Discounting Politics: The Impact of Large Retailers on American Communities

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Abstract

As Wal-Mart, Home Depot, and other large-scale retail chains have spread across the country, many critics have denounced this "commercial sprawl" for nothing less than destroying American communities. Three criticisms are especially salient: that large retailers harm local social networks, that they undermine residents' attachment to their communities, and that they privatize public space. But despite the popular resonance of these charges, they have not been systematically tested using large-sample survey data. By combining the 2000 Social Capital Community Benchmark Survey (n=29,233) with ZIP code-based U.S. Census data, this paper is able to measure local retail environments and estimate their impact with unprecedented precision. While two of the criticisms fall flat, large firms do have a marked effect on a community's engagement in politics. All else equal, the presence of a large retailer in a respondent's ZIP code will diminish her likelihood of participating in several forms of politics by 1 to 6 percentage points. These results hold up using both parametric and non-parametric approaches, a strong sign of their robustness.

"Twenty years ago, our village of 1,200 souls had a bakery, a flower shop, a small supermarket with an excellent butcher, a hardware store and a pharmacy. Ever since Wal-Mart and other chains expanded in Columbia County, we have lost all of those stores and services. Prices may be low at Wal-Mart, but life in Kinderhook is not richer as a consequence."

–SAMUEL O. J. SPIVY, Kinderhook, New York
Letter to the Editor, *The New York Times*, December 7, 2003

Introduction

Wal-Mart now has approximately 3,500 stores across the U.S., and plans to add another 335 in 2004 (Bianco and Zellner 2003). The bulk of that expansion will come from 210 new superstores (Bianco and Zellner 2003), enormous warehouse-style buildings that average 187,000 square feet and sell everything from furniture to groceries (Associated Press 2003). With its unprecedented scale, the chain has transformed the commercial landscape across the country (Lohr 2003), and now draws at least one annual purchase from 82% of American households (Bianco and Zellner 2003). What's more, Wal-Mart is just the most visible outgrowth of a trend towards retail consolidation that has brought megastores like Home Depot, Target, and Costco to communities around the country.

But in recent years, the criticism of Wal-Mart has grown as big as the chain itself, with everyone from *Time* to *Business Week* asking a variant of the question: "is Wal-Mart good for America?"(Lohr 2003). By all accounts, Wal-Mart and other large retailers have transformed the communities around their stores, although whether for good or ill has been the subject of fierce debate (see Lohr 2003; Ortega 1998; Quinn 1998; and Scott 1994). In the eyes of their detractors, the arrival of large, discount retailers in a community leads to the "deterioration of historic commercial centers" and the "homogenization of

rural landscapes" (Norman 1999). A single Wal-Mart can force many smaller businesses to close up shop: as just one example, 67% of Iowa's mens' clothing stores closed in the two decades after Wal-Mart came to the state in 1983 (Alexander 2002). Ehrenhalt (1995) shows how community life can suffer as a result of this commercial displacement. When smaller, neighborhood-based retailers go out of business, the dense web of relationships that they hosted goes with them.

The rhetoric of the debate suggests just how high the stakes are, at least according to the participants. Writing about the broader trend of sprawl, for instance, Kunstler argues that it has produced "a landscape of scary places, the geography of nowhere, that has simply ceased to be a credible human habitat"(1993:15). Yet despite the almost apocalyptic terms in which sprawl is discussed, few social scientists have tested the variety of claims made by its critics. We have a rich tradition of interpretive studies and careful histories illustrating how the local retail environment can induce major changes in community life, but many of its most provocative hypotheses remain untested in large samples. This paper aims to help fill that void.

By coupling the Social Capital Community Benchmark Survey (SCCBS) with ZIP code-based data on retail patterns, this paper provides a more in-depth exploration of the impact of commercial sprawl than has previously been possible. It also engages with the related methodological questions, including selection bias and multi-level modeling. Given the passion that surrounds the debate, the findings are likely to surprise many. The presence of large retailers has only tiny impacts on both local social networks and levels of community attachment. But large retailers do transform the political character of a community, making individuals 1 to 6 percentage points less likely to join political groups or participate in marches and demonstrations. Whether one uses basic parametric

models or propensity score matching, these central findings prove robust.

The Social Science of Sprawl

When people use the term "sprawl," they are referring to a bundle of related but distinct phenomena, as Williamson (2002) rightly points out. Here, I focus just on commercial sprawl, defined as the tendency to establish large retail stores at or near major roadways and away from communities' historic downtowns. This includes two separate trends, both of which have emerged since the 1960s. The first, highlighted above, is towards large, stand-alone retail establishments like Wal-Mart and Home Depot. A second, equally critical, is towards the construction of large-scale shopping centers with multiple retailers. As with the megastores, these shopping centers tend to be located outside historic downtowns, and are oriented chiefly around car traffic (Farrell 2003). There are now some 45,115 shopping centers in the U.S., with average annual sales of $26 million each (Urban Land Institute 2002).

If the notion of "sprawl" is too broad, so too are the notions of "community" or "community life" that commercial sprawl is said to impact so negatively. Instead, this paper focuses on the interwoven concepts of social capital, community engagement, and political participation as more coherent dependent variables. Recently renovated by Putnam (2000; 1993), Coleman (1994; 1990) and others, the term "social capital" refers to "connections among individuals–social networks and the norms of reciprocity and trustworthiness that arise from them" (Putnam 2000:19). Community engagement, in turn, captures an individual's psychological orientation towards her community. Following Verba, Schlozman, and Brady (1995), political participation is taken to be those voluntary actions that aim

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to influence government policy, and includes a range of activities from voting to attendance at marches. By focusing on all three types of dependent variables simultaneously, we can identify how the impact of large retailers varies across categories, and can gain added leverage in determining what causal mechansism are at work.

To date, the most detailed study of the effects of commercial sprawl and retail consolidation comes from Humphries (2001). Even so, Humphries' study has limitations which make further research critical. Perhaps most importantly, because its empirical data is drawn from the 1996 National Election Study, it can only quantify the effect of economic scale on political participation, leaving us to wonder about its impact on less political networks and activities. At the same time, Humphries' key causal variable is "economic scale," a broad rubric that encompasses everything from commuting to shopping to small business ownership. This paper focuses more narrowly on the local retail environment. The goal: to provide a thorough look at the mechanisms that relate community life to specific configurations of local retailers.

1 Theory: Of Malls and Mechanisms

To some authors on the topic, the notion that sprawling malls and giant retail stores undermine local communities is so intuitively compelling that it needs little probing or development (i.e. Norman 1999; Ortega 1998; and Kunstler 1993). Others who approach questions of sprawl do so from more interpretive traditions like urban planning and history; although these authors provide a textured analysis of the interplay between space and social capital, they sometimes fail to distinguish among their interlocked hypotheses (i.e.

Jackson 1985; Jacobs 1961). The result, as Humphries (2001) and Williamson (2002) suggest, is a lack of conceptual clarity. Past works give us strong reason to suspect that the size of local firms has an effect on community life, but the specific mechanisms remain unclear. Drawing on the rich and varied work in these traditions, this section aims to identify the likely pathways through which the retail environment impacts social capital and community engagement. It sorts the relevant hypotheses into three groups, including those emphasizing anonymity and interpersonal contact; those emphasizing residents' attitudes towards their community; and those emphasizing the privatization of shared space.

To varying extents, these hypotheses rely on assumptions about the structure of social networks and the special importance of weak ties, assumptions I should state explicitly. As Granovetter (1973) shows, weak interpersonal ties can be critical in connecting individuals to others outside their clique, thus facilitating the transmission of information. Weak ties also enlarge the group of people that can be mobilized, making such ties valuable political resources for communities. What's more, weak ties are the stuff that neighborhoods are made of (Jacobs 1961): geographic proximity and shared space allow people to maintain functional relationships without needing to become intimate. If we can connect commercial sprawl to a community's social organization, we can connect it to that community's capacity for political mobilization and information transmission as well.

1.1   Severing Store-Based Ties

Moving on to the mechanisms themselves, we should first consider how retail size affects neighborhood-based social networks and interpersonal contact. Central to Ehrenhalt's

narrative about Chicago is the retail environment (1995). Since the 1950s, local banks and stores have been replaced by larger, more distant shopping centers. Shopping once meant participating in a series of weak but consistent relationships with bankers, butchers, barbers, and other community members: in Ehrenhalt's words, "butchers were treated like family members"(1995:255). But with the shifts away from local main streets and local ownership, these relationships disappeared. Wal-Mart may employ greeters at the entrance to many of its stores (Zukin 2004), but their purpose is to simulate familiarity, not to actually build it.

Local businesses also provided community members with a shared space that facilitated short, chance encounters with one another (Ehrenhalt 1995; Jacobs 1961). By contrast, large retail stores are anonymous. Since they typically draw customers from multiple communities, shopping in them involves less accidental socializing. This is compounded by transportation: whereas the location and concentration of old retail establishments made people likely to walk to them, driving is now a prerequisite to get to many stores (Farrell 2003), further reducing chance encounters with one's neighbors. Arguably, maintaining weak ties is a positive externality that stems from small-store shopping in one's own neighborhood. When shopping in larger, more distant stores, that externality vanishes. As large firms become more central to the retail environment, we might expect people to socialize with neighbors and community members a bit less, and also to know fewer local business owners.

1.2 Attachment to Generic Communities

A second type of hypothesis, noted by Humphries (2001), relates to the way that retail establishments affect an individual's attachment to her community. Communities whose retail environments are dominated by chain stores risk losing their individual character, as their more historical downtown is replaced by strip malls and larger retailers on the outskirts of town (Farrell 2003; Norman 1999). Notes Sorkin: "with its components reduced to a repetitive minimum, space is departicularlized"(1992). My own travels in the U.S. serve as anecdotal evidence on this point. Whether in Skagit County, Washington or Cuyahoga County, Ohio, I have noticed again and again how similar American towns are in terms of their layout and shopping prospects. Individuals might well feel less attached to a seemingly generic town, which in turn could lead them to downplay their involvement in its projects and social networks.

Or perhaps the problem is not so much weakened loyalties as divided loyalties. Explaining the gap between political participation in small suburbs and comparably sized but isolated rural towns, Verba and Nie (1972) emphasize the importance of a community's "boundedness."[1] Boundedness is a synonym for how well-defined a community is. In rural towns, the municipal boundaries are likely to define residents' economic and social horizons as well, creating a well-bounded community. The retail establishments reinforce the community's sense of boundedness by drawing residents into the same space. In the suburbs, on the other hand, residents belong to several distinct communities simultaneously, including the city where they work and the town where they sleep. When large retailers located outside the downtown area dominate, individuals have yet another spa-

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[1]Coleman (1990) makes a similar point, although he phrases it in terms of the community's "closure."

tially distinct community that lays claim to some part of their allegiance.[2] Residents'
willingness to engage in their residential community suffers as a result.

1.3 Privatizing Shared Space

Earlier in his career, 1950s mall architect Victor Gruen would have been perplexed to
read the hypotheses above, as his intention was exactly the opposite: to create a space
that could serve as a community center for a new age (Cohen 2003; Moe and Wilke 1997).
For many critics, though, the very nature and design of malls makes them unsuitable
as public spaces. They are regulated and commercial, designed not to build connections
but to sell products. Thus as customers move from public streets to private malls, these
critics contend that the content of their social interactions becomes constrained and de-
politicized.

Democracy, according to some authors, has spatial prerequisites. In Mattson's words,
"[a]s a democratic society, we need places where citizens can congregate and associate
with one another"(2002:45). Oldenburg (1989) helps gives us a concrete picture of these
spaces. In his view, they are comfortable and pluralistic; they encourage loitering, con-
versation, and the occasional disagreement. But malls and other large-scale retail spaces
are just the opposite. As Farrell (2003) points out, they are designed to move people
efficiently from store to store. Consider the food courts, which let customers grab a bite
to eat without having an incentive to linger away from the stores. What's more, speech in
retail environments is consciously regulated, with anything deemed "political" excluded.
To enter a mall, in Farrell's words, is to "trade political rights for commercial opportu-

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[2]Putnam (2000) makes this point as well.

nities"(2003:218). This privatization hypothesis merits special attention, as the point is made by many of the most prominent authors on the topic (Cohen 2003; Barber 2002; Davis 1992; Jackson 1985). Put simply, the hypothesis predicts that as large retailers and especially malls become more prominent in a community, the community's residents will miss out on recruitment opportunities and its political activity will decline.

1.4   Roads Not Taken

In short, planners, scholars, and author-advocates have developed a broad range of hypotheses about how the dual shifts in retailing–towards larger stores, and towards stores outside the center of towns–impact local residents' interconnections, both in the strength of those connections and in their content. Certainly, the list could have been far broader absent my focus on the mechanisms highlighted by social capital theory. For instance, the hypotheses above say nothing about these firms' sizable economic impact on a community, even though Wal-Mart alone creates one out of every 20 new U.S. jobs (American Demographics 2003). I left out some explicitly political mechanisms as well, ignoring the ways a large retailer can reconfigure local politics.[3] Questions of shopping and diversity are also omitted, although Barber (2002), Dreier et. al. (2001) and Davis (1992) give us good reason to suspect that large retail establishments are exclusionary places insofar as one must have a car to reach them. On the flip side, I ignore mechanisms that suggest how large retailers could build social capital, whether by allowing employees to network or through a corporate culture that builds skills and encourages customer service.[4] While

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[3]One aspect of this process, the pivotal role of local businessmen in local politics, is well handled by Humphries (2001).

[4]In doing so, I am following the lead of the existing literature, which is overwhelmingly skeptical of the claim that large retailers can build community.

all these components of the trend towards commercial sprawl are undoubtedly essential to a full assessment of it, they are beyond the scope of the present analysis. With a list of plausible mechanisms that is already quite extensive, I now consider how best to test them.

2   Data and Measurement: Spotting a Wal-Mart

When matched with ZIP Code Tabulation Area data from 2000 Census Summary File and the Census Bureau's 2000 ZIP Code Business Patterns data, the SCCBS becomes an unparalleled tool with which to explore the relationships postulated above. It provides researchers with a wealth of data on 29,233 individuals,[5] 26,230 of whom live in 40 identifiable areas around the country. The survey probed respondents on a wide range of community-related variables, including their trust, efficacy, alienation, politics, tolerance, and social activity. This section discusses how to apply to SCCBS to the problem of commercial sprawl. Readers interested in the specific coding and descriptive statistics should see Tables 1, 2, and 3.

2.1   Dependent Variables: Quantifying Community

Past work suggests three distinct mechanisms through which commercial sprawl might impact social capital. To test the first group of hypotheses–those related to the social side-effects of small business–I focus on the respondent's trust in the employees where

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[5]The sample of 500 residents from Oakland and surrounding areas in Northern California is omitted from this study.

Variable	Range	Mean	Coding
Social Networks			
Trust Employees	1 - 4	2.00	Lower is more trusting
Business Friend	0 - 1	0.65	1 is yes
Neighborhood Socializing	1 - 7	5.09	Higher is more social
General Trust	1 - 3	1.57	Higher is more trusting
Community Attachment			
Alienation	1 - 5	2.5	5 is alienated
Quality of Life	0 - 3	2.24	Lower is better
Sense of Community	0 - 1	.79	1 is yes
Political Activity			
Signed Petition	0 - 1	.39	1 is signed
Attended Public Mtg.	0 - 1	.35	1 is participated
Reform Group	0 - 1	.21	1 is participated
Pol. Group	0 - 1	.10	1 is participated
Registered to Vote	0 - 1	.83	1 is registered
Voted in 96	0 - 1	.74	1 is registered
Attended a March	0 - 1	.08	1 is participated

Table 1: Describing the dependent variables: the descriptive statistics above show the mean value and the range for the 29,233 respondents in the full dataset on each of the 14 focal dependent variables. Some dependent variables are binary; others have up to 7 values.

she shops, whether the respondent counts a business owner as a personal friend, and how often the respondent socializes with her neighbors. To give some sense of what these numbers mean, I also analyze more general levels of trust. If the authors above are right, individuals in areas dominated by large retailers should be less familiar with business people, less trusting of the people in the places where they do shop, and perhaps less social with their neighbors as well.

The second group of hypotheses focus on the respondent's attachment to her community. To gage this, I look at dependent variables including: whether the respondent is alienated from community leaders; how the respondent rates her community's quality of life; and whether the respondent reports that her city gives her a sense of community. While all measure community attachment, the last will be employed with caution, as it has 14,498 missing values.[6]

The third type of hypothesis is an argument about the content of social interactions, and it emphasizes the apolitical nature of large retail spaces. The best available measure of this is whether an individual has signed a petition in the last year, as petitioning is precisely the kind of activity that malls prohibit. Since the more powerful version of this hypothesis is a broader cultural argument about the side-effects of commercialization, I also analyze a host of other political activities, including attendance at public meetings, participation in local reform groups or political groups, voter registration, voter history, and participation in marches or demonstrations.

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[6]Throughout this analysis, I use listwise deletion as the missing data strategy, although in future drafts I plan to impute missing values to enhance the accuracy of the estimates as recommended by King et. al. 2001. Occasional demographic variables have large numbers of missing values, including the respondent's income (3,017 missing), race (630 missing), and age (650 missing).

2.2 Measuring Commercial Sprawl

What exactly counts as commercial sprawl? From the highway, it might look obvious: large retailers or malls with enormous parking lots, often near major traffic junctions. But we need to quantify it. Humphries (2001) uses the mean number of employees per retail firm in each ZIP code, a valid measure. Since the theoretical discussion above emphasized the very small and very large retail establishments, though, I focus instead on those businesses, and measure: 1) the percentage of retail firms in a ZIP code that employ fewer than 5 people; 2) the percentage of retail firms that employ fewer than 10 people; 3) the percentage of retail firms that employ more than 50 people; and 4) the percentage of retail firms that employ more than 250 people. Table 2 and Figure 1 help give meaning to these variables. While small retail activity occupies a relatively fixed percentage of an area's commercial life, the largest stores are clearly concentrated in medium-density locales.

Variable	Range	Mean	Standard Dev.
Megastore in ZCTA	0 - 1	.315	.46
% Firms < 5 Employees	0 - 1	.466	.15
% Firms <10 Employees	0 - 1	.682	.15
% Firms > 50 Employees	0 - 1	.052	.04
% Firms > 250 Employees	0 - .25	.003	.006
Retail Firms per Person	0 - 655	.004	.01
Retail Firms in ZCTA	0 - 932	119.7	149.46
People in ZCTA	0 - 114,300	26,010	17,564

Table 2: How to measure commercial sprawl? The descriptive statistics above illustrate the key measures employed here. All variables are at the level of the ZCTA, or ZIP Code Tabulation Area. Even given the seeming explosion of large retail firms, firms with over 250 employees represent just 0.3% of the total firms in the average ZIP code.

Additionally, I mark those towns with megastores–that is, single retail establishments em-

ploying over 250 people–with a dummy variable, providing an absolute measure alongside the relative measures. A critic might object that these variables will be more effective at identifying stand-alone retailers such as Wal-Mart than malls, as malls can be composed of separate, mid-sized retailers which might fall below the 50-person or 250-person threshold. But given that most malls have large anchor tenants (Farrell 2004), the measurement error on this score is likely to be slight, especially when employing the 50-employee threshold.

More profoundly, skeptical readers might object that the percentage of large retailers in a respondent's ZIP code is not an effective measure of her retail environment. Imagine a respondent who lives at the border of a ZIP code without any large retailers: if she often shops at the large Kmart just over the ZIP code boundary, she will be miscoded. Undoubtedly, using ZIP codes as a measure of one's community introduces some measurement error, and is likely to attenuate any effects that I report. Still, ZIP codes are often drawn around natural or social boundaries that reflect, at least to some extent, the orientations of local residents. The presence of a large retailer in the respondent's ZIP code does make her more likely to shop there, even if the relationship is far from deterministic. And to the extent that this coding impacts my estimates, any relationship that I uncover using ZIP code-level data is likely to be *stronger* when measured with more precision.

2.3   Other Explanatory Variables

To avoid obscuring or overstating the impact of commercial sprawl, controlling for factors that are likely to be correlated with both the dependent variables and the key causal vari-
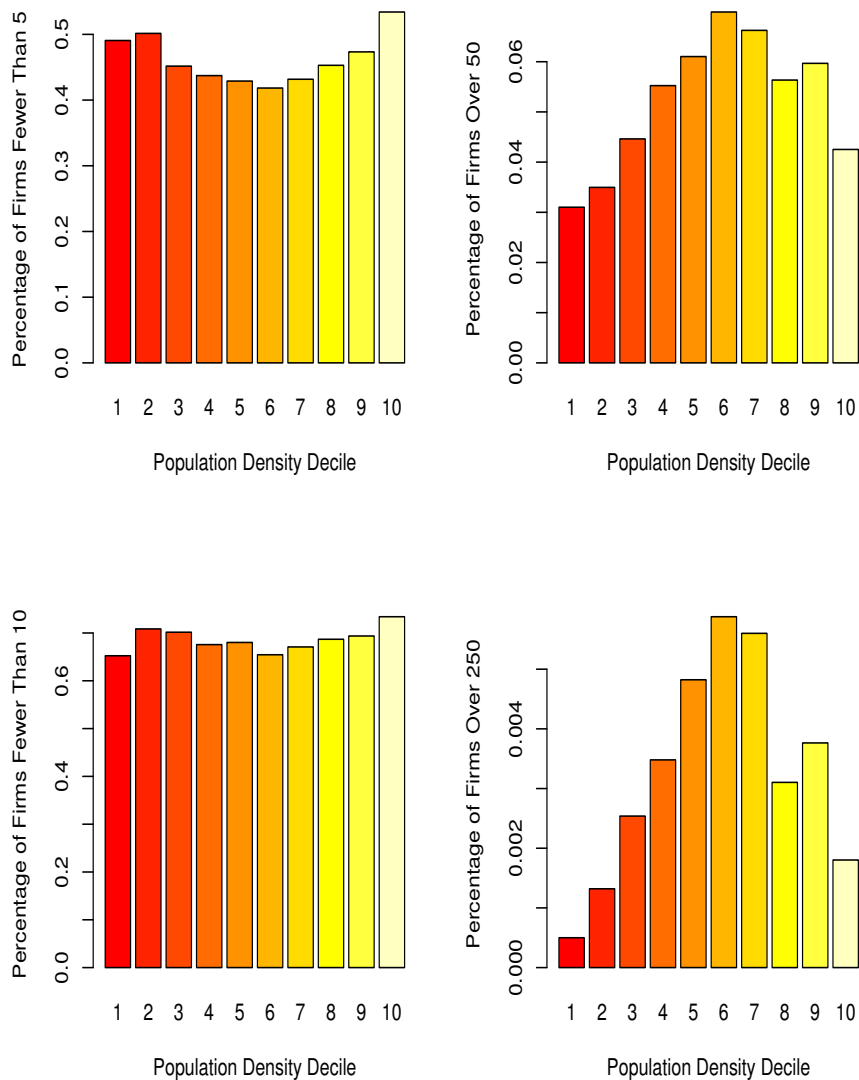
Figure 1: Picturing commercial sprawl: these plots show how the percentage of firms with different numbers of employees varies by population density. The retail world around you depends crucially on where you live, with those Americans in suburban environments (i.e. medium density) living among the largest proportion of large retailers. This is precisely what we would expect given anecdotal accounts. As Cohen (2003) points out, as recently as the 1950s, the opposite was the case: the largest retailers were located entirely within central cities. Also noteworthy is the seeming preponderance of small firms across all kinds of space.

Variable	Range	Mean	Standard Dev.
Race	0 - 1	0.129	0.33
Income	2 - 15	6.491	3.98
Education	1 - 7	3.652	1.84
South	0 - 1	0.297	0.46
Outside MSA	0 - 1	0.140	0.347
Outside Central County	0 - 1	.135	0.342
Outside Central City	0 - 1	0.214	0.410
Tenure in Community	1 - 6	3.572	1.48

Table 3: In control: this Table presents descriptive statistics for select explanatory variables. Rural communities appear to be slightly over-represented in the SCCBS: in the national sample, 9.2% of respondents are from rural areas, as opposed to 13.5% above. An "MSA" is a "Metropolitan Statistical Area" as defined by the U.S. Census Bureau. The regional variables are mutually exclusive dummy variables, so that "Outside Central County" marks only those individuals outside the central country who are not coded as outside an MSA.

able is crucial. Respondent-level controls include race,[7] gender, age in years, education,[8] household income,[9] length of residence,[10] and dummy variables indicating residency in the South[11] and home ownership status.[12]

At the ZIP code level, I control for economic activity through an area's retail density–that is, the number of total retail firms per person in the ZIP code–as well as the total number of firms in the ZIP code. These controls are vital, as they are likely to be foremost in the minds of decision-makers at large retail chains as they determine where to locate. I

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[7]Race is coded as a dummy variable, with African Americans and Hispanics who identify their race as "Black" coded as 1, and others coded as 0.

[8]Education is coded on a scale of 1 to 7, with 7 indicating those who have an advanced degree and 1 indicating those who have not completed high school.

[9]There are 6 income categories, coded such that 3.1 marks the category whose midpoint is $31,000 per year.

[10]An individual's length of residence is coded in 6 categories, with 1 marking an individual who has been in the community less than a year and 6 marking a life-long resident.

[11]As defined by the Census, the South includes Texas, Maryland, and the rest of the southeastern U.S.

[12]Those demographics that are under-represented in the dataset receive proportionately larger weights. For the full dataset, the mean weight is 1.00 and the standard deviation is .63.

also include population density decile at the ZIP code level, which should help distinguish other types of sprawl from the commercial sprawl that is our focus here. This control is doubly important since anecdotal evidence suggests that rural Americans often drive long distances to shop: failing to clearly distinguish them would bias my estimates. At the broader community level, I use dummy variables to mark the 40 distinct samples, and omit one as a baseline. Finally, following Verba and Nie (1972), I use three dummy variables to account for an individual's census classification.[13]

3 Methods

Recent years have seen a resurgence in interest in community-level variables, whether in political science (Campbell unpublished; Oliver 2001; Humphries 2001; Putnam 2000; Oliver 2000; Oliver 1999), economics (Alesina and La Ferarra 2000), sociology (Harding forthcoming; Sampson et. al. 2002), and psychology (Oliver 2003). But community-level analysis raises special methodological concerns about multi-level modeling and selection bias, concerns I take up here.

3.1 Multi-Level Data

One such concern emerges from the SCCBS's stratified survey design. Whereas standard statistical analyses focus on variables that are measured at a single level or perhaps two, studies of community effects are often interested in variables at multiple levels.

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[13]The three census classification dummy variables mark those outside a Metropolitan Statistical Area (MSA), those inside a MSA but outside the central county, and those outside the central city but inside the central county. Central city residence is thus the baseline.

With the dataset analyzed here, for instance, there are actually several different levels of observation: the 29,233 respondents are spread among 2 regions, 4 kinds of census classifications, 40 different samples, and 5,010 different ZIP codes. To deal with this multi-level structure, Humphries (2001) employs a hierarchical model, and other users of the SCCBS (Campbell unpublished; Rahn and Rudolph 2002) have done so as well.

But two conditions must hold to make a hierarchical model appropriate. First, the quantity of interest must be at one of the higher levels of aggregation. Otherwise, we could simply employ a fixed effects model and control for all community-level impacts through a dummy variable. And indeed, since we are not focusing here on the effect of the 40 community samples or the census classifications, I use this technique for those levels. Second, we must also have a prior reason to believe that there is an interaction between the contextual variables and the individual-level variables. In Humphries' words, it is precisely because standard models assume "a deterministic relationship between the contextual variable and the individual-level parameters" that standard single-level modeling produces biased standard errors (Humphries 2001:684-685). But since the theories developed above do not direct out attention to interactions between the individual-level coefficients and the ZIP code-level variables, a one-level model with fixed effects remains appropriate. I thus employ a straightforward logit model for binary dependent variables and Ordinary Least Squares for those dependent variables with four to seven linear responses.[14] At the same time, the main parametric findings have been verified through the use of hierarchical models–specifically, generalized linear models with random effects

[14]One could fairly argue that an ordered probit would be more appropriate, as OLS assumes unbounded dependent variables. In future work, I will test this, although the differences are likely to be minor. I also plan to test the effect of hierarchical modeling, although I do not expect the results presented below to change significantly. With 5,010 different ZIP code-based groups, the effect of grouping in giving us added leverage on local differences is likely to be far less pronounced than for other work (e.g. Rahn and Rudolph 2002).

for each community–which confirms that emphasizing more straightforward models as opposed to hierarchical models does not impact our substantive conclusions.

3.2   Selection Bias

Selection bias also makes studying community effects with observational data a challenging business (Harding forthcoming; Sander 2002; Sampson et. al. 2002). If we observe that communities with large chains exhibit more social cohesion, is it because of those chains, or because of underlying but unobservable characteristics that led those chains to choose that site in the first place? Some communities fight large retailers and try to stop their arrival; others, seeing them as a sign of progress and economic development, welcome them with open arms (Ortega 1998; Scott 1994). The problem is further complicated by the fact that there are at least two dynamic[15] processes taking place simultaneously. Just as large firms are choosing sites, residents are choosing where to live, based in no small part on the character of the contending communities.

How, then, to deal with selection bias? Some works in political science have employed Heckman-type selection models (Lemke and Reed 2001; Reed 2000; Berinsky 1999; Timpone 1998), which model both the selection process and the eventual outcome of interest jointly. But recent methodological advances have focused instead on semi-parametric or non-parametric models (Sartori 2003; Lee 2000; and Winship and Morgan 1999), including propensity score matching. Drawing on Harding (forthcoming), this paper uses matching to test the robustness of the findings generated by more familiar parametric

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[15]Among the many productive areas for future work on this topic, longitudinal studies of communities seem especially promising, as they can more explicitly acknowledge and scrutinize how the effects of commercial sprawl vary over time.

models. Readers unfamiliar with the technique and those interested in the specifics of the matching performed here should see the Appendix.

4 Parametric Results: Boxing Out Politics

To read critics of Wal-Mart and other chains, the arrival of a large-scale retailer signals the doom of collective life in a town. According one popular author, Wal-Mart is "destroying America" (Quinn 1998); for another, the right word is "devouring"(Ortega 1998). If the pervasive critique is right, we should expect to see huge effects in communities with large retailers. Analyzing the SCCBS data using a variety of techniques, though, we see nothing of the sort. Simply put, there is no evidence that the social network or community attachment hypotheses are correct. Only political involvement seems affected by the composition of firms in one's community, with effects that translate into 1-6 percentage point changes in political activity. Thus the impact of large retailers hinges on the type of dependent variable in question.

Below, I present results using single-level OLS and logit models and then move on to consider the findings estimated through propensity score matching. I focus only on two causal variables, the percentage of firms with more than 250 employees and the percentage with fewer than 5. But broadly similar results are obtained by using alternate measures of the retail environment, including the dummy variable for megastores, the percentage of firms that employ fewer than 10 people, and the percentage of firms that employ more than 50 people.

	Mean	St. Dev.	% Small Shops	% Megastores
Social Networks				
Trust Employees	2.00	.84	.02 (-.006,.05)	-0.02 (-.04,.008)
Business Friend	0.65	.48	-.002 (-.016,.01)	-.005 (-.017,.007)
Neighborhood Soc.	5.09	1.82	**-.03 (-.07,-.01)**	.003 (-.02,.03)
Trust	1.56	.61	.022 (-.005,.05)	-.024 (-.050,.0007)
Community Attachment				
Alienated	2.5	1.39	**.04 (.01,.07)**	**-.04 (-.07,-.01)**
Quality of Life	2.24	.73	-.02 (-.05,.001)	.02 (-.0008,.05)
Sense of Community	2.60	.78	.04 (-.05,.15)	-.07 (-.16,.02)
Political Activity				
Petition	.39	.48	**.02 (.008,.04)**	-.01 (-.02,.001)
Public Mtg.	.35	.47	**.014 (.002,.027)**	.002 (-.010,.016)
Reform Group	.21	.40	**.011 (.0007,.023)**	-.007 (-.018,.0035)
Pol. Group	.097	.30	**.009 (.001,.017)**	**-.016 (-.024,-.008)**
Registered	.83	.38	-.004 (-.013,.004)	.009 (-.0003,.018)
Voted 96	.744	.44	.006 (-.006,.018)	.002 (-.009,.013)
March	.07	.27	**.007 (.001,.014)**	**-.01 (-.018,-.004)**

Table 4: First Differences: the entries in this table are first differences calculated by holding all explanatory variables at their means and varying the retail environment from one standard deviation below its mean to one standard deviation above it. The numbers in parentheses are 95% confidence intervals, and bold entries indicate a confidence interval that does not include 0. Although 10 of 28 first differences are statistically different from 0 at the 95% level, the changes are nonetheless minute outside the political variables, as the standard deviations makes clear. At the same time, the variables indicating political activity do show changes that are both substantively and often statistically significant. Larger retailers can depress certain activites by a few percentage points, and small retailers have the opposite effect. "Small shops" refers to the percentage of local retailers employing under 5 people; "megastores" refers to the percentage employing more than 250.

4.1 First Differences, Few Differences

For each of the 14 dependent variables that measure some aspect of social capital or community engagement, I used OLS or logit models to estimate predicted probabilities for two hypothetical individuals.[16] The first individual is assigned the mean value for all control variables save the measure of the retail environment, where she is assigned one standard deviation below the mean.[17] The second individual is otherwise identical, but is assigned a retail measure one standard deviation above the mean. By simulating the difference between these two individuals' probability of expressing a certain view or participating in a certain activity, we can estimate the effect of a reasonable change in commercial sprawl.

As Table 4 illustrates, though, the effects of commercial sprawl are practically non-existent for two of the three categories of variables: neither the social networks hypothesis nor the community attachment hypothesis bears quantitative scrutiny. 11 of the 14 first differences in these two categories are statistically indistinguishable from 0. And given the relatively narrow confidence intervals on many of them, we can conclude that they are in fact precisely estimated zeroes–and not just a reflection of imprecision in estimation. Even in the three cases where the confidence interval indicates that retail scale matters for social networks or community attachment, a quick comparison between the first difference and the dependent variable's standard deviation shows that statistical significance does not imply substantive significance. Consider the effect of the percentage of megastores on alienation as just one example. While the first difference is statistically distinct from zero

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[16]These models exclude the national sample and other respondents with missing data on any covariate, still leaving them with at least 23,010 valid observations for most models. Not counting the national sample, this is a listwise deletion rate of no more than 13%.

[17]When considering the percentage of firms with fewer than 5 employees, for instance, one standard deviation below the mean is 32%, and one standard deviation above the mean is 58%.

at the 95% confidence level, the substantive effect is -0.04 on a variable whose standard deviation is 1.37. This represents just 3% of a standard deviation. There is the faintest hint that megastores make individuals less alienated. But nothing more. This is certainly nothing approaching the dramatic increase in alienation that some critics might condition us to expect.

4.2   Retail Politics

The clear and critical exception to these null effects comes from the dependent variables indicating political activity. With small shops, moving from one standard deviation below the mean to one standard deviation above it increases the percentage of respondents who signed a petition by 2.0 percentage points, or about a 5% increase over the mean. This makes sense: the sidewalks that connect small shops make an excellent site for petitioning. The trend is similar for other political activities. For public meetings, the increase is 1.4 percentage points, and for reform groups it is 1.1 percentage points. Having proportionately more small businesses in one's ZIP code does lead to increases in political engagement.

Notice, too, that there are inverse effects, just as the hypotheses about the privatization of public space would predict. As the percentage of local megastores increases, one's probability of participating in a march or demonstration declines by 0.7 percentage points, a significant change on a baseline probability that is just 7 percentage points. One's probability of participating in a political group likewise declines by 1.6 percentage points. Echoing Williamson (2002), we see a distinction based on the type of political activity when looking at the impact of large retailers. Megastores matter most when it comes

to the more confrontational political activities, including marches and joining political groups. What's more, across most of the political dependent variables, small businesses seem to help more than large retailers hurt.

5   Matching Results

Even with the measurement error that comes with looking for ZIP code-level effects, the parametric analyses find a consistent relationship between political activity and retail scale. Still, the results outlined above are vulnerable to cries of selection bias. A critic might argue that they tell us more about how firms choose their locations than about the causes of social capital and community engagement. How can we know that the large retailers *caused* the changes in political activity, and didn't simply choose sites where local opposition was likely to be limited? And just as large retailers are selecting among communities, so too are individuals, creating another layer of potential selection bias. If large retailers signal an economically vibrant community, perhaps the influx of new residents lacking in social connections dampens political participation.

Undoubtedly, data about when firms arrived in a given community would help us untangle these questions, as we would expect firms that have been in a community longer to have a more marked impact on local residents. But even in its absence, by restricting my dataset to the 15,351 respondents who have lived in their communities for at least 10 years, we can limit the impact of selection bias caused by individual-level decisions as to where to live. Wal-Mart and other retailers expanded tremendously in the 1990s; by focusing our attention on those who lived in their community before that expansion, we can limit

worries about selection bias based on individual residential choices.

Although it is virtually impossible to control for all aspects of large retailers' site selection, this paper consistently includes controls for retail density and the total number of firms as coarse estimates of the local economic climate. The underlying assumption is simple: in the end, firms worry far more about the economic climate than they do about the political climate in making location decisions. Can firms even accurately observe the local political climate? we should wonder. Moreover, since political participation is observed at the level of the individual, it is safe to assume that in virtually all cases the causal arrow runs from the retail environment to the individual's participation, and not vice versa.

Propensity score matching provides an alternate, non-parametric way of estimating the effect of living near a large retailer, one that strengthens my claim that selection bias is not driving these results. Almost entirely, the matching-based analysis confirms the conclusions suggested by the models above, and at times identifies an even stronger dampening effect on political activity. Alongside a justification of matching as a technique for dealing with selection bias, the Appendix illustrates that the matching performed here was effective. That is, it produced a treatment group and control group that are quite similar on the observed covariates.

Table 5 presents the mean values for the dependent variables for the 3,723 treated individuals as well as a matched and identically-sized control group. The p-values give us a sense of the strength of the conclusions we can draw from the results. First, consider social networks and community attachment. As is evident, several differences across the two populations reach statistical significance at the traditional .05 level, suggesting that control group respondents tend to be slightly less trusting of the people who work where

they shop; that control group respondents are slightly less likely to socialize with their neighbors; and that control group respondents are ever so slightly more likely to rate the quality of their community highly. Still, when talking about the social networks or community attachment measures, even those results that reach statistical significance have little substantive significance. For instance, none of those three variables change by even one tenth of its standard deviation across the two populations.

Again here, political activity is a different story entirely. Living near a megastore has an especially depressive effect on the most explicitly confrontational and political activities, making individuals 3.7 percentage points less likely to participate in a march or 5.6 percentage points less likely to be a member of a political group. These are huge changes considering that fewer than 10% of the population participates in those activities to begin with. An individual's likelihood of participating in a public meeting or a local reform group both decrease as well, public meeting attendance by 3.7 percentage points and participation in local reform groups by 5.3 percentage points.[18]

Those who would claim that the central conclusions of this paper are driven by methodological choices have a hard case to make, as the same results appear using a non-parametric approach, a more limited dataset of spatially fixed respondents, and a different specification of the key causal variable. And those who believe that the results are driven by an omitted variable face a challenging task as well: they must identify a variable that is both theoretically reasonable and not among the 18 highlighted in Table 6 in the Ap-

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[18]In certain cases, these estimates actually grow larger when a matching analysis is performed on the entire dataset. After listwise deletion, we are left with 23,710 respondents, and can then create treatment and control groups of 7,395 respondents each. Having done so, the estimated impact on march participation is a decline of 1.6 percentage points and the impact on political group membership is 6.2 percentage points. The probability that the respondent attended a public meeting drops by 3.8 percentage points, and the probability of participating in a local reform effort drops by 6.9 percentage points.

	Control	Treatment	Diff.	SD	P-value
Social Networks					
Trust Employees	1.95	1.91	-0.04	0.81	**0.02**
Business Friend	0.67	0.67	0.00	0.47	0.66
Neighborhood					
Socializing	5.20	5.29	0.09	1.68	**0.02**
Trust	1.52	1.50	-0.02	0.61	0.08
Community Attachment					
Alienation	2.41	2.46	0.04	1.37	0.16
Quality of Life	2.29	2.32	0.03	0.72	**0.04**
Sense of Community	2.69	2.67	-0.02	0.71	0.43
Political Activity					
Petition	0.43	0.42	-0.01	0.49	0.33
Public Mtg.	0.41	0.37	-0.04	0.48	**0.00**
Reform	0.27	0.22	-0.05	0.41	**0.00**
Political Group	0.16	0.11	-0.06	0.31	**0.00**
Voted in 1996	0.82	0.83	0.01	0.39	0.25
Registered to Vote	0.89	0.90	0.01	0.31	0.11
March	0.10	0.07	-0.04	0.25	**0.00**

Table 5: Matching results for those who have lived in their community for over 10 years, illustrating that the presence of at least one large retailer depresses political activity but has only minor and substantively insignificant effects on social networks or community attachment. The standard deviation is for the population who have lived at their residence for more than 10 years, and is reported to help contextualize the difference in the two means. The original universe is n=15,351, and that figure drops to n=12,226 after listwise deletion. The 3,723 treated respondents–those living in a ZIP code with a Wal-Mart–were matched with 3,723 respondents from the control group. Bold indicates p-values less than .05. Numbers may not add perfectly due to rounding.

pendix. Perhaps most importantly, any challenge to the results presented here must be able to explain the differential impact that retail scale has on *political* activities.

Conclusion

Many contemporary critics simply assume that large retailers weaken communities. But in truth, the impact of commercial sprawl hinges on how we conceive of community strength. If we understand communities' assets to lie chiefly in their social networks or in the attachment they command from their residents, then the common critique of large retailers appears misplaced. Regardless of our methodological approach, neither seems to be significantly impacted by changes in the retail environment, with the important caveat that ZIP codes are an imperfect measure of one's retail environment. Turning our attention to political activity, though, we have far more reason to fault large retailers. Large retailers appear to dampen a variety of forms of political activity by anywhere from 1 to 6 percentage points. If anything, these effects are understated. From the research presented here, only those hypotheses that emphasize the depoliticized nature of large retail space hold up.

What explains the substantial gap between our expectations and our empirical observations when talking about community engagement and social networks? Part of the discrepancy stems from the implicit comparisons that critics are making when they argue that large retailers harm communities. Their baseline might not be some neighboring town without a large retailer, but instead their own town 30 or 40 years back. Put differently, perhaps the relevant variation isn't cross-sectional but instead longitudinal. If all

communities have been impacted at least to some extent by changes in economic scale, then my estimates–which compare towns at just one point in time–will miss the full extent of the transformation as it has been experienced over the years. Still, the choice that confronts towns is not about which decade they want to live in: it is about whether they want to permit large retailers to move in or not. And this analysis might just help communities make that decision, by quantifying the very real declines in political activity that are likely to follow when Wal-Mart comes to town.

APPENDIX: Propensity Score Matching

Although matching has been discussed more fully elsewhere (i.e. Harding forthcoming), it merits a brief explanation. Drawing on the vocabulary of experimental science, we can consider those individuals who actually live near a large retailer to be in the treatment group. Our goal is to identify a comparable control group, a group that doesn't live near a large retailer but is otherwise similar to those individuals who do. To do so, I divided the relevant subgroup of SCCBS respondents (the 12,226 individuals who report living in their communities for at least 10 years and who had complete information on the covariates used in matching) into a treated population (n=3,723) and a control group (n=8,503). Using the relevant covariates[19] and the binary variable for nearby megastores as the dependent variable, I then fit a logit model. By using the resulting coefficients, we can compute each respondent's underlying propensity to live in a ZIP code with a large retailer. These propensity scores are a one-number summary of the factors that make individuals more or less likely to live near a large retailer, meaning that individuals with similar propensity scores should differ only in their actual place of residence. Making use of the program "MatchIt," (Ho et. al. 2004) I then matched members of the control group and the treatment group, allowing replacement. Done correctly, this process produces two groups that are quite similar in their observable propensity to live near a large retailer.

One might object that some of the covariates described above are inappropriate in creating a propensity score for one's place of residence. But the appropriateness of the covariates

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[19]Individual-level attributes inlcude an individual's education, age, income, homeownership status, marital status, race, gender, and tenure in the community. In addition, I included the survey weight indicating the likelihood of reaching an individual of that demographic. Community-level variables include whether the individual's community is outside an MSA, inside an MSA but outside the central country, or inside the central county but outside the central city, as well as variables indicating the number of retail establishments, the retail density, the population density, and southern regional status.

	Control	Treatment	Diff.	SD	P-value
Rural	0.13	0.13	0.00	0.34	0.94
Within an MSA	0.11	0.13	0.02	0.34	0.01
Within Central County	0.20	0.22	0.02	0.41	0.04
Total Retail Est.	169.39	183.26	13.87	146.74	0.00
Retail Density	0.0050	0.0051	0.0001	0.01	0.13
Pop. Density	5.53	4.83	-0.70	2.87	0.00
City Income	3.17	3.19	0.02	0.36	0.01
Local % Black	0.12	0.12	0.01	0.10	0.01
South	0.26	0.30	0.04	0.45	0.00
Education	3.82	3.74	-0.08	1.82	0.05
Age	48.65	49.38	0.73	16.87	0.05
Income	7.06	6.99	-0.07	3.99	0.47
Homeowner	0.76	0.81	0.05	0.38	0.00
Married	0.50	0.54	0.03	0.50	0.01
Race	0.08	0.08	0.00	0.31	0.64
Gender	1.55	1.58	0.03	0.49	0.00
Tenure	4.83	4.82	-0.01	0.68	0.69
Survey Weights	0.99	1.00	0.01	0.65	0.46

Table 6: This Table reports mean values for the treatment group (n= 3,723), the matched control group (n=3,723), the difference, the variable's standard deviation, and the p-value for the difference. The effectiveness of matching can be tested by comparing the resulting control group and treatment group. And as the Table above illustrates, they are in fact quite similar. On a few of the strongest predictors of political participation, including income, the two groups are statistically identical, as indicated by the p-vale above .05. And on virtually all explanatory variables, the differences in the samples are not substantively significant, a fact which is illustrated by a comparison between the mean difference and the much larger standard deviations.

can be judged entirely on whether they produce two groups that are similar in the relevant ways. And as Table 6 illustrates, matching has successfully created two groups that are quite comparable on virtually all the major covariates. Although the differences between the groups often reach the level of statistical significance, they are rarely substantively significant, a point which is reinforced by comparing the mean differences with the standard deviations. Especially noteworthy is the fact that the individual-level predictors are more effectively matched than the community-level predictors, a result which may stem both from the greater measurement error inherent in community-level variables and from the centrality of individual-level variables in predicting residential choices. Still, even on the community-level variables, matching produced two groups whose means differ by no more than a tenth of a standard deviation on any variable save population density. And since population density has a mixed impact on political participation (Williamson 2002), even the difference there (roughly one-quarter of a standard deviation) should be considered minor. In other words, matching appears to have worked, and created two comparable datasets for further analysis.

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