Does Entry Regulation of Big-box Stores Protect the Retail Sector? Evidence from Store Cap Ordinances in the U.S.

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This version: January 3, 2017

Abstract

Since the rapid growth of big-box retailers in 1990s, many local governments and municipalities have enacted store cap ordinances (SCOs) to constraint store sizes in order to prevent entry of big-box retailers and to protect local retail business. By exploiting SCOs introduced in different municipalities, at different periods of time and with different levels of restrictiveness, I analyze the effects of entry regulation on the retail sector in the US. I address the endogeneity problem by constructing instrumental variables using political composition and the geographic and time pattern of the passage of SCOs. My findings suggest that, in contrast to the objectives of these regulations, the retail sector was actually harmed by the creation of entry barriers.

Acknowledgements

I thank John Clapp for his invaluable comments. I also thank Gary Jacobson for generously sharing data on political composition. I am grateful to Victor Couture, Nathan Yang, Kristian Behrens, Chongyu Wang and Denis Schweizer for constructive suggestions. All remaining errors and omissions are my own.

1. Introduction

At least since Joseph Schumpeter it is widely believed that firm creation is an important channel of economic growth, particularly in many service activities. Low costs of entry can create more competition, lead to better technology adoption and offer lower prices to consumers. But, in OECD countries, retail trade is subject to substantial governmental regulations that are often used to curb the entry of large retail stores, known as "big-box retailers." The objective is to preserve local communities and, more importantly, to protect the local business and employment.

There exists relatively little empirical evidence on the costs and benefits associated with entry barriers of big-box retailers. A small but growing literature criticizes the effect of entry regulations on the efficiency of the retail sector (Bertrand and Kramarz, 2002; Cheshire, Hilber and Kalanis, 2015; Schivardi and Viviano, 2011; Sadun, 2015; Maican and Orth, 2016). In particular, entry deterrence of big-box retailers could reduce economics of scale, slow down innovation and hinder the reallocation of resources and employment between firms (see Foster, Haltiwanger and Krizan, 2006). To my knowledge, none of the existing studies focuses on the world's largest retail market – the U.S.[1]

Since the rapid growth of big-box retailers in the U.S. in 1990s, many local governments and municipalities have enacted store cap ordinances (SCOs) to constraint store sizes in order to prevent entry of big-box retailers. The objective of this "smart planning policy" is to protect and promote individual business and local employment. In this paper, I analyze the effect of SCO on the performance and market structure in the US retail trade sector by exploiting regulation changes at municipality level from 1997 to 2012. The uniqueness of this study is that SCOs were passed

[1] In 2014, total sales in the retail trade industry in the US was more than 5.2 trillion. China might bypass the US and become the largest retail market in the world in 2016.

by different municipalities at different periods of time. In addition, their levels of restrictiveness vary across places. This setting creates a great deal of heterogeneity cross time and cross places and thus offers a unique opportunity to study the impact of entry regulation at a very fine geographical level.

The retail industry in the US has experienced tremendous productivity growth, which is mainly driven by big-box retailers such as Wal-Mart, Target and Home Depot (Basker, 2005, 2012, 2015, 2016; Basker, Klimek and Van, 2012; Foster, Haltiwanger, Krizan, 2006; Haltiwanger, Jarmin and Krizen, 2010; Hausman and Liebtage, 2007; Homles, 2001; Hortaçsu and Syverson, 2015; McKinsey Global Institute, 2001). According to Global Insight, an economic organization, the expansion of Wal-Mart from 1985 to 2004 is associated with a 9.1% decline in the average cost of food prepared at home and a 3.1% decline in the overall CPI, an increase of about 0.9% on real disposable income. By 2004, it has saved $263 billion for consumers or $2,329 per household.[2]

However, it is widely observed that local activists protest the entry of big-box stores, especially Wal-Mart.[3] Opponents of mega-stores have tried to block their entry for well-grounded reasons. In addition to the preservation of historical culture, prevention of urban sprawl, and protection of the environment, the main concerns are economic: Big-box retailers drive out local businesses, eliminate local jobs and result in lower wages (Ellickson and Grieco, 2013; Foster, Haltiwanger, and Krizan, 2006; Haltiwanger, Jarmin and Krizen, 2010; Neumark, Zhang and Ciccarella, 2008). In her book, "Big-Box Swindle", Stacy Mitchell writes "as we'll see, corporate

[2] For details, please refer to http://www.ihsglobalinsight.com/publicDownload/genericContent/11-03-05_walmart.pdf (last accessed: March 3, 2016). Hausman and Liebtage (2007) find that poor consumers have benefited most from these savings. Consumers earning less than $10,000 per year gained almost 30% in purchasing power over food because of Wal-Mart, whereas those earnings $100,000 gained a 20% rise in their ability to purchase good.

[3] From 1998 to 2005, Wal-Mart floated 1,599 proposals to open new stores. 563 of them received protests. 65% of these protests were successful (see, Ingram, Yue and Rao, 2010). In his book, "When Principles Pay: Corporate Social Responsibility and the Bottom Line," Geoffrey Heal writes "Wal-Mart is currently facing over 8,000 lawsuits and determined and effective political opposition when trying to open new stores." (Heal, 2008)

chains have left us worse off – as wage earners and even as consumers – and they've imposed significant costs on the environment and our communities." (Mitchell, 2006, pp. 32).[4]

Many local governments and municipalities have moved further by adopting regulations in curbing the growth of mega-retailers. Perhaps one of the most important sources of cost efficiency is the size effect with a highly standardized operation (Basker, 2009; Basker, Klimek and Van, 2012; Foster, Haltiwanger and Krizan, 2006; Hortaçsu and Syverson, 2015; Holmes, 2011; Heal, 2008). According to Institute for Local Self-Reliance (ILSR), Wal-Mart has been replacing its superstores (with a standard size of 100,000 sq. ft.) with supercenters (with a standard size of 250,000 sq. ft.). Super Target has a comparable average store size of a Wal-Mart super center. A standard Home Depot and Lowe's is about 140,000 sq.ft, compared with a typical main street retailer of 20,000 sq. ft. These big-box retailers have huge cost advantages in terms of their sizes which produce agglomeration economies under one roof.

SCOs that limit the square footage of retail stores have gained popularity due to its effectiveness and low cost since the rapid growth of big-box retailers in 1990s. The purpose of SCO is to constrain the size of retail establishments, especially mega-stores that sell grocery items, so that standardized big-box stores could not find a way to conform. Even if they could conform, they have to reduce their cost advantages. The size limits are not randomly chosen; size caps are designed to best protect the local businesses and communities. Section 3 provides more details as well as examples. According to the Retail Industry Leaders Association (RILA), 52 localities in

[4] A recent study in 2013 by Americans for Tax Fairness, a coalition of 400 national and state-level progressive groups, concludes that Wal-Mart's low-wage workers cost U.S. taxpayers an estimated public assistance of $6.2 billion on food stamps, Medicaid and subsidized housing, and that "a single Walmart Supercenter cost taxpayers between $904,542 and $1.75 million per year, or between $3,015 and $5,815 on average for each of 300 workers."For full report, please refer to http://www.americansfortaxfairness.org/files/Walmart-on-Tax-Day-Americans-for-Tax-Fairness-1.pdf (last accessed: March 3, 2016)

20 states had ever passed or proposed size-cap ordinance as of August 2004. The average size cap is about 50,000 sq. ft.

Does SCO help promote or at least protect local employment and businesses? And does SCO change the nature of competition and affect the retail market structure? To answer these questions, I hand-collect detailed data on store cap regulations from various sources, including Institute for Local Self-Reliance (ILSR), Sprawl-Busters and Lexis-Lexus. Details of the SCO regulation include the year of passage, size limit of retail stores and other accompanying restrictions.[5] To my knowledge, these data have not been used in any of the existing economic studies.[6] As most of store cap regulations were passed by local governments and municipalities, the unit of analysis is incorporated place ("place"). During my sample period from 1997 to 2012, 247 places have passed SCOs.[7]

I construct two time-varying measures of entry barriers. The first and the more important one uses the cross-sectional variation in store size limits, normalized for total employment prior to the regulation. The second measure is an indicator of the presence of SCO regulation. My measures of economic performance include retail sectoral establishment, employment, sales and payroll. To measure market structure, I calculate the fraction of sub-sector establishments over the total number of establishment in all sectors. A measure on employment fraction is constructed in the same way.[8] I merge the SCO data with the Economic Census, the US Census and the Bureau of

[5] Some SCOs have restriction on retail spaces allocated to grocery items. In data section, I explain in details how I construct an overall proxy to measure the restrictiveness of entry.
[6] In a sociology context, Ingram, Yue and Rao (2010) collect protest data from Sprawl-Busters to examine the impact of uncertainty over protest occurrence on strategic interaction between companies and activists. Please refer to Section 4. Data for detailed explanations on data sources.
[7] This number is more than five times larger than "52 localities" reported by RILA in 2004. One reason is that more SOCs were passed after 2004. Another reason is that localities in the RILA report refer to a mixture of both municipalities and counties.
[8] Similar measures could not be calculated using sales and payroll because data are aggregated differently across industries.

Economic Analysis (BEA) and obtain a panel dataset at place level. Based on the stated purposes of SCOs, of which the main target is big-box retailers, the focus of this study is on the 4-digit NAICS sub-sectors in the retail trade industry (NAICS=44-45) that potentially compete with big-box retailers such as supercenters and department stores (e.g. Wal-Mart and Target), warehouses (e.g. Costco) and "category killers" (e.g. Home Depot).

I start my analyses with fixed-effect models to estimate the impact of store caps on retail sectoral establishments, employment, sales revenue and payroll with place fixed effects, industry fixed effects and year fixed effects. Results suggest that more stringent entry regulations are associated with poorer retail performance: coefficient estimates of the SCO adoption are negative and significant for all the four performance measures. In addition, establishment and employment shares are negatively related to entry barriers created by SCOs. These results hold when I compare places with and without SCO. Interestingly, models with county fixed effects provide the opposite results, suggesting the importance to control for place-level heterogeneity in local zoning regulations on land uses.

Next, I employ a differences-in-differences approach to identify the effect of SCO by comparing the difference in retail performance and market structure after and before the SCO regulation for places affected by the regulation to the same difference for unaffected places. The treatment group includes places that passed SCOs. The control group includes places without SCO in the same county to better control for heterogeneity. Consistent with results based on the fixed effect models, I find that the passage of SCO negatively affects retail performance as well as business and employment shares.

The decisions to enact SCOs is likely to be subject to endogenous bias (Besley and Case, 2000). To mitigate this concern, I exploit two instrumental variables (IVs): political composition

and distance to the nearest place with SCO. Both instrumental variables vary by time and across places. I calculate political composition as the deviation from the median value of Democrat's share of two-party votes for each election year. Similar to the one used by Bertrand and Kramarz (2002) and Sadun (2015), this strategy exploits the fact that a decision on passing SCO was influenced by the political attitude toward large stores rather than unobserved factors to the retail businesses. In particular, municipalities with a greater fraction of Democratic shares tend to impose more stringent entry restrictions on big-box stores as Democratic voters are more likely to be concerned about the positive competitive effects generated by big-box retailers, as well as about the negative externalities created by big stores due to increased congestion and crime, urban sprawl and environmental issues.

My second IV exploits the geographical and time-series pattern of passing SCOs across places in the US. The "spreading out" pattern suggests that the passage of SCO is likely to be positively related to the distance to the nearest place that previously passed SCO. I show that the two instruments have an independent effect on regulation change and do not affect the outcome variables. This would help get rid of the previously mentioned drawbacks in fixed effect models and differences in differences estimations. Results based on IVs confirm the negative impact of entry barriers on retail performance, as well as on market structure. In conclusion, the retail trade industry was actually harmed by the creation of entry deterrence against big-box retailers.

The economic magnitude is not trivial. According to the baseline fixed-effect and IV estimates, one standard deviation increase in entry restrictiveness reduces establishment by 2.2%-2.4% and employment by 5%-6.2%. The presence of SCO regulation accounts for 4.3% to 6% of the reduction in establishments and 13% to 17% of the reduction in employment, respectively. Turning to market structure analysis, a one-standard-deviation increase in SCO restrictiveness

decreases establishment shares (employment shares) by 3%-10% (5%-7%). Compared with places without SCOs, places with SCOs have 18%-36% (14%-27%) less percentage of sectoral establishments (employment) in the retail industry as a total of all industries.

I conduct falsifications test and find no evidence that the SCO regulation hinder the performance of another industry, nonstore retailers, which entry regulations should not cover. Lastly, I examine all possible boundary changes among places and verify that my results are largely robust.

This study provides several contributions. First, there is little empirical evidence on the effects of entry regulations in the retail sector and none of the existing studies focuses on the world's largest retail market – the U.S. In this study, SCOs were passed by local governments at different periods of time and that their levels of restrictiveness vary across places. This setting creates a great deal of heterogeneity over time and across places and offers a unique opportunity to study the impact of entry regulation.

Second, this paper adds to the growing literature that studies the impact of big-box retailers on small business and local employment. There is an ongoing debate on the expansion of Wal-Mart on prices (Basker, 2005; Hausman and Leibtag, 2005), wages (Dube, Lester and Eidlin, 2005), employment (Basker, 2005; Neumark, Zhang and Ciccarella, 2008) and mom and pop retailers (Haltiwanger, Jarmin and Krizan, 2010; Jia 2008). Overall, existing studies produce mixed results (see Section 2 for detailed discussions). This paper provides new empirical evidence from a different angle by examining the consequences of entry prevention of big-box retailers on local retail performance and competition.

In addition, and related to the previous point, most of the studies on big-box retailers finding mixed results use county-level data and ignore local zoning regulations. This study is one of the rare empirical studies on regulations at a very fine geographical level. The sharp contrast between results using place fixed effects and those using county fixed effects highlights the importance of controlling for place-level heterogeneity in local zoning regulations on land uses.

Lastly, this paper offers important policy implications on the effectiveness of entry regulation. In many US communities, there has been a concern about the possible negative impact on the size and structure of the retail industry caused by big-box retailers. As a result, many local activists and organizations have been protesting the entry of big-box stores, claiming that local employment and businesses would get protected and improved by the creation of entry barriers. My results suggest that it is not the case, the creation of entry barriers actually harm the local business and employment and hinder the growth of the retail sector.

2. Literature Review

This paper is related to several strands of the literature. Bertrand and Kramarz (2002) were among the first to investigate the effect of planning regulation on employment. They find that stronger deterrence of entry increased retailer concentration and harmed employment growth in the French retail industry. Djankov, La Porta, Lopez-de-Silanes and Shleifer (2002) and Klapper, Laeven and Rajan (2006) provide cross-country evidence that entry regulation procedures restrict competition and are negatively correlated with business start-ups and economic growth. Viviano (2008) use data in Italy and applies a differences-in-differences approach to show that entry regulations have a negative impact on employment growth. In a related study, Schivardi and

Viviano (2011) find that entry barriers are associated with higher profit margins and lower productivity of incumbent firms. Sadun (2015) analyzes a planning reform in the UK in the 1990s. She concludes that entry restrictions have negative impact on independent retailers. Based on the UK data, Griffith and Harmgart (2008) find that entry restrictions reduce the equilibrium number of large supermarkets and are associated with higher food prices. Cheshire, Hilber and Kaplanis (2015) provide more evidence on the effect of planning regulation on the UK retail sector and show a significant and negative effect of regulation on firm productivity. Vidal (2016) exploits a regression discontinuity design and study the commercial regulation in Spain. Her conclusion suggests that unregulated municipalities experienced more openings than regulated ones and the openings of big-box openings drive out grocery stores. A recent study by Maican and Orth (2016 a) studies the welfare effects of entry regulation in the retail food stores in Sweden. They suggest that imposing higher entry costs for large stores is not welfare improving. In contrast, welfare increases when lower entry costs enhance competition.

This paper is also related to a considerable debate about the impact of big-box retailers on retail employment and independent businesses. Basker (2005) suggests that a new Wal-Mart is associated with an increase of 100 jobs in the year of entry, although this gain disappears over the next five years. Neumark, Zhang and Ciccarella (2008) find opposite results as in Basker (2005) and conclude that the effect of Wal-Mart entry on employment and payroll is negative. The magnitude is quite large: a new Wal-Mart reduces retail employment by about 150 workers, a 2.7% reduction in retail employment in an average county and $1.4 million payroll, a 1.5% decrease. Ellickson and Grieco (2013) exploit a detailed panel data and study the impact of entry by Wal-Mart on competition in the supermarket industry. They find that Wal-Mart's entry negatively affect its competitors. However, this effect is quite localized within a two-mile radius. Haltiwanger,

Jarmin and Krizan (2011) find a substantial negative impact of big-box entry on the employment growth if the big-box store is in the immediate area and in the same detailed industry. The negative competitive effect is more pronounced for smaller chain stores. Based on data of retail food stores in Sweden, Maican and Orth (2016 b) find that incumbents' productivity increases after the entry of big-box stores.

Finally, this study is related to market structure and performance in retail trade (for example, Yang, 2016; Blevins, Khwaja and Yang, 2016), retail locations (e.g. Schuetz, 2015; Zhou and Clapp, 2015, 2016) and policy implications of restrictions on large establishments (e.g. Guner Ventura, and Xu, 2008).[9]

3. Institutional Background of Store Cap Ordinances

Since "*Village of Euclid v. Ambler Reality Co*" in 1926, states have been allowing local authorities to enact zoning ordinances. The "Euclidean" zoning was used by local authorities to regulate land uses. Later, "smart growth" ordinances evolved with the purpose of combating the rise of urban sprawl. However, "smart growth" ordinances were complex and difficult to execute. As big-box stores are the most visible symbol of urban sprawl, store size-cap ordinances, which simply limit the square footage of retail stores, became popular because they are easy to execute.[10]

In the face of a rapid growth of big-box retailers in 1990s, store cap ordinances gained popularity. According to activists, a litany of sins of big-box retailers includes massive government

[9] Guner, Ventura and Xu (2008) find that "policies that reduce the average size of establishments by 20% lead to reductions in output and output per establishment up to 8.1% and 25.6% respectively, as well as large increases in the number of establishments (23.5%)." pp 721.
[10] For details of the history of store cap ordinances, see Denning and Lary, 2005.

subsidies, unfair competition, urban blights caused by dark stores and dead malls.[11] It is widely believed that big-box retailers, especially Wal-Mart, negatively affects local employment and wages. Local communities respond to the big-box threats by enforcing store cap ordinance to limit the square footage of retail stores to ensure that big-box stores could not find a way to conform the size-cap of retail stores. Even if they could conform, they have to reduce its competitive edge with substantial costs.

Figure 1 shows the pattern of SCOs. I plot the centroids of places with SCOs in red dots. The figures from 1998 to 2009 illustrate how SCOs spread out geographically throughout the United States. Most of SCOs are concentrated in the East and West Coasts. Within a region, most likely within a State (such as California), we observe that when there is one or two places that passed SCO, more places passed SCOs subsequently. The "regional spreading-out" pattern suggests that SCOs passed by one place is likely to affect the likelihood of the passage of SCO in a neighborhood place.

As the purpose of SCOs is to constrain the size of retail establishments to best protect the local businesses and communities, the size limits vary among places. For example, Bellingham WA, with population of about 71,000 in 2007, enacted an ordinance that prohibited stores over 90,000 sq. ft. Recently, Easthampton, MA, a 16,000-person town, approved an ordinance that capped new retail stores at 50,000 sq. ft. in 2015. In California, there were a few places adopted

[11] It has been widely argued by activists that too much public subsidies from local government distort competition and threaten locally owned businesses. For example, according to Good Jobs First, a national policy resource center which provides information on best practices in state and local job subsidies to promote corporate and government accountability in economic development, Wal-Mart has received over $1 billion subsidies from municipal, county, and state. In many big-box lease agreements, non-compete clauses (such as deed restrictions) forbid property owners from leasing the building to another company without the original tenant's approval. As a result, many empty big-box stores remained vacant for years. Dark stores and dead malls increase crime and vandalism, lower property values nearby and negatively affect local business. There are other types of regulations that help prevent dark spaces of big-box retailers such as demolition bond and dark store ordinances. This paper only focuses store cap ordinances, one of the most effective zoning regulations preventing entry of the big-box retailers.

"grocery ordinances", which ban stores devoting a certain percentage of their store footage to grocery items. For example, in 2004 the Turlock City Council in California adopted an ordinance that prohibits stores over 100,000 sq. ft. that devote at least 5 percent of floor space to non-taxable items (groceries). Later, Wal-Mart filed a lawsuit, claiming that the policy was unconstitutional because it interfered with the company's right to conduct interstate commerce and discriminated against a specific type of business, in violation of the equal protection clause. In 2006, the ordinance was upheld by both state and federal courts. Other examples of "grocery caps" in California include Elk Grove (150,000 sq. ft. with at least 10 percent of floor space to groceries), Galt (140,000 sq. ft. with at least 10 percent of floor space to groceries), Santa Clara (80,000 sq. ft. with at least 5 percent of floor space to groceries) and Stockton (100,000 sq. ft. with at least 10 percent of floor space to groceries).

Store cap ordinances could be passed by local governments at municipality or county level and their stringency vary across places. For example, Mount Shasta, CA enacted the ordinance which caps stores at 50,000 sq. ft. in March 2005. In September 2008, Whatcom County, WA adopted a store size cap of 65,000 sq. ft. in urban growth areas and no more than 35,000 sq. ft. in the rest of the county. According to the Retail Industry Leaders Association (RILA), 52 localities in 20 states had ever passed or proposed size-cap ordinance as of August 2004. The average size cap is about 50,000 sq. ft.

One might question that these mega stores could divide a standard store into smaller ones in order to get around the size cap regulation. In fact, most SCOs also ban two or more adjacent buildings operated by one company as a single retail stores and leave no loophole in practice. For example, in 2005 Wal-Mart proposed two separate stores, standing next to each other, in town of Dunkirk, Calvert County, Maryland, which limit retail stores to 120,000 sq. ft. However, the

company finally dropped its proposal due to the public outcry.[12] Another concern might be that big-box retailers would force a referendum and try different tactics to overturn the SCOs. But, this strategy rarely succeeded. The only two overturn cases in my sample are Contra Costa, CA and Ravalli County, MT. The former ban big-box stores larger than 90,000 sq. ft. in 2003. The ordinance was overturned in 2004. The size limit of 60,000 sq. ft. in the latter was adopted in April 2006 but quickly overturned in November of the same year.[13]

4. Data

As most of SCOs were passed by local governments and municipalities, the unit of analysis in this paper is an incorporated place ("place"), which refers to a self-governing city, town, village or borough.[14] Incorporated places are chartered by States. Based on Census 2010, there are 19,505 incorporated places in the US and an average place is of 6.14 square miles in land areas and contains 4,095 households and 9,669 people. On average, a place is more than 10 times smaller than a county.[15]

[12] http://lubbockonline.com/stories/032705/bus_032705114.shtml#.VvlkxeIrLIU (last accessed: March 3, 2016)

[13] These two cases should not affect my analysis because my sample periods are 5-year intervals and I lag the SCO variables by one year. For example, a place needs to have the SCO regulation in effect at least from 2006 in order to affect the results of the 2007 Economic Census. Nevertheless, I delete these two cases in the analyses.

[14] According to Census, a place could be an incorporated place or a census-designed place (CDP). I only focus on incorporated places with an active government providing primary general-purpose functions. CDPs are areas that lack separate municipal government and designed for statistical purposes. The sample also includes towns in the New England states, Wisconsin and New York, which have government unites on a similar level as incorporated places. Note that as SCOs could be passed at county level, some CDPs would also be entitled to SCO regulations. However, these CDPs are deleted from the analysis.

[15] Based on Census 2010, there are 3,221 counties in the US and an average county is of 1,097.59 square miles (land areas) and contains 41,397 households and 97,011 people. Incorporated places are chartered by States and no place may extend into more than one State. Multi-county places are possible. In later analysis, I match each place to its nearest county by calculating the distance between the centroid of the place and that of the county.

Data on store cap regulations are hand collected from three different sources. First, Institute for Local Self-Reliance (ILSR), a non-profit organization and advocacy group aims to strength communities development and promote local economies, provides news articles on anti-big-box stores.[16] Second, Sprawl-Busters, an Anti-Wal-Mart organization collecting news about anti-big-box store protests from a variety of sources (such as media reports, court results, independent institution's reports and government information releases) since 1998. In addition, a rigorous search on Lexis-Lexus was performed on keywords such as "retail or store size cap," "retail or store size cap ordinance," and "retail or store size limit." Details of the SCO regulation include the year of passage, size limit of retail stores and other accompanying restrictions. Cases are verified among different sources and duplicated cases are dropped.[17]

To measure the level of restrictiveness, I went through each regulation and for each place constructed a proxy, *SCO restrictiveness*, as the ratio of the total employment (prior to the SCO adoption) to the size limit under the SCO. A higher level of *SCO restrictiveness* (the ratio of total employment to the permissible square footage) represents less big-box areas permitted per a potential employee and therefore a greater restrictions on entry.[18] For places without SCO, this proxy is set to be zero. I use this variable as my preferred measure of entry barriers. One complication is that size caps could be applied either to place-level or to an entire county. In addition, within a city, ordinances could vary across neighborhood areas. I carefully investigate the details and assign size limits that applied at county-level to individual places.[19] There were

[16] For example, https://ilsr.org/search/?q=store%20cap (last accessed: March 3, 2016).
[17] In some of the cases, store cap regulations were passed at county level. I manually match with their corresponding incorporated places. For example, according to ILSR, Calvert County, Maryland "has enacted regulations limiting retail stores to 120,000 sq. ft. in the town of Prince Frederick, 75,000 sq. ft. in three other towns, and 25,000 sq. ft. in the rest of the county." Therefore, I create my SCO variables based on these details. (Source: https://ilsr.org/maryland-county-mandates-smaller-stores/). (last accessed: March 3, 2016)
[18] The results are robust to an alternative measure based on total population.
[19] There were 10 counties that applied SCOs at the county-level.

only 4 cases where the degree of restrictiveness varied across neighborhoods within the city. I apply the average size limit to these places.[20] Four places in California had enacted only "grocery caps" without indicating the exact amount of size limit. For such cases, I estimate the size limit by dividing the median of grocery proportions among the places indicated limits on both total spaces and grocery spaces. In total, there were 247 places that passed store cap regulations during 1997 to 2012.

My outcome variables include different measures of retail sectoral performance, including number of establishments, employment, sales and payroll. Based on Hortaçsu and Syverson (2015), the retail sector is defined as "retail trade" by the North American Industry Classification System (NAICS) as "engaged in retailing merchandise, generally without transformation, and rendering services incidental to the sale of merchandise." I focus on 20 4-digit NAICS sub-sectors in the retail trade industry (NAICS=44-45) that potentially compete with "big-box" stores such as supercenters and department stores (e.g. Wal-Mart and Target), warehouses (e.g. BJs and Costco) and "category killers" (e.g. Home Depot).[21] In fact, my sample includes all the 4-digit sub-sectors except Motor Vehicle and Parts Dealers (NAICS=4411, 4412 and 4413), Gasoline Stations (NAICS=4471), and Nonstore Retailers (NAICS=4541, 4542 and 4543).

All the outcome variables are collected from the Economic Census. As the Economic Census is conducted every 5 years and the majority of the SCO regulations were passed in late 1990s and 2000s, my sample period starts from 1997 and ends in 2012. According to the Economic

[20] Because there were only 4 cases, my results are robust when I apply either the minimum or the maximum.

[21] The reason of focusing on 4-digit NAICS level instead of 5-digit or even 6-digit level data is that data are often suppressed by the Census in order to protect the confidentiality of information reported by individual businesses. In addition, there were few changes at 5-digit NAICS level. For example, some categories were no longer used in the recent Economic Census due to functional obsolesces. Examples include Prerecorded Tape, Compact Disc, and Record Stores (NAICS=45122).

Census, a retail establishment is a single physical location at which business is conducted. Employment is the number of paid employees (both full-tome and part-time). Sales refers to sales revenue and receipts from merchandise sold for cash or credit. Payroll includes all forms of compensation such as salaries, wages, commissions, dismissal pay, bonuses, vacation allowances, sick-leave pay, and employee contributions. Data on sales and payroll are converted to 2000 constant dollars.

Table 1 presents statistics on the timing, geographical variation, and clustering of SCOs. The first and second rows show the number of places, counties and States that passed SCO in each five-year period. Most of places passed SCOs during 2002-2007. The SCO passage has substantial geographical variation: they were passed in 29 States. During my sample period, SCOs were passed in 24 different States. The third row shows the distinct number of counties that passed SCO. As places are defined directly under "State" instead of under "county", I match the centroids of places with those of counties. For each place, I pick the nearest distance pair as its corresponding county. Despite a substantial variation among States, the SCO adoption within States was quite concentrated: it clustered within 86 out of 1,634 counties in the 29 States. Figure 1 provides a visual illustration on this "spreading out" pattern.

The fourth row of Table 1 shows that SCO regulations have become less restrictive over time. The median square footage limit passed before 1997 is 47,500 sq. ft., compared with 75,000 sq. ft. in 2002-2007 and 2007-2012. Consistently, the fifth row shows that the restrictiveness had been decreasing over the sample period.

Table 2 compares summary statistics of the outcome variables and control variables between "places with SCOs" and "places without SCO" and explore factors driving the cross-sectional heterogeneity in entry regulation. The comparison shows that places with SCOs have

larger number of establishments and employment in all sectors.[22] Turning to the retail sector, places with SCOs also have higher numbers in establishment, employment, sales and payroll.[23] To explore the market structure, I construct "%" variables as the number of sectoral establishments (or employment) divided by the total number in all sectors. Results show that places with SCOs have relatively smaller shares of businesses in the retail sector.

I construct a set of time-varying variables to capture the differences in the economic and demographic characteristics. The preferred measures would be at place level. As the Decennial Census is the only source that provides place-level economic data, I interpolate and extrapolate data based on Census 1990, 2000 and 2010 and match them with the Economic Census.[24] Panel A shows statistics based on the full sample. Panel B shows statistics based on a restricted sample (used in later multivariate analyses) which drops places that passed SCOs before 1997, with less than two observations, with initial population less than 1,000 and outside the 24 states that passed SCOs during 1997-2012. In both Panel A and Panel B we observe a substantial heterogeneity in observable characteristics: places that passed SCOs are larger, have more population, higher population density, higher per capita income (in 2000 constant dollar) and lower unemployment rate. Overall, the passage of SCO appear to be related to place characteristics and it is important to control for the observable heterogeneity.

[22] All sectors in NAICS include 11 – Agriculture, Forestry, Fishing and Hunting, 21 – Mining, 22 – Utilities, 23 – Construction, 31-33 – Manufacturing, 42 – Wholesale Trade, 44-45 – Retail Trade, 48-49 – Transportation and Warehousing, 51 – Information, 52 – Finance and Insurance, 53 – Real Estate Rental and Leasing, 54 – Professional, Scientific, and Technical Services, 55 – Management of Companies and Enterprises, 56 – Administrative and Support and Waste Management, 61 – Educational Services, 62 – Health Care and Social Assistance, 71 – Arts, Entertainment, and Recreation, 72 – Accommodation and Food Services, 81 – Other Services and 92 – Public Administrations.

[23] Only establishment and employment are the two common variables for all the sectors. Data on sales and payroll are not available for "all sectors".

[24] Alternatively, I could match Census 1990 with Economic Census 1997, Census 2000 with Economic Census 2002 and Census 2010 with Economic Census 2012. The drawback is that Economic Census 2007 will be dropped because there is no matching data from Census. In addition, the time lags between these three pairs are different. Nevertheless, I use this method to construct the control variables and find similar results.

5. The Impact of Store Cap Ordinances: Basic Results

5.1. Empirical Methodology

My basic empirical approach consists in using observed entry barriers as measures of the stringency of the entry regulation. I start with the following basic panel regressions with location fixed effects, industry fixed effects and year fixed effects:

$$y_{ijt} = \theta\ SCO\ Restrictiveness_i \times D_{it} + \delta X_{it} + \alpha_i + \beta_t + \gamma_j + \varepsilon_{ijt} \quad (1)$$

$$y_{ijt} = \theta\ SCO\ Dummy_i \times D_{it} + \delta X_{it} + \alpha_i + \beta_t + \gamma_j + \varepsilon_{ijt} \quad (2)$$

where y_{ijt} is the outcome variable in place *i*, industry *j* and year *t*, $SCO\ Restrictiveness_i$ is the measure of entry restrictiveness of place *i*, $SCO\ Dummy_i$ indicates if SCO was passed in place *i*, D_{it} is a dummy equals one if the SCO was passed in place *i* in year *t*. I lag this variable by one year in order to allow for a lag effect.[25] X_{it} is the a vector of time-varying characteristics, α_i are place fixed effects, β_t are year fixed effects, $\gamma_j$are industry fixed effects and ε_{ijt} is an error term.

The objective of fixed effect model is to examine whether retail performance are affected by the stringency of SCO (Equation (1)), as well as simply by the presence of SCO (Equation (2)). For the outcome variables, y_{ijt}, I focus on four key measures of performance: (1) establishment, (2) employment, (3) sales and (4) payroll, as well as two measures of retail market structure: (1) % establishment and (2) % employment, defined as the number of retail sectoral establishments or

[25] As my time periods are five-year intervals, my results are not sensitive to alternative lags. For example, the effect of a SCO adoption during 1997-2001 would be reflected in the 2002 Economic Census. Nevertheless, in robustness tests I replace the requirement of "at least one year" with "at least two years" and "at least three years". The results are qualitatively similar.

employment divided by the total number of all sectors. Unfortunately, similar percentage measures could not be calculated using sales and payroll either because these data are not applicable or because data are aggregated differently across industries.[26] The year dummies capture aggregate economic shocks as well as any policy change that apply to all the places and might affect the outcome variables. Location and industry fixed effects control for any time-invariant factors that might affect the level of establishment and employment in a given location for a given industry. Such factors are for example the size of the place and the retail stock that was in place prior to the passage of SCO. Control variables, X_{it} are designed to capture economic characteristics following the literature and based on the comparison between places with and without SCO in the previous section. Because the Economic Census is conducted every 5 years and economic variables at place level are published by the Census every 10 years, I interpolate and extrapolate data based on the 1990, 2000 and 2010 Census. Standard errors are clustered at place level to control for autocorrelation patterns of unknown form (Bertrand, Duflo and Mullainathan, 2004).

The key coefficient estimate is θ in equation (1) and (2). Under the assumption that conditional on the other controls, $SCO\ Restrictiveness_i \times D_{it}$ and $SCO\ Dummy_i \times D_{it}$ are uncorrelated with ε_{ijt}. The coefficient θ identifies the effect of entry barriers created by SCO on the outcome variables. My basic empirical approach relies on the assumption that SCOs are passed exogenously. In other words, a regulation change affects outcomes for identifiable subjects who are otherwise indistinguishable from those not directly affected by the regulation change. Therefore, the effects of SCO are identified by comparing outcomes over time, across industries and across places. There are several reasons why this assumption might be violated in practice. In

[26] For example, no data on sales is available for the construction industry (NAICS=23).

Section 7 I discuss these reasons in detail and apply an instrumental approach that attempts to isolate a more exogenous source of variation in the restrictiveness of entry barriers.

5.2 Results

Table 3 summarizes the results of the estimation of the two basic regressions (1) and (2). As described in Table 2, the sample used in multivariable regressions drops places that passed SCOs before 1997, with less than two observations, with initial population less than 1,000 and outside the 24 states that passed SCOs during 1997-2012. The dependent variables are the four measures of retail performance: log of establishments in Column (1), log of employment in Column (2), log of sales in Column (3) and log of payroll in Column (4). The test variables are *SCO Restrictiveness* in Panel A and *SCO Dummy* in Panel B which equal zero in pre-regulation periods and only turns on in post regulation periods.[27] The sample covers 20 4-digit NAICS industries and 4,042 places. All the regressions are estimated by OLS with place fixed effects, 4-digit NAICS fixed effects and year fixed effects. Standard errors are clustered at place level and *t*-statistics are reported in parentheses.

In Column (1) through (4) of Panel A, all the coefficient estimates are negative and statistically significant, suggesting that higher entry barriers of big-box retailers are associated with lower establishments, less employment, smaller sales volume and less payroll to employees. Given that the dependent variable is in logarithms, the effect of one-standard-deviation change in *SCO Restrictiveness* on retail establishment is 2.2%, calculated as the standard deviation of *SCO*

[27] For brevity, I simply use "SCO Restrictiveness" to indicate "$SCO\ Restrictiveness_{\ i} \times D_{it}$" and use "SCO Dummy" to indicate "$SCO\ Dummy_{\ i} \times D_{it}$".

Restrictiveness (0.18) times the estimated coefficient of 0.123 in Column (1). The effect on employment shown in Column (2) is even bigger. A one-standard-deviation increase in *SCO Restrictiveness* is associated with a 5% reduction on employment (=0.18*0.266). Similarly, a one-standard deviation increase in *SCO Restrictiveness* would reduce sales by 2.5% and payroll by 1.6%, shown in Column (3) and (4), respectively.

In Panel B, results based on *SCO Dummy* are consistent with those in Panel A, except that the coefficient estimates in model specifications using sales and payroll as dependent variables (i.e. Column (3) and (4)) are no longer significant. In Column (1), I find that the number of establishments in places with SCO is 6% less than that in places without SCO. Results in Column (2) suggest that the presence of SCO regulations are related to 17% less employment, a quite sizable effect.

In both Panel A and B, control variables are of the expected signs. All the four measures of performance are positively associated with population and per capita income. Establishment and employment (sales and payroll) are decreasing (increasing) in unemployment rate and population density.

Does SCO affect the market structure in the retail industry? Table 4 shows the results based on retail establishments (employment) as a percentage of total establishments (employment) of all sectors. Again, the test variables include *SCO Restrictiveness* in Column (1) and (2) and *SCO Dummy* in Column (3) and (4). The negative and significant coefficients on the test variables indicate negative responsiveness to the entry deterrence created by SCOs. Noted that the dependent variable is calculated as the number of establishments (or employment) in 4-digit NAICS subsectors in the retail trade industry divided by the total number of all industries. The economic magnitudes of these estimated coefficients are, in fact, far from negligible: an average place has a

"% establishment" of 0.55% and "% employment" of 0.73%, implying that a one standard-deviation increase of *SCO Restrictiveness* reduces the percentage of sub-sector retail establishment by 3% (=0.18*(0.1%/0.55%)) and the percentage of sub-sector retail employment by 5% (=0.18*(0.2%/0.73%)), shown in Column (1) and (2) respectively. Results in Column (3) and (4) suggest that the presence of SCO reduces establishment shares (employment shares) by 0.2% (0.2%), a 36% (27%) reduction relative to the mean.

Overall, my baseline results suggest that the entry deterrence created by SCO exert a strong effect on performance as well as on market structure in the retail industry. The entry barriers are negatively associated with establishment, employment, sales and payroll. The creation of entry deterrence also changes the market structure: compared with places without SCO, places with SCOs have less retail establishments and employments as a percentage of all industries. The results are statistically and economically significant.

As most of prior studies on big-box retailers use county-level data and ignore the local zoning regulation, I examine county-level evidence by constructing county-level time-vary variables based on the US Census and Bureau of Economic Analysis (BEA). Results are summarized in Appendix 1. Interestingly and surprisingly, the effect of SCO on the outcome variables are positive and significant when county fixed effects and county-level time-vary controls are used, suggesting the importance to control for place-level heterogeneity in local zoning regulations on land uses.

Equation (1) and (2) are based on the assumption that SCOs were a binding constraint on the retail sector and consequently affected market structure. It might be that a market is close to saturation in terms of big-box retailers. If it is the case, a high level of SCO restrictiveness would not necessarily imply a high entry barrier because the incentive to enter is low anyway. I believe

that this is not the case. As the Economic Census does not provide data on big-box retailers there is no way to check this issue directly. However, if places with SCOs have more big-box retailers, we expect to find a higher number of employees per establishment on average. I conduct a preliminary instigation and find that, although the average employment per establishment in places with SCOs is 11.8, slightly higher than 11.2 in places without SCO, the differences are not statistically significant. I also examine the sub-sector of "General Merchandise Stores" (NAICS=452), which consists of primarily big-box retailers such as department stores, supercenters and warehouses. By comparing places with and without SCO, I find similar levels of employment. Furthermore, places with SCO have even lower sales from the sector of "General Merchandise Stores" per capita ($1,080 versus $1,010). Again, these differences are not statistically significant.

One could argue that the SCO regulation might be correlated with different trends (as opposed to levels) in local retail performance. For example, places where retail business are growing relatively faster are those where incumbents exert more pressure for entry barriers. To support the validity of this identification assumption against correlated differences in trends, I repeat all the regressions by shifting the value of the test variables backward. For example, for a place that passed SCO in 2010, of which the test variable would be turned on in the 2012 Economic Census, I now assign the value to the 2007 Economic Census and repeat the analysis. If entry regulation is correlated with unobservable factors also determining the trend of the outcome variables, one would reasonably expect that these factors influenced retail performance even before the adoption of SCO. The finding of insignificant coefficients of the test variables (unreported) support the lack of correlation between the SCO adoption and past differences in trends.

6. The Impact of Store Cap Ordinances: Differences-in-Differences

6.1. Empirical Methodology

In this section, I apply a differences-in-differences (DID) approach to circumvent the potential endogeneity problems that arise when making comparisons between heterogeneous places (Meyer, 1995). The treatment group includes places that passed SCOs during 1997-2012. Each place in the treatment group is matched with places that never passed SCO in the same county, as the control group, in order to better control for heterogeneity. Similar to the data requirement in the previous section, I delete places that passed SCOs before 1997, with less than two observations and with initial population less than 1,000. I estimate six model specifications for the following outcome variables: (1) log of establishments; (2) log of employment; (3) log of sales; (4) log of payroll; (5) number of establishments as a percentage of total in all sectors and (6) number of employment as a percentage of total in all sectors. For each outcome variable, denoted y_{icjt}, I estimate the following equation:

$$y_{icjt} = \theta\, SCO_i \times Post_{it} + \varphi SCO_i + \sigma Post_{it} + \delta X_{it} + \alpha_c + \beta_t + \gamma_j + \varepsilon_{ijt} \qquad (3)$$

where y_{icjt} is the outcome variable in place *i*, county *c*, industry *j* and year *t*, SCO_i is a dummy variable equals one for the treatment group which consists of places with SCOs and zero for the control group, $Post_{it}$ is a post-regulation dummy which takes the value of one for post-regulation periods and zero otherwise. Again, I lag this variable by one year to account for the lag effect. $SCO_i \times Post_{it}$ is an interaction term equal to the product the post-regulation dummy and the treatment group dummy. The interaction term is equal to one if the place has passed the SCO regulation *and* data refer to the post-regulation period and equal to zero otherwise. Thus, θ is the

differential effect on the outcome variables due to the entry regulation. X_{it} is the a vector of time-varying characteristics, α_c are county fixed effects, β_t are year fixed effects, $\gamma_j$are industry fixed effects and ε_{ijt} is an error term. The standard errors of the regressions are clustered at the place level, following Bertrand, Duflo and Mullainathan (2004).

6.2. Results

Results in Table 5 are highly consistent with the previous findings based on the fixed effect models: the DID estimators suggest that the passage of SCO exerts a negative and significant impact on the number of establishments, the number of paid employees, as well as sectoral shares of establishment and employment. The coefficient estimates of sales and payroll are negative and close to marginal significant. After the SCO adoption, the number of establishments (number of paid employees) decreases by 9% (17%) and the percentage of sectoral establishments (percentage of sectoral employment) relative to the total number of all sectors decreases by 0.1% (0.1%), a 18% (14%) reduction relative to its mean.

There are two strong identification assumptions in the DID estimation. First of all, except for the control variables, there are no other forces affecting the treatment and the control groups differently pre- and post-treatments. Second, the composition of the treatment and control group must remain stable over the period. In other words, the outcome variables such as employment in places that passed SCO regulations and in places without SCO grew at the same rate before the regulation and that the differences observed after the regulation are caused only by the regulation. If the two groups are affected by different trends in a period preceding the regulation, one could find statistically significant but spurious treatment effects even when no treatment occurs. A

possible solution is to select the control group very similar to the treatment group. The requirement of the control group to be places within the same county, a relatively small geographical region, might partially mitigate the concern that the regulation is not influenced by the characteristics of the retail trade sector. In the next section, I pursue an instrumental variable approach that isolates the variation in entry barriers determined by SCO from that determined by local economic conditions.

7. The Impact of Store Cap Ordinances: Instrumental Variables

7.1. Empirical Methodology

It is noted that models with fixed effects and DID estimations have drawbacks that might generate biased coefficient estimates (Besley and Case, 2000). The decision by a municipality to enact SCO is likely to subject to endogenous bias. Fixed-effect models that directly include the regulation change in an incidence equation might leave estimates open to omitted variable bias. Any omitted variables that potentially affect both the regulation and the outcome variables may bias estimates of the regulation's effect. According to Besley and Case (2000), there are two potential sources of bias: (1) omitted variable bias caused by observable variables that affect the regulation and that have independent effect on the outcome variables and (2) omitted variable bias caused by unobservable variables that determine both the regulation and the outcome variables. In a DID estimation, two potential sources of bias include: (1) omitted factors that might affect the treatment and the control groups differently pre and post; (2) the composition of the treatment and the control group might be unstable pre and post (i.e. the regulation works in a different way in the two groups). Specifically, the bias could go either way. If incumbents in declining places advocate

for the creation of entry deterrence, this might result in a downward bias of the fixed-effects estimates; if incumbents who are enjoying large profits spend more resources on lobbying for barriers, this would result in an upward bias.

I exploit two time-varying IVs: (1) political composition and (2) distance to the nearest place that passed SCOs. The first IV of political composition is based on the deviation from the median value of Democrat's share in two-party votes in each election year.[28] Similar to the ones used in Bertrand and Kramarz (2002), Schivardi and Viviano (2010) and Sadun (2015), this strategy exploits the fact that the decision on the SCO adoption was influenced by the political attitude toward large stores rather than unobserved factors to the retail businesses. In particular, local authorities with a higher Democratic share of two-party votes are more likely to pass SCOs. Democratic voters are more likely to be concerned about the positive competitive effects generated by big-box retailers, as well as about the negative externalities created by big stores due to increased congestion and crime, urban sprawl and environmental issues.

Data of political composition are based on congressional districts (CDs), areas from which people are elected to the U.S. House of Representatives. Members of the House of Representatives serve two-year terms. The number of CDs in each State can change after a decennial census in order to be as equal in population. For example, there are 435 CDs in the 114th Congress for the duration from January 2015 to January 2017. For a given election term, I calculate the median of Democrat's shares among all the CDs. Then for each CD I calculate the deviation from the median. Based on the CD Shapefiles provided by the Census, I calculate the centroid of each CD by taking the average of the maximum and minimum of its latitude and longitude, respectively. Next, I create

[28] I thank Gary Jacobson for generously sharing data on political composition.

all the possible place-CD pairs within the same State and calculate the distances. I keep the pair with the shortest distance and use the value in the prior election as an instrumental variable.[29] For example, the 2002 Economic Census data is matched with the 2000 election.

My second IV exploits the geographic pattern of the SCO adoption over time. Based on the discussions of Figure 1, a SCO passed by one place is likely to affect the likelihood of the SCO adoption in its neighborhood places. In particular, the "spreading out" pattern predict a positive relation between the probability of the SCO passage and the distance to the nearest place that have enacted such a regulation. Therefore, for each place without SCO, I calculate distances to all places with SCOs. I repeat this procedure for each period and use the shortest distance in the previous year as the second IV. For example, for a place with no entry barriers in the 1997 Economic Census, the IV is the distance to the nearest place with SCO in 1996. The endogenous effect of regulation change is identified by using the time-vary distance to the nearest place that had adopted SCO. This method is in a similar spirit of Neumark, Zhang and Ciccarella (2008), in which they instrument for the opening of Wal-Mart with interactions between time and the distance between a new store and Wal-Mart's headquarter, Benton County, Arkansas.[30]

[29] If a value is missing, I replace it with the nearest lag of non-missing value.

[30] Noted that my method is not subject to the criticism on Dube, Eidlin and Lester (2005) made by Neumark, Zhang and Ciccarella (2008). Dube, Eidlin and Lester (2005) estimate the effect of Wal-Mart on retail earning growth and instrument for the number of stores per person with distance-time interactions. Neumark, Zhang and Ciccarella (2008) criticize that this model is misspecified because it is openings of Wal-Mart that predicted by the distance-time interactions, not the total number of stores. Although I estimate models in levels, my endogenous variable is regulation change, instead of the number of stores.

7.2. Results

Table 6 presents the IV regressions with outcome variables of retail trade performance. In Panel A, the endogenous variable is *SCO Restrictiveness*. To implement the IV I interact the instrument with the post-regulation dummy because the measure of entry barriers is only turned on for this period. The results of the first stage regressions are summarized in the first half of the table.[31] All regressions include, in addition to the IVs, year fixed effects, place fixed effects, industry fixed effects as well as time-varying controls. Standard errors are clustered at place level.

The results of first-stage regressions suggest that political composition is an important determinant of the SCO adoption: places with a larger representation of the Democratic Party is more likely to pass SCO. Consider Column (1), for example. A 1 percent increase in the excess political representation of the Democratic Party increases the *SCO Restrictiveness* by 1.2 percent. In addition, the entry barriers are increasing in the distance to the nearest place with SCO.

For the instruments to be valid, they need to be correlated with the decision on SCO, but uncorrelated with other influences on establishment and employment. I report Hansen *J* Statistics for over-identification tests, Cragg-Donald Wald *F* statistic for weak instrument tests and Kleibergen-Paap rk *LM* statistic for under-identifications tests. The statistics suggest that the over-identification restrictions are valid and the IVs are not subject to weak instruments problems.

Turning to the second-stage results, the IV coefficients in Panel A of Table 6 are larger than the fixed effects ones both in Model (1) (-0.133 versus -0.123 in Column (1) Panel A Table 3) and in Model (2) (-0.347 versus -0.266 in Column (2) Panel A Table 3). The one-standard

[31] In fact, the first-stage results are identical if the model has the same endogenous variable and the same sample size. For example, Model (1) and (2) in Panel A should have the same coefficient estimates.

deviation effects are 2.4% and 6.2%, respectively. The more negative IV estimates suggest that the correlation between the omitted variables and the decision of enacting SCO and the correlation between the omitted variables and the outcome variables are of the same signs. Places with better perspectives in the retail market are more likely to pass the SCOs. As a result, omitting any unobservable market perspectives is likely to bias our previous results upwards. However, in Column (3) and (4), the coefficients of instrumented *SCO Restrictiveness* are statistically insignificant. Together, the results suggest that entry barriers reduce the number of retail establishments and employment but have no effect on sales and payroll. It might suggest that the entry deterrence created by SCOs increases prices, labor costs and profit margins among incumbent firms, as documented by Schivardi and Viviano (2010). Results based on instrumented *SCO Dummy* presented in Panel B lines up with these findings.

Table 7 displays the effects of entry deterrence on retail market structure in the IV regressions. Column (1) and (2) suggest that a one standard-deviation increase of instrumented *SCO Restrictiveness* reduces the percentage of sub-sector retail establishment as a total of all sectors by 10% and the percentage of sub-sector retail employment by 7%, respectively. Results in Column (3) and (4) suggest that the creation of entry deterrence by SCO (instrumented) reduces the sectoral percentage by 6% for establishment and by 7% for employment. Again, the magnitudes of the IV estimates are bigger than the fixed-effect estimates. The magnitude of IV estimates in models on market structure are comparable to those estimated by the DID approach in Section 6.

8. Robustness and Falsification Tests

I conduct a falsification exercise in which I examine another sector that should not be affected by the entry barriers of big-box retailers, and verify that the SCO adoption had a negligible effect on it. I examine the sector of Nonstore Retailers (NAICS=454), including Electronic Shopping and Mail-Order Houses (NAICS=4541), Vending Machine Operators (NAICS=4542) and Direct Selling Establishments (NAICS=4543). I summarize the results based on fixed effect models and the IV results in Table 8. The coefficient estimates of my test variables are insignificant in all the model specifications. These estimates provide evidence against the presence of unobservable heterogeneity affecting the IVs that is correlated with the passage of SCO.

I also notice that there were boundary changes at places during my sample period. The two major categories of boundary changes are annexation and detachments. Annexation involves the transfer of territory outside the jurisdiction of municipality into an incorporated place. Detachment is the reverse of annexation, whereby an incorporated place relinquishes territory to another jurisdiction. Due the growth of the cities, annexations occur considerably more frequently than do detachments.[32] In fact, this pattern of boundary changes should run against my previous results because if SCOs were passed in fast growing cities my estimates would bias towards zero. After deleting the places with boundary changes, most of which were annexations, the coefficient estimates should be more negative.

To verify this issue directly, I hand collect information on boundary changes from the Census. [33] I delete observations with annexations, detachments, disincorporation, territory

[32] An expansion (annexation) in one incorporated place does not mean a contraction (detachment) in another because most of annexations in my sample are between incorporated places and census designated places (CDPs).
[33] For details please refer to https://www.census.gov/geo/reference/boundary-changes.html

exchanges, mergers, consolidation and new incorporation and reproduce the FE and IV results in Table 9. The results based fixed effect models clearly support my previous argument: including fast growing places in my sample is likely to bias against finding the negative impact of entry barriers on performance measures and market structure in the retail trade industry. Lastly, the IV results are highly consistent and similar to the IV results in the previous section.

9. Conclusion

In many communities, activists and organizations have raised serious concerns about the negative impact on the size and structure of the retail industry caused by big-box retailers. They have been protesting the entries of big-box stores and claim that local employment and businesses would get protected and improved by the creation of entry barriers. Does entry regulation of big-box stores protect the retail sector? In this paper, I exploit regulation changes in store cap ordinances at municipality level in the US from 1997 to 2012. The stated objective of SCOs is to constraint the size of big-box stores and to protect and promote local business and employment. However, I find that the entry prevention of big-box stores does not protect retail employment and small business. In contrast, the retail trade industry was actually harmed by the creation of entry barriers. My results are robust to different estimation methods and identification strategies.

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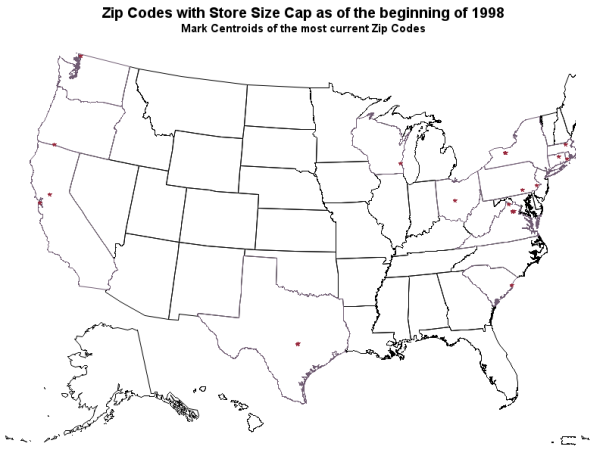
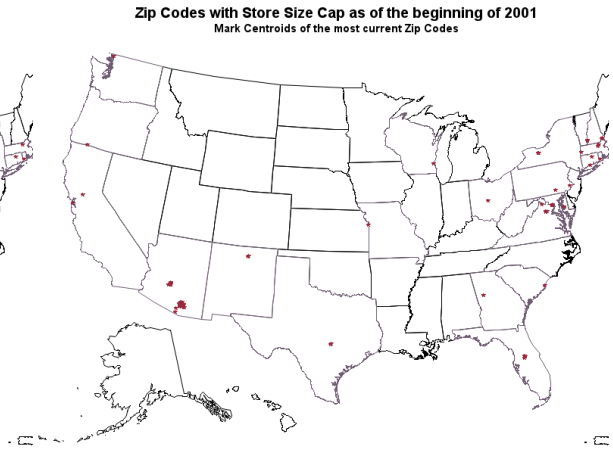
Figure 1: Pattern of Store Size Ordinance, 1998-2009

1998



2001



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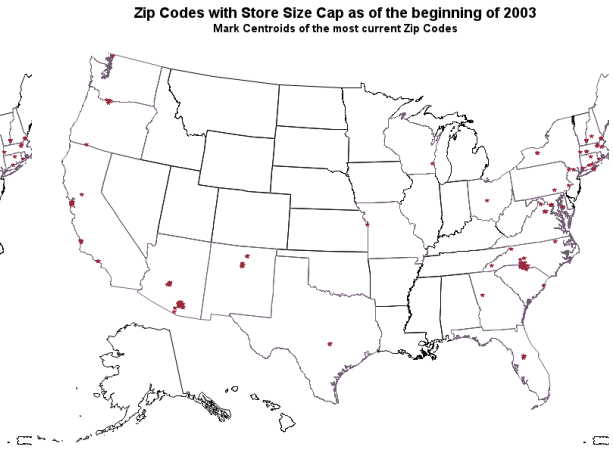

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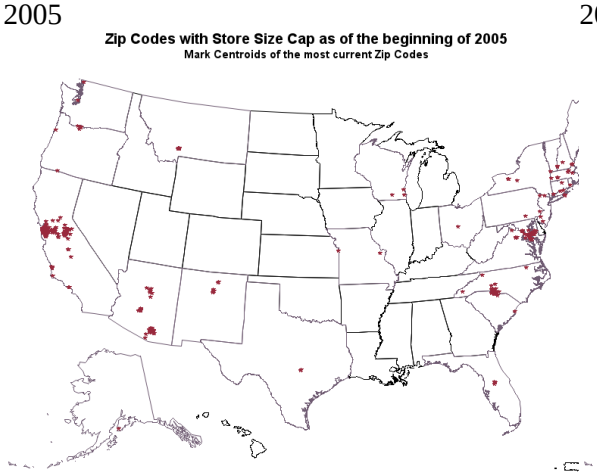
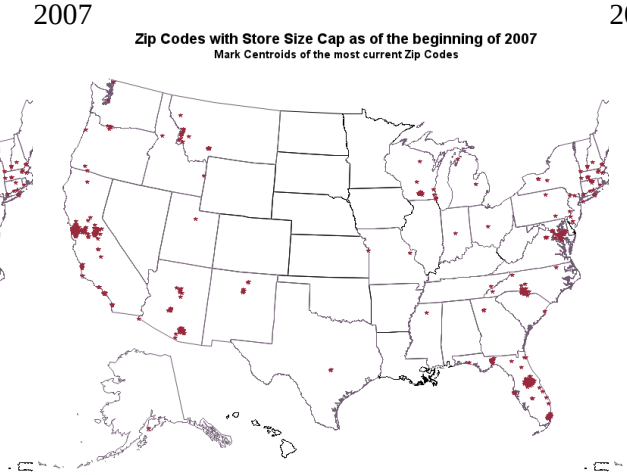


2007



2009

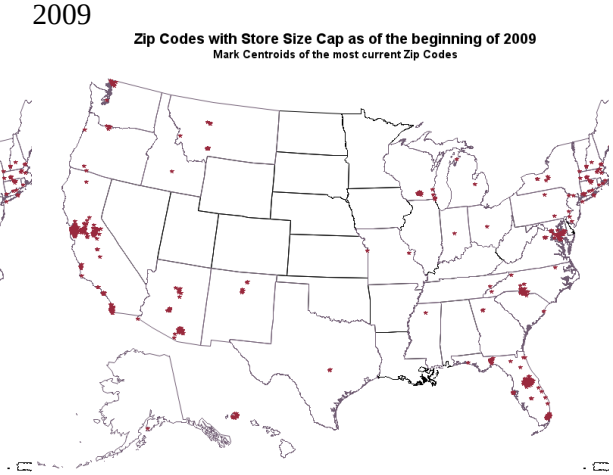


Table 1: SCO Adoption

This table presents statistics on the timing, geographical breadth, clustering of SCO adoption and restrictiveness of SCO during the sample period of 1997-2012. SCO Restrictiveness is the ratio of the total employment (prior to the SCO adoption) to the size limit under the SCO. A higher level of SCO restrictiveness represents a greater restriction on entry.

	Before 1997	1997-2002	2002-2007	2007-2012	Full Sample
Number of places passed SCO	16	28	170	33	247
Number of States passed SCO	12	13	20	9	29
Number of counties passed SCO	15	21	46	14	86
Median sq. ft. limit under SCO regulation	47,500	67,500	75,000	75,000	75,000
Median SCO Restrictiveness	0.0903	0.0624	0.0370	0.0276	0.0413

Table 2: Summary Statistics

This table compares key variables in places with and without store cap ordinances (SCOs). The averages are computed across places that passed SCOs and across places without SCO during 1997-2012. Panel A shows statistics based on the full sample. Panel B shows statistics based on a restricted sample which deletes places that passed SCOs before 1997, with less than two observations in the 1997-2012 Economic Census, with initial population less than 1,000 and outside the 24 States that passed SCOs during 1997-2012. "log of establishments" is natural logarithm of number of establishments. "log of employment" is natural logarithm of number of paid employees. "log of sales" is natural logarithm of sales and receipts in thousands of 2000 constant dollars. "log of payroll" is natural logarithm of payroll in thousands of 2000 constant dollars. "% establishments in retail sector" ("% employment in retail sector") is the number of establishments (employment) in retail sector divided by the total number of establishments (employment) in all sectors. "Log Population" is log of population. "Log Per Capita Income" is log of per capita income in 2000 constant dollars. "Unemployment Rate" is unemployment rate. "Population Density" is per square mile population. "*t*-stat" ("z-stat") shows the *t*-statistics (*z*-statistics) of the mean (median) differences based on t-tests (rank sum tests).

Panel A: Full Sample

	(1) Places passed SCO			(2) Places without SCO			(2) – (1)	
	N	Mean	Median	N	Mean	Median	*t*-stat	*z*-stat
Log of Establishments in All Sectors	247	5.49	5.38	14,702	4.83	4.75	-8.04***	-7.13***
Log of Employment in All Sectors	247	7.90	7.82	14,702	7.25	7.21	-6.70***	-6.06***
Log of Establishments in Retail	247	3.84	3.97	14,702	3.40	3.43	-5.14***	-5.00***
Log of Employment in Retail	247	6.23	6.39	14,702	5.67	5.79	-5.45***	-5.43***
Log of Sales in Retail	224	12.03	12.05	13,741	11.28	11.25	-7.44***	-6.94***
Log of Payroll in Retail	224	9.71	9.75	13,741	5.67	5.70	-8.25***	-7.73***
% Establishments in Retail	247	0.22	0.22	14,702	0.27	0.27	7.23***	7.67***
% Employment in Retail	247	0.23	0.23	14,702	0.25	0.24	2.49**	1.88*
Log Population	134	9.97	10.00	6,891	9.14	8.91	-8.56***	-7.19***
Log Per Capita Income	134	10.09	10.04	6,891	9.86	9.80	-7.34***	-8.42***
Unemployment Rate	134	5.46	5.07	6,891	5.85	5.42	1.64	1.75*
Population Density	134	3,095	2,526	6,891	2,273	1,594	-3.83***	-4.88***

Panel B: Restricted Sample

	(1) Obs. with SCO			(2) Obs. without SCO			(2) – (1)	
	N	Mean	Median	N	Mean	Median	*t*-stat	*z*-stat
Log of Establishments in All Sectors	168	6.00	6.86	7,444	5.19	5.12	-8.60***	-7.54***
Log of Employment in All Sectors	168	8.44	8.26	7,444	7.65	7.63	-7.22***	-6.30***
Log of Establishments in Retail	168	4.41	4.39	7,444	3.74	3.71	-7.04***	-6.41***
Log of Employment in Retail	168	6.88	6.89	7,444	6.09	6.10	-6.90***	-6.47***
Log of Sales in Retail	165	12.39	12.42	7,312	11.55	11.56	-7.23***	-6.68***
Log of Payroll in Retail	165	10.06	10.06	7,312	9.16	9.17	-7.69***	-7.16***
% Establishments in Retail	168	0.23	0.22	7,444	0.27	0.26	4.79***	5.28***
% Employment in Retail	168	0.24	0.24	3,940	0.26	0.24	1.50	0.89
Log Population	115	10.23	10.27	3,940	9.29	9.06	-8.86***	-7.39***
Log Per Capita Income	115	10.07	10.02	3,940	9.88	9.82	-5.35***	-6.55***
Unemployment Rate	115	5.67	5.18	3,940	6.05	5.62	1.52	1.70*
Population Density	115	3,296	2,729	3,940	2,778	1,969	-1.87*	-2.81***

Table 3: Entry Regulation and Retail Trade Performance: Fixed Effect Model

The dependent variables include (1) log of establishments, (2) log of employment, (3) log of sales ('000 in 2000 constant dollars) and (4) log of payroll ('000 in 2000 constant dollars). In Panel A, the test variable is SCO restrictiveness. In Panel B, the test variable is SCO dummy. All regressions are estimated by OLS with place fixed effects, 4-digit NAICS fixed effects and year fixed effects. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

Panel A: SCO Restrictiveness

	Establishments	Employment	Sales	Payroll
	(1)	(2)	(3)	(4)
SCO Restrictiveness	**-0.123*** **	**-0.266*** **	**-0.138*** **	**-0.090** **
	(-4.95)	**(-4.49)**	**(-2.95)**	**(-2.39)**
Log Population	0.399***	0.877***	1.377***	1.080***
	(15.20)	(15.92)	(10.24)	(10.25)
Log Per Capita Income	0.146***	0.318***	0.328**	0.262***
	(5.39)	(4.82)	(2.57)	(2.66)
Population Density	-0.000*	-0.000***	0.000***	0.000***
	(-1.81)	(-3.16)	(3.33)	(3.46)
Unemployment Rate	-0.001	-0.006**	0.011**	0.009***
	(-1.36)	(-2.14)	(2.39)	(2.58)
Obs.	291,540	291,540	220,192	220,192
R-squared	0.686	0.595	0.658	0.669
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Panel B: SCO Dummy

	Establishments	Employment	Sales	Payroll
	(1)	(2)	(3)	(4)
SCO Dummy	**-0.057*** **	**-0.167*** **	**0.092**	**0.089**
	(-2.74)	**(-3.52)**	**(1.02)**	**(1.28)**
Log Population	0.402***	0.884***	1.375***	1.078***
	(15.20)	(15.94)	(10.22)	(10.22)
Log Per Capita Income	0.148***	0.326***	0.315**	0.251**
	(5.46)	(4.94)	(2.47)	(2.55)
Population Density	-0.000*	-0.000***	0.000***	0.000***
	(-1.88)	(-3.20)	(3.28)	(3.41)
Unemployment Rate	-0.001	-0.005**	0.011**	0.009**
	(-1.31)	(-2.08)	(2.36)	(2.55)
Obs.	291,540	291,540	220,192	220,192
R-squared	0.686	0.595	0.658	0.669
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 4: Entry Regulation and Retail Market Structure: Fixed Effect Model

The dependent variables include retail establishments as a percentage of total establishments of all sectors in column (1) and (3), and retail employment as a percentage of total employment of all sectors in column (2) and (4). In column (1) and (2), the test variable is SCO restrictiveness. In column (3) and (4), the test variable is SCO dummy. All regressions are estimated by OLS with place fixed effects, 4-digit NAICS fixed effects and year fixed effects. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

	% Establishments	% Employment	% Establishments	% Employment
	(1)	(2)	(3)	(4)
SCO Restrictiveness	**-0.001*** **	**-0.002*** **		
	(-3.86)	**(-3.92)**		
SCO Dummy			**-0.002*** **	**-0.002*** **
			(-7.95)	**(-6.12)**
Log Population	-0.000	0.002***	-0.000	0.002***
	(-0.92)	(2.75)	(-0.67)	(2.91)
Log Per Capita Income	0.000	0.002	0.000	0.002*
	(0.06)	(1.62)	(0.28)	(1.74)
Population Density	-0.000***	-0.000***	-0.000***	-0.000***
	(-3.72)	(-2.87)	(-3.74)	(-2.88)
Unemployment Rate	-0.000***	-0.000***	-0.000***	-0.000***
	(-8.76)	(-7.27)	(-8.70)	(-7.22)
Obs.	291,540	291,540	291,540	291,540
R-squared	0.279	0.199	0.279	0.199
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 5: The Effect of Entry Regulation: Differences-in-Differences

The treatment group includes places that passed SCOs during 1997-2012. The control group includes matched places without SCO but in the same county of the treatment group. The dependent variable is log of establishment in column (1), log of employment in column and (2), log of sales ('000 in 2000 constant dollars) in column (3), log of payroll ('000 in 2000 constant dollars) in column (4), percentage of establishments in all sectors in column (5) and percentage of employment in all sectors in column (6). The test variable is SCO Dummy*Post. All regressions are estimated with county fixed effects, 4-digit NAICS fixed effects and year fixed effects. Place-level controls include log population, log per capita income, population density and unemployment rate. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

	Establishments	Employment	Sales	Payroll	% Establishment	% Employment
	(1)	(2)	(3)	(4)	(5)	(6)
SCO Dummy	0.131*	0.252**	0.519**	0.402**	0.001	0.000
	(1.74)	(2.14)	(2.01)	(2.00)	(1.30)	(0.44)
Post	0.039**	0.043	0.165**	0.125*	-0.000	-0.000
	(2.05)	(1.19)	(2.02)	(1.95)	(-0.93)	(-0.92)
SCO Dummy*Post	**-0.088****	**-0.173*****	**-0.224**	**-0.168**	**-0.001*****	**-0.001****
	(-2.51)	**(-2.77)**	**(-1.65)**	**(-1.56)**	**(-3.86)**	**(-2.03)**
Obs.	62,200	62,200	47,225	47,225	62,200	62,200
R-squared	0.618	0.544	0.591	0.603	0.256	0.207
Place-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Entry Regulation and Retail Trade Performance: Instrumental Variables

This table presents results based on instrumental variables (IVs). The dependent variables include (1) log of establishments, (2) log of employment, (3) log of sales ('000 in 2000 constant dollars) and (4) log of payroll ('000 in 2000 constant dollars). The results of the first stage regressions are reported in the first four rows. IVs are deviation of year-median democrat share (Dev. Democrat share) and distance to the nearest place with SCO ("Distance2nearestSCO"). In Panel A, the test variable is instrumented SCO restrictiveness. In Panel B, the test variable is instrumented SCO dummy. All regressions are estimated with place fixed effects, 4-digit NAICS fixed effects and year fixed effects. Place-level controls include log population, log per capita income, population density and unemployment rate. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

Panel A: SCO Restrictiveness

	Establishments	Employment	Sales	Payroll
	(1)	(2)	(3)	(4)
First stage				
Democrat share	0.012***	0.012***	0.014***	0.014***
	(3.16)	(3.16)	(2.99)	(2.99)
Distance2nearestSCO	0.001**	0.001**	0.001**	0.001**
	(2.43)	(2.43)	(2.53)	(2.53)
Second stage				
SCO Restrictiveness	**-0.133****	**-0.347****	**0.066**	**0.096**
(Instrumented)	**(-2.25)**	**(-2.27)**	**(0.30)**	**(0.54)**
Hansen J statistic	0.816	0.207	0.550	0.421
(*p*-value)	0.366	0.649	0.458	0.516
Cragg-Donald Wald F statistic	2.5e+04	2.5e+04	2.1e+04	2.1e+04
Kleibergen-Paap rk LM statistic	12.274	12.274	10.431	10.431
(*p*-value)	0.002	0.002	0.005)	0.005
Obs.	291,540	291,540	220,192	220,192
R-squared	0.686	0.595	0.658	0.669
Place-level Controls	Yes	Yes	Yes	Yes
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Panel B: SCO Dummy

	Establishments	Employment	Sales	Payroll
	(1)	(2)	(3)	(4)
First stage				
Democrat share	0.018***	0.018***	0.019***	0.019***
	(7.53)	(7.53)	(8.63)	(8.63)
Distance2nearestSCO	0.002***	0.002***	0.002***	0.002***
	(4.18)	(4.18)	(4.50)	(4.50)
Second stage				
SCO Dummy	**-0.043***	**-0.131***	**0.067**	**0.069**
(Instrumented)	**(-1.89)**	**(-1.65)**	**(0.56)**	**(0.77)**
Hansen J statistic	2.303	1.334	0.392	0.181
(*p*-value)	0.129	0.248	0.532	0.670
Cragg-Donald Wald F statistic	1.1e+05	1.1e+05	9.6e+04	9.6e+04
Kleibergen-Paap rk LM statistic	42.354	42.355	41.842	41.842
(*p*-value)	0.000	0.000	0.000	0.000
Obs.	291,540	291,540	220,192	220,192
R-squared	0.686	0.595	0.658	0.669
Place-level Controls	Yes	Yes	Yes	Yes
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 7: Entry Regulation and Retail Market Structure: Instrumental Variables

This table presents results based on instrumental variables (IVs). The dependent variables are percentage of establishments in all sectors in column (1) and (3), and percentage of employment in all sectors in column (2) and (4). The results of the first stage regressions are reported in the first four rows. IVs are deviation of year-median democrat share (Dev. Democrat share) and distance to the nearest place with SCO ("Distance2nearestSCO"). The test variables are instrumented SCO restrictiveness in column (1) and (2) and instrumented SCO dummy in column (3) and (2). All regressions are estimated with place fixed effects, 4-digit NAICS fixed effects and year fixed effects. Place-level controls include log population, log per capita income, population density and unemployment rate. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

	% Establishments	% Employment	% Establishments	% Employment
	(1)	(2)	(3)	(4)
First stage				
Democrat share	0.012***	0.012***	0.018***	0.018***
	(3.16)	(3.16)	(7.53)	(7.53)
Distance2nearestSCO	0.001**	0.001**	0.002***	0.002***
	(2.43)	(2.43)	(4.18)	(4.18)
Second stage				
SCO Restrictiveness	**-0.003*** **	**-0.003** **		
(Instrumented)	**(-3.51)**	**(-2.54)**		
SCO Dummy			**-0.001*** **	**-0.001** **
(Instrumented)			**(-4.38)**	**(-2.14)**
Hansen J statistic	0.622	0.042	0.914	0.376
(*p*-value)	0.430	0.837	0.339	0.540
Cragg-Donald Wald F statistic	2.5e+04	2.5e+04	1.1e+05	1.1e+05
Kleibergen-Paap rk LM statistic	12.274	12.274	42.354	42.354
(*p*-value)	0.002	0.002	0.000	0.000
Obs.	291,540	291,540	291,540	291,540
R-squared	0.279	0.199	0.279	0.199
Place-level Controls	Yes	Yes	Yes	Yes
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 8: Falsification Tests based on Nonstore Retailers (NAICS=454)

This table summarizes results of falsification tests based on 4-digit subsectors of Nonstore Retailers (NAICS=454), including Electronic Shopping and Mail-Order Houses (NAICS=4541), Vending Machine Operators (NAICS=4542) and Direct Selling Establishments (NAICS=4543). The dependent variables include (1) log of establishments, (2) log of employment, (3) log of sales ('000 in 2000 constant dollars) and (4) log of payroll ('000 in 2000 constant dollars). The results of the OLS regressions are reported in the first two rows. The test variable is SCO restrictiveness. The results of second-stage IV regressions are reported after the OLS results. IVs are deviation of year-median democrat share (Dev. Democrat share) and distance to the nearest place with SCO ("Distance2nearestSCO"). The test variable is instrumented SCO restrictiveness. All regressions are estimated with place fixed effects, 4-digit NAICS fixed effects and year fixed effects. Place-level controls include log population, log per capita income, population density and unemployment rate. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

	Establishments	Employment	Sales	Payroll
	(1)	(2)	(3)	(4)
FE				
SCO Restrictiveness	**0.068**	**-0.012**	**0.048**	**0.031**
	(1.12)	**(-0.09)**	**(0.27)**	**(0.20)**
Obs.	43,731	43,731	34,388	34,388
R-squared	0.607	0.509	0.543	0.556
IV – Second stage				
SCO Restrictiveness	**0.265**	**0.367**	**-0.559**	**-0.431**
(Instrumented)	**(1.13)**	**(1.27)**	**(-0.69)**	**(-0.68)**
Hansen J statistic	1.962	2.654	1.643	1.800
	0.129	0.103	0.199	0.180
Cragg-Donald Wald F statistic	3752	3752	2946	2946
Kleibergen-Paap rk LM statistic	12.274	12.274	9.766	9.766
	0.002	0.002	0.008	0.008
Obs.	43,731	43,731	34,388	34,388
R-squared	0.607	0.509	0.543	0.556
Place-level Controls	Yes	Yes	Yes	Yes
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 9: Robustness Tests – Boundary Changes

This table summarizes results of robustness tests accounting for boundary changes at place level. Information on boundary change are collected from Census. Observations with annexation, detachment, disincorporation, territory exchange, merger/consolidation and new incorporation are deleted. The dependent variables are (1) log of establishments, (2) log of employment, (3) percentage of establishments in all sectors and (4) percentage of employment in all sectors. The results of the first stage regressions are suppressed. IVs are deviation of year-median democrat share ("Democrat share") and distance to the nearest place with SCO ("Distance2nearestSCO"). The test variable is instrumented SCO restrictiveness. All regressions are estimated with place fixed effects, 4-digit NAICS fixed effects and year fixed effects. Place-level controls include log population, log per capita income, population density and unemployment rate. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

	Establishments	Employment	% Establishments	% Employment
	(1)	(2)	(3)	(4)
FE				
SCO Restrictiveness	**-0.128*** **	**-0.278*** **	**-0.002*** **	**-0.002*** **
	(-4.57)	**(-4.24)**	**(-3.68)**	**(-3.71)**
Obs.	285,560	285,560	285,560	285,560
R-squared	0.683	0.593	0.277	0.199
IV – Second stage				
SCO Restrictiveness	**-0.150** **	**-0.393** **	**-0.003*** **	**-0.003** **
(Instrumented)	**(-2.29)**	**(-2.34)**	**(-3.45)**	**(-2.51)**
Hansen J statistic	0.869	0.290	0.334	0.003
(*p*-value)	0.351	0.590	0.563	0.953
Cragg-Donald Wald F statistic	2.2e+04	2.2e+04	2.2e+04	2.2e+04
Kleibergen-Paap rk LM statistic	12.078	12.078	12.078	12.078
(*p*-value)	0.002	0.002	0.002	0.002
Obs.	285,560	285,560	285,560	285,560
R-squared	0.683	0.593	0.277	0.199
Place-level Controls	Yes	Yes	Yes	Yes
Place FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Appendix 1: Entry Regulation and Retail Market Structure: with County Fixed Effect

This table shows results in contrast to Table 4. Instead of place fixed effects, county fixed effects are used. The dependent variables include (1) log of establishments, (2) log of employment, (3) log of sales ('000 in 2000 constant dollars) and (4) log of payroll ('000 in 2000 constant dollars). The test variable is SCO restrictiveness. All regressions are estimated by OLS with county fixed effects, 4-digit NAICS fixed effects and year fixed effects. County-level controls include log of population and log of per capita income. The time periods include 1997, 2002, 2007 and 2012. Standard errors are clustered at place level. *t*-statistics are in parentheses.

	Establishments	Employment	Sales	Payroll
	(1)	(2)	(3)	(4)
SCO Restrictiveness	**1.147*** **	**1.693*** **	**3.435*** **	**2.826*** **
	(4.44)	**(4.20)**	**(3.90)**	**(3.97)**
Obs.	482,880	482,879	363,388	363,388
R-squared	0.243	0.239	0.247	0.251
County-level Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
NAICS FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes