Shops and the City

Evidence on Local Externalities and Local Government Policy from Big-Box Bankruptcies*

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Abstract

We report three findings: (1) Using evidence from chain bankruptcies and data on 12-18 million establishments per year, we show that large retailers produce significant positive spillovers. (2) Local governments respond to the size of these externalities. When a town's boundaries allow it to capture a larger share of retail spillovers, it is more likely to offer retail subsidies. (3) These subsidies partially crowd out private-sector mechanisms that also subsidize large retailers, such as shopping malls. These facts provide powerful evidence of the Coase theorem at work and highlight a concern for local development policies even when externalities can be targeted.

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I Local Governments and Retail Externalities

The Concise Encyclopedia of Economics begins its article on public goods and externalities (Cowen, 1993) with the statement that "[m]ost economic arguments for government intervention are based on the idea that the marketplace cannot... handle externalities." The article explains that "markets often [do] solve (..) externalities problems" when property rights are well defined (as in Coase, 1960). This idea, that there are often both public and private mechanisms for internalizing externalities, has been central to public finance but difficult for empirical researchers to observe in all of its implications. There are few situations where an externality can be identified in a dataset, fewer where the relative size of this externality can be correlated with policy decisions, and fewer still where these decisions can be explored alongside alternative private internalization systems.

In this paper, we study precisely such externalities, policies, and private mechanisms.[1] We set out by estimating the size of a specific type of externalities: the spillovers produced by big-box retail stores. The question of whether and how much they affect surrounding businesses is a contentious one that is of interest to economists and policymakers alike. To estimate the size of these externalities, we exploit the nationwide store closings associated with the bankruptcies of national chains of bookstores (Borders), department stores (Mervyn's), electronics stores (Circuit City Stores and CompUSA), and housewares stores (Linens 'n Things). We contrast the sudden, locally exogenous disappearance of these chain stores with the continuing presence of comparable chains in the same sectors (Barnes & Noble, Kohl's, JC Penney, Best Buy, and Bed Bath and Beyond). Using geographically detailed data on establishments we find robust, positive spillovers at the short distances relevant for municipalities - half of the towns in the country span less than 3 miles at their widest point.[2] After the disappearance of a big-box store, nearby businesses flounder. There are fewer of them, and they employ fewer workers. Based on data on household shopping behavior we establish that this happens in part because when consumers stop visiting the big-box store, they also visit nearby stores less frequently than they would have otherwise. This decline is absolute, and not just relative to stores close to big-box stores that did not close.

We then explore how local governments deal with these externalities. Externalities are a common justification for government intervention. If local governments are aware of the spillovers we observe, we might expect those local governments that face the largest positive externalities to be the ones that address them by implementing the most generous development policies. We test this hypothesis by exploiting the fact that some localities have shapes that allow them to capture more of these local spillovers, under the assumption that local governments care disproportionately about economic activity within their borders. We show that while the boundaries of the resident town does not affect the size of a big-box store's spillover at a given radius (i.e. the economic spillover), the "right" geographical shape of the locality makes it more likely that these positive spillovers are contained within city limits. Localities with more compact shapes are more

[1] We will occasionally use the term "externalities" to refer to the impact of externalities, to avoid unnecessarily impenetrable language.

[2] We use the terms "town," "municipality," "city" and "locality" interchangeably except where specificity is required to identify which data we use.

likely to target retail development with subsidies and tax expenditures, conditional on total area. These results hold when we instrument for the shape of the political unit using a variety of legal and geographical impediments.

Of course, the right to locate near one of these large retailers is generally "well-defined." The Coase theorem would predict that, in such a situation, the private sector should provide mechanisms for internalizing these spillovers. In fact, prior research has shown that foot-traffic generating big-box "anchor" stores are heavily cross-subsidized through private mechanisms like shopping malls, in which they often pay heavily reduced rental rates or even no rent at all (see Gould and Pashinigan, 1998, Gould, Pashigan, and Prendergast, 2005). Brooks and Strange (2011) find that large retailers disproportionately benefit from quasi-private organizations like Business Improvement Districts (BIDs) as well. In this paper we show that cities that are more likely to offer public retail development subsidies are less likely to have these private and quasi-private incentive provision systems. The public internalization mechanisms crowd out the private-sector ones.

These estimates are important for several reasons. In a recent International City/County Management Association (ICMA) survey (ICMA, 2009), roughly forty percent of local governments in the United States reported focusing on retail incentives to spur economic development. Be it through tax increment financing, income and property tax credits, land use subsidies, sales tax rebates, infrastructure assistance, or any of a myriad of other mechanisms, the goal is the same: to make it more attractive for large retailers to set up shop in town. The justification for local development efforts is the notion that these retailers produce positive externalities for the existing local businesses and promote the arrival of new businesses, increasing municipal tax collection and employment.

The amount of public spending involved in these types of subsidies is large and economically significant. Local governments in the St. Louis area, for example, provided over \$5.8 billion in local subsidies over the past 20 years (East-West Gateway Council of Governments, 2011), of which some 80% went to retail stores. Since 1986, Chicago has "lost" \$5.5 billion in revenue from committed tax increment financing districts (TIFs) (Orr, 2013). The perceived impact of these subsidies has made them one of the most common development strategies used by policymakers at the local level, leading to a developing regulatory push for increased transparency regarding their use and cost (Government Accounting Standards Board, 2014).

Despite the ubiquity of local economic development policies, earlier research expresses skepticism about the ability of governments to perceive and target true local externalities (Glaeser and Gottlieb, 2008). This paper sharpens this argument by demonstrating that even if governments are capable of identifying and targeting true externalities, government intervention may crowd out private mechanisms aimed at the same goal. In this sense, this paper provides rare evidence on the applicability of the Coase theorem: the situation we study features demonstrable external effects, observable variation in the level of public and private efforts dedicated to addressing them, but nonetheless similar economic outcomes.

This work also relates to the literature on the size of spillovers in the retail trade. Previous research has found fertile ground in the expansion of Wal-Mart, the largest firm by number of employees in the United States. Basker (2005) explores the impact Wal-Mart store openings had on local employment, and finds that while overall retail employment rises after a store opening, it does so by much less than the number of workers at the Wal-Mart store itself. Neumark, Zhang, and Ciccarella (2008) paint an even starker picture, of total retail employment dropping after Wal-Mart enters a market, suggesting particularly strong negative spillovers for other retail firms. This suggestion is supported by Jia (2008), who finds that almost half of the decrease in the number of small discount stores that occurred from the late 1980s to the late 1990s was driven by Wal-Mart's expansion, and by Merriman, Persky, David and Baiman (2012)'s case study of a Wal-Mart opening on Chicago's West Side. That is, not only do competing retailers shrink their work force, some are forced out of business entirely.

On the other hand, Bertrand and Kramarz (2002) and Sadun (2014) find that entry barriers meant to deter large retail stores harm job growth and smaller, independent retailers, respectively. Benmelech, Bergman, Milanez, and Mukharlyamov (2014) observe, in a similar vein, that retail stores bankruptcies produce negative spillovers for other retail establishments. Positive externalities from economic activity nearby are also found in the broader agglomeration literature (e.g. in Arzhagi and Henderson (2008) and Greenstone, Hornbeck, and Moretti (2010)).We bring an explanation to the table that unifies these contrasting views: whereas the overall spillover effects we find are positive, directly competing retailers are harmed by the presence of a big-box store. This conclusion is similar to that of Haltiwanger et al. (2010), who study entry into the Washington, D.C., metro area.

The overall positive spillovers we find do, of course, have very different implications for firms and policymakers, and we dedicate the second half of our paper to those implications. In that part of the paper we establish that policymakers respond to the spillovers we observe by targeting retail development more aggressively in places where they are larger. This is at least partially superfluous because it crowds out private-sector action that accomplishes similar goals along the lines of the Coase theorem's predictions.

The paper proceeds as follows. In section 2, we demonstrate the ex ante similarity of our comparison neighborhoods and document the absence of pre-trends in the data. We then use difference-in-differences regressions to estimate the externality associated with a major big-box closing on outcomes for neighborhoods of differing sizes around the store. Relying on locally exogenous variation produced by national-chain bankruptcies, we find strongly negative overall effects that decay with distance. As independent confirmation of our results, and to establish the mechanism through which the observed spillovers arise, we therefore study consumer behavior. We find that after a big-box store closes, consumers rapidly and significantly reduce their visits to stores they used to visit on the same day as the big-box store. We also demonstrate here that consumers do not just substitute away toward stores near competitor big-box stores.

In section 3, we use an instrumental-variables approach to show that the size of these effects for political units (cities and towns) depends upon a city's geographic shape, or "compactness." While these compactness measures do not affect the size of the externality as measured over fixed distances both inside and outside the political unit, they do affect the size of the spillover within a city's borders and, as a result, a city's propensity to offer retail development incentives. Section 4 shows that this propensity for public subsidies is negatively correlated with the presence of shopping centers and BIDs, suggesting significant crowd-out effects. Section 5 concludes with a discussion of our results, and why they matter for both the development of urban centers and for our understanding of the history of the city.

II Externalities from Big-Box Retail Stores

Between early 2007 and early 2008, CompUSA, a consumer electronics chain, ended all of its retail operations (McWilliams, 2007). Also in 2008, Mervyn's, a chain of department stores, filed for Chapter 11 bankruptcy and liquidated its stores in seven states (Dodes and McCracken, 2008; Mervyn's Brands, LLC, 2008). Later that year, Circuit City Stores entered bankruptcy and by January 2009 started closing its remaining Circuit City Superstores (Chang and Zimmerman, 2009). Around the same time, Linens 'n Things, a major chain of housewares stores, closed its remaining locations (Dealbook, 2008). Finally, in 2011, Borders Group, an international chain of book and music stores, applied for Chapter 11 bankruptcy protection (later converted to Chapter 7), could not find a suitable buyer, and liquidated its stores across the United States (Border's Group, Inc., 2011; Spector and Trachtenberg, 2011). Not all big-box retail chains of the same size and in the same industries suffered the same fate. The book stores of Barnes & Noble, the department stores of JCPenney and Kohl's, Best Buy's electronics stores, and Bed Bath and Beyond's housewares stores continue to operate even today. The bankrupt and surviving chains we study are similar in geographic dispersion, and we will see that the features of the local business environments they faced cannot account for the different futures they were destined to live through. Instead, accounts in the popular press emphasize high-level strategic choices related to capital structure or on-line competition made by the firms in explaining their survival or non-survival (see, among many other examples, Galuszka, 2008; West, 2009; Lowrey, 2011; Farrell, 2012).[3] In addition, we use as our measure of bankruptcy timing the year in which the national chain filed for bankruptcy, so that our measure of timing is not driven by local idiosyncracies either.

In this section, we compare the economic activity around the surviving big-box stores to that around the locations of the now defunct stores formerly operated by CompUSA, Mervyn's, Circuit City, Linens 'n Things, and Borders, to produce estimates of the spillovers produced by the presence of these large retail stores. We do so by using data on establishments and the workers they employ, as well as data on consumer purchases. In this way we derive the estimates of interest from two independent data sources, from the supply and demand side of the economy.

II.a Nearby Establishments

We locate big-box stores and neighboring establishments by using commercial Esri Business Analyst data supplied by Harvard's Center for Geographic Analysis.[4] The data are annual and run from 2003 to 2012; for selected summary statistics, see Table 1. These geo-coded establishment data originate with InfoUSA,

[3]An intern collected 25 news articles on the causes of the bankruptcy of each of the five treatment chains. Two other coders then agreed that just one of these 125 articles mentioned the local business environment as a driving factor behind a bankruptcy, McCracken (2011): a remarkable degree of inter-coder concordance.

[4]Our Data Appendix provides detailed information on the provenance of the different datasets we use throughout.

a business partner of Esri, which collects lists of establishments from phone directories, business filings, utility connections, press releases, web directories, annual reports, and other sources. InfoUSA then surveys each establishment by phone (between 12 and 18 million establishments per year) and collects, among other things, data on employment and industry. We calculate distances between each establishment and the big-box retailers in our comparison sample, and we then aggregate outcomes in terms of store counts and employment for neighborhoods of varying radii around big-box retailers and for the city containing the big-box retailers. Although the Esri data are not produced by the federal statistical system, we believe that this is the correct choice for a variety of reasons. First and foremost, Esri includes address data, which allows us to work at a high-precision geographical level. It includes establishments not counted in the Census' County Business Patterns dataset (unincorporated or no employees) - someone teaching violin lessons might show up, for example. This broader measure of economic activity is appropriate for our purposes (and for many other purposes as well, which is why there is a market for these datasets), although there may be systematic scale differences. We always include fixed effects, and our estimates are in percentage terms. In addition, our results stand even if one worries about the quality of the data. We use establishment counts as a dependent variable, and as long as the noise in the Esri data is not correlated with a chain eventually becoming defunct, a type of correlation that strikes us as wildly implausible, the noise merely inflates our standard errors. That said, we do not believe that even measurement error is particularly severe. Establishment counts at the county level are strongly correlated with County Business Patterns data from the Census Bureau in both levels and in log changes over time, as depicted in our Data Appendix. Additionally, where we can, we replicate our results using LODES data, which is derived from the Census Bureau's LEHD and uses a completely different methodology. These replication exercises strengthen our confidence in the robustness of our findings to the point of practical certainty.

Now, although the above mentioned combinations of chains are in similar industries, the establishments of comparison pairs (e.g. Borders and Barnes & Noble) were not necessarily identical prior to chain-level bankruptcies. Table 2 addresses this concern. The table shows estimates of the coefficient on a dummy variable indicating whether a store would eventually go bankrupt in the following regression equation:

$$\Delta\ \mathit{ln(Stores,\ Employment)}_i = \alpha + \mathit{(Bankrupt)}_i \beta + \gamma_t + \varepsilon_i,$$

where γ_t are fixed effects for each big-box store type (clothing, for example), and β measures the difference in the growth rate of the number of stores or total employment in the concentric areas around big-box retailers that disappeared and that survived. Observations are neighborhoods around big-box stores. The first two columns show pooled estimates of the log increase from 2006 to 2007 in the number of stores and level of employment for radii of increasing size (0.5, 1, 2, and 3 miles) around defunct stores compared to non-defunct stores. There are no significant differences between trends in the number of area stores or area employment between defunct and non-defunct stores. As Columns 3 and 4 show, the same holds true for differences in the growth of the number of stores and of employment within the different radii over the preceding 5-year period from 2003 to 2007.[5]

Perhaps even more convincingly, Figure 1 graphically represents the evolution over time of the log number of establishments within a mile of defunct big-box stores relative to the number of stores within one mile of non-defunct chains, with year-store type and neighborhood fixed effects removed. Neighborhoods in our sample that were home to ultimately defunct big-box stores followed similar trends to the control neighborhoods until the national-chain bankruptcy, when they started diverging. Similarly, Appendix Table 1 provides a basic pre-bankruptcy comparison of the level of economic activity in 2006 around defunct and non-defunct big-box stores. In 2006, there were no significant differences between the neighborhood business environments within half a mile, a mile, or two miles of the eventually defunct and surviving stores, which is reassuring.

Of course, pre-trends may mask heterogeneity across chain locations in susceptibility to subsequent shocks like the 2007-2009 recession and other regional shocks. To address this possibility, we exploit the geographic detail of our data to estimate the impact of closures within zip codes. As a robustness test on our main results, we include *zip-code-by-year fixed effects* (i.e. dummies for 02138 in 2010, 20037 in 2010, 02138 in 2011, etc.) alongside our main effects. Even when using only within zip code variation, and thus controlling for arbitrary zip code level trends, we recover a similar result.

Having confirmed the similarity of our treatment and control neighborhoods, we can now turn to our baseline estimates of the spillovers of interest. Table 3 provides these. The equation we estimate here is the following:

$$\mathit{ln(Stores,\ Employment)}_{i,t} = \alpha_i + \gamma_t + \mathit{(Post\ x\ Bankrupt)}_{i,t}\beta + \varepsilon_{i,t},$$

where α_i are fixed effects for each big-box location neighborhood, γ_t are year-store type (or year-zip code area) fixed effects, and β measures the differential impact of a post-closure year on neighborhoods where the big-box retailer chain has closed. The identifying assumption is that neighborhoods around defunct and non-defunct retailers would have fared similarly, were it not for the nationally determined closures. This assumption makes sense given the factors driving these corporate bankruptcies and their specific timing, both, as we have seen, plausibly orthogonal to local developments. It is for this reason that we use the timing of the corporate bankruptcies, not the potentially endogenous precise closing date of each individual store, to identify our spillovers.

Panel A of Columns 1 and 2 in Table 3 shows that after a chain-wide bankruptcy, the number of stores within a quarter-mile radius of a defunct store decreases by some 8 to 11%, while employment drops by 7.5 to 13% compared to the same area around non-defunct stores. [6] As discussed above, to control for any

[5] We can confirm that the data produced by Esri Business Analyst is highly similar to data collected by the U.S. Census Bureau. The correlation between Census Business Patterns data on the number of establishments in all zip codes, in zip codes with over 50 establishments, and in the zip codes that one of our big-box stores calls home and the comparable Esri numbers is over 0.9 for each year in our dataset. We prefer to rely upon Esri data instead of Census Bureau data in most of the rest of the paper as the Esri data allow us to map addresses and distances to specific stores as opposed to only census tracts and zip codes, but we confirm our basic results based on the Census data in Appendix Table 2 and 3. Part C of the Data Appendix discuss the Esri data and its reliability more extensively.

[6] Appendix Figure 1 visualizes our empirical strategy for two electronics stores in Massachusetts.

potential regional differences, we repeat this exercise controlling for arbitrary trends at the zip code level by including zip-code year fixed effects. The impact remains large and significant with these controls, although the effect size is smaller.[7] This fact is consistent with a greater weight on overlapping neighborhoods when using solely within zip code-year variation. To test for the importance of overlapping neighborhoods in our general results, in row 3 we re-estimate the effect using only on zip code that contain at most one store per store type. These estimates lie quite close to the baseline effects. Moreover, we demonstrate similar effects using fixed, never overlapping geographies in Appendix Table 4.[8]

In the median neighborhood these effects involve around 20 stores and 300 employees: fairly large numbers, of a magnitude reminiscent of the results found by Jia (2008) and Neumark et al. (2008).[9] They do not, of course, materialize instantly. Figure 1 shows, for the 1-mile radius, that the effect built gradually over a period of multiple years. The estimates are also very precise, and we can reject the null of no effect at the 1% level.

The Panel A part of Columns 3 through 8 show similar estimates for the number of stores and employment within a half-mile, one-mile and two-mile radius. The most striking feature of this table is that we see significant decay in the effect size of the spillovers we observe as we move farther away from the big-box store of interest, reminiscent of Campbell, Giglio and Pathak (2011). Within one mile, the number of stores decreases by 3% post bankruptcy while within two miles, we see a drop in the number of stores that is just below 2%.[10][11] Employment levels show a similar declining pattern.[12] It is not immediately clear from these numbers whether these effects are driven primarily by shifting economic activity across locations or by changes in the aggregate number of establishments and employees, but the effects we observe at the very local level are strongly negative and are not balanced out by positive effects somewhat farther out. The pattern of decay is evident from different approaches as well, as demonstrated in Appendix Table 4. In this table,

[7] Appendix Figure 2 visualizes our empirical strategy for two stores in the same zip code.

[8] These results are not driven by a shift toward the neighborhood surrounding the big-box store's direct competitor. This most easily seen in the next subsection, in which we study consumer shopping behavior directly.

[9] An alternative scaling of the coefficients could allow for the effect of a big-box closing to vary with a store's estimated sales volume. To estimate this effect, we run a regression of $ln(Stores) = \alpha_i + \gamma_t + \textit{(Post x Sales Volume)}_{i,t}\beta_1' + \textit{(Post x Bankrupt)}_{i,t}\beta_2' + \textit{(Post x Bankrupt x Sales Volume)}_{i,t}\beta_3' + \varepsilon_{i,t}$ at the 1 mile radius. We estimate β_3 to equal a statistically significant -0.011, with a standard error of 0.004. The typical big-box store in our sample had sales of roughly $7.5 million in 2007, with an interquartile range of $4.0 million to $19.4 million. This implies that a big-box store closing at the 75th percentile led to a 1.7 log point greater decline in neighborhood stores relative to a store closing at the 25th percentile of the size distribution.

[10] We also estimate the one mile radius effect on the number of stores using data for only one Combined Statistical Area at a time. Roughly 70% of these CSA-level estimates are negative. The mean estimate is -0.027, with a standard error of 0.006, and is highly statistically significant. The median estimate is -0.021. These effects are quite close to our baseline estimate of -0.030 in Table 3.

[11] Though baseline demographics are absorbed by our difference-in-differences approach and our within-zip-code-year regressions, it is possible to control for potential differential trends along demographic lines. To assess the importance of such trends, we use data from the 2000 Census at the zip code level to create median-house-price-by-year dummies, percent-black-by-year dummies, and unemployment-rate-in-2000-by-year dummies. Without these controls, our baseline result in Table 3 at 1 mile was 0.030 ($SE = 0.005$); with these controls it is virtually identical at 0.029 ($SE = 0.005$). This is further evidence that our results are not driven by pre-existing differences across neighborhoods.

[12] Another possible set of specifications uses concentric rings rather than concentric circles. These specifications return very similar results: a bankruptcy is associated with a -0.65 ($SE = 0.001$) change in log stores at half a mile, a -0.012 ($SE = 0.005$) change from from half a mile to one mile, a -0.012 ($SE = 0.005$) change from from 1 to 1.5 mile, and a -0.009 ($SE = 0.004$) change from 1.5 mi to 2 miles. In other words, these regressions show a quantitatively similar decline in effect size at the distances shown in the table.

instead of using concentric circles around the big-box store of interest, we study its impact on census tracts and cities at least parts of which are located within certain increasing distances. Note that the perspective from which we observe spillovers here is different: the unit of observation here is the census tract or city, not the big-box store, and as a consequence there are no areas that overlap or are observed repeatedly, as opposed to there being no such stores. The effects we find are qualitatively similar to those in our baseline specification.

In Panel B of Table 3 we disaggregate our estimates by type of big-box store. Remarkably, the externalities we observe as created by the different store types are comparable not just qualitatively but even quantitatively: the number of stores around a newly bankrupt big-box store drops by between 11 and 13%, while employment drops between 9 and 19%, whether we focus on book stores, electronics stores, clothing stores, or linen stores. This is a level of variation that is small compared to the variation induced by store size or the range of variation created by stores of the same type. The rest of Panel B demonstrates that the pattern of decaying spillover sizes observed around different store types is also roughly similar, and we therefore focus our attention on pooled estimates in the rest of the paper.

Now, just like the impact felt by nearby stores differs from that felt by stores farther away, different types of stores are affected differentially as well. Table 4 sheds some light on the differences between spillovers as perceived by four mutually exclusive pairs of store types: businesses that depend on foot traffic versus businesses that do not; firms that do not compete with the big-box stores we track versus firms that do; new entrants versus incumbents; and businesses in areas with relatively high versus relative low levels of Internet access (see the Data Appendix for more detail on variable construction). It shows results for regressions similar to those in Column 3 of Table 3 (effects within a half-mile radius), with year-store type fixed effects, focusing only on the type of store of interest in a particular cell for construction of the dependent variable. One would expect, and we explore this mechanism in more detail in the following subsection, that businesses that depend heavily on foot traffic (see Appendix Table 5 for the precise classification) are affected more heavily by the disappearance of a nearby anchor store. That is correct, as we see in Columns 1 and 2: 9% of retail businesses that rely heavily on foot traffic disappear, while firms that do not are not significantly affected. We see an even starker contrast in Columns 3 and 4. Firms that used to compete with one of our big-box stores flourish after said store's disappearance, growing in number by 4%. Firms that did not are hit with the negative externality we observe in the aggregate, and their number decreases by 3%. In the case of the big-box stores we focus on, the overwhelming majority of stores do not compete with our big-box stores, of course, which explains the positive overall effects we measure. It is also, we believe, a potential explanation for the discrepancy highlighted in the introduction between the negative overall spillovers produced by the opening of a Wal-Mart, which competes with many existing businesses, and the positive spillovers observed in the literature around other large retailers, which compete with a smaller share of incumbent businesses.

A different dichotomy of interest is that between incumbent firms and new entrants. In Columns 5 and 6 of Table 4 we see that both categories suffer when a neighborhood suddenly loses its big-box stores. The number of incumbent firms decreases by some 2.5%, while the number of entrants goes down by even more: a little over 3%. Given that new entrants account for only 17.3% of establishments, in absolute terms the lion's share of the burden of big-box store disappearances is borne by existing firms. Columns 7 and 8 confirm the importance of on-line competition in recent years: the spillovers we observe are larger in states with higher levels of Internet access, possibly because more stores there were already on the brink of bankruptcy even before the nearby big-box store disappeared.

II.b Consumer Behavior

Our spillover measures so far have been based on data on establishments. As a check on their validity, and to assess the mechanism through which these spillovers come into being, we now turn our attention to the customers of these establishments. To do so we use household-level data on shopping behavior from Nielsen. The data describe the purchases of retail goods (not services) of some 100,000 households in 20,000 zip code areas between 2004 and 2009, for a total of 300 million transactions. While the stores visited by these households are identifiable in the sense that the visits to the same store can be tracked, Nielsen shields the precise names and locations of the stores, forcing us to rely upon a characterization of the store's line of business and the first three digits of its zip code. The stores we look at here are the electronics stores, department stores, and housewares stores, as Borders went bankrupt too late for us to be able to study the impact of its bankruptcy using these data.

Our empirical strategy is as follows. We focus on households that live in a three-digit zip code in which one of the ultimately defunct big-box stores is located. We can only identify large categories of stores, so within this group of households we treat households who visited an electronics, department or housewares store as households who visited a store of the corresponding ultimately bankrupt big-box chain of interest on that day. This leaves us with between 500 and 1,000 households for each of the chains (see Appendix Table 6 for other summary statistics). The variable we track is the ratio between the monthly number of stores visited on the same day as the imputed ultimately defunct stores, and the number of stores visited on other days. If this ratio goes down, it means that stores that are visited on the same day as stores we have identified with CompUSA, Mervyn's, Circuit City and Linens 'n Things are being frequented less often or even close down entirely. Appendix Table 6 shows that this ratio went down after the chains we study disappeared.[13] Appendix Table 7 shows the results of simple regressions of this ratio on a post-bankruptcy dummy, with household fixed effects included; they show consistently significant decreases in this ratio. These estimates, however, underestimate the true impact of bankruptcy on surrounding retail. We can observe how this happens in Figure 2. After bankruptcy, households take a while to adopt new shopping habits, but the stores they used to visit whenever they visited the now-bankrupt big-box store clearly suffer the consequences of their big neighbor's demise.[14] In some cases they see a decrease in the number of visits of as much as 20% relative to non-affected stores, where this includes an end to all visits to stores that no longer exist, not

[13] This figure is an illustration, not rigorous proof of a structural break.

[14] The exact bankruptcy dates we use are May 2008 for Linens 'n Things, July 2008 for Circuit City and CompUSA, and October 2008 for Mervyn's. There is some variation in the closing dates of individual stores, but our choice of bankruptcy date only affects the locations of the vertical lines in this figure, not our estimates of monthly shopping behavior as such.

just decreases in visits to stores that continue to operate. Household fixed effects do not change our results qualitatively.

II.b.1 Substitution or Disappearing Spillovers?

The results we presented in Table 3 compared outcomes for neighborhoods around defunct chains relative to neighborhoods around competitor chains. This difference-in-difference strategy could not distinguish between declining store counts around defunct chains and rising store count around competitor chains. Moreover, a simple before-and-after comparison for defunct stores only is not useful in this context, because total store counts change based on aggregate economic conditions.

It is important to note, then, that the Nielsen data help us distinguish between these possible interpretations to at least a certain extent. As we explained, we focus on households that live within a zip code in which one of our ultimately defunct chains was located. We then mark as a "same-day store" any store visited on a day when that household purchased the specified category of good. In other words, for an individual near a Circuit City, any store he visited on a day he made an electronics purchase was tagged as a same-day store. The decline in same-day store purchases by this individual, relative to non-same-day store purchases, cannot then reflect an increase in visits to a nearby Best Buy. It must reflect a decline in visits to stores near all local electronics stores - or a drop in traffic to the defunct neighborhood. Note that we are not suggesting that this excludes the possibility of substitution effects explaining part of our establishment-based estimates; it simply confirms that at least some of the decline in economic activity around defunct store is absolute, not just relative.

This result is confirmed by another test, in which we estimate the same difference-in-difference regressions in Table 3 on only the set of neighborhoods around ultimately defunct stores. These regressions identify the impact of the bankruptcy off differential timing: Borders went bankrupt later than the other chains. These regressions produce somewhat smaller, but highly significant declines due to bankruptcy. Again, these results show that our findings are capturing, in part, a decline around defunct stores.

Now that we have demonstrated the existence of big-box store externalities, and what drives them, we ask: how are they related to town characteristics? Are local governments aware of the sizable externalities we have observed, do they care about them, and do they use policy to attract the stores that create them to their towns? In the following section, we examine the interplay between city shape, externality size, and local government behavior.

III Externalities, Local Geography, and Local Government

The sizable spillovers from big-box retail stores that we have identified have not gone unnoticed by policymakers. In this section, we analyze the development incentives offered by local governments to address them. The shape and boundaries of localities affect the extent to which externalities are internalized (Hayashi, Nishikawa, and Weese, 2011), and we will see in this section that that certainly holds true here. Town-level externalities are strongly related to the shape of the town, and as a consequence, local-government development policies are related to town shape as well, confirming the finding of Felix and Hines (2011) that geography plays a central role in shaping policies such as the ones we focus on here.

Economic Impact Models

It is not just academic research that supports the idea that the geographical shape of local government units is an important driver of development policies. Many city governments - 73% of them according to ICMA (2009) - claim to perform a formal cost/benefit analysis prior to offering business incentives. There is a robust market for economic-development consultants, and the models used by these consultants alert city governments to the importance of city boundaries: these so-called economic impact models often emphasize the importance of taking into account how the shape of a town or other political unit interacts with spillovers to determine the incidence of the externalities created by government subsidies. The Federal Reserve Board of Governors' Fiscal Impact Tool (2003), for example, explains that "[p]rojects located on the edge of a town, closer to the shopping and transportation access of a neighboring community, will likely experience greater-than-average retail leakage to that other community." The Bureau of Economic Analysis' Regional Input-Output Modeling System (RIMS II), which produces regional "multipliers" that are meant to be used to estimate the economic impact of projects within a region, explicitly models "money leaking out of the economy" (Bureau of Economic Analysis, 2012). Other economic impact models also take into account factors like the share of sales tax receipts in the city as a share of total receipts within a driving radius (e.g. Kennington, 2011, and more generally all of the tools discussed by Morgan, 2010), reflecting the importance of town shape in capturing externalities.[15] Retail, for which physical visits by customers are particularly important, is a key driver of these considerations.

III.a Town Shape and Externalities

To speak to these policy-relevant concerns, in this section we move to town-level regressions, where we use Census Incorporated Places to define towns. This allows us to focus on the externalities of direct interest to local policymakers, instead of the perhaps more intuitive radius-based ones highlighted before. Table 5 shows these results in Panel A, while Panel B shows results for the 1-mile radius in an otherwise identical specification. In columns 1 and 8 we see that the negative impact on the number of stores and total employment in a city after one of our big-box stores closes is sizable and significant, as it was within the 1-mile radius around the store. On average, a big-box bankruptcy in a city leads to a 0.7% decrease in both the total number of stores and employment for that city, or the disappearance of 28 establishments and 333 jobs.

Big-box store externalities, though smaller in percentage terms than for the immediate vicinity of a big-box store, thus extend to city-wide effects. This is intuitive, given that according to the Harvard Center for Geographic Analysis 75% of cities in our sample span less than 4 miles at their widest and longest points. A logical consequence of the comparable scale of spillovers and municipalities is that the precise shape of municipalities matters. We therefore turn our attention now to estimating how much larger the town-level

[15] An example of this phenomenon in our data is visualized in Appendix Figure 3.

spillovers from big-box stores are in towns shaped in ways that allow them to capture most of the total radial spillover as compared to towns shaped differently.

Specifically, the measures of town shape we will use are all based on the same basic notion of "compactness," similar to the notion employed by Harari (2015). Imagine, for example, a city shaped like a perfect circle. If a big-box store is placed in the middle, that city will capture nearly all of the benefit. On the other hand, if we place a big-box store in the center of a city shaped like the letter "L", the benefit is shared with neighboring localities. With this intuition in mind, we employ five measures of compactness, most of which originated in the political-science literature on the shape of congressional districts:

- The ratio of town area to the product of the maximum width and length of the town, which we will refer to as the rectangular area fraction (RAF);

- The ratio of town area to the area of the smallest circle that can enclose the district, as proposed by Roeck (1961);

- The ratio of town area to the area of a circle with the same perimeter, first suggested by Polsby and Popper (1991);

- The ratio of the town's perimeter to that of a circle with the same area as the town, from Schwartzberg (1966)[16];

- The ratio of town area to the area of its convex hull, which we call the convex ratio.

These measures are strongly correlated, and for clarity's sake we typically aggregate them into an index: the different measures' first principal component.[17]The five compactness concepts are illustrated in Figure 3. Figure 4 shows the RAF for the Boston metropolitan area as an example. Figure 5 demonstrates that the average distance from stores to city limits is greater in more compact cities, as one would expect. As one moves from the low end to the high end of the compactness range, the expected average distance to city limits goes up by a little short of half a mile. That is precisely the order of magnitude of the distances within which most of the externalities take place.

While much of the country's land area is not contained in an incorporated place, the figures show that the compactness of cities varies considerably within states and metro regions. And while city shapes may not be randomly assigned, there is no obvious a priori reason to suspect that they are correlated with other determinants of local development policy. In the remainder of Table 5 we relate these measures of compactness to the the size of radius and town-level spillovers by estimating the following equation:

$$\begin{aligned}\textit{ln(Stores, Employment)}_{i,t} &= \alpha_i + \alpha_t + \textit{(Defunct)}_{i,t}\beta_1 + \\ &\textit{(Defunct x Compactness Measure)}_{i,t}\beta_2 + \varepsilon_{i,t},\end{aligned}$$

[16]Note that the Schwartzberg ratio is larger when the town is *less* compact, whereas greater values for the other three measures indicate a more compact town.

[17]The first principal component has an eigenvalue of 3.9, and accounts for 79% of the variance. Table 5 and Appendix Table 3 show the main results separately for each compactness measure.

where α_i are town (or neighborhood) fixed effects, α_t are store-type-year fixed effects, Defunct replaces Post x Bankrupt for ease of notation, and β_1 and β_2 measure the size of the spillover and the interaction of the spillover and the compactness ratio. Columns 2 through 7 in Panel A show that the number of stores that go out of business after a big-box bankruptcy is significantly higher in compact towns than in non-compact ones, even though the radial externality does not differ significantly, as shown in Panel B. Columns 9 through 14 show very similar results for the level of employment. What this means is straightforward: even though the radial externalities are the same for big-box stores in compact and non-compact towns, compact towns derive larger town-level externalities from their presence.[18] The absence of an effect for the radius suggests that the interaction effect for the town is causally driven by a town's shape, and not some unmodeled factor. To give a sense of the magnitude of the effect, let us look at a concrete example. As one moves from the less compact city of Silver Spring, MD, to more compact Arlington, VA[19], our estimates in Column 7 and 14 suggest that the number of within-town stores that disappear after a bankruptcy increases by 1.7 percentage points, while the employment drop grows by 1.2 percentage points.

III.b Town Shape and Development Efforts

Having established that localities that are less compact are faced with smaller externalities within their borders, let us see if they also make less of an effort to attract big-box stores. This would make intuitive sense: whether local government officials want to get reelected, maximize tax revenue, or create jobs for local residents, local-development policies are commonly seen as effective instruments to attain their goals. Retail subsidies are a particularly popular way to attract business activity, as 40% of local governments report focusing on them (ICMA, 2009). The question we will ask here is whether it is compact towns that are more likely to turn to such incentives.

We address this question by estimating the following equation:

$$\textit{Retail Focus Development}_i = \beta_0 + \textit{Compactness Index}_i \beta_1 + X_i \gamma + \varepsilon_{i,t},$$

where the dependent variable is an indicator for retail focused development in the ICMA 1999, 2004, and 2009 Economic Development Surveys, the compactness index is as described above, and X_i are various control variables for the municipalities. Results are shown in Panel A of Table 6. Column 1 shows the results of these regressions without controls, and as expected, more compact cities are strongly associated with increased development focus under most measures. Similar results are obtained in Column 2, which reports the coefficients controlling for population decile dummies, state dummies, the logarithm of the median house price, and the percentage of the population that does not speak English, is in poverty, and holds a bachelor's degree. Column 3 controls for the logarithm of the total sum spent on development strategies as well as the total number of development strategies (retail and non-retail) pursued by the town in question. Column 4 controls with dummy variables indicating that towns have a large retail tax base and a sales tax. Column 5 reports the coefficients controlling for Combined Statistical Area (CSA) fixed effects. Across all of these controls and measures, there is a consistent relationship indicating that more compact cities - cities more

[18] Appendix Table 8 makes this point in a different, perhaps more explicit way: it shows that town-level externalities are greater near town borders for more compact town.

[19] Arlington is a county, but its county government is its only general-purpose local government.

likely to capture retailer externalities - are most likely to offer programs aimed at promoting them. Note that Column 5 indicates that this holds even for towns *within* CSAs. This is an important result, because it means that local governments are sensitive to the size of the externalities they face when setting development policy; the extensive battery of town features we control for makes it hard for us to imagine that this relationship is a purely coincidental one or one driven by omitted-variable bias.[20] Let us once again compare Silver Spring, MD, to Arlington, VA, to clarify what this means in more concrete terms. As one moves across the Potomac to Arlington, VA, our estimates in Column 1 through 4 suggest that the likelihood of the town focusing on retail subsidies increases by between 4.5 and 12.1 percentage points.

Instrumental-Variable Estimation

We have shown, first, that the compactness of a town's borders is uncorrelated with the size of economic spillovers size at a fixed radius, second, that compactness is strongly related to the size of the spillover within the town's jurisdiction, particularly along city boundaries, and third, that compactness is predictive of a town offering retail subsidies targeting these spillovers, even among cities within the same metro area. We argue that this relationship is, in part, causal: towns are responding to differences in spillovers.

It is, of course, possible that this spillover-to-subsidy relationship is spurious, despite the anecdotal evidence we have presented on economic development consultancy services. For example, compactness could be correlated with a confounding factors, such as local corruption, that both make retail subsidies more likely and correlate with the size of retail spillovers. We address the possibility that this confounding channel accounts for the entire relationship in three ways.

First, in Panel B of Table 6 we report four different instrumental-variable estimates of the relationship between compactness and retail focus. These instruments draw on very different sources of variation in city shapes: from state laws to geographic features to historical incorporation patterns. Each of these instruments affects the compactness of towns and the size of within-jurisdiction spillovers. There is not an intuitive reason, however, that all of these different instruments should individually be correlated with other factors that make subsidies more likely. The instruments we use are the following:

- An index indicating the stringency of legislation states have adopted regarding annexation by local governments, based on Steinbauer, Hudson, Hayes, and Facer (2002). We code a dummy variable for whether a state requires an impact plan, a service plan, judicial review, and a public petition in order for cities to annex land. Our index is the sum of these dummy variables.

- A dummy variable indicating whether a municipality contains a river within its borders, from the 2011 Multi-Resolution Land Characteristics Consortium's National Land Cover Database.

- An indicator for the presence of wetlands, also from the 2011 Multi-Resolution Land Characteristics

[20] As an additional robustness test, we use the procedure outlined in Altonji, Elder, and Taber (2008) and Oster (2014) to assess potential selection on unobservables. We regress our survey measure of retail focus on population bins, metropolitan-area dummies, a indicator for a large retail sales tax base, the sales tax rate, the number of total dollars spent on economic development, the number of development programs, area controls and year controls. The R-squared relative to the baseline specification then increases from 0.14 to 0.45, and the coefficient on compactness changes from 0.055 to 0.037. Assuming equal selection on unobservables and the suggested $\pi = 1.3$ to select the maximum R-squared, we would still recover a lower-bound coefficient of 0.030 on our compactness measure. The effect therefore seems unlikely to be driven by omitted-variable bias.

Consortium's National Land Cover Database.

- The average compactness of all cities incorporated in the same decade as the city of interest, excluding the city itself. The data on incorporation dates are from the 1987 Census of Governments (Bureau of the Census, 1990). Incorporation rates and average compactness levels correlate and have varied over time, with both series peaking from the Civil War to World War I. The time series of both rates is in Appendix Figure 4.

As a first-pass check, in Table 7 we test whether these instruments for compactness are correlated with the most obvious non-spillover determinants of retail subsidies and shopping centers. We find that there is no relationship between compactness, instrumented for with these variables, and potential confounders like local income, housing prices, and levels of education. While not dispositive, this suggests that any pure omitted-variable bias violating the exclusion restriction for all of the instruments would have to operate quite subtlely (that is, not through income, housing prices, etc.).

Though the instruments are not correlated with these potential confounders, we can see in the second panel of Table 6 that compactness as identified using each of these instruments individually is strongly predictive of retail subsidy offerings. This is true across and within metro areas, as well as conditional on a large suite of controls.[21]

Interestingly, our IV estimates are larger than our OLS estimates. Part of this is due to measurement error. As shown in Appendix Figure 5 for Clearwater, FL, the official boundaries used by the Census for many cities exaggerate their non-compactness. Boundaries can differ for voting rights, taxation authority, city services, and many other purposes. The carve-outs shown in the figure are pervasive in the data, but as far as we can tell they do not reflect true carve-outs with respect to a town's political economy or tax base. As such, these carve-outs introduce substantial measurement error into our compactness measure, biasing our result towards zero. Our IV estimates do not suffer from this problem.

Our second approach to testing whether the spillover-to-subsidy relationship is entirely spurious is to check whether compact and non-compact towns act similarly when it comes to non-foot traffic industries. Our causal hypothesis predicts differences only in retail, whereas one might suppose that an omitted factor would manifest itself in other subsidies as well. Panel A of Appendix Table 10 shows regressions similar to Table 6 for retail and non-retail development programs in the 2009 ICMA wave. We find no relationship between compactness, using OLS and instrumented with our four instruments from Panel B of Table 6, and subsidies for agriculture, nonprofit institutions, tourism, transportation, technology, and "other" activities. We only find a relationship with retail subsidies. Again, this suggests a causal relationship between foot traffic spillovers and retail development.

To strengthen this point, we turn to a database of actual subsidies. This database, previously used in

[21] The relationship between each instrument and a city's compactness, the first stage in our IV estimates, can be found in Appendix Table 9. The instruments are strongly predictive of a city's realized compactness index. When using all four instruments concurrently, we estimate a coefficient on compactness of 0.194 (SE: 0.053) when controlling only for year and area (clustered by state). The first stage F-statistic equals 17.6, while the Hansen J-statistic equals 2.107 (p-value: 0.55) rejecting overidentification. When controlling for CSA fixed effects (which renders state law variation unusable), we get a point estimate of 0.144 (SE: 0.047), a first stage F-statistic of 27.8 and a Hansen's J-statistic of 1.2 (p-value: 0.55).

Story, Fehr, and Watkins (2012) and Ossa (2015), was collected by Good Jobs First (GJF), a non-profit organization in Washington, D.C., in its Subsidy Tracker, and reflects subsidies provided to corporations across the United States. We include towns in which GJF observed at least one subsidy program since the start of its dataset in 1976, and construct dummy variables for each of these local governments that indicate whether there was at least one retail and at least one non-retail program in the locality. Although these data are subject to selection bias (though not obviously selected on compactness), they have the advantage of being derived from behavior, not expressed intent. In Panel B of Appendix Table 10, we show the results of a regression of these dummy variables on our compactness measure (also, again, as instrumented for with our four instruments from Panel B of Table 6). Our results confirm our central findings in this section: more compact towns, those that can capture a larger share of the externalities big-box stores create, see more of a focus on retail development than less compact ones.

Finally, we once again note that compactness matters for economic activity near town boundaries. This implies that compactness should be a larger determinant of subsidies in small jurisdictions. While we continue to control for area as in Table 6 and Appendix Table 10, our regressions in Appendix Table 11 also split the sample at the median based on jurisdiction size. We then report the impact of compactness, again using OLS and IV, for large and small cities. As predicted, we find a much larger effect in towns where the border effects are likely to be larger. It also implies that all else equal, more compact towns should be more likely to provide subsidies close to town borders, where the potential for "leakage" is particularly high. Figure 6 shows that this is indeed the case.

The combined weight of these results, we argue, indicates that cities with larger potential spillovers are more likely to offer retail subsidies. An alternative explanation of the facts requires something correlated independently with each of four instruments and compactness itself that is not also correlated with city-level income, house prices, population density, education levels, and municipal expenditures and revenues. This omitted variable would also have to affect retail subsidies alone, not other industries, and operate primarily in smaller towns where more of the jurisdiction is proximate to the border. Given this evidence and the anecdotal evidence from our survey of economic development consultants, we think the msot compelling interpretation is that compactness and spillovers indeed causally affect local government behavior.

IV Private Subsidies and Compactness

The previous section demonstrated that measures of city compactness are strongly correlated, not with the size of overall retail externalities, but with the size of the externality effect falling within city borders. Local governments are seemingly aware of this phenomenon, and more compact municipal governments target retail development more aggressively than less compact governments do. This, in turn, takes us back to Coase's observation from the introduction, that well-defined property rights and limited transaction costs should allow for private-sector mechanisms to internalize spillovers like the ones we have identified. As long as the land surrounding potential big-box stores can be controlled by the same party, or the owners of nearby stores can resolve their collective-action problem, there ought to be no need for government intervention. Such private-sector mechanisms have indeed come to fruition. In this section we study two of them: shopping malls and Business Improvement Districts (BIDs). The central questions we address here are whether these organizations actually subsidize big-box stores, and whether public subsidies crowd out these private alternatives.

IV.a Shopping Malls

Shopping malls are enclosed shopping areas that contain a large variety of different retail establishments. Gould et al. (2005) demonstrate that anchor stores in shopping centers pay substantially discounted rents, with 73% of anchor stores in their sample paying no rent at all to the mall owners. We present similar results in Table 8, based on data from ICSC (2004). Anchor stores pay much lower rental rates than other stores no matter how large they are, for an average of \$1.84 per square foot, as compared to an operating cost of \$16.37 per square foot for the mall.

This steep discount is recouped by shopping center ownership via the premium charged on rental space to non-anchor stores that hope to benefit from the externalities (foot traffic) provided by the anchor stores. As an example, food court tenants in our sample pay over \$60 per square foot. In this manner, joint ownership of both the anchor and non-anchor stores in the form of a shopping center or mall provides a private-sector mechanism for internalizing the externality effects. To test whether this private mechanism is affected by local government subsidization, we analyze data on the prevalence of shopping centers by location. The data, provided by Esri, contain information on over 7,000 geo-coded malls and shopping centers, including store counts and anchor tenant lists. We have been able to match more than 6,000 of these shopping centers to incorporated places for which we have data.[22] We test the relationship between shopping center prevalence and compactness, which as we saw is an important source of variation in a town's propensity to promote retail development, by estimating the following equation:

$$\textit{Shopping Mall Indicator}_i = \beta_0 + \textit{Compactness Index}_i \beta_1 + X_i \gamma + \varepsilon_{i,t}$$

The results for this estimation are presented in Columns 1 through 7 of Table 9. The dependent variables there are a dummy for the existence of a mall or shopping center, In Panel A, the explanatory variable is our compactness index, as described in Section III, except for in Column 1, where it is a dummy for retail focus in the ICMA surveys.[23] In Panel B we instrument for the compactness index as in the previous section.[24] X either contains only land area or land area, population decile dummies and CSA fixed effects. Column 1 shows us that towns with more of a retail focus have fewer shopping malls: the probability of having at least one is reduced by seven percentage points there. Such towns are more compact than others, and it should not come as a surprise that Columns 2 and 3 suggest that more compact towns are indeed significantly less likely to contain a shopping center.[25] This result holds in all of our specifications, and suggests that the aggressive

[22] Selected summary statistics are shown in Appendix Table 12.

[23] Column 1 through 4 of Appendix Table 13 show similar results for our different compactness measures instead of the aggregate index.

[24] Appendix Table 14 shows the same estimates using individual instruments.

[25] We again use the Oster (2014) procedure to assess the selection-on-unobservables problem. Using the suggested $\pi = 1.3$, and

attempts to attract retail carried out by compact towns make it less likely for those towns to harbor shopping centers.

Column 4 through 7 of Table 9 explore where these marginal shopping malls are, and finds that they are largely located within 2 miles of the city limits, as expected: those are, after all, the areas that the governments of compact towns will disproportionately focus their subsidies on compared to those of non-compact towns.[26] Column 8 through 11 study the intensive margin: the log number of malls/centers, and the log number of stores in malls/centers, conditioning on the existence of a major shopping center. They demonstrate that, even conditional on having a major shopping center, more compact towns will have fewer such centers and fewer stores within such centers. Note that these results hold even for different towns within the same CSA. In addition, Figure 7 shows that in more compact towns, the percentage of stores that find a home in shopping centers is also lower. All in all, the evidence for public-sector development initiatives crowding out shopping centers is strong and consistent.

IV.b Business Improvement Districts

The second private-sector mechanism that can substitute for local-government provision of incentives to internalize externalities is the establishment and operation of Business Improvement Districts (Brooks, 2007). In some jurisdictions, property owners in commercial neighborhoods can agree to found such districts by majority vote. BIDs then have the power to tax and spend. Assessments are mandatory and typically used for the provision of public goods, such as safety (Brooks, 2008). That said, the net benefits from BIDs do not flow to all participants equally. Brooks and Strange (2011) find that it is typically the large anchor stores participating that reap the direct monetary benefits, much like in a shopping mall.

We would therefore expect for local-government subsidies to big-box stores to affect BIDs in a way similar to what we saw for shopping malls, and the tests we run are similar as well. Instead of our shopping mall-related dependent variables, we create a dummy variable that indicates whether a town contains a BID using data from the Rose Institute of State & Local Government (2013). We then estimate how much less likely a more compact town is to see a BID arise, and as before we present our results both with and without controls, and based on both OLS and IV specifications. What we find is, as Column 12 and 13 of Table 9 show, that BIDs are more likely in less compact towns. To return to our example from the previous section: as one moves from a city a compact as Silver Spring to one as compact as Arlington, the likelihood of encountering a BID increases by 14 to 20 percentage points,[27] confirming our findings regarding shopping

comparing Columns 2 and 3 of Table 9, we find that we would still recover a lower-bound effect of -0.01 if unobservables were equally selected. Again, this test suggests that omitted-variable bios does not fully drive the relationship between compactness and malls or shopping centers.

[26]The effect of compactness on the likelihood of having a mall decreases with distance to the border examined. A 1 standard deviation increase in compactness makes a town 4.1 percentage points less likely to have a town within half a mile of the town border. The same increase in compactness is associated with a 1.4 percentage point decrease in the likelihood of having a mall 0.5-1.0 miles from the border, a 0.5 percentage point decrease in the likelihood of having a mall $1.0 - 1.5$ miles from the border, and a 0.2 percent decline in the likelihood of having a mall $1.5 - 2.0$ miles from the border. The effect becomes statistically insignificant at greater distances.

[27]Column 5 and 6 of Appendix Table 13 show similar results for our different compactness measures instead of the aggregate index.

malls.[28] It is striking that these two private-sector alternatives to direct government subsidies both appear to be crowded out by said subsidies, in spite of the quite different ways in which they are brought to life.

V Discussion and Conclusion

We have, so far, reported three central empirical findings. First, using detailed data on households and establishments, we established that there are large, positive local externalities associated with big-box stores that increase the number of non-competing businesses nearby, as well as employment at those businesses. Second, local government recognize that these externalities exist, and use subsidies and tax expenditures to attract big-box stores to their towns more aggressively the more the externalities are contained within their locality. And third, we observe that these public programs make private-sector initiatives through which the externalities can be internalized, such as shopping malls or BIDs, less common. The latter finding is consistent with some standard crowd-out models, although it is certainly not a necessary implication of all of them.

The relative efficiency of local government subsidies versus private sector mechanisms is a difficult question. In Table 5, we saw that compactness (a strong predictor of public intervention) was not significantly linked to the economic externality at fixed distances. This suggests that public subsidies do not impact the efficiency of the retailer's location choices as measured by these external effects. It is important to realize that this result - a manifestation of Coase's (1960) central insight - is only true in the aggregate. The localities where big-box stores end up, and where their sizable positive spillovers materialize, may well benefit from well-crafted development policies if crowd-out is not complete and the subsidies offered are smaller than the spillovers realized.[29]

A rough back-of-the-envelope calculation shows that this may well be the case on many occasions. Using Esri's assessments of sales, and following an approach similar to that used in Section II, we estimate a 3.1% increase in sales within a mile of one of our big-box stores, or a \$35 to \$40 million annual swing for the median neighborhood. The externality is even bigger for the median town as a whole: north of \$100 million a year. Now, this is total sales, not just increased income to local store owners, tax revenue, consumer surplus, and salary hikes, but it dwarfs the average subsidy paid to attract a Wal-Mart Superstore,\$2.8 million (Mattera and Purinton, 2004). It also dwarfs the cross-subsidy received by anchor stores in shopping malls, which, at \$15 per square foot equals around \$2 to \$3 million for the typical anchor store.

But these ballpark numbers do not even come close to painting a full picture of the role these public policies play in shaping the living and shopping environments of the hundreds of millions of people who live in urban and suburban areas in the United States alone. Although entrepreneurs grew aware of the potential role shopping malls could play in harnessing the agglomeration effect and stimulating almost as soon as cars

[28] It should therefore not come as a surprise that there is, indeed, a BID in Arlington (in Ballston, specifically: `http://www.ballstonbid.com`), whereas Silver Spring, to the best of our knowledge, does not have one.

[29] Even with complete crowd-out there is one party that benefits: the non-big-box stores that now see taxpayer money being used to subsidize the big-box stores instead of their own inflated rent payments. This incidence result is particularly interesting given the political rhetoric that often surrounds these subsidies, one example of which can be found in Mattera and Purinton (2004).

came into wide-spread use and suburbs started being developed, it was not until changes in federal tax policy in the mid-1950s made the construction of new commercial real estate more attractive that they started to spread rapidly (Hanchett, 1996). As the percentage of the U.S. metropolitan population living in central cities went down from 69% in 1940 to 40% in 1990, businesses followed their customers and employees to suburbs and malls, large firms first, small firms later, especially in retail (Steinness, 1982). Perhaps as a response to these developments, the use of local development policies started growing rapidly in the 1970s (Andersson and Wassmer, 2000).[30] Based on our findings we would expect for this to lead to a decrease in the number of shopping malls opening up.[31] This is indeed what has happened, potentially setting the stage for the revival of bustling downtown leisure areas.

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[30]Appendix Figure 6 depicts the use of the term "development initiatives" in books published in the last 2000, seemingly confirming this trend.

[31]We provide suggestive evidence of this effect in Appendix Table 15. Tax Increment Financing districts (TIFs) are a popular tool used by local governments to subsidize retail development. As many as 46% of the cities pursuing retail development in the 2009 ICMA data report using this tool. TIFs can only be authorized after state-level enabling legislation is enacted, which happened at different times in different states. In Appendix Table 15, we regress the number of malls per capita opened for each state year on dummies for state, year, and whether TIF-enabling legislation has been enacted. We find that fewer malls opened after local governments had been authorized to use TIFs, expanding their toolset with which to provide retail subsidies.

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Table 1. Summary Statistics

	Book Stores	Clothing Stores	Electronic Stores	Linens Stores
A: Store Level Variables (2006)				
Employment	34.7	68.7	42.1	24.0
	(26.8)	(83.0)	(60.5)	(38.4)
Sales ($1000s)	4,619.0	12,071.0	10,875.9	3,304.0
	(3,553.6)	(11,192.8)	(11,351.3)	(3,433.7)
B: 1 Mile Radius Neighborhoods (2006)				
Stores	1,703.2	642.3	1,000.0	770.8
	(5,765.0)	(578.1)	(3,830.1)	(1,960.4)
Employment	21,214.2	7,153.9	12,170.9	9,668.1
	(64,103.4)	(7,267.1)	(40,808.2)	(24,194.7)
Sales ($1,000,000)	3,304	1,248	2,115	1,639
	(11,006)	(1,026)	(7,401)	(3,868)
C: Zip Level Variables (2000)				
Median House Sale Price	328,780	328,295	290,971	290,790
	(218,357)	(188,198)	(177,544)	(164,510)
Percent Black	.137	.076	.135	0.126
	(.125)	(.080)	(.115)	(.111)
D: City Level Variables				
Income per capita	30,173	26,127	29,007	29,308
	(11,625)	(7,848)	(9,329)	(9,257)
Population	478,879	222,043	370,021	289,945
	(1,400,423)	(452,012)	1,125,635	(921,749)
Population Density (pop/mi^2)	4,165	2,854	3,547	3,208
	(4,701)	(1,981)	(4,087)	(3,251)

Notes: Data from Esri and the Census Bureau. Standard deviations in parentheses.

Table 2. Exogeneity Check for Establishments: Pre-Trends

Dependent Variable	Δ ln Stores 2006-2007	Δ ln Employment 2006-2007	Δ ln Stores 2003-2007	Δ ln Employment 2003-2007
	(1)	(2)	(3)	(4)
0.5 Mile Radius				
Eventually Defunct Store	-0.00233	-0.0125	0.00139	-0.00477
	(0.00545)	(0.00759)	(0.00438)	(0.00477)
1 Mile Radius				
Eventually Defunct Store	-0.00229	-0.00518	0.00278	-0.00561
	(0.00341)	(0.00475)	(0.00366)	(0.00398)
2 Mile Radius				
Eventually Defunct Store	-0.00044	-0.00315	0.00409	-0.00485*
	(0.00217)	(0.00303)	(0.00294)	(0.00294)
3 Mile Radius				
Eventually Defunct Store	0.000853	-0.000962	0.00127	-0.00427
	(0.00178)	(0.00230)	(0.00316)	(0.00332)
N	7,003	7,003	6,174	5,355

Notes: The observations are outcomes for geographic neighborhoods of different sizes surrounding big box retailers. Each coefficient represents a separate regression.All retail chains were extant through 2007, with defunct stores closing between 2008 and 2012. A list of retailers can be found in the text. The reported coefficients are from a regression of the change in log stores and employment from 2006-2007 or 2003-2007 on a dummy for those chains that eventually close. The data for 2003 were provided in a different format. Due to concerns about data quality in some areas (we remove >40% swings), this generated different observation counts across columns.The table demonstrates the absence of pre-closure trend differences. Standard errors are clustered by zip code.

Table 3. Spillovers from Big-Box Stores: Establishments and Employment

	0.25-Mile Radius		0.5-Mile Radius		1 Mile Radius		2 Mile Radius	
Defunct - Non Defunct Pair	ln Stores (1)	ln Employment (2)	ln Stores (3)	ln Employment (4)	ln Stores (5)	ln Employment (6)	ln Stores (7)	ln Employment (8)
Panel A								
Pooled								
Defunct	-0.117***	-0.131***	-0.0652***	-0.0798***	-0.0300***	-0.0314***	-0.0182***	-0.0105*
	(0.0134)	(0.0157)	(0.00884)	(0.0114)	(0.00472)	(0.00658)	(0.00320)	(0.00450)
With Zip $\times$ Year Fixed Effects								
(i.e. 02138 in 2010, 02138 in 2011,etc.)								
Defunct	-0.0819***	-0.0754***	-0.0374***	-0.0344***	-0.00805*	-0.0110*	-0.00196	-0.00553
	(0.0184)	(0.0209)	(0.0103)	(0.0123)	(0.00444)	(0.00577)	(0.00225)	(0.00352)
Neighborhoods with Only 1 Store per								
Type in Zip Code								
Defunct	-0.107***	-0.0953***	-0.0548***	-0.0594***	-0.0312***	-0.0284***	-0.0206***	-0.0167***
	(0.0208)	(0.0240)	(0.0140)	(0.0169)	(0.00701)	(0.00991)	(0.00442)	(0.00609)
Panel B								
Neighborhoods Around Book Stores								
Defunct	-0.116***	-0.0998***	-0.0470***	-0.0847***	-0.0201**	-0.0308**	-0.00513	-0.0186**
	(0.0263)	(0.0312)	(0.0157)	(0.0211)	(0.00832)	(0.0121)	(0.00564)	(0.00903)
Neighborhoods Around Electronics Stores								
Defunct	-0.121***	-0.145***	-0.0815***	-0.0641***	-0.0364***	-0.0325***	-0.0222***	-0.0147**
	(0.0230)	(0.0266)	(0.0148)	(0.0183)	(0.00745)	(0.0105)	(0.00459)	(0.00646)
Neighborhoods Around Clothing Stores								
Defunct	-0.122***	-0.185***	-0.0948***	-0.0955***	-0.0586***	-0.0472***	-0.0380***	-0.0278***
	(0.0278)	(0.0325)	(0.0181)	(0.0208)	(0.0129)	(0.0149)	(0.00870)	(0.00969)
Neighborhoods Around Linen Stores								
Defunct	-0.111***	-0.129***	-0.0513***	-0.0873***	-0.0184*	-0.0207	-0.00740	0.00578
	(0.0286)	(0.0346)	(0.0184)	(0.0254)	(0.0103)	(0.0143)	(0.00657)	(0.00871)

Notes: This table reports the results of regressions of the form $\ln(y_{it}) = a_i + a_t + b \times \text{Defunct} + u_{it}$ where a_i are fixed effects for the neighborhood (or radius) and a_t are year (or year-store type in pooled specification) fixed effects.The observations are annual outcomes for neighborhoods of various sizes around big-box retailers from 2006-2012. Each coefficient represents a separate regression. Panel B controls for zip-year fixed effects, identifying the effect within zip codes. The retailers included in this regression are: Borders and Barnes & Noble; CompUSA, Circuit City, and Best Buy; JC Penney, Mervyn's, and Kohl's; Bed, Bath, and Beyond, and Linens 'n Things. Defunct is a time-varying dummy equal to 1 if the big-box chain is closed in that year. The standard errors are clustered by zip

Table 4. Spillovers from Big-Box Stores: Incumbency and Industry

	ln Stores			
Defunct - Non Defunct Pair	Retail NAICS 44-45 (1)	Non-Foot Traffic (see list in AT4) (2)	Non-Competitor (3)	Competitor (4)
Defunct	-0.0885***	-0.0109	-0.0321***	0.0418***
	(0.011)	(0.008)	(0.00414)	(0.00841)
N	48896	30052	55807	55807
Defunct - Non Defunct Pair	Entrant (5)	Incumbent (6)	Internet Penetration >Median (7)	Internet Penetration <Median (8)
Defunct	-0.0307***	-0.0244***	-0.0429***	-0.0216***
	(0.00824)	(0.00428)	(0.0123)	(0.00732)
N	47834	47834	24822	25361

Notes: This table reports the results of regressions of the form $\ln(y_{it}) = a_i + a_t + b \times \text{Defunct} + u_{it}$ where a_i are fixed effects for the neighborhood (or radius) and a_t are year-store type fixed effects. The observations are annual outcomes for neighborhoods of half mi around big box retailers from 2006-2012. Each coefficient represents a separate regression. Defunct is a time-varying dummy equal to 1 if the big box chain is closed in that year. A list of industries classed as retail (NAICS 44 & 45) and non-foot traffic (NAICS 11, 21, 22, 23, 31, 32, 33, 42, 48, 49, 51, 55, 56, 92, & 99) are listed in Appendix Table 5. Competitors are establishments in the same four digit NAICS code, otherwise they are a non-competitor. Entrant/Incumbency status is determined annually for each establishment based on existance in the BA data the prior year. Internet penetration is computed at the state level using data from the Census. The standard errors are clustered by zip code.

Table 5. Town Shape and Externalities - Part 1

Panel A	ln Stores (Town)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pooled							
Defunct	-0.00734***	0.0211*	0.00528	0.00939	-0.0188***	0.0254*	-0.00775***
	(0.00253)	(0.0116)	(0.00403)	(0.00775)	(0.00697)	(0.0146)	(0.00215)
Defunct X		-0.0552***	-0.0588***	-0.0761**	0.00333	-0.0488**	-0.00931***
Geographic Ratio		(0.0213)	(0.0149)	(0.0320)	(0.00213)	(0.0209)	(0.00337)
Measure	-	Rectangular Area Fraction	Polsby-Popper	Reock	Schwartzberg (larger is less compact)	Convex Ratio	Compactness Index
N	46358	46358	46358	46358	46358	46358	46358

Panel B	ln Stores (1-Mile Radius)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pooled							
Defunct	-0.0255***	-0.00702	-0.0180***	-0.0139	-0.0338***	-0.00376	-0.0272***
	(0.00482)	(0.0167)	(0.00677)	(0.0129)	(0.00891)	(0.0207)	(0.00457)
Defunct X		-0.0385	-0.0414*	-0.0589	0.00198	-0.0344	-0.00650
Geographic Ratio		(0.0309)	(0.0224)	(0.0528)	(0.00223)	(0.0297)	(0.00467)
Measure	-	Rectangular Area Fraction	Polsby-Popper	Reock	Schwartzberg (larger is less compact)	Convex Ratio	Compactness Index
N	46357	46357	46357	46357	46357	46357	46357

Note: Continued on next page.

Table 5. Town Shape and Externalities - Part 2

Panel A	ln Employment (Town)						
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Pooled							
Defunct	-0.00677**	0.0184	0.000672	0.00446	-0.0177***	0.0223	-0.00576**
	(0.00279)	(0.0115)	(0.00440)	(0.00824)	(0.00574)	(0.0142)	(0.00282)
Defunct X		-0.0461**	-0.0294*	-0.0456	0.00365***	-0.0412**	-0.00690**
Geographic Ratio		(0.0219)	(0.0168)	(0.0343)	(0.00137)	(0.0209)	(0.00315)
Measure	-	Rectangular Area Fraction	Polsby-Popper	Reock	Schwartzberg (larger is less compact)	Convex Ratio	Compactness Index
N	46358	46358	46358	46358	46358	46358	46358

Panel B	ln Employment (1-Mile Radius)						
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Pooled							
Defunct	-0.0303***	-0.0295	-0.0276***	-0.0157	-0.0247**	-0.0423	-0.0303***
	(0.00589)	(0.0222)	(0.00857)	(0.0159)	(0.0101)	(0.0270)	(0.00620)
Defunct X		-0.00152	-0.0116	-0.0634	-0.00174	0.0175	-0.000462
Geographic Ratio		(0.0408)	(0.0258)	(0.0651)	(0.00249)	(0.0385)	(0.00576)
Measure	-	Rectangular Area Fraction	Polsby-Popper	Reock	Schwartzberg (larger is less compact)	Convex Ratio	Compactness Index
N	46355	46355	46355	46355	46355	46355	46355

Notes: This table reports the results of regressions of the form $\ln(y_{it}) = a_i + a_t + b_1$ x Defunct + b_2 x Defunct x Geographic Ratio + u_{it} where a_i are fixed effects for the town in Panel A and neighborhood in Panel B, and the a_t are store-type-year fixed effects. The observations in Panel A are the annual outcomes for the town/political unit containing big-box retailers. The observations in Panel B are annual outcomes for the 1-mi neighborhoods around big-box retailers. Defunct is a time-varying dummy equal to 1 if the big-box chain is closed in that year. The geographic ratios are defined in the text. Note that a lower Scwartzberg ratio indicates a more compact shape, unlike the other measures. The standard errors are clustered by zip.

Table 6. Compactness and Local Development Policy

Dependent Variable	Survey Retail Bidding Indicator				
Compactness	(1)	(2)	(3)	(4)	(5)
Panel A					
OLS	0.0553***	0.0679***	0.0545***	0.0328**	0.0254*
	(0.0141)	(0.0129)	(0.0130)	(0.0155)	(0.0135)
N	2015	2015	1562	1993	2015
Panel B					
State Annex. Law IV	0.170**	-	0.190**	0.185**	-
	(0.0751)		(0.0815)	(0.0797)	
First Stage F-Statistic	17.6		14.7	14.3	
Rivers IV	0.454***	0.205**	0.529**	0.347**	0.314***
	(0.168)	(0.0882)	(0.214)	(0.148)	(0.111)
First Stage F-Statistic	7.8	10.9	9.0	7.5	14.9
Wetlands IV	0.246***	0.169**	0.453***	0.203**	0.148**
	(0.0738)	(0.0808)	(0.135)	(0.0812)	(0.0640)
First Stage F-Statistic	22.8	34.0	12.5	20.7	45.0
Incorp. Date IV	0.184***	0.120*	0.261***	0.173***	0.128*
	(0.0672)	(0.0716)	(0.0849)	(0.0647)	(0.0693)
First Stage F-Statistic	30.9	27.8	37.5	31.4	31.9
Controls	Area	Area, Pop & State Dummies, Log Med. House Prices, Share in Poverty, Share Non-English	Area, Total Development Strategies Development Budget	Area, Large Retail Base, Sales Tax	Area, Combined Statistical Area Fixed Effects

Notes: The table reports regressions of the form Retail Subsidies$_i$ = a+b x Compactness + cX$_i$ +u$_i$i where retail subsidies is an indicator for reporting retail development focus in the ICMA 1999, 2004, and 2009 Economic Development Surveys. Each coefficient represents a separate regression. The compactness measure is described in the text. All regressions include controls for area and year. Column 1 regressions include no additional control variables. Column 2 reports the coefficients controlling for population bin dummies, state dummies, log median house price, and the percentages of the population that do not speak English, are in poverty, and hold a bachelor's degree. Column 3 controls for the log dollars spent on development strategies and the total number of development strategies (retail and non-retail) pursued by the responding town. Column 4 regressions control for the responding town's indication that they have a large retail tax base and a sales tax. Column 5 reports the coefficients controlling for metro (CSA) fixed effects. Details about the instruments are included in the text. Note that the state-level IV strategy can not be used in columns 2 and 5. Standard errors clustered by state in parentheses.

Table 7. Exogeneity of Instrumented Compactness

	Log Income Per Capita	Log Median House Price	Log Pop Per Sqmi	Percent Non-English Speaker	Percent College Grad	Unemployment Rate	Local Gov't Expenditure Per Capita	Local Gov't Revenue Per Capita
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Compactness	0.0152	-0.101	0.0914	-1.576	-2.060	-0.448	-0.142	-0.220
	(0.0393)	(0.103)	(0.186)	(2.779)	(1.395)	(0.436)	(0.438)	(0.462)
First Stage F-Statistic	27.4	28.0	27.4	27.4	27.4	27.4	27.3	27.3
N	15492	15350	15533	15525	15523	15513	15510	15510

Notse: This test examines whether the instruments for compactness are orthogonal to other potential determinants of retail subsidies and shopping centers. Observations are Census places. Demographic data from 2000 are from the Census Bureau. and 2002 local government expenditure data from the Census of Governments. The absence of any relationship between these variables and instrumented compactness lessens concerns that the observed relationship between compactness and retail subsidies is spurious. Following Tables 6 and 8, standard errors are clustered by state. Results remain insignificant when clustering at the CSA level.

Table 8. Rental Contracts for Anchor and Non-Anchor Stores

Median Rental Income Per Sqft	Total Sample	Less than 500,000 Sqft	500,000 to 799,999	800,000 to 999,999	1,000,000 or Greater
Rental Income					
Anchors	$1.84	$3.91	$1.98	$2.13	$1.14
Mall Stores	$22.67	$13.33	$20.70	$21.78	$26.36
Food Court Tenants	$60.68	$36.19	$62.55	$58.17	$73.39
Outparcels	$8.89	$8.13	$6.85	$8.99	$16.21
Total Rent	$10.45	$9.27	$9.50	$10.20	$11.57
Total Operating Expense	$16.37	$11.94	$14.01	$15.48	$18.20

Source: ICSC (2004) *Shopping Center Operations, Revenue and Expenses,* ICSC Research.

Table 9. Compactness, Shopping Malls and Business Improvement Districts - Part I

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Shopping Center Indicator			
	All Distances			< 2 Mi From Border	< 2 Mi From Border	> 2 Mi From Border	> 2 Mi From Border
Panel A							
Survey Retail Focus	-0.0487*						
	(0.0262)						
Compactness (OLS)		-0.0472***	-0.0175***	-0.0449***	-.0163488***	-.0010104	.0004958
		(0.00568)	(0.00389)	(.0054435)	(.0035139)	(.0008895)	(.0006494)
N	1948	28940	28940	28940	28940	28940	28940
Panel B							
Compactness (IV)		-0.175***	-0.0588***	-.2288186***	-.1219457***	-.0133154***	-.0102008***
		(0.0192)	(0.0121)	(.0221639)	(.0162713)	(.0042783)	(.0030231)
N		28438	28438	28438	28438	28438	28438
Controls	None	X	Population Dummies and CSA Dummies	X	Population Dummies and CSA Dummies	X	Population Dummies and CSA Dummies

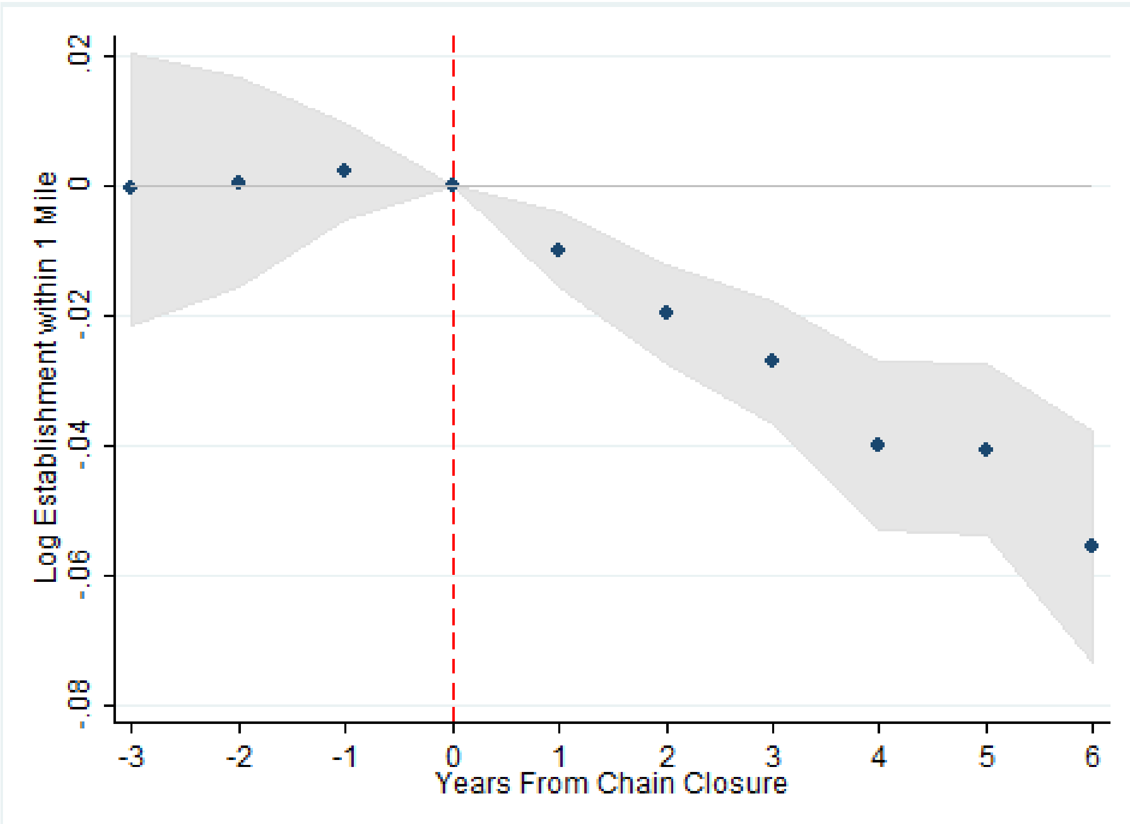
Note: Continued on next page.

Table 9. Compactness, Shopping Malls and Business Improvement Districts - Part II

	(8)	(9)	(10)	(11)	(12)	(13)
	Log (Shopping Centers)		Log (Center Stores)		Business Improvement District Indicator	
Panel A						
Compactness (OLS)	-0.0525**	-0.0747***	-0.0661**	-0.0646*	-0.114**	-0.0790**
	(0.0208)	(0.0234)	(0.0311)	(0.0324)	(0.0348)	(0.0259)
	2219	2219	2217	2217	293	293
Panel B						
Compactness (IV)	-0.252*	-0.366***	-0.361	-0.530***	-0.277***	0.0436**
	(0.149)	(0.136)	(0.248)	(0.199)	(0.0463)	(0.0208)
N	2206	2206	2164	2164	293	293
Controls	X	Population Dummies and CSA Dummies	X	Population Dummies and CSA Dummies	X	Population Dummies and CSA Dummies

Note: The table reports regressions of the form Y_i = a + b x Geographic Ratio + cX_i +u_i where the dependent variable is (in order) a dummy for the existence of a mall or shopping center, the log number of malls/centers, the log number of stores in malls/centers, and the existence of a business improvement district. The exception is column 1, where the explanatory variable is a dummy for retail focus in the ICMA surveys. Each coefficient represents a separate regression and all regressions control for area. Additional controls in both panels are listed in the final row of the table. The geographic ratios are described in the text. The instruments are the river and wetlands indicators, and again,details about the instruments are included in the text . Standard errors clustered by state in parentheses.

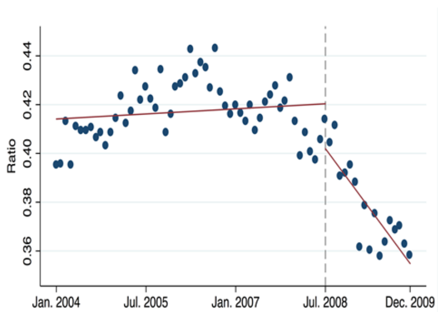
Figure 1. Event Study



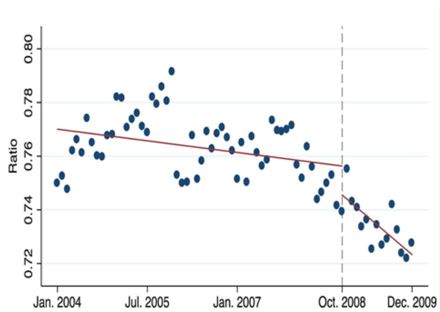
Notes: This figure displays the coefficients bt and confidence interval from the regression: $\ln(y_{it})=a_i+a_t$ x 1(t=T)+ b x Defunct x 1(t=T) $+u_{it}$ where a_i are fixed effects for the neighborhood (or radius) and a_t are year fixed effects.The observations are annual outcomes for neighborhoods around big-box retailers from 2006-2012. The retailers included in this regression are: Borders, Barnes & Noble, CompUSA, Circuit City, Best Buy, Mervyn's, JC Penney, Kohl's, Bed, Bath,and Beyond and Linen and Things. Defunct is a time-varying dummy equal to 1 if the big-box chain is closed in that year. The standard errors are clustered by zip.

Figure 2. Spillovers from Big-Box Stores: Consumer Behavior

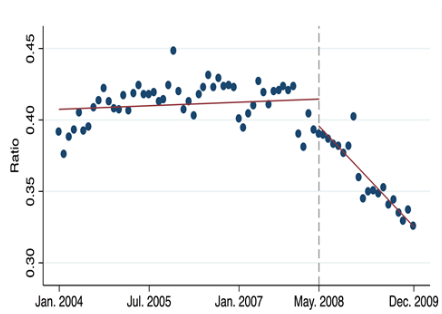
Aggregate Ratios



Electronics: Ratio of Same-Day Visits to Total Visits

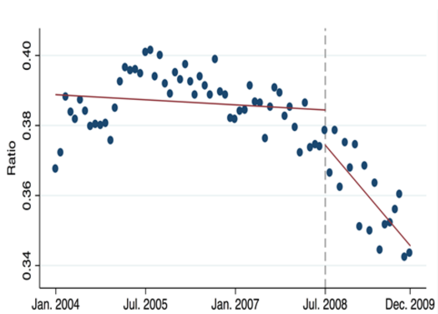


Department: Ratio of Same-Day Visits to Total Visits

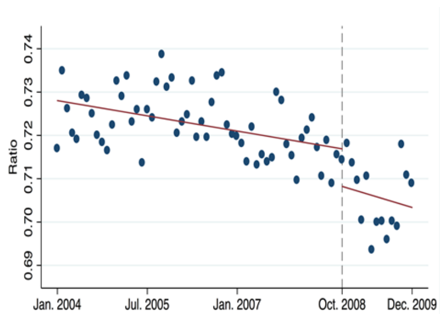


Furnishings: Ratio of Same-Day Visits to Total Visits

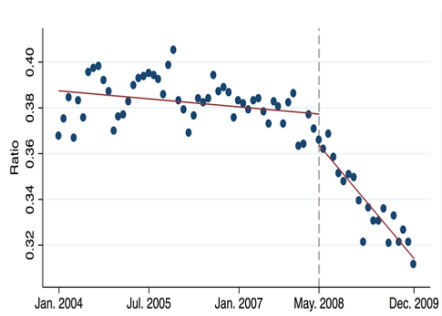
Individual Fixed Effects



Electronics: Ratio of Same-Day Visits to Total Visits



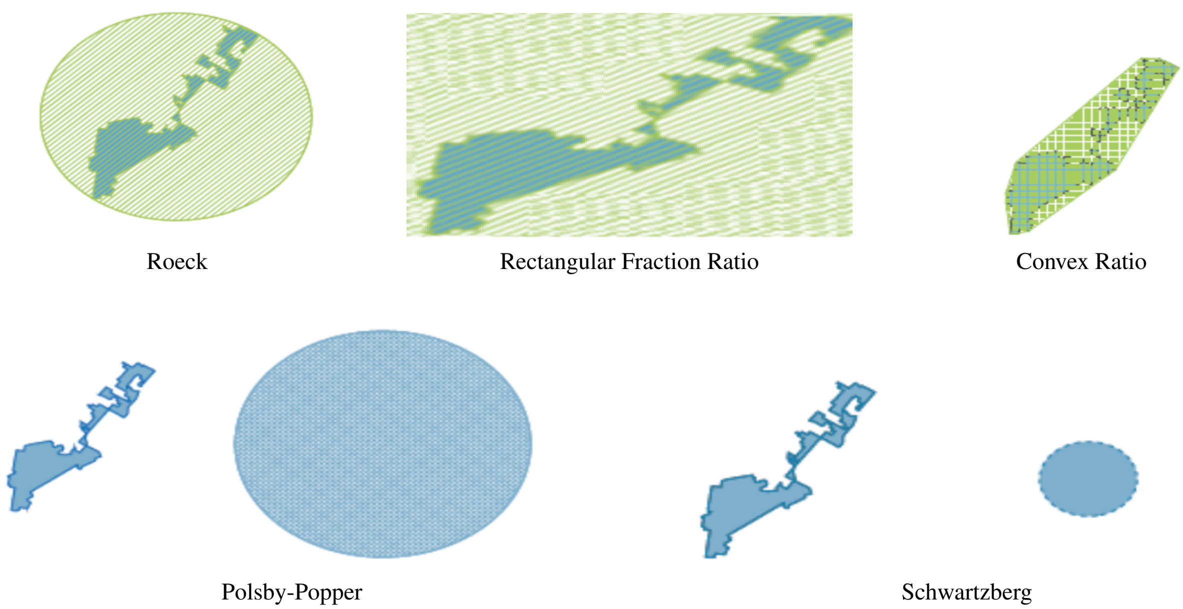
Department: Ratio of Same-Day Visits to Total Visits



Furnishings: Ratio of Same-Day Visits to Total Visits

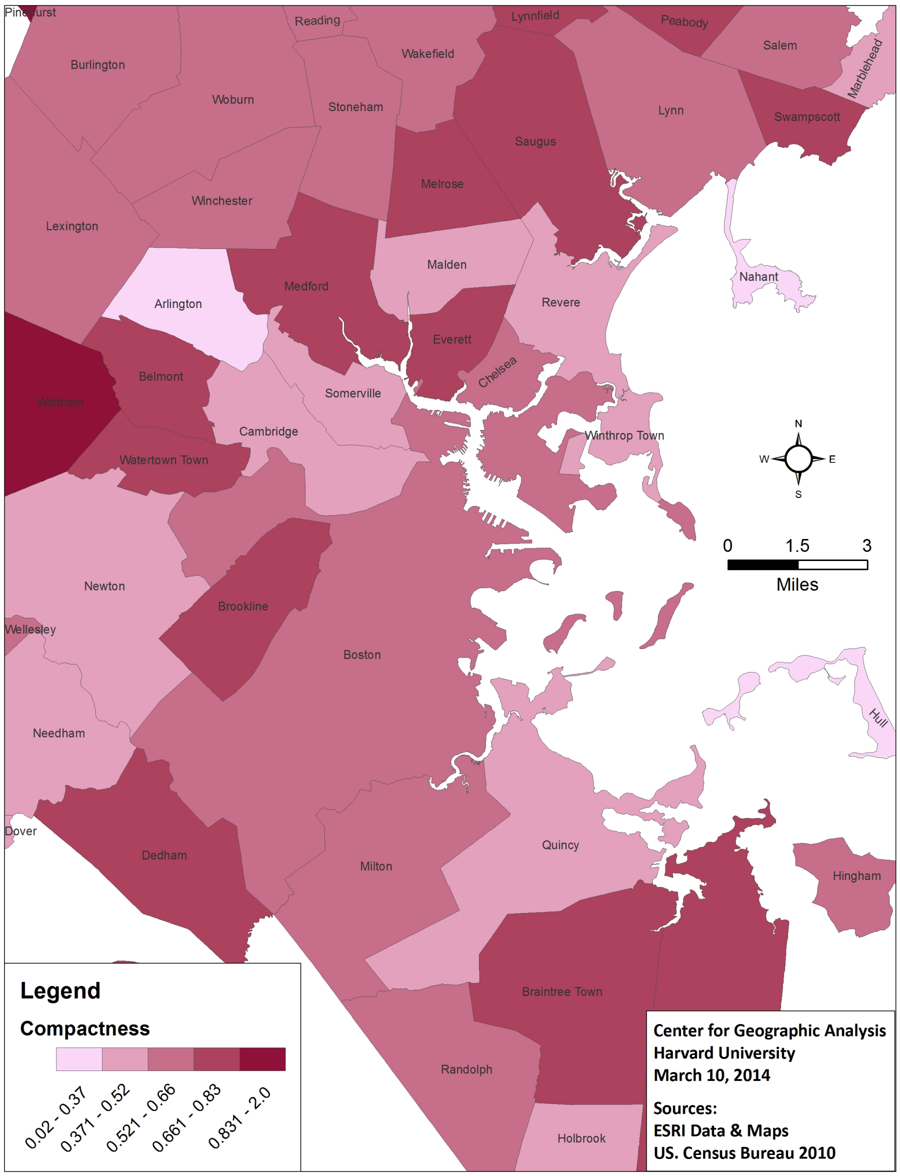
Notes: These graphs depict the ratio of visits at stores visited on the same day as the big-box store to total visits at all stores. Observations are binned at the year-month level and are restricted to households observed in 48 or more of the year-month bins in defunct zip codes only. Closing date is May 2008 for Linens ‘n Things, July 2008 for Circuit City and CompUSA, and October 2008 for Mervyn’s.

Figure 3. Examples of Compactness Measures



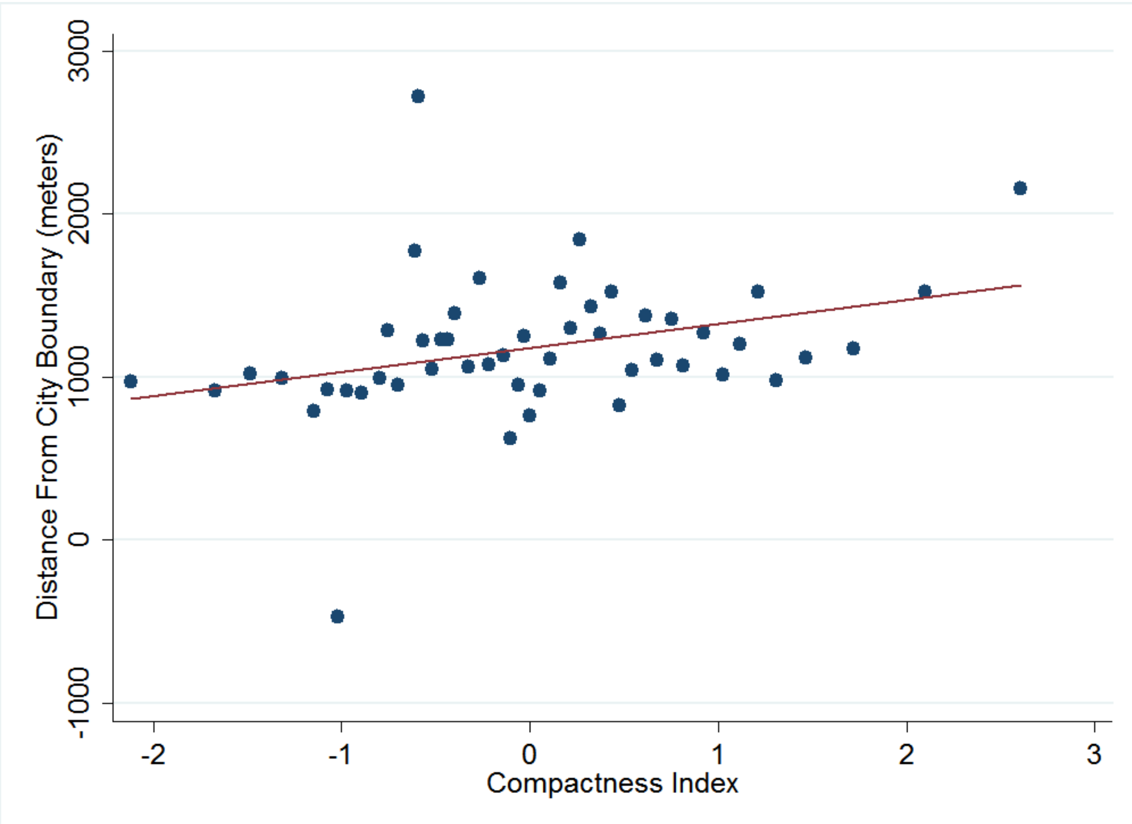
Notes: This figure provides visual examples of the five compactness measures. More details about geographic compactness measures are reported in the text.

Figure 4. Boston Metro Area Compactness Example



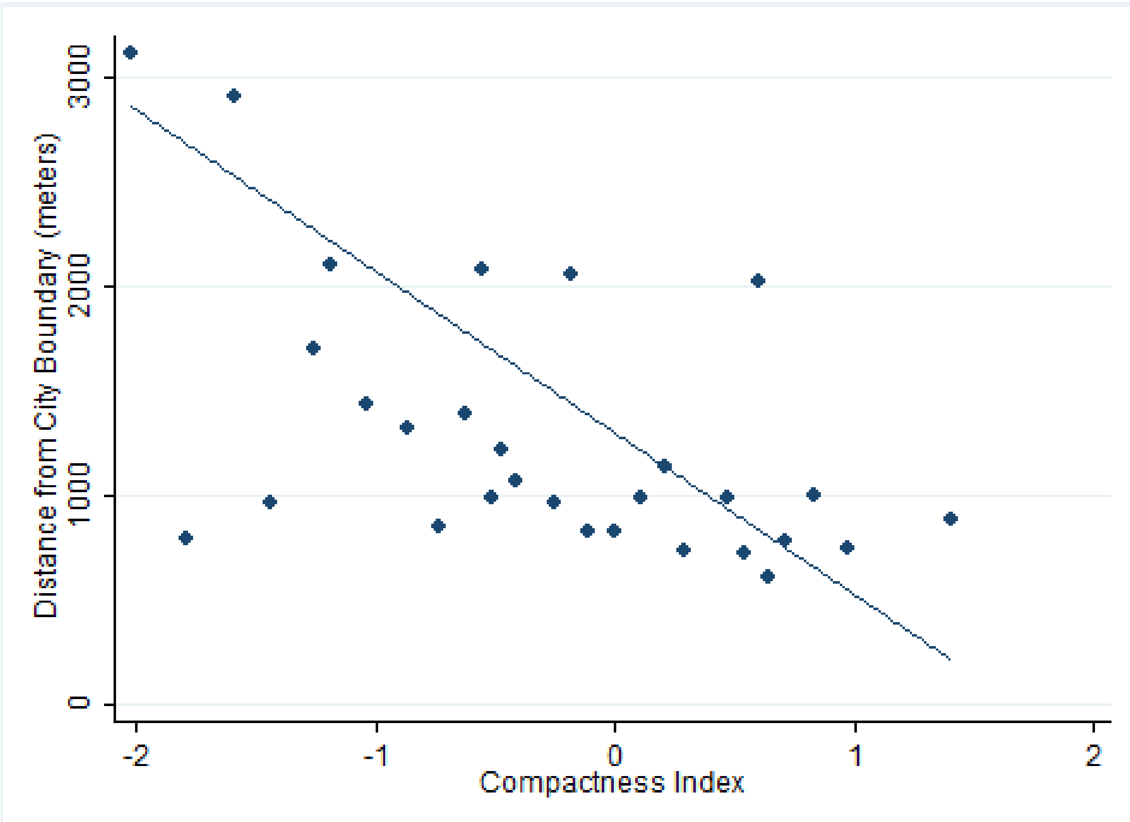
Notes: This map plots the distribution of the rectangular area ratio for Census incorporated places in the Boston Metro Area. More details about geographic compactness measures are reported in the text.

Figure 5. City Compactness Index and Store Distance from Boundary



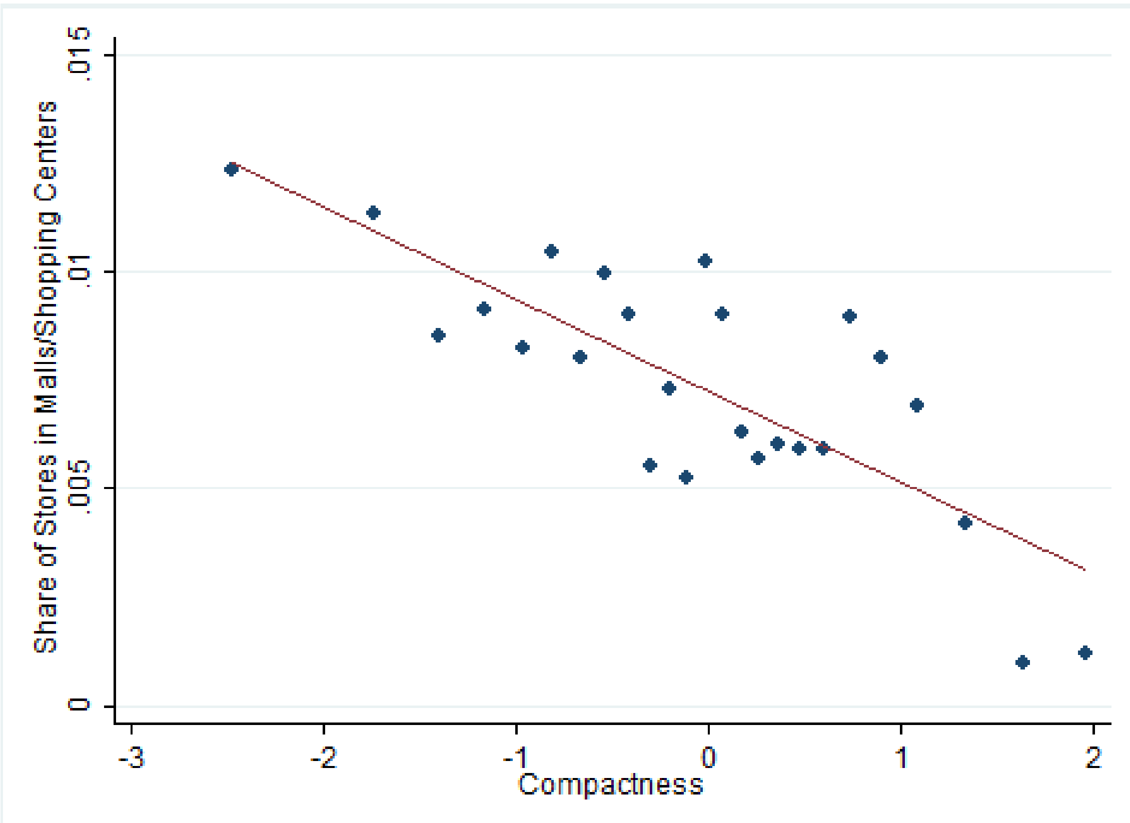
Notes: This figure plots the average distance to the city border (in meters) for big-box stores in our sample against the value of the city's compactness index. The retailers included in this regression are: Borders, Barnes & Noble, CompUSA, Circuit City, Best Buy, Mervyn's, JC Penney, Kohl's, Bed Bath and Beyond, and Linen 'n Things. The construction of the city compactness measure is described in the text. To ease visibility, cities are grouped into 25 bins according to their compactness index, and we plot average distances in meters for each bin.

Figure 6. Distance vs. Compactness



This figure plots the average distance to the city border (in meters) for subsidized projects in the Good Jobs First data against the value of the subsidizing city's compactness index. The figure shows that non-compact cities are more likely to subsidize projects farther away from town boundaries, providing within-city evidence of the spillover-to-subsidy mechanism described in the text. This relationship is statistically significant at the 1% level when clustering by state. Note that Figure 5, shows that big box stores (whether subsidized or not) are also farther from town boundaries, on average, in compact towns. Details regarding the construction of the city compactness measure and the Good Jobs First data are provided in the text. Geocodeable locations were only available for roughly 23% of GJF subsidies. Though GJF data are incomplete, they are not obviously biased is in their coverage by compactness and distance to boundary. To ease visibility, projects are grouped into bins according to their compactness index, and we plot average distances in meters for each bin.

Figure 7. Fraction of Stores in Malls vs. Town Shape



Notes: This figure plots the fraction of establishments in a city that are in a shopping center or mall against our principal measure of city compactness. To ease visibility, cities are grouped in to 25 bins according to their compactness index and average values for each bin are plotted above. Additional description of these variables can be found in the text.

Table A.1. Exogeneity Check: Levels

Dependent Variable	Number Stores 2006	Employment 2006	Number Stores 2003	Employment 2003
	(1)	(2)	(1)	(2)
1/2 Mile Radius				
Eventually Defunct Store	13.40	387.0	-1.852	-168.5
	(44.26)	(513.2)	(119.2)	(1575.0)
1 Mile Radius				
Eventually Defunct Store	-45.27	-85.48	-150.9	-2928.1
	(119.6)	(1270.5)	(243.5)	(3181.2)
2 Mile Radius				
Eventually Defunct Store	-219.9	-2100.8	-191.6	-4710.4
	(248.0)	(2624.3)	(338.5)	(4293.1)
N	7705	7705	7570	6807

Notes: The observations are outcomes for geographic neighborhoods of different sizes surrounding big-box retailers. Each coefficient represents a separate regression. A list of retailers can be found in the text. All chains were extant in 2006. The reported coefficients are from a regression of the number of stores or employment in 2006 or 2003 on a dummy for those chains that eventually close and a dummy for store type. The table demonstrates the absence of pre-closure differences. Standard errors are clustered by zip code. 2003 Data arrived in a different format and featured significantly lower observation counts.

Table A.2. Spillovers from Big-Box Stores: Establishments and Employment (Census Data)

	Census Tract	Town
Defunct - Non Defunct Pair	ln Employment	ln Employment
	(1)	(2)
Panel A		
Defunct	-0.0142***	-0.0066***
	(0.0030)	(0.0026)
N	45019	41835
Panel B		
Defunct	-0.0140***	-0.0063***
	(0.0033)	(0.0024)
N	40572	41835

Notes: This table reports the results of regressions of the form $\ln(y_{it}) = a_i + a_t + b \times \text{Defunct} + u_{it}$ where a_i are fixed effects for the tract or town and a_t are year fixed effects. The observations are annual outcomes for tracts and towns in the Census LODES data for 2006-2011. This data does not include Massachusetts due to the state's lack of participation. Defunct is a time-varying dummy equal to 1 if the big-box chain is closed in that year. The standard errors are clustered by tract/town. Panel A measures the total employment impact in the tract or town. Panel B uses employment numbers that only include individuals who work and live in the same town. The standard errors are clustered by tract or zip.

Table A.3. Town Shape and Externalities (Census Data)

A	ln Employment (Town)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pooled							
Defunct	-0.0071***	0.0153	0.0038	0.0045	-0.0229***	0.0189	-0.0070***
	(0.0024)	(0.0097)	(0.0037)	(0.0067)	(0.0043)	(0.0121)	(0.0024)
Defunct X Geographic Ratio		-0.0425**	-0.0509***	-0.0505*	0.0048***	-0.0381**	-0.0082***
		(0.0185)	(0.0137)	(0.0294)	(0.0010)	(0.0181)	(0.0026)
Measure	-	Rectangular Area Fraction	Polsby-Popper	Reock	Schwartzberg (larger is less compact)	Convex Ratio	Compactness Index
N	40227	40227	40227	40227	40227	40227	40227

Notes: This table reports the results of regressions of the form $\ln(y_{it}) = a_i + a_t$ + b x Defunct + b2 x Defunct x Geographic Ratio + u_{it} where a_i are fixed effects for the town and the a_t are store-type-year fixed effects. The observations are the annual outcomes for the town/political unit containing big-box retailers. Employment information is from Census LODES data for 2006-2011. This data does not include Massachusetts due to the state's lack of participation. The un-interacted geographic ratio cannot be included due to the fixed effects. Defunct is a time-varying dummy equal to 1 if the big-box chain is closed in that year. The geographic ratios are defined in the text. Note that a lower Scwartzberg ratio indicates a more compact shape, unlike the other measures. The standard errors are clustered by zip.

Table A.4. Spillovers from Big-Box Stores: Establishments (Non-Overlapping Tracts and Cities)

	Panel A: Census Tracts			Panel B: City	
	ln Stores (1)	ln Stores (2)		ln Stores (3)	ln Stores (4)
Defunct within 0.25 mi	-0.0472*** (0.0169)	-0.0507*** (0.0184)	No. Big-Box Stores x Post	0.0003 (0.0006)	0.0001 (0.0006)
Defunct within 0.5 mi	-0.0239* (0.0143)	-0.0258* (0.0155)	No. Defunct Big-Box Stores x Post	-0.0025** (0.0011)	-0.0035*** (0.0013)
Defunct within 1.5 mi	-0.0131*** (0.00392)	-0.0174*** (0.00368)	No. Big-Box Stores x Post x Compactness		0.0001 (0.0004)
Defunct within 2.5 mi	-0.00828* (0.00465)	-0.0148** (0.00683)	No. Defunct Big-Box Stores x Post x Compactness		-0.0026*** (0.0009)
Measure of Defunct Variables	Dummy	Count		Count	Count
Observations	445405	445405		13835	13835

Notes: Panel A of this table reports regressions of the form ln(y_{it}) = a_i + a_t + a x Big-Box Stores x Post at Distance D+ b x Defunct BB Stores at Distance D x Post + u_{it}, where a_i are fixed effects for the census tract and a_t are year fixed effects. The observations are annual outcomes for census tracts within 30 miles of a big-box retailer from 2006-2012. Observations do not overlap and are not duplicated. The retailers included in this regression are: Borders and Barnes & Noble, CompUSA, Circuit City, and Best Buy, Mervyn's, and Kohl's, Bed, Bath, and Beyond and Linen and Things. In column 1 defunct is a time-varying dummy equal to 1 if the big-box chain is closed in that year at that distance to the tract centroid. In column 2 the time-varying defunct measure is a count rather than a dummy. Controls for store counts at greater distances not reported. The standard errors are clustered by census tract. Panel B of this table displays regressions of the form ln(y_{it}) =a_i + a_t+a x Big-Box Stores x Post + b x Defunct BB Stores x Post + u_{it}, where a_i are fixed effects for towns and a_t are year fixed effects. Observations are town-years; towns are not duplicated and do not overlap. In both types of regressions the store measures are counts of big-box stores within the town. In column two, the count measures are interacted with the compactness index for the town. Standard errors are clustered by city.

Table A.5. List of NAICS Codes Classified as Foot Traffic Dependent

Foot Traffic	
Retail Bakeries	Zoos and Gardens
Retail Trade	Amusement Arcades
Postal Service	Gambling
Depository Credit Intermediation	Fitness
Consumer Goods rental	Bowling
Tax preparation services	Drinking Places
Veterinary services	Restaurants and Other
Sports and recreation	Footwear and Leather Repair
Exam prep and tutoring	Personal Care (eg. pedicures)
Pet Care	Photo Finishing
Museums	Religious Organizations
Dry-cleaning and Laundry	
Non-Foot Traffic	
Agriculture	
Mining	
Warehousing and Transportation	
Utilities	

Table A.6. Nielsen Consumer Behavior Data: Total Visits by Type

	Electronics		Furnishings		Department	
	Operating	Defunct	Operating	Defunct	Operating	Defunct
Same-Day Visits	5.5596	4.9577	5.6081	4.9926	6.2150	5.4763
	(4.3555)	(3.9994)	(4.2136)	(4.0537)	(4.5406)	(4.3128)
Different-Day Visits	5.2013	5.0227	5.2152	5.0981	4.0537	4.0069
	(3.7185)	(3.7588)	(3.9165)	(3.7621)	(3.0807)	(2.9403)
Defunct Date	July 2008		May 2008		October 2008	
Households	1081		796		547	

Notes: The table displays the mean of monthly visits in pre- and post-bankruptcy periods with standard deviations in parentheses. Data includes households in defunct zip codes only that were observed in at least 48 months of the sample.

Table A.7. Spillovers from Big-Box Stores: Consumer Behavior

	Electronics	Furnishings	Department
Defunct	-0.01902***	-0.02262***	-0.01384***
	(0.00378)	(0.00448)	(0.00427)
Observations	67,747	49,221	33,037
R^2	0.9018	0.8978	0.9247

Notes: The table depicts a regression of the form: ratio_{it} = b x defunct_t +c_i + u_{it}t where ratio_{it} is the ratio of stores visited on the same day as the big-box store to total visits at all stores, defunct is an indicator for a defunct store and ci is a household fixed effect. Data includes households in defunct zip codes only that were observed in at least 48 months of the sample. Observations are at the household-by-month level, and standard errors are clustered by household.

Table A.8. Compactness Matters More Near Boundaries

	ln Stores (1)
Pooled	
Defunct	-0.0073597**
	(0.0037525)
Defunct X Compactness Index	-.0102568** (0.0035375)
Defunct X Compactness X $>$ 2 mi Border	 .0179528*** (0.0068214)
N	29436

Notes: This table reports the results of regressions of the form $\Delta\ln(y_{it})$=a_i +a_t +b x Defunct + b2 x Defunct x Geographic Ratio +b3*Defunct x Far From Border + b4*Defunct x Far From Borderx Geographic Ratio +u_{it} where a_i are fixed effects for the town in Panel A and neighborhood in Panel B, and the a_t are year fixed effects. The annual outcomes are for the town/ political unit containing big box retailers. The uninteracted geographic ratio can not be included due to the fixed effects. Defunct is a time-varying dummy equal to 1 if the big box chain is closed in that year. The geographic ratios are defined in the text. The standard errors are clustered by zip.

Table A.9. First Stage Regressions: ICMA Sample

	City Compactness Index								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
State Law Annexation Difficulty Index	-0.311***							-0.305***	
	(0.0718)							(0.0686)	
River Dummy		-0.229***	-0.236***					-0.164*	-0.158***
		(0.0788)	(0.0645)					(0.0892)	(0.0601)
Wetlands Dummy				-0.499***	-0.515***			-0.373***	-0.459***
				(0.101)	(0.0806)			(0.0801)	(0.0823)
Incorporation Date Instrument						9.793***	6.520***	9.313***	6.123***
						(1.708)	(1.075)	(1.780)	(1.149)
Additional Controls	None	None	Combined Statistical Area Fixed Effects	None	Combined Statistical Area Fixed Effects	None	Combined Statistical Area Fixed Effects	None	Combined Statistical Area Fixed Effects
N	2015	2015	2015	2015	2015	1868	1868	1868	1868

Notes: The table reports regressions of the form Compactness_i=a+b x Instrument + cX_i +u_i where the outcome variable is the compactness composite measure described in the text and the dependent variables are the instruments described there as well. All regressions control for area, and standard errors are clustered by state (or CSA when CSA fixed effects are included).

Table A.10. Additional Data on Subsidies

	ICMA Development Focus on:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A	Retail	Agriculture	Institution/ Non-Profit	Tourism	Transportation	Technology	Other
Compactness							
OLS	0.0525**	-0.00808	-0.0159	-0.0146	-0.0149	-0.0156	0.000354
	(0.0238)	(0.00657)	(0.0117)	(0.0146)	(0.0122)	(0.0122)	(0.0139)
IV	0.258**	-0.00885	-0.0345	0.00597	-0.0675	-0.0455	-0.00157
	(0.101)	(0.00960)	(0.0312)	(0.0462)	(0.0504)	(0.0380)	(0.0458)
N	643	643	643	643	643	643	643

Panel B	Dummy for a NYTimes / GJF Subsidy Tracker Program in:			
	Retail		Non-Retail	
Compactness	0.0169***	0.0341*	-0.00116	-0.00438
	(0.00564)	(0.0201)	(0.00181)	(0.00664)
Measure	OLS	IV	OLS	IV
N	4259	4253	4259	4253

Notes: Panel A uses data from the 2009 ICMA Economic Development Survey. The dependent variables are dummies for the cities reporting an economic development focus in the listed economic sector. Each coefficient is an independent regression. The compactness measure and instruments are explained in the text. The standard errors are clustered by state, and all regressions control for population bin dummies and area. Panel B uses data from the Good Jobs' First database. Only cities with at least one program are included in the analysis. Retail and Non-Retail programs were coded based on the programs description and recipient. The dependent variable are dummies indicating at least one retail or non-retail program for the city. Again, the compactness measures and instruments are explained in the text. All regressions control for population bin dummies and area.

Table A.11. Effect of Compactness By Town Size

	Survey Retail Focus	
	(1)	(2)
Panel A		
Compactness (OLS)	0.0554402**	0.02501
	(0.0229512)	(0.0165188)
Area	Below Median	Above Median
N	1010	1005
	(3)	(4)
Panel B		
Compactness (IV)	0.3280543***	0.0899482
	(0.1144617)	(0.054939)
Area	Below Median	Above Median
N	1010	1005

Notes: The table reports regressions of the form Retail Subsidies$_i$=a+b x Geographic Ratio + cX$_i$ +u$_i$ where retail subsidies is an indicator for reporting retail development focus in the ICMA 1999, 2004, and 2009 Economic Development Surveys. Each coefficient represents a separate regression. The compactness measure is described in the text. All regressions include controls for area and year. Columns 1 and 2 show the same specifications run on the sample of jurisdictions with smaller / larger than sample median areas respectively. Details about the instruments is included in the text. Standard errors clustered by state in parentheses.

Table A.12. Summary Statistics

Shopping Centers & Malls: Median Statistics	
Gross Leasable Area	337,648
Total Sales	78,500,000
Number of Stores	33
Anchor Square Footage	184,544
Most Common Anchors:	JC Penney
	Walmart
	Target
	Sears
	Macy's
	Kohl's
	Home Depot
	Lowe's
	Ross Dress for Less
	Best Buy
	Mashalls
	Bed, Bath, & Beyond
Kosmont-Rose Cost of Doing Business Survey:	
Business Improvement Districts	46.4%
Taxable Retail Store Sales (Median)	909,708,100
Population (Median)	77,500
Sales Tax Rate	8.25%

Notes: The table reports selected summary statistics from Esri Shopping Centers and Malls database and from the Kosmont-Rose Institute at Claremont McKenna College, Cost of Doing Business Survey 2013.

Table A.13. Town Shape, Shopping Malls, and Business Improvement Districts

	(1)	(2)	(3)	(4)	(5)	(6)
	Subsidy Indicator		Mall Indicator		Business Improvement District Indicator	
Rectangular Area Fraction	0.336***	0.335***	-0.179***	-0.0603***	-0.958**	-0.940**
	(0.0924)	(0.0927)	(0.0222)	(0.0159)	(0.342)	(0.336)
Polsby-Popper	0.261***	0.242***	-0.268***	-0.0984***	-0.867***	-0.720***
	(0.0877)	(0.0745)	(0.0271)	(0.0188)	(0.0970)	(0.0851)
Roeck	0.218	0.271*	-0.174***	-0.0571***	-0.508*	-0.594*
	(0.149)	(0.152)	(0.0281)	(0.0198)	(0.252)	(0.263)
Schwartzburg (larger is less compact)	-0.0226**	-0.0162*	0.0700***	0.0303***	0.0740	0.0624
	(0.0104)	(0.00959)	(0.0111)	(0.00702)	(0.0541)	(0.0525)
Convex Ratio	0.320***	0.307***	-0.0472***	-0.0182***	-0.831**	-0.790**
	(0.0850)	(0.0873)	(0.00568)	(0.00398)	(0.302)	(0.305)
Additional Controls	None	Pop and State Dummies	None	Pop and State Dummies	None	Pop and State Dummies
N	2022	2022	28940	28940	293	293

Notes: The table reports regressions of the form $y_i = a_i + b \times \text{Geographic Ratio} + cX_i + u_i$ where the outcome variables are indicators for retail development subsidies in the ICMA surveys, the presence of a mall, and the presence of a Business Improvement District in the town. Each coefficient is from a separate regressions, all regressions control for area, and the geographic measures are described in the text. Note that a higher Schwartzberg ratio means a town is less compact. Standard errors are clustered by state.

Table A.14. Town Shape and Malls Robustness Tests

Dependent Variable	Mall Dummy		
Compactness	(1)	(2)	(3)
Panel A			
OLS	-0.0472***	-0.0182***	-0.0444***
	(0.00568)	(0.00406)	(0.00522)
N	28940	27663	28940
Panel B			
State Annex. Law IV	0.0634	-	-
	(0.0559)		
First Stage F-Statistic	12.6		
Rivers IV	-0.206***	-0.0764***	-0.187***
	(0.0241)	(0.0154)	(0.0186)
First Stage F-Statistic	50.1	156.0	154.8
Wetlands IV	-0.226***	-0.0911***	-0.169***
	(0.0402)	(0.0297)	(0.0234)
First Stage F-Statistic	35.5	22.2	159.5
Incorp. Date IV	-0.329***	-0.252***	-0.346***
	(0.0787)	(0.0797)	(0.0653)
First Stage F-Statistic	16.1	11.3	45.7
Additional Controls	None	Pop & State Dummies, Log Med. House Prices, Share in Poverty, Share Non-English	Combined Statistical Area Fixed Effects

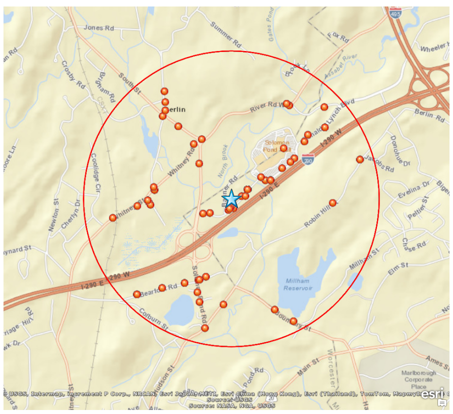
Notes: The table reports regressions of the form Mall Dummy$_i$=a+b x Compactness + cX$_i$ +u$_i$ where Mall Dummy i is an indicator for the presence of a mall or shopping center in the Esri data. Each coefficient represents a separate regression. The compactness measure is described in the text. All regressions include controls for area. Column 1 regressions include no additional control variables. Column 2 reports the coefficients controlling for population bin dummies, state dummies, log median house price, and the percentages of the population that does not speak English and is in poverty. Column 3 reports the coefficients controlling for metro (CSA) fixed effects. Details about the instruments are included in the text. Note that the state-level IV strategy cannot be used in column 2. Standard errors clustered by state in parentheses.

Table A.15. Tax Increment Financing and New Shopping Malls

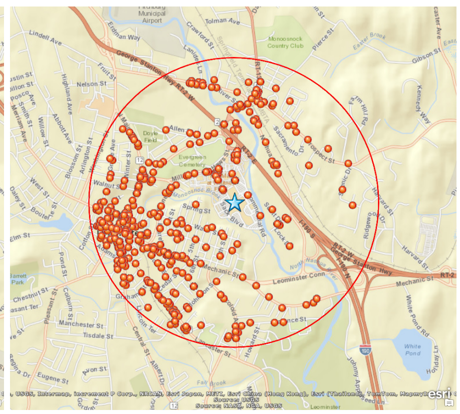
Defunct - Non Defunct Pair	New Shopping Centers & Malls per 100,000 people	New Shopping Centers & Malls per 100,000 people
	(1)	(2)
Post TIF Enabling Legislation Dummy	-0.011**	-0.012**
	(0.005)	(0.005)
Additional Controls	X	Census Divison Trends
N	2,400	2,400

Notes: This table reports the results of regressions of the form New Malls Per Capita = a_i + a_t + b x Post TIF Enabling Legislation + u_{it} where a_i are fixed effects for the state and at are year fixed effects. HI and AK were dropped due to missing data. The observations are state years from 1961-2010. Each coefficient represents a separate regression. The date of the TIF enabling legislation comes from the Council of Development Finance Agencies website. The standard errors are clustered by state.

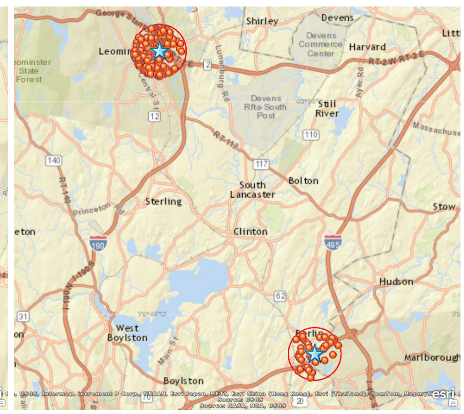
Figure A.1. Empirical Strategy: Visualization



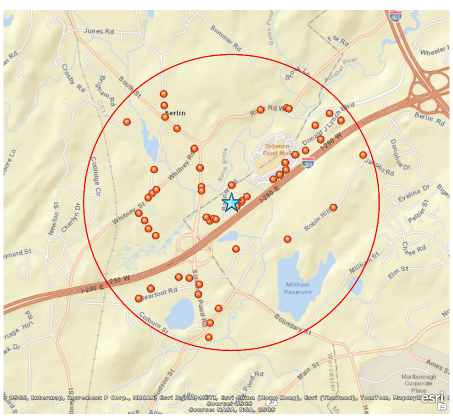
Best Buy in Marlborough, MA in 2006



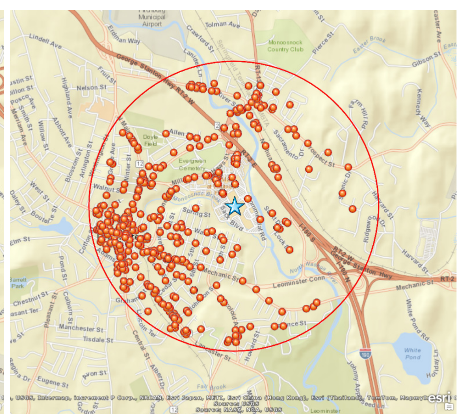
Circuit City in Leominster, MA in 2006



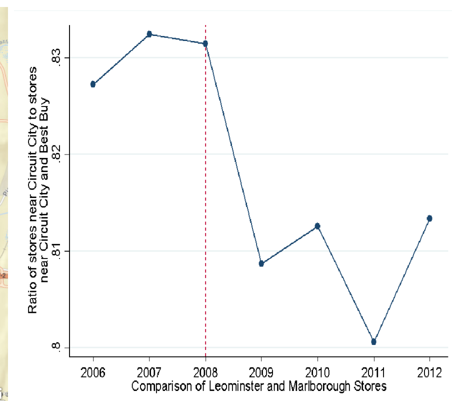
Circuit City in Leominster, MA and Best Buy in Marlborough, MA



Best Buy in Marlborough, MA in 2012



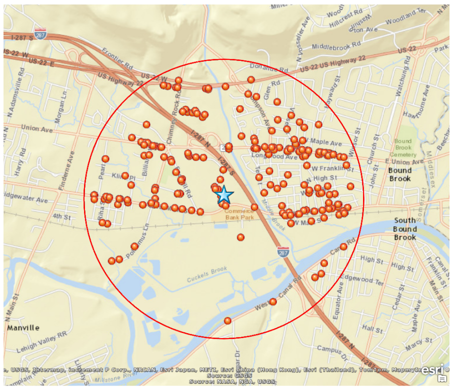
Circuit City Location in Leominster, MA in 2012



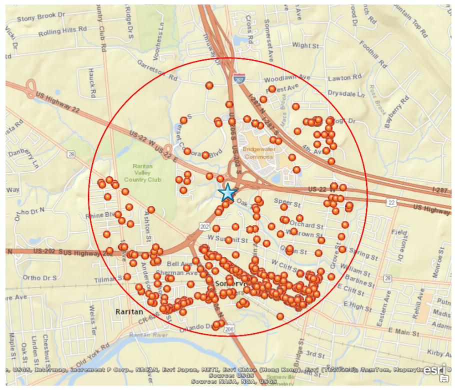
2008 Circuit City Bankruptcy Impact

Notes: The Blue star represents a given store or its location (if it closed). The red circle is the one mile radius around a given store. Other stores are represented by the red dots. The final figure plots the ratio of stores near this eventually defunct Circuit City relative to this non-defunct Best Buy over time.

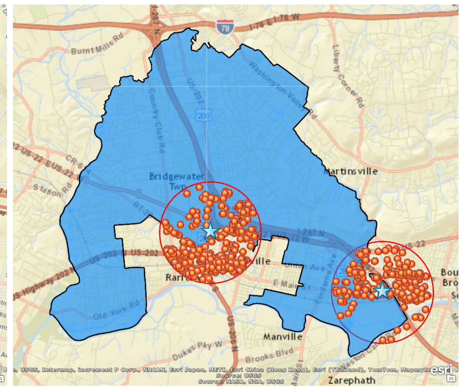
Figure A.2. Zip Code Fixed Effects Empirical Strategy: Visualization



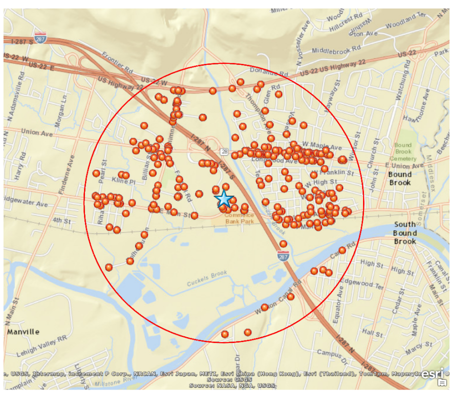
Linens ’n Things in zip code 08807 in 2006



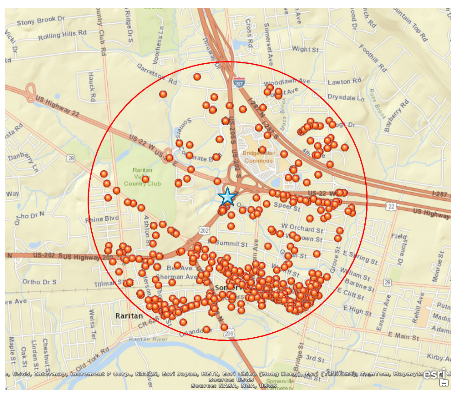
Bed, Bath & Beyond in zip code 08807 in 2006



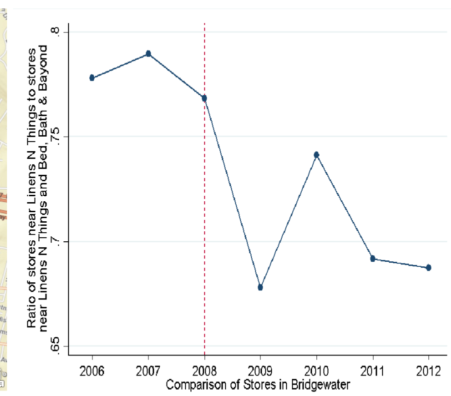
Linens ’n Things and Bed, Bath & Beyond in zip code 08807



Linens ’n Things Location in zip code 08807 in 2012



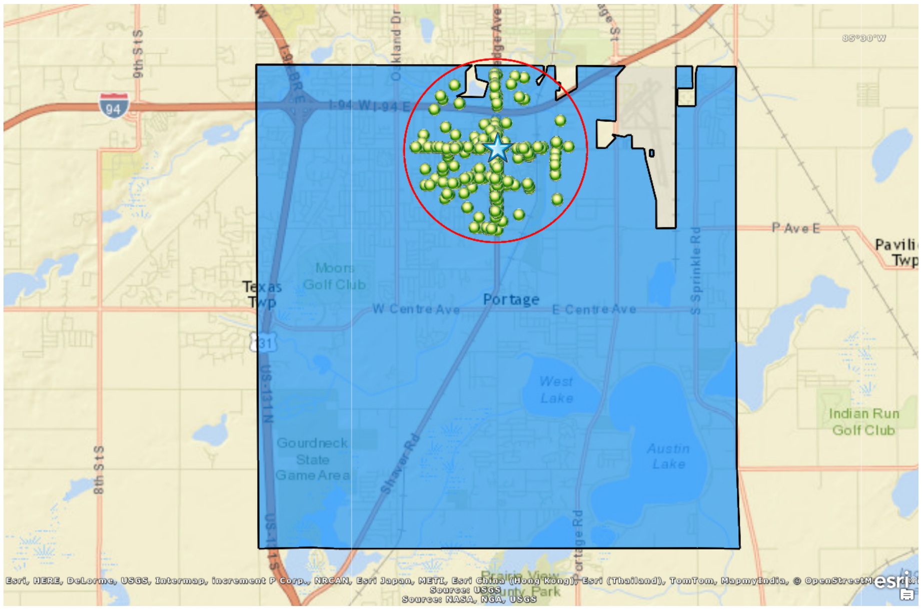
Bed, Bath & Beyond in zip code 08807 in 2012



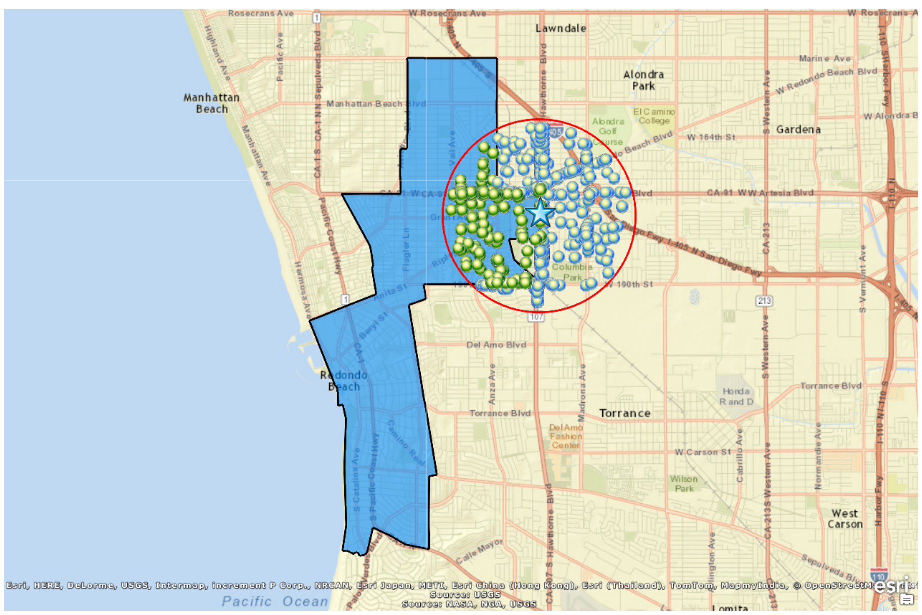
2008 Linens ’n Things Bankruptcy Impact

Notes: The Blue star represents a given store or its location (if it closed). The red circle is the one mile radius around a given store. Other stores are represented by the red dots. The zip code area is highlighted in blue. Both the defunct and non-defunct store are located in the same zip code. The final figure plots the ratio of stores near this eventually defunct Linens ’n Things relative to the non-defunct Bed, Bath, and Beyond over time.

Figure A.3. Examples of Compactness and City Shape



Circuit City in Portage, MA



CompUSA in Redondo Beach, CA

Notes: The city boundaries are highlighted in blue. The green dots represent stores that are in the city. The blue dots represent stores that are outside city boundaries, but within the one mile radius of a big-box store, which is represented by a blue star. Redondo Beach's compactness index value is -0.475, while Portage's is 2.299.

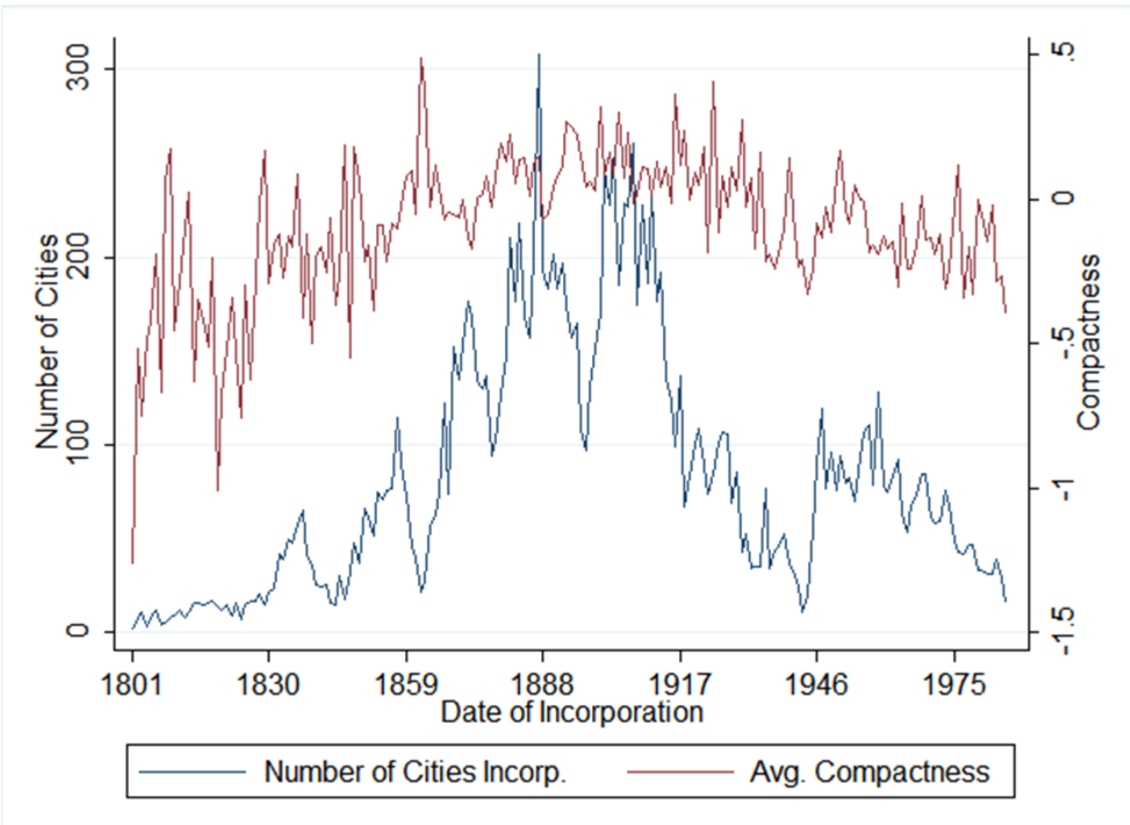
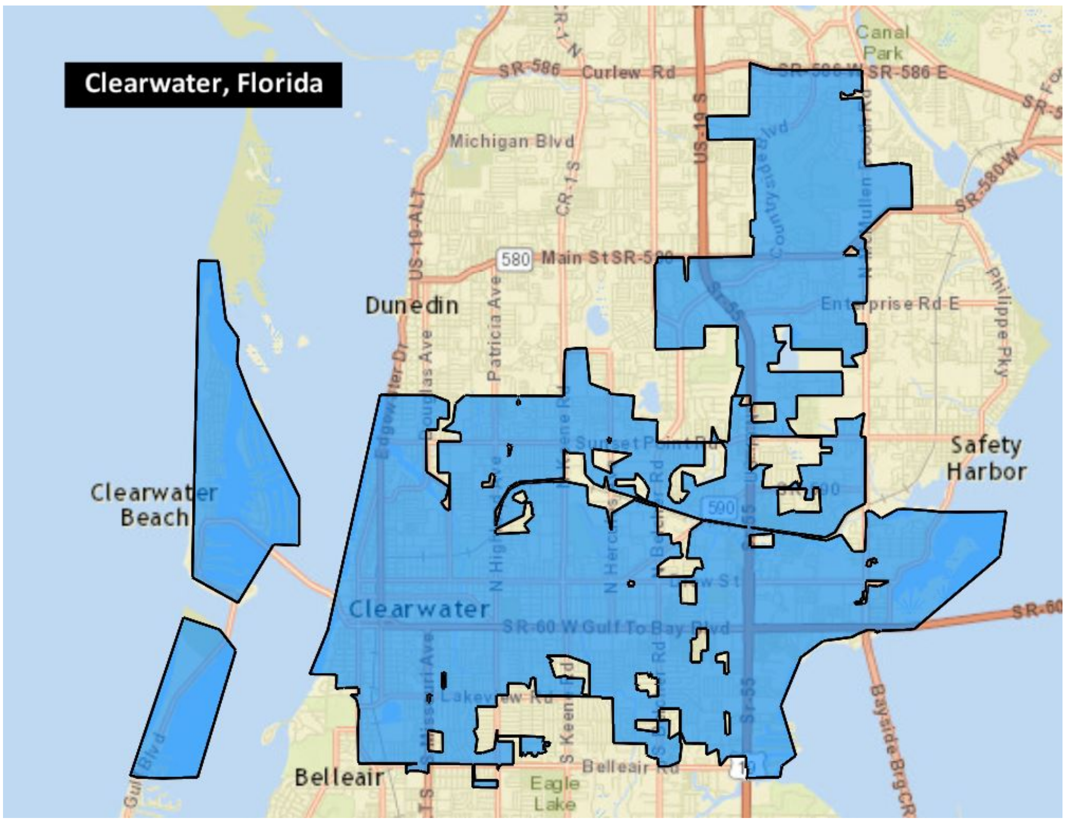


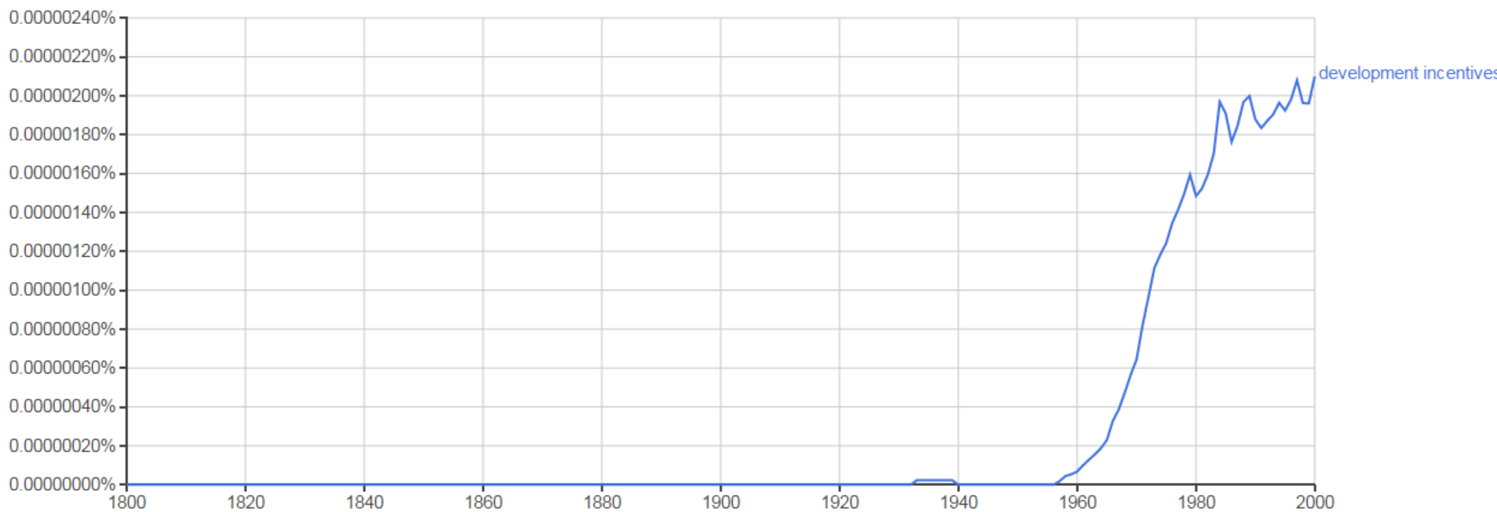
Figure A.4. Data are for cities existing in 2010. Information on incorporation from the 1987 U.S. Census of Governments.

Figure A.5. Compactness Example



Note: This figure maps the boundary of the town of Clearwater, Florida.

Figure A.6. Development Incentives Popularity



Note: This figure plots the yearly occurrence of the bigram ”development incentives” as a percentage of total bigrams in Google Books.

DATA APPENDIX

A. Data Sources

1987 Census of Governments. To identify the decade in which each city was incorporated, we used the 1987 Census of Governments as a source.

2002 Census of Governments. To identify local government expenditure per capita and local government revenue per capita, we used the 2002 Census of Governments.

An Assessment of Municipal Annexation in Georgia and the United States: A Search for Policy Guidance, 2002. We use this paper by Steinbauer, Paula E., Betty J. Hudson, Harry W. Hayes, and Rex L. Facer II to create an index of the stringency with which states allow annexations by local governments.

Cartographic Boundary Shapefiles – Places, 2006 & 2012. In order to provide a visual representation of the relationship between city compactness and Big-box externalities, we use the Cartographic Boundary Shapefiles for the years 2006 and 2012 to draw simple city boundaries.

Cartographic Boundary Shapefiles - ZIP Code Tabulation Areas, 2013. In order to demonstrate our zip-code fixed effects identification strategy, we draw simple zip code boundaries using the 2013 Cartographic Boundary Shapefiles for zip-codes.

Census, 2000. To compare the similarity of our treatment and control Big-box chain locations, we use the 2000 Census for socio-economic data that includes: median house sale price, percent black, income per capita, population and population density (pop/mi^2). Furthermore, information from the Census is also used to test the validity of our instrument. To do that we again use income per capita, median house price, population density (pop/mi^2) as well as percent non-English speakers, percent who have graduated college and the unemployment rate.

Council of Development Finance Agencies, 1961 - 2010. To identify the date each state enabled Tax Increment Financing we use information provided on the Council of Development Finance Agencies website. From data this we create the Post TIF Enabling Legislation Dummy.

County Business Patterns, 2006 – 2012. To test the robustness of our results to different datasets, we obtain zip code level establishment counts from Census County Business Patterns and aggregate them to the census place level.

Current Population Survey, Computer and Internet Use, 2007, 2009 – 2012. To test for the relationship between internet penetration and retail bankruptcies, we use data from CPS Computer and Internet Use supplement to create the variables: Stores in States with Internet Penetration > Median (0.5-mile radius, log) and Stores in States with Internet Penetration < Median (0.5-mile radius, log).

Esri Business Analyst Data, 2006 - 2012. To measure establishment and employment counts within a certain radius of a Big-box retailer we use the geocoded information provided by Esri Business Analyst Dataset. The precise geographic information available allows us to calculate store and employment counts at the ¼ mile, ½ mile, one mile, two mile and three mile radii. We also use the provided city information to calculate these two counts for Incorporated Places.

Esri Major Shopping Centers. To identify the presence of malls and shopping centers within an incorporated place, we use data on over 7,000 shopping centers and malls provided by Esri and match more than 6,000 of them to places for which we have data. From this source we include the following variables: an indicator for the presence of a shopping center or mall, the number of shopping centers or malls (log) and the number of establishments within a shopping center or mall.

Good Jobs First Subsidy Tracker 2.0, 1976 - 2014. To measure the presence of retail and other subsidies provided by municipalities we create dummy variables that indicate whether there was at least one subsidized retail development and at least one subsidized non-retail development.

International City/County Management Association (ICMA) Economic Development Surveys, 1999, 2004 and 2009. To measure development strategies of cities, we use the ICMA Economic Development surveys for the years 1999, 2004 and 2009. We include the following variables: total number of development strategies, total spend on development strategies, indicators for the presence of a sales tax, presence of a large retail base and seven dummies for development focus on either retail, agriculture, non-profit/institutional, tourism, transportation, technology or other categories.

International Council of Shopping Centers (ICSC) Shopping Center Operations, Revenue and Expenses, 2004. To demonstrate the differences in rent paid by store type in a shopping center or mall we use ICSC data on Shopping Center Operations for 2004.

Kosmont-Rose Institute Cost of Doing Business Survey, 2013. Data on the presence or absence of a Business Improvement District within a given city is from the Kosmont-Rose Institute Cost of Doing Business Survey, 2013. We create a dummy BID variable using this source.

LEHD Origin-Destination Employment Statistics (LODES), 2006-2011. The robustness of our employment results to different datasets is checked for by replicating select parts using LODES data for 2006-2011.

Multi-Resolution Land Characteristics Consortium's National Land Cover Database, 2011. To instrument city compactness, we use geographic features that affected city shape, but are no longer economically relevant. From the Multi-Resolution Land Characteristics Consortium's National Land Cover Database we use dummies for the presence of a river or wetlands within the city borders as instruments.

National Census Tracts Gazetteer File. To calculate the distance between 2010 census tracts and big-box retailers, we use the geographic coordinates for census tracts provided in the National Census Tracts Gazetteer file.

National Consumer Panel, 2004 - 2009. To identify consumer responses to Big-box Bankruptcies, we obtain data on shopping behavior for a representative consumer sample from AC Nielson. We create the following variables: department ratio, electronics ratio and furnishings ratio.

B. Notes on Variables

Establishment Variables

Ln Competitor Stores (0.5-mile radius). The logarithm of the number of competitor stores that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. Establishments are classified as competitors if they share the same four-digit NAICS code as our Big-box retailers.

Ln Entrant Stores (0.5-mile radius). The logarithm of the number of new entrants that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. An establishment is classified as an entrant in the first year of its existence.

Ln Foot Traffic Stores (0.5-mile radius). The logarithm of the number of foot traffic stores that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business

Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. Foot traffic classification is based on six-digit NAICS codes and a full list of industries classified as such is available in Appendix Table 5.

Ln Incumbent Stores (0.5-mile radius). The logarithm of the number of incumbents that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. An establishment is classified as an incumbent for all the years it is open other than the first.

Ln Non-Competitor Stores (0.5-mile radius). The logarithm of the number of non-competitor stores that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. Establishments are classified as non-competitors if they have a different four-digit NAICS code than our Big-box retailers.

Ln Non-Foot Traffic Stores (0.5-mile radius). The logarithm of the number of non-foot traffic stores that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. Non-foot traffic classification is based on six-digit NAICS codes and a full list of industries classified as such is available in Appendix Table 5.

Ln Stores (0.25-mile radius). The logarithm of the number of stores that are within a quarter mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Stores (0.5-mile radius). The logarithm of the number of stores that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Stores (1-mile radius). The logarithm of the number of stores that are within a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Stores (2-mile radius). The logarithm of the number of stores that are within a two mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Stores (city). The logarithm of the number of stores that are within the same city as a Big-box retailer for the years 2006 to 2012.

Ln Stores in States with Internet Penetration > Median (0.5-mile radius). The logarithm of the number of stores that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided by the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. The sample is limited to establishments in states with internet penetration rates greater than the median. Internet penetration is calculated as the average percent of individuals who live in households with internet access for the years 2007, 2009 – 2012. The data on Internet use is from supplements to the Current Population Survey (not available for 2008).

Ln Stores in States with Internet Penetration > Median (0.5-mile radius). The logarithm of the number of stores that are within half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided by the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. The sample is limited to establishments in states with internet penetration rates less than the median. Internet penetration is calculated as the average percent of individuals who live in households with internet access for the years 2007, 2009 – 2012. The data on internet use is from the Current Population Survey.

Δ *ln Stores (1 Mile Radius).* This is the rate of change for the number of stores that are within a one mile radius of a Big-box store between 2006 and 2007. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Stores (1 Mile Radius).* This is the rate of change for the number of stores that are within a one mile radius of a Big-box store between 2003 and 2007. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Stores (2 Mile Radius).* This is the rate of change for the number of stores that are within a two mile radius of a Big-box store between 2006 and 2007. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Stores (2 Mile Radius).* This is the rate of change for the number of stores that are within a two mile radius of a Big-box store between 2003 and 2007. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Stores (3 Mile Radius).* This is the rate of change for the number of stores that are within a three mile radius of a Big-box store between 2006 and 2007. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Stores (3 Mile Radius).* This is the rate of change for the number of stores that are within a three mile radius of a Big-box store between 2003 and 2007. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Number Stores, 2006 (0.5-mile radius). The logarithm of the number of stores that are within half a mile radius of a Big-box store for the year 2006. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Number Stores, 2006 (1 Mile Radius). The logarithm of the number of stores that are within a one mile radius of a Big-box store for the year 2006. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Number Stores, 2006 (2 Mile Radius). The logarithm of the number of stores that are within two mile radius of a Big-box store for the year 2006. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Number Stores, 2003 (0.5-mile radius). The logarithm of the number of stores that are within half a mile radius of a Big-box store for the year 2003. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Number Stores, 2003 (1 Mile Radius). The logarithm of the number of stores that are within one mile radius of a Big-box store for the year 2003. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Number Stores, 2003 (2 Mile Radius). The logarithm of the number of stores that are within two mile radius of a Big-box store for the year 2003. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Sales ($1,000,000). The estimated number of sales per each establishment.

<u>Employment Variables</u>

Employment, 2006 (0.5-mile radius). The number of workers that are within a half a mile radius of a Big-box store for the year 2006. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Employment, 2006 (1 Mile Radius). The number of workers that are within a one mile radius of a Big-box store for the year 2006. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Employment, 2006 (2 Mile Radius). The number of workers that are within a two mile radius of a Big-box store for the year 2006. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Employment, 2003 (0.5-mile radius). The number of workers that are within a half a mile radius of a Big-box store for the year 2003. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Employment, 2003 (1 Mile Radius). The number of workers that are within a one mile radius of a Big-box store for the year 2003. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Employment, 2003 (2 Mile Radius). The number of workers that are within a two mile radius of a Big-box store for the year 2003. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Employment (0.25-mile radius). The logarithm of the number of workers that are within a quarter mile radius of a Big-box store for the year 2006 to 2012. To calculate this number we use the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Employment (0.5-mile radius). The logarithm of the number of workers that are within a half a mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Employment (1-mile radius). The logarithm of the number of workers that are within one mile radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Employment (2-mile radius). The logarithm of the number of workers that are within two miles radius of a Big-box store for the years 2006 to 2012. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Ln Employment (city, Esri Business Analyst). The logarithm of the number of employees that are within the same city as a Big-box retailer for the years 2006 to 2012. Employment counts are based on Esri Business Analyst data.

Ln Employment (city, LODES). The logarithm of the number of employees that work in the same city as a Big-box retailer for the years 2006 to 2012. Employment counts are based LODES 2010 census tract work-area characteristics data and are aggregated up to city. If there are multiple cities in a census tract employment numbers are split evenly among them.

Ln Employment (census tract). The logarithim of the number of employees that work within the same 2010 census tract as a Big-box retailer for years 2006 to 2012. The Center for Geographic Analysis at Harvard matched establishments to tracts by geocoding addresses.

Ln Employment (census tract, LODES). The logarithim of the number of employees that work within the same 2010 census tract as a Big-box retailer for years 2006 to 2012. The employment counts are based on LODES work-area-characteristics data.

Ln Employment (same city). The logarithm of the number of employees that work within the same city as a Big-box retailer and live in the same city for the years 2006 to 2012. The name of the city is provided in the Esri Business Analyst data. However, the employment numbers are from LODES 2010 census tract origin-destination data and are aggregated up to city. If there are multiple cities in a census tract employment numbers are split evenly among them. The origin-destination files allow us to know the number of employees who live in one census tract (the origin) and work in another (the destination).

Ln Employment (same city census tract). The logarithim of the number of employees that work within the same 2010 census tract as a Big-box retailer and live in the same city as the retailer for years 2006 to 2012. The employment counts are based on LODES origin-destination data. The origin-destination files allow us to know the number of employees who live in one census tract (the origin) and work in another (the destination). We use this information to calculate city numbers only for employees who live in the same city as the Big-box retailer.

Δ *ln Employment (1 Mile Radius).* This is the rate of change for the number of employees that are within a one mile radius of a Big-box store between 2006 and 2007. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. Employment in stores that are within one mile is then summed.

Δ *ln Employment (1 Mile Radius).* This is the rate of change for the number of employees that are within a one mile radius of a Big-box store between 2003 and 2007. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula. Employment in stores that are within one mile is then summed.

Δ *ln Employment (2 Mile Radius).* This is the rate of change for the number of employees that are within a two mile radius of a Big-box store between 2006 and 2007. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Employment (2 Mile Radius).* This is the rate of change for the number of employees that are within a two mile radius of a Big-box store between 2003 and 2007. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Employment (3 Mile Radius).* This is the rate of change for the number of employees that are within a three mile radius of a Big-box store between 2006 and 2007. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

Δ *ln Employment (3 Mile Radius).* This is the rate of change for the number of employees that are within a three mile radius of a Big-box store between 2003 and 2007. To calculate this number we use the employment count and the latitude and longitude coordinates of each establishment provided in the Esri Business Analyst dataset. Then the distance from a Big-box retailer is calculated using the great circle formula.

<u>Bankruptcy Variables</u>

Defunct. A time-varying dummy equal to 1 if the big-box chain is closed in that year.

Defunct within 0.25 mi. (dummy). A time-varying dummy equal to one if a 2010 census tract is less than 0.25 miles away from a defunct big-box retailer. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

Defunct within 0.5 mi. (dummy). A time-varying dummy equal to one if a 2010 census tract is between 0.25 miles and 0.5 miles of a defunct big-box retailer. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

Defunct within 1.5 mi. (dummy). A time-varying dummy equal to one if a 2010 census tract is between 0.5 miles and 1.5 miles of a defunct big-box retailer. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

Defunct within 2.5 mi. (dummy). A time-varying dummy equal to one if a 2010 census tract is between 1.5 miles and 2.5 miles of a defunct big-box retailer. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

Defunct within 0.25 mi. The number of defunct big-box retailers that are less than 0.25 miles from a 2010 census tract. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

Defunct within 0.5 mi. The number of defunct big-box retailers that are between 0.25 miles and 0.5 miles from a 2010 census tract. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

Defunct within 1.5 mi. The number of defunct big-box retailers that are between 0.5 miles and 1.5 miles from a 2010 census tract. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

Defunct within 2.5 mi. The number of defunct big-box retailers that are between 1.5 miles and 2.5 miles from a 2010 census tract. We use the latitude and longitude coordinates provided by the Census bureau for each census tract to calculate this distance.

No. Big-box Stores. The total number of Big-Box stores within the city. ERSI has city identifiers.

No. Defunct Big-box Stores. The number of Big-box stores in a city from retail chains that did file for Bankruptcy.

<u>Compactness Variables</u>

Convex Ratio. This ratio is the area of the town divided by the area of the town's convex hull.

Polsby-Popper ratio. This ratio is the town area multiplied by 4π and then divided by the squared value of the town's perimeter.

Ratio Index. The different ratios are aggregated using factor analysis. The index is the first principal component of the different measures.

Rectangular Area Fraction ratio. This is the town area divided by the area of the smallest rectangle that would enclose the whole town.

Roeck ratio. This is calculated by dividing the town area by $\times \frac{1}{2} \times \max(width, length)^2$. Here *width* is the width and *length* is the length of the smallest rectangle that would enclose the entire town.

Schwartzberg ratio. This ratio is the town perimeter divided by $2 \times \pi \sqrt{\frac{town\ area}{\pi}}$.

<u>City Development Variables</u>

Agriculture Focus. A dummy variable indicating if the city primarily focuses on agriculture development. It is equal to one if the respondent selected agriculture to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to development focus.

Economic Development Budget. The respondent answer to the question "How much did your local government budget for economic development activities for FY [1999/2004/2009]?"

Large Retail Base. Indicates if a city has a large retail base. It is equal to one if the respondent selected retail to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to economic base.

Local Government Expenditure Per Capita. The city expenditures per capita for 2002.

Local Government Revenue Per Capita. The city revenues per capita for 2002.

Non-profit/Institution Focus. A dummy variable indicating if the city primarily focuses on non-profit/institutional development such as government contracts. It is equal to one if the respondent selected non-profit/institution to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to development focus.

Non-retail Program. A dummy equal to one if the city subsidizes at least one non-retail development.

Other Focus. A dummy indicating if city has a development focus that is not retail, agriculture, non-profits, tourism, transportation or technology. It is equal to one if the respondent selected other to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to development focus.

Post TIF Enabling Legislation Dummy. This is a dummy variable that is equal to one for each year in which Tax Increment financing is legal in a state.

Retail Focus. A dummy variable indicating if the city primarily focuses on retail development. It is equal to one if the respondent selected retail to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to development focus.

Retail Program. A dummy equal to one if the city subsidizes at least one retail development.

Sales Tax. Indicates if a city has a sales tax. It is equal to one if the responded selected sales tax to the question "Which of the following taxes does your local government levy and what is the rate?"

Technology Focus. A dummy variable indicating if the city primarily focuses on technology development. It is equal to one if the respondent selected technology to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to development focus.

Total Development Strategies. The number of development strategies that the local government is pursuing. It is the sum of positive responses to the question "Does your local government support any of the following programs to promote or support economic development?"

Tourism Focus. A dummy variable indicating if the city primarily focuses on tourism as its main plan of development. It is equal to one if the respondent selected tourism to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to development focus.

Transportation Focus. A dummy variable indicating if the city primarily focuses on transportation development. It is equal to one if the respondent selected transportation to the question "Which of the following best describes your local government's primary economic base (primary source of revenue) and focus of your economic development efforts? (Check only one in each column)" in regards to development focus.

<u>Shopping Mall and BID variables</u>
Business Improvement District. An indicator for the presence of a Business Improvement District in a town.

Center Stores (log). The logarithm of the number of town stores that are in a shopping center or a mall.

New Shopping Centers & Malls. The number of new shopping centers and malls per 100'000 state residents for the years 1961-2010. These data come from the Esri Shopping Center dataset, which records mall openings.

Shopping Center Indicator. An indicator for the presence of a shopping center or mall in a town.
Shopping Centers (log). This is the logarithm of the number of shopping centers or malls in a town. If the city does not have a shopping center it is then dropped from the sample.

<u>Instruments</u>

Incorporation date. The average compactness of all cities incorporated in the same decade as the city of interest, excluding the city itself. This is field 12 in Attachment 4 in the 1987 Census of Governments.

Rivers. An indicator variable. It takes on the value of one if a city has a river within its borders and zero otherwise.

State annexation index (0-4). An index indicating the stringency of legislation that states have adopted in regards to annexation by local governments, based on Steinbauer, Hudson, Hayes, and Facer (2002). We code a dummy variable for whether a state requires an impact plan, a service plan, judicial review, and a public petition in order for cities to annex land. Our index is the sum of these dummy variables.

Wetlands. An indicator variable. It takes on the value of one if a city has a wetlands area within its borders and zero otherwise.

Demographic Variables

All data are taken from the 2000 Census.

Income per capita. The income per capita for a city in the year 2000.

Income per capita (log). The logarithm of municipal income per capita for the year 2000.

Median House Sale Price. The median price of all the houses sold in a zip code in 2000.

Median House Sale Price (log). The logarithm of the median sales price of a house in a municipality in the year 2000.

Percent Black. The percentage of a zip code's population that was African-American in 2000.

Percent College Grad. The percentage of individuals in a municipality who have a bachelor's degree or higher in 2000.

Population Density. The population density for the municipality in the year 2000.

Population Density (log). The logarithm of the population density for the municipality in 2000.

Population Dummies. The city population in 2000 split into ten evenly sized bins.

Share in Poverty. The percentage of individuals in a municipality that live below the poverty line in 2000.

Share non-English. The percentage of the municipality's population that do not speak English 2000.

Unemployment Rate. The percentage of the municipality's population that was unemployed in 2000.

Consumer Variables

Department Ratio. This is the ratio of visits at stores visited on the same day as a Department store to total visits at all stores for each month from 2004 to 2009.

Electronics Ratio. This is the ratio of visits at stores visited on the same day as an Electronics store to total visits at all stores for each month from 2004 to 2009.

Furnishings Ratio. This is the ratio of visits at stores visited on the same day as a Furnishings store to total visits at all stores for each month from 2004 to 2009.

C. A Note on Esri Business Analyst Data

In our main specifications studying establishment data we use data from Esri Business Analyst. As we discuss in the main text, although the Esri data are not produced by the federal statistical system, we believe that this is the correct choice for a variety of reasons:

“First and foremost, Esri includes address data, which allows us to work at a high-precision geographical level. It includes establishments not counted in the Census' County Business Patterns dataset (unincorporated or no employees) - someone teaching violin lessons might show up, for example. This broader measure of economic activity is appropriate for our purposes (and for many other purposes as well, which is why there is a market for these datasets), although there may be systematic scale differences. That said, we always include fixed effects, and our estimates are in percentage terms. In addition, our results stand even if one worries about the quality of the data.

We use establishment counts as a dependent variable, and as long as the noise in the Esri data is not correlated with a chain eventually becoming defunct, a type of correlation that strikes us as wildly implausible, the noise merely inflates our standard errors. That said, we do not believe that even measurement error is particularly severe. Establishment counts at the county level are strongly correlated with County Business Patterns data from the Census Bureau in both levels and in log changes over time, as depicted in our Data Appendix. Additionally, where we can, we replicate our results using LODES data, which is derived from the Census Bureau's LEHD and uses a completely different methodology. These replication exercises strengthen our confidence in the robustness of our findings to the point of practical certainty.”