Laws of Attraction: Regulatory Arbitrage in the Face of Activism in Right-to-Work States

SAGE

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Abstract

Past research recognizes that firms exploit regulatory variations to their advantage but depicts such regulatory arbitrage as a dyadic process between firms and regulators. We extend this account by including a firm's non-market rivals and suggest that firms view regulatory differences as part of a corporate political opportunity structure and exploit regulatory variations to disadvantage their rivals. Empirically, we focus on variations in right-to-work (RTW) laws that signal the pro-business climate in a state; these laws exist in 22 U.S. states. Using a spatial-regression discontinuity design, we analyze how Walmart locates new stores in the face of anti-Walmart activists and exploits regulatory discontinuities on the borders between RTW and non-RTW states. We find that Walmart is more likely to propose new stores, and to open those stores even if they are protested, at the borders of RTW states, compared with the borders of neighboring non-RTW states. We conclude with a discussion of implications for the study of regulation, social movements, and organizations.

The metaphor of regulatory races has inspired a body of research that predicts the convergence of regulations governing business firms across states. The mechanism underlying these arguments is regulatory arbitrage—if regulatory policies do not suit business firms' interests, they will locate their operations in pro-business states, creating an incentive for other states to become pro-business (Drezner 2001; Murphy 2004). Tiebout (1956) suggested that jurisdictions will be compelled to provide an efficient mix of public goods and taxes or will face an exodus of residents to other jurisdictions. In a detailed review of the literature, Carruthers and Lamoreaux (2009:45) observe

that "regulatory races . . . are much clearer in theory and political rhetoric than they are in reality. In many situations, a substantial degree of regulatory variation endures." Hence, there are ample opportunities for corporations to engage in regulatory arbitrage.

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As nation-states are weakening, activists are increasingly targeting corporations (see Baron and Diermeier 2007; King and Pierce 2010). Corporations are more vulnerable than states to the threat of delegitimation, but they lack states' weapons to repress, routinize, or channel activists' protests (Walker, Martin, and McCarthy 2008). Yet, corporations do have wide latitude to exploit regulatory variations in their response to protests. Corporations treat protests as signals of regulatory costs (Ingram, Yue, and Rao 2010), but the existing structure of regulation is likely to moderate signal quality. Although a few studies examine how firms exploit regulatory variations when locating operations (e.g., Dube, Lester, and Eidlin 2008; Holmes 1998), these accounts depict regulatory arbitrage as a dyadic contest between firms on the one side and regulators on the other. The image is one of regime shopping for the most favorable jurisdiction. However, firms also have to contend with non-state political rivals when locating their operations, and any micro-account of a firm's location decisions must consider how firms exploit regulatory variations to disadvantage these rivals. In this context, large business firms may have to contend with social movement activists who seek to limit or disrupt their operations. Indeed, formal control of the state and more diffuse forms of social control are often substitutes for each other (King 2009; Schneiberg and Bartley 2001; Simons and Ingram 1997; Soule 2009; Weber, Rao, and Thomas 2009). This substitutability may create arbitrage opportunities. For example, Cowie (1999) shows how RCA moved operations from Camden, NJ to other cities as the risk of unionization increased.

These considerations supply the motivation for us to study the role of social activists in Walmart's location decisions, as the firm chooses between states with right-to-work (RTW) laws and those without. Why do we expect a firm such as Walmart to exploit variation in RTW laws? We do not think threat of unionization is the answer. Walmart is non-unionized and has had very few union organizing efforts directed against it (Lichtenstein 2009).

We suggest that RTW laws signal a positive business climate and lower the risk that protesters will seek to establish restrictive regulations on large firms. RTW laws therefore constitute an element of the political opportunity structure for large firms and for activists (McAdam, McCarthy, and Zald 1988). We expect Walmart's proposals to open establishments to abruptly increase when we cross the borders of an RTW state. We expect Walmart is more likely to open a store on the border of an RTW state even when it faces protests. In an earlier study, Ingram and colleagues (2010) show that Walmart treats protests as signals of subsequent regulatory costs; Walmart walks away from locations to prevent the snowballing of protests into a hostile regulatory regime spanning multiple locations in a given state. We extend this line of reasoning by suggesting that the existing regulatory context moderates the efficacy of protests. As a result, protests in RTW states convey less negative information: Walmart is unlikely to believe the community will be anti-business because legislators and voters have already revealed they are pro-business. In short, we contend that regulatory arbitrage plays a role in mediating the efficacy of protest (Amenta 2005). We predict that large organizations such as Walmart are more likely to commence operations despite protests when they cross the border of RTW states, compared with non-RTW states.

We focus on Walmart for three reasons. First, it is arguably the most consequential firm in the U.S. economy. Walmart's decisions are of interest to economic sociologists and political sociologists alike. Second, its proposals to open new stores often encounter protests. Since 1988, Walmart has been opening supercenters that have 150,000 to 250,000 square feet of space; these stores have grocery sections and offer a wide array of products. In general, Walmart's entry leads to a 3 percent overall price decline in competing stores, and in the case of some items, the declines are as high as 13 percent (Basker

2005; Hausman and Leibtag 2005). Due to Walmart's impact on local retail trade and the increase in congestion and traffic, activists often protest proposed new stores. Many of these activists are seeking to preserve Main Street or are driven by not-in-my-backyard (NIMBY) motivations. Typically, protesters want to establish stringent size limits on the size of new stores to insulate towns against the entry of big-box retailers such as Walmart, Home Depot, and Target. Indeed, *Forbes* magazine identified activists leading protests as Walmart's principal enemy. In view of these protests, Walmart likely has incentives to engage in regulatory arbitrage. Finally, by focusing on the location decisions of one firm, we reduce the problems that unobserved heterogeneity across firms presents for analysis.

REGULATORY ARBITRAGE: LAWS, POLITICAL OPPORTUNITY, AND MEDIATION OF PROTEST

When there is regulatory variation, business organizations may engage in regulatory arbitrage in a variety of ways. For example, when the 1863 National Banking Act imposed a 10 percent tax on banking notes issued by state banks, U.S. banks shifted from state to federal charters to avoid the tax (White 1983). A more complex form of arbitrage across geographic borders occurs when European financial institutions shift poorly monitored risk exposures to taxpayers in a different country through cross-border mergers (Carbo-Valverde, Kane, and Rodriguez-Fernandez 2008). By contrast, a simpler form of regulatory arbitrage across geographic borders occurs when firms shift geographic location in response to legislation.

Legal and regulatory variations across states provide large business firms political opportunities to disadvantage rivals such as activists. Much of the discussion of political opportunity structures, however, has been from the vantage point of activists challenging state authorities (see McAdam, Tarrow,

and Tilly 2003; Meyer 2004). Some scholars suggest that one also ought to think of how political opportunity influences policy outcomes. Political mediation theory holds that the "ability of a challenger to win collective benefits depends partly on conditions it can control, including its ability to mobilize, its goals and program . . . including issue framing and other claims-making. However, the impact of even well-mobilized challengers also depends on political context" (Amenta, Caren, and Olasky 2005:519–20). Strong versions of the political mediation model hold that activism matters only when the political context is favorable, and weaker versions insist that political context intensifies the effect of activism (Soule 2009). In particular, ease of participation in the political system, existence of patronage politics, availability of support from bureaucrats, and, most of all, whether a regime is open to claims determine the effectiveness of activist mobilization (Amenta, Carruthers, and Zylan 1992; Amenta, Dunleavy, and Bernstein 1994).

In the case of private politics, activists are battling large corporations (Baron and Diermeier 2007). As a result, one ought to emphasize corporate opportunity structure to understand political mediation (King 2008; Soule 2009). Nonetheless, studies such as King (2008) and Soule (2009) define opportunity structure from the perspective of the activist and identify factors such as poor performance or leadership changes as opening up windows of opportunity. We extend this line of work by focusing on opportunities from the point of view of the corporate target.

What might a corporate target consider as part of an opportunity structure as it seeks to disadvantage rivals? A number of studies suggest that corporations look at the legal infrastructure for guidance on contested issues (Edelman and Suchman 1997; Soule 2009). We argue that corporations do more than that—they pay great attention to how legal and regulatory variability magnifies or reduces activists' effectiveness and make location choices accordingly. We suggest that corporate targets are concerned about laws that signal a pro-business climate and restrict

activists' ability to put restrictive regulations in place. We focus on Walmart and how it sees RTW laws as undermining activists' effectiveness and therefore locates stores right across the borders of RTW states.

The 1935 Wagner Act enabled union organizing and identified unfair labor practices that management were not allowed to use. The 1947 Taft-Hartley Act undid some of the provisions. Almost uniquely among federal laws, this act allowed individual states to weaken the legal protection afforded to unions. In particular, it allowed states to exempt new employees of unionized firms from being required to join a union and from paying dues, but it gave these employees the benefits of the union contract. Twenty-two states, mostly in the south, passed RTW laws; in 2001, Oklahoma was the last state to enact such a law (Reed 2003). A number of studies show that RTW laws have a negligible effect on wages (for a review, see Moore 1998), but a recent study finds 2 percent higher wages in RTW states (Greer 2004). Some evidence shows that RTW laws reduce union membership (Davis and Huston 1993; Ellwood and Fine 1987) and induce unions to abandon organizing drives (Ferguson 2008). Because only 22 states enacted RTW laws, substantial heterogeneity remains, creating an opportunity for regulatory arbitrage.

Why would Walmart engage in regulatory arbitrage on account of RTW laws even when it has not faced a serious threat of unionization? Moore and Newman (1985) observe that while it is difficult to directly measure the business climate of a state, the division of powers between management and unions is one signal of a pro-business climate. Early on, RTW laws were a narrow signal that it was costly to organize unions in a state. Since then, the laws have become a broad-based signal of a state's pro-business ideology. As Holmes (1998:673) observes, "the same forces that lead to the passage of right-to-work laws also lead to the adoption of other pro-business policies." In fact, states routinely market themselves as pro-business by proclaiming that they have an RTW law—it telegraphs the state's ideology. In a 2010 CEO survey of best and worst states for businesses, 9 of the 10 best states had an RTW law, and none of the worst 10 states did (ChiefExecutive.net 2010).

For organizations such as Walmart, an RTW law signals that protests might be hard to organize in that state. Even if anti-Walmart groups organized a protest, be they NIMBY activists or small businessmen concerned about Main Street, they would find it hard to gain the support of legislators, government authorities, voters, or consumers. For large businesses, an RTW law signals that it is less likely regulatory restrictions will be implemented on the size of their stores. The nuclear option of regulatory responses against Walmart is a size-cap restriction that limits the size of retail stores in a municipality to preclude big-box retailers. Groups that protest against Walmart stores in the name of protecting Main Street business and reducing urban sprawl often seek to mobilize popular support for a size-cap regulation that would limit a store's footprint to 30,000 square feet or less. This renders the economics unviable for Walmart, which typically establishes superstores with 150,000 to 200,000 square-foot floor plans. As of 2005, about 23 percent of RTW states had some local size-cap legislation, while about 56 percent of non-RTW states had such laws.

Walmart's retail model fits the possibility of arbitrage well. A 200,000 square-foot store draws customers from many miles around, particularly in the rural areas that are Walmart's base. Given the gravity of a Walmart, it is possible to reach the same customer from any one of a number of potential locations. If Walmart does not find a favorable policy and cultural context in one place, it may siphon retail customers from that place by locating nearby. The process resembles the bargaining over drilling rights between land owners and oilmen depicted in Wes Anderson's feature film *There Will Be Blood*. In this analogy, Walmart is the oil man, and communities are the landowners who want the best deal possible (in terms of planning, taxes, and good

jobs) but risk having their retail dollars sucked into a neighboring jurisdiction if they bargain too hard. Stone (1997) provides a good example of Walmart's arbitrage at state borders. In that case, Walmart built stores on the New Hampshire and New York borders to suck trade out of Vermont, a state that implemented hostile policies toward Walmart in an attempt to protect small merchants.

In the context of private politics, where activists and their targets seek to gain advantage, a pro-business climate is a key part of political opportunity structure in favor of the target and against activists—it signals the tastes of voters, elected legislators, and regulators. Abraham and Voos (2000) report that stockholder wealth rose when Louisiana enacted an RTW law in 1976 and when Idaho did so in 1985 to 1986, presumably because investors anticipated higher future profits with weaker unions, and lower probabilities of restrictive regulations. Stevans (2009) finds that even after correcting for endogeneity, self-employment increased in RTW states and the ratio of bankruptcies to number of firms declined significantly. In view of these findings, the presence of RTW laws would be a signal of favorable opportunity and should increase Walmart's proposals to open new stores. This leads to Hypothesis 1:

> *Hypothesis 1:* Walmart is more likely to issue proposals for new stores at the borders of RTW states than it is in comparable places in neighboring non-RTW states.

Even if there are protests against proposed stores in the border area of an RTW state, Walmart is likely to open the store because the pro-business climate implies support from elected officeholders and bureaucrats. Amenta and colleagues (1992, 1994) suggest that protests' effectiveness against a target is mediated by such support. Put simply, the "productivity of collective action of state-oriented challengers is mediated by political circumstances" (Amenta 2006:8). More specifically, in a polity where there are resource constraints on activists, collective action is

likely weakened. Moreover, partisan regimes that undermine social movements are also likely to dampen the effect of protests. In an environment propitious for protesters, sheer mobilization might be enough for activists to exert influence on a target, but in an unfavorable environment, a movement's impact is severely weakened. King (2008), for example, finds that political context mediates the effectiveness of consumer boycotts directed against private firms.

Ingram and colleagues (2010) observe that Walmart often accedes to protests against proposed stores; they argue that protests serve as a signal of a community's capacity for collective action. Such signals are less clear in RTW states, where the pro-business climate makes Walmart more confident of maintaining or gaining the support of elected officeholders, regulators, and voters. Similarly, in places with a pro-business climate, anti-Walmart protests may be less likely to dent customer patronage. Ingram and colleagues (2010) argue that acceding to protests is relatively cheap for Walmart, because it can typically find another location of comparable business value nearby. This is less true, however, in places in RTW states that border non-RTW states, because some of the nearby locations are in states with a less favorable business climate. For these reasons, we predict the following:

> *Hypothesis 2:* Walmart is more likely to open new stores despite protests at the borders of RTW states than it is in comparable places in neighboring non-RTW states.

DATA AND METHODS

Regression discontinuity designs are an econometric method used to evaluate causal effects of interventions. They take advantage of the fact that, although treatment and control groups may be systematically different, their differences within a small bandwidth of a cutoff point are slight. Regression discontinuity designs identify local differences at the cutoff point (Imbens and Lemieux 2008). Spatial

regression discontinuity designs are a special case in which geographic borders are sharp cutoff points (Moore 2009). By assigning places within a limited range of geographic distance on one side of borders into a treatment group and those on the opposite side to a control group, spatial regression discontinuity designs help establish a causal relationship if an abrupt change can be observed across borders. For example, Holmes (1998) compares places within 25 miles of the border of an RTW state with their twins—that is, places within 25 miles of the border of an adjacent non-RTW state. The design's strengths are that (1) geographic characteristics tend to be similar on both sides of a border; (2) the high cost of moving far away from a border makes the cutoff meaningful; (3) the design helps avoid ecological fallacy by localizing estimates; and (4) it can be widely applied to many contexts. The treatment assignment process is completely known and perfectly measured, a feature that regression discontinuity designs share with randomized controlled trials (Shadish, Cook, and Campbell 2002).

In our case, manipulation of the treatment variable (i.e., RTW laws) occurred in the treatment area before measurement of the outcomes (i.e., proposals to locate Walmart stores and whether stores opened despite protests). Because geographic conditions are approximately the same on both sides of a border, what differs is the effect of state policies. To the extent that pro-business policies pursued by RTW states have resulted in regulatory arbitrage, there should be an abrupt change in Walmart's behavior.

Our data set consists of places located within 25 miles of the border with a neighboring state that has a different status of RTW law. The border between two states with different RTW laws (i.e., one state has such a law and the other does not) is defined as a contrast border (for a list of states with contrast borders, see Table S1 in the online supplement [http://asr.sagepub.com/supplemental]). Place is our unit of analysis, which refers to a city, town, village, or unincorporated census area. Place is generally a smaller unit than county, and there were 25,375 places in the United States in

2000. Our sample has 3,179 unique places located within 25 miles of contrast borders.[1] Places on RTW and non-RTW sides of contrast borders each constitute about 50 percent of our border sample. Table S2 in the online supplement lists the basic social demographic and economic characteristics of places on both sides of contrast borders. Results show that places on both sides of borders are largely comparable.

To calculate the distance from a place to the closest contrast state border, we first obtained a longitude and latitude list of the points at state borders from the website of the National Atlas (http://nationalatlas.gov) and a list of the longitude and latitude of the center of each place from the 2000 Census. We then calculated the distance between the center of a place located on either side of a contrast state border to the closest contrast border point and selected places within 25 miles of contrast state borders. Figure 1 illustrates the geographic distribution of these places.

We define a new store proposal as a proposal to open a new Walmart (i.e., a discount store, a supercenter, or a neighborhood market). We do not treat a relocated store (i.e., moving an existing store to a new location in the same community) as a new store. We compiled data about Walmart's proposals, protests, and openings mainly from two sources. First, for proposals that resulted in actual store openings, we obtained a list of all Walmart store openings from 1962 to 2007.[2] We estimate proposal time for each opened store as 789 days before the opening, a figure that represents the average time between proposal and opening for stores where both dates are available. Second, for aborted proposals, we collected data about Walmart's proposals from Sprawl-Busters, an anti-Walmart organization that has been documenting anti–big-box store protests from various sources since 1998.[3] From the Sprawl-Busters database, we selected all protests that targeted Walmart's store proposals from 1998 to 2005 in border places.[4] We also collected reports of Walmart's proposals from other activists' websites. In addition, we conducted a media search for reports about Walmart's store
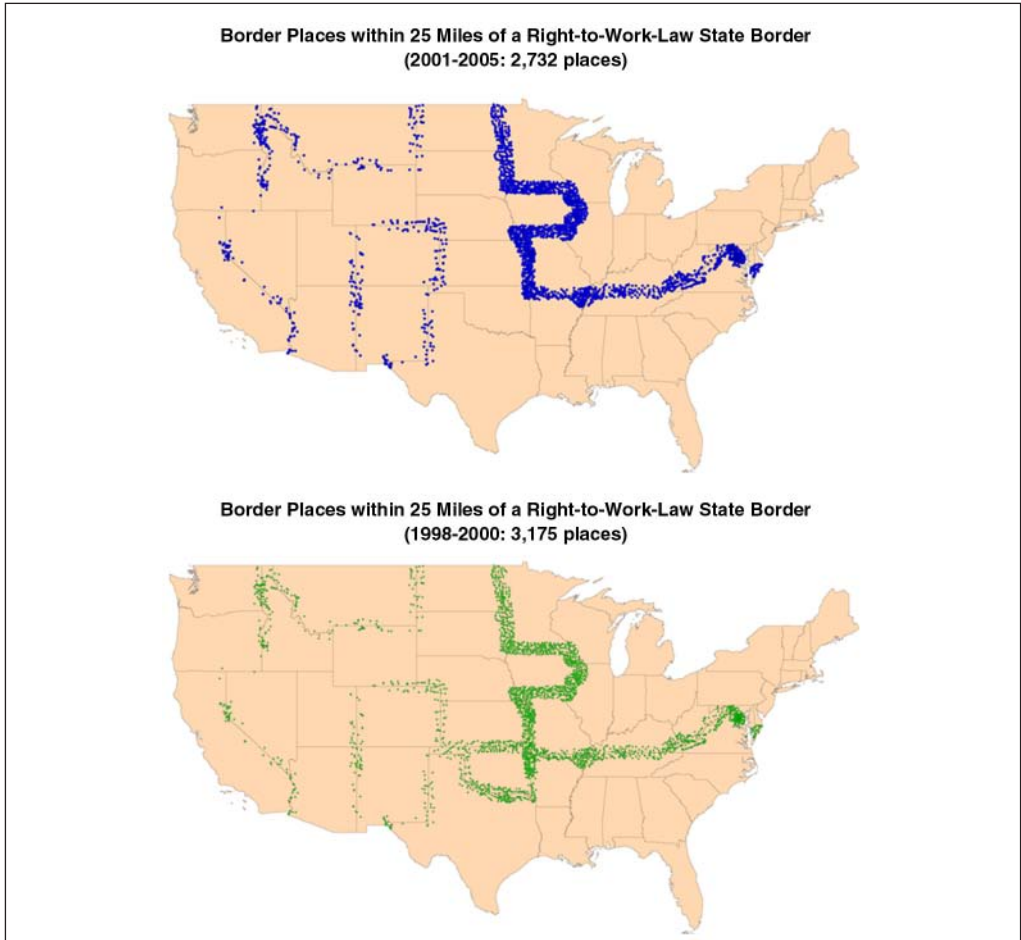
Figure 1. Border Places within 25 Miles of a Right-to-Work-Law Contrast State Border

proposals from 1998 to 2005 using Lexis-Nexis and America's News databases.

From our search through activists' sites and news media, we coded whether a specific proposal was protested. We coded protests as occurring if our sources reported that individuals or organizations did any of the following in response to a proposed Walmart store: encouraged public hearings; collected citizens' signatures to initiate a referendum; demanded additional studies of Walmart's impact on local businesses, traffic, and environment; highlighted environmental hazards; deployed zoning restrictions; lobbied for store-size cap legislations; or filed lawsuits against Walmart or local government. A protest

against a proposed Walmart store can be reported multiple times; we coded multiple reports as one protest as long as they were targeted at the same store proposal.

Finally, we matched data of proposed stores and protests obtained from the above sources and dropped duplicate cases. From 1998 to 2005, Walmart made 1,592 proposals in the 48 continental states and Washington, DC, and 563 of these were protested. These proposals resulted in 1,034 new store openings. Within 25 miles of contrast RTW borders, Walmart made 102 proposals, 34 of which were protested, and 73 stores eventually opened. Our multiple sources of data with different interests, including Walmart,

protesters, and the media, mitigate the concern about selection bias that would loom large if we relied on only one source. Overall, 94 percent of Walmart's proposals resulted in store openings or appeared in more than one of our sources.

A potential challenge to our methodology is that although there are almost equal numbers of places on either side of a border, we are not sure they are symmetrically distributed along state borders. Put another way, analyses of all places will compare two columns of observations on either side of the contrast border, and we therefore need two adjacent cells on either side of the border so that apples are being compared with apples. This requires that we create a grid of observations so that cells can be paired together. We adopted two strategies. One was to pair each place with other places within 50 miles but on the opposite side of contrast state borders. This is a multiple pairing strategy; a place can appear in the sample multiple times if there is more than one qualified pair partner on the opposite side of the border. We ended up with 88,495 pairs that result in 1.28 million observations. The second strategy was to define the cells more narrowly and construct unique pairs by pairing each bordering place with its geographically closest neighbor on the other side of the RTW border. Because unique pairing by distance has to be symmetric for both partners, our sample is substantially reduced to 386 unique pairs that result in 5,472 observations. We also report results using the sample of unpaired places in the robustness check.

Dependent Variables and Estimation

Our first dependent variable is whether Walmart proposed to open a store in a place in a year. Proposal is a dichotomous variable coded 1 if Walmart proposed to open a store in a place in a year. We use a probit model to estimate the effect of RTW laws on Walmart's proposal behavior. Because most states did not experience a change in the status of RTW

laws during our observation period, variance regarding RTW laws is mainly cross-sectional. We therefore also report results of the pooled cross-sectional probit analysis.[5]

Our second dependent variable is a dichotomous variable that indicates whether a proposed Walmart store opened. We coded an opening as 1 if a proposed store successfully opened by the end of 2007. We use a pooled probit model to estimate the effect of RTW laws on the probability of opening proposed stores. However, we confronted a non-random assignment problem. Protests are not likely to happen randomly; communities consider their chances of success when deciding whether to organize protests. An added issue is that protests are conditional on a proposal from Walmart, and these proposals are not distributed randomly.

We therefore use the inverse probability treatment weighting (IPTW) method that was recently developed and widely adopted by biostatisticians to resolve the non-random assignment problem in observational data (Azoulay, Ding, and Stuart 2007; Robins, Hernan, and Brumback 2000). IPTW relies on the logic of counterfactuals and compares each treated subject or observation with a pseudo-population; the difference between the two groups represents the average treatment effect. We assigned each observation in the sample a stabilized weight,[6]

$$sw_i = \frac{P(A = a_i)}{P(A = a_i | L = l_i)}, \text{ where } a_i = \{0,1\} \text{ indi-}$$

cates potential treatment (i.e., protest or not) and l_i represents the observed confounding variables. Places that protested Walmart's proposals received the weight $sw_i^T = \dfrac{\frac{1}{n}(\sum\limits_{i=1}^{n} a_i)}{p_{i11}}$,

where P_{i11} is the predicted probability of place i to protest if Walmart proposed to open a store. The numerator is the sample proportion of places that actually protested. Similarly, places that did not protest received the weight

$$sw_i^C = \frac{1 - \frac{1}{n}(\sum\limits_{i=1}^{n} a_i)}{p_{i01}}, \text{ where } P_{i01} \text{ is the predicted}$$

probability of place *i* not to protest if Walmart proposed to open a store. In this way, the IPTW method simultaneously counterbalances the estimation bias caused by Walmart's selection of places to propose new stores and activists' choices to protest. We calculate probability of the incidence of protests from a Heckman two-stage selection model that estimates the incidence of protests using our independent and control variables as predictors.

Independent Variables

RTW place is a dichotomous variable coded 1 if a place is located in an RTW state (recall that all places in our sample are on contrast borders). Hypothesis 1 predicts that RTW places should be more likely to receive a proposal from Walmart to open a new store. Hypothesis 2 predicts that Walmart is more likely to overcome protesters in pro-business border places, so we created an interaction term between protest and the RTW place indicator. We expect the interaction term will have a positive effect on the opening of Walmart stores.

Control Variables

For the analysis of each dependent variable, we include a set of control variables. First, we include control variables about community characteristics. We control for population size, unemployment rate, income per capita, and percentage of urban population in a place. We also control for race homogeneity of a place, which we measure by a Herfindahl index for

each place *i*: $\sum_i \left(\dfrac{population_{ij}}{population_i} \right)^2$, where *j* rep-

resents any of the following six race groups, White, Black, Hispanic, Asian, Native Indian, or other. We collected these data from the 2000 Population Census. In addition, we include an indicator of location in the Midwest to control for the fact that most RTW contrast border places are located in the Midwest.

Second, we include control variables related to a place's political ideology and local

government. We measure liberal ideology using a place's pro-Democrat political orientation, which we calculate as the county-level[7] vote margins of individuals supporting a Democratic presidential candidate over those supporting a Republican candidate during the nearest past presidential election. We collected data for county-level presidential election results from 1996 to 2004 from *U.S. News and World Report*. We control for hazard of institutional escalation with a dichotomous variable that indicates whether a locality in a state had legislation in force in the prior year that restrains store size. We collected data about municipal-level store size legislation from the Institute for Local Self-Reliance. To control for the effect that prior protests may figure into Walmart's decisions, we measure the effect of prior protest in nearby communities by including the geographic distance weighted count of prior protests. We created a variable to indicate local governments' financial health, government's debt per capita, measured by a county government's total outstanding debt divided by the county's population. We collected the data from the Census of Government in 1997 and 2002. Finally, to control for business incentive policies at the state level (Jenkins, Leicht, and Wendt 2006), we include the number of tax and financial incentive policies adopted by each state. We obtained the data from the *State Business Incentives* report (2nd edition) published by the Council of State Governments.

Third, we add control variables related to the organization of mobilization. We control for union density measured by the percentage of workers who are union members in a state's private sector in the previous year. We obtained union data from the Current Population Survey. We control for number of churches per capita in a county in 2000, collected from the Association of Religion Data Archives. We also insert a dichotomous variable that indicates if a place was enrolled with the Main Street Program in a year. The Main Street Program is a national nonprofit organization that organizes community-based training, guidance, and support to revitalize a town's traditional commercial district. The

National Trust developed the program in the late 1970s, and it has since enrolled more than 1,200 communities in 35 states. We obtained data about the Main Street Program's local branches from its membership directories and the state-level Main Street Program offices.

Fourth, we include control variables related to the potential profitability of a proposed store. We include a variable that measures a place's distance to the closest Walmart distribution center. We also control for number of Walmart stores within 50 miles. These two variables are good indicators of profitability because distance to the closest distribution center captures efficiencies of distribution, while the number of existing stores within 50 miles measures threat of cannibalization (Holmes 2009).

Fifth, we add control variables about a place's retail economy. We measure percentage of the civil labor force employed in the retail sector using data from the 2000 Census. We control for state-level count of stores affiliated with Walmart's two major competitors, Target and K-Mart, lagged by one year. We collected data from Target and K-Mart's annual reports, as well as K-Mart store closing lists before and after its bankruptcy. We control for whether a state has favorable tax transfer for retailers by inserting a dichotomous variable to indicate if a state has nonceiling sales tax compensation for retailers. In the United States, 26 states provide compensation for retailers for collecting sales tax, and the 13 states without a ceiling (i.e., compensation is proportional to sales tax collected) provide more generous compensation for large retailers (Mattera and McIlvaine 2008). Walmart is regarded as a particular beneficiary of the non-ceiling policy (Mattera and McIlvaine 2008). Finally, we also include rate of retail sales tax for each state to control for the fact that Walmart may locate at the border places of a state with a low tax rate to attract shoppers from a neighboring state with a high tax rate.

Sixth, we create control variables related to the effect of media. Social movement researchers note that specialized gatekeepers such as media or editors select some messages that can evoke reactions from others. These scholars argue that such resonant messages become relevant, prominent, and speed diffusion of a social movement (Koopmans and Olzak 2004). We thus control for the influence of media's attention on anti-Walmart protests using two variables: the annual count of editorials with Walmart as a key word, lagged by one year,[8] and the annual percentage of editorials that present an unfavorable attitude about Walmart. We collected data from the America's News database.

Finally, we inserted a time trend, in case the incidence of store proposals and openings increased or decreased during this period.

RESULTS

Table 1 presents the results of Walmart's proposals. Models 1 and 2 include the multiple-paired sample. Model 1 includes control variables only, and Model 2 reports the main effect of RTW places. We see that Walmart is more likely to propose new stores in pro-business bordering areas ($b = .230$, $p < .01$), which supports Hypothesis 1 (i.e., Walmart will engage in regulatory arbitrage). Similarly, Models 3 and 4 present analysis of the unique-paired sample. Model 3 includes control variables only, and Model 4 reports the main effect of RTW states and shows that the positive effect of RTW places remains robust ($b = .941$, $p < .05$). The fact that Walmart's proposal behavior increases abruptly when crossing the border from an anti-business to a pro-business state suggests that Walmart does strategically respond to RTW laws.

Table 2 presents an analysis of Walmart's store openings. Model 5 tests the main effect of protest and shows that protests were very effective in reducing the probability of Walmart opening stores in border places ($b = -2.221$, $p < .01$). In addition, Model 5 shows that the average opening rate of Walmart stores was significantly higher in RTW states ($b = .584$, $p < .01$) before considering protests.

Model 6 shows that on the RTW side of a contested border, protests are less likely to

Table 1. Probit Analysis of Walmart's Proposals

	Model 1	Model 2	Model 3	Model 4
Population	.501**	.506**	.608**	.636**
	(.008)	(.008)	(.144)	(.148)
Distance to distribution center	−.008**	−.007**	−.014	−.009
	(.001)	(.001)	(.011)	(.011)
Walmart within 50 miles	−.009**	−.005**	−.018	.001
	(.001)	(.001)	(.016)	(.018)
Unemployment percent	−.300	−.076	.205	1.180
	(.186)	(.187)	(2.722)	(2.840)
Income per capita	−.017**	−.018**	−.032	−.036
	(.001)	(.001)	(.032)	(.037)
Urban percent	1.102**	1.108**	1.069**	1.115**
	(.019)	(.020)	(.334)	(.355)
Retail worker percent	1.756**	1.790**	2.857	3.273*
	(.104)	(.104)	(1.667)	(1.598)
Walmart's competitors	−.349**	−.304**	−.272	−.165
	(.019)	(.020)	(.228)	(.195)
Union member percent	−.339	2.254**	−.158	8.131
	(.216)	(.324)	(2.994)	(4.397)
Church per capita	−1.166**	−.968**	−1.899	−.663
	(.085)	(.087)	(1.341)	(1.450)
Debt per capita	.024**	.024**	.019	.014
	(.002)	(.002)	(.037)	(.045)
Pro-Democrat	−.905**	−.950**	−.814	−1.024
	(.030)	(.030)	(.595)	(.644)
Race homogeneity	−.225**	−.222**	−.544	−.923
	(.037)	(.037)	(.720)	(.798)
Main Street Program	.260**	.267**	.253	.360
	(.019)	(.019)	(.346)	(.366)
Political hazard	−.265**	−.230**	−.003	.325
	(.017)	(.017)	(.338)	(.385)
Year	.054**	.053**	.051	.071
	(.006)	(.006)	(.120)	(.122)
Total editorial	−.001**	−.001**	−.001	−.000
	(.000)	(.000)	(.002)	(.002)
Unfavorable editorial percent	.878**	.919**	1.917	2.143
	(.086)	(.086)	(1.698)	(1.736)
Retailer compensation	.422**	.382**	.237	.122
	(.021)	(.021)	(.293)	(.290)
Retail sales tax	.084**	.066**	.261	.261
	(.010)	(.010)	(.201)	(.204)
Midwest	.184**	.113**	.363	.235
	(.017)	(.018)	(.297)	(.310)
Prior protest	.060*	.037	−.157	−.327
	(.025)	(.026)	(.689)	(.720)
Business incentive	.014**	.015**	.037	.041
	(.002)	(.002)	(.036)	(.038)
RTW state		.230**		.941*
		(.022)		(.385)

Table 1. *(continued)*

	Model 1	Model 2	Model 3	Model 4
Constant	−112.612**	−110.309**	−107.974	−149.004
	(11.931)	(11.921)	(239.851)	(245.343)
N	1,280,018	1,280,018	5,271	5,271
Log lik.	−3.42e+04	−3.42e+04	−93.516	−90.216
Chi-squared	18542.945	18656.229	86.954	93.553

Note: Sample used in estimating Models 1 and 2 includes the multiple paired places with distance less than 50 miles on both sides of contrast state borders of the RTW law. Sample used in estimating Models 3 and 4 includes the uniquely paired places located within 25 miles of both sides of contrast state borders of the RTW law. We also adopted a probit model with fixed-effects at pair level and found the positive effect of RTW border place remains robust. Standard errors in parentheses.
*$p < .05$; **$p < .01$ (one-side test for hypothesized variables and two-side test for control variables).

dissuade Walmart from opening a store ($b = 1.228$, $p < .01$). To further demonstrate the meaning of this interaction effect, we calculated the predicted probability of Walmart opening stores under four conditions: (1) in an RTW state with protest, (2) in a non-RTW state with protest, (3) in an RTW state without protest, and (4) in a non-RTW state without protest.

As Figure 2 shows, when other variables are set at their means, the probability of opening a protested store is reduced from 33 to 6 percent when moving from an RTW to a non-RTW state, while the chances of opening non-protested stores are roughly the same in RTW and non-RTW states (92 versus 94 percent). Figure 2 clearly demonstrates that the effectiveness of protest is the major driving force motivating Walmart to engage in regulatory arbitrage. These results support Hypothesis 2, that protests are less effective in unfavorable political environments. The fact that Walmart's rate of overcoming protesters increases abruptly when crossing the border from an anti-business to a pro-business state also suggests that protesters are systematically weaker on the RTW side of a border.

Because the sample size for unique pairs is too small for an analysis of store openings, we present only the results for multiple pair matching. Tables S3a and S3b in the online supplement, however, report results using the unpaired full sample. Table S3a shows that Walmart made more store proposals in RTW

border places ($b = .379$, $p < .01$, Model 2). Table S3b shows that Walmart is more likely to open stores despite protests on the borders of RTW states ($b = 1.454$, $p < .05$, Model 5). Together, these results provide strong support for our hypotheses.

Does Walmart Exploit Regulatory Variations in Minimum Wage or Fair Employment Laws?

While we argue that Walmart locates in pro-business states as indicated by RTW laws, something else about these states could serve as the attraction. Two possibilities are that Walmart exploits variations in minimum wage laws and fair employment laws due to cost considerations. It is also possible that such laws increase activists' effectiveness by allowing them to make social justice claims more persuasively. If this were true, it would be consistent with our overall arguments regarding triadic regulatory arbitrage, but it would indicate that something other than RTW laws are the marker of an arbitrage opportunity.

We obtained minimum wage data for each state between 1998 and 2005 from the U.S. Department of Labor. We defined contrast borders as those between states with different levels of minimum wage. We selected all places within 25 miles of contrast borders and conducted analyses similar to those for RTW laws. In addition, we controlled for the

Table 2. IPTW Probit Model of Store Openings

	Model 5	Model 6
Population	.056**	.029
	(.020)	(.020)
Distance to distribution center	−.001	−.003
	(.004)	(.004)
Walmart within 50 miles	−.007	−.008
	(.007)	(.007)
Unemployment percent	5.948**	4.555**
	(1.213)	(1.260)
Income per capita	.058**	.063**
	(.006)	(.006)
Urban percent	−.327*	−.580**
	(.156)	(.187)
Retail worker percent	−2.254*	−5.526**
	(.979)	(.995)
Walmart's competitors	.180	.158
	(.103)	(.095)
Union member percent	9.013**	8.560**
	(2.080)	(2.067)
Church per capita	−2.619**	−3.163**
	(.484)	(.488)
Debt per capita	.004	.033**
	(.005)	(.006)
Pro-Democrat	−.260	−.475**
	(.154)	(.160)
Race homogeneity	.330	.532**
	(.194)	(.205)
Main Street Program	.184**	.276**
	(.058)	(.063)
Political hazard	.546**	.531**
	(.079)	(.086)
Year	.363**	.419**
	(.032)	(.034)
Total editorial	.001*	.001*
	(.000)	(.001)
Unfavorable editorial percent	5.737**	5.968**
	(.414)	(.408)
Retailer compensation	.019	−.047
	(.113)	(.109)
Retail sales tax	−.233**	−.321**
	(.064)	(.063)
Midwest	−.211*	−.335**
	(.100)	(.098)
Prior protest	−2.670**	−3.216**
	(.159)	(.205)
Business incentive	.141**	.142**
	(.009)	(.010)
Protest	−2.221**	−3.097**
	(.054)	(.096)

(continued)

Table 2. *(continued)*

	Model 5	Model 6
RTW place	.584**	−.113
	(.118)	(.135)
Protest x RTW place		1.228**
		(.111)
Constant	−730.829**	−841.803**
	(65.111)	(68.528)
N	7,007	7,007
Log lik.	−2454.620	−2398.907

Note: Sample includes the multiple paired places with distance less than 50 miles on both sides of contrast state borders of an RTW law. The sample size for uniquely paired places is too small (with 21 proposals) to support an IPTW regression. Standard errors in parentheses.
*$p < .05$; **$p < .01$ (one-side test for hypothesized variables and two-side test for control variables).
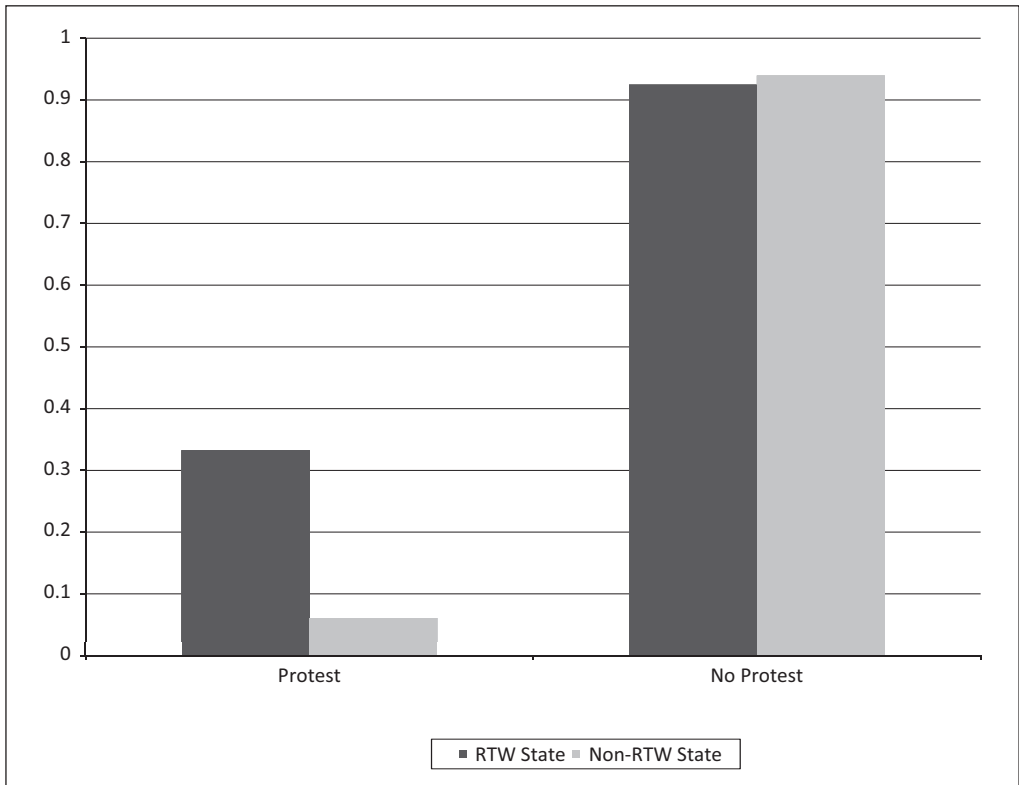
Figure 2. Probability of Store Openings in RTW and Non-RTW States

number of cities that have adopted living wage ordinances, an acute version of minimum wage, in a state in the previous year using data reported by the Living Wage Resource Center. As reported in Tables S4a and S4b in the online supplement, we find evidence in line with our theory of triadic regulatory arbitrage: high-minimum-wage states have a negative coefficient for Walmart proposals and for the efficacy of protests, but the effects are not statistically significant.

For fair employment, we defined contrast state borders using two sets of criteria. First, we defined contrast borders as those occurring between a state without extensive protection beyond federal law and a state with at least one protected category not included in federal law. Second, we defined contrast borders as those between a state without extensive protection beyond federal law and a state with at least three protected categories not included in federal law. The second definition captures a subgroup of states defined by the first, representing borders with stronger contrast. Similar to other analyses, we included all places within 25 miles of these contrast borders. As Tables S5a and S5b show, although we find directional effects consistent with the prediction of triadic regulatory arbitrage—Walmart was less likely to propose and to open stores when protested in states with more pro-labor protection—these effects are not statistically significant.

In unreported analyses of paired down models with fewer control variables, we do find statistically significant evidence that Walmart is significantly less likely to propose new stores in states with more labor protection and that community protesters are more effective in dissuading Walmart in these states. These results indicate another form of arbitrage, by which Walmart exploits state differences to disadvantage their activist rivals. While this is fully in line with our core theoretical claims, the relatively weak effects in these additional analyses for minimum wage and fair employment laws leave us with the conclusion that RTW laws are the best indicator of Walmart's regulatory arbitrage opportunities.

Other Robustness Checks

We conducted a number of other robustness tests but do not report the results for the sake of brevity; we present a summary here. We expanded our sample to not only compare RTW and non-RTW states across contrast borders, but also across regular borders. We created a sample of places located within 25 miles to any type of state border in all U.S. continental states. We ended up with four types of border places: places in an RTW state facing another RTW state, places in an RTW state facing a non-RTW state, places in a non-RTW state facing another non-RTW state, and places in a non-RTW state facing an RTW state. As expected, Walmart is more likely to propose new stores, and to open stores despite protests, in RTW states with contrast borders.

In another unreported analysis, we extended the border place sample to include all places within 50 miles from contrast borders, and we defined places within RTW states as having positive distances to contrast borders and places within non-RTW states as having negative distances to contrast borders. Our analyses suggest that Walmart is more likely to issue proposals for new stores in the interior land of non-RTW states than in their border areas. Furthermore, protesters are less likely to dissuade Walmart from opening a store in the interior area of non-RTW states than in their border areas. Protesters are more likely, however, to dissuade Walmart from opening a store in the bordering area of RTW states than in their interior land. This is consistent with our view that contrast borders present opportunities for arbitrage for large retailers. As Walmart considers places farther into the interior of a non-RTW state, the possibility of locating on the border in an adjacent RTW state becomes less feasible.

DISCUSSION AND CONCLUSIONS

Thompson (1967) suggests that exchange agreements hinge upon prior consensus regarding the domain of an organization—that is, a set of expectations about what the organization

will and will not do. Fligstein (2001) observes that market building is a political project. There has been little discussion, however, as to how the economic geography of organizations is shaped by both regulatory variations and protests launched by activists. Indeed, since Chandler (1977), organizations' geographic spread has been depicted mainly as an outcome of internal capabilities and portrayed as an exercise in replication. Our work suggests that this replication is anything but automatic and involves regulatory arbitrage by firms in a bid to overcome protests.

To unpack domain consensus it is useful to imagine a triad, with the state at the top and corporations and activists on the base. One of the dyadic links, that between activists and the state, is already the focus of extensive attention in the social movement literature. Our findings extend the understanding of the other two links: states and corporations, and corporations and activists.

State–corporate link. The state's influence on the form and survival of corporations and other organizations is well documented in institutional and organizational ecology literature (e.g., Dobbin 2009; Fligstein 2001; Hannan, Polos, and Carroll 2007). Our contribution concerns the potential of influence to flow the other way, from corporations to the state. It is well known that states' autonomy may be compromised by organizations (see Hacker and Pierson 2002), but, to date, state autonomy has been characterized as a function of relative size and capacity. State autonomy is seen as threatened only in the case of a very powerful organization or a particularly weak state (Simons and Ingram 2003; Strange 1996). Yet, just as market competition can empower a lone consumer facing a powerful corporation, so too can jurisdictional competition shift the power between states and their (potential) subjects, even if the subjects are small and weak relative to the state.

To date, the idea of jurisdictional competition spurred by regulatory arbitrage has been more convincing in theory than in evidence. Tiebout's (1956) model predicts an equilibrium—states should reach a regulatory

convergence driven by the pressure of competition, and citizens should be sorted into communities that maximize their personal utility. But this equilibrium thesis faces two basic challenges. First, we do not know much about how the sorting process happens—that is, how individuals, activists, and firms match with jurisdictions. By examining the strategic location decisions of a single firm, we are able to document regulatory arbitrage, providing mechanism-level evidence consistent with the aggregate differences in employment and wages that are documented in past comparisons of RTW and non-RTW states. The spatial discontinuity design we employed presents a powerful identification strategy that will likely have broad applications in sociological research on the influence of regulations on firm behavior.

A second challenge is that despite regulatory arbitrage, regulatory convergence does not happen as neatly in reality as the theory would predict (Carruthers and Lamoreaux 2009). Here, we may return to the idea of a triad to understand why corporate power to choose regulatory jurisdictions may not result in quick regulatory convergence. Our findings suggest that the sorting of firms across jurisdictions does not occur in a dyadic contest between firms choosing from a menu of regulatory variations. Instead, there is also interaction between firms and their rivals—in our case, social movement activists. The presence of social movement activists may tip the balance of power between states and corporations. Activists may also cloud state policymakers' understanding as to why corporations locate where they do.

In our analysis, Walmart's increasing prevalence on the RTW side of contrast borders emerges from two processes: Walmart's increased likelihood of proposing stores on the RTW side, and the greater likelihood that protests will dissuade Walmart from opening stores on the non-RTW side. In the former case, protests are a weak signal of voter and community preferences for greater regulation—the very fact that legislators have revealed their pro-business preferences implies that protests are not a credible signal of future regulatory costs. In the latter

case, protests are a strong signal that communities and legislators may impose even more stringent regulations. Thus, the effectiveness of movement demands is moderated by variations in regulatory regimes. Wallace (2007) describes labor laws as state administered truces; we argue that RTW law embodies a pro-business settlement in a state, which is why Walmart seeks to exploit such legal variation.

Our results also shed light on the interdependence of public and private regulation. In their review, Schneiberg and Bartley (2008:551) observe that "hard" laws are being supplemented by "soft" laws (i.e., private certification, rating systems, and information disclosure rules), and they urge researchers to study how "multiple forms of regulation intersect, raising questions about the extent to which they undermine or reinforce each other." Our study shows that when explicit deregulation in one domain is a credible signal of the pro-business bent of legislators and voters in a state, the effectiveness of protests as a signal of future regulatory costs is muted. By contrast, in non-RTW states, protests do signal future regulatory costs; so protests matter more in more regulated states. The prevalent view in law and society circles is that social movement activists can play a key role in the hard enforcement of soft laws; our study, however, shows there is far greater interdependence between public and private regulation.

While our study analyzes regulation in a federal context, its implications for understanding state–corporate interaction go beyond the boundaries of a nation-state. The idea that corporations choose the location of their incorporation on the basis of tax and governance regulations is familiar (Bebchuk and Cohen 2003). In the wake of the current financial crisis, some commentators have offered the specter of regulatory arbitrage as a reason for caution when imposing new regulations, as when politicians claim that poorly conceived U.S. regulations would drive the finance industry to London or Hong Kong (Kenny 2010). Activists further complicate the picture internationally (e.g., global opponents of genetically modified food).

Future work should thus look at the interaction of corporations, states, and activists globally, and document the resulting effects on the distribution of economic activity and on regulation. Similarly, the triad we examine of states, corporations, and political rivals could be expanded to include industry competitors.

Corporate–activist link. While social movement literature has been criticized for over-emphasizing the state as a target of protest, recent efforts have rebalanced the consideration of how movements and activists affect other actors, particularly corporations (e.g., King 2008; King and Soule 2007; Soule 2009). Still, it is worth recalling the triad we began with, which suggests that the state looms over interactions between activists and corporations. Our findings suggest that large firms and activists strategically interact with each other against the background of regulatory variation. Walmart seeks to locate proposals for new stores such that it disadvantages a decentralized rival: anti-Walmart activists. Walmart thus offers proposals in the borders of states with a pro-business ideology proxied by RTW laws; even if protests occur at the borders of RTW states, Walmart is able to overcome them. This suggests that organizations may have a larger portfolio of responses to protest than simply accede or resist. Future research should consider that corporations may also choose jurisdictions to disadvantage political rivals.

Although we did not hypothesize about whether protests are less likely at the borders of RTW states, it is worth pointing out that we do not observe large differences in the incidence of protests across contrast borders (31 percent of proposals on the RTW side were protested, versus 38 percent on the non-RTW side). One implication is that activists are less responsive than Walmart to arbitrage opportunities presented by laws, perhaps because the anti-Walmart movement is localized in nature and lacks national coordination. Local activists may have neither the incentive nor the ability to engage in arbitrage. The other implication is that Walmart cannot predict protest location with accuracy (Ingram et al. 2010).

Walmart must therefore locate in places where it has natural allies: pro-business officials, legislators, and the public. In these places, even if protests arise in the borders of RTW states, Walmart is unlikely to be dissuaded and can open a store.

Indeed, our findings point to the unintended impact of movements. Giugni (1998) observes that social movements may fail to achieve activists' goals but can still exert secondary and indirect effects. Our study shows that regulatory arbitrage is an indirect effect of protests targeting private corporations. Although a venerable line of work suggests that regulatory policies constitute an important influence on firms (DiMaggio and Powell 1983; Fligstein 2001), there have been repeated calls for the study of how firms can respond to coercive regulatory pressures through manipulation (Oliver and Holzinger 2008). Our study speaks to such calls by showing how regulatory arbitrage is one way in which firms exploit institutional pressures. However, as our study demonstrates, regulatory variation in laws constitutes an element of corporate opportunity structure for the target firm and disadvantages activists. We enlarge the concept of corporate opportunity structure such that it works for targets and not just activists or protesters, and we describe firms' location strategies as an important response to anti-business activism.

In our study, Walmart engaged in regulatory arbitrage, but activists did not. Future research needs to investigate whether more coordinated activists exploit regulatory variations in tandem with their opponents. Should this happen, one might expect protests to be rare, because large firms would only locate where protest is unlikely, and protesters would only protest where they are likely to succeed. Yet, there are many departures from such a model of full information, and future research should investigate how the organizational structure of large firms and activist groups influences the perception of opportunity, decision making, and ultimately the process of regulatory arbitrage.

Notes

1. Oklahoma enacted an RTW law during our observation period in 2001. This legislation change decreases the number of contrast border places from 3,175 to 2,732 (with 447 places dropped and four places added). Thus, the total number of observations in our sample should be 23,185 (3 x 3,175 + 5 x 2,732). Due to missing values, the number of observations used in our sample varies slightly from this total. Figure 1 reflects these numbers.

2. The 1962 to 2005 part of this list was published by Walmart Inc. on its website and then removed. We thank Panle Jia for sharing the data with us. This data set can be downloaded from http://www.econ.umn.edu/~holmes/data/WalMart/index.html, accessed on March 13, 2010. We obtained store openings for 2006 and 2007 from Walmart's official website.

3. Sprawl-Busters has been collecting information on anti–big-box store protests from a variety of sources, including media reports, government information releases, court results, independent institutions' research reports, and activists' self-reports. We were not concerned that Sprawl-Busters would attempt to inflate the perceived efficacy of anti-Walmart efforts by omitting reference to protests that failed to stop stores because they report protests as they happen, before it is known whether a protest will succeed in stopping the store opening.

4. We started our observation in 1998 because one of our data sources (the Sprawl-Busters database of protests) began to collect data on Walmart's proposals and protests from 1998 onward. We ended in 2005 because we need a time interval of at least two years to determine whether a proposed store opened.

5. Fixed-effect models are often used to accommodate individual heterogeneity in panel data, but they are inappropriate here due to the structure of our data. Most places in our sample did not experience proposals during the observation period. The fixed-effect model cannot utilize these observations and thus results in significant bias. In addition, adding fixed-effects at the state level remains unfeasible because RTW laws are measured at the state level and remain largely unchanged during our observation period. Random-effect models produce results similar to those of pooled cross-sectional models.

6. Stabilized weighting enhances the efficiency of estimation.

7. For a small number of places that are located in parts of multiple counties, we calculated the place-level

variable as the mean of the county-level variables of all the counties in which the place is located.

8. We use editorials rather than the total number of newspaper reports because editorials reflect media's attitude and are less likely to be a function of ongoing protests.

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