

**Movements, Markets, and Fields:
The Effects of Anti-Sweatshop Campaigns on U.S. Firms, 1993-2000**

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Abstract: How do social movements influence corporations? Recent work suggests that movements can inflict material damage on their targets and shape categories of evaluation in organizational fields. Extending these ideas, we examine the effects of anti-sweatshop campaigns on sales, stock performance, reputation, and specialized ratings of U.S. firms, using fixed effects regression models and event study methods. The analysis demonstrates that social movements can in some circumstances shape both the markets and fields that firms inhabit. Specifically, anti-sweatshop campaigns (a) had negative effects on sales (though only among certain types of firms), (b) influenced stock prices, and (c) shaped specialized ratings of corporate responsibility. They also diminished previously positive corporate reputations (to a modest degree) but did not radically alter reputational hierarchies in the business community.

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Introduction

While social movements were once assumed to be primarily oriented to nation states, they now routinely target corporations. This focus is neither exclusive nor entirely new (Van Dyke, Soule and Taylor 2004; Walker, Martin and McCarthy 2008), but it is clear that anti-corporate campaigns became a staple of social movement repertoires in the 1990s. Environmental, labor, religious, and GLBT activists have waged campaigns to “name and shame” and otherwise pressure numerous firms in the U.S. and Europe. Firms have responded with a dizzying array of codes of conduct, “corporate social responsibility” policies, and certification and labeling schemes. Among social scientists, scholars of strategy are increasingly portraying activism as a game-changing challenge for firms (Baron and Diermeier 2007; Lenox and Eesley 2009), and neo-institutional theorists are increasingly treating social movements as motors of institutional change (Davis et al. 2005; King and Pearce 2010). Both empirically and theoretically, the intertwining of movements and firms has become important.

Nevertheless, it is not always clear why social movement pressure would matter to companies. Through what processes could “naming and shaming” generate “sticks and stones?” In this paper, we apply ideas from a growing body of theory on the intersections of social movements and organizations to examine the varied processes through which movements might affect their corporate targets. Perhaps the most common theoretical claim is that activism imposes costs (or “material damage”) on its targets and thus shapes organizations’ bottom lines in some fashion (den Hond and de Bakker 2007). Yet this claim is more often assumed than empirically assessed, with few notable exceptions (King and Soule 2007; Luders 2006). A second account draws attention to the ways in which social movements shape cultural categories, frames, and evaluative procedures in organizational fields (Lounsbury, Ventresca and Hirsch 2003; Weber, Heinze and DeSoucey 2008) or generate new cultural models of organization (Rao, Morrill and Zald 2000; Schneiberg, King and Smith 2008). Finally, some work argues that movements affect the internal dynamics of organizations, whether by fostering conflict and debate (Weber, Rao and Thomas 2009) or by spawning internal reform coalitions that gain power under particular micro-institutional conditions (Kellogg 2009; Raeburn 2004).

Building on this work, we assess several paths through which the labor rights and anti-sweatshop movement of the 1990s in the U.S. may have influenced its corporate targets. We examine both “bottom line” effects (on sales and stock performance) and particular types of categorical effects (on reputation and specialized ratings) among large U.S.-based apparel, textile, and footwear firms during the rise of the contemporary anti-sweatshop movement (1993-2000).¹ The anti-sweatshop movement is a critical case for such an analysis, since (1) it is a quintessential case of the corporate-targeted “naming and shaming” strategy, which influenced activism in other sectors (Soule 2009; Spar and LaMure 2003), (2) social movement activity here clearly spawned a range of responses from firms (Seidman 2007), putting a premium on documenting *how* campaigns mattered and why firms were attentive, and (3) the availability of detailed data on movements in this arena allows us to construct a rich, longitudinal dataset, suitable for rigorously assessing effects across a number of firms. Whereas much previous work has merely assumed social movement influences (den Hond and de Bakker 2007) or looked only at the most successful campaigns and heavily targeted firms (Spar and LaMure 2003), we seek to document these effects systematically, while still capturing the texture of this particular case.

Our analysis is the first to demonstrate that anti-sweatshop campaigns did indeed have a range of systematic effects on their U.S.-based targets. We find that anti-sweatshop campaigns had discernible negative impacts on sales and stock prices. Anti-sweatshop campaigns also shaped external evaluations of firms: They strongly influenced specialized ratings of corporate social responsibility and slightly diminished previously positive corporate reputations. These findings show that social movements can have significant effects on the markets and organizational fields that firms inhabit, though they also highlight the limits of those effects and the importance of careful, conditional theorizing rather than broad assumptions about the power (or lack of power) of this style of activism.

Background: Anti-Sweatshop Activism and Corporate Responses

The anti-sweatshop movement of the 1990s sought to expose poor labor conditions and violations of labor rights associated with the increased globalization of apparel and footwear manufacturing. Through protests, media exposés, congressional hearings, and a variety of other campaign activities, coalitions of labor, human rights, religious, and student activists “named and shamed” a variety of firms. In addition to well-known campaigns highlighting conditions at Nike factories in Vietnam, Indonesia, and China and exposing child labor in Wal-Mart suppliers in Bangladesh and Honduras, anti-sweatshop activists exposed exploitation, mistreatment, and repression in the garment districts of the U.S. and the global supply chains of a number of companies. For example, in 1995, when union organizing attempts at the Mandarin apparel factory in El Salvador were repressed, the National Labor Committee and the Union of Needletrades, Industrial, and Textile Employees (UNITE) led a campaign targeting retailers that sourced from the factory—JC Penney, Eddie Bauer, Target, and especially the Gap. Over the next two years, protestors demonstrated at Gap headquarters and stores around the country, pressuring the Gap to neither cover up the problems nor “cut and run” from the factory. Similarly, labor, student, and religious activists waged protests at Phillips-Van Heusen, Liz Claiborne, JC Penney, and Kohl’s stores at several points between 1993 and 2000 to demand that the companies support union rights in their Central American suppliers. A growing interest in legal tactics came to head in 1999, when Global Exchange, UNITE, and other groups coupled protests with a lawsuit charging major manufacturers and retailers with complicity in gross violations of labor law in the U.S. territory of Saipan, where indentured workers from Asia sewed garments labeled as “Made in the USA.”

As shown in Table 1, anti-sweatshop campaigns had a range of sponsors, geographical foci, and tactics. NGOs and labor unions were the most frequent sponsors, with the National Labor Committee and its media-savvy strategies being the single most prominent NGO. Contrary to the view that anti-sweatshop activists only focused on foreign factories, more than a third of the campaign activity during this period concerned garment districts in the U.S. Though nearly all campaigns included some degree of public protest and the mobilization of media attention, some also included lawsuits, thus using more than “soft” shaming against their corporate targets.

[Table 1, Profile of Anti-sweatshop Campaigns, about here]

Though the precise processes behind its influence remain unclear, it is clear that the anti-sweatshop movement sparked responses by apparel, textile, and footwear firms. Most commonly, in the midst of anti-sweatshop pressure, firms often adopted codes of conduct for their supply chains. This practice very quickly diffused to other firms, including those that had not been singled out by activists.² When it came to responses that required firms to give up even a small degree of autonomy—such as participating in multi-stakeholder factory monitoring initiatives—direct social movement pressure appears to have been necessary. As shown in Table 2, among the 50 largest U.S. based apparel, footwear, and textile firms, *none* participated in any of the three leading multi-stakeholder initiatives (Fair Labor Association, Social Accountability International, or Ethical Trading Initiative) between 2001 and 2004 without having been targeted by activists at some point in the 1990s. Though pressure was not sufficient for participation, other work demonstrates that higher degrees of pressure raised the odds of participation, controlling for other factors (Bartley 2009). Of course, the poor track record of factory monitoring (Locke, Qin and Brause 2007; Seidman 2007; Sum and Pun 2005) underscores that “influence” should not be equated with “success.” Nevertheless, along with other evidence that anti-sweatshop pressure spawned a variety of corporate responses (Bonacich and Appelbaum 2000; Soule 2009), these findings beg the question of exactly *how* this pressure affected companies.

[Table 2, Illustrating the Consequences of Campaigns, about here]

How Do Social Movements Affect Corporations? Arguments and Hypotheses

Taken together, literatures on the social movement-organizations nexus, anti-sweatshop campaigns, and “private politics” suggest several routes through which movements might affect firms. As one form of material damage, many scholars argue that activist campaigns filter into the preferences of consumers, who are the key force in punishing or rewarding firms. A growing set of economic models of corporate social responsibility start with the assumption that consumers are sensitive (albeit differentially so) to information about how a product is made (Bagnoli and Watts 2007) and that social movements supply information that allows a market for “credence goods” (those valued for qualities that are not directly observable) to function (Feddersen and Gilligan 2001).

Scholars of the anti-sweatshop movement have often argued that consumers are sensitive to sweatshop scandals and are potentially willing to alter their purchases on this basis. Elliott and Freeman’s (2003) influential account is perhaps the strongest statement. Citing survey data, they argue that “what gives the antisweatshop movement power is that consumers care not only about the physical attributes of goods and services but also about the workplace conditions associated with them” (p.28) and that “if consumers were indifferent to those conditions, firms would ignore activist complaints about poor standards” (p.28). Moving beyond the limitations of survey evidence on this topic, experiments conducted by Prasad et al. (2004) and Hiscox and Smyth (2007) have identified segments of consumers who will pay more for products labeled for decent working conditions, with demand among these consumers remaining fairly inelastic up to 20-40% price premiums. *Negative* events might be expected to shape consumer behavior even more, given cognitive biases toward negative information and the power of dramatic events to mobilize mass concern (Hilgartner and Bosk 1988). To the extent that consumers do indeed respond to information about sweatshops by avoiding targeted firms or minimizing their purchases, we would expect (*ceteris paribus*) that

H1a: Anti-sweatshop campaigns will decrease targeted firms’ sales.

Although the sovereign, concerned consumer remains an article of faith for much of the literature, some have highlighted the limited power of social movements to alter consumer behavior. Vogel (2005) argues that campaigns that seek to shape consumer purchases are “more bark than bite,” since few have been strong enough to influence mass consumer preferences or close the gap between intentions and actions. This may be especially the case for labor issues, which have been less fully integrated into “ethical consumerism” movements and “common good” frames than environmental issues have (Bartley 2010). Furthermore, research on anti-sweatshop campaigns reveals that many campaigns struggle to gain leverage over their targets or maintain the fragile cross-border coalitions that many campaigns depend upon (Anner and Evans 2004; Armbruster-Sandoval 2005; Brooks 2007; Rodríguez-Garavito 2005). More generally, only a subset of social movement campaigns manage to gain strength, while others end up as fleeting, partial, or overlooked. As some research suggests, social movement campaigns vary in their capacity to generate robust forms of activism and conspicuous moments in which current practices are de-legitimated (Armstrong and Cragg 2006; Ganz 2000). These considerations suggest a more restricted hypothesis about social movements’ capacity to influence consumer behavior, namely

H1b: More intense anti-sweatshop campaigns will decrease targeted firms’ sales, while less intense campaigns will have little impact.

Scholars have also tempered claims about the effects of social movements by theorizing the types of firms that are most vulnerable to changes in consumer behavior. Though much of this work has sought to explain which firms, arenas, and industries initially become targets of activism (Luders 2006; Schurman 2004; Walker, Martin and McCarthy 2008), it also helps to consider which types of firms, when targeted, will suffer the greatest consequences. Minimally, for campaigns to shape sales, consumers must be able to easily connect firms to products. This is less likely for firms that sell in business-to-business markets or make undifferentiated or unrecognizable inputs for other products, as both the recent literature (Luders 2006) and older theories of “corporate liberalism” point out. Furthermore, if consumers

respond to social movements by avoiding a particular set of products, then firms that specialize should be especially vulnerable, while generalists that sell a wider array of products may have more of a cushion. Adapting these ideas to the apparel and footwear industries, we test two other claims:

H1c: Effects of anti-sweatshop campaigns on firms' sales will be more discernible among firms that sell products that are recognizable to consumers, rather than unbranded textile firms that sell primarily to other businesses.

H1d: Effects of anti-sweatshop campaigns on firms' sales will be more discernible among such firms if they specialize in apparel or footwear, rather than having greater diversity in their product lines.

Regardless of whether anti-sweatshop campaigns affect consumer behavior, they may shape the bottom line by influencing investors and undercutting firms' stock performance. Since investors are fewer in number, less dispersed, and more informed than consumers, one might expect their reactions to protest and scandal to be especially powerful. Vogel (2005) argues that the "market for virtue" among individual consumers is slight and fragile but that shareholder responses to campaigns have sometimes proven powerful. King and Soule (2007) argue that protest activity sends signals to shareholders regarding future market trends, stakeholder discontent, or new costs that will be imposed on the firm. They find evidence of protest events depressing stock performance, as do studies of the 1999 Seattle WTO protests (Epstein and Schnietz 2002) and news of sweatshop abuses (Rock 2003). In this literature, scholars focus on discrete events rather than ongoing campaigns, partly for methodological reasons and partly because they expect stock markets to adjust quickly to signals and pressures from social movements. In sum, prior work provides both theoretical and empirical warrants for expecting that

H2a: Major anti-sweatshop campaign events will generate negative stock returns for targeted firms.

Yet existing research also suggests that investors are unlikely to respond equally to all events, or even all major events. Though investors may consider the risk of anti-sweatshop activism affecting consumer decisions (as discussed above), they should be even more attuned to risks of concentrated financial losses. Easley and Lenox (2006) argue that lawsuits may impose this kind of damage, and they find that firms were more likely to make concessions to environmental campaigns that included a lawsuit than those using other types of tactics. Similarly, research on the anti-sweatshop movement and corporate accountability movements shows that lawsuits and threats of legal liability have provoked some of the most aggressive counter-mobilization by firms (Bonacich and Appelbaum 2000; Shamir 2004). In addition to legal risks, one would expect activism to be more salient to investors when it is covered in the national media, as King and Soule (2007) found. For these reasons, we test the following additional claims:

H2b: The filing of a sweatshop-related lawsuit will have greater effects on stock returns than other kinds of events.

H2c: Effects of anti-sweatshop events on stock returns will be greater when events are covered in the national press.

Some scholars have looked beyond consumer and investor behavior to consider the ways in which social movements might shape status orders and categories of evaluation in markets and organizational fields. As King (2008) puts it, boycotts "may not need to affect sales at all in order to be effective. Rather, boycotters' influence stems from their ability to . . . generate negative public perceptions of the corporation" (p.413-414).

Perhaps the most widespread claim is that activist campaigns can leverage the power of corporate reputations. Reputational investments increase opportunities for activists to "name and shame" firms, turning reputation from an asset to a liability. A growing literature on industry-targeting activism and "private politics" emphasizes the role of reputational risk in shaping campaigns and firm responses

(Baron and Diermeier 2007; Schurman 2004). Existing work has shown that activists disproportionately target firms with high-profile, valued reputations (Bartley and Child 2009; Lenox and Eesley 2009) and that prior reputational declines make firms more likely to concede to activist demands (King 2008). But it remains to be seen if activist campaigns successfully tarnish the reputations of targeted firms. To the extent that they do, this would provide evidence of new types of evaluative standards taking hold in an organizational field. Notably, in a somewhat different setting, Love and Kraatz (2009) find that downsizing events (between 1985 and 1994) undermined large firms' reputations, and they argue that this derived from evaluators questioning firms' character and integrity. By a similar rationale, one would expect social movement campaigns to raise questions about managerial negligence and future costs and thus diminish reputations, even among the arbiters of corporate reputation in the business community. Indeed, the contemporary anti-sweatshop movement was founded on the idea of targeting high profile brands, and many commentators have argued that it has often succeeded in casting a pall over the reputations of firms like Nike, Disney, and the Gap (Best and Lowney 2009; Klein 1999; Soule 2009; Spar and LaMure 2003). Therefore, we expect that

H3a: Anti-sweatshop campaigns will damage targeted firms' reputations.

Yet as one moves beyond case studies of the most successful campaigns, it becomes clear that such effects are far from automatic. Corporate reputations—especially in the business community—may be sticky or subject to “halo effects” that only the strongest campaigns can dislodge. Firms may also be able to respond to campaigns with new policies and public relations strategies that preserve their reputations with key audiences (Doane 2005; Ross 2004), though continued social movement pressure makes the limits of these responses clear (Bartley 2005; Spar and Burns 2000). Furthermore, like investors, evaluators of corporate reputation may be more sensitive to campaigns that include lawsuits, since they introduce the risk of concentrated damages and undermine perceptions of the firm's legal propriety. For these reasons,

H3b: More intense anti-sweatshop campaigns will damage firms' reputations, while less intense campaigns will have little impact.

H3c: Anti-sweatshop campaigns that include a lawsuit will have a larger effect on firms' reputations than other types of campaigns.

Reputational effects may also depend on the prior position of firms in a reputational hierarchy. Here we find two different predictions in the literature. First, many of the arguments about activists leveraging corporate reputations are arguments about *spoiling* or *sullying* otherwise good reputations. As Spar and LaMure (2003) put it, “NGOs frequently target the otherwise positive image that a firm may have spent decades nurturing. . . . Accordingly, the greater the importance of a brand to a firm, the more susceptible its managers are to activist pressure” (p.84). Soule's (2009) study of a range of campaigns finds that vulnerability comes in part from “when a favorably perceived company does something that is counter to its positive image” (p.148). By this logic, firms that have poor to average reputations may not suffer much from additional criticism. On the other hand, King (2008) argues that firms for which performance and reputation have been declining are more susceptible to activist pressure, and he finds that prior reputational declines increase the chances that boycotts spur corporate concessions. Though intended to explain why firms make concessions, this argument also implies that some firms may experience greater reputational damage than others. If a company has already begun to lose favor among the arbiters of corporate reputation, then social movement activity could be a “last straw” that leads evaluators to further downgrade the firm's standing.³ For these reasons, we test the following claims:

H3d: Effects of anti-sweatshop campaigns on corporate reputation will only be discernible among firms that have good reputations to begin with.

H3e: Effects of anti-sweatshop campaigns on corporate reputations will be more discernible among firms that have experienced a recent decline in their reputations.

Finally, apart from generalized corporate reputations, recent work draws attention to more focused systems of categorization, rating, and evaluation. Such systems can powerfully enforce particular logics of appropriateness and shape organizational performance (Sauder and Lancaster 2006; Zuckerman 1999). While some studies have emphasized how social movements inspire new systems of categorization and evaluation (Rao 1998), others have analyzed changes in the content of existing systems and their evaluations of particular organizations (Rao, Monin and Durand 2005). Information generated through social movements may not only inform individual consumer choices—as Feddersen and Gilligan (2001) argue—but perhaps even more powerfully, it may inform the findings of specialized evaluators in the organizational field. By legitimating and delegitimizing certain practices with attentive critics and raters, social movements may foster institutional change by altering modes of classification.

In the present context, one outcome of the anti-sweatshop movement has been the rise of new systems for rating “corporate social responsibility” (CSR). The ratings produced by KLD Research and Analytics have become the “the most exhaustive and widely used measure of CSR” (Vogel 2005), with ratings of global labor issues being added in the late 1990s. Recent research shows that KLD ratings, when negative, can generate powerful incentives for change (Chatterji and Toffel 2010) and, when positive, can buffer firms from events that would otherwise hurt their performance (Doh et al. 2010). Yet the rating bodies have questionable capacities to measure the complex state of labor conditions in firms’ global supply chains (Chatterji, Levine and Toffel 2009), making it more likely that they will respond to information generated through social movement campaigns. For these reasons, one may expect social movements to shape field-level evaluation in an additional way, namely that

H4: Anti-sweatshop campaigns will shape the evaluations of specialized corporate social responsibility rating bodies.

Though these hypotheses do not exhaust the possible effects of anti-sweatshop campaigns on firms, they seek to capture arguments that are both common in the theoretical literature and suggested in the case-based research on the anti-sweatshop movement.

Data and Methods

We test these hypotheses by combining a comprehensive dataset on anti-sweatshop campaigns from 1993-2000 with data on large U.S.-based firms. Our sample consists of the largest firms in the apparel, textile, and footwear industries, including relevant retailers (i.e., specialty apparel retailers, department stores, and discount retailers that sell apparel) (N=168).⁴ All variables are measured yearly—or at more precise levels in the case of our stock price analysis.

Our key independent variables concern the occurrence of anti-sweatshop campaigns. To measure these, we developed a comprehensive database of labor-related movement activity in this industry from 1993 to 2000. While social movement researchers often rely on newspaper reports (especially the *New York Times*) to count protest events (Earl et al. 2004), we utilized more specialized sources of information to develop a rich dataset of campaigns in one particular sector. We draw on the detailed coverage of industry politics provided by the two leading trade journals—*Bobbin* and *Women’s Wear Daily* (WWD). As the industry’s *daily* newspaper, WWD provided especially detailed coverage of social movement activity. In total, we collected 1,467 articles on labor-related conflicts and coded them to generate a record of movement activity targeting particular firms. Social movement activity was operationally defined as any attempt to force change in the labor practices of the industry (or its firms) led by advocacy groups based outside companies, trade associations, or government. In practice, this included protests at stores, leafleting and other non-confrontational demonstrations, solidarity pickets, and human rights lawsuits. (This diversity of activity reflects the multi-faceted character of anti-sweatshop campaigns, which often link organizing efforts at the point of production to broader human rights or social justice concerns, mobilize multiple forms of pressure, and involve coalitions of sponsoring groups.) To avoid conflating anti-sweatshop campaigns with fully localized labor relations, we excluded cases if solely

about union negotiations in the U.S. with only labor unions as sponsors. We supplemented the trade journal data with information from the leading secondary sources on the anti-sweatshop movement (Armbruster-Sandoval 2005; Bonacich and Appelbaum 2000; Louie 2001; Manheim 2001; Ross 1997).⁵ To further validate our database of campaigns, we compared its coverage to that of the *New York Times* and eight other large newspapers in the *Newsbank* database in selected years. This revealed no clear evidence of additional corporate targets and demonstrated the newspaper coverage to be under-representative of campaign activity, especially early in a protest cycle.⁶ We believe our database provides a more comprehensive record of anti-sweatshop activity in the U.S. during the 1990s than is available from any other source.

We measure the presence and character of anti-sweatshop activity for a given firm-year in several ways. First, we use a dichotomous measure of the presence or absence of anti-sweatshop activity. In total, 23.8% of all firms in the sample were targeted at some point, with campaign activity occurring in 10.4% of all firm-years. Second, to consider the effects of legal tactics, we use a trichotomous measure that differentiates instances in which a lawsuit was filed (21.9% of targeted firm-years) from other types of anti-sweatshop campaign activity. Third, to measure the intensity of campaign activity, we use the number of articles discussing a firm's labor rights issues during a year of an anti-sweatshop campaign. This count varied from 0 to 24 articles, with a mean across all firm-years of 0.335 articles.⁷ To adjust for skewness and preserve zero values, we used a square root transformation of this count.⁸ Though it does not measure the aggressiveness of particular protest events, this measure is intended to capture the extent to which campaign activity was vibrant and sustained as opposed to fleeting or bounded. We also constructed dummy variables for low and high intensity of campaign activity (treating no campaign as the reference category), with cases above the 75th percentile of articles per targeted firm-year (four articles) coded as high intensity and others with campaign activity coded as low intensity.

A series of models allow us to test our hypotheses. To test for effects on sales (H1a-d), we use longitudinal regression models with fixed effects (using the *xreg* command in Stata). The dependent variable here is the natural log of annual sales (to reduce skewness), with sales figures taken from the Compustat Industrial Annual database. Fixed effects models control for unobserved heterogeneity among firms, such as the propensity to be targeted by activists or "selected into" social movement pressure (Allison 2009).⁹ Prior work shows that anti-sweatshop activists tended to target larger and higher profile firms during this period (Bartley and Child 2009). We control for sales in the prior year and for firm size (log of total assets) and financial performance (return on assets (ROA), defined as net income/total assets), since changes over time in these may affect sales. All of these measures come from Compustat, using restated values when available and converting to real dollars using the Consumer Price Index. To control for marketing activities, we use a dichotomous measure of whether a company was listed in the previous year in *Advertising Age's* annual list of "100 Leading National Advertisers" (based on expenditures) or "Marketing 100" list (of innovative "branding" campaigns). To control for market-wide time trends, we include dummy variables for each year after 1993. Since we have repeated measures for companies over time, we use robust standard errors, clustered by company. In general, these fixed effects models are geared toward rigorously estimating how social movement campaigns might alter firms' sales.

Similar fixed effect models are used to test for effects on corporate reputation (H3a-e). Here, the dependent variable is a firm's relative reputation score—that is, the difference between a firm's reputation score and the mean for its industry—as measured in *Fortune* magazine's ratings of "America's Most Admired Companies," a standard measure of corporate reputation. *Fortune* conducts a yearly survey of executives, analysts, and directors, who rate the largest firms in their industry on attributes like "quality of management," "quality of products or services," "wise use of corporate assets," "social responsibility" and others. Previous research shows these attributes are highly correlated and argues for using the overall reputation score as a measure of a firm's reputation in the business community (Fombrun and Shanley 1990; Roberts and Dowling 2002). Researchers have used these ratings to examine how reputation

affects performance (Fombrun and Shanley 1990; Roberts and Dowling 2002), influences vulnerability to external pressures (King 2008), and most importantly for our purposes, how particular events affect reputations. Prior work shows how particular management techniques improved reputations (Staw and Epstein 2000) and downsizing hurt corporate reputations (Love and Kraatz 2009). Our relative reputation measure takes account of the comparative nature of the *Fortune* ratings (Love and Kraatz 2009) and mirrors Roberts and Dowling's (2002) approach. Since the *Fortune* ratings are only assigned for the largest companies in a particular industry segment, and since our interest is in change over time, our analyses are based on the 35 companies from our sample that were rated for at least two consecutive years during our period. We control for reputation score in the previous year, and standard measures of performance (sales and ROA, both from Compustat), following Flanagan et al. (2008), who show that these two factors effectively capture the "halo effect" in the *Fortune* reputation ratings. Following Love and Kraatz (2009), we also control for changes in performance from the previous year, since big gains or losses could have major implications for the reputational hierarchy.

A third set of longitudinal models allows us to assess the relationship (H4) between anti-sweatshop campaigns and specialized ratings of corporate social responsibility, produced by KLD Research and Analytics (now owned by Riskmetrics). Growing out of the Domini 400 socially responsible investment index, KLD began in the 1990s to generate binary indicators of specific corporate "strengths" and "concerns," focusing initially mainly on community involvement, corporate governance, and environmental performance. These ratings have become widely used in the sustainable investment community and by scholars of corporate social responsibility (Briscoe and Safford 2008; Chatterji and Toffel 2010; Doh et al. 2010). In the late 1990s, KLD began issuing "labor rights concern" ratings pertaining to firms' supply chains. We use this yearly dichotomous "concern" indicator as our dependent variable (and estimate with xtlogit in Stata). Research on environmental ratings has shown that KLD's "concern" ratings are more closely linked to actual performance records than are "strength" ratings (Chatterji, Levine and Toffel 2009). Furthermore, a labor rights "strength" measure was not added until 2002, too late for our period of analysis.

KLD evaluates a subset of large firms (in the 1990s, only those in the S&P 500 or Domini 400 indices), which allows us to examine the 45 firms in our sample that were rated at some point between 1999 (when the labor rights concern rating was first applied) and 2001. Given this shorter time series of data, during which no firm had a labor rights concern rating removed, we model only the first occurrence of a concern and exclude firms from the analysis after they receive that rating. We control for firm size and for characteristics of firms' production processes that might shape the likelihood of receiving a labor rights concern rating. Specifically, by coding all firms' annual reports (accessed through SEC Edgar and Thomson Research databases), we developed dichotomous indicators of whether a firm was an "early globalizer" of production processes or a previously domestic producer that later shifted to foreign production.¹⁰

To assess effects of anti-sweatshop campaigns on stock prices (H2a-c), we use event study methods. These are widely used by scholars of finance and organizations to estimate "abnormal" stock price returns at particular moments, such as the announcement of stock buyback plans (Zajac and Westphal 2004), regulatory announcements (Binder 1985), and acquisitions (Beckman and Haunschild 2002). Recently, researchers have used this method to estimate the effects of social movement activity (Epstein and Schnietz 2002; King 2008; King and Soule 2007; Rock 2003). Event study methods predict "normal" stock price returns for a given firm, then compare that to actual returns to calculate "abnormal" returns. If statistically discernible, negative abnormal returns at a particular moment suggest that firms are being penalized in the stock market.

To utilize this method, we shift from a focus on ongoing campaigns to discrete events, examining 39 major events in the anti-sweatshop movement of this period involving publicly traded firms. (See

Appendix A.) These were identified in several ways. For each firm-year coded as having high degrees of campaign activity, we identified the most important event (including the “Kathie Lee Gifford” scandal over child labor, large protests against Nike, the Gap, Federated, and others, and revelations of slave labor in El Monte, California). In addition, we examined our entire database to identify additional events that substantive knowledge of the case and the secondary literature reveal to be especially important moments (e.g., early exposés of child labor for Wal-Mart in Bangladesh and Liz Claiborne in Guatemala). We also added additional cases of lawsuits and public protests, in order to better assess whether lawsuits had different effects than other tactics. Following previous literature, we excluded 11 events for which a search of *The Wall Street Journal* uncovered evidence of confounding events—that is, changes in top-level executives or announcements of changes in sales or earnings—which might be expected to generate abnormal returns on their own. Overall, our sample of events includes the moments of intense activism and scandal that would be likely to generate negative stock market reactions. 38.5% of events involved lawsuits, and 30.8% were covered in the *New York Times*.

We performed event study analyses using the Eventus procedure in SAS (via Wharton Research Data Services), with daily stock price data from the Center for Research in Securities Prices (CRSP). To estimate normal rates of return, we follow others in using the simple “market model” (King 2008; King and Soule 2007; Zajac and Westphal 2004), which estimates daily normal rates of return by regressing firms’ returns on the returns of the full set of firms in the CRSP data during a period prior to the event (here, the 250-day period ending 30 days before the event, similar to other studies).¹¹ One then calculates abnormal returns by subtracting predicted from actual rates of return for event days, and these are summed to generate “cumulative abnormal returns” for particular “event windows.”¹² Zajac and Westphal (2004), McWilliams and Siegel (1997), and Rock (2003) recommend using short event windows in order to avoid confounding events and noise associated with market corrections. Accordingly, we examine an 11-day (5 to +5 days around the event) and two-day (-1 to 0 days) window (following King and Soule (2007), Zajac and Westphal (2004) and others) and add a moderate five-day (-2 to +2 days) window as well. Days prior to the event are commonly used in this type of analysis to allow for the possibility of early information leakage. In additional models, we examined other windows (including substantially longer periods and the single event day) and found substantively identical results. We supplement the conventional Patell’s Z test statistic with bootstrap tests, as recommended by McWilliams and Siegel (1997), and use t-tests to assess differences by legal tactics and press coverage (H3a-b).

Results

Table 3 shows the effects of anti-sweatshop campaigns on sales. We find no support for H1a, which predicted that anti-sweatshop campaigns in general would decrease sales. Though the coefficient for anti-sweatshop campaigns is in the expected direction in model 1, it is not statistically significant. The control variables work as expected: Firms’ sales were shaped by sales in the prior year, as well as notable advertising/marketing campaigns and changes in firm size. Hypothesis 1b receives no support when tested among all firms. In model 2, intensity of campaign activity has the expected negative coefficient but is not statistically significant. Neither are the dummy variables for low and high intensity campaigns significant in model 3.

[Table 3, Effects of Anti-sweatshop Campaigns on Sales, about here]

H1c suggested that effects of anti-sweatshop campaigns would be more discernible among firms with products that are recognizable to consumers. Models 4-6 therefore exclude unbranded textile firms (all textile firms except Fruit of the Loom, Russell, and Tultex). Here, we find evidence that more intense campaigns had discernible effects on sales, though we still find no effect of campaigns in general. In model 5, the intensity of a campaign is negatively and significantly associated with sales. In model 6, the dummy variable for high intensity campaigns is negative and significant. Exponentiating this coefficient

indicates that firms that faced intense anti-sweatshop activity saw a 3.6% decline in annual sales on average, compared to all others.

Models 7-9 further restrict the sample, following H1d, which suggested that effects of campaigns would be more discernible among firms that specialize in apparel or footwear, as opposed to generalists firms that have a buffer. Here, we exclude general retailers that sell more than apparel and footwear, such as Target, Wal-Mart, and Sears, plus the conglomerate, Sara Lee. We find a great deal of support for H1d, since the effects of any anti-sweatshop campaign in model 7 and campaigns of varying intensities in models 8-9 are all negative and statistically significant. Among firms with recognizable consumer products that specialize in apparel or footwear, being targeted in an anti-sweatshop campaign was associated with a 3.6% decrease in annual sales on average ($1-e^{-0.0366}$). More intense campaigns appear to generate slightly larger decreases (as shown by the intensity measure). The dummy variables show that the most intense campaigns were linked to a 4.7% decrease on average and less intense campaigns a 3.4% decrease on average (though the difference between these two is not statistically discernible).¹³

In sum, though we find no support for the most general claims about anti-sweatshop campaigns undermining sales, we find compelling evidence that some firms—especially specialized and recognizable ones—experienced notable declines in sales as they faced anti-sweatshop campaigns. Since our models control for prior levels of sales and notable marketing campaigns, the effects of anti-sweatshop campaigns may be best understood as limits on the growth of sales. Looking at particular cases, while some targeted firms had clear declines in absolute terms, such as Nike in 1997-98, Guess in 1997-98, and Liz Claiborne in 1994-95, in other cases, anti-sweatshop campaigns appear to have kept firms from selling as much, relative to their competitors, as prior performance would warrant. In supplemental models, we also considered whether the tactics of the campaign might matter, by comparing those with lawsuits with those using other tactics, including public protest. Lawsuits were not associated with changes in sales. Instead, as one might expect, it was the occurrence of campaigns using other kinds of tactics that influenced the sales of specialized, recognizable companies.

Table 4 assesses whether major anti-sweatshop events undermined firms' stock performance, as predicted by H2a. The mean level of abnormal returns is negative and significantly different from zero for both the 5-day and 2-day windows, providing support for H2a. On average, our sample of anti-sweatshop events (excluding those with confounding events) coincided with stock prices that were 1.52% lower (than the level expected by past performance) during the 5-day window and 1.26% lower during the 2-day window. Looking at events individually, we find several clear instances of significant declines. When the Gap faced protests over a factory in El Salvador in December of 1995, its stock price fell more than 12% below the expected level. Wal-Mart's fell 3.75% when it was exposed for using child labor in Bangladesh in late 1992, and Oshkosh B'gosh's fell 9.66% when it was charged with complicity in labor abuses in Saipan. All of these occurred in our smallest (2-day) event window and were highly statistically significant in event-specific tests. For several other events—including the campaigns against Wal-Mart in 1995 and Federated in late 1996, and lawsuits against Target in 1995 and May Department Stores in 1999—negative abnormal returns were discernible over the 5-day window or at somewhat lower levels of significance. On the other hand, some major moments in the anti-sweatshop movement, like the Kathie Lee Gifford child labor scandal and the *Kasky* lawsuit against Nike, seem to have had no effect on the stock performance of implicated firms. In total, 64.1% and 56.4% of all events had negative returns during the 5-day and 2-day windows, respectively, though the fact that a minority of these event-specific estimates were statistically significant leads us to slightly temper our otherwise compelling support for H2a.

[Table 4, Effects of Anti-sweatshop Events on Stock Returns, about here]

We find only limited support for the idea (H2b) that investors responded more to lawsuits than events using other kinds of tactics. The filing of lawsuits did correspond with statistically significant negative abnormal returns over the 2-day window, on average, but the difference between those and the returns for other types of events is only marginally significant ($p < .10$ for a 1-tailed test). Similarly, although events that were covered in the press did have statistically significant negative returns over the 5-day window, these were not significantly different from other events, thus providing little support for H2c. Overall, our full set of events was clearly linked to an average decline in stock prices, but the factors predicted to differentiate events had, at best, a limited effect.¹⁴

The possibility that campaigns might shape reputations, rather than just the bottom line of sales and stock performance, is assessed in Table 5. When looking at all rated firms, we find no evidence of anti-sweatshop campaigns shaping corporate reputations. Neither the occurrence of a campaign (H3a) nor the intensity of campaign activity (H3b), nor the filing of a lawsuit (H3c) had a statistically significant effect on firms' reputations relative to others in their industry. Instead, corporate reputations were fairly "sticky" over time and shaped mainly by levels of sales, consistent with research on the "financial halo effect" in corporate reputation ratings.

[Table 5, Effects of Anti-sweatshop Campaigns on Corporate Reputations, about here]

When we shift to H3d, which expected effects to be discernible among firms with positive reputations to begin with, we see some influence of anti-sweatshop campaigns. Models 5-8 look only at firms with higher than average reputation scores in the previous year. Model 5 finds no effect of campaign activity in general, but the coefficient for intensity is statistically significant in model 6, indicating that increases in the intensity of anti-sweatshop campaigns brought discernible decreases in relative reputation scores. In model 7, we find that the highest intensity campaigns were linked to an average decrease of approximately -0.22 points in relative reputation score. However, this effect is not substantively large: Being intensely targeted is predicted to reduce relative reputation score by only around 1/3 of a standard deviation in this group ($sd = .589$), from a mean of .60 to a still positive value of .38 points. Model 8 shows no evidence that lawsuits were more powerful in spoiling reputations than other tactics. Finally, model 9 looks only at firms whose relative reputation scores had declined from $t-2$ to $t-1$ or who had fallen off the *Fortune* reputation rating altogether during that period. We find no support for the idea (H3e) that anti-sweatshop campaigns are especially damaging to corporate reputations that are already on the decline. Instead, these firms' reputations are driven largely by recent changes in their sales, which allowed declining companies to recover quickly. Additional models not shown (for the sake of space) likewise find no evidence of more intense campaigns or lawsuits mattering for firms with a prior reputation decline.

Overall, these results demonstrate that anti-sweatshop campaigns did not really "spoil" good reputations but did slightly undercut them. Looking at particular cases, it is notable that Wal-Mart was one of the most heavily targeted firms from 1993-2000 but also stayed at the top of the reputational hierarchy in its industry throughout this period and climbed to number three overall by 2000. Though we do find evidence that anti-sweatshop activism affected reputations in the business community, it clearly did not fully penetrate this reputational order.

Table 6 examines a different way in which anti-sweatshop campaigns might shape external evaluations, here focusing on KLD's specialized ratings of CSR.¹⁵ We find strong support for H5. Firms that had recently been targeted by anti-sweatshop campaigns were far more likely to receive a "concern" rating from KLD, as shown in model 1. Similarly, the intensity of campaign activity is significantly associated with the likelihood of receiving a poor rating, as shown in model 2. Examining controls, firms were also more likely to receive a concern rating if they were among those that led the shift toward global production networks.

[Table 6, Effects of Anti-sweatshop Campaigns on Specialized Evaluations, about here]

These results suggest that anti-sweatshop campaigns did effectively shape the content of specialized ratings, perhaps because raters relied on information generated in anti-sweatshop campaigns. Though it is logically possible that activists and raters could have independently converged on the same judgments about particular firms, this is unlikely in practice, since anti-sweatshop campaigns preceded the ratings and CSR raters admit to having minimal capacities to independently gather primary data, especially early in the life of a particular rating (Igalens and Gond 2005; author interviews). Indeed, looking at KLD's report on Nike, one finds numerous references to anti-sweatshop organizations and campaigns in describing concerns about Nike's labor practices (KLD Research & Analytics 2007).

In sum, we find strong support for H1d (and to a lesser extent, H1c), since the presence and intensity of anti-sweatshop campaigns affected sales but only among particular types of firms. We find support for H2a, since our sample of key anti-sweatshop events generated, on average, negative abnormal stock returns. We find some support for H3d, since more intense anti-sweatshop campaigns did influence previously positive corporate reputation, though only to a small degree. Finally, we find strong support for H4, since the presence and intensity of anti-sweatshop campaigns influenced ratings of corporate social responsibility.

Conclusion

How does “naming and shaming” by social movements translate into “sticks and stones” and other consequences for their corporate targets? Analyzing the anti-sweatshop movement of the 1990s, we find that the processes were varied and sometimes powerful. Anti-sweatshop campaigns suppressed the sales of specialized firms with recognizable products, and major events diminished the stock prices of implicated firms. These findings are consistent with claims that social movements can inflict material damage on their targets through changes in both consumer and investor behavior. Anti-sweatshop campaigns also powerfully informed specialized ratings, and intense campaigns diminished previously positive corporate reputations. These findings speak to at least some of the ways in which social movements shape categorization and evaluation in organizational fields. While observers of the anti-sweatshop movement have often suggested that campaigns can matter to firms, our analysis is the first to systematically document these multiple effects. Scholars of movements and organizations have typically examined *either* material damage or field-level evaluation, but our research suggests that both may be important for understanding a given movement.

At the same time, our findings highlight several important limits and contours of these processes. Campaigns affected sales and reputation only for particular types of firms, not across the industry as a whole. Effects on corporate reputation were substantively small. Lawsuits, while an important part of the movement, did not have dramatically different effects than other tactics on reputation or stock performance. Such findings emphasize the importance of paying close attention to the scope conditions and nuances of theorizing about social movement effects on corporations

We recognize several methodological limitations of our findings. First, we have followed other scholars in using the *Fortune* reputation ratings, which are especially useful for tracking changes in reputation in the business community and have been shown to respond to contentious events (Love and Kraatz 2009). Since this is not necessarily the main audience social movements are trying to influence, our findings may be conservative estimates of social movements' impact on corporate reputations. Future work should explore the possibilities for developing or making accessible other longitudinal measures of reputation, so that scholars could better tap into firm reputation as perceived by consumers, potential employees, regulators, legislators, and institutional investment funds. Second, our findings contribute to the growing body of work arguing that social movements can influence stock prices, though the number

and scope of our events are more limited than in some other work (e.g., King and Soule 2007). Nevertheless, specificity in this type of analysis may have certain advantages, especially in allowing greater attention to event-specific abnormal returns and tests of their individual statistical significance; these paint a more mixed picture of our stock price effects than is gained through an exclusive focus on mean abnormal returns. Third, it would be premature to generalize the results from anti-sweatshop campaigns to other movements using similar strategies, such as environmental, GLBT, and conservative Christian groups. Nevertheless, in bringing new data to bear on multiple paths of influence, our analysis strengthens substantive understandings of the anti-sweatshop movement, contributes to a growing body of research on movements and organizations, and suggests a methodological template for research on other movements.

Taken together, our findings paint an intriguing portrait of the routes by which social movements influence corporations. While it is certainly only a limited number of consumers and investors that alter their behavior based on information from social movements, the number that do so may be sufficient to generate changes in sales and stock prices. The same cannot be said of the general business community, whose evaluations of firms appear largely unaffected by social movement campaigns. On the other hand, specialized rating bodies seem to rely heavily on information provided by or uncovered through social movements. We suspect that these routes intertwine, such that instances of sales declines provoke greater attention by investors, which may in turn generate demand for specialized rating bodies to rationalize the identification of “concerns” about firms and perhaps influence future investment activity. It would be naïve to assume that social movements have the power to fully control these processes as they unfold, but they do seem to have the power to nudge them in particular directions.

One reason social movements do not fully control these processes is that firms are not passive targets; they respond strategically and with more resources at their disposal. The anti-sweatshop movement led many apparel and footwear firms to adopt codes of conduct for their supply chains, send auditors to assess compliance, and provide a variety of assurances to concerned audiences. It is beyond the scope of this paper to assess how particular impacts of activism conditioned these responses, and systematic longitudinal data on these evolving responses during the 1990s is scant. But our research suggests that such an analysis would be wise to (1) attend to both material and categorical influences of social movements (and their interpenetration), (2) take organizational fields (not merely consumer perceptions) seriously as sites in which the credibility of various types of assurances is constituted, and (3) consider how the degree of damage (material or categorical) spurred by social movements influences the likelihood that firm responses go beyond the shallowest of assurances.

Notes

1. Though important in broad theoretical context, we do not focus on arguments about internal reform coalitions. These are important for GLBT, race- and gender-based activism within organizations, but there is little evidence of such coalitions forming around sweatshop issues in U.S. firms.
2. Among the 39 apparel, textile, or footwear firms that are known (from existing inventories) to have codes of conduct at some point between 1996 and 1999, most (61.5%) were implicated in anti-sweatshop campaigns but a striking 38.5% adopted a code of conduct without being singled out for sweatshop abuses.
3. By the same logic, one could also argue that social movement campaigns could “grease the slippery slope” of sales declines, though previous studies have found no evidence of such an effect (see King 2008). In supplemental models, we assessed whether anti-sweatshop campaigns’ effects on sales are amplified by prior sales declines and found no evidence of this.
4. Specifically, these are firms in the Compustat North American Industrial Annual File (all publicly traded corporations and other large firms that report to the SEC) with the appropriate SIC codes and a minimum of \$300 million in either total assets or total sales at some point in the 1990s. We restricted the analysis to relatively large firms to be confident that all companies would be covered in our data on social movement activity. Appropriate primary SIC codes are textiles (SIC 22, except those pertaining to rugs/carpets or yarn/thread), apparel manufacturing (SIC 23), footwear and accessories (SIC 302 codes pertaining to rubber footwear, SIC 31 codes pertaining to leather footwear, gloves, or handbags), wholesale apparel or footwear (SIC 513), general/mass retailers (SIC 53), apparel retailers (SIC 56), or catalog and mail order houses (SIC 5961). To construct an appropriate sample, we also read companies’ annual reports to confirm their relevance to apparel, textile, or footwear production, which led to the exclusion of several non-apparel catalog companies and firms with only tangential connections to apparel or footwear during the period of analysis (e.g., Claire’s Stores). Retailers were included only if there was evidence that they were responsible for the production of some “private label” apparel, textiles, or footwear—a common practice that was true of all but a handful of firms. We *included* one conglomerate firm (Sara Lee), which is a major player in the apparel industry (maker of Hanes, Playtex, and others) but categorized in a different SIC.
5. The primary utility of the secondary sources (especially Manheim 2001) was to report that a campaign was ongoing over a series of years (e.g. 1996-1998) when trade journals neglected to report on the “middle” years of a campaign. The secondary sources overwhelmingly confirmed what was found in the trade journal data—listing only one corporate target that was absent from the trade journals. The trade journal data were much more detailed than the secondary sources, and revealed many corporate targets that would have been overlooked if only using secondary sources. Our assumption is that if a company was not mentioned as a target in the trade journal articles *or* in the secondary literature, then it was not a target of movement activity during this period.
6. We examined the *New York Times* plus eight large newspapers that were available in Newsbank for the period of our analysis: *USA Today*, *Washington Post*, *Chicago Tribune*, *Philadelphia Inquirer*, *San Francisco Chronicle*, *Dallas Morning News*, *Minneapolis-St. Paul Star Tribune*, and *St. Petersburg Times*. We used keyword searches for “sweatshop(s)” and “child labor” to find coverage of anti-sweatshop campaigns in 1994 and 1996. We found no clear evidence of targeted companies that were not already coded as such. The press sources captured ¼ of the companies we had coded as targeted in 1994 and ½ in 1996. The one instance in which press sources captured material that went beyond our sources had to do with Reebok’s labor practices in China and Indonesia, but these articles were ambiguous about campaign activity within the U.S.
7. In the case of a campaign discussed in a secondary source but not in the trade journals in a given year, we counted this as equivalent to a single article.
8. Using a log transformation (after adding a constant of one) generates nearly identical coefficients and substantively identical results.
9. Formally, the model is $Y_{jt} = \mu_t + \beta X_{jt} + \gamma Z_j + \alpha_j + \varepsilon_{jt}$ where μ_t is an intercept, X_{jt} is a set of time-varying predictors (for firm j at time t), Z_j is the time-invariant dummy for each firm, α_j is the unobserved individual effect, and ε_{jt} is an error term (Allison 2009).
10. Missing data on globalization takes the effective sample size to 42, which does not alter the substantive findings.
11. Specifically, $R_{jt} = \alpha_j + \beta_j R_{mt} + \varepsilon_{jt}$ where R_{jt} is the predicted return for firm j on day t , R_{mt} is the return for the equally weighted portfolio of firms in the CRSP data on day t , β_j and α_j are firm-specific slopes and intercepts, respectively, and ε_{jt} is an error term.
12. Abnormal returns $s_{jt} = R_{jt} - (a_j + b_j R_{mt})$ where R_{jt} are the actual stock price returns for firm j on day t and $(a_j + b_j R_{mt})$ is the predicted value of returns for firm j on day t .

13. Models of sales and reputation with adjustments for 1st order autocorrelation and Baltagi-Wu tests generate substantively identical results.

14. We also tested to see if specialized firms experienced a greater penalty in the stock market than general retailers or department stores, extending the logic from our sales analysis. We found no evidence of this, since differences between these groups were small and far from significant.

15. We present models with random effects, but supplemental population-averaged models with robust standard errors produced nearly identical coefficients and substantively identical findings.

Table 1: Profile of Anti-sweatshop Campaigns Targeting Large, U.S.-based Apparel, Textile, and Footwear Firms, 1993-2000

<i>Characteristics of campaign activities</i>	<i>% of campaign-firm-years</i>
Sponsors	
Labor unions	69.4
Student groups	14.3
NGOs and advocacy organizations	88.8
National Labor Committee	34.7
Other NGOs	67.3
Location of focal factories	
Central America and Caribbean	55.1
Asia	30.6
U.S.	35.7
U.S. territories	22.4
Linked to child labor	15.1
Use of lawsuit	21.9

Table 2: Illustrating the Consequences of Campaigns: Pressure and Response among the 50 Largest Firms

<i>Participant in multi-stakeholder monitoring initiative, 2001-04</i>	<i>Targeted by activists, 1993-2000</i>		
	No	Yes	Total
No	22 (100%)	19 (68%)	41 (82%)
Yes	0 (0%)	9 (32%)	9 (18%)
Total	22	28	50

Table 3: Effects of Anti-sweatshop Campaigns on Sales: Fixed Effects Models, 1993-2000

<i>DV=log sales</i>	<i>All firms</i>			<i>Recognizable</i>			<i>Recognizable and specialized</i>		
Predictors	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
targeted by campaign	-.0156 (-1.136)			-.0192 (-1.385)			-.0366* (-2.174)		
intensity of campaign		-.0089 (-1.495)			-.0109* (-1.837)			-.0172* (-2.133)	
low intensity			-.0132 (-.869)			-.0163 (-1.063)			-.0349* (-1.990)
high intensity			-.0297 (-1.423)			-.0362* (-1.775)			-.0484** (-2.363)
log sales (t-1)	.4783*** (14.11)	.4787*** (14.10)	.4789*** (14.07)	.4758*** (12.80)	.4764*** (12.79)	.4765*** (12.76)	.4841*** (11.33)	.4853*** (11.36)	.4847*** (11.30)
leading advertiser/brand (t-1)	.0634*** (3.793)	.0637*** (3.813)	.0642*** (3.767)	.0611*** (3.595)	.0614*** (3.612)	.0620*** (3.571)	.0625** (3.002)	.0642** (3.159)	.0630** (2.975)
firm size	.4361*** (8.949)	.4359*** (8.943)	.4359*** (8.936)	.4404*** (8.023)	.4400*** (8.015)	.4401*** (8.007)	.4425*** (6.972)	.4415*** (6.962)	.4423*** (6.956)
1994	.0078 (.758)	.0077 (.751)	.0078 (.754)	.0146 (1.295)	.0145 (1.286)	.0145 (1.290)	.0094 (.674)	.0090 (.643)	.0094 (.669)
1995	.0168 (1.312)	.0168 (1.312)	.0170 (1.324)	.0172 (1.239)	.0172 (1.239)	.0174 (1.255)	.0092 (.536)	.0092 (.534)	.0093 (.538)
1996	-.0154 (-.812)	-.0154 (-.814)	-.0153 (-.809)	-.0074 (-.366)	-.0074 (-.368)	-.0073 (-.361)	-.0239 (-.936)	-.0244 (-.953)	-.0240 (-.939)
1997	-.0030 (-.139)	-.0030 (-.138)	-.0028 (-.127)	.0057 (.238)	.0057 (.240)	.0060 (.254)	-.0122 (-.379)	-.0126 (-.390)	-.0122 (-.376)
1998	-.0035 (-.180)	-.0036 (-.186)	-.0035 (-.178)	.0048 (.228)	.0046 (.220)	.0048 (.230)	-.0027 (-.101)	-.0030 (-.113)	-.0026 (-.0974)
1999	-.0172 (-.891)	-.0171 (-.892)	-.0167 (-.869)	.0020 (.0989)	.0021 (.106)	.0027 (.134)	-.0147 (-.599)	-.0156 (-.630)	-.0144 (-.583)
2000	-.0178 (-.799)	-.0178 (-.801)	-.0176 (-.790)	-.0070 (-.281)	-.0071 (-.284)	-.0067 (-.270)	-.0218 (-.688)	-.0223 (-.705)	-.0217 (-.687)
Constant	.8674*** (6.432)	.8660*** (6.428)	.8646*** (6.404)	.8832*** (6.139)	.8813*** (6.139)	.8796*** (6.114)	.7965*** (5.046)	.7944*** (5.062)	.7940*** (5.043)
N (firms)	168	168	168	147	147	147	101	101	101
N (firm-years)	1097	1097	1097	954	954	954	671	671	671
R-squared	.879	.879	.879	.888	.888	.888	.884	.884	.884

Notes: Robust t-statistics in parentheses. P-values for one-tailed tests for hypothesized effects, two-tailed for controls.

* p < .05 ** p < .01 *** p < .001

Table 4: Effects of Anti-sweatshop Events on Stock Returns: Cumulative Abnormal Returns for 39 Events

Set of events	11 day window (-5 to 5)	5 day window (-2 to 2)	2 day window (-1 to 0)
All events	.17%	-1.52%*	-1.26%*
P-value from Patell's Z (2-tailed)	(.7480)	(.0440)	(.0172)
P-value from bootstrap test (2-tailed)	(.3380)	(.0390)	(.0330)
Lawsuit	-.46%	-2.25%	-2.27%*
Other tactics	.56%	-1.06%	-.63%
Difference	-1.02	-1.19	-1.64
P-value from t-test of difference (1-tailed)	(.377)	(.233)	(.086)
NYT covered events	-1.62%	-2.74%*	-1.23%
Other events	.96%	-.98%	-1.28%
Difference	-2.58	-1.76	.05
P-value from t-test of difference (1-tailed)	(.223)	(.150)	(.487)

Note: P-values from 2-tailed test based on Patell's Z

* $p < .05$ ** $p < .01$ *** $p < .001$

Table 5. Effects of anti-sweatshop campaigns on corporate reputation: Fixed effects models, 1993-2000

<i>DV=rel. reputation score</i>	<i>All rated firms</i>				<i>Firms w/ positive rep. in prev. year</i>				<i>Firms w/ rep. decline</i>
VARIABLES	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
targeted by campaign	-.0079 (-.144)				-.0158 (-.271)				.0392 (.402)
intensity of campaign		-.0392 (-1.469)				-.0587* (-1.971)			
low intensity			.0179 (.293)				.0346 (.482)		
high intensity			-.1059 (-1.022)				-.2196** (-2.333)		
without lawsuit				.0334 (.462)				.0258 (.306)	
with lawsuit				-.1094 (-1.453)				-.0945 (-1.210)	
rel. reputation score (t-1)	.4982*** (6.582)	.5014*** (6.406)	.4925*** (6.578)	.4917*** (6.847)	.4645*** (3.601)	.4619*** (3.479)	.4584*** (3.810)	.4632*** (3.583)	.2526* (1.988)
Return on assets (ROA)	1.6646 (1.469)	1.6814 (1.477)	1.7212 (1.506)	1.7072 (1.494)	4.0631 (1.521)	4.1157 (1.558)	4.2088 (1.658)	4.1000 (1.543)	1.7072 (1.569)
Diff. in ROA (t-1 to t)	.4835 (.661)	.4563 (.607)	.4292 (.569)	.4276 (.565)	-.6253 (-1.107)	-.7847 (-1.419)	-.8997 (-1.579)	-.7482 (-1.330)	-.4159 (-.478)
sales	.0096*** (7.953)	.0098*** (7.638)	.0103*** (6.160)	.0099*** (8.376)	.0104*** (9.812)	.0108*** (9.735)	.0123*** (7.914)	.0108*** (9.730)	.0067* (1.732)
% change in sales (t-1 to t)	.3468 (1.257)	.3204 (1.162)	.3080 (1.103)	.3507 (1.339)	.0318 (.0701)	.0627 (.134)	-.0918 (-.197)	.0885 (.189)	1.7033*** (2.985)
Constant	-.2253*** (-4.447)	-.2121*** (-4.140)	-.2364*** (-4.399)	-.2357*** (-4.500)	-.2192 (-.984)	-.2016 (-.914)	-.2524 (-1.212)	-.2366 (-1.062)	-.3164*** (-4.733)
N (firms)	35	35	35	35	29	29	29	29	30
N (firm-years)	192	192	192	192	120	120	120	120	92
R-squared	.396	.398	.399	.400	.289	.299	.317	.296	.399

Notes: Robust t-statistics in parentheses. In additional models among firms with a prior reputation decline, measures of campaign activity do not approach significance. P-values for one-tailed tests for hypothesized effects, two-tailed for controls.

* p < .05 ** p < .01 *** p < .001

Table 6. Effects of anti-sweatshop campaigns on specialized evaluations, 1999-2001

<i>DV: first KLD "labor rights concern"</i>		
VARIABLES	Model 1	Model 2
targeted by campaign (prev. 2 years)	4.2676*** (3.354)	
intensity of campaign (prev. 2 years)		3.1650** (2.775)
firm size	.0994 (.269)	.0752 (.191)
early globalizer	2.6988* (2.056)	2.8170* (2.041)
domestic to global production	1.5734 (1.242)	1.8519 (1.383)
Constant	-3.8186 (-1.160)	-3.9217 (-1.114)
N (firms)	42	41
N (firm-years)	68	67
Wald Chi-squared test	25.22***	17.72***

Notes: Z-statistics in parentheses. P-values for one-tailed tests for hypothesized effects, two-tailed for controls.

* p<.05 ** p<.01 *** p<.001

Appendix A: Firms and events for event study analysis

Wal-Mart, Dec. 22 1992: Dateline NBC expose of Bangladeshi child laborers making Wal-Mart's private label clothing

Phillips Van-Heusen* and JC Penney*, May 22, 1993: Beginning of national wave of protests over factory in Guatemala

Leslie Fay, May 31, 1994: Apparel workers union first calls for boycott of Leslie Fay

Liz Claiborne, Sept. 22 1994: Senate testimony by a child laborer from Liz Claiborne contractor in Honduras

Wal-Mart, April 25, 1995: Beginning of a larger Bangladesh apparel boycott campaign, focusing largely on Wal-Mart

Target/Dayton Hudson, July 6, 1995: Protest at corporate headquarters over factory conditions and child labor in El Salvador and Honduras

Gap, July 21, 1995: Beginning of the campaign over the Mandarin factory in El Salvador

JC Penney, Aug. 9, 1995: Protest at JC Penney headquarters over sweatshops in Central America

Target/Dayton Hudson, Aug. 9, 1995: Subsidiary (Mervyn's) is linked to slaveshop in El Monte, CA (the first linkage to a major retailer)

Target/Dayton Hudson, Oct. 25, 1995: Charged in lawsuit over El Monte slaveshop

Gap, Dec. 1, 1995: Protests in NYC over decision to pull out of Mandarin factory in El Salvador

Wal-Mart and Kellwood, April 30, 1996: Initial expose of child labor in Kathie Lee Gifford line, sold exclusively at Wal-Mart and sourced through Kellwood

Nike*, July 15, 1996: Beginning of a major wave of store protests

May Dept. Stores, Aug. 2, 1996: Protests at stores in LA over links to Guess sweatshops

Federated, Aug. 2, 1996: Protests at Macy's in LA over links to Guess sweatshops

May Dept. Stores, Dec. 6, 1996: Large protest outside Lord & Taylor in Manhattan over domestic and foreign sweatshops

Federated, Dec. 6, 1996: Large protest outside Macy's in Manhattan over domestic and foreign sweatshops

Nike, Dec. 12, 1996: Anti-sweatshop protests at headquarters and stores

May Dept. Stores, Feb. 27, 1997 Demonstrations at May stores nationwide over sweatshops in private label brands

Nike, Sept. 27, 1997: Inner-city youth participate in "Great Sneaker Give Back" protest at Niketown

JC Penney*, Kmart*, and Wal-Mart*, Nov. 11 1997: Hard Copy exposé of sweatshops in Nicaragua

Nike, April 20 1998: *Kasky* lawsuit filed, charging Nike with lying about its factory conditions

Limited, Nordstrom, Gap, Target, May, Oshkosh B'gosh, Jones Apparel, Gymboree, Cutter & Buck, Liz Claiborne, The Dress Barn, Warnaco, JC Penney*, Sears*, Wal-Mart*, and Tommy Hilfiger*,; Jan. 13, 1999: Lawsuit filed over labor abuses in Saipan

Gap, March 6 1999: Major wave of student protests at Gap stores in various parts of the country, partially in response to recent lawsuit over labor abuses in Saipan

Kohl's, July 15, 2000: Beginning of a campaign over a factory in Nicaragua

Wal-Mart July 18, 2000: Imports of items from Burma revealed

Gap*, Aug. 13, 2000: Large protest at Gap store in L.A. during Democratic National Convention

Nike Dec. 6, 2000: Large protests at Niketowns in New York, Chicago, L.A., and other cities

Donna Karan, June 7, 2000: Protest at DNKY and announcement of class action lawsuit

Federated, June 17, 2000: Protest at Macy's over sweatshop labor in Regis Philbin clothing line

Kenneth Cole, Dec. 19, 2000: Protest at NYC store over factories in Burma

* event dropped from analysis due to confounding event

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