LAWS OF ATTRACTION: REGULATORY ARBITRAGE IN THE FACE OF ACTIVISM IN RIGHT-TO-WORK STATES

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Extant 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 the political rivals of a firm 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 which signal the pro-business climate in a state and exist in twenty-two of the 50 American states. Using a spatial-regression discontinuity design, we analyze how Wal-Mart locates new stores in the face of anti-Wal-Mart activists and exploits regulatory discontinuities on the borders between RTW and non-RTW states. We find that Wal-Mart is more likely to propose new stores at the borders of RTW states, and to open those stores if they are protested, compared to the borders of neighboring non-RTW states. We discuss implications for the study of regulation, social movements, and organizations.

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

As nation-states are being weakened, corporations are increasingly becoming a target of activists (see King and Pierce, 2010). However, corporations are more vulnerable to the threat of delegitimation than states, but lack the weapons of the state to repress, routinize, or channel protests by activists (Walker, Martin and McCarthy, 2008). Be that as it may, corporations have wider 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 there are a few studies of how firms exploit regulatory variations when locating operations (e.g. Holmes, 1998; Dube, Lester, and Eidlin, 2008), 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 needs to 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 operation. Indeed, formal control of the state

and more diffuse forms of social control are often substitutes for each other (Simons and Ingram, 1997; Schneiberg and Bartley, 2001; Weber, Thomas and Rao, 2009; King, 2009; Soule, 2009). This substitutability may create arbitrage opportunities. For example, Cowie (1999) showed how RCA moved operations from Camden to other cities as the risk of unionization increased.

These considerations supply the motivation for us to study the role of social activism in Wal-Mart's location decisions between states with RTW laws and those without. Why do we expect a firm such as Wal-Mart engages in regulatory arbitrage due to RTW laws? We do not think the reason is the threat of unionization. Wal-Mart 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 risks of protests that seek to establish restrictive regulations on large firms and thereby constitute an element of the political opportunity structure for both large firms, and activists (McAdam, McCarthy and Zald, 1988). We expect proposals from Wal-Mart to open establishments to abruptly increase when we cross the borders of an RTW state. We expect Wal-Mart is more likely to open a store on the border of an RTW state even when it faces protests. In an earlier study, Ingram, Yue and Rao (2010) showed that Wal-Mart treated protests as signals of subsequent regulatory costs, and therefore, chose to walk 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 information because Wal-Mart is unlikely to believe that the community will be anti-business since legislators and voters have already revealed that they are pro-business. In short, we contend that regulatory arbitrage plays a role in mediating the efficacy of protest (Amenta, 2005), and predict that large organizations such as Wal-Mart commence operations despite protests when they cross the border of RTW states in comparison to non-RTW states.

We focus on Wal-Mart for three reasons. First, it is arguably the most consequential firm in the American economy, whose decisions are of interest to economic sociologists and political sociologists alike. Second, its proposals to open new stores often encounter protests. Since 1988, Wal-Mart began to open supercenters – stores with 150,000-250,000 square feet of space that had a grocery section and offered a wide array of products. In general, Wal-Mart's entry leads to a 3% overall price declines in competing stores, and in the case of some items, the declines are as high as 13% (Basker, 2005; Hausman and Leibtag, 2005). In view of their impact on the local retail trade

and the increase in congestion and traffic, Wal-Mart store opening proposals often evoke protests from activists seeking to preserve Main Street or driven by not in my backyard (NIMBY) motivations. Typically, protesters seek to establish stringent size limits on the size of new stores to insulate towns against the entry of Big-Box stores or retailers such as Wal-Mart, Home Depot, and Target. Indeed, Forbes magazine identified activists leading protests as Wal-Mart's principal enemy. In view of these protests, Wal-Mart is likely to have 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, POLTICAL 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 National Banking Act that imposed 10% tax on the banking notes issued by state banks was passed in 1863, American 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 can shift poorly monitored risk exposures to taxpayers in a different country through cross-border mergers (Carbo, Kane and Rodriguez, 2008).

By contrast, a simpler form of regulatory arbitrage across geographic borders occurs when firms shift geographical location in response to legislation.

Legal and regulatory variations across states provide large business firms political opportunities to disadvantage rivals such as activists. However, much of the discussion of political opportunity structure has been from the vantage point of activists challenging state authorities (See (McAdam, Tarrow and Tilly, 2003; Meyer, 2004). Some students suggest that one ought to think of how political opportunity influences policy outcomes and not just the mobilization of activists. 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: 519-520). 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, the ease of participation in the political system, the existence of patronage politics, the availability of support from bureaucrats, and most of all, whether the regime is open to claims determine the effectiveness of activist mobilization (Amenta, Carruthers and Zyland, 1992; and Amenta, Dunleavy and Bernstein 1994).

In the case of private politics, activists are battling large corporations (Baron and Diermeier, 2007), and as a result, one ought to emphasize *corporate opportunity structure* to understand political mediation (King, 2008; Soule, 2009). Nonetheless, studies such as King (2008) or 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 for activists. We seek to 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; See also Soule, 2009:43-45). We argue that corporations do more than that – they pay great attention to how legal and regulatory variability magnifies or reduces the effectiveness of an activist, and accordingly make location choices. We suggest that corporate targets are concerned about laws that signal a pro-business climate and restrict the ability of activists to put restrictive regulations in place. Below, we focus on Wal-Mart and how it sees RTW laws as undermining the effectiveness of activists and so locates stores right across the borders of RTW states.

The 1935 Wagner Act enabled union organizing and identified unfair labor practices that could not be used by the managements of firms. 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 gave the employees the benefits of the union contract. RTW laws were passed by twenty two states, mostly in the south, with Oklahoma in 2001 being the last state to enact such a law (Reed, 2003). A number of studies show that RTW laws had a negligible effect on wages (See Moore, 1998 for a review) but a recent study found a 2% advantage in wages for RTW states (Greer, 2004). There is some evidence that RTW laws reduce union membership (Ellwood and Fine, 1987; Davis and Huston, 1993) and induce unions to abandon organizing drives (Ferguson, 2008). Since only 22 states enacted RTW laws, substantial heterogeneity remains, thereby creating an opportunity for regulatory arbitrage.

Why would a firm such as Wal-Mart engage in regulatory arbitrage on account of RTW laws even when it has not faced a serious threat of unionism? 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, but since then have become a broad-based signal of pro-business ideology in the state. 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." So much so that states routinely market themselves as pro-business by proclaiming that they have an RTW law – it telegraphs the ideology of the state. In the 2010 CEO survey of best and worst states for businesses, nine of the best 10 states had an RTW law, and none of the worst 10 states did (CheifExecutive.net, 2010).

For organizations such as Wal-Mart, an RTW law signals that protests might be hard to organize in a given state. Even if a protest were organized by anti-Wal-Mart protesters, be they NIMBY activists or small businessmen concerned about Main Street, they would find it hard to gain the support of legislators, governmental authorities, voters, or consumers. For large organizations such as Wal-Mart, an RTW law signals that regulatory restrictions on the sizes of their stores are unlikely to be implemented. The "nuclear option" of regulatory responses against Wal-Mart is a size-cap restriction, which limits the size of retail stores in the municipality to preclude big box retailers. Often, those who protest against Wal-Mart stores in the name of protecting Main Street business and reducing urban sprawl seek to mobilize popular support for a size-cap regulation which limits the footprint of a store to 30,000 square feet or less, thereby rendering the economics unviable for Wal-Mart which typically seeks to establish superstores with 150,000-200,000 square foot floor plans. As of 2005, about 23% of the RTW states had some incidence of local size-cap legislation while about 56% of the non-RTW states had such laws.

Wal-Mart'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 Wal-Mart's base. Given the gravity of a Wal-Mart, it is possible to reach the same customer from any one of a number

of potential locations. If Wal-Mart 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 is some resemblance to the bargaining over drilling rights between land owners and oilmen depicted in Wes Anderson's feature film "There Will Be Blood." Wal-Mart in this analogy, 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) provided a good example of Wal-Mart's arbitrage at state borders. In that case, Wal-Mart built stores on the New Hampshire border and on the New York border to suck the trade out of Vermont, a state that implemented hostile policies toward Wal-Mart 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, and signals the tastes of voters, elected legislators, and regulators. Abraham and Voos (2000) reported that stockholder wealth rose when Louisiana enacted an RTW in 1976 and when Idaho did so in 1985-1986, presumably, because investors anticipated higher future profits with weaker unions, and lower probabilities of restrictive regulations. Stevans (2009) also found 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 arguments, the presence of RTW laws would be a signal of favorable opportunity and would increase proposals by Wal-Mart to open new stores. Therefore:

H1) Wal-Mart is <u>more</u> likely to issue proposals at the borders of RTW states than it is in comparable places in neighboring non-RTW states.

Even if there are protests against stores proposed in the border area of an RTW state, Wal-Mart is likely to open the store because the pro-business climate implies support from elected officeholders and bureaucrats. Amenta, Carruthers and Zyland (1992) and Amenta, Dunleavy and Bernstein (1994) suggest that the effectiveness of protests 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 to be weakened. Moreover, regimes that are partisan and undermine the social movement 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) found that political context mediated the effectiveness of consumer boycotts directed against private firms.

Ingram, Yue and Rao (2010) observed that Wal-Mart often accedes to protests against proposed stores, and argued 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 serves to make Wal-Mart more confident of maintaining or gaining the support of elected officeholders, regulators, and voters. Similarly, in places with a pro-business climate, anti-Wal-Mart protests may be less likely to make dent on customer patronage. Ingram et al. (2010) also argued that acceding to protests is relatively cheap for Wal-Mart, because they can typically find another location of comparable business value nearby. That 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:

H2) Wal-Mart is <u>more</u> 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 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 geographical 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 to establish a causal relationship if an abrupt change can be observed across borders. For example, Holmes (1998) compared places within 25 miles of the border of an RTW state with their 'twins' – places within 25 miles of the border of an adjacent non-RTW state. The strengths of the design are that a) geographic characteristics tend to be similar on both sides of the border; b) the high cost of moving far away from the border makes the cutoff meaningful; c) the design helps avoid ecological fallacy by localizing estimates; and d) 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, the manipulation of the treatment variable (RTW laws) occurred in the treatment area before the measurement of the outcomes: proposals to locate Wal-Mart stores and whether stores were opened despite protests. Since the geographical conditions are approximately the same on both sides of borders, what differs is the effect of state policies. To the extent that the pro-business policies pursued by the RTW states have resulted in regulatory arbitrage, there should be an abrupt change in Wal-Mart's behaviors.

Our dataset consists of the places that are 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. States with contrast borders are listed in Appendix 1. 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 U.S. in 2000. In our sample, there are 3,179 unique places that are located within 25 miles of contrast borders[1]. Places on RTW- and non-RTW side of contrast borders each constitute about 50% of our border sample. Appendix 2 lists the basic social demographic and economic characteristics of places on both sides of contrast borders. The results show that places on the both sides of the borders are largely comparable.

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

Insert Figure 1 here

A new store proposal was defined as a proposal to open a new Wal-Mart (a discount store, a supercenter, or a neighborhood market). A relocated store (i.e. moving an existing store to a new location in the same community) was not treated as a new store. We compiled the data about Wal-Mart's proposals, protest, and openings mainly from two sources. First, for the proposals that resulted in actual store openings, we obtained a list of all Wal-Mart store openings from 1962 to

[1] Oklahoma enacted the right-to-work law during our observation period in 2001. We coded Oklahoma as a non-RTW state for 1998-2000 and as a RTW state for 2001-2005. The legislation change decreases the number of contrast border places from 3175 to 2732. Thus, the total number of observations in our sample should be 23,185 (3 x 3175 + 5 x 2732). Due to missing values, the number of observations used in our sample varies slightly from this total.

2007.[2] We estimated the proposal time for each of the opened stores 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 the proposals that were aborted, we collected the data about Wal-Mart's proposal from Sprawl-Busters, an anti-Wal-Mart organization that has been documenting anti-big-box store protests from various sources since 1998[3]. From the Sprawl-Busters database, we selected all the protests that targeted Wal-Mart's store proposals from 1998 to 2005 in border places[4]. We also collected reports of Wal-Mart's proposal from other activists' websites. In addition, we conducted a media search for reports about Wal-Mart's store proposals from 1998 to 2005 using the Lexis-Nexis and the America's News databases.

From our search over the 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 Wal-Mart store: encouraged public hearings, collected citizens' signatures to initiate a referendum, demanded additional studies of Wal-Mart's impact on local businesses, traffic and environment, highlighted environmental hazards, deployed zoning restrictions, lobbied for store-size cap legislations, or filed lawsuits against Wal-Mart or local government. A protest against a proposed Wal-Mart store can be reported multiple times, and we coded the multiple reports as one protest as long as they were targeted at the same store proposal.

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

[3] Sprawl-Busters has been collecting the information of anti big-box store protests from a variety of sources, including media reports, governments' 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-Wal-Mart efforts by omitting reference to protests that failed to stop stores because they report protests as they happen, before it is known whether or not the 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 Wal-Mart's proposal and protests from 1998 onwards. We ended in 2005 because we need a time interval of at least two years to determine whether a proposed store was opened.

Finally, we matched the data of proposed stores and protests obtained from the above sources and dropped duplicated cases. For our observation period from 1998 to 2005, Wal-Mart made 1,592 proposals in the 48 continental states and Washington D.C., and 563 of these were protested. Wal-Mart managed to open 1,034 stores. Within 25 miles of contrast RTW borders, Wal-Mart made 102 proposals, out of which 34 were protested and 73 were eventually opened. The multiple sources of our data with different interests in the contention, including the representations of Wal-Mart, protestors and the media, mitigate the concern about selection bias that would loom large if we relied on only one source. Overall, 94% of proposed stores either resulted in store opening 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 the border, we are not sure they are symmetrically distributed along state borders. Put another way, the analyses of all the places will compare two 'columns' of observations on either side of the contrast border, and therefore we need to have 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 and a place can appear in the sample multiple times if there are more than one qualified pair partner on the opposite side of the border. We ended up with 88,495 pairs that resulted 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 a RTW border. Because the unique pairing by distance has to be symmetric for either partner, our sample is substantially reduced to 386 unique pairs, which result in 5,472 observations. Besides, we also report the analysis results using the sample of unpaired places in the robustness check.

Dependent Variables and Estimation

Our first dependent variable is where Wal-Mart proposed to open a store. Proposal is a dummy variable that is coded 1 if Wal-Mart proposed to open a store in a place in a year. We used a probit model to estimate the effect of the RTW laws on Wal-Mart's proposal behaviors. Furthermore, since most states didn't experience a change in the status of the RTW laws during our period of observation, the variance regarding the RTW laws is mainly cross-sectional. We therefore also reported the results of the pooled cross-sectional probit analysis[5].

Our second dependent variable is a dummy variable that indicates whether a proposed Wal-Mart store was opened. Opening is coded 1 if a proposed store was successfully opened by the end of 2007. We used a pooled probit model to estimate the effect of the RTW laws on the opening probability of proposed stores. However, we confronted a non-random assignment problem: protests are not likely to happen randomly; communities choose whether to organize protests in the first place and consider their chances of success when they do so. An added issue is that protests are conditional on a proposal from a Wal-Mart, and in turn, these proposals are also not distributed randomly.

We therefore adopted the inverse probability treatment weighting (IPTW) method that was recently developed and widely adopted by biostatisticians to resolve the nonrandom assignment problem in observational data (Robins, Herman, and Brumback, 2000; Azoulay, Ding, and Stuart, 2007). The IPTW relies on the logic of counterfactuals and compares each treated subject or observation to a pseudo-population and the difference between the two groups represents the

[5] Fixed-effect models are often used to accommodate individual heterogeneity in panel data, but are inappropriate here due to the structure of our data. Most places in our sample did not experience proposals during the period of our observation. 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 the 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.

average treatment effect. Each observation in the sample is assigned a stabilized weight[6], $sw_i = \frac{P(A = a_i)}{P(A = a_i | L = l_i)}$, where a_i = {0,1}indicates potential treatment (i.e., protest or not) and l_i represents the observed confounding variables. For those places that protested Wal-Mart's proposals, they receive the weight, $sw_i^T = \frac{\frac{1}{n}(\sum_{i=1}^{n} a_i)}{p_{i11}}$, where p_{i11} is the predicted probability of place i to protest if Wal-Mart proposed to open a store. The numerator is the sample proportion of places that actually protested. Similarly, for those places that did not protest, they receive the weight, $sw_i^C = \frac{1 - \frac{1}{n}(\sum_{i=1}^{n} a_i)}{p_{i01}}$, where p_{i01} is the predicted probability of place i not to protest if Wal-Mart proposed to open a store. In this way, the IPTW method simultaneously counterbalances the estimation bias caused by Wal-Mart's selection of a place to propose and the activists' choice to protest. We calculated the 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 dummy variable that is coded 1 if a place is located in a 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 Wal-Mart's proposal to open a new store. Hypothesis 2 predicts that Wal-Mart is more likely to overcome protestors in the pro-business border places, and we create an *interaction term between protest and the RTW place indicator*. We expect the interaction term will have a positive effect on the opening of Wal-Mart stores.

[6] Stabilized weighting enhances the efficiency of estimation.

Control Variables

For the analysis of each dependent variable, we include a list of control variables. First, we include a list of control variables about community characteristics. We controlled for *population size, unemployment rate, income per capita, the percentage of urban population* in a place. We also controlled the *race homogeneity* of a place, which is measured by a Herfindahl index for each place i: $\sum_i \left(\frac{population_{ij}}{population_i} \right)^2$, where j represents either of the following six race groups, White, Black, Hispanics, Asian, Native Indian, and others. All these data were collected from the 2000 Population Census. In addition, we included an indicator of location in the *Midwest* to control for the fact that most of RTW contrast border places are located in the Midwest.

Second, we control variables related to a place's political ideology and local government. We measured *liberal ideology* using a place's *Pro Democrat* political orientation, which is calculated as the county-level[7] vote margins of those supporting a Democrat presidential candidate over those supporting a Republican candidate during the nearest past presidential election. The data were collected from the county-level presidential election results from 1996 to 2004 reported by the U.S. News and World Report. We controlled for the *hazard of institutional escalation* by including a dummy variable that indicates whether a locality in a state had legislation in force that restrains store size in the prior year. We collected the data about the municipal-level store size legislation from the Institute for Local Self-Reliance. To control for the effect that prior protests may figure into Wal-Mart's decisions, we measured the effect of *prior protest in nearby communities* by including the geographical distance weighted count of prior protests. Moreover, we created a variable to indicate the financial health of local governments, the *government's debt per capita*, measured by the total outstanding debt of a county government divided by the county's population. The data were

[7] For a small number of places that are located in parts of multiple counties, the place-level variable is calculated as the mean of the county-level variables of all the counties that a place is located at.

collected from the Census of Government in 1997 and 2002. Finally, to control for the business incentive policies at the state level (Jenkins et al., 2006), we included the number of tax and financial incentive policies that each state adopted. The data were obtained from the *State Business Incentives* report (2nd edition) published by the Council of State Governments.

Third, we controlled variables that are related to the organization of mobilization. We controlled the *union density* measured by the percentage of workers that are union members in a state's private sector in the previous year. The union data were obtained from the Current Population Survey. We controlled the number of *churches per capita* in a county in 2000, collected from the Association of Religion Data Archives. We also included a dummy variable that indicates if a place is enrolled with the *Main Street Program* in a year. The Main Street Program is a national nonprofit organization that aims at organizing community-based training, guidance, and support to revitalize the traditional commercial district. The program was initially developed by the National Trust in the late 1970s and has since developed into a national program enrolling more than 1,200 communities in 35 states. We obtained the data about the Main Street Program's local branches from its membership directories and the state-level Main Street Program offices.

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

Fifth, we controlled a set of variables about a place's retail economy. We measured the percentage of civil labor force employed in *the retail sector* using the data from the Census of 2000. We controlled the state-level count of stores that are affiliated with *Wal-Mart's two major competitors*,

Target and K-Mart, lagged by one year. The data were collected from Target and K-Mart's annual reports as well as K-Mart store closing lists before and after its bankruptcy. We controlled whether a state has *favorable tax transfer* for retailers by controlling a dummy variable to indicate if a state has non-ceiling sales tax compensation for retailers. In the U.S. there are 26 states that provide compensation for retailers for collecting sales tax, and 13 of them without a ceiling (i.e. compensation is proportional to sales tax collected) provide more generous compensation for large retailers (Mattera and McIlvaine, 2008). Wal-Mart is regarded as a particular beneficiary of the non-ceiling policy (Mattera and McIlvaine, 2008). Finally, we also include *the rate of retail sales tax* for each state to control for the effect that Wal-Mart 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 control variables related to the effect of media. Social movement researchers noted that specialized gatekeepers such as media or editors select some messages which can evoke reactions from others, and argue that such resonant messages become relevant, prominent, and speed diffusion of a social movement (Koopmans and Olzak, 2004). So we controlled the influence of *media's attention* on anti Wal-Mart protests using two variables. One was the annual count of editorials with "Wal-Mart" as a key word, lagged by one year[8]. The other was the annual percentage of editorials that holds an unfavorable attitude about Wal-Mart. The data were collected from the America's News database.

Finally we controlled for a time trend, in case the incidence of store opening increases or decreases during the period we analyze.

RESULTS

[8] We chose to use editorials rather than the total number of newspaper reports because editorials reflects media's attitude and less likely to be a function of on-going protests.

Table 1 presents the results of Wal-Mart's proposal. 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. The result shows that Wal-Mart is more likely to propose in the pro-business bordering areas (b=0.230, p<.01), which supports our Hypothesis 1 that Wal-Mart engaged in regulatory arbitrage. Similarly, models 3 and 4 include the unique-paired sample.

Insert Table 1 here

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 remain robust (b=0.941, p<.05). The fact that Wal-Mart's proposal behavior increases abruptly when one crosses the border from an anti-business state to a pro-business state suggests that Wal-Mart does strategically respond to RTW laws.

Table 2 presents the analysis of Wal-Mart's store openings. Model 5 tests the main effect of protest and provides confirmative evidence that protests are very effective in reducing the opening probability of Wal-Mart stores in border places (b=-2.221, p<.01). In addition, model 5 also suggests that the average opening rate of Wal-Mart stores is significantly higher in RTW states (b=0.584, p<.01) before considering protests.

Insert Table 2 here

Model 6 shows that protests are less likely to dissuade Wal-Mart from opening a store on the RTW side of a contested border (b=1.228, p<.01). To further demonstrate the meaning of this interaction effect, we calculated the predicted opening probability of Wal-Mart stores under four conditions: (1) in a RTW state with protest, (2) in a non-RTW state with protest, (3) in a RTW state without protest, and (4) in a non-RTW state without protest.

Insert Figure 2 here

As Figure 2 shows, when other variables are set at their means, the opening chance of a protested store is reduced from 33% to 6% when one moves from a RTW state to a non-RTW state,

while the opening chances of non-protested stores are roughly the same in both RTW and non-RTW states (92% vs. 94%). Figure 2 clearly demonstrates that the effectiveness of protest is the major driving force that motivates Wal-Mart to engage into regulatory arbitrage. These results support our Hypothesis 2 that protests are less effective in an unfavorable political environment. The fact that Wal-Mart's rate of overcoming protestors increases abruptly when one crosses the border from an anti-business state to a pro-business state also suggests that protesters are systematically weaker on the RTW side of a border.

Since the sample size for unique pairs is too small for an analysis of store openings, we present only the results of the multiple pair matching. But in the Appendices 3a and 3b, we report the results using the unpaired full sample. Appendix 3a shows that Wal-Mart made more store proposals in the RTW border places ($b=.379$, $p<.01$, model 2). Appendix 3b shows that Wal-Mart is more likely to open stores despite protests on the borders of RTW states ($b=1.454$, $<.05$, model 5). Together, these results provide strong support for our hypotheses.

Does Wal-Mart Exploit Regulatory Variations in Minimum Wage or Fair Employment Laws?

While we have argued that Wal-Mart located in pro-business states as indicated by RTW laws, it could be that there was something else about those states that served as the attraction. Two possibilities are that Wal-Mart exploits variations in minimum wage laws and fair employment laws due to cost considerations. Additionally, it is also possible that such laws increase the effectiveness of activists by allowing them to make social justice claims more persuasively and thereby, dissuade targets from locating stores in states with such laws. If this were true, it would be consistent with our overall arguments regarding triadic regulatory arbitrage but it would indicate that something besides RTW laws were the marker of an arbitrage opportunity.

We obtained the data of minimum wage 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 the places within 25 miles of contrast borders and conducted the analyses similar to those of the RTW laws. In addition, we also controlled the number of cities that have adopted the living wage ordinances, an acute version of minimum wage, in a state in the previous year using the data reported by the Living Wage Resource Center. As reported in Appendix 4a and 4b we found evidence that was in line with our theory of triadic regulatory arbitrage, with high-minimum wage states having a negative coefficient for Wal-Mart proposals and for the efficacy of protests, but the effects were not statistically significant.

For fair employment, we defined contrast state borders by two sets of criteria. First, we defined contrast borders as those occurring between a state without extensive protection fbeyond 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, all the places within 25 miles of these contrast borders are included. As appendix 5a and 5b show, although we find directional effects consistent with the prediction of triadic regulatory arbitrage in that Wal-Mart was less likely to propose and open when protested in states with more pro-labor protection, these effects are again not statistically significant.

In unreported analyses of paired down models with fewer control variables, we did find statistically significant evidence that Wal-Mart was significantly less likely to propose in states with more labor protection and that community protestors were more effective in dissuading Wal-Mart in those states. Those results indicate another form of arbitrage, by which Wal-Mart exploited 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 Wal-Mart's regulatory arbitrage opportunities.

Other Robustness Checks

We conducted a number of other robustness tests but do not display the results for the sake of brevity, and hence, present a summary. 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 that are located within 25 miles to any type of state borders in all U.S. continental states. We thus end up with four types of border places: those in a RTW law state facing another RTW law state, those in an RTW law state facing a non-RTW law state, those in a non-RTW law state facing another non-RTW law state, and those in a non-RTW law state facing a RTW law state. We found that as expected proposals are more likely in RTW states with contrast borders. We also found that Wal-Mart is likely to open stores despite protests in RTW states.

In another unreported analysis, we extended the border place sample to include all the places within 50 miles from contrast borders, and 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 Wal-Mart is more likely to issue proposals in the interior land of non-RTW states than the bordering area of these states, and that protesters are less likely to dissuade Wal-Mart from opening a store in the interior area of non-RTW states than the bordering area of these states as well as the bordering area of RTW states than the interior land of RTW states. This is completely consistent with our view that contrast borders present opportunities for arbitrage for the large retailer we study. As Wal-Mart considers places farther into the interior of a non-RTW state, the possibility of locating instead on the border in an adjacent RTW state becomes less feasible.

DISCUSSION AND CONCLUSION

Thompson (1967) suggested that exchange agreements hinge upon prior consensus regarding the domain of an organization – a set of expectations about what the organization will and will not do. Fligstein (2001) also observed that market building was a political project. However, there has been little discussion as to how the economic geography of organizations is shaped on the one hand by regulatory variations, and on the other hand by protests launched by activists. Indeed, for the most part, the geographic spread of organizations has been depicted since Chandler (1977) as an outcome of internal capabilities and portrayed as an exercise in replication. Our work suggests that the 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 linkages, 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 linkages; states and corporations, and corporations and activists.

State-Corporate Linkage. That the state influences the form and survival of corporations and other organizations is well documented in the institutional and organizational ecology literatures (e.g. Fligstein, 2001; Dobin, 2009; Hannan, Polos and Carroll, 2007). Our contribution is about the potential of influence to flow the other way, from corporations to the state. Of course, it is well known that the autonomy of states 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. It is only in the case of a very powerful organization, or a particularly weak state, that state autonomy is seen as threatened (Strange, 1996; Simons and Ingram, 2003). 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 those subjects are small and weak relative to the state.

To date, however, the idea of jurisdictional competition spurred by regulatory arbitrage has been more convincing in theory than 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. The first is that we do not know much about how the sorting process happens – 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 have been documented in past comparisons of RTW and non-RTW states. The spatial discontinuity design we employed presents a powerful identification strategy that is likely to 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 Lamoureux, 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 the understanding of state policy makers as to why corporations locate where they do.

In our analysis, the increasing prevalence of Wal-Marts on the RTW side of contrast borders emerges from two processes: The increased likelihood of Wal-Mart to propose stores on the RTW

side, and the greater likelihood that protests will dissuade Wal-Mart 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 for Wal-Mart. In the latter case, protests are a strong signal that communities and legislators may be open to impose even more stringent regulations. Thus, the effectiveness of movement demands is moderated by variations in regulatory regimes. Wallace (2007) spoke about labor laws as state administered truces, we argue that the RTW law embodies a pro-business settlement in the state, which is why Wal-Mart 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 (private certification and rating systems, and information disclosure rules), and 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 there is explicit deregulation in one domain that is a credible signal of the pro-business bent of legislators and voters in the 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; but our study there is far greater interdependence between public regulation 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). Very recently, the specter of regulatory arbitrage has been offered as a

reason for caution when imposing new regulations in the wake of the financial crisis, as when politicians claim that poorly conceived American regulations would only serve to drive the finance industry to London or Hong Kong (Kenny, 2010). Furthermore, activists also exist to complicate the picture internationally, (e.g. global opponents of genetically modified food). So an important agenda for future work is to look at the interaction of corporations, states and activists globally, and to 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 also be expanded in the future to include industry competitors.

Corporate-Activist Linkage. While the social movement literature has in the past 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 and Soule, 2007; King, 2008; 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 a large firm such as Wal-Mart and activists strategically interact with each other against the background of regulatory variation. Wal-Mart seeks to locate proposals such that it disadvantages a decentralized rival: anti-Wal-Mart activists. So Wal-Mart offers proposals in the borders of states with a pro-business ideology proxied by RTW laws and even if protests occur at the borders of RTW states, Wal-Mart 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 they 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 worthwhile to point out that we did not observe large differences in the incidence of protests across contrast borders (31% of proposals on the RTW side were protested, while 38% of proposals on the non-RTW side were). One implication is that activists are less responsive to the

arbitrage opportunities presented by laws than Wal-Mart. One reason is that the anti-Wal-Mart movement is localized in nature and lacks national coordination – so local activists may neither have the incentive or the ability to engage in arbitrage. The other implication is that Wal-Mart cannot predict protest location with accuracy (Ingram, Yue and Rao, 2010), and so has to locate in places where it has natural allies: pro-business officials, legislators, and the public. Hence, even if protests arise in the borders of RTW states, Wal-Mart can open a store in the face of protests, and is unlikely to be dissuaded by protest.

Indeed, our findings point to the unintended impact of movements. Giugni (1998) observes that social movements may fail to achieve the goals of activists, but can exert secondary and indirect effects. Our study shows that an indirect effect of protests targeting private corporations is regulatory arbitrage. 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 we demonstrate in our study, regulatory variation in laws such as the RTW law constitutes an element of corporate opportunity structure for the target firm and disadvantages activists. By doing so, we enlarge the concept of corporate opportunity structure such that it works for targets and not just activists or protesters, and describe location strategies of firms as an important response to anti-business activism.

In our study, firms engaged in regulatory arbitrage, but activists did not. If local activists did not engage in regulatory arbitrage, 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 are likely to protest where they are likely to succeed. Yet, there are many departures from

such a model of full information, and future research needs to 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.

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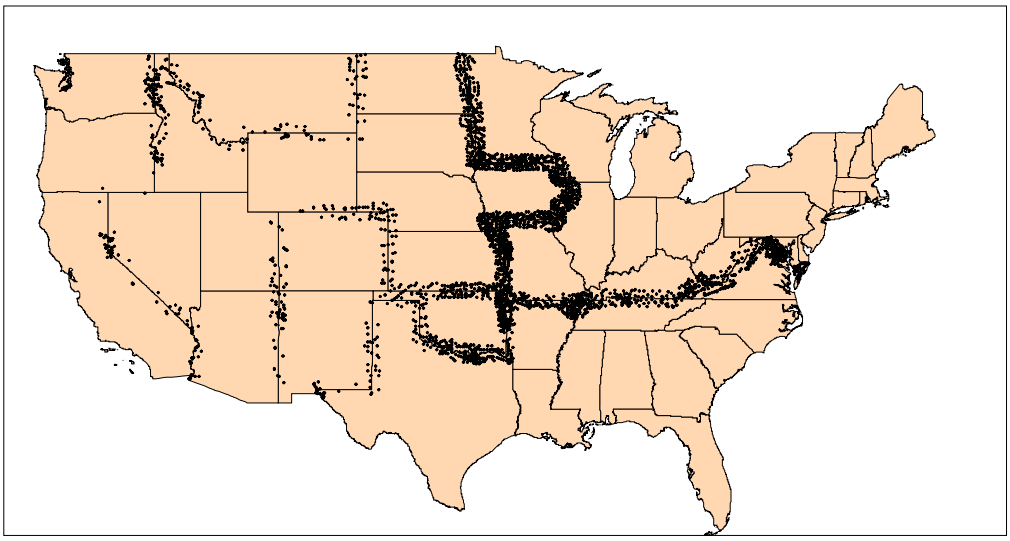
Figure 1. Border Places within 25 Miles to the Right-to-Work-Law Contrast State Border

1998-2000



2001-2005

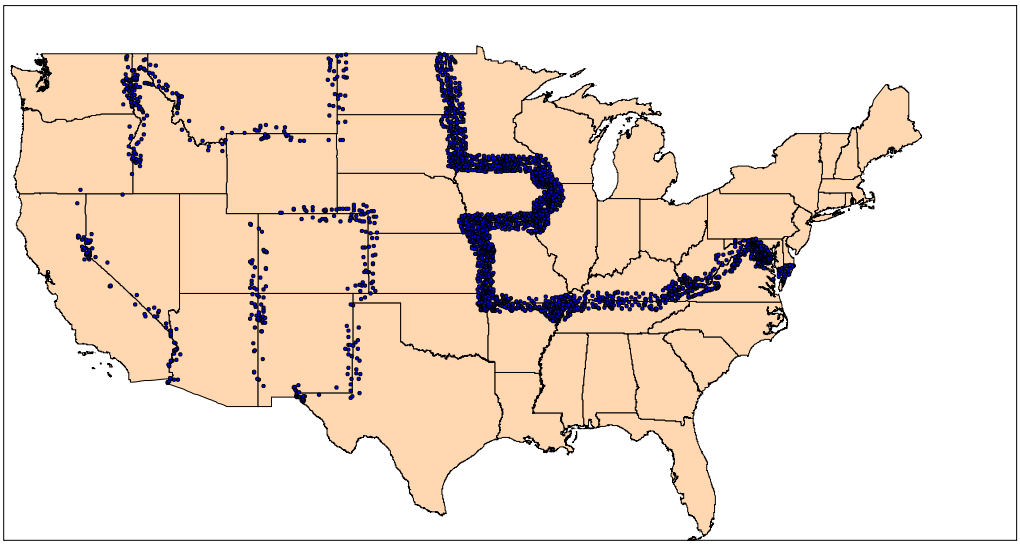


Figure 2. Predicted Store Opening Probablity

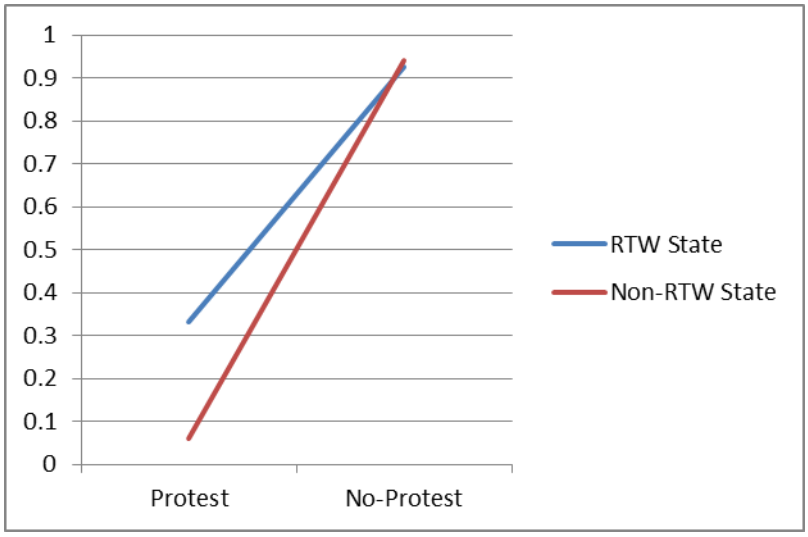


Table 1. Probit Analysis on Wal-Mart Proposal

	(1)	(2)	(3)	(4)
Population	0.501***	0.506***	0.608***	0.636***
	(0.008)	(0.008)	(0.144)	(0.148)
Distance to distribution center	-0.008***	-0.007***	-0.014	-0.009
	(0.001)	(0.001)	(0.011)	(0.011)
Wal-Mart within 50 miles	-0.009***	-0.005***	-0.018	0.001
	(0.001)	(0.001)	(0.016)	(0.018)
Unemployment %	-0.300	-0.076	0.205	1.180
	(0.186)	(0.187)	(2.722)	(2.840)
Income per capita	-0.017***	-0.018***	-0.032	-0.036
	(0.001)	(0.001)	(0.032)	(0.037)
Urban %	1.102***	1.108***	1.069***	1.115***
	(0.019)	(0.020)	(0.334)	(0.355)
Retail worker %	1.756***	1.790***	2.857	3.273**
	(0.104)	(0.104)	(1.667)	(1.598)
Wal-Mart's competitors	-0.349***	-0.304***	-0.272	-0.165
	(0.019)	(0.020)	(0.228)	(0.195)
Union member %	-0.339	2.254***	-0.158	8.131
	(0.216)	(0.324)	(2.994)	(4.397)
Church per capita	-1.166***	-0.968***	-1.899	-0.663
	(0.085)	(0.087)	(1.341)	(1.450)
Debt per capita	0.024***	0.024***	0.019	0.014
	(0.002)	(0.002)	(0.037)	(0.045)
Pro Democrat	-0.905***	-0.950***	-0.814	-1.024
	(0.030)	(0.030)	(0.595)	(0.644)
Race homogeneity	-0.225***	-0.222***	-0.544	-0.923
	(0.037)	(0.037)	(0.720)	(0.798)
Main Street Program	0.260***	0.267***	0.253	0.360
	(0.019)	(0.019)	(0.346)	(0.366)
Political hazard	-0.265***	-0.230***	-0.003	0.325
	(0.017)	(0.017)	(0.338)	(0.385)
Year	0.054***	0.053***	0.051	0.071
	(0.006)	(0.006)	(0.120)	(0.122)
Total editorial	-0.001***	-0.001***	-0.001	-0.000
	(0.000)	(0.000)	(0.002)	(0.002)
Unfavorable editorial %	0.878***	0.919***	1.917	2.143
	(0.086)	(0.086)	(1.698)	(1.736)
Retailer compensation	0.422***	0.382***	0.237	0.122
	(0.021)	(0.021)	(0.293)	(0.290)
Retail sales tax	0.084***	0.066***	0.261	0.261
	(0.010)	(0.010)	(0.201)	(0.204)
Midwest	0.184***	0.113***	0.363	0.235
	(0.017)	(0.018)	(0.297)	(0.310)
Prior protest	0.060**	0.037	-0.157	-0.327
	(0.025)	(0.026)	(0.689)	(0.720)
Business incentive	0.014***	0.015***	0.037	0.041
	(0.002)	(0.002)	(0.036)	(0.038)
RTW State		0.230***		0.941**
		(0.022)		(0.385)
Constant	-112.612***	-110.309***	-107.974	-149.004
	(11.931)	(11.921)	(239.851)	(245.343)
N	1280018	1280018	5271	5271
Log lik.	-3.42e+04	-3.42e+04	-93.516	-90.216
Chi-squared	18542.945	18656.229	86.954	93.553

Sample used in estimating models 1-2 includes the multiple paired places with the distance less than 50 miles on both sides of contrast state borders of the RTW law. Sample used in estimating models 3-4 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)

Table 2. IPTW Probit Model of Store Opening

	(5)	(6)
Population	0.056***	0.029
	(0.020)	(0.020)
Distance to dist. centr.	-0.001	-0.003
	(0.004)	(0.004)
Wal-Mart within 50m	-0.007	-0.008
	(0.007)	(0.007)
Unemployment %	5.948***	4.555***
	(1.213)	(1.260)
Income per capita	0.058***	0.063***
	(0.006)	(0.006)
Urban %	-0.327**	-0.580***
	(0.156)	(0.187)
Retail worker %	-2.254**	-5.526***
	(0.979)	(0.995)
Wal-Mart's competitors	0.180	0.158
	(0.103)	(0.095)
Union member %	9.013***	8.560***
	(2.080)	(2.067)
Church per capita	-2.619***	-3.163***
	(0.484)	(0.488)
Debt per capita	0.004	0.033***
	(0.005)	(0.006)
Pro Democrat	-0.260	-0.475***
	(0.154)	(0.160)
Race homogeneity	0.330	0.532***
	(0.194)	(0.205)
Main Street Program	0.184***	0.276***
	(0.058)	(0.063)
Political hazard	0.546***	0.531***
	(0.079)	(0.086)
Year	0.363***	0.419***
	(0.032)	(0.034)
Total editorial	0.001**	0.001**
	(0.000)	(0.001)
Unfavorable editorial %	5.737***	5.968***
	(0.414)	(0.408)
Retailer compensation	0.019	-0.047
	(0.113)	(0.109)
Retail sales tax	-0.233***	-0.321***
	(0.064)	(0.063)
Midwest	-0.211**	-0.335***
	(0.100)	(0.098)
Prior protest	-2.670***	-3.216***
	(0.159)	(0.205)
Business incentive	0.141***	0.142***
	(0.009)	(0.010)
Protest	-2.221***	-3.097***
	(0.054)	(0.096)
RTW place	0.584***	-0.113
	(0.118)	(0.135)
Protest*RTW place		1.228***
		(0.111)
Constant	-730.829***	-841.803***
	(65.111)	(68.528)
N	7007	7007
Log lik.	-2454.620	-2398.907

Sample includes the multiple paired places with the distance less than 50 miles on both sides of contrast state borders of the 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)

Appendix 1. States Located By Contrast Right-To-Work Borders

1998-2000		2001-2005	
RTW State	Non-RTW State	RTW State	Non-RTW State
AR	OK	AZ	CA
AZ	CA	AZ	CO
AZ	CO	AZ	NM
AZ	NM	IA	IL
IA	IL	IA	MN
IA	MN	IA	MO
IA	MO	IA	WI
IA	WI	ID	MT
ID	MT	ID	OR
ID	OR	ID	WA
ID	WA	KS	CO
KS	CO	KS	MO
KS	MO	ND	MN
KS	OK	ND	MT
ND	MN	NE	CO
ND	MT	NE	MO
NE	CO	NV	CA
NE	MO	NV	OR
NV	CA	OK	CO
NV	OR	OK	MO
SD	MN	OK	NM
SD	MT	SD	MN
TN	KY	SD	MT
TN	MO	TN	KY
TX	NM	TN	MO
TX	OK	TX	NM
UT	CO	UT	CO
UT	NM	UT	NM
VA	DC	VA	DC
VA	KY	VA	KY
VA	MD	VA	MD
VA	WV	VA	WV
WY	CO	WY	CO
WY	MT	WY	MT

Appendix 2. Economic and Social Demographic Characteristics of RTW and Non-RTW Border Places

	RTW Border Places		**Non-RTW Border Places**	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Distance to contrast borders	11.86	7.23	11.43	7.21
Population (in million)	0.05	0.20	0.04	0.22
Median household income	34729.59	15086.46	35486.37	18731.12
Income per capita	17098.15	7045.41	17375.16	9115.87
White ratio	0.90	0.16	0.86	0.22
Unemployment rate	0.06	0.05	0.06	0.05
Retail employment out of labor force	0.11	0.06	0.11	0.05
Population % with college education	0.10	0.07	0.10	0.07

Appendix 3a. All Places: Probit Model of Proposal

	(1)	(2)
Population	0.495***	0.501***
	(0.054)	(0.054)
Distance to distribution center	-0.012**	-0.011**
	(0.005)	(0.005)
Wal-Mart within 50 miles	-0.009	-0.005
	(0.007)	(0.007)
Unemployment %	0.030	0.309
	(1.219)	(1.222)
Income per capita	-0.003	-0.004
	(0.007)	(0.007)
Urban %	1.029***	1.051***
	(0.140)	(0.142)
Retail worker %	1.945***	1.998***
	(0.659)	(0.650)
Wal-Mart's competitors	-0.194	-0.142
	(0.111)	(0.106)
Union member %	0.034	3.472
	(1.175)	(1.773)
Church per capita	-0.823	-0.542
	(0.589)	(0.598)
Debt per capita	0.015	0.016
	(0.015)	(0.015)
Pro Democrat	-0.387	-0.410
	(0.231)	(0.234)
Race homogeneity	-0.547	-0.574**
	(0.287)	(0.291)
Main Street Program	-0.015	0.024
	(0.187)	(0.189)
Political hazard	-0.245	-0.123
	(0.126)	(0.137)
Year	0.084	0.082
	(0.049)	(0.050)
Total editorial	-0.000	-0.000
	(0.001)	(0.001)
Unfavorable editorial %	1.339	1.365
	(0.719)	(0.720)
Retailer compensation	0.215	0.146
	(0.118)	(0.120)
Retail sales tax	-0.027	-0.053
	(0.047)	(0.047)
Midwest	0.112	0.032
	(0.113)	(0.118)
Prior protest	-0.325	-0.396
	(0.261)	(0.268)
Business incentive	-0.015	-0.011
	(0.014)	(0.014)
RTW State		0.379***
		(0.150)
Constant	-171.024	-167.621
	(99.147)	(99.499)
N	23122	23122
Log lik.	-489.213	-485.944

Sample includes places located within 25 miles of both sides of contrast state borders of the RTW law. Standard errors in parentheses; ** p<.05, *** p<.01 (one-side test for hypothesized variables and two-side test for control variables)

Appendix 3b. All Places: IPTW Probit Model of Opening

	(3)	(4)	(5)
Population	0.746	0.761	0.799
	(0.503)	(0.501)	(0.505)
Distance to distribution center	0.033	0.032	0.028
	(0.028)	(0.028)	(0.029)
Wal-Mart within 50 miles	0.003	-0.015	-0.008
	(0.032)	(0.035)	(0.036)
Unemployment %	8.558	8.379	7.412
	(6.575)	(6.833)	(6.679)
Income per capita	0.043	0.046	0.051
	(0.046)	(0.046)	(0.044)
Urban %	-0.418	-0.501	-0.358
	(0.939)	(0.998)	(0.963)
Retail worker %	1.600	0.297	-2.110
	(5.820)	(6.205)	(5.975)
Wal-Mart's competitors	-0.122	-0.211	-0.091
	(0.298)	(0.305)	(0.309)
Union member %	2.707	-5.718	-6.294
	(5.428)	(8.037)	(8.423)
Church per capita	-0.888	-1.080	-1.093
	(2.649)	(2.767)	(2.784)
Debt per capita	-0.064	-0.067	-0.051
	(0.056)	(0.047)	(0.053)
Pro Democrat	-1.903	-1.878	-2.125
	(0.983)	(1.003)	(1.129)
Race homogeneity	-0.631	-0.419	0.033
	(1.428)	(1.466)	(1.381)
Main Street Program	0.218	0.395	0.398
	(0.629)	(0.661)	(0.651)
Political hazard	-0.260	-0.536	-0.346
	(0.515)	(0.597)	(0.627)
Year	-0.110	-0.103	-0.095
	(0.219)	(0.212)	(0.213)
Total editorial	0.000	-0.001	0.000
	(0.004)	(0.004)	(0.004)
Unfavorable editorial %	3.308	3.189	2.748
	(3.168)	(3.182)	(3.107)
Retailer compensation	0.192	0.421	0.466
	(0.430)	(0.475)	(0.503)
Retail sales tax	0.167	0.269	0.286
	(0.163)	(0.186)	(0.188)
Midwest	0.842	1.099**	0.991
	(0.465)	(0.531)	(0.582)
Prior protest	1.079	1.409	0.851
	(1.216)	(1.343)	(1.215)
Business incentive	0.112	0.108	0.103
	(0.058)	(0.060)	(0.062)
Protest	-1.560***	-1.527***	-2.429***
	(0.383)	(0.390)	(0.766)
RTW State		-0.904	-1.067**
		(0.620)	(0.630)
Protest* RTW State			1.454**
			(0.813)
Constant	214.390	201.532	185.058
	(438.420)	(424.370)	(427.522)
N	102	102	102
Log lik.	-4073.948	-4009.260	-3943.075

Sample includes places located within 25 miles of both sides of contrast state borders of the RTW law.
Standard errors in parentheses; ** p<.05, *** p<.01 (one-side test for hypothesized variables and two-side test for control variables)

Appendix 4a. Wal-Mart Proposal at Minimum Wage Contrast State Border

	(6)	(7)
Population	0.046***	0.046***
	(0.013)	(0.013)
Distance to dist. centr.	-0.015***	-0.015***
	(0.005)	(0.005)
Wal-Mart within 50 miles	-0.005	-0.005
	(0.004)	(0.004)
Unemployment %	-0.243	-0.244
	(1.078)	(1.079)
Income per capita	-0.018***	-0.018***
	(0.005)	(0.005)
Urban %	1.157***	1.159***
	(0.141)	(0.141)
Retail worker %	1.612	1.613
	(1.019)	(1.018)
Wal-Mart's competitors	-0.135	-0.129
	(0.097)	(0.098)
Union member %	-1.312	-1.370
	(0.848)	(0.855)
Church per capita	-2.613***	-2.612***
	(0.695)	(0.695)
Debt per capita	0.009	0.009
	(0.007)	(0.007)
Pro Democrat	0.094	0.094
	(0.196)	(0.196)
Race homogeneity	-0.743***	-0.737***
	(0.220)	(0.221)
Main Street Program	0.262	0.261
	(0.156)	(0.156)
Political hazard	-0.172**	-0.179**
	(0.081)	(0.081)
Year	0.085**	0.085**
	(0.036)	(0.036)
Total editorial	0.001	0.001
	(0.001)	(0.001)
Unfavorable editorial %	0.650	0.653
	(0.463)	(0.463)
Retailer compensation	0.072	0.057
	(0.100)	(0.104)
Retail sales tax	-0.027	-0.026
	(0.025)	(0.025)
Midwest	-0.122	-0.125
	(0.101)	(0.101)
Prior protests	-0.470***	-0.477***
	(0.148)	(0.148)
Business incentive	0.003	0.005
	(0.013)	(0.014)
Living wage city	-0.004	-0.006
	(0.020)	(0.020)
High minimum wage state		-0.038
		(0.069)
Constant	-172.563**	-173.306**
	(72.194)	(72.258)
N	46650	46650
Log lik.	-1825.723	-1825.566

Sample includes all places within 25 miles to contrast minimum wage state borders.
** p<.05, *** p<.01 (one-side test for hypothesized variables and two-side test for control variables)

Appendix 4b. Wal-Mart Opening at Minimum Wage Contrast State Border

	(8)	(9)	(10)
Population	-0.022	-0.012	-0.012
	(0.055)	(0.054)	(0.053)
Distance to dist. centr.	0.006	0.006	0.005
	(0.017)	(0.016)	(0.016)
Wal-Mart within 50 miles	0.018	0.017	0.016
	(0.014)	(0.014)	(0.014)
Unemployment %	12.532***	12.663***	12.764***
	(4.010)	(4.054)	(4.125)
Income per capita	0.016	0.013	0.015
	(0.026)	(0.026)	(0.026)
Urban %	0.813	0.790	0.763
	(0.567)	(0.568)	(0.563)
Retail worker %	11.170**	11.350**	11.284**
	(5.256)	(5.223)	(5.216)
Wal-Mart's competitors	-0.111	-0.073	-0.059
	(0.244)	(0.247)	(0.245)
Union member %	3.310	2.683	2.592
	(2.454)	(2.422)	(2.429)
Church per capita	-0.637	-0.628	-0.672
	(2.007)	(2.000)	(2.001)
Debt per capita	0.027	0.027	0.029
	(0.040)	(0.040)	(0.041)
Pro Democrat	-0.353	-0.323	-0.355
	(0.608)	(0.615)	(0.619)
Race homogeneity	0.019	0.130	0.155
	(0.791)	(0.810)	(0.828)
Main Street Program	1.172***	1.233***	1.212***
	(0.413)	(0.429)	(0.428)
Political hazard	-0.280	-0.328	-0.334
	(0.246)	(0.247)	(0.246)
Year	-0.225	-0.224	-0.218
	(0.131)	(0.130)	(0.130)
Total editorial	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)
Unfavorable editorial %	-1.410	-1.328	-1.250
	(1.985)	(1.975)	(1.977)
Retailer compensation	0.713***	0.623**	0.618**
	(0.274)	(0.291)	(0.289)
Retail sales tax	0.058	0.067	0.069
	(0.060)	(0.061)	(0.061)
Midwest	0.570	0.519	0.499
	(0.303)	(0.309)	(0.308)
Prior protests	-0.134	-0.133	-0.117
	(0.462)	(0.459)	(0.464)
Business incentive	-0.068	-0.053	-0.054
	(0.041)	(0.041)	(0.041)
Living wage city	0.063	0.058	0.063
	(0.062)	(0.063)	(0.063)
Protest	-1.289***	-1.288***	-1.154***
	(0.262)	(0.260)	(0.336)
High minimum wage state		-0.238	-0.102
		(0.202)	(0.241)
Protest* High minimum wage state			-0.351
			(0.456)
Constant	449.402	447.393	435.199
	(262.905)	(260.203)	(260.762)
N	375	375	375
Log lik.	-2.08e+04	-2.07e+04	-2.06e+04

Sample includes all places within 25 miles to contrast minimum wage state borders.

** p<.05, *** p<.01 (one-side test for hypothesized variables and two-side test for control variables)

Appendix 5a. Wal-Mart Proposal at Fair Employment Contrast State Border

	(11)	(12)	(13)
Population	0.429***	0.428***	0.204***
	(0.053)	(0.053)	(0.049)
Distance to dist. centr.	-0.004	-0.004	-0.011
	(0.004)	(0.004)	(0.006)
Wal-Mart within 50 miles	-0.001	-0.001	-0.007
	(0.005)	(0.005)	(0.006)
Unemployment %	-1.051	-1.073	-0.928
	(1.166)	(1.170)	(1.302)
Income per capita	-0.005	-0.005	-0.002
	(0.006)	(0.006)	(0.007)
Urban %	1.241***	1.239***	1.177***
	(0.144)	(0.144)	(0.202)
Retail worker %	1.999***	1.960**	1.417
	(0.766)	(0.771)	(0.788)
Wal-Mart's competitors	-0.066	-0.069	-0.244
	(0.088)	(0.088)	(0.177)
Union member %	0.892	1.149	1.514
	(0.932)	(0.980)	(1.758)
Church per capita	-1.031	-1.028	-0.553
	(0.621)	(0.622)	(0.875)
Debt per capita	0.018	0.017	0.025
	(0.016)	(0.016)	(0.018)
Pro Democrat	-0.553***	-0.535***	-0.452
	(0.205)	(0.207)	(0.353)
Race homogeneity	-0.522**	-0.539**	-0.872***
	(0.257)	(0.258)	(0.326)
Main Street Program	0.360**	0.370**	0.561**
	(0.145)	(0.146)	(0.219)
Political hazard	-0.210**	-0.210**	-0.113
	(0.084)	(0.084)	(0.132)
Year	0.103***	0.104***	0.035
	(0.034)	(0.034)	(0.055)
Total editorial	-0.000	-0.000	0.001
	(0.001)	(0.001)	(0.001)
Unfavorable editorial %	0.765	0.769	-0.165
	(0.462)	(0.462)	(0.717)
Retailer compensation	0.078	0.063	0.210
	(0.094)	(0.096)	(0.177)
Retail sales tax	0.018	0.017	0.082
	(0.042)	(0.042)	(0.070)
Midwest	-0.220**	-0.237**	-0.026
	(0.101)	(0.103)	(0.133)
Prior protests	-0.531***	-0.534***	-0.610**
	(0.151)	(0.151)	(0.270)
Business incentive	-0.004	-0.006	0.007
	(0.012)	(0.013)	(0.024)
Fair employment state		-0.077	-0.153
		(0.089)	(0.185)
Constant	-209.600***	-212.908***	-73.199
	(67.639)	(67.848)	(110.387)
N	52916	52916	16908
Log lik.	-1754.297	-1753.918	-621.346

Samples in model 6-7 include all places within 25 miles to contrast fair employment state borders. Sample in model 8 includes places within 25 miles to the stronger contrast fair employment state borders. ** p<.05, *** p<.01 (one-side test for hypothesized variables and two-side test for control variables)

Appendix 5b. Wal-Mart Opening at Fair Employment Contrast State Border

	(14)	(15)	(16)	(17)	(18)
Population	0.419	0.402	0.407	-0.364	-0.364
	(0.224)	(0.221)	(0.220)	(0.214)	(0.214)
Distance to dist. centr.	0.004	0.006	0.006	0.033	0.033
	(0.013)	(0.013)	(0.013)	(0.023)	(0.024)
Wal-Mart within 50 miles	-0.017	-0.017	-0.017	-0.062**	-0.061**
	(0.013)	(0.013)	(0.013)	(0.026)	(0.026)
Unemployment %	5.916	6.464	6.511	-2.668	-3.328
	(4.582)	(4.448)	(4.480)	(6.205)	(6.385)
Income per capita	0.008	0.007	0.008	0.005	0.009
	(0.025)	(0.024)	(0.024)	(0.032)	(0.032)
Urban %	-0.046	-0.093	-0.077	0.506	0.515
	(0.500)	(0.494)	(0.493)	(0.811)	(0.790)
Retail worker %	1.202	2.028	2.079	-9.277	-8.499
	(4.377)	(4.417)	(4.439)	(7.806)	(7.748)
Wal-Mart's competitors	-0.283	-0.299	-0.292	-0.235	-0.131
	(0.194)	(0.196)	(0.202)	(0.422)	(0.472)
Union member %	-0.484	-1.396	-1.455	-14.203**	-14.553**
	(2.626)	(2.670)	(2.669)	(6.605)	(6.741)
Church per capita	-2.202	-2.378	-2.330	-0.077	0.096
	(1.698)	(1.691)	(1.693)	(3.293)	(3.274)
Debt per capita	0.070	0.080	0.080	0.264	0.266
	(0.048)	(0.057)	(0.058)	(0.149)	(0.150)
Pro Democrat	-0.231	-0.268	-0.254	2.731**	2.988**
	(0.559)	(0.561)	(0.565)	(1.285)	(1.317)
Race homogeneity	0.816	0.886	0.913	0.967	0.953
	(0.774)	(0.776)	(0.764)	(1.447)	(1.457)
Main Street Program	0.268	0.292	0.298	0.316	0.330
	(0.268)	(0.283)	(0.282)	(0.660)	(0.681)
Political hazard	-0.200	-0.209	-0.215	0.557	0.557
	(0.291)	(0.295)	(0.298)	(0.667)	(0.664)
Year	0.217	0.212	0.213	0.050	0.039
	(0.126)	(0.127)	(0.127)	(0.201)	(0.206)
Total editorial	-0.005**	-0.005**	-0.005**	-0.012**	-0.012**
	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)
Unfavorable editorial %	0.040	-0.013	-0.041	0.063	-0.141
	(1.847)	(1.859)	(1.862)	(2.806)	(2.807)
Retailer compensation	0.118	0.197	0.206	-0.906	-0.974
	(0.212)	(0.229)	(0.229)	(0.642)	(0.678)
Retail sales tax	-0.062	-0.054	-0.053	0.243	0.246
	(0.106)	(0.110)	(0.109)	(0.274)	(0.275)
Midwest	0.038	0.124	0.124	-0.576	-0.641
	(0.278)	(0.293)	(0.294)	(0.496)	(0.527)
Prior protests	-0.414	-0.344	-0.353	1.961	1.917
	(0.398)	(0.405)	(0.407)	(1.422)	(1.489)
Business incentive	0.010	0.016	0.017	0.045	0.036
	(0.029)	(0.030)	(0.030)	(0.071)	(0.073)
Protest	-1.180***	-1.201***	-1.150***	-1.205***	-1.106**
	(0.218)	(0.217)	(0.288)	(0.414)	(0.480)
Fair employment state		0.253	0.309	0.032	0.111
		(0.228)	(0.264)	(0.637)	(0.642)
Protest* Fair employment state			-0.141		-0.573
			(0.424)		(0.902)
Constant	-433.291	-424.061	-426.218	-99.624	-75.782
	(252.605)	(254.058)	(253.928)	(402.494)	(411.427)
N	382	382	382	126	126
Log lik.	-1.89e+04	-1.88e+04	-1.88e+04	-5106.798	-5090.672

Samples in model 9-11 include all places within 25 miles to contrast fair employment state borders. Samples in model 12-13 include places within 25 miles to the stronger contrast fair employment state borders. ** p<.05, *** p<.01 (one-side test for hypothesized variables and two-side test for control variables)