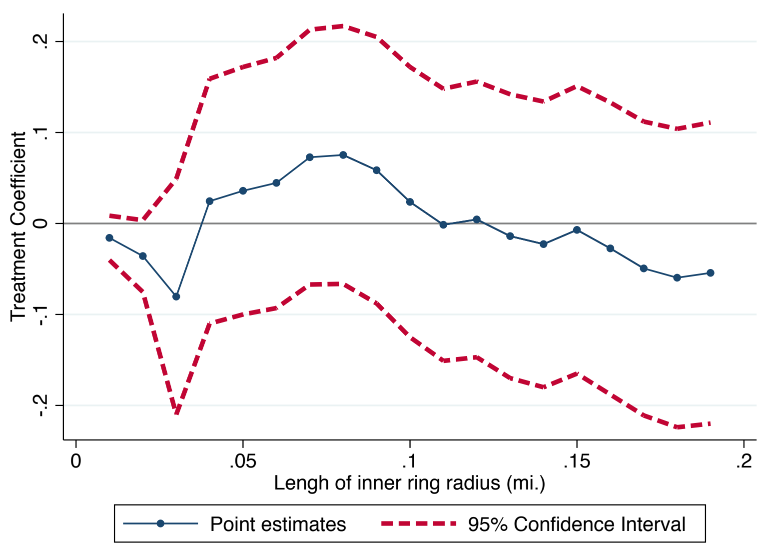
Note: For both figures rings are defined by an inner radius of 0.1 mi. and an outer radius of 0.14 mi. Local polynomial lines are calculated from separate pre and post local linear regressions using an Epanechnikov kernel with a five week bandwidth for pre periods and a six week bandwidth for post periods.

Figure 6: Within-City Sensitivity Analysis with Respect to Inner Radius

Regression coefficients from Sunday sample (days affected by policy)



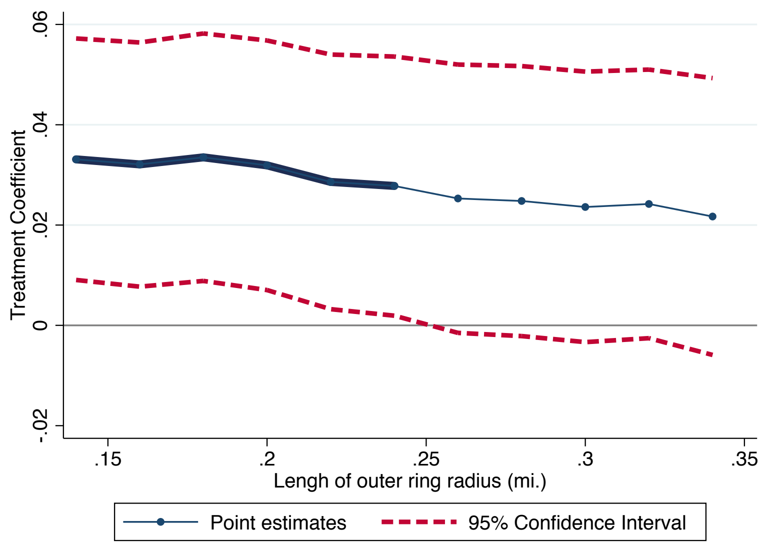
Regression coefficients from Tuesday thru Saturday sample (days not affected by policy)



Note: Confidence intervals are calculated from robust standard errors clustered at the concentric ring level; bold line segments represent multiple coefficients significant at 5%. Each regression uses an outer concentric ring equal in area to the inner ring of the given inner radius, in cases of nonintersecting rings. Data is police-incident data from Hartford and spans from 01/02/2011 to 12/27/2014.

Figure 7: Within-City Sensitivity Analysis with Respect to Outer Radius

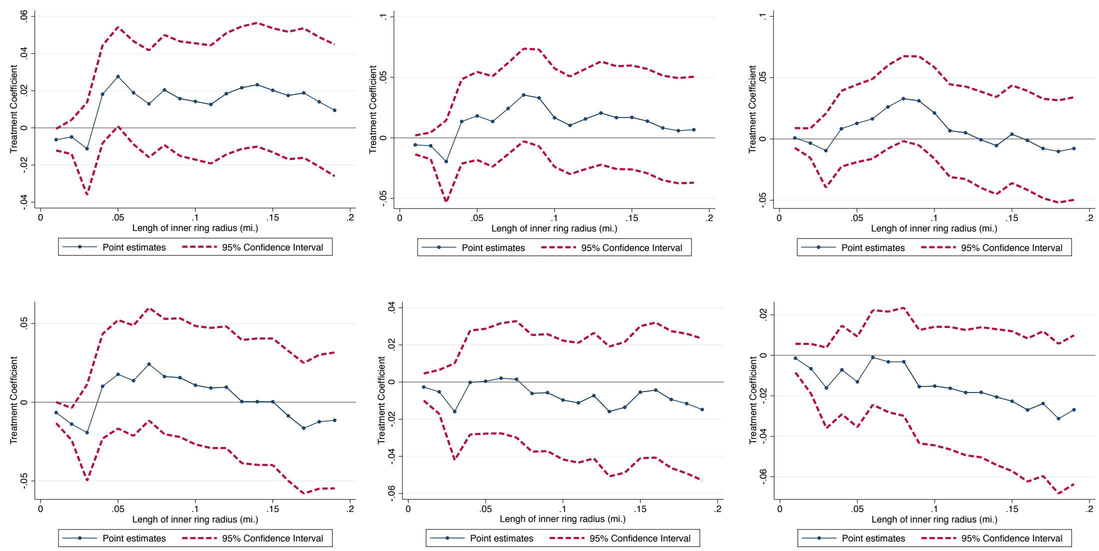
Regression coefficients from Sunday sample (days affected by policy)



NOTE: Confidence intervals are calculated from robust standard errors clustered at the concentric ring level; bold line segments represent multiple coefficients significant at 5%. Each regression uses a inner radius of 0.1 mi. Data is police-incident data from Hartford and spans from 01/02/2011 to 12/27/2014.

Figure 8: Within-City Sensitivity Analysis, Inner Radius, Individual Days

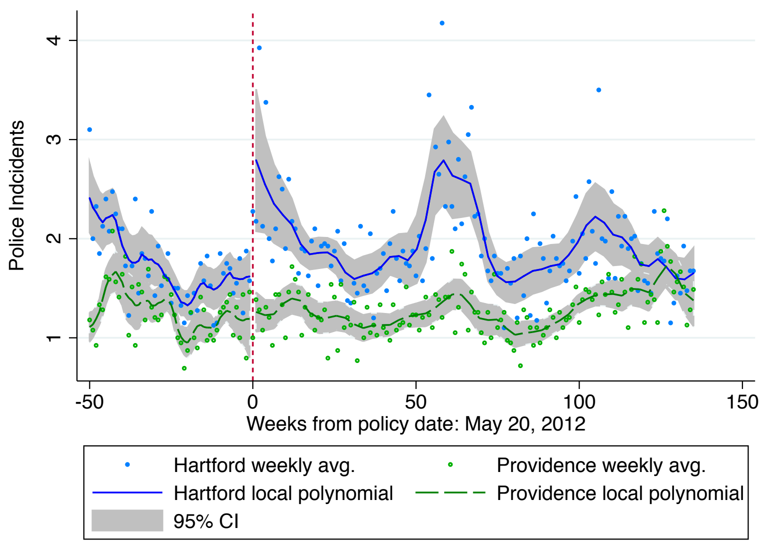
Regression coefficients from individual days of the week (days not affected by policy)



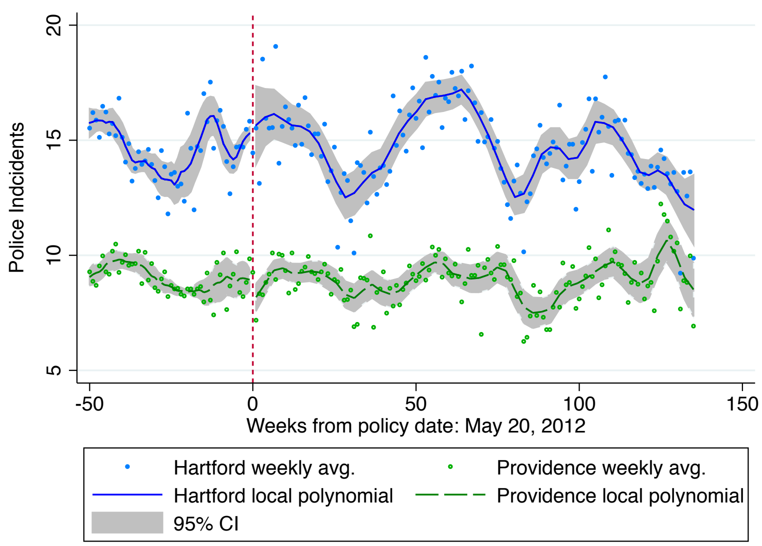
NOTE: Going from left to right is: (first row) Mondays, Tuesdays, Wednesdays; (second row) Thursdays, Fridays, Saturdays. Confidence intervals are calculated from robust standard errors clustered at the concentric ring level; bold line segments represent multiple coefficients significant at 5%. Each regression uses an outer concentric ring equal in area to the inner ring of the given inner radius, in cases of nonintersecting rings. Data is police-incident data from Hartford and spans from 01/02/2011 to 12/27/2014.

Figure 9: Scatter Plots of Weekly Averages Across Tracts by City

Counts of police incidents on Sundays (days affected by policy)



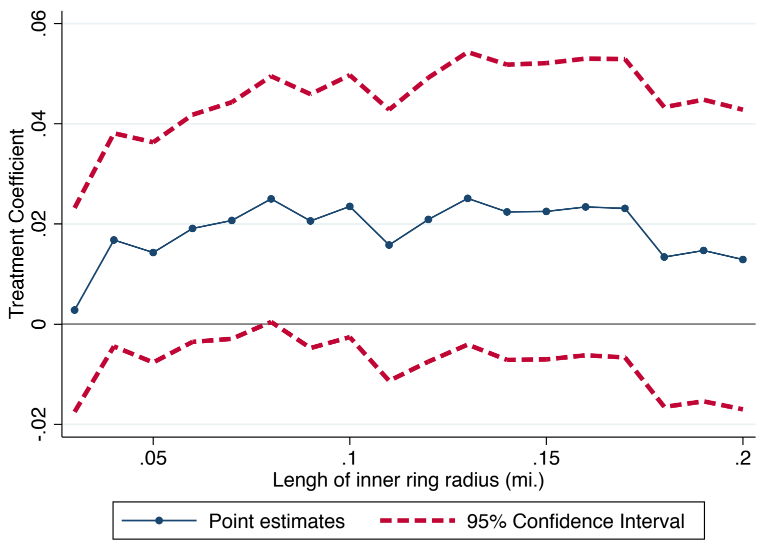
Counts of police incidents on Tuesdays thru Saturdays: (days not affected by policy)



NOTE: Local polynomial lines are calculated from separate pre and post local linear regressions using an Epanechnikov kernel with a four week bandwidth for pre periods and a five week bandwidth for post periods.

Figure 10: Within-City Sensitivity Analysis, Inner Radius, Placebo Test

Regression coefficients from Sunday sample: (days affected by policy)



NOTE: Results are from a placebo test of a false policy on May 19$^{\text{th}}$, 2013, one year after the actual policy date. Confidence intervals are calculated from robust standard errors clustered at the concentric ring level; bold line segments represent multiple coefficients significant at 5%. Each regression uses a inner radius of 0.1 mi. Data is police-incident data from Hartford and spans from 01/02/2011 to 12/27/2014.

2.9 TABLES

Table 1: Summary Statistics of Citywide Police Incidents

Hartford, CT

COUNTS	Mean	Std Dev	Min	Med	Max
Monthly	13,521	1,245	11,383	13,434	15,169
Weekly	780.1	86.70	532	774	993
Daily	111.4	25.59	37	112	251

Providence, RI

COUNTS	Mean	Std Dev	Min	Med	Max
Monthly	6,906	2,023	1,785	7,351	9,022
Weekly	482.7	50.30	358	482.5	665
Daily	68.95	12.76	14	69	117

Note: Hartford data spans from 01/02/2011 to 12/27/2014; Providence data spans from 06/05/2011 to 12/27/2014.

Table 2: Summary Statistics of Within-City Weekly Police Incidents

	(1)		(2)		(3)	
Inner Radius	$r_I = 0.08$ mi		$r_I = 0.09$ mi		$r_I = 0.1$ mi	
	(422 ft)		(475 ft)		(528 ft)	
Outer Radius	$r_O = 0.113$ mi		$r_O = 0.127$ mi		$r_O = 0.141$ mi	
	(596 ft)		(670 ft)		(744 ft)	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Sundays: Inner, Pre	0.170	0.461	0.189	0.487	0.214	0.518
Sundays: Inner, Post	0.198	0.519	0.219	0.546	0.249	0.580
Sundays: Outer, Pre	0.0729	0.289	0.0792	0.306	0.0771	0.305
Sundays: Outer, Post	0.0777	0.310	0.0843	0.326	0.0784	0.316
Tue − Sat: Inner, Pre	1.541	1.695	1.722	1.825	1.950	2.015
Tue − Sat: Inner, Post	1.568	1.919	1.734	2.026	1.942	2.167
Tue − Sat: Outer, Pre	0.620	1.056	0.665	1.122	0.615	1.061
Tue − Sat: Outer, Post	0.571	0.985	0.619	1.038	0.582	0.987

NOTE: Data is from Hartford, CT and spans from 01/02/2011 to 12/27/2014.

Table 3: Within-City OLS

	(1)	(2)	(3)
Inner Radius	$r_I = 0.08$ mi	$r_I = 0.09$ mi	$r_I = 0.1$ mi
	(422 ft)	(475 ft)	(528 ft)
Sundays	0.0234**	0.0249**	0.0330***
	(0.0115)	(0.0120)	(0.0122)
Tue thru Sat	0.0753	0.0585	0.0237
	(0.0720)	(0.0744)	(0.0754)
Observations	60,736	60,736	60,736
Number of Stores	146	146	146

NOTE: Each cell is the coefficient of interest from a separate regression at the spatial level of the column and for the sample of the row. Data is police-incident data from Hartford, CT and spans from 01/02/2011 to 12/27/2014. Robust standard errors clustered at the concentric-ring level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Summary Statistics of Within-City Weekly Police Incidents by Type

	(1)		(2)		(3)	
Inner Radius	$r_I = 0.08$ mi (422 ft)		$r_I = 0.09$ mi (475 ft)		$r_I = 0.1$ mi (528 ft)	
Outer Radius	$r_O = 0.113$ mi (596 ft)		$r_O = 0.127$ mi (670 ft)		$r_O = 0.141$ mi (744 ft)	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Type I: Inner, Pre	0.0463	0.222	0.0526	0.236	0.0618	0.254
Type I: Inner, Post	0.0549	0.246	0.0616	0.262	0.0727	0.283
Type I: Outer, Pre	0.0252	0.172	0.0256	0.173	0.0224	0.161
Type I: Outer, Post	0.0268	0.169	0.0286	0.174	0.0241	0.159
Type II: Inner, Pre	0.103	0.346	0.114	0.361	0.127	0.383
Typer II: Inner, Post	0.121	0.403	0.133	0.421	0.148	0.443
Type II: Outer, Pre	0.0396	0.204	0.0447	0.224	0.0453	0.226
Type II: Outer, Post	0.0425	0.228	0.0462	0.239	0.0458	0.241
A-R: Inner, Pre	0.00333	0.0592	0.00352	0.0608	0.00352	0.0608
A-R: Inner, Post	0.00786	0.107	0.00831	0.109	0.00871	0.111
A-R: Outer, Pre	0.000571	0.0239	0.000951	0.0338	0.00143	0.0402
A-R: Outer, Post	0.00136	0.0369	0.00186	0.0535	0.00186	0.0545

NOTE: A-R stands for "alcohol-related" crimes. Data is from Hartford, CT and spans from 01/02/2011 to 12/27/2014.

Table 5: Within-City OLS with Types of Police Incidents

	(1)	(2)	(3)
Inner Radius	$r_I = 0.08$ mi	$r_I = 0.09$ mi	$r_I = 0.1$ mi
	(422 ft)	(475 ft)	(528 ft)
Serious crime (UCR Part I)	0.00693*	0.00602	0.00912**
minus alcohol-related	(0.00397)	(0.00410)	(0.00431)
Less-serious crime (UCR Part II)	0.0149*	0.0176*	0.0202**
minus aclohol-related	(0.00900)	(0.00931)	(0.00953)
Alcohol-related crime	0.00374**	0.00388**	0.00476***
	(0.00149)	(0.00171)	(0.00173)
Observations	60,736	60,736	60,736
Number of Stores	146	146	146

Note: Each cell is the coefficient of interest from a separate regression using Sundays counts at the spatial level of the column and for the sample of the row. Data is police-incident data from Hartford, CT and spans from 01/02/2011 to 12/27/2014. Robust standard errors clustered at the concentric-ring level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Summary Statistics of Dual-City Weekly Police Incidents

Hartford, CT

	(1) Neighborhoods		(2) Tracts		(3) Block Groups	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Sundays: Pre	4.101	3.169	1.737	1.755	0.724	1.102
Sundays: Post	4.612	3.787	1.954	2.063	0.814	1.271
Tue − Sat: Pre	34.64	19.96	14.66	9.618	6.108	5.213
Tue − Sat: Post	34.67	20.69	14.68	10.74	6.118	6.171

Providence, RI

	(1) Neighborhoods		(2) Tracts		(3) Block Groups	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Sundays: Pre	1.982	1.951	1.269	1.428	0.321	0.659
Sundays: Post	2.006	1.978	1.286	1.451	0.326	0.665
Tue − Sat: Pre	14.11	10.06	9.036	6.937	2.289	2.697
Tue − Sat: Post	13.97	9.817	8.950	6.704	2.267	2.671

Note: Both Hartford and Providence data spans from 06/05/2011 to 12/27/2014.

Table 7: Dual-City OLS

	(1) Neighborhoods	(2) Tracts	(3) Block Groups
Sundays	0.486***	0.200***	0.0861***
	(0.134)	(0.0688)	(0.0248)
Tue thru Sat	0.164	0.111	0.0322
	(0.946)	(0.407)	(0.152)
Observations	7,812	14,694	46,500
Number of Areas	42	79	250

NOTE: Each cell is the coefficient of interest from a separate regression at the spatial level of the column and for the sample of the row. Data is police-incident data from Hartford, CT and Providence, RI and spans from 06/05/2011 to 12/27/2014. Robust standard errors clustered at the spatial area level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Dual-City OLS Across All Days of the Week

	(1) Neighborhoods	(2) Tracts	(3) Block Groups
Mondays	0.124	0.0345	0.0242
	(0.215)	(0.0934)	(0.0308)
Tuesdays	0.163	0.0824	0.0298
	(0.265)	(0.116)	(0.0397)
Wednesdays	0.0581	0.0440	0.00908
	(0.269)	(0.117)	(0.0401)
Thursdays	-0.00965	0.00296	-0.00141
	(0.266)	(0.113)	(0.0377)
Fridays	0.149	0.0763	0.0285
	(0.234)	(0.1000)	(0.0444)
Saturdays	-0.196	-0.0947	-0.0337
	(0.321)	(0.135)	(0.0516)
Observations	7,812	14,694	46,500
Number of Areas	42	79	250

NOTE: Each cell is the coefficient of interest from a separate regression at the spatial level of the column and for the sample of the row. Data is from Hartford, CT and Providence, RI and spans from 06/05/2011 to 12/27/2014. Robust standard errors clustered at the spatial area level in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9: Summary Statistics of Dual-City Weekly Police Incidents by Type

Hartford, CT

	(1) Neighborhoods		(2) Tracts		(3) Block Groups	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Sun, Type I: Pre	0.994	1.163	0.421	0.726	0.176	0.465
Sun, Type I: Post	1.044	1.185	0.443	0.733	0.184	0.468
Sun, Type II: Pre	2.819	2.470	1.194	1.417	0.519	0.909
Sun, Type II: Post	3.242	3.180	1.372	1.771	0.596	1.103

Providence, RI

	(1) Neighborhoods		(2) Tracts		(3) Block Groups	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Sun, Type I: Pre	0.732	1.021	0.468	0.777	0.119	0.376
Sun, Type I: Post	0.654	0.941	0.419	0.724	0.106	0.348
Sun, Type II: Pre	1.144	1.330	0.733	0.989	0.187	0.478
Sun, Type II: Post	1.237	1.449	0.793	1.089	0.203	0.512

NOTE: Both Hartford and Providence data spans from 06/05/2011 to 12/27/2014.

Table 10: Dual-City OLS with Types of Police Incidents

	(1) Neighborhoods	(2) Tracts	(3) Block Groups
Serious Crime	0.127**	0.0704**	0.0213**
(UCR Part I)	(0.0608)	(0.0282)	(0.00919)
Less-Serious Crime	0.330***	0.119**	0.0616***
(UCR Part II)	(0.111)	(0.0570)	(0.0225)
Observations	7,812	14,694	46,500
Number of Areas	42	79	250

NOTE: Each cell is the coefficient of interest from a separate regression using Sundays counts at the spatial level of the column and for the sample of the row. Data is from Hartford, CT and Providence, RI and spans from 06/05/2011 to 12/27/2014. Robust standard errors clustered at the spatial area level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: Dual-City OLS Placebo Test

	(1) Neighborhoods	(2) Tracts	(3) Block Groups
Sundays	0.164	0.0383	0.0308
	(0.119)	(0.0633)	(0.0287)
Tue thru Sat	-0.222	-0.125	-0.0352
	(0.983)	(0.396)	(0.162)
Observations	7,812	14,694	46,500
Number of Areas	42	79	250

NOTE: Results are from a placebo test of a false policy on May 19^{th}, 2013, one year after the actual policy date. Each cell is the coefficient of interest from a separate regression at the spatial level of the column and for the sample of the row. Data is police-incident data from Hartford, CT and Providence, RI and spans from 06/05/2011 to 12/27/2014. Robust standard errors clustered at the spatial area level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

3.0 CORPORATE DENTISTRY, COMPETITION, AND THE PROVISION OF DENTAL CARE

3.1 INTRODUCTION

In 2015, U.S. dental care expenditures exceeded $117.5 billion and made up about 14% of out-of-pocket health expenditures for U.S. households (Wall and Vujicic, 2016). This sizable proportion of spending on oral health has been mirrored by an increasing policy awareness and steps to integrate oral and general health care, particularly following the U.S. Surgeon General's alarming report *Oral Health in America* (IOM, 2011). At this same time, the dental profession is currently undergoing a dramatic change as the number of dentist offices operating under a dental support organization (DSO) has rapidly increased. Often referred to as "corporate dentistry" or "dental chains," DSOs are a polarizing issue among the dental profession. This essay is the first to study DSOs, analyzing their strategic behavior and impact on the provision of dental care.

In the U.S., dentistry is regulated by laws at the state level and typically governed by state dental boards. The past several years have seen intense public debates about how state legislatures and dental boards should be responding to the expansion of DSOs. Concerns over various practices have created calls to regulate DSOs on ethical justifications, to ensure that dentists' decisions are being made strictly based off the medical needs of patients and not corporate profits. Critics of this regulation argue that it is redundant to safeguards that already exist in the dental profession and may decreases competition, potentially making consumers worse off. For example in DeSanti, Feinstein, and Farrell (2012), the Federal Trade Commission advised the North Carolina state legislature "to consider the potential anticompetitive effects" of their proposed bill aimed at severely restricting and regulating

DSOs. But to date, there exists no empirical studies or policy reports on DSO competition.

The dental profession is coincidentally one of the industries originally used in the entry and exit literature by Bresnahan and Reiss (1987, 1991) and more recently is estimated in a dynamic model by Dunne et al. (2013), but it has never been studied allowing for multi-establishment firms. Most empirical applications of chain stores are of the retail industry, despite the prevalence of multi-establishment firms in almost all sectors of our economy. Applications focusing on chain behavior in the health care industry can also provide crucial empirical evidence for public health policy in other industries experiencing similar changes (e.g. hospitals, primary care providers, and nursing homes).

Using new data spanning ten western states from 2003 to 2015, I analyze the strategic entry and exit of DSO offices. This data provides an ideal window during the period of rapid DSO expansion. Descriptive results show that DSOs entry is concentrated to mostly urban and suburban areas in selective states, while other states see zero or very little entry even in markets with similar demand characteristics. Exploiting this variation in state-level entry, potentially caused by the unique and turbulent regulatory system DSOs face, I use a difference-in-differences framework to estimate the impact DSOs entry has on non-chain dentists. Estimates indicate that DSO entry has a small but negative and statistically significant impact on the number of independent dentist offices.

In addition, following a profit-inequalities technique developed by Ellickson, Houghton, and Timmins (2013) I estimate Fox (2007)'s pairwise maximum score estimator (MSE) to study the competition between DSOs. This technique is a revealed-preference approach that requires minimal structural assumptions and involves a differencing-like procedure that allows for unobserved firm and market heterogeneity. I find that in most markets DSOs face business-stealing effects from opening an additional office that overshadow local chain effects, suggesting that DSOs are willing to cannibalize portions of their own customer base. Results also indicate that DSOs face strong competitive pressure from other DSOs.

Literature. This essay contributes to the literature on models of entry and exit by providing a new application that allows for multi-establishment firms. Following advances by Mazzeo (2002) and Seim (2006) in the static entry framework,[1] Jia (2008) was the first

[1] For a review of static discrete game estimation and guidance in implementation see Ellickson and Misra

to allow chain behavior by removing the previously-common assumption of independent decisions across markets. Looking at the competition between Wal-Mart and Kmart, Jia (2008) uses a complex solution algorithm which limits her to only two chains. Several other papers have also studied various aspects of discount-retail chains looking at Wal-Mart (e.g. Holmes (2011), Zhu and Singh (2009), Zhu, Singh, and Manuszak (2009), and Ellickson, Misra, and Nair (2012)). Examples of other industries include Nishida (2015), who looks at convenience stores in Japan, and Igami and Yang (2016), who study fast food burger chains in Canada using a dynamic model. However, little if any research is devoted to multi-establishment firms in non-retail industries.

Firm competition is an important topic in health economics and has been studied in other healthcare industries, particularly hospitals. Competition is difficult to analyze though, since it can involve many moving pieces. A few notable papers use policy changes and political outcomes to look at hospital competition in the United Kingdom's National Health Service: Propper, Burgess, and Green (2004) show policy-induced hospital competition decreased quality, as measured by death rates; alternatively Bloom et al. (2015) demonstrate that increased competition increases managerial quality and reduces heart attack mortality rates; and Cookson, Laudicella, and Donni (2013) find evidence that hospital competition does not impact socio-economic equity, a concern often associated with higher levels of competition.

This essay is also naturally related to health economics research on policies regulating firm entry. Polsky et al. (2014) look at state laws regulating the entry of home care services and find that regulations do not achieve the intended goals of improved quality but do substantially reduce the number of service providers and volume of service. Schaumans and Verboven (2008) develop and estimate a static entry model to study the regulation of Belgium pharmacies. They estimate the direct impact the regulation had on the number of pharmacies and an indirect impact on physician offices.

However, there exists a general lack of empirical work in health economics on the dental market, and existing policy reports of DSOs use data sources that can only tell at best an imprecise picture. From a policy perspective this essay hopes to help fill this void, providing a descriptive summary and useful policy estimates for legislatures and regulators. In addition,

(2011); for a review of the dynamic literature see Doraszelski and Pakes (2007).

it serves as new application of a chain entry and exit model, helping us understand how multi-establishment firms compete with each other and with existing single-location firms.

The rest of this essay is organized as follows: Section 3.2 provides relevant background information about DSOs and the dental profession, including ongoing policy issues; Section 3.3 describes the data set used and discusses data definitions and limitations; Section 3.4 uses reduced-form methods to analyze the impact DSO entry has on independent dentist offices; Section 3.5 presents a structural profit function and maximum score estimates of competition and strategic interaction among DSOs; Section 3.6 discusses policy implications and concludes.

3.2 INDUSTRY AND POLICY BACKGROUND

Dental support organizations (DSOs) are broadly defined by Guay et al. (2014) as companies whose "core function is the direct provision of or significant support in decision making related to the management of activities [of multiple dental practices]." DSOs are often referred to by a variety of names, including dental management organizations, dental service organizations, group dental organizations, dental group practice associations, dental franchises, and more generally as corporate or retail dentistry. Nonclinical services offered by DSOs to their affiliated dental practices can include staffing, marketing, advertising, human resource, IT services, payroll, billing, accounting, legal support, and additional services (Guay et al., 2014). In a handful of states DSO are allowed legal ownership of their affiliated dental offices, while in most states all dental offices must be owned by licensed dentists. In all states, however, dentistry and clinical decisions are only permitted to be made by licensed dentists.

Corporate dentistry has become a polarizing issue among the dental profession and state regulators. Calls for regulation are often based of concerns of corporate profits influencing the clinical decisions of dentists. This worry is exacerbated by the fact that many DSOs are owned by external private equity investors (Sizemore et al., 2017). It should be noted that concerns over unethical practices at DSO offices are not unfounded. Several DSOs have been involved in wide-spread scandals of fraud and questionable practices. For example in

2004, CSHM (Church Street Health Management) faced criticism and eventually regulation in Colorardo for their excessive use of papoose boards, full-body restraining devices, on children patients (Vogrin, 2004). In 2013, the U.S. Senate Finance and Judiciary Committees released a staff report investigating Medicaid cases involving various DSOs (including CSHM, Heartland Dental, and Aspen Dental) of unethical treatment and violations of state regulations (U.S. Senate, 2013).

To address some of the concerns discussed above, this essay answers three policy-relevant questions: (1) where are DSOs locating?; (2) how are DSOs impacting non-affiliated dental offices?; and (3) how competitive are DSOs?

DSOs choose where to open new affiliated dental offices. While much attention has focused on the rapid expansion of DSOs across the U.S. (e.g. Wall and Guay (2015)), little attention has been given to the general patterns and types of locations DSOs are expanding into. This is of particular concern in dental public health policy, since many areas in the U.S. face a shortage of dentists. The U.S. Health Resource and Services Administration has a federal subsidy program that designates underserved areas as dental Health Professional Shortage Areas (HPSAs).[2] Dunne et al. (2013) estimates that the HPSA dental subsidy reduces entry cost by about 11% and thus is by no means a negligible public health program. Advocacy groups for DSOs often argue that DSOs are targeting different types of underserved populations (e.g. ASDSO (2014)), but no empirical studies have analyzed if DSOs are locating in markets that have a shortage of dentists.

Second, DSOs are having an unmeasured impact on non-affiliated offices. While from a policy perspective the direct motivation for this concern may be more rooted in protectionism than public health, it is nevertheless a real concern in state legislatures. In a report to the New Mexico legislature, Abbey (2013) reasons that, "[d]entistry is experiencing a recession of its own because of the growth of corporate dentistry." Yet again, there has been no attempts to quantify this impact. An accurate estimate of this impact is a crucial piece of information to quantify the impact of regulating DSOs.

Third, it would be beneficial to have some measure of the competition among DSOs.

[2]This subsidy program provides loan repayments and scholarships to individual dentists who commit to working in dental HPSAs.

Many calls against regulating DSOs claim that any further restrictions impacting the competition of the dental market may hurt consumers and society (e.g. DeSanti, Feinstein, and Farrell (2012) and Gleason (2016)). Since the number of DSOs, particularly large national DSOs, is relatively small, it is unclear how competitive DSOs entry is, and thus this needs to be empirically studied before being considered as a potential benefit of not passing regulation.

3.3 DATA

3.3.1 Data Sources and Summary

This essay is the first to use business-list data to study DSOs and specifically uses Infogroup's ReferenceUSA database. Other papers and research briefs in dental policy have relied on either American Dental Association (ADA) survey data (Guay et al., 2012), the U.S. Census Bureaus Statistics of U.S. Businesses (Wall and Guay, 2015), or the Census Bureau's Economic Census (Guay et al., 2012). These other data sources do not provide information on particular DSOs or their locations and can only measure the counts of dental firms by classifications of total employee size. Measuring DSOs by total employee size is prone to error, since DSOs vary in office size and legal structure.

Infogroup's ReferenceUSA includes annual business lists of dentist offices. From this data, I identify DSO offices in a systematic procedure, discussed in detail in Appendix B.1.2. Data used in this essay ranges from 2003 to 2015 and includes the following states: Colorado, Idaho, Montana, Nebraska, North Dakota, Oregon, South Dakota, Utah, Washington, and Wyoming. Entry and exit are measured as the annual change of the number of offices for each DSO in a given market,[3] defined at the county level.

There are twenty DSOs operating in this region during this time period.[4] These DSOs vary in their size, geographical scope, and temporal patterns of entry and exit. For example,

[3] Often a dentist offices moves to a new building a short distance away in the same area. Using annual changes thus does not count this as an entry or exit.

[4] These twenty DSOs include: Advantage Dental, Affordable Dentures, Aspen Dental, Birner Dental Management Services, CSHM, ClearChoice, Comfort Dental, DentalOne, Emergency Dental Care USA, Family 1st Dental, Heartland Dental, Hero Practice Services, InterDent, Kaiser Permanente Dental, Pacific Dental Services, Risas Dental, Smile Brands Group, Smiles Services, Sunrise Dental, and Williamette Dental.

Sunrise Dental and Williamette Dental only operate in Oregon, Washington, and Idaho, while Pacific Dental Services and Smile Brands Group are spread out across the U.S. in over fifteen different states. Figures 11 and 12 show the spatial distribution of DSO offices in this region. In both 2005 and 2015, DSO offices are concentrated in the large metropolitan areas of: Denver, CO; Seattle, WA; Portland, OR; and Salt Lake City, UT. By 2015, some DSOs entered markets outside these metro areas, particularly in Nebraska, Washington, Oregon, and Idaho. However, even in 2015, no DSO offices existed in North Dakota, South Dakota, and Wyoming and only one DSO office operated in Montana. There also appear to be some general patterns in the timing of entry in these regions. Figure 13 shows the trends across time of the total number of DSO offices by state. Initial expansion in these states is rather gradual followed by a period of rapid expansion between 2008 and 2013. Since 2013, expansion has seemed to slow down to levels of growth similar to those of independent dentist offices in this region.

Market characteristics come from the U.S. Bureau of Economic Analysis's Local Area Personal Income and from the U.S. Department of Health and Human Services. Additional details about the data and data sources can be found in Appendix B.1. To further investigate where these DSOs are locating, Table 12 presents summary statistics of markets for various ranges of DSO office concentration. DSOs seem to be locating in markets that on average seem to have larger and denser populations. Moreover, these markets have higher per capita income, have less agricultural-based workforces, and are less likely to be designated dental HPSA.

3.3.2 Market and Chain Definition

One's market definition is a crucial choice in all studies of firm entry and exit. Throughout this essay, I use a county as my definition of a market. When looking at the strategy of chains a market definition needs to exhaustively cover a geographic area. As thus looking at isolated cities and towns, as is common in the entry and exit literature, would not be applicable for looking at DSOs. In addition, the county is the smallest spatial unit for which accurate demographic data exist on an annual basis.

Organically-defined markets that do not use political boundaries are desirable in many settings. Netz and Taylor (2002) define markets as circles with varying radii around each gas station, and Igami and Yang (2016) draw circles with a fixed radius around clusters of fast food chains. But organically-defined markets are difficult to implement in industries which consumers often travel substantial distances. Therefore, a larger market definition is required since individuals are likely to travel for dental and medical services,[5] more than many other goods and services modeled in the entry and exit literature.[6]

Finally, choosing a larger market definition also mitigates potential issues caused by zoning restrictions, which Datta and Sudhir (2013) show can create biased estimates of demand and competition variables of entry and location choices. Defining a market at the county level ensures that a variety of different types of zoning is present in each market.

Another important definitional choice is how to define a chain, in this case a DSO. In many industries there is a clear distinction between large multi-establishment firms and small businesses that operation multiple but a small number of stores. In the dental profession, however, there is a wide variety of the scope and behavior of multi-establishment firms. Many larger dental partnerships and groups may have multiple offices in a single or adjacent counties but do not behave as chains in making strategic entry and exit decisions across multiple markets. As thus I define a DSO to be any multi-establishment firm that operates fifteen or more dental offices, counting all offices not only in this study area but across the U.S.[7] Details on how DSO offices were identified in the data are described in Appendix B.1.2. This general definition is flexible enough to include small but rising DSOs and strict enough to exclude all large local partnerships and group practices.[8] For purposes of this essay, offices that not associated with a DSO are referred to as "independent" offices, regardless if they are solo practices, partnerships, or small multi-establishment groups.

[5]There is empirical evidence supporting this claim. Using data from 2001, Probsta et al. (2006) calculate the average trip across the U.S. for medical and/or dental care takes about 22 minutes and covers 10.2 miles.

[6]E.g. fast food restaurants, convenience stores, video rental stores.

[7]This then includes large national DSOs like Heartland Dental and Aspen Dental who have a small presence in the study area but are likely making strategic decisions.

[8]While attempting to classify and define DSOs, Guay et al. (2014) discusses additional details which this essay ignores, including ownership of the DSO and variation in the nature of the agreement between the practicing dentists and the DSO. However, each of the twenty DSOs that meet the definition used in this essay, do follow the behavior and characteristics that are the defining features of DSOs as described by Guay et al. (2014) and Sizemore et al. (2017).

3.4 THE IMPACT OF DSOS ON INDEPENDENT DENTISTS

3.4.1 Design

A fundamental but unanswered question surrounding DSOs is whether or not they are displacing non-affiliated dentists. Are they like a Wal-Mart closing down main street? Or are the DSOs moving into new previously-undeveloped areas and targeting new customer bases?

To answer these questions, I opt to use a "reduced-form" strategy to analyze the impact of DSOs on independent dentist offices. As seen in the summary presented in Section 3.3.1, DSO offices in this region are concentrated in a few main states, while four states have zero DSO offices up until 2014. Anecdotal evidence suggests that selective entry into different states is due to the nonpermanent nature of state legislation and policies regulating dentistry and DSOs. This unique regulatory situation lends itself to a difference-in-differences framework, comparing markets in entered states with similar markets in states that experienced no entry.

As figures 11 and 12 show, there is no DSO entry in Montana, North Dakota, South Dakota, and Wyoming.[9] This selective entry by state boundaries seems to disregard demand characteristics at the market level. A possible explanation is due to the volatile regulatory environment DSOs find themselves in. Since dentistry is regulated at the state level and enforced by a professional state board, DSOs could quickly find themselves in bad regulatory waters. DSOs naturally lobby state governments to help ensure smooth sailing weather but doing so is costly. Lobbying potential legislation by itself creates large costs and demonstrates the high stakes DSOs face from unfriendly regulation. These factors create a large and changing state fixed cost for operating in a given state. As thus, it seems natural that a state must have more than a handful of enterable markets in order to overcome the potential costs and risk associated with sudden regulatory changes. This barrier to entry at the state level is plausibly exogenous to a local market defined at the county level.

These proposed control states are substantially different than those in which DSOs entered and in whole are by no means a valid comparison group. However, many markets in these non-entered states are rather similar to entered markets in other states. Figure

[9] One DSO office, a Comfort Dental office, opened in Montana in 2015.

14 demonstrates this similarity for 2012 in terms of population, the most-important characteristic of market demand. To ensure I am only comparing similar markets between the treated (entered states) and control (non-entered states) groups, I employ a hyperrectangle method based on demand characteristics, in the flavor of the method suggested by Porro and Iacus (2009). In this setting, this involves dropping treated markets with a population larger than the 99-percentile largest control market and dropping control markets with a smaller population than the one-percentile smallest treated market. This hyperrectangle trimming is redone for each treatment definition. By doing so, I only use the entered markets that have support in the control states, as demonstrated by the blue scatter plots with control diamonds below them in Figure 14.

Specifically, I estimate the following equation using ordinary least squares (OLS),

$$IND_{k,t} = \beta_0 + \gamma TREAT_{k,t} + \boldsymbol{\beta}\mathbf{X_{k,t}} + \theta_k + \delta_t + \epsilon_{k,t} \tag{3.1}$$

where $IND_{k,t}$ is the number of independent (non-DSO) dentist offices in market k in year t, $\mathbf{X_{k,t}}$ is a matrix of time-varying market characteristics, and θ_k and δ_t are market and year fixed effects. Standard errors are calculated using the Huber-White sandwich variance-covariance estimator and clustered at the market level.

Implementing a "natural-experiment" design on a setting in which treatment is not clearly defined by a policy or particular temporal characteristics creates difficult decisions regarding the timing of treatment. When should the markets that DSOs enter into be considered treated? A lone DSO office in a heavily-populated market should presumably have zero impact on the majority of existing dentists nor future potential independent entrants. For this reason, simply using the number of DSO offices or entries does not seem reasonable. Thus, I define treatment as when a market surpasses a threshold of 0.2 DSO offices per 10,000 people (one DSO office for a population of 50,000). This choice of threshold is of course rather arbitrary, and thus I check to make sure results are robust to different cutoffs. Table 13 presents summary statistics comparing treatment and control groups for this design and chosen threshold. Lastly, following the hyperrectangle method described earlier, markets in treated states that have zero DSO entry or never reach above this threshold are dropped for estimation, since there are potentially confounding concerns why DSO never entered these markets.

An alternative and preferred specification use Equation 3.1 with a continuous treatment effect, counting markets treated after the first DSO office opens. This is a more natural design, since markets that have a larger presence of DSOs have a larger potential for impact on independent offices and since it avoids an arbitrary definition of treatment timing. Figures 15 and 16 show raw data trends for control and treatment groups by year of first DSO entry and suggests that this timing of treatment allows for comparable pre-treatment groups.

3.4.2 Results

Table 14 and Table 15 present the OLS difference-in-differences results for binary and continuous treatment, respectively. When using a binary treatment, the estimate for the impact on independent dentists is -0.808 without any market demand variables and ranges from -0.696 to -0.650 with various measures of market demand. Since most markets in this region are experiencing positive population growth during this time period, this amounts to a very small decrease that is likely outweighed by the entry of new DSOs. The choice of a threshold for the binary treatment is robust from 0.19 to 0.24 DSO offices per 10,000 people for all specifications and is robust for specification (4) for a range over double that length.

When using a continuous treatment presented in Table 15, the estimate for the impact on independent dentists ranges from -1.144 without any market demand variables to -0.509 with. All specifications have treatment effects significant at the one-percent level, although this decrease is substantially small. A one-unit increase in the market share of DSOs, measured by DSO offices per 10,0000 people, only decreases the number independent offices by a fraction of an office.

It appears that while DSO entry does have a negative and statistically significant impact on the number of independent DSO offices, as expected, this impact is small and results in a net increase in total number of dental offices.

3.5 DSO COMPETITION AND STRATEGIC INTERACTION

3.5.1 Profit Inequalities of Entry and Exit

In order to understand how competitive DSOs are, I employ a profit inequality method developed by Ellickson, Houghton, and Timmins (2013). This method uses a revealed preference argument to estimate the structural parameters of an equivalent static entry/exit game. Besides the flexibility and minimal structure required in their method, this choice of modeling technique was chosen for a fundamental reason: it allows a large number of multi-establishment firms. There are twenty DSOs operating in this Western region during this time. For almost all other models of entry and exit, using only firm location and market data, this large number of players would prove intractable or computationally infeasible.

Another consideration for this modeling choice is data availability. This model only uses firm location data and publicly available demographic data. This lends itself to how this essay can pave the way for state policy makers to use this type of analysis in additional settings. State governments often do not have access to restricted revenue data and for many industries accurate price data may not exist, but they can easily require registration for all dentist offices, including DSOs, and use relevant contact information to determine firm locations.

Specifically, I model the per-office profit for DSO i in market k by,

$$\begin{aligned}\pi_k^i =& \beta^1 \ln(N_k^i + 1) + \beta^2 \ln(N_k^{-i} + 1) \\ &+ \beta^3 \ln(HQ_k^i) + \beta^4 \ln(FirstEntry_k^i) + \eta_i + \theta_k\end{aligned} \tag{3.2}$$

where N_k^i is the number of offices firm i operates in market k and HQ_k^i and $FirstEntry_k^i$ are the distances from market k's center of population to firm i's headquarters and firm i's first office location in k's state, respectively. η_i is a firm dummy. θ_k is assumed to have a particular functional form, $\theta_k = \gamma^X X_k + \xi_k$, where X^k includes observable market characteristics including a market dummy and where ξ_k is a structural error component.[10]

[10] As Ellickson, Houghton, and Timmins (2013) demonstrate, estimates from their first stage do not actually require a linear functional form for γ^X and are valid for a more a general definition of the market fixed effect of $\theta_k = f(X_k; \gamma^X) + \xi_k$.

Ex ante per-office profits are identical at every office in a given market for a given DSO. Thus, the total profit for firm i in market k is a function of N_k^i and is given by,

$$\Pi_k^i(N_k^i) = N_k^i \Big\{ \beta^1 \ln(N_k^i + 1) + \beta^2 \ln(N_k^{-i} + 1) + \beta^3 \ln(HQ_k^i) + \beta^4 \ln(FirstEntry_k^i) + \eta_i + \theta_k \Big\} \tag{3.3}$$

DSO i's marginal profitability of adding an additional office to market k, $\frac{\partial \pi_k^i}{\partial N_k^i}$, is driven by β^1 and represents the net chain or business-stealing effects within a market.[11] This measure the net effect of a DSO local chain network. The benefits an additional office may have on an existing DSO office include shared advertising and brand name recognition as well as sharing dentists and other employees between local offices. One would expect these benefits to increase as the number of offices a DSO operates in a market increases. However, an additional dentist office may decrease profit among the DSO's existing offices by stealing away customers. An advantage of using the design in Ellickson, Houghton, and Timmins (2013), is that it is flexible enough to accommodate chain agglomeration effects and business-stealing cannibalization effects at the same time.

Benefits from being a larger chain can of course operate across across markets as well, including better bargaining power for contracts and prices involving insurance, dental labs, equipment, and supplies. In a static framework, these are accounted for by firm fixed effects, since they should be equal across all of a DSO's offices. Similarly, controlling for unobserved market heterogeneity is also crucial, since the DSOs undoubtedly have better and more information on markets and demand than is present in public demand data. And hence, the market fixed effect θ_k is included in the profit function.

To estimate the parameters of Equation 3.3, I assume the observed configuration of DSO offices is the result of each DSO optimizing profits in each market. This implies, that for a given market a in which DSO i has N_a^i offices, any other number of offices that is not equal to N_a^i must result in a market profit that is less than or equal to $\Pi_a^i(N_a^i)$. Using this revealed-preference reasoning, one can create profit inequalities comparing the actual latent profit, calculated from the data, to hypothetical latent profits, calculated from hypothetically

[11]Recall a market is defined to be a county, which in this region is relatively large.

changing the number of offices in a given market. The use of Fox (2007)'s pairwise maximum score estimator allows estimation while only using one profit inequality per observation. Given the large number of markets and DSOs, this is key to having a tractable estimation procedure.

The inclusion of a market fixed effect creates econometric endogeneity issues, since N_a^i is a function of the structural error component ξ_k. To remedy this, I follow the procedure of creating profit inequalities by summations as employed by Ellickson, Houghton, and Timmins (2013). Consider two markets (a, b) and two firms (i, j) and define the observed summation of profits as,

$$\Pi S(i, j, a, b) = \Pi_a^i(N_a^i) + \Pi_b^i(N_b^i) + \Pi_a^j(N_a^j) + \Pi_b^j(N_b^j) \tag{3.4}$$

Now consider the following hypothetical situations: suppose we remove one of firm i's offices from market a and add it to market b; suppose we remove one of firm j's offices in market b and add it to market a. Since the observed outcome of offices in the data is assumed to be an equilibrium strategy, each of these hypothetical situations should result in lower profits for that particular firm. Hence, when summed together they should also result in lower total combined profits than the observed $\Pi S(i, j, a, b)$. Specifically, define the summation of these hypothetical profits as,

$$\widetilde{\Pi S}(i, j, a, b) = \Pi_a^i(N_a^i - 1) + \Pi_b^i(N_b^i + 1) + \Pi_a^j(N_a^j + 1) + \Pi_b^j(N_b^j - 1) \tag{3.5}$$

Comparing this hypothetical profit sum $\widetilde{\Pi S}(i, j, a, b)$ with the observed $\Pi S(i, j, a, b)$ "eliminates" θ_k. This is a result of the profit summations and the particular creation of the hypothetical observations. In some way this mirrors the technique of a difference in differences, and thus is referred to by Ellickson, Houghton, and Timmins (2013) as a "double-differencing" procedure. Now that the problematic θ_k have been "differenced" out, the β's can be estimated by the pairwise maximum score estimator proposed by Fox (2007). Note also that the firm fixed effects have also been differenced out, and so while this procedure allows for unobserved firm and market heterogeneity neither are actually estimated. In future work, I will use a modified version of Ellickson, Houghton, and Timmins (2013) second estimation stage to back out these estimates.

3.5.2 Estimation

Fox (2007) develops a pairwise maximum score estimator for multinomial discrete choice. His estimator is consistent when only using data on a pair of possible choices. As thus, while the single hypothetical situation derived above is only one out of many possible alternative office location configurations the involved DSOs could have chosen, the parameters of the profit function can be consistently estimated only using a pairwise comparison with the observed configuration. Ellickson, Houghton, and Timmins (2013) explains in further details how this particular method of profit summations preserves the rank ordering property required by Fox (2007).

The pairwise maximum score estimator is then,

$$Q_M(\vec{\beta}) = \frac{1}{M} \sum_{m=1}^{M} \mathbf{1}\left[\text{m is observed}\right] \cdot \mathbf{1}\left[\Pi S_m(i,j,a,b) > \widetilde{\Pi S}_m(i,j,a,b)\right] \tag{3.6}$$

where M is the number of comparisons. To create these pairs of profit summations, each containing a sum of real profits and hypothetical profits, I randomly draw 5,000 pairs of DSO offices ($\{i,a\}$ and $\{j,b\}$) and create the corresponding $\Pi S(i,j,a,b)$ and $\widetilde{\Pi S}(i,j,a,b)$. Great care is taken to ensure the selection of office $\{j,b\}$ is seemingly random despite the fact that it is drawn conditional on office $\{i,a\}$ already being chosen.

Multinomial discrete choice models require a scale and location normalization. As required with maximum score estimation, I follow the suggestions of Fox (2007) and employ the scale normalization of fixing one parameter, $\beta^l = \pm 1$.[12] Moreover, I use a smoothed version of the pairwise maximum score estimator developed by Yan (2017). Section B.3 provides additional details about smoothing, kernel choice, and bandwidth selection.

To optimize the smooth pairwise maximum score function, I use differential evolution, a genetic algorithm based on vector differences.[13] A global optimization technique is needed

[12] Specifically, I normalize β^4 and run the maximum score estimation for both $\beta^4 = -1$ and $\beta^4 = +1$ and choose the normalization that results in a higher score.

[13] In particular I use a form of differential evolution classified as DE/rand/1/bin with dither and follow Price, Storn, and Lampinen (2005) and Chakraborty (2008) for design and tuning control parameters. Code is available at `https://github.com/ian-mmm`.

in this setting, since maximum score and smooth maximum score functions can have many local maximum.[14]

For inference, I employ an empirical bootstrap similar to that proposed by Horowitz (1992, 2002) while using the asymptotic results developed by Yan (2017). This procedure involves calculating a bootstrapped test statistic for each estimate, from which corresponding p-values can be calculated for determining statistical significance. Section B.3 provides additional details about the bootstrap procedure.

3.5.3 Results

Table 16 presents results from the smooth maximum score estimation of the profit summations. Bootstrapped *p-values* are reported in parentheses, calculated using 400 bootstrap samples. Looking at the estimated coefficients, we see that both an additional own office and an additional competitor's office decrease a DSO's profits. A negative and significant estimate for the "Own" variable indicates that business-stealing effects from opening an additional office are greater than the benefits of increasing one's local chain network. If there is continued entry by these DSOs into these particular markets, particularly the saturated ones, it will suggest that many DSOs are willing to cannibalize portions of their own customer base to keep out rival DSOs. However, looking at the estimate for "Other" we see an additional competitor's office decreases profits over 3.2 times more than one's own additional entry. The coefficient for "Dist. First Entry" ($FirstEntry_k^i$) in specification (1) is also as expected, negative and significant, indicating that markets further away from a DSO's "home turf" are less profitable. This suggests that local chain economies or local chain network effects are indeed important to DSO profits, despite that the general net "Own" impact is negative. These results also provide suggestive evidence of a competitive environment among DSOs. I leave interpretations of marginal effects for future work.

Results are practically identical to using an alternative measure for $FirstEntry_k^i$ that discounts that distance by the number of years firm i has been operating in market k's state. In addition, to ensure that forcing β^4 (parameter for HQ_k^i) to be nonzero is not driving

[14] While not commonly used in economics, differential evolution has been gaining in popularity as an optimization choice (e.g. Fox and Bajari (2013) and Cieslak and Povala (2016)).

the results, specification (2) uses an alternative normalization of $\beta^3 = \pm 1$ (parameter for $FirstEntry_k^i$). Again an additional competitor's office decreases profits over three times more than one's own additional entry. As a result of using a different normalization though, estimates from (2) cannot be easily compared with the other specifications. In future work, I will look at a subsample of the large DSOs in this region with additional firm-specific parameters.

3.6 CONCLUSION

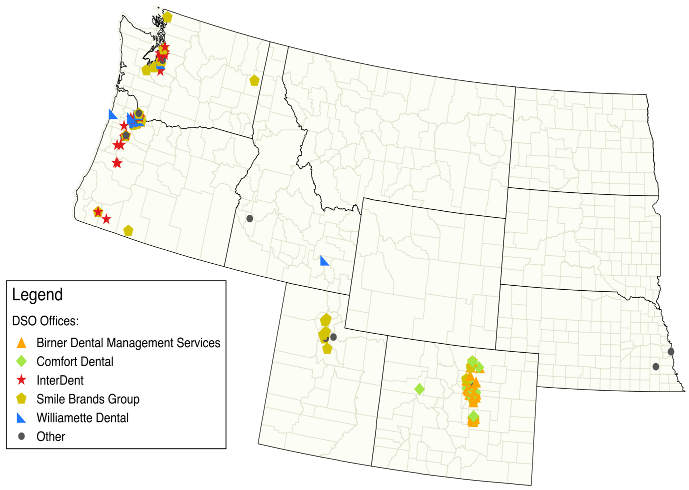
Results show that DSOs do indeed have a negative impact on independent dentists, but that this impact is negligible and does not seem to be displacing a large number dental offices. Among the geographic areas studied in this essay, DSO entry appears to be concentrated in certain states and completely absent in others. This pattern highlights the potential effects the threat of state regulations can have on DSO entry and expansion.

Moreover, results from a profit inequality framework suggest that in 2015 DSOs seem to be operating in a rather competitive environment. Both additional rival and own offices have a negative impact on a DSO's latent profits, suggesting DSOs are in competition with each other and are willing to cannibalize market share to compete with rivals. Regulation preventing or slowing DSO entry may diminishes this competition among DSOs. Future work will determine how DSO entries across time impact competition within the entire dental market.

Results from this essay do not speak to the potential benefits from regulating DSOs. Certain DSOs have undoubtedly used unethical business practices, and work should be done to see how and why these occurred. It may be in the best interest for many, if not all, states to carefully regulate and monitor DSOs. However, policy makers should note that this regulation may come at the cost of decreasing competition in the dental profession and further creating barriers of access for oral health in their states. Ideal legislation would ensure the safety and quality of DSO services without decreasing competition or providing protectionism for independent dentists.

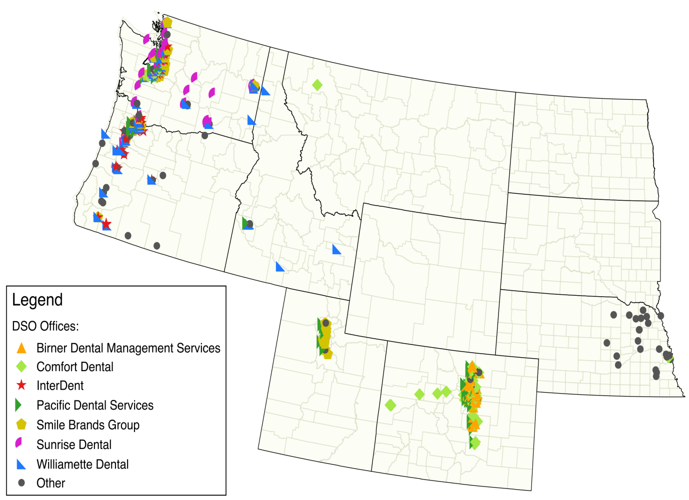
3.7 FIGURES

Figure 11: DSO Locations in 2005



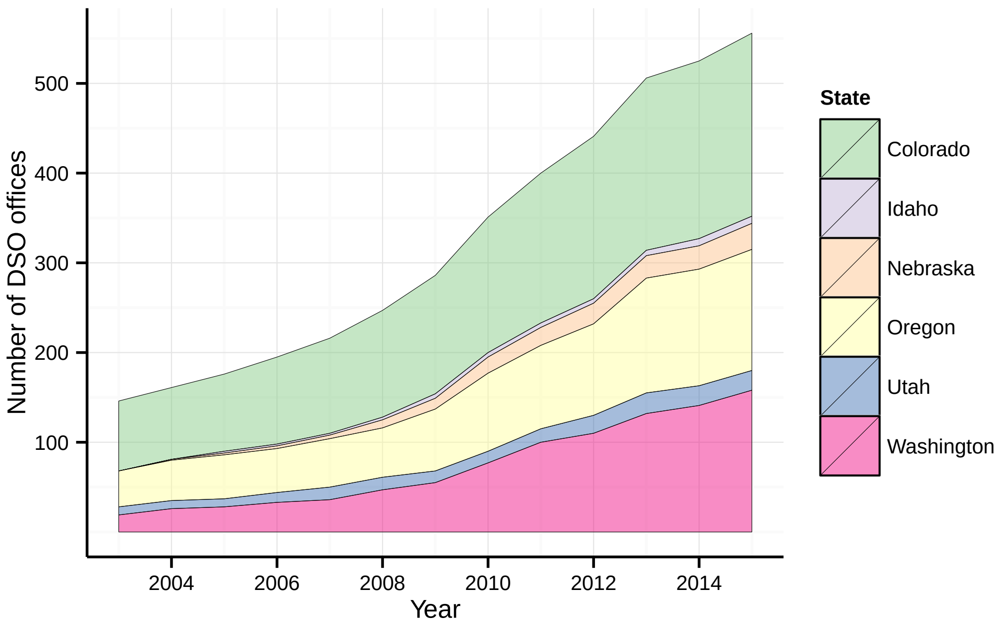
NOTE: In 2005, existing DSOs include: Birner Dental Management Services (39 offices), Comfort Dental (31), InterDent (34), Smile Brands Group (34), Williamette Dental (13). Other DSOs include Affordable Dentures (2), Aspen Dental, CSHM (2), DentalOne (6), Emergency Dental Care USA (1), Kaiser Permanente Dental (11), and Sunrise Dental (2).

Figure 12: DSO Locations in 2015



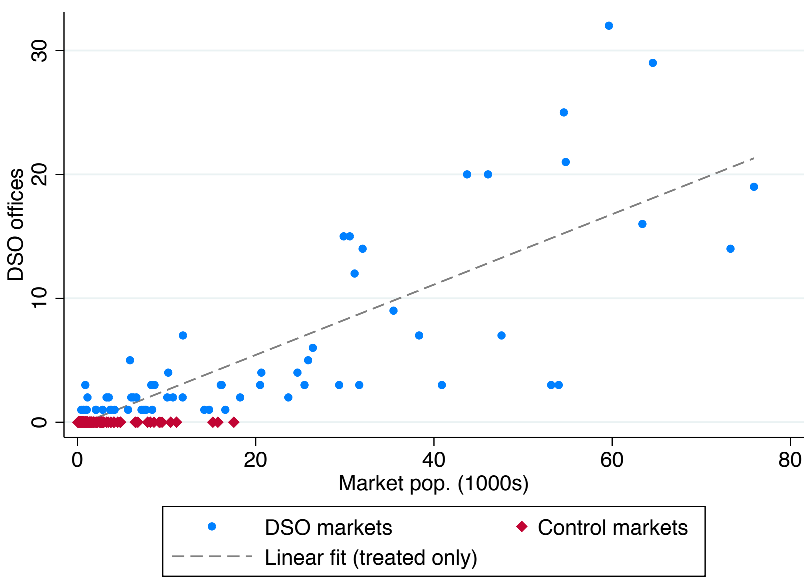
Note: In 2015, existing DSOs include: Birner Dental Management Services (47 offices), Comfort Dental (77), InterDent (71), Pacific Dental Services (68), Smile Brands Group (60), Sunrise Dental (45), and Williamette Dental (54). Other DSOs include Advantage Dental (14), Affordable Dentures (13), Aspen Dental (17), CSHM (4), ClearChoice (3), DentalOne (15), Emergency Dental Care USA (9), Family 1st Dental (17), Heartland Dental (4), Hero Practice Services (7), Kaiser Permanente Dental (16), Risas Dental (2), and Smiles Services (14).

Figure 13: Aggregate DSO Offices by State Across Time



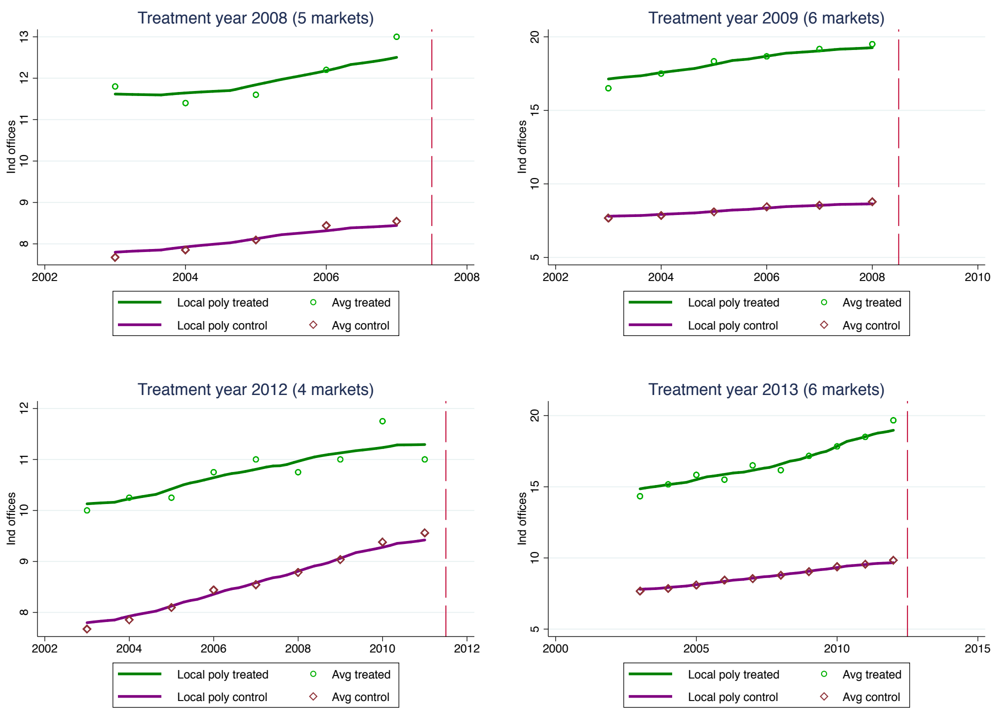
NOTE: The single DSO office in Montana, a Comfort Dental office that opened in 2015, is not included. Data ranges from 2003 to 2015.

Figure 14: DSO Offices Versus Market Population in 2012



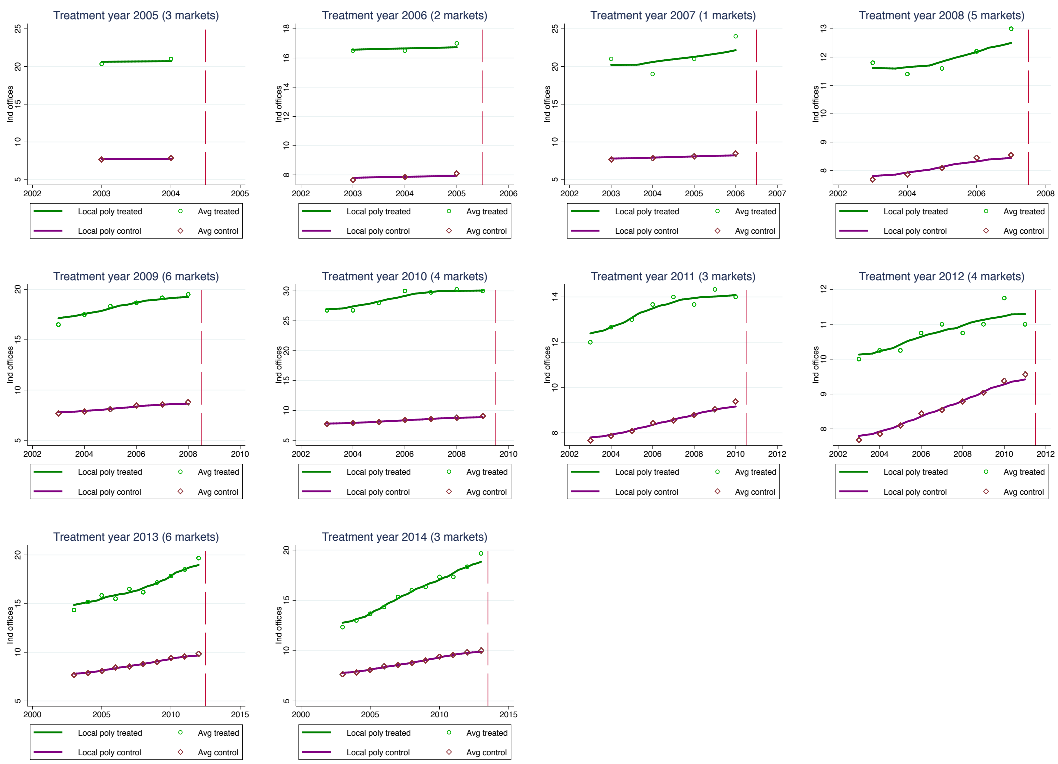
NOTE: Three outlier observations are not plotted for scaling purposes (King Co., WA; Salt Lake City Co., UT; Pierce Co., WA). Control markets are all markets located in SD, MT, ND, and WY and are shown here for illustrational purpose only.

Figure 15: Pre-Treatment by Year of DSO Entry, Four Largest Years



NOTE: These are the top-four years with the largest number of markets entered. The timing of treatment is defined to occur when the first DSO enters and is analogous of the timing used for continuous treatment. Local polynomial lines are drawn using an Epanechnikov kernel.

Figure 16: Pre-Treatment by Year of DSO Entry, All Years



NOTE: The timing of treatment is defined to occur when the first DSO enters and is analogous of the timing used for continuous treatment. Local polynomial lines are drawn using an Epanechnikov kernel.

3.8 TABLES

Table 12: Market Summary Statistics by Number of DSO Offices

	0 Offices		1-5 Offices		6-15 Offices		16+ Offices	
VARIABLES	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.
Pop. (10k)	1.839	3.052	16.74	17.18	49.40	33.90	70.62	43.38
Pop. % change	0.297	1.799	1.097	1.119	1.465	1.070	1.622	0.512
Pop. density (ppsm.)	16.26	48.56	220.6	371.7	669.8	871.7	931.0	920.5
Income p. capita	36.47	13.34	37.99	9.480	40.93	7.981	46.47	9.473
Proportion HPSA	0.456	0.498	0.471	0.500	0.243	0.430	0.0349	0.185
Proportion farmers	0.338	0.103	0.238	0.0654	0.229	0.0536	0.217	0.0366
Offices p. capita (10k)	3.328	2.131	4.054	1.063	3.886	0.763	4.321	0.645
Observations	5,822		495		136		86	
No. of markets	474		72		25		14	

NOTE: Population percent change is calculated on an annual basis. Proportion HPSA is the proportion of markets that have designation as dental Health Professional Shortage Areas. Proportion farmers is the proportion of jobs that are designate as farming or farmhand jobs. A market is defined at the county level. Data ranges from 2003 to 2015.

Table 13: Summary Statistics of Control and Treatment Groups for Binary Treatment

	Full		Control		Treated	
VARIABLES	mean	s.d.	mean	s.d.	mean	s.d.
Indep. offices	10.30	12.98	9.036	12.91	14.89	12.19
Pop. (10k)	2.742	3.169	2.395	3.109	4.002	3.070
Pop. growth (level)	292.4	639.6	284.2	659.3	322.4	562.1
Pop. density (ppsm.)	29.79	136.2	14.76	23.79	84.28	283.1
Income p. capita	38.57	14.76	39.25	15.43	36.09	11.73
Proportion HPSA	0.353	0.478	0.332	0.471	0.428	0.495
Offices p. capita (10k)	3.618	1.327	3.521	1.323	3.966	1.284
Avg. earnings	39,108	10,198	39,106	10,642	39,113	8,406
UE comp. p. capita	0.138	0.118	0.122	0.0933	0.195	0.169
Proportion farmers	0.289	0.0851	0.295	0.0857	0.269	0.0794
No. of markets	148		116		25	

NOTE: Proportion HPSA is the proportion of markets that have designation as dental Health Professional Shortage Areas. Proportion farmers is the proportion of jobs that are designate as farming or farmhand jobs. A market is defined at the county level. Data ranges from 2003 to 2015.

Table 14: OLS Binary Treatment

VARIABLES	(1)	(2)	(3)	(4)
TREAT	-0.808*	-0.650**	-0.663**	-0.696**
	(0.435)	(0.318)	(0.319)	(0.317)
Proportion HPSA		-0.111	-0.129	-0.119
		(0.203)	(0.199)	(0.202)
Pop. (10k)		5.549***	5.676***	5.741***
		(0.352)	(0.343)	(0.343)
Income p. capita		-0.0374***	-0.00951	-0.00941
		(0.0132)	(0.0148)	(0.0138)
Avg. earnings			-3.12e-05**	-2.87e-05*
			(1.54e-05)	(1.53e-05)
Pop. growth (level)			-0.000445**	-0.000505**
			(0.000191)	(0.000196)
UE comp. p. capita				-1.961***
				(0.697)
Proportion farmers				2.546
				(3.025)
Observations	1,924	1,924	1,924	1,924
No. of markets	148	148	148	148
R^2	0.212	0.633	0.642	0.646

NOTE: Robust standard errors clustered at the market level in parentheses; *** p<0.01, ** p<0.05, * p<0.1. All specifications include year and market fixed effects. Proportion HPSA is the proportion of markets that have designation as dental Health Professional Shortage Areas. Proportion farmers is the proportion of jobs that are designate as farming or farmhand jobs. A market is defined at the county level. Data ranges from 2003 to 2015.

Table 15: OLS Continuous Treatment

VARIABLES	(1)	(2)	(3)	(4)
TREAT	-1.144***	-0.536***	-0.509***	-0.555***
	(0.240)	(0.185)	(0.183)	(0.188)
Proportion HPSA		0.00226	-0.0570	-0.0536
		(0.203)	(0.196)	(0.198)
Pop. (10k)		5.571***	5.571***	5.617***
		(0.551)	(0.542)	(0.549)
Income p. capita		-0.0416***	-0.00664	-0.00617
		(0.0138)	(0.0139)	(0.0128)
Avg. earnings			-3.31e-05**	-3.01e-05**
			(1.51e-05)	(1.48e-05)
Pop. growth (level)			-0.000565**	-0.000619**
			(0.000229)	(0.000238)
UE comp. p. capita				-2.201***
				(0.717)
Proportion farmers				3.305
				(3.134)
Observations	2,106	2,106	2,106	2,106
No. of markets	162	162	162	162
R^2	0.251	0.665	0.677	0.681

NOTE: Robust standard errors clustered at the market level in parentheses; *** p<0.01, ** p<0.05, * p<0.1. All specifications include year and market fixed effects. Proportion HPSA is the proportion of markets that have designation as dental Health Professional Shortage Areas. Proportion farmers is the proportion of jobs that are designate as farming or farmhand jobs. A market is defined at the county level. Data ranges from 2003 to 2015.

Table 16: Smooth Pairwise Maximum Score Estimation

	(1)	(2)
VARIABLES		
Own	-20.96**	-3.786***
	(0.038)	(0.000)
Other	-68.37**	-12.37***
	(0.038)	(0.00)
Dist. First Entry	-5.460**	
	(0.040)	
Dist. HQ		-0.217*
		(0.050)
Firm FE	X	X
Market FE	X	X
Observations	5,000	5,000
Unique offices	555	555
SMS	0.8933	0.8911

NOTE: Bootstrapped p-values in parentheses, calculated from 400 bootstrap samples; *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Specification (1) normalize the coefficient on Distance HQ to -1. Specification (2) alternatively normalizes the coefficient on Distance First Entry to -1. A market is defined at the county level. Data is from 2015.

Table 17: Market Summary Statistics, Full Data Set

VARIABLES	mean	s.d.	min.	med.	max.
Pop. (10k)	4.861	13.99	0.0428	0.926	211.7
Pop. % change	0.399	1.759	-8.202	0.340	18.62
Pop. density (ppsm.)	57.36	246.2	0.254	7.032	4,461
Proportion HPSA	0.447	0.497	0	0	1
No. of DSOs	0.288	1.054	0	0	10
DSO offices	0.643	3.031	0	0	44
Indep. offices	18.49	58.63	0	3	1,049
Offices p. capita (10k)	3.408	2.049	0	3.474	21.65
Income per capita	36.81	13.02	11.75	34.19	195.6
Earnings per worker	38,151	11,632	7,071	36,382	164,447
UE benefits per capita	0.146	0.128	0.00737	0.103	1.204
Proportion farmers	0.326	0.105	0.0576	0.321	0.695

NOTE: $n = 6,539$; population percent change is calculated on an annual basis. Proportion HPSA is the proportion of markets that have designation as dental Health Professional Shortage Areas. "Independent" offices include all offices that are not associated with an identified DSO. Proportion farmers is the proportion of jobs that are designate as farming or farmhand jobs. A market is defined at the county level. Data ranges from 2003 to 2015.

Table 18: Summary Statistics by DSO Office, Full Sample, All DSOs

VARIABLES	mean	s.d.	min.	med.	max.
Pop. (10k)	60.15	50.47	0.415	49.13	211.7
Pop. density (ppsm.)	715.2	863.7	0.962	439.7	4,461
Income per capita	49.47	10.73	33.96	46.05	73.11
No. of DSOs	6.088	2.534	1	7	10
Indep. offices	252.9	252.1	0	216	1,049
Dist. HQ	804,090	861,188	920.3	565,742	3.841e+06
Dist. First Entry	38,728	69,380	390.5	9,018	408,459
Alt. Dist. First Entry	4,403	8,120	30.04	996.5	70,615

NOTE: n = 555; "independent" offices include all offices that are not associated with an identified DSO. Distances are in kilometers and are calculated from the center of population of the market the DSO office is located in. First entries are the first DSO office opened in that state. "Alternative Distance First Entry" discounts this distance by the number of years ago that DSOs first entry was made. A market is defined at the county level. Data is from 2015 only.

4.0 STATE CAMPAIGN CONTRIBUTIONS FROM SHALE GAS DEVELOPMENT

4.1 INTRODUCTION

At all levels of government in the United States, organized corporate interests spend great resources to influence politicians and policies. In addition to being an important political issue, campaign finance and lobbying have been the focus of a large volume of both theoretical and empirical research. Taking advantage of the opportunity of a controversial fast-growing industry only regulated at the state level, this essay looks at the campaign contributions to state candidates and state lobbying expenditures from unconventional natural gas operators in the Marcellus Formation in Pennsylvania. At almost the same time as the introduction and expansion of the shale gas industry, changes in Pennsylvania lobbying regulations required the reporting of all expenditures on lobbying state politicians. This unique opportunity allows an empirical examination of state campaign finance and lobbying.

Using novel data, I analyze the trends and strategies of these unconventional natural gas operators to influence state government and policies. Lobbying expenditures seem to be increasing rapidly up to the state legislature's passing of a new law regulating unconventional natural gas development. Campaign contributions are small relative to lobbying expenditures and total contributions raised. I find observational results that do not support two commonly assumed frameworks of campaign contributions. Potential explanations and policy implications are discussed as a result.

Of particular interest to the literature, and this essay, is contributions from political action committees (PACs), specifically those formed by corporations. Campaign donations from individuals seem to have a natural grounding, often explained as being a form of "political consumption," another option outside of voting. Companies, however, do not have the right to vote, and so society and the literature have extensively questioned and studied campaign contributions from PACs. In a crude classification, the explanation for why PACs give campaign contributions can be broken into two broad groups: directly buying legislative votes or policy favors and buying access to politicians for lobbying. This essay contributes additional evidence to the discord found in the campaign finance literature from a previously unstudied setting. It should also be noted that there are a variety of evidence and theories explaining why candidates raise and spend so much money,[1] but that this essay focuses on the PAC side of the contribution process.

A variety of studies present evidence that contributions do indeed buy votes and favors. Stratmann (2002) looks at how changes in contributions from financial PACs affected U.S. Representatives' roll call votes on financial legislation. He finds that contributions are effective in switching legislators votes, especially for more junior representatives. Gordon, Hafer, and Landa (2007) exploit variation in executive performance pay and find that campaign contributions from corporate executives appear to be political investments.

Another popular belief is that corporate campaign contributions gain access for lobbying: a PAC donates to a candidate's campaign to show support and possibly help him or her win (or to pay an implicit cost of lobbying). Then if the candidate is elected, he or she makes time out of their limited schedule to meet and listen to the corporation's lobbyists. The elected candidate, now a politician, has more lobbyists who want to meet with him or her than physically possible, and so he or she uses campaign contributions as means of allocating this scarce resource. Many papers in political economy have modeled campaign contributions in this or similar ways, such that contributions "gain access" [see Austen-Smith (1995), Austen-Smith (1998), and Cotton (2012)]. Alternatively, Hall and Deardorff (2006) argue and model lobbying as a legislative subsidy instead of providing information or buying votes; they conjecture that if you add contributions into their model "[contribution] money buys access only to ones allies." Empirical evidence for access includes Humphries (1991), who shows that at the federal level there is a clear "organizational link" and strategy behind PACs and lobbying and is able to characterize which type of firms have PACs. There is also

[1] See Stratmann (2005) for a brief survey.

evidence that there is a reward from lobbying. Kang (2016) develops and estimates a game-theoretic model of lobbying. Using data from 2007-2008 of the U.S. Congress and lobbying expenditures from the energy industry, she finds that while lobbying efforts have a low probability of changing policy and often are canceled out by efforts from other lobbyists, the expected returns from lobbing are substantial and worthwhile. Unfortunately for this essay the coarseness of the lobbying data in Pennsylvania severely constrains any investigation of theories on lobbying.

Worth mentioning is an alternative view that special interests and corporations form PACs to participate in the political process for reasons similar to the motivations of many individuals. Ansolabehere, de Figueiredo, and Jr. (2003) argue that campaign contributions are a form of consumption or political participation, not a type of investment, providing empirical evidence on PACs from both extended replications of a variety of papers and original analysis. Bronars and Lott (1997) look at contributions to members of the House of Representatives during their last term and find that contributions have no effect on how Representatives vote on legislation. They argue that this is evidence that contributions follow "ideological sorting," groups give to politicians who already support their ideology.

In addition, campaign finance has mostly been studied at the federal level. Stratmann (2006), one of the few studies looking at state campaign contributions, looks at the returns and effectiveness of campaign contributions of winning votes, exploiting variation in state campaign finance laws. Hogan (2012) finds a positively correlated relationship between campaign spending and voter turnout in state legislative elections. Drazen, Limão, and Stratmann (2007) also uses variation in state campaign finance laws to show that caps on contributions can increase the incentives for additional special interests to form and give donations, worsening the problems associated with special interests. This essay also contributes to understanding how lobbying and campaign finance operate at the state level. One may be concerned, that compared to similar analysis at the federal level, studying only one particular state may be less general, but in this situation it also provides a "cleaner" lens to view the political landscape, since there is often less independent campaign spending and convoluted special interests at the state level.

The rest of this essay is organized as follows: Section 4.2 outlines and explains the institutional backgrounds and laws in Pennsylvania; Section 4.3 briefly describe data sources and summarizes the main datasets; Section 4.4 examines two frameworks of political giving and provides observational results of whether there is any evidence of these framework in Pennsylvania state politics; Section 4.5 concludes and discusses policy implications for regulating campaign contributions and lobby. Figures, Tables, and additional technical details are presented in respective Appendices.

4.2 BACKGROUND

The Pennsylvania Department of Environmental Protection (PA DEP) is the primary regulatory body in charge of regulating upstream unconventional natural gas development and enforcing state laws and regulations. An unconventional well is defined by the PA DEP as a "well that is drilled into an unconventional formation, which is defined as a geologic shale formation below the base of the Elk Sandstone or its geologic equivalent where natural gas generally cannot be produced except by horizontal or vertical well bores stimulated by hydraulic fracturing," Office of Oil & Gas Management (2014). Upstream development is typically defined as the exploration, discovery, drilling, and extracting phases of natural gas development (where as midstream is transportation and refinement and downstream is delivery and service to end consumers). Act 13 of 2012 amended Title 58 Oil and Gas of the Pennsylvania Consolidated Statutes, the governing laws on oil and gas development. Signed into law on February 14, 2012, Act 13 changed much of the regulations for unconventional drilling and extracting and is believed to be the main focus of lobby efforts from shale gas developers.

The commonwealth of Pennsylvania has a bicameral state legislature, the Pennsylvania General Assembly, which consists of an upper chamber, the Senate, and a lower chamber, the House of Representatives. The state Senate consists of fifty seats with one Senator being elected from each of the state senatorial districts. Senators are elected for four year terms, which are scheduled such that even and odd districts are staggered across election years. The state House of Representatives consists of 203 seats also elected from single member districts. Representatives serve two year terms, and thus every district has an election for the state House every state election cycle (every even calendar year). The General Assembly is a full-time state legislature.

While all members of the General Assembly played a role is passing Act 13 of 2012, certain members had a larger influence. Both the Senate Environmental Resources and Energy Committee, a standing committee of ten members, and the House Environmental Resources and Energy Committee, a standing committee of fifteen members of the majority party and ten members of the minority party (Pennsylvania General Assembly (2013a) and Pennsylvania General Assembly (2013b)), drafted, amended, and led discussions on the bill. Other state positions of interests are elected statewide, including Governor, State Treasure, Attorney General, and Auditor General. Each of these positions are elected every four years.

Campaign finance law is enforced by the Attorney General and District Attorneys and is partially regulated by the Pennsylvania State Ethics Commission. However, records and reports are collected, maintained, and published by the Pennsylvania Department of State (PA DOS), Bureau of Commissions, Elections and Legislation. The Pennsylvania Election Code (25 P.S. 3241-2360b) requires all candidates and committees to submit a series of financial statements and expense reports. Reporting follows an annual seven-cycle calendar with the timing of cycles tied to primary and general election dates. All candidates and active committees must submit at least an annual/cycle 7 expense report, regardless of their activity. If a candidate or committee has less than $250 in aggregate contributions and expenses in a given cycle, they may submit a one paged "financial statement" which is not published, Pennsylvania Department of State (2010).

Corporations cannot give directly to Pennsylvania state candidates or candidate committees;[2] however, they are allowed to set up and administer state political action committees. Corporations cannot also give direct contributions to a PAC but can give money to pay for any administrative costs in addition to administering and deciding PAC spending and strategies. PACs are allowed to give unlimited contributions (in amount and frequency) to

[2] This is true for the data used in this essay, but has since changed. Following *Citizens United v. Federal Election Commission* (2010), the Pennsylvania campaign finance regulations were challenged in court, resulting in the Preliminary Injunction Order in *General Majority PAC v. Carol Aichele, et al.* (March 10, 2014) which allows direct contributions from corporations under certain conditions.

state candidates or candidate committees.

The Lobbying Disclosure Law, Act 134 of 2006, required for the first time disclosure of all lobbying of state officials and politicians. 65 Pa.C.S. 13A and 51 Pa. Code 51.1-51.3 require any party that spends money to lobby state government or employees to register as a principal and report quarterly expense reports while actively registered. “Lobbying” is defined in Section 13A05(b)(2.1) as any “effort to influence legislative action or administrative action in the Commonwealth.

4.3 DATA

4.3.1 Data Sources

The dataset used in this project is a compilation from ten different data sources. In Appendix C.1, I provide detailed steps on how I constructed this dataset, and for interested readers, I have also provided in Appendix C.6 an unabridged reference list of all sources. The time range of the dataset ranges from 2006 to 2013. Since state elections only occur on even year in Pennsylvania, all of the analysis on campaign contributions only uses data up to 2012. The longitudinal unit varies across data sources, ranging from a quarter level and to a two-year election-year level.

Data on natural gas wells and drilling was obtained from the Pennsylvania Department of Environmental Protection. Specifically from the PA DEP Oil & Gas Reporting website I obtained reports and data on individual well permits, individual SPUD drilling dates and information, and statewide reports on all active wells. Lobbying data is from the Pennsylvania Lobbying Disclosure Registration (LDR) database, the official and exhaustive database for all state lobbying reporting. Campaign finance data was obtained from a variety of sources including the PA DOS Campaign Finance website, the PA DOS Election Information website, and the National Institute on Money in State Politics’ “Follow the Money” database. Data on state candidates and elections is also from a variety of sources including PA DEP State Election Information website and Election Results website. In addition redistricting information and data was obtained from the PA Spatial Data Access database and the PA General Assembly Legislative Reapportionment Commission website. Lastly, committee membership data was obtained from PA House of Representatives records and PA State Senate records. Appendix C describes all of these data sources in greater detail.

4.3.2 Data Summary

The main datasets used throughout this essay is only from sixteen companies identified as the "politically active" unconventional upstream natural gas operators in Pennsylvania.[3] By construction these sixteen companies make up 100% of all lobbying expenditures and campaign contributions for shale gas operators. Between 2006 and 2013, these companies account for 79.7% of all filed unconventional gas permits and 76.4% of all unconventional gas wells drilled. Since disclosure of state lobbying expenditures was not required prior to 2007, it is difficult to understand the natural pattern of lobbying from these companies. It is reasonable to assume that prior to the shale gas boom, many of these companies had less interest in lobbying state officials, but this can not be confirmed empirically.

The amount of lobbying and campaign contributions suddenly increased from these companies at the exact time the shale gas industry started booming. Figure 17 shows aggregated quarterly totals for reported lobbying expenses, campaign contributions, well permits, and wells drilled for these sixteen companies. On both an aggregate level and for each company, much more money is spent on lobbying than campaign contributions. Looking at summary statistics presented in Table 19, it seems that lobbying is the main strategy these companies employ to influence the state government. This may be due to the fact that organizing and raising money for a PAC (which cannot receive direct contributions from corporations during this time period) is more difficult than directly spending money on a lobbying firm. However, this could also be reflecting the rather low cost of running a state campaign.[4] Naturally, both lobbying expenses and campaign contributions are dramatically lower than those seen

[3]See Appendix C.1 for how these companies we identified and how the datasets were constructed.

[4]From 2006 to 2013, the average PA state House candidate raised $92,569 each election, while the average state Senatorial candidate raised $408,822. Compare this to the average U.S. House candidate raising $649,694 and the average U.S Senatorial candidate $2,785,167 for the 2012 election season (Center for Responsive Politics, 2014).

at the federal level, but it is reasonable to believe that campaign costs and lobbying costs do not scale down proportionally from the federal to state level.

The majority of campaign contributions from these companies were made to state House candidates (46% of total contributions; 60% of the number of contributions with an average contribution per election of \$721) and state Senatorial candidates (35% of total contributions; 32% of the number of contributions with an average contribution of \$1,005). Contributions to candidates for Governor and Lieutenant Governor made up 12% of total contributions, while contributions for Attorney General, Auditor General, and State Treasure candidates combined only made up 6% of total contributions. Contributions for these non-legislative candidates were on average larger than those for Senators and Representatives, per election. The average donation to Governor and Lieutenant Governor candidates each election was \$3,039 (\$2,128 for Attorney General; \$2,571 for State Treasure). More Republican candidates received contributions than Democrats (65% to 35%), and Republicans received more in aggregate than Democrats (78% to 22% of total contributions from these companies). Not a single third party or independent candidate received any campaign contributions from these companies.

While state lobbying registration and disclosure are mandatory, organizations and lobbyists are only required to break down lobbying expenditures into three imprecisely-defined categories. Disclosure of which officials are lobbied and on what issues and legislation is not required. This limits the ability to connect lobbying efforts and campaign contributions beyond temporal and company association. Figure 18 show the aggregated patters of these types of expenditure across time. The main and dynamic component of lobbying expenditure seems to be "direct communication."

Regardless of the disparity in magnitudes of lobbying expenditures and campaign contributions, it is interesting to study why these companies are establishing PACs and how they are directing these PACs to support and give to candidates. To do so it is useful to analyze this spending in multiple perspectives, using different lens of the political economy literature.

4.4 EMPIRICAL EVIDENCE

To understand the why these companies are giving to state candidates it is necessary to examine campaign contributions in competing frameworks. In the following subsections, I present two motivating frameworks and corresponding observational results.

4.4.1 Buying Policy

4.4.1.1 Framework A common and default presumption of campaign contributions is that they directly influence policy outcomes. In this framework, campaign contributions are thought of as "political investments." This type of campaign spending is often assumed to play out through various mechanisms. One such being that PACs give contributions to influence election outcomes, giving to candidates they prefer to be elected. Many political economy models, particularly those with informative campaigning, assume some variation of this mechanism, e.g. Coate (2004a,b); Schultz (2007). An alternative mechanism is that the contributions are intended to influence candidates actions after the election, serving as what many may call "legal bribes."

To directly influence policies, both of these mechanisms require the successful election of the candidate receiving the contributions. Moreover, to be an optimal strategy for the PAC, the contributions must be able to help the political candidate get elected or convince the winning candidate to take action on behalf of the donor. As a result close contested elections should see more corporate contributions than less-contested elections. It is unclear how contributions should look for extremely one-sided races and races with candidates running unopposed. Along one dimension, contributions may be less since candidates are under no threat in the election and hence need less funds for campaigning. Along the other dimension one may expect to see contributions relatively large in magnitude, since contributions must convince the candidate to act solely on their magnitude and not from the additional help in winning the election.

Candidates can always reject campaign contributions. Since all state campaign contributions are public record, this is more likely if the donation is coming from a corporate PAC that may have contentious public relations among voters, such as shale gas companies in Pennsylvania. Thus perhaps the political investment framework is only applicable to the subset of politicians sympathetic to the industry the corporate PAC belongs to.

A final consideration is that while contributions from any given company may be too small in magnitude to influence election outcomes or political actions, the total contributions from all companies in a particular industry or grouping may be influential. This of course creates a host of free-riding incentives that would likely decrease contributions from individual corporate PACs, but the overall giving could still be large enough to buy policy favorable to the industry.

4.4.1.2 Estimation To understand why these companies are forming PACs and giving campaign contributions, I employ panel fixed-effect ordinary least squares (OLS) regressions using a variety of related explanatory variables. In this section, all official candidates are considered even if they received zero contributions from these companies or received relatively low vote shares; hence observations are company per candidate per general election. This allows a general view of to whom and why these company are giving. To do so I use a general specification presented in Equation 4.1,

$$y_{i,c,t} = \beta_0 + \beta X_{i,c,t} + \delta_i + \eta_c + \theta_t + \epsilon_{i,c,t} \tag{4.1}$$

Here $y_{i,c,t}$ is the total campaign contributions from company c to candidate i in election t and $X_{i,c,t}$ is composed of various variables on the candidates, companies, and elections. The included fixed effects vary across samples and specifications. Standard errors are clustered at the company-office level, meaning each company and particular office position form a group[5] and are estimated with the Huber-White sandwich variance-covariance estimator.

[5] For example, all contributions from company c to all candidates running for a particular office, e.g. Pennsylvania Senator District 43, across all elections make up a single cluster group; this clustering results in 2,570 cluster groups.

4.4.1.3 Results Summary statistics of campaign contributions from 2006 to 2013 show that 89.5% of the candidates receiving contributions from these companies were incumbents, and 95.0% of them ended up wining their election. 79.7% of the candidates ran unopposed in the primary, and even 40.0% ran unopposed in the general election. These statistics suggest that these companies are giving to the favored candidates and not necessarily attempting to change the election outcomes.

Tables 20 and 21 presents various specifications using different election variables. Column (1), in both tables, presents results on all candidates who ran in both the primary and general elections and shows that primary-election variables behave as one would expect if contributions bought policy via influencing election outcomes: running unopposed in the primary and having more primary votes, likely indicating a heavily-favored candidate, is associated with less contributions. However, in column (1) and in other specifications, general-election variables are either insignificant or often have the opposite relationship with contributions one would expect for a policy-shopping story. Column (2), again in both tables, presents similar results on all general-election candidates. Columns (1) and (2) reiterate that incumbency is important, even when controlling for additional factors. Thus column (3) only uses incumbent candidates and uses a individual-candidate fixed effect to control for the same individual appearing in multiple elections across time. Many coefficients are in the expected direction but not statistically significant. Finally column (4) runs the specification and sample used in column (2) but with company-office fixed effects. In both tables, this specification results in incumbency having a positive coefficient but losing significance. Worth noting is that in Table 21, is the strong positive relationship between running unopposed in the general election and contributions from these companies. This positive but relatively-small effect is in conflict with policy-buying story of campaign contributions.

I also focus on contributions from only General Assembly candidates in order to take into account the spatial aspects of the political offices these contributions are going to. Figure 19 shows the spatial distribution of the districts of the state Senatorial candidates receiving contributions as well as the spatial distribution of well permits and wells drilled, while Figure 20 shows the same for state House candidates. This clear spatial pattern is interesting. In a policy-buying framework, the legislative candidates should be treated equally across districts since legislative votes are equal in the General Assembly. Hence, this pattern suggests legislative votes on bills are not the desired investment return of contributions. A potential irregularity to this could be candidates who serve on their chamber's Environmental Resources and Energy (ERE) committee may be consider more valuable than other legislative candidates and hence could be explaining the uneven distribution of contributions across legislative districts.

Tables 22 and 23 examine these relationships in specifications of the same respective ordering to those presented in Tables 20 and 21. In both Tables 22 and 23 only General Assembly candidates from the major parties are used, which allows a cleaner picture and the inclusion of company drilling variables at the district level. As discovered before, contributions are highly correlated with candidates being Republican incumbents. In columns (1) and (3), in both tables, running unopposed in the primary and having a larger primary vote share decrease contributions, but coefficients across specifications are almost always insignificant. Being on an ERE committee appears to be positively correlated with contributions, adding about $300 from each company.[6] This may suggest that to some degree these contributions do influence policy but through influencing the drafting and creation of bills instead of buying votes to pass bills. Alternatively, company drilling variables are all significantly-positively related with contributions. A company's existing stock of wells and current flow of well permits seem to explain a large portion of these contributions, suggesting that companies are giving to candidates that represent districts where they have their economic assets. The ERE members do tend to come from districts with heavy shale-gas activity, and thus the association with ERE members may be instead caused by spatial business ties.

The amount of contributions these companies are giving to candidates is relatively small. As noted before, from 2006-2012 General Assembly House candidates raised on average $92,569 per each election, while Senatorial candidate raised on average $408,822. Summing up contributions from all of these companies, conditional on receiving at least one donation from a shale gas operator, House candidates received an average of $2,168 in total from shale gas operators each election and Senatorial candidates only received and average total

[6] On the incumbent sample in Column (3) the ERE coefficient is negative; this is most likely caused by the included candidate fixed effect, since over this time period the members of the two ERE committees were relatively stable.

of \$7,770. Figure 21 shows the percentage of this aggregated "industry contribution" to total campaign contributions for General Assembly candidates, again conditional on receiving at least one donation from these companies.[7] The median of these candidates only received 0.7% of their total contributions from the shale gas industry, and the candidate who received the most funding from the shale gas industry in a single election only received 36.2% of their funding from the industry.

Hence, there does not seem to be evidence that these contributions are serving as political investments buying policy. Many political variables are not statistically significant in the proper directions and magnitudes to suggest contributions are going to candidates to influence election outcomes. Total contributions from this industry also appear to be small in relative magnitude, suggesting that these contributions may not be enough to solicit candidates to act on the companies' behalf after the election. Moreover, existing and proposed economic assets seem to be strongly correlated with how these contributions are spatially distributed across legislative districts.

4.4.2 Buying Access to Lobbying

4.4.2.1 Framework An alternative framework is that campaign contributions buy access to candidates. Specifically contributions serve as a cost or fee that allows candidates to decided who to allow access to or listen to during lobbying. This general setup is common among political economy models, although the literature varies greatly in the additional theoretical structure. For example, Bennedsen and Feldmann (2006) develop a model in which contributions and lobbying "messages" are sent at the same time. Contributions provide information about the interest groups information, which is designed around the assumption that lobbying provides useful information to policy makers. Cotton (2009) develops a theoretical model that bridges both frameworks, modeling candidates choosing to sell policy favors or access for campaign contributions. He is able to characterize when a candidate sells favors versus access, finding that if an issue is of great enough importance, a candidate will

[7] The candidate total contribution data is from the National Institute on Money in State Politics, and hence due to missing data not all candidates used in other analyses are included.

sell access to gain better information from lobbying.[8] Lohmann (1995) develops a model potentially relevant to this essay's setting, where groups with similar ideology have free access to lobbying but ideologically-opposed groups must give contributions to persuade politicians to take their information into consideration.

In all of these models though, there must exist a direct link between PAC contributions and corporate lobbying. This framework assumes campaign contributions and lobbying act as compliment goods instead of being alternative strategies of influencing government. In addition, the natural timing of the elections and lobbying is such that candidates raise campaign contributions during the election and then once elected "open" their doors to lobbyists. Hence one would expect to see some temporal link between a company's contributions and lobbying expenditures.

4.4.2.2 Estimation In order to use the state lobbying data, I aggregated the campaign finance data across candidates to the company-quarter level.[9] To discover any temporal links between contributions and lobbying I employ panel fixed-effect OLS regressions defined by Equation 4.2,

$$y_{c,t} = \alpha + \sum_{j=0}^{6} \beta_j x_{c,t-j} + \gamma Z_{c,t} + \eta_c + \theta_t + \epsilon_{i,c,t} \tag{4.2}$$

Here $y_{c,t}$ is the total lobbying expenditures from company c during quarter t, $x_{c,t}$ is company c's total campaign contributions during quarter t, and $Z_{c,t}$ are various drilling related variables. Since there are only sixteen companies, I estimate the standard errors by cluster bootstrapping at the company level with 400 replications. All of these results are not sensitive to using at least five different random seeds and to using a wild bootstrap with 200 replications.

In addition for alternative specifications I employ a first-order-autoregressive, AR(1), fixed-effect estimator, using a specification identical to Equation 4.2 except for omitting the

[8] This result is coming again coming from the assumption that interest groups have better information about a particular issue.

[9] This data limitation unfortunately prevents any analysis on individual candidates, but as mentioned earlier this is the only state lobbying data available for Pennsylvania.

quarter fixed effects. Reported ρ statistics are calculated using the corresponding Durbin-Watson statistics.

4.4.2.3 Results A crude first pass can be made at looking at lobbying immediately following when a company initially starts giving contributions. Figure 22 show local linear kernel density estimates of the three categories of lobbying expenditures, aligned relative to a company's first observed campaign contribution. Recall, from Figure 18, that direct communication is the major and driving category. Here with the local linear estimates, it appears to be increasing up to two years before the initial contribution and has no noticeable change in trend within two years post. This prior spending seems to discredit the access framework, but potentially could be some "boots on the ground" spending, setting up offices and a presence before the main lobbying effort. It is also worth noting the peak occurring in direct communication expenses approximately 14 quarters afterwards, but this may be caused by the peak immediately following the passage of Act 13 since multiple large companies had their initial contribution during the same quarter approximately at that time.

Table 24 presents results that more formally test this link between contributions and lobbying. Column (1) shows lagged contributions have a positive but mostly insignificant relationship with lobbying expenditures. Column (2) adds company drilling variables, which wipe out all significance of the contribution coefficients. Current number of well permits and a company's existing stock of wells, prior to the start of the quarter, seem to be strong indicators of the timing of lobbying expenditures. Columns (3) and (4) present the same specifications as (1) and (2), respectively, but using a within fixed-effect estimator with AR(1) modeled errors. Here contribution coefficients are mixed, while number of well permits and a company's existing stock of wells are again positive and significant.

Overall this suggest there is limited or no evidence that these contributions are serving to gain access for lobbying. This lack of evidence could be driven by the relatively small time window being used, 2006 to 2013, since elections only occur every two or four years. More generally though, these findings by no means suggest that these companies are not influencing state officials and policies. On the contrary, summary statistics presented in this essay suggest that these companies are spending a large amount of money to lobby state officials. These results simply suggest that there do not appear to be any conclusive evidence that these campaign contributions are being strategically used in some of the standard ways assumed in the political economy literature.

4.5 CONCLUSION

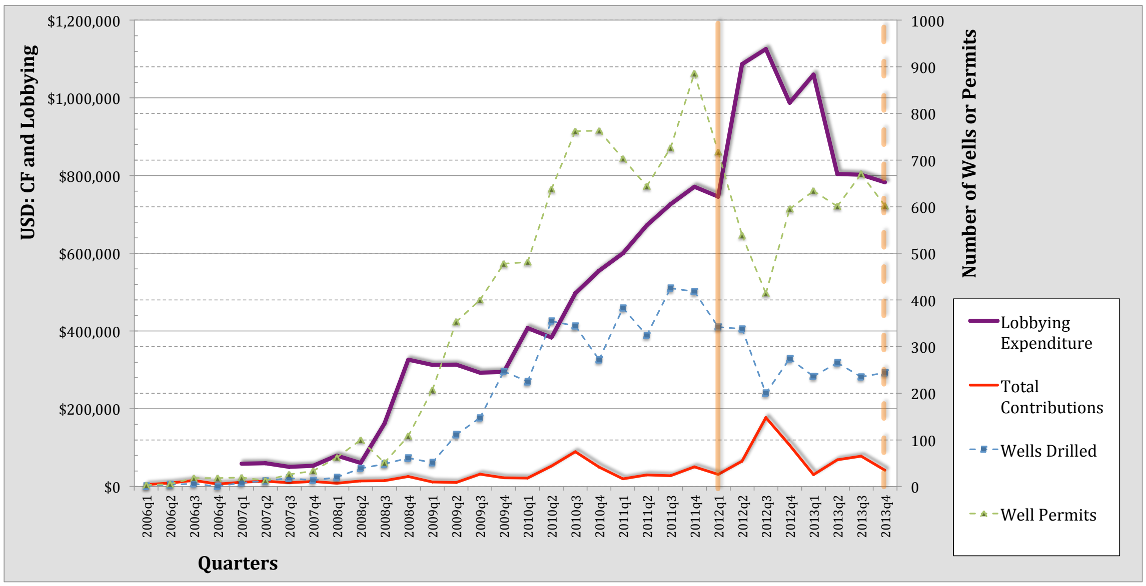
The unique opportunity of the rise of the shale gas industry allows me to look at a meaningful period of Pennsylvania state politics. After looking at how unconventional upstream natural gas operators donate campaign contributions and spend money lobbying, I find that the main channel of influencing the state government is through lobbying. After examining multiple frameworks to explain how these companies are strategically giving contributions, I find that contributions are generally not associated with election competition, and that these contributions, even when aggregated across all companies in the shale gas industry, form a relatively small percentage of each candidate's total contributions. Hence it does not appear that the these contributions are buying legislative votes or directly buying state policy. Moreover, I find no evidence of a lagged temporal link between campaign contributions and lobbying expenditures, suggesting contributions are not being used to gain access for lobbying.

There does, however, appear to be correlation among campaign contributions and drilling activity. A potential explanation for for this essay finding a lack of evidence for campaign contributions buying policy or access may be that contributions are determined by the location of a firm's economic assets and activity. Contributions could be serving as bribes for local political influence and favors or as public signals to win over local populations. These unusual reasons for giving warrant future research on state and local elections.

An alternative potential explanation may be the disparity in the mandated disclosure levels between Pennsylvania campaign contributions and lobbying expenditures. Perhaps these companies spend more money on lobbying since the required disclosure is very general, requiring no details on the how the money was spent besides breaking each quarterly aggregated expense into three broad categories. Companies are not required to disclose which public officials they communicated and interacted with; nor are they required to disclose which policies or legislation they were targeting. Campaign contributions from PACs, on the other hand, involve a long paper trail including the both the committee's and candidate's campaign finance reports. This puts forward an important policy implication. In order to accurately monitor the strategies and actions of corporations, lobbying and campaign contributions need to both have the similar levels of regulation and mandated disclosure. If only one of the two require detailed disclosure, corporations may shift their strategy to the other to avoid public disclosure.

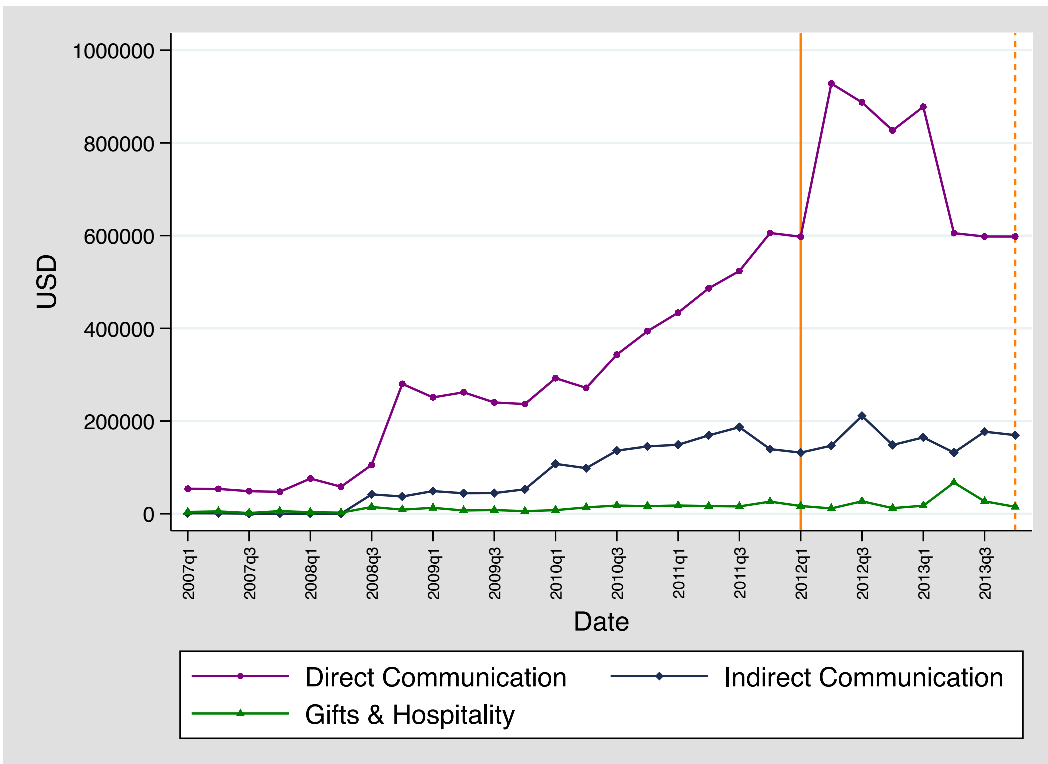
4.6 FIGURES

Figure 17: Aggregated Lobbying Expenditures, Campaign Contributions, Well Permits, and Wells Drilled



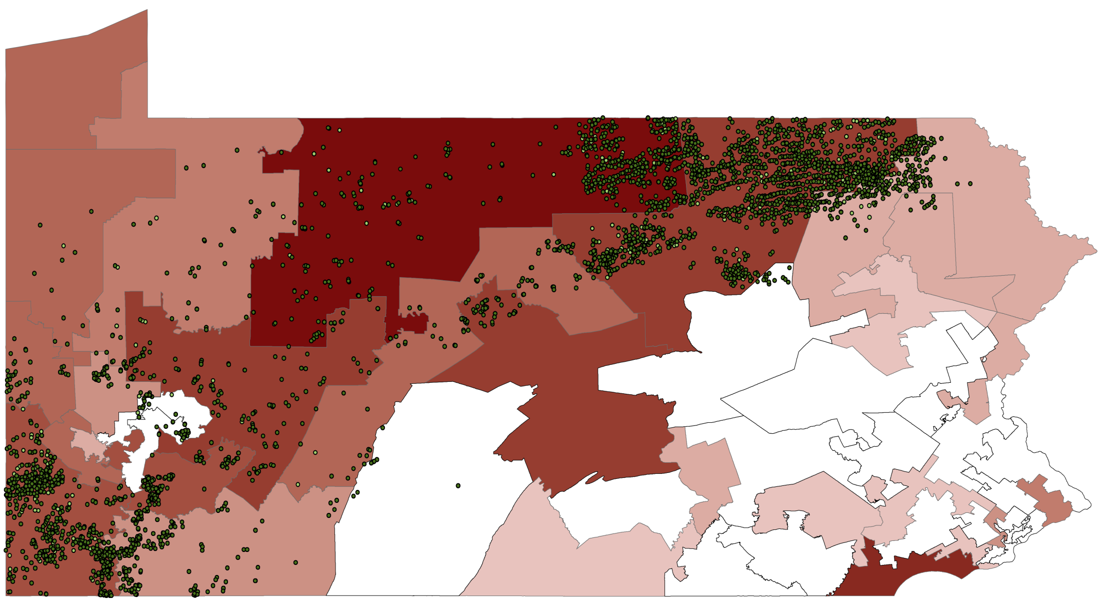
Note: The sold orange vertical line marks when Act 13 was signed as law. The dotted orange vertical line marks when parts of Act 13 were determined to be unconstitutional by the PA Supreme Court. Aggregation is across the sixteen companies identified and studied in this essay. Lobbying expenditures were not reported prior to quarter 1 of 2007, and hence the lack of values for lobbying for 2006 are missing data.

Figure 18: Aggregated Lobbying Expenditures by Type



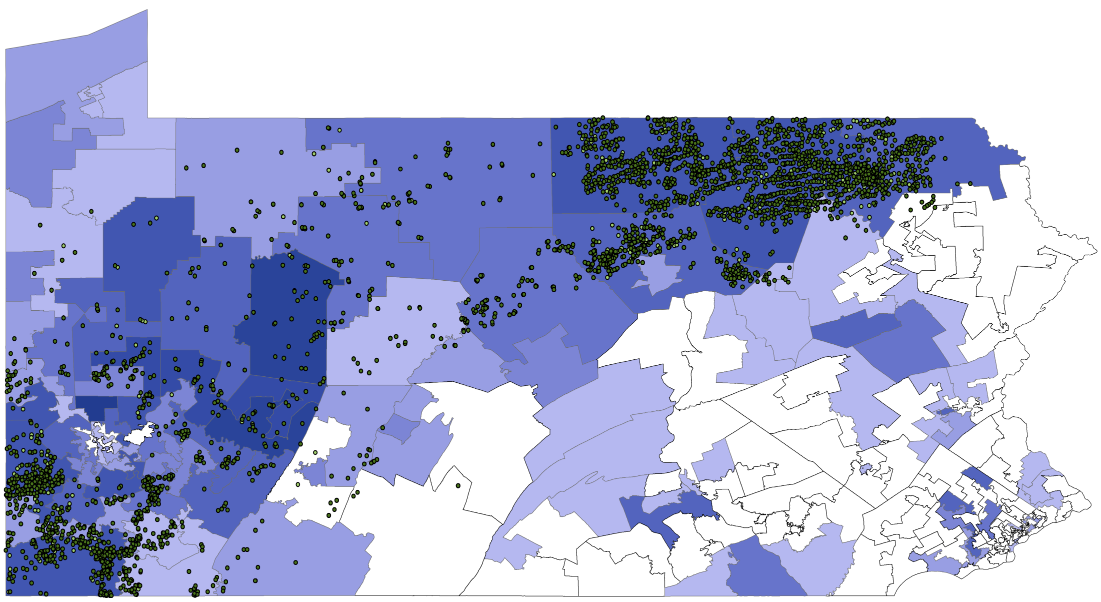
NOTE: The sold orange vertical line marks when Act 13 was signed as law. The dotted orange vertical line marks when parts of Act 13 were determined to be unconstitutional by the PA Supreme Court. Aggregation is across the sixteen companies identified and studied in this essay.

Figure 19: Total Campaign Contributions to State Senators by Senatorial Districts



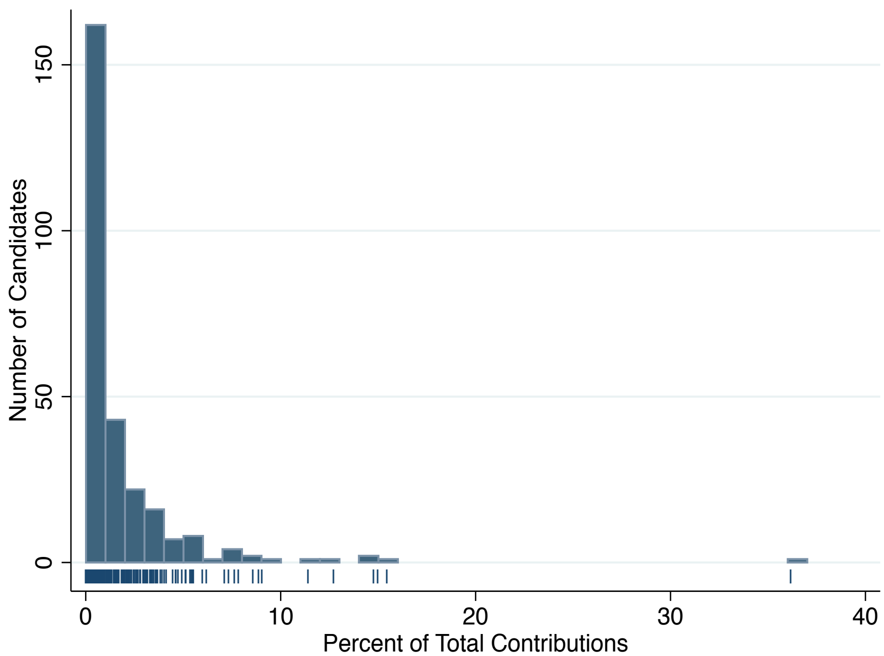
NOTE: Contributions vary with color: districts in white had \$0, districts in dark red had the highest amount; dark green dots represent wells drilled; light green dots represent well permits only. Data ranges from 2006 to 2012. Well data is only for the sixteen companies identified and studied in this essay.

Figure 20: Total Campaign Contributions to State Representatives by Legislative Districts



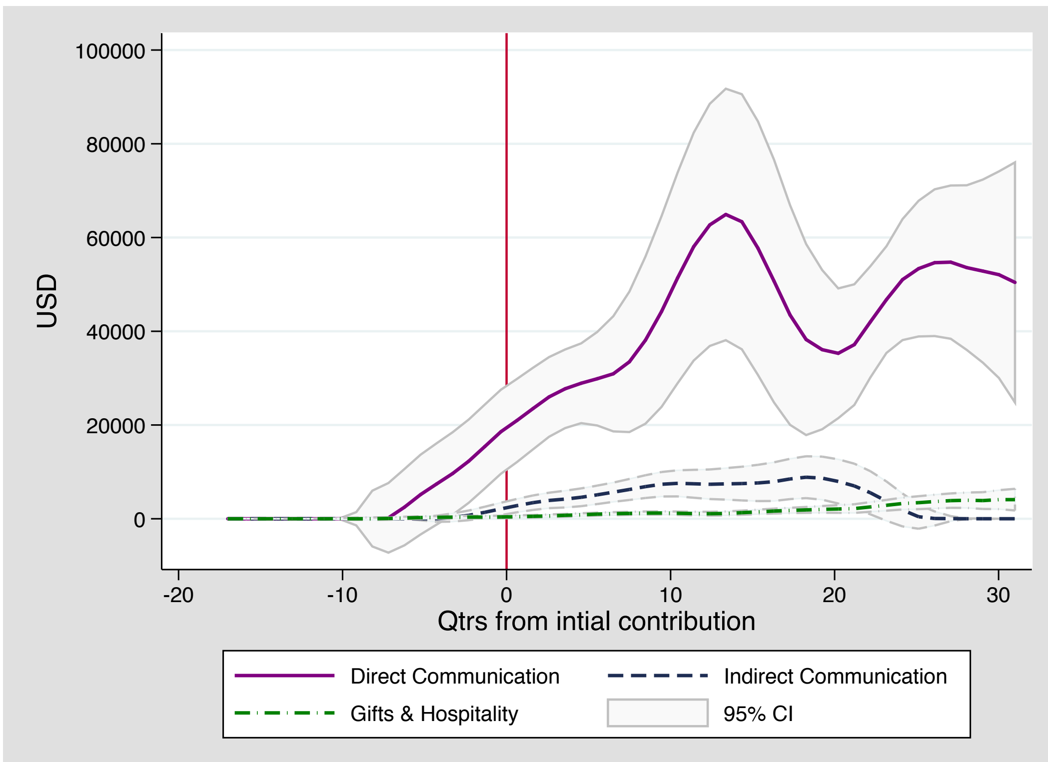
Note: Contributions vary with color: districts in white had \$0, districts in dark blue had the highest amount; dark green dots represent wells drilled; light green dots represent well permits only. Data ranges from 2006 to 2012. Well data is only for the sixteen companies identified and studied in this essay.

Figure 21: Histogram of Percent of Contributions from Industry per General Assembly Candidate



NOTE: Percentage of total was calculated aggregating contributions from all identified companies for each candidate, per each election, and dividing by the total contributions that candidate raised that election. Only General Assembly candidates receiving a nonzero amount from an identified company's PAC are included.

Figure 22: Lobbying Expenditures by Type Relative to Initial Contributions



NOTE: Local polynomial lines are calculated using local linear regressions using an Epanechnikov kernel with a four-quarter bandwidth. Expenditures from each company are lined up relative to each company's initial quarter with positive campaign contributions. Data only includes the 10 companies with registered PACs.

4.7 TABLES

Table 19: Summary Statistics of Parent Companies

	Lobbying (quarterly expenditures)		PAC (campaign contributions)	
Parent Company	Mean	Std Dev	Mean	Std Dev
Anadarko Petroleum	10,782	14,429	1,242	1,621
Cabot Oil & Gas	17,268	17,379	3,750	3,012
Chesapeake Energy	69,428	39,064	1,780	2,886
Chevron	9,527	14,306	1,300	548
Chief Oil & Gas	15,856	18,503	N/A	
Consol Energy	37,898	20,652	1,959	2,717
EQT Corporation	56,455	34,051	1,290	2,198
EXCO Resources	30,787	16,666	N/A	
Exxon Mobil	11,396	14,574	3,125	2,959
National Fuel Gas Co.	12,716	3,863	1,205	1,814
PA General Energy Co.	12,448	12,224	N/A	
Range Resources	146,613	139,840	2,060	4,383
Shell Oil	48,943	45,105	N/A	
Talisman Energy	18,886	22,627	N/A	
Southwestern Energy Co.	3,507	6,691	N/A	
The Williams Companies	14,222	13,016	1,857	2,329

NOTE: Lobbying statistics are reported for quarterly total expenditures from 2007 to 2013, while PAC statistics are reported for total contributions per candidate per election, conditional on any positive contribution, from 2006 to 2012.

Table 20: OLS Campaign Contributions to All Candidates

	(1)	(2)	(3)	(4)
VARIABLES	dependent variable = contribution (USD)			
Incumbent	64.55***	53.89***		20.10
	(21.84)	(19.52)		(21.89)
Vote pct in primary	-52.83*		-90.33	
	(31.39)		(72.80)	
Vote pct in general	18.52	51.64	104.0*	107.7**
	(36.67)	(35.83)	(56.17)	(50.36)
Won election	27.35	24.58	-10.17	33.91
	(23.72)	(22.60)	(44.26)	(25.65)
Company FE	X	X	X	
Party FE	X	X		X
Office-type FE	X	X		
Candidate FE			X	
Company-office FE				X
Observations	13,460	15,550	7,420	15,550
R^2	0.100	0.090	0.404	0.354

NOTE: Samples vary across specifications as follows: (1) includes all general-election candidates who participated in the primary election; (2) and (4) include all general-election candidates; (3) includes only incumbent candidates. The dependent variable for each observation is total contributions from each company to each official candidate in each state general election. All regressions include election-year fixed effects. Robust standard errors clustered at the company-office level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 21: OLS Campaign Contributions to All Candidates

	(1)	(2)	(3)	(4)
VARIABLES	dependent variable = contribution (USD)			
Incumbent	67.70***	55.24***		23.83
	(20.47)	(19.79)		(22.05)
Unopposed in primary	-37.45**		-37.30	
	(16.45)		(28.91)	
Unopposed in general	12.56	24.40*	25.87*	51.66***
	(14.64)	(13.99)	(14.43)	(19.95)
Won election	26.53	36.19**	4.881	58.70***
	(18.61)	(18.32)	(45.51)	(21.26)
Company FE	X	X	X	
Party FE	X	X		X
Office-type FE	X	X		
Candidate FE			X	
Company-office FE				X
Observations	13,460	15,550	7,420	15,550
R^2	0.100	0.090	0.404	0.354

NOTE: Samples vary across specifications as follows: (1) includes all general-election candidates who participated in the primary election; (2) and (4) include all general-election candidates; (3) includes only incumbent candidates. The dependent variable for each observation is total contributions from each company to each official candidate in each state general election. All regressions include election-year fixed effects. Robust standard errors clustered at the company-office level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 22: OLS Campaign Contributions to General Assembly Candidates

VARIABLES	(1)	(2)	(3)	(4)
	dep. var. = contribution (USD)			
Incumbent	245.7***	183.6***		36.64***
	(59.28)	(42.72)		(10.62)
Vote Pct in primary	-204.7**		-300.9	
	(97.78)		(256.3)	
Votes Pct in general	-101.2	-107.1	328.2*	125.0**
	(94.34)	(89.69)	(193.3)	(51.87)
Won election	-5.442	38.70	-108.0	0.166
	(63.35)	(46.32)	(161.8)	(14.63)
Party (Rep=1)	108.2***	100.8***		57.09***
	(26.87)	(25.31)		(11.29)
Office type (Senator=1)	536.4***	380.3***		
	(105.2)	(75.94)		
ERE Committee	324.8***	341.1***	-120.7*	111.5***
	(91.98)	(95.93)	(67.98)	(33.75)
Total wells	3.808**	4.076**	3.229***	
	(1.682)	(1.657)	(1.186)	
Drilling permits	0.581***	0.573***	0.848***	0.216***
	(0.175)	(0.173)	(0.232)	(0.0628)
Wells Drilled				7.879***
				(2.188)
Company FE	X	X	X	
Candidate FE			X	
Company-office FE				X
Observations	13,300	14,390	7,380	14,390
R^2	0.145	0.136	0.513	0.383

Note: Samples vary across specifications as follows: (1) includes all General Assembly general-election candidates who participated in the primary election; (2) and (4) include all General Assembly general-election candidates; (3) includes only incumbent General Assembly candidates. The dependent variable for each observation is total contributions from each company to each official candidate in each state general election. All regressions include election-year fixed effects. Robust standard errors clustered at the company-office level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 23: OLS Campaign Contributions to General Assembly Candidates

VARIABLES	(1)	(2)	(3)	(4)
	dep. var. = contribution (USD)			
Incumbent	218.9***	174.6***		41.65***
	(54.42)	(41.58)		(9.742)
Unopposed in primary	-59.79		-73.40	
	(39.27)		(78.90)	
Unopposed in general	-40.41	-33.92	94.20	58.05***
	(40.83)	(39.66)	(58.72)	(20.52)
Won election	-6.260	17.84	-51.68	29.46***
	(52.60)	(39.44)	(157.8)	(8.324)
Party (Rep=1)	102.9***	94.82***		53.14***
	(27.52)	(25.50)		(10.06)
Office type (Senator=1)	531.9***	378.0***		
	(105.1)	(76.28)		
ERE Committee	323.6***	342.1***	-122.4*	109.4***
	(91.71)	(95.54)	(67.98)	(33.46)
Total wells	3.800**	4.079**	3.225***	
	(1.682)	(1.656)	(1.189)	
Drilling permits	0.581***	0.573***	0.848***	0.216***
	(0.176)	(0.173)	(0.232)	(0.0628)
Wells Drilled				7.881***
				(2.189)
Company FE	X	X	X	
Candidate FE			X	
Company-office FE				X
Observations	13,300	14,390	7,380	14,390
R^2	0.145	0.136	0.513	0.383

NOTE: Samples vary across specifications as follows: (1) includes all General Assembly general-election candidates who participated in the primary election; (2) and (4) include all General Assembly general-election candidates; (3) includes only incumbent General Assembly candidates. The dependent variable for each observation is total contributions from each company to each official candidate in each state general election. All regressions include election-year fixed effects. Robust standard errors clustered at the company-office level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 24: OLS Quarterly Lobbying Expenditures and Contributions

	(1)	(2)	(3)	(4)
VARIABLES	dep. var. = total lobbying (USD)			
Contributions	0.260	0.328	0.127	0.0937
	(0.287)	(0.224)	(0.204)	(0.200)
Contributions (lagged 1 qtr)	0.436	0.353	0.287	0.166
	(0.330)	(0.248)	(0.240)	(0.232)
Contributions (lagged 2 qtr)	0.519	0.348	0.466*	0.297
	(0.443)	(0.375)	(0.251)	(0.243)
Contributions (lagged 3 qtr)	0.568*	0.235	0.281	0.0348
	(0.299)	(0.216)	(0.263)	(0.256)
Contributions (lagged 4 qtr)	0.358	0.0149	0.0970	-0.227
	(0.284)	(0.276)	(0.271)	(0.269)
Contributions (lagged 5 qtr)	0.706**	0.152	-0.0521	-0.429
	(0.354)	(0.310)	(0.296)	(0.299)
Contributions (lagged 6 qtr)	1.299	0.412	-0.167	-0.414
	(1.116)	(0.659)	(0.295)	(0.300)
Well Permits		140.7**		106.2*
		(66.31)		(61.59)
Well Permits (lagged 1 qtr)		99.01		71.78
		(75.16)		(62.05)
Existing Wells		169.3*		134.7***
		(101.8)		(29.31)
Company FE	X	X	X	X
Quarter FE	X	X		
Observations	512	512	400	400
R^2	0.333	0.444	0.222	0.439
ρ			0.789	0.713

NOTE: The dependent variable for each observation is quarterly total lobbying expenditures for each company. Reported ρ statistics are calculated using the corresponding Durbin-Watson statistic. Columns (1) and (2) present bootstrapped standard errors clustered at the company level in parentheses. Columns (3) and (4) present conventional standard errors after accounting for the AR(1) process. *** p<0.01, ** p<0.05, * p<0.1

5.0 CONCLUSION

My hope is that the results and conclusions presented in this dissertation may be used to improve our society. By better understanding these effects, we can improve policies that directly regulate firms, e.g. regulations on competition or campaign finance and lobbying regulations, as well as understand all policies may have unintended consequences which may play out through firms, e.g. alcohol regulations. Although our knowledge and foresight of these things will always be incomplete, I hope research such as mine pushes us, as a society of firms, to think more broadly of the potential far reaching impact of regulation and the lack thereof.

APPENDIX A

BLUE LAWS, LIQUOR STORES, AND CRIME

A.1 DATA DETAILS

For police-incident data for both Hartford and Providence, any incidents marked as duplicates, misreports, clerical errors, or occurring outside the municipal boundary are dropped from all analyses. Moreover across the two and half year period of police incidents from Providence, 21 daily logs are missing from the city's records. These missing logs appear to be clerical errors instead of days free of police incidents. As thus, I use a hot deck imputation of crimes from the previous and following week to impute the crimes for these missing logs; specifically, for a missing daily log, I take the daily logs for the same day of the week from the previous and following weeks, and then randomly choose police incidents from those two logs with 0.5 probability to populate an imputed daily log. Hot deck imputation is a long-lasting method from computer science, deriving its name from the recently-run stack of punch cards that felt "hot." While slightly dated, this method preserves the marginal distribution of the variable containing missing data (Cameron and Trivedi, 2005). This ensures that the imputed crimes follow the average or general temporal, spatial, and categorical pattern of the similar days in the sampling pool, i.e. the "hot deck." While these imputed police incidents comprise a small amount of the Providence dataset, I check most of the empirical results against using the data hot-deck imputed with different random seeds and against treating all missing logs as days with no crimes. All of the results I have checked are robust to these alternative methods of imputation.

Unlike on-premises alcohol which must be consumed during permitted business hours of on-premises establishments, package alcohol may be consumed after off-premises stores are closed. Thus for all Sunday counts I count any crime that occurred after retail sales began on Sundays in Connecticut, 10:00am, and before retail sales began on Monday mornings, 9:00am. Results do not seem to be driven by including this over night period. For simplicity and to minimize any residual effects from off-premises purchases, I count crimes occurring between 12:00am Tuesday and 11:59pm Saturday for the other days sample. All other days of the week results are all robust to including Mondays.

A.2 LIST OF DATA SOURCES

- **Connecticut Open Data Portal:**

  Homepage: https://data.ct.gov/

  Dataset: State Licenses and Credentials [05/06/2015 snapshot]

  https://data.ct.gov/Business/State-Licenses-and-Credentials/faub-mjkr

- **Hartford Open Data Portal:**

  Homepage: https://data.hartford.gov/

  Datasets:

  - Police Incidents 01012005 to Current [Accessed 10/10/2015]

    https://data.hartford.gov/Public-Safety/Police-Incidents-01012005-to-Current/889t-nwfu

  - various GIS data

    https://data.hartford.gov/Community/Hartford-GIS-Data/9t3u-k43z

- **Providence Rhode Island city website:**

  Homepage: http://www.providenceri.com/

  Dataset: Daily Case Logs[1]

  http://www.providenceri.com/police/case-logs

[1]Since November 2015, daily logs have been posted on Providence's Open Data Portal: https://data.providenceri.gov/Public-Safety/Providence-Police-Case-Log-Past-180-days/rz3y-pz8v

- **Rhode Island Geographic Information System:**

  Homepage: http://www.edc.uri.edu/rigis/

  Dataset: Census 2010: Summary File 1

  http://www.edc.uri.edu/rigis/data/data.aspx?ISO=society

APPENDIX B

CORPORATE DENTISTRY, COMPETITION, AND THE PROVISION OF DENTAL CARE

B.1 DATA DETAILS

In particular covariates on market characteristics come from the U.S. Bureau of Economic Analysiss Local Area Personal Income, including the U.S. Census Bureau's Annual Estimates of the Resident Population. Designation of HPSA status at the county-year level is obtained from the U.S. Department of Health and Human Services, Health Resources and Services Administration's HPSA data set. Summary statistics for the entire panel data set are provided in Table 17.

All calculated distances used in Equations 3.2 and 3.3 use 2010 county centers of population from the Census Bureau and are measured in kilometers. Headquarter locations are from LexisNexis Academic's database of company profiles and companies' websites, including archived websites from Internet Archive's archived database. When calculating $FirstEntry_k^i$, if firm i opened multiple offices in market k's state at the same time, the office that is closest to market's k center of population is used as firm i's first entry. The alternative measure for $FirstEntry_k^i$, which discounts that distance by the number of years firm i has been operating in that state, is calculated by dividing the distance by $2015 - yr + 1$ where yr is the year of the first entry. Summary statistics for the full sample used in the MSE estimation are presented in Tables 18.

B.1.1 Business Listing Quality and Cleaning

Business listings from InfoUSA have been used in numerous academic studies (e.g. Seim and Sinkinson (2016) and McDevitt (2011)) and from the experiences of this essay appear to be exceptionally accurate. Using information from any type of business or mail listings for statistical analysis requires large amounts of data cleaning and preprocessing and combining duplicate entries, which can decrease the accuracy of the data. For example, this essay only uses locations that have licensed dentists practicing dentistry on patients and does not use locations that may hire dentists or be categorized as dentistry but does not practice dentistry (e.g. dental labs, home addresses of dentists, dental equipment companies).

To ensure the quality of this cleaning, I check 250 unique locations listed in 2016, chosen as a random sample stratified by states to be representative by state of the total number of 2016 offices. These locations are individually checked on Google Maps and Google Street View to verify, when possible, if a dentist office exists at that location, the listed office name is correct, and the accuracy of the geographical coordinates provided in the business listing.

In addition as a result of cleaning, some true dental offices may be missing.[1] Thus I impute observations that have an office in the exact location the year before and the year afterwards of the same company (e.g. independent office or same DSO). Less than 3.2% of all office observations are imputed. Many of the results are checked for sensitivity to this imputation, and out the results that are checked, all are robust to not imputing these missing observations.

B.1.2 Identifying Dental Chains

Of utmost importance for the analysis in this essay is the ability to accurately identify DSO offices from the business lists of all dentist offices. I take great lengths to do so and outline my procedure here.

First, I compose a list of DSOs from various known listings[2] that operate in this general

[1] The timing of the snapshot of the business listing may create a missing observation as well.

[2] In particular I use the following sources: LexisNexis Academic's database of company profiles, the Association of Dental Support Organizations publicly-available membership list, a list of DSOs compiled by the McGill Advisory (documented in McGill (2011)), various news articles, and general web searches.

region. From this initial list I run a series of probabilistic record linking[3] on the combined business lists for the DSO brand names, manually verifying linked results. Second, among the unidentified (i.e. non-matched offices), I create a list of potential DSOs from offices with duplicate names across space and from offices listed as a branch or subsidiary. These potential DSOs are each manually verified from LexisNexis and/or company websites to ensure they are bona fide dental chains, satisfying my definition of operating fifteen or more offices, and then added to the original DSO list.

With this improved list, I repeat the record linking multiple times to optimize tuning parameters and ensure a high accuracy in matching. Any DSOs not searched in LexisNexis in the previous steps, are searched in the end to ensure they are independent companies and not subsidiaries of another DSO operating in this region.

A few DSOs operate their offices under unbranded names.[4] In the business list data, these unbranded offices are often listed as the parent company or linked to the parent company as a branch location, in which case the method I use to identify branded offices is sufficient. To identify individual unbranded DSO offices when this is not the case, I manually obtain office names and addresses from these DSOs' company websites, including past archives of their websites through the Internet Archive's database of archived webpages. This information is then used to identify these offices in the business listings across time and space. It is worth noting, that for both unbranded and branded DSOs I do not add any locations or entries to my original business list data from these additional sources but simply use them to identify which offices have DSO affiliation.

B.2 LIST OF DATA SOURCES

- **Infogroup's ReferenceUSA:**

  Homepage: https://resource.referenceusa.com/

  Data set: U.S. Historical Business Database, 2003 to 2016

[3] Also often referred to as "fuzzy matching."

[4] In the study area for this essay, the following companies fall into this category: DentalWorks, Heartland Dental, and Pacific Dental Services.

– available through subscription.

- **LexisNexis Academic:**

  Homepage: https://www.lexisnexis.com/en-us/home.page

  Data set: Company Profiles

  – available through subscription.

- **U.S. Bureau of Economic Analysis:**

  Homepage: https://www.bea.gov/

  Data set: Local Area Personal Income

  – URL: https://www.bea.gov/regional/

- **U.S. Census Bureau:**

  Homepage: https://www.census.gov/

  Data sets: 2010 county centers of population

  – URL: https://www.census.gov/geo/reference/centersofpop.html

- **U.S. Department of Health and Human Services, Health Resources and Services Administration:**

  Homepage: https://www.hrsa.gov/index.html

  Data set: Health Professional Shortage Areas

  – URL: https://datawarehouse.hrsa.gov/data/dataDownload.aspx

B.3 TECHNICAL DETAILS

Kernel. I use a smoothed version of the pairwise maximum score estimator developed by Yan (2017). In particular, I use the following smoothing function:

$$K(v) = \begin{cases} 0 & \text{if } v < -1 \\ 0.5 + \frac{105}{64}(v - \frac{5}{3}v^3 + \frac{7}{5}v^5 - \frac{3}{7}v^7) & \text{if } |v| \leq 1 \\ 1 & \text{if } v > 1 \end{cases} \tag{B.1}$$

This smoothing function is considered "low" high-order, the integral of a fourth-order kernel, and is used by Horowitz (1992, 2002). For the binary smoothed maximum score estimator, he demonstrates this smoothing function benefits from the increased asymptotic efficiency of being high-order, while also being less sensitive to bandwidth selection than kernels of additional orders.

Undersmoothing. A smoothed estimator requires the selection of a bandwidth, which can often change finite-sample estimates. I choose an approximation of the optimal bandwidth suggested by Horowitz (1992, 2002) and apply undersmoothing by taking a fraction of that value. Results presented in Table 16 use a bandwidth equal to 0.5 of the approximated optimal bandwidth. During the bootstrapped procedure a new bandwidth is calculated in this manner for each bootstrap sample.

Bootstrap. Due to the difficult nature of the optimization and the properties of differential evolution (DE), the DE optimization requires specific tuning to ensure it finds a global maximum. This tuning, however, can be difficult if not impossible to automatically implement during the bootstrap procedure. If the DE blatantly diverges on a particular bootstrap sample, I discard the corresponding estimate and test statistic. In the current results, this occurs less than 1.5% of the time. Future work will remedy this by using a more time-intensive DE algorithm to ensure a global maximum can always be found.

APPENDIX C

STATE CAMPAIGN CONTRIBUTIONS FROM SHALE GAS DEVELOPMENT

C.1 DATASET CONSTRUCTION

To assist readers in understanding how the datasets were compiled, I provide a concise chronological road map of the construction:

- First, I constructed a list of all companies listed as operators of unconventional gas wells during the interested time range (117 operators in total).
- Second, I looked up each operator in LexisNexis Academic, using a variety of databases[1] to determine if each operator was independently owned and operated or a subsidiary of a larger company (resulting in 80 independent companies). If an operator was subsidiary, I also collected information on the parent company, including all their other subsidiaries in exploration, operations, and upstream development of natural gas. Many of these operators started as independent companies, but were bought by larger companies as the shale gas boom went into full swing. To confirm acquisitions and dates, I also used LexisNexis Academic to find documents on company acquisitions and sales.
- Third, I searched the state lobbying disclosure database for each operator, parent company, and related subsidiary. Any company having expenditures on lobbying state officials, must register as lobbying principal in this database.

[1]See Appendix C.6 for the exact databases used.

- Fourth, I searched a database of all published state campaign finance reports and registration files, looking for PACs for each operator, parent company, and related subsidiary. In addition I also searched PACs listed in some of the lobbying principal files.
- Finally, from this large exhaustive list of operators with linked registered lobbying principals and linked PACs, I dropped companies which had minimal shale activity and minimal combined lobbying and campaign finance activity.

This resulted in a dataset of sixteen companies, six of which have no campaign contributions during the time range (see Table 19 for a complete list). A few of these sixteen companies have either multiple registered lobbying principals or multiple PACs, and several of them own multiple operators.

C.2 WELLS AND DRILLING DATA

The Pennsylvania Department of Environmental Protection (PA DEP) publishes a wide variety of reports and data on all oil and gas production in the state. Specifically from the PA DEP Oil & Gas Reporting website I obtained reports and data on individual well permits, individual SPUD drilling dates and information, statewide reports on all active wells, and individual well inspection information including violations and fines. To create a the list of all operators of unconventional natural gas wells, I populated a list of all operators who, for unconventional gas and "gas and oil" wells, received a well permit or drilled a well between 2005 to 2013 and who had existing wells (active or inactive status) in 2005. From this list I then dropped any operator who had less than two combined total permits, drills, or wells.

C.3 LOBBYING DATA

All of the lobbying data comes from the Pennsylvania Lobbying Disclosure Registration (LDR) database. This is the official and exhaustive database for all state lobbying reporting and is administered by the Pennsylvania Department of State (PA DOS), Bureau of Commissions, Elections and Legislation. Every lobbyist and organization that hires or spends money lobbying state officials are required by law to be registered in this database. Each registered principal has registration files containing information on the company or organization, information on hired registered lobbyists and lobbying firms, and lists PACs associated with that principal. When searching the campaign finance database for operators, parent companies, related subsidiaries, I also searched for these listed PACs names. Each registered principal also has quarterly expense reports for each quarter they were registered. These quarterly expense reports break lobbying expenses down into three mutually exclusive and exhaustive categories: direct communication, indirect communication, and "total expenditures for gifts, hospitality, transportation, and lodging for State officials or employees or their immediate families" (which I will refer to as "gifts and hospitality"). Unfortunately there is no further breakdown of expenditures or any data on who and how these lobbyists lobbied.

C.4 CAMPAIGN FINANCE DATA

For the campaign finance data, I obtained data from all of the published campaign finance expense reports, available from the PA DOS Campaign Finance website, as well as lists of all registered political committees (candidate committees as well as PACs), available from the PA DOS Election Information website. Each expense reports contains total contributions, itemized contributions over \$50, itemized in-kind contributions worth over \$50, and an itemized statement of expenditures. Hence, contributions over \$50 from a PAC (or other types of committees) to a candidate (or a candidate's committee) are recorded twice in the set of all expense reports, once as an expenditure on the PAC's report and also on the candidate's report as a contribution.

I compiled all the itemized expenses from all registered PACs to construct my database of campaign contributions, which I then searched for each operator, parent company, and related subsidiary. After finding all contributions from my linked PACs, I had to determine which state candidate each expense was to.[2]

[2]This would seem a trivial step, but due to the misspellings and wrong wordings of candidate's and their

My decision to use the PAC reports instead of the candidates' reports was due two reasons: first, while the PAC expenses were messy and difficult to match, the candidate contributions were worse (many candidates still handwrite their reports); second, as explained in the next paragraph, some of the expense reports are not published, and it is easier to check a dozen or so PACs than over 2,000 candidates.

After searching, I noticed that for my sixteen parent companies, forty-nine expense reports were recorded as being submitted but not "published" (i.e. available online). After contacting the PA DOS, it was confirmed that some of these missing reports were improperly coded as "reports" instead of "statements" and hence where not published since they do not exit. For others, however, the PA DOS did not know why the were not published. This was problematic, since most of the missing files are just from a few PACs, which are associated with some of the larger shale gas developers (including Consol, Chesapeake, and EQT).

To escape this data deadened, I turned to an additional external dataset. The National Institute on Money in State Politics has an ambitious goal of creating a complete online database of all state campaign finance records in the United States. Understandably, their data work takes time, and at the time when I accessed their database, some of the Pennsylvania reports had not been collected still. Contrary to how I constructed my database, the National Institute uses itemized contributions from each candidate's and candidate's committee's reports. Using candidate names reported of PAC reports instead of PAC names reported on candidate statements resulted in a much more accurate dataset, and hence (minus these few missing files) I chose to compile data directly from PA DOS. But I did use their online "Follow the Money" database for the missing files, I looked up contributions from the companies with missing files to Pennsylvania state candidate during the cycle dates of each missing file. This resulted in some success, and I added these contributions into my dataset to supplement the missing expense reports. [Hence, while it is very small percentage of the data, every analysis using campaign contribution data is potentially using some data from the National Institute on Money in State Politics.] However, I do not believe that this has "found" all the missing data, and in the future wish to track down these missing files.

committee names and due the voluntary/mistaken inclusion of itemized contributions to local and federal candidates it was indeed nontrivial.

In addition, I used the National Institute on Money in State Politics "Follow the Money" database to calculate the total contributions raised by each candidate. Any statistics or analysis referring to the total, is using this variable calculated from data from their database.

C.5 ELECTIONS, CANDIDATES, AND DISTRICTS DATA

To obtain comprehensive data on state candidates and elections, I used a variety of data sources. First, to construct a list of all candidates in state general and primary elections, I used both the Candidate lists, found on the PA DEP State Election Information website, and the official election returns, found on the PA DEP Election Results website. Second, to obtain data on election outcomes I obtained all of the official state election returns, again found on the Election Results website. From this I was able to construct a wide variety of election variables including election outcomes, incumbency, whether candidate ran unopposed, and margin of victory.

Pennsylvania had a redistricting for the entire General Assembly in 2012 following the 2010 U.S. Census. Due to the Pennsylvania Supreme Court ruling on an appeal to the original 2011 redistricting plan, the redistricting plan was not approved in time to be used for the 2012 state elections and thus did not go into effect until 2013, Pennsylvania Legislative Reapportionment Commission (2013). To locate which Senatorial and Legislative district each well and permit is located in, I obtained spatial data for both the old state districts (drawn in 2002) and the new districts from 2012. I obtained data on the old districts from the Pennsylvania Spatial Data Access database and on the new districts from the Pennsylvania General Assembly Legislative Reapportionment Commission website.

Finally, to determine which politicians held influential committee positions, I obtained committees roll call vote records from the PA House of Representatives records and PA State Senate records, both found through the Pennsylvania General Assembly website. From these records I was able to determine which Senator and Representative was on influential committees in each legislative session, such as the House Environmental Resources and Energy Committee and the Senate Environmental Resources and Energy Committee.

C.6 LIST OF DATA SOURCES

- **LexisNexis Academic database, Company Profiles:**

  Zoom Company Information, Experian Powered Business Reports, GlobalData Locations and Subsidiaries, LexisNexis Corporate Affiliations, Hoover's Company Records

  - Available through private subscription only.

- **National Institute on Money in State Politics:**

  Pennsylvania state campaign finance data

  - Available online, through customizable reports and downloadable as csv files.
  - Website: http://www.followthemoney.org/

- **Pennsylvania Department of Environmental Protection Oil & Gas Reporting Website:**

  SPUD Data reports, Permits Issued Detail reports, Statewide Data downloads, Operator Information, Compliance reports

  - Available online, through customizable reports (variety of download formats).
  - Website: http://www.dep.pa.gov/DataandTools/Reports/Oil%20and%20Gas%20Reports/Pages/default.aspx

- **Pennsylvania Department of State Campaign Finance Website:**

  Campaign Finance Expense Reports, Committee Registration Reports

  - Note: under the PA Bureau of Commissions, Elections and Legislation.
  - Available online, through search and downloadable as PDFs and text files.
  - Website: https://www.campaignfinanceonline.state.pa.us/pages/CampaignFinanceHome.aspx

- **Pennsylvania Department of State Election Information Website:**

  Candidate Database, Registered Committee Lists

  - Note: under the PA Bureau of Commissions, Elections and Legislation.
  - Available online, through search and downloadable as spreadsheet.
  - Website: https://www.pavoterservices.state.pa.us/ElectionInfo/electioninfo.aspx

- **Pennsylvania Department of State Election Results Website:**

  Election Results

  - Note: under the PA Bureau of Commissions, Elections and Legislation.
  - Available online, through search.
  - Website: http://www.electionreturns.state.pa.us/

- **Pennsylvania General Assembly Website:**

  Senate Committee Roll Call Votes, House Committee Roll Call Votes

  - Available online.
  - Website: http://www.legis.state.pa.us/

- **Pennsylvania General Assembly Legislative Reapportionment Commission:**

  Redistricting Maps (new districts drawn in 2012)

  - Available online, downloadable as shapefiles.
  - Website: http://www.redistricting.state.pa.us/Data.cfm

- **Pennsylvania Lobbying Disclosure Registration Website:**

  Principal registration files, Expense Reports

  - Note: under the PA Bureau of Commissions, Elections and Legislation.
  - Available online, through search and downloadable as PDFs.
  - Website: https://www.palobbyingservices.state.pa.us/

- **Pennsylvania Spatial Data Access:**

  TIGER/Line Shapefiles, Pennsylvania 2009 Current State Legislative District, Upper and Lower Chambers (old districts drawn in 2002)

  - Available online, downloadable as shapefiles.
  - Website: http://www.pasda.psu.edu/

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