



# The disparate economic outcomes of stigma: Evidence from the arms industry

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## Abstract

**Research Summary:** Organizational stigma has been commonly associated with a number of negative economic externalities in prior literature, but the mechanism by which this occurs and the extent of the associated consequences have received little attention. We address these gaps by theorizing that stigmatizing labels damage the legitimacy of the target by highlighting a deviation from the expectations of relevant audiences. We also argue that the content and focus of stigmatizing labels, as well as the features of the stigmatizer audience deploying them, will affect the magnitude of the negative economic consequences of stigma. Through an analysis linking the condemnation of arms producers in the media between 1998 and 2016 to cumulative abnormal returns (CAR) in the stock market, we find broad support for our arguments.

**Managerial Summary:** This paper examines the economic impact of stakeholder criticism on firms, using data from the global arms industry to develop a typology of criticism based on its content, focus, and origin. We find that criticism can negatively affect a firm's stock market returns, particularly when those criticizing have the authority to condemn specific behaviors. Civil society entities like nonprofits or the media,

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politicians, and economic players such as investors each have unique authority to criticize harmful behavior, illegal behavior, and unethical affiliations, respectively. Understanding the different types of criticism and their potential economic consequences can help firms better manage stakeholder relationships and mitigate negative impacts on their financial performance.

#### KEY WORDS

arms producers, cumulative abnormal returns, labels, legitimacy, stigma

## 1 | INTRODUCTION

Stigma is harmful to organizations, as it is associated with a variety of unique challenges and negative economic externalities (Devers, Dewett, Mishina, & Belsito, 2009). Taking stock of this phenomenon, a growing number of studies (e.g., Helms & Patterson, 2014) have begun looking into how stigma impacts the performance, functioning, and economic valuation of organizations (e.g., Diestre & Santaló, 2020). Yet, there is now growing evidence that the extent to which organizations suffer negative economic consequences as a result of stigma varies greatly not only across, but also within settings. Even in industries characterized by pervasive stakeholder disapproval (Hudson, 2008), in fact, some firms find their economic bottom-line significantly impacted, whereas others suffer only minor consequences. While existing research has looked into the negative consequences of stigma—both in terms of its tangible consequences and how organizations cope with it (Durand & Vergne, 2015)—this body of work has yet to explicitly examine what factors might explain this variance in outcomes.

In this paper, we start from the premise that stigma is fundamentally rooted in instances of labeling-based condemnation from specific social actors. We argue that these expressions of disapproval translate into economic sanctions because their use damages the legitimacy of the target by highlighting a deviation from the expectations of relevant audiences (Suchman, 1995), leading to a generalized withdrawal of support for the organization, and ultimately to depressed economic valuations (Lamont, 2018). The specific facet of the organization's legitimacy that is put under threat, however, is likely to hinge on the content of stigmatizing labels, and the economic consequences associated with the latter are as well. Similarly, stigmatizer audiences (Aranda, Helms, Patterson, Roulet, & Hudson, 2023) are not interchangeable, inasmuch as they are associated with distinct domains of expertise and institutionalized roles of authority within society, so that market actors will perceive their efforts to vilify organizational targets differently depending on how stigmatizing labels are articulated and who articulates them. Exploring these complexities is paramount if scholars are to understand why the economic outcomes of stigma vary dramatically, and specifically why the associated penalties tend to be more severe under certain circumstances over others.

In the remainder of this paper, we present a mixed methods study (Teddlie & Tashakkori, 2011) of stigma and its negative economic impact on firms in the global arms sector. First, we briefly review the literature on stigma in organizational settings to lay out the

foundations of our theoretical framework. After discussing our empirical setting—the global arms industry—and its appropriateness for investigating the questions at hand, and building upon related work (Vergne, 2012), we then engage in an abductive analysis of the media coverage of arms producers through which we moved back and forth between our data and existing theory to make sense of our observations (Alvesson & Kärreman, 2007; Sætre & Van de Ven, 2021), to uncover what types of audiences seek to vilify firms in this setting, and how they do so. Drawing on this typology and building on the relevant literature, we then develop theory concerning the differential association of stigmatizing labels with negative economic consequences for arms producers in the form of context-specific hypotheses. Finally, we test our predictions by means of a study of cumulative abnormal returns (CAR) in the stock market and conclude by discussing the implications of our findings and directions for future research.

## 2 | LITERATURE REVIEW

In the past couple of decades, organizational stigma has been at the receiving end of a burgeoning amount of scholarly attention (Zhang, Wang, Toubiana, & Greenwood, 2021). As a result, the body of research on this topic exhibits remarkable breadth. In terms of organizational settings, for example, existing work has looked at men's bathhouses (Hudson & Okhuysen, 2009), abortion clinics (Augustine & Piazza, 2022), financial services firms (Roulet, 2019) and companies engaged in nuclear power generation (Piazza & Perretti, 2015), among others. Along similar lines, scholars have explored a range of related topics including broad examinations of the attributional origins of stigma, ranging from traumatic events such as bankruptcies (Sutton & Callahan, 1987) and instances of misconduct (Jonsson et al., 2009; Paruchuri & Misangyi, 2015) to more enduring forms of disapproval targeting the core activities the firm engages in or the customers it caters to (Hudson & Okhuysen, 2009). Yet other topics of inquiry include the potential organizational responses to stigma such as boundary work (Hudson & Okhuysen, 2009), disengagement (Durand & Vergne, 2015; Piazza & Perretti, 2015), and impression management (Aranda, Conti, & Wezel, 2021; Carberry & King, 2012; McDonnell & King, 2013).

While the negative economic consequences of stigma for organizations have been the subject of much work, theoretical and empirical alike, what is less clear is how such penalties materialize. Indeed, most of the work in this area has started from the premise that stigma is associated with negative social and economic consequences (Sutton & Callahan, 1987), often building the latter into the very definition of the construct. For instance, the most cited definition of organizational stigma by Devers et al. (2009: 157) describes it as a labeling-based “negative social evaluation that casts the firm specifically as what that group ‘is not,’ which leads stakeholders to disidentify with the organization and *to actively impose harmful social and economic sanctions on it*” (emphasis added).

Along the same lines, little or no empirical research has looked into what explains the variance in negative economic externalities that stigmatized organizations frequently experience. All too often, in fact, research has made the implicit assumption that being at the receiving end of disapproval from stakeholders is sufficient for the tangible consequences of such disapproval to materialize, without problematizing the ways in which these come into being. Yet, not all instances of condemnation are alike, and the degree to which economic exchange partners are receptive to stigmatization is likely a function of *who* the stigmatizer is as well as of *what* the object of vilification is. In turn, these factors may also affect partners' decisions to disengage



from the targeted organization (Helms & Patterson, 2014). The current state of research, however, has not yet begun to address these complexities, painting a picture of organizational stigma that is unilaterally focused on its targets. As a result, we remain unable to explain why the economic externalities of stigma vary dramatically, at times damaging the bottom line of targeted organizations while having relatively more minor consequences at other times.

Moving past this state of affairs, we argue, requires approaching stigma from an *agentic* standpoint: that is, not as something that organizations experience passively due to a core attribute or event, but rather as a byproduct of the fact that organizations are constantly judged and evaluated by audiences (Bitektine, 2011). In our case, we posit that the negative consequences of stigma are the direct consequence of audiences—functioning as stigmatizers—viliifying and condemning organizational targets (Helms, Patterson, & Hudson, 2019). Given a growing body of empirical evidence that the degree to which stigma is economically damaging to organizations can vary dramatically not only across (Grougiou, Dedoulis, & Leventis, 2016), but also within settings (Khessina, Reis, & Verhaal, 2021), doing so would be a useful first step towards a more nuanced theory of how stigma can beget economic damage. Therefore, in the remainder of the manuscript we seek to address the following overarching question: *what causes stigmatizing labels to translate into varying economic consequences for targeted organizations?*

### 3 | STIGMATIZING LABELING IN THE GLOBAL ARMS INDUSTRY

#### 3.1 | The empirical setting

The Stockholm International Peace Research Institute (SIPRI), a prominent observer of the arms industry, defines an arms producer as a firm that devotes a significant share of its activities to the design, manufacture, and selling of products and services intended specifically for military use (Markusen & Costigan, 1999). In 2019, global defense spending amounted to \$1.9 trillion, or nearly 2.2% of the gross world product. Despite the economic significance of the industry and the moral contestation associated with it, the economic impact of stigma on defense firms has, to our knowledge, not been investigated. Firms in the global arms industry are, however, uniquely suitable for investigating whether stigma might produce a negative market reaction—and if so, under what circumstances—for a number of reasons.

First, stigmatizing labels are highly prevalent in this sector and can be studied at scale (Vergne, 2012). Due to the morally contested nature of arms manufacturing and sale, as well as its association with war and death, firms in the arms industry frequently become the target of condemnation from various audiences (Durand & Vergne, 2015). Further, previous studies of stigma in this context (e.g., Durand & Vergne, 2015; Vergne, 2012) support our assertion that firms in the industry face widespread social disapproval. Second, compared to other stigmatized industries such as gambling, mixed martial arts (Helms & Patterson, 2014), or nuclear power generation (Piazza & Perretti, 2015), the greater prevalence of public firms in the arms industry provides substantial access to stock market data, which is crucial to our study. While the literature on organizational stigma has benefited from strong theorizing on the social and economic hardships inflicted by stigma (e.g., Devers et al., 2009), to date such economic externalities have yet to be quantitatively tested. Access to stock market data thus makes the global arms industry an excellent context to test the theoretically sound but largely taken-for-granted assumption that stigmatized organizations face economic hardships. Third, like in other stigmatized



industries (e.g., Helms & Patterson, 2014; Piazza & Perretti, 2015), stigmatization in the arms industry generally unfolds through media coverage. In this regard, the availability of detailed textual data in the form of media articles served as an excellent source for unearthing nuanced information about the accounts used by different audiences in the stigmatization of firms.

### 3.2 | Our approach

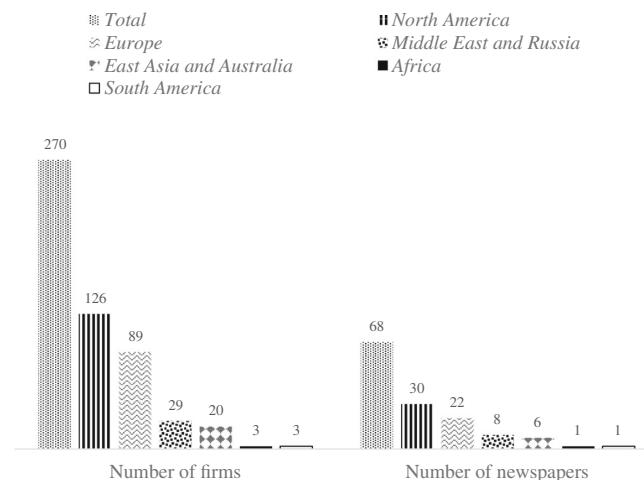
Following the lead of Vergne's (2012) prior work on stigmatized categories in the arms sector, we engaged in a mixed-methods study, integrating both qualitative and quantitative methods (Teddlie & Tashakkori, 2011). Mixed methods are appropriate for addressing questions that neither quantitative nor qualitative methods can answer independently. In our case, because stigmatizing labels and audiences are likely to be context-specific and no existing typology has been developed for the arms industry, we engaged in what is characterized as an exploratory sequential mixed method design (Creswell & Plano Clark, 2007). Specifically, a first empirical phase involved the abductive analysis of media coverage associated with firms in the arms industry to classify different types of stigmatizing labels, as well as to identify different audiences that vilify arms producers. A second empirical phase then involved a quantitative study of the impact of stigmatizing labels on the stock market performance of firms in the arms sector. The research design is mixed in that the classification of labels and audiences developed in our initial qualitative study formed the basis of our quantitative analysis of the impact of stigmatizing labeling on the stock market performance of arms producers.

### 3.3 | Abductive content analysis of stigmatizing labels in the arms industry

To explore the negative economic externalities of stigma and the nuances surrounding them, we assembled a comprehensive dataset of media articles covering the arms industry, which we leveraged to identify and classify stigmatizing labels aimed at arms producers. As a first step, we gathered data about global arms manufacturers from 1998 to 2016.<sup>1</sup> During this period, because of three major conflicts taking place in Afghanistan, Iraq and Syria, as well as several other long-running regional struggles such as the Israeli occupation of Gaza, the industry was subjected to much criticism in the media. A relatively long observation period (20 years) allowed us to test our hypotheses on a sufficiently large sample of media articles. Moreover, because there is no unique SIC code for the arms industry, and many arms manufacturers frequently report SIC codes that are unrelated to arms production, we hand-selected the firms in our sample from the two major sources that rank firms by their arms sales: the *Stockholm International Peace Research Institute (SIPRI)* and *Defense News*. Overall, our sample included all the

<sup>1</sup>Our first year of observation marks the end of the post-Cold War consolidation period in the global arms industry (Durand & Vergne, 2015). Since the end of the Cold War, support from nation-states has become weaker (Baum & McGahan, 2013; Harkavy & Neuman, 1994) and as a result, arms manufacturers have become more concerned with avoiding scrutiny and stakeholder challenges to their legitimacy. In the absence of the legitimizing role of the nation-state, stock market actors may also become more sensitive to stigmatizing judgments by external audiences, making the study of market reactions even more relevant during this period.

[Correction made on 5 May 2023 after first online publication: Introduction was removed on page 5 and the heading was renumbered accordingly.]



**FIGURE 1** Geographical distribution of firms in our sample and the newspapers used in our analysis.

firms that appeared at least once in these rankings between 2002 and 2016—a total of 270 firms.<sup>2</sup> Figure 1 offers additional details about the geographical distribution of the firms in our sample.

As a first step, using the online repository Factiva and following the methodological blueprint laid out in previous research (e.g., Durand & Vergne, 2015; Vergne, 2012), we searched for arms producers by name together with meaningful keywords such as critic\*, attack\*, under pressure\*, condemn\* (see Table A1 for the comprehensive list of keywords used in this step). In order to minimize the geographical, political and outlet-specific bias of media coverage, we selected 68 generalist news outlets that met the following criteria: (a) no ties to governments or to private entities involved with the arms industry; (b) broad circulation. Additionally, to ensure geographic representativeness, we sought to include outlets from across the world,<sup>3</sup> and we did our best to make sure that the number of outlets in each geographic area was commensurate with the number of defense firms from that area. Finally, we selected the outlets so as to be representative of a diverse set of political ideologies—for example, conservative versus liberal in the U.S., and left versus right in Europe. We complemented these with 25 newspapers and magazines specializing in defense (e.g., *Defense Daily*, *Jane's Defense Weekly*), accessed through Factiva, to preempt potential bias and increase the representativeness of our data. On the right side of Figure 1, we have detailed the geographical breakdown of generalist newspapers in our study. These steps, which were carried out by the first author, resulted in a sample of 2231 articles containing 1119 unique instances of stigmatizing labels.

With this collection of media articles in hand, we took an abductive approach (Roulet, 2015) as we conducted a content analysis of the material thus collected (Helms & Patterson, 2014), working with both data and existing theory to make sense of what we were observing (Alvesson & Kärreman, 2007; Sætre & Van de Ven, 2021) and developing the basis of what ultimately became a typology of stigmatizing labels in the arms industry. In so doing, we largely

<sup>2</sup>2002 is the first year for which SIPRI and Defense News published their rankings.

<sup>3</sup>In North America (e.g., *New York Times*, *Washington Post*, *The Globe and Mail*), Europe (e.g., *The Guardian*, *The Independent*, *Le Figaro*, *El País*, *The Moscow Times*), the Middle East (e.g., *Turkish Daily News*, *Jerusalem Post*), Africa (e.g., *All Africa*, *Business Day*) and the Asia-Pacific region (e.g., *The Australian*, *The Korea Herald*, *Times of India*, *China Daily*).

followed an iterative approach guided by prior research (Helms & Patterson, 2014; Phung, Buchanan, Toubiana, Ruebottom, & Turchick-Hakak, 2021; Roulet, 2015) to code stigmatizing labels. Specifically, each of the authors read and coded all the media articles, extracting keywords and identifying textual patterns that ultimately formed the basis of the broad theoretical concepts (i.e., stigma focus, stigma content, audiences) underpinning the typology of stigmatizing labels that we developed and the nuances of each of these concepts. Abductively, we drew from prior findings and theory on other stigmatized sectors to identify distinct, arms-industry-specific stigmatizing labels.<sup>4</sup> We employed a similar process to identify the unique audiences that stigmatize firms in the sector, cross-coding the different labels to categorize them by stigmatizer audience (Ferns, Lambert, & Gunther, 2022). Our analysis revealed the unique civil society actors (e.g., anti-arms trade social movement organizations) and political actors (e.g., politicians) that actively evaluated firms. This produced a unique set of stigmatizing labels and accounts deployed in the arms sector as well as audiences that utilized them. The keywords, patterns, and themes were then shared and discussed among the author team and assessed for similarities and differences in interpretation. Naturally, this entailed the continuous reassessment of the data as we converged towards a framework with adequate explanatory power (Siggelkow, 2007).

When disagreements arose within the author team, we reviewed the whole article from which a specific stigmatizing label was coded to have a more accurate understanding of the latter's context. Once the process was completed and distinct themes were identified, we asked an independent coder—a Ph.D. student from a different institution—to recode a random subsample (10%) of the accounts in our data into the themes we abductively developed. The inter-coder agreement level was 91%, a highly acceptable level (Hayes & Krippendorff, 2007). Our coding of the media articles ultimately yielded a context-specific typology of stigmatizing labels organized along the following dimensions that we examine quantitatively: (i) *who is being labeled*; (ii) *what is the content of labeling*; and (iii) *who the stigmatizer is* (i.e., who is doing the labeling). We discuss each of these in turn.

### 3.3.1 | Focus

First, through our coding of media articles, we noted key differences in their focus—that is, in who or what was being stigmatized. Specifically, we observed that while in some cases stigmatizing labels were directly focused on arms producers and their actions, in others, condemnation was directed at economic exchange relationship between arms producers and third parties. To make sense of these labels, we iterated between our coding and the literature. Ultimately, based on our reading of the literature on stigma by association (e.g., Goffman, 1963; Pontikes, Negro, & Rao, 2010; Roulet, 2015)—also known as courtesy stigma—we came to draw a distinction between *direct*, if aimed at arms producers, or *associative*, if directed at their exchange relationships with other actors. An example of the former from our data is “At the 18th and M offices of Halliburton Co., the energy firm once headed by Vice President Cheney, some in the crowd chanted, “Halliburton, war criminals!”” (Fern & Perlstein, 2003), while an example of the latter was “the organizers of the summer’s Glasgow Commonwealth Games have been

<sup>4</sup>For example, accusations of illegality have been levied in the mixed-martial-arts setting explored by Helms and Patterson (2014), where fights were often labeled as “lawless”. Claims of harm and harm-doing are similarly prevalent across the literature (see Amengual & Bartley, 2022; Helms & Patterson, 2014; Schrempf-Stirling, Palazzo, & Phillips, 2016).



accused of signing a ‘totally unacceptable’ deal with a major arms company to provide security for the event. [...] Anti-arms trade campaigners said the involvement of Selex ES in a major sporting event was immoral” (Milmo, 2014).

### 3.3.2 | Content

Next, we observed that stigmatizing labels also varied in terms of their content, that is, of *the specific accusations levied by audiences*. Here, we identified two main “lines of attack”, or themes, among the stigmatizing labels we coded. The first theme, *illegality*, emerged from accounts that accused arms producers of rule violations, fraud, bribery, and the leaking of secrets.<sup>5</sup> For example, a media report noted how a watchdog group that had been investigating Booz Allen Hamilton for some time had uncovered “shady practices by the company, including potentially illegal surveillance systems, corruption between company and government officials, warrantless wiretapping, and several other questionable surveillance projects” (Richmond, 2011). The second theme, *harm*, highlighted how the conduct of arms producers caused physical and social harm to stakeholders and, by extension, to society at large, emphasizing aspects such as death, killing, damage, torture, and the promotion of conflict, as well as global insecurity and instability. For example, Reprieve—a human rights charity that has been campaigning against the use of drones to *kill suspects*—named and criticized a subsidiary of General Electric as one of the companies producing such drones. By contrast, as we content-coded instances of associative stigmatizing labels—that is, aimed at economic exchange relationships between arms producers and third parties—our analysis did not reveal meaningful differences in terms of content. Rather, we found that audiences generally tended to problematize exchange relationships (e.g., investing in, working for, or being sponsored by arms producers) in terms of their perceived *inappropriate* and *unethical* nature—attributes that then extended to arms producers’ exchange partners.

In consulting the literature to compare the typology thus developed with existing work, we found our approach to be theoretically consistent with previous studies in the stigma literature, which further buttresses its validity. The accusations of harm and illegality we identified in the data, in particular, map onto the harm-based and lawlessness-based forms of stigma theorized by Helms and Patterson (2014). Meanwhile, associative stigmatization is also a common theme in studies of stigma, such as Jensen’s (2006) study of client defections from the scandal-stricken accounting firm Arthur Andersen (see also Pontikes et al., 2010). Accordingly, in what follows we classify stigmatizing labels along two main dimensions: (a) whether they are focused on arms producers or their exchange partners *who* is stigmatized; see also Table A2 in Appendix; and (b) whether the behavior of arms producers is shamed as being illegal or harmful, or its affiliations as inappropriate (*what* the content of the stigmatizing label is; see also Table A3 in Appendix).

### 3.3.3 | Stigmatizers

Lastly, we gathered information on which social actors utilized stigmatizing labels in our data (Helms et al., 2019). Based on our content analysis, we found that the main stigmatizers in our setting were: (a) *civil society actors* (i.e., journalists, activists, religious organizations);

<sup>5</sup>Since the focus of our study is on stigmatizing labeling, we code articles based on whether an allegation of illegal behavior is made, rather than whether a given action was actually illegal.



(b) *financial actors* (i.e., investors); and (c) *political actors* (i.e., politicians and elected officials). Overall, based on our typology of stigmatizing labels thus developed, stigma in the arms industry can be broken down based on its focus and content, as well as enacted by different actors serving as stigmatizers.

Examples of stigmatizing labels broken down by focus, target, and stigmatizer audience are reported in Tables A2–A4 in Appendix. Below, we develop testable, context-specific hypotheses concerning how stigmatizing labels will translate to negative economic consequences for arms producers, as well as the role that these labels' characteristics, and those of stigmatizers, play in shaping the extent of such consequences.

## 4 | THEORY DEVELOPMENT

### 4.1 | From stigmatizing labels to market penalties

A central argument of the organizational stigma literature is that the economic consequences of stigma are typically brought about by the economic exchange partners of the targeted actors, whether current or potential. This is well exemplified by Sutton and Callahan's (1987): p. 431 observation that stigma often produces ostracism from exchange partners in the form of "disengagement, reduction in the quality of participation, and bargaining for more favorable exchange relationships". Moreover, signaling theory arguments (Spence, 1974) hold that market actors rely on informational signals in determining whether to endorse, or transact with, certain firms over others (Kvåle & Murdoch, 2021; Suchman, 1995). Stigmatizing labels (Ashforth & Humphrey, 1997) are therefore interpretable by market actors as evidence of "dissatisfaction among stakeholders, and investors may see this dissatisfaction as a threat to a firm's reputation and legitimacy" (King & Soule, 2007: p. 417). Under such conditions, its current and potential transaction partners might view safeguarding their own reputation and reassuring their stakeholders as an especially pressing concern, making the prospect of economic exchange unappealing (Jensen, 2006) and generating avoidant responses (Jonsson et al., 2009). In turn, when market actors choose to avoid, attenuate, or withdraw from transactions in the wake of public stigmatization by audiences, they also jeopardize the targeted firms' access to key resources (Harel & Klement, 2007; Harris, Evans, & Beckett, 2011) leading to depressed economic valuations.

At a baseline level, we therefore argue that stigmatizing labels are a signal to a firm's economic supporters and potential investors that its legitimacy is in jeopardy—that is, the organization's actions have deviated from the social norms, values, and expectations of relevant audiences (Ashforth & Gibbs, 1990; Jonsson et al., 2009; Suchman, 1995). This deviation can increase uncertainty about the organization's ability to secure resources through exchange relationships (Devers et al., 2009; Flammer, 2013), leading to a discount in the market value of targeted firms. Our baseline hypothesis is thus:

**Hypothesis H0.** *The use of stigmatizing labels by audiences will elicit negative market reactions to arms producers.*

### 4.2 | The disparate economic outcomes of stigmatizing labels

After outlining our baseline prediction, we now elaborate on the conditions under which stigmatizing labels will be more (or less) strongly associated with negative economic outcomes.



Our starting argument here is that different kinds of stigmatizing labels threaten different facets of the target organization's legitimacy (Scott, 1981). Importantly, noting prior observations that the wealth of legitimacy typologies in the literature are not mutually exclusive and can be applied simultaneously in a single context (see Bitektine, 2011), we draw on two frequently embraced typologies (i.e., Scott, 2014; Suchman, 1995) to build out our argument. First, we argue that illegality-based labels—that is, those that paints the behavior of a firm as at odds with established rules and laws—jeopardize a firm's *regulative* legitimacy based on conformity to laws and rules (Scott, 2014) by problematizing its ability to conform to the regulatory demands of its environment. Next, harm-based labels—that is, those that depicts the behavior of the firm as harmful to society and inconsistent with its values—tarnish the firm's *normative* legitimacy (also referred to as moral legitimacy by Suchman, 1995), delineated by a "morally governed" (Scott, 2014: 60) evaluation of whether an "activity effectively promotes societal welfare" (Suchman, 1995: p. 579) by problematizing whether it is "doing the right thing" and contributing to, rather than hurting, social welfare. Finally, stigmatizing labels that depict exchange transactions between the firm and third parties as unethical and inappropriate affect the firm's *pragmatic* legitimacy that "rests on the self-interested calculations of an organization's most immediate audiences" (Suchman, 1995: p. 578) because labeling affiliations with a firm as inappropriate jeopardizes its acceptability as an economic exchange partner, making these affiliations potentially problematic.

Following Maguire and Hardy (2009), we make the argument that stigmatizer audiences are associated with both specific *subject positions*—institutionalized roles in society that enable them to engage in specific kinds of labeling (Berger & Luckmann, 1968; Bitektine & Haack, 2015; Suchman, 1995).<sup>6</sup> Because subject positions "provide the actors that occupy them with rights to speak and act" (Maguire & Hardy, 2009: 150; Maguire, Hardy, & Lawrence, 2004), and only "a limited number of subject positions are understood as meaningful, legitimate, and powerful" (Hardy, Lawrence, & Grant, 2005: 65), the impact of a given stigmatizing label is likely to be contingent on whether the audience articulating it does so from a subject position that warrants voice (Potter & Wetherell, 1997). This line of argument dovetails with established sociological (Baldassarri & Grossman, 2011; Roth, 1972) and psychological (Kelley, 1971) accounts of how individuals and organizations alike are perceived as legitimate when occupying particular roles of authority (Zelditch, 2004) or serving in particular functions in society (Durkheim, 1961) that have become "socially embedded in a system of actors whose social identities and interests impel them to recognize [those roles] as a power resource" (Hall, 1997: p. 594).

Based on the above line of argument, our first prediction is that illegality-based stigmatizing labels (Schwartz & Skolnick, 1962) will result in relatively more severe market consequences when they are articulated by political actors. This is because the firm's regulative legitimacy is likely to be more severely tarnished when labels of illegality are put forward by such actors. Not only are these actors best positioned to evaluate compliance with legal requirements (Kerwin & Furlong, 2018), but they are also able to conduct investigations and impose burdensome sanctions. And because the regulative legitimacy of the firm is likely to suffer greater damage under such conditions, this will be reflected into market actors' valuation of the firm, leading to a discount in market value. Thus:

**Hypothesis H1a.** *Stigmatizing labels characterizing an arms producer's behavior as illegal will elicit stronger negative market reactions when they are enacted by political actors.*

<sup>6</sup>Wiesenfeld, Wurthmann, & Hambrick (2008) refer to these as the *arbiters* of society.



Next, we posit that harm-based stigmatizing labels will result in relatively more severe market consequences when they are articulated by civil society actors. While illegality-based labels tarnish the firm's regulative legitimacy, or its alignment with the rules and laws that characterize its environment, accusing the firm of doing harm threatens the firm's normative legitimacy by questioning its ability to do what is morally "right." Yet, relative to illegality, which constitutes a violation of codified law and unambiguously falls within the purview of the government, what constitutes harm is somewhat subjective and socially constructed through the work of moral entrepreneurs (Becker, 1963). As a result, normative legitimacy comes from those actors who define what is "good and proper" (Ocobock, 2020: p. 519). And in this regard, civil society actors are the quintessential moral entrepreneurs, because such actors' role is to editorialize and evaluate the activities of firms with respect to the overall well-being of society (Abbott & Snidal, 2013), and as a result, they have been recognized as monitors and claimants of the harm that companies generate. As such, civil society actors have normative power in shaping what constitutes harmful behavior by firms and in calling out such behavior, with other actors—such as political and financial entities—generally lagging far behind. This is exemplified by the leading role played by activists, NGOs, and the media in calling out harmful corporate behavior time and time again. Civil society actors made environmental pollution into a social problem, leading to the creation of the EPA, and pioneered consumer advocacy (Rao, 1998) due to governmental inattention to product liability and deceptive advertising. They were also among the early movers in calling attention to the dangers of tobacco. Under such conditions, market actors will associate the firm's tarnished normative legitimacy with widespread condemnation from society, which may be indicative of worse economic prospects in the future (King & Soule, 2007), leading to a market devaluation of the firm. We therefore predict:

**Hypothesis H1b.** *Stigmatizing labels characterizing an arms producer's behavior as harmful will elicit stronger negative market reactions when they are enacted by civil society actors.*

Our final hypothesis concerns the relative effectiveness of what we have referred to as associative stigmatization—that is, the condemning of affiliations and transactions between arms producers and third parties as inappropriate. By problematizing the firm as an exchange partner, associative stigmatizing labels jeopardize its pragmatic legitimacy concerned with "the expected value" that "constituents perceive from transacting with the organization" (Suchman, 1995: p. 579). In particular, such labels create ambiguity regarding whether transacting with the firm is ultimately in the interest of current or potential exchange partners. Financial actors, however, are arguably the best positioned to articulate such labels because—being market actors—they have the knowledge base and expertise required to calculate the costs and benefits of doing so. It follows that labels coming from these "economically rational" and self-interested financial actors should be especially compelling, and thus more likely convince other financial actors (i.e., investors) to reconsider their own exchange relationships with the firm, relative to claims from actors who do not share these characteristics (such as civil society or political actors). In other words, because of the specific subject position they occupy, financial actors have referent power among broader market actors.

The application of such labels also indicates disagreement among economic actors within the field, *de facto* drawing a line between "virtuous" actors that see affiliating with arms producers as problematic and actors that do not, which might create incentives for the latter to reduce or eliminate their involvement (Jensen, 2006). Under such conditions, we argue that the



pragmatic legitimacy of the firm will be called into question especially strongly, resulting in widespread disengagement and ultimately in economic penalties for the firm in the form of a selloff. Thus:

**Hypothesis H2.** *Stigmatizing labels characterizing exchange relationships between arms producers and their exchange partners as inappropriate will elicit a stronger negative market reaction when they are enacted by financial actors.*

## 5 | THEORY TESTING

Having developed a typology of stigmatizing labels and stigmatizer audiences and having developed theory regarding the conditions under which stigmatizing labels will be associated with more or less severe economic penalties, we now move on to testing our predictions. To this end, we rely on stock market data. Seeking to build a sample of firms from around the world, we collected data from the Center for Research in Security Prices (CRSP) for U.S. firms and from Compustat Capital IQ-Global data for non-U.S. firms. Table 1 reports descriptive statistics and pairwise correlations for all the variables used in our main analysis as well as accompanying robustness checks.

### 5.1 | Event study methodology

Strategic management research has recently witnessed a substantial increase in the use of event-study designs to explore the effects on firm's stock market performance of a variety of discrete events, both positive and negative, such as eco-harmful (or eco-friendly) corporate behaviors (Flammer, 2013), the introduction of employee benefit plans (Groening & Kanuri, 2013), corporate crime (Song & Han, 2017), media coverage of organizational wrongdoing (Baker, Derfler-Rozin, Pitesa, & Johnson, 2019), firms giving testimony before Congress (Ridge, Ingram, Abdurakhmonov, & Hasija, 2019), the appointment of new executives (Gligor, Novicevic, Feizabadi, & Stapleton, 2021), and activist protests (Dorobantu, Henisz, & Nartey, 2017). Here, we chose to focus on stock market performance as our outcome variable both for consistency with prior work in this area (e.g., Baker et al., 2019; Dorobantu et al., 2017; Flammer, 2013) and because change in a firm's stock market performance in the aftermath of an event can capture the expected value of other potential penalties the firm may incur in the future as a result of the same event (Luo, Kaul, & Seo, 2018).

Event studies measure the effect of an unanticipated event on stock prices, where abnormal returns reflect shareholders' reactions to the availability of new information and are calculated by subtracting the expected return for the stock from its actual return (McWilliams & Siegel, 1997). Significant abnormal returns (positive or negative) indicate the average causal effect of an event on the value of the firm—that is, the presence of significant abnormal returns suggests that the event affected firm value (Hawn, Chatterji, & Mitchell, 2018). In our study, the event of interest is the audiences' stigmatizing labeling of firms in the arms industry, conveyed through media coverage. We follow prior literature (Baker et al., 2019; Flammer, 2013) and use the publication date of the corresponding article as the event date (day 0). We consider (0,1)—a two-day event window—as our primary

TABLE 1 Descriptive statistics and pairwise correlations.

Variables	Mean	S.D.	1	2	3	4	5	6	7	8	9	10	11	12	13
(1) CAR for (0,1)	-0.0034	0.026	1												
(2) Stigmatization volume	0.75	2.33	-0.04	1											
(3) Firm size	9.48	2.03	-0.01	0	1										
(4) Disapproval of peers	30.52	17.25	0.01	0.02	0.05	1									
(5) Disapproval in the past	2.14	3.29	0.04	0.05	0.13	0.05	1								
(6) Scandal	0.14	0.35	-0.04	-0.02	-0.01	-0.25	0.32	1							
(7) Media visibility	3.74	1.95	0.08	0.03	0.27	0.07	0.28	-0.17	1						
(8) Firm reputation	3.07	1.38	-0.02	-0.06	-0.01	-0.06	0.26	0	0.48	1					
(9) Firm performance	0.09	0.06	-0.12	-0.03	-0.18	-0.09	-0.08	-0.04	0.08	0.1	1				
(10) Market share	0.0033	0.025	-0.09	-0.03	0.48	0.05	-0.07	-0.05	-0.22	-0.18	-0.14	1			
(11) Sigma dilution	0.33	0.33	-0.04	-0.01	0.21	-0.13	-0.12	0.26	-0.64	-0.29	-0.21	0.22	1		
(12) Nonmilitary disapproval	3.21	5.64	0.02	0.1	0.11	-0.05	0.29	0.23	0.11	0.07	0.02	-0.05	0.16	1	
(13) Capital expenditure	5.72	1.96	-0.02	0	0.43	-0.03	0.11	0.04	0.21	-0.05	-0.11	0.48	0.25	0.14	1



event window. In our robustness checks, however, we account for the possibility that the content of stigmatizing labels might refer to events taking place on the day before the article was published by including the previous trading day (day -1) in the event windows—effectively by looking at (-1, 3), (-1, 1), and (-1, 2) windows (see MacKinlay, 1997).

We applied standard data filters to clean the initial repertoire of events classified as stigmatizing labeling and obtain the final sample required to run our event study. Out of 270 firms in our initial sample, for 77 we found no evidence of any stigmatizing labeling by audiences during our observation window. For the rest of sample—that is, 193 firms—we excluded events in the following cases: (1) other significant activities (e.g., leadership changes, earnings announcements, mergers and acquisitions, important external events such as terrorist attacks) that may also impact firms' CARs happened in a (-3, +3) time window relative to the event (see McWilliams & Siegel, 1997) (accordingly, 5 firms and 89 instances of stigmatizing labels were removed from our sample); (2) the company of interest was not publicly traded and hence information on its share price was not available (in this step 51 firms and 179 instances of stigmatizing labels were dropped); (3) no stock market information was available during the estimation period (subsequently, 11 firms and 84 stigmatizing labels were eliminated from our sample); and (4) multiple actors criticized the firm on the same day, hence making it impossible to attribute the drop in share price to a specific actor (this step led to the deletion of 13 firms and 233 instances of stigmatizing labels). After applying these criteria, our final sample comprises 534 events targeting 113 firms.<sup>7</sup> Of these, 170 levied stigmatizing labels at affiliates by association and 364 targeted arms producers directly; within the latter category, 168 were allegations of firms' harmful behavior and 196 concerned allegations of illegal behavior.

For each firm  $i$ , we computed abnormal returns using the market model. The market model assumes that the return to an individual stock is linearly related to the market return (Kabir, Cantrijn, & Jeunink, 1997). The coefficients  $\alpha_i$  and  $\beta_i$  of the market model are estimated by ordinary least squares over the period  $t = -250$  to  $t = -50$ , where  $t = 0$  was the day of the event (McWilliams & Siegel, 1997). Formally, we estimated the following equation:  $R_{it} = \alpha_i + \beta_i * R_{mt} + \varepsilon_{it}$ , where  $R_{it}$  is the return on the stock of company  $i$  on day  $t$ ,  $\alpha_i$  is the intercept,  $\beta_i$  is the systematic risk of stock  $i$ ,  $R_{mt}$  is the daily return of the equally weighted CRSP (or Compustat Capital IQ-Global) market portfolio, and  $\varepsilon_{it}$  is the daily risk-adjusted residual for firm  $i$ . The corresponding estimated return on the stock of firm  $i$  on day  $t$  is given by  $R^{\hat{}}_{it} = \alpha_i + \beta_i * R_{mt}$ . We then calculated the abnormal daily return (AR) of company  $i$  on day  $t$  as follows:  $AR_{it} = R_{it} - R^{\hat{}}_{it}$ . Next, we computed the cumulative abnormal returns (CARs) for each time interval by summing up the abnormal returns within the specific time window (e.g., [-1, 1]). Finally, to test the baseline hypothesis (i.e., whether stigmatizing labels affect the stock price of targeted firms), we assessed whether or not these CARs were statistically significant, by running four tests commonly used in event studies (i.e., Csect, Patell's Z, CDA, and StdCs). [Correction made on 5 May 2023 after first online publication: The text “onsulting the literature to compare the typology thusde” was removed.]

In robustness checks, we also complement the main analysis we carried out to test the baseline Hypothesis ( $H_0$ ) by experimenting with different asset pricing models and alternative windows, as well as by running an OLS regression with an additional explanatory variable (i.e., stigmatization volume). To examine the effect of interaction between audiences and the focus of stigmatizing labels on stock market reactions ( $H_{1a}$ ,  $H_{1b}$ ,  $H_2$ ), we test whether each

<sup>7</sup>Applying the filters described led us to drop about half of the stigmatizing judgments and 157 firms in the original sample; however, the resulting sample size is in line with other event studies of this type (e.g., Flammer, 2013).



type of label has a different impact on stock market reactions when it is deployed by a given audience (e.g., civil society actors) vis-à-vis by the remaining audiences.<sup>8</sup> We complement the analyses for H1a, H1b, and H2 by running the same analyses with OLS models and control variables. Finally, we ran several additional analyses to check the robustness of our results.

## 5.2 | Market reactions to stigmatizing labels

Our event study analysis starts with a test of the baseline hypothesis, which concerns whether the market will react negatively to instances of stigmatizing labels aimed at arms producers. The results are presented in Table 2. For the selected event window (i.e., [0,1]), we report the mean CAR as a percentage, along with the counts of positive and negative individual CARs, corresponding statistical parameters, and *p*-values in parentheses. The standard event study methodology assumes that the sample consists of independent events, but since stigmatizing labels may cluster around specific dates (e.g., firms' annual meetings), this assumption may be violated. Therefore, we derive Z-statistics using four tests: (a) the cross-sectional test (CSect), where the standard error for each date (or window) in event time is computed across securities, not across time; (b) the Patell's test (referred to as a standardized abnormal return test), which assumes cross-sectional independence of returns; (c) the time-series standard deviation test (CDA), using Brown & Warner's (1980, 1985) "crude dependence adjustment," where the standard error is computed from the time series of portfolio mean abnormal returns during the estimation period; and (d) the standardized cross-sectional test (StdCs), where we compensate for a possible variance increase on an event date by incorporating a cross-sectional variance adjustment. The standardized cross-sectional and Patell tests are similar to each other, aside from their differing cross-sectional variance adjustments (Riley, Michael, & Mahoney, 2017). The event window of interest is (0,1). In support of Hypothesis H1, the average CAR is negative, exhibiting a 0.35% drop (*p* = .002) after each instance of a stigmatizing label by audiences. This percentage is -0.34, -0.36, -0.43, and -0.32 for alternative event windows (i.e., (-1,1), (0,2), (-1,2), and (-1,3)) as reported in Table A7 in the Appendix. A substantial majority of the 534 individual CARs are negative in (0,1) event window (301 negative vs. 233 positive) and follow the same pattern in all the alternative event windows (e.g., 294 negative vs. 240 positive for (-1,1) window reported in Table A7).<sup>9</sup> Given that on average each firm is at the receiving end of about 5 (534/113 = 4.72) instances of stigmatizing labels in our sample, the average drop in a firm's stock price is 1.75%. In the case of a firm such as Lockheed Martin, with a market value of \$117 billion, the order of magnitude of this loss would therefore be approximately \$2 billion (as of January 2020).

<sup>8</sup>We also ran a set of analyses removing those data points for which journalists were the stigmatizer audience. Because the media is the vehicle through which negative evaluations are conveyed, in fact, the inclusion of journalists as stigmatizers is potentially problematic. The results (available from the authors) remain consistent with those in our main analyses, indicating that our findings are not driven by the inclusion of journalists as a stigmatizer audience.

<sup>9</sup>To explore the recovery time of the stock price after a stigmatizing event, we tested the effect of the labeling events on stock market reactions in (1,2), (2,3), and (3,3) windows. The average drops in the stock price are respectively -0.22 percent with *p*-value = 0.031, 0.04 percent with the *p*-value = 0.716, and 0.12 percent with *p*-value = 0.121. These results indicate that the stock price starts to recover about three days after a stigmatizing labeling event. This number is consistent with findings of similar event studies in strategy and finance research (e.g., Baker et al., 2019; Flammer, 2013; Luo et al., 2018).

**TABLE 2** The effect of stigmatizing labels on firms' CARs (H0) with (0,1) window

	Percentage mean	Numbers (-/+)	CSeC test	Patell Z	CDA test	CtdCs Z
Event window (0,1)	-0.35%	534: 301/233	-3.14 (.002)	-2.68 (.003)	-2.95 (.003)	-2.66 (.008)

Note: All tests are two-tailed except for Patell Z. *p*-values in parentheses.

### 5.3 | Market reaction to stigmatizing labeling by specific audiences

In Hypotheses H1a, H1b, and H2 we test whether the market will react differently to different forms of stigmatizing labels based on which audiences articulate them. As shown in Table 3, we find support for these hypotheses. Consistent with H1a, in fact, we do find that stigmatizing labels that shame the firms' behavior as illegal leads to a negative market reaction when it is done by political actors—an average drop of -0.61% ( $p = .030$ ), while the average drop in CARs when this type of stigmatizing labeling is carried out by other actors is not separable from zero ( $p = .698$ ). These coefficients are also statistically different from each other ( $p = .026$ ). We observe similar results when considering alternative event windows with an average drop of 1.07 ( $p = .003$ ), 0.81 ( $p = .062$ ), 0.70 ( $p = .056$ ), and 0.93 ( $p = .056$ ) percent for (-1,1), (-1,2), (0,2), and (-1,3) windows, when political actors shame arms producers' behavior as illegal; results are otherwise nonsignificant (Table A8 in the Appendix). Concerning H1b, shaming arms producers' behavior as harmful leads to 0.60% drop in CARs when it is done by civil society actors ( $p = .002$ ). However, the average drop in CARs when this type of stigmatizing label is used by other actors is not different from zero ( $p = .187$ ), with the coefficient for the former not being statistically different from that of the latter ( $p = .058$ ). Although this result does not support H1b, when considering alternative event windows we find strong support for H1b with the average price drop of 0.42 ( $p = .016$ ), 0.44 ( $p = .036$ ), 0.76 ( $p = .000$ ), and 0.32 ( $p = .070$ )

**TABLE 3** The interactive effects of audiences and stigmatizing labels on firms' CARs (Hypothesis H1a, Hypothesis H1b, and Hypothesis H2 with (0,1) window

Event window (0,1)	Percentage mean	Numbers (-/+)	CSeC test	Patell Z	CDA test	CtdCs Z
Illegality/political actors	-0.61%	57: 35/22	-2.83 (.030)	-1.49 (.068)	-1.95 (.051)	-1.88 (.060)
Illegality/nonpolitical actors	0.09%	139: 64/75	0.39 (.698)	0.283 (.388)	0.432 (.666)	0.30 (.764)
Harm/civil society actors	-0.60%	135: 83/52	-3.22 (.002)	-1.78 (.037)	-2.12 (.037)	-1.91 (.059)
Harm/other actors	-0.66%	33: 18/15	-1.35 (.187)	-1.37 (.084)	-1.44 (.162)	-1.43 (.165)
By association/financial actors	-0.67%	53: 39/14	-2.65 (.011)	-2.20 (.014)	-2.60 (.013)	-2.40 (.021)
By association/nonfinancial actors	-0.21%	117: 62/55	-0.80 (.428)	-0.88 (.188)	-1.35 (.185)	-0.885 (.381)

Note: All tests are two-tailed except for Patell Z. *P*-values in parentheses.

percent for  $(-1,1)$ ,  $(-1,2)$ ,  $(0,2)$ , and  $(-1,3)$  windows, when civil society actors shame arms producers' behavior as harmful, and nonsignificant share drops otherwise (Table A8,  $p$ -values for the statistical difference between coefficients are respectively .001, .002, .000, and .000). Finally, we observe that shaming affiliations between arms producers and third parties as inappropriate leads to a substantial drop in CARs when it is done by financial actors (the average reaction is  $-0.67\%$ ,  $p = .011$ ). However, when this is done by other actors the average drop in CARs is not separable from zero ( $p = .426$ ), lending empirical support to H2 ( $p = .033$  for the statistical difference between coefficients). Once again, when running the same analyses with alternative event windows, we find similar results with an average drop of  $0.91$  ( $p = .007$ ),  $1.30$  ( $p = .016$ ),  $1.01$  ( $p = .048$ ), and  $1.25$  ( $p = .027$ ) percent for  $(-1,1)$ ,  $(-1,2)$ ,  $(0,2)$ , and  $(-1,3)$  windows, when inappropriate affiliations are shamed by financial actors and nonsignificant results otherwise (Table A8).

We supplement the analyses presented so far by running OLS regressions with three dummy variables and several other control variables that can influence arms producers' CARs. We control for *firm size* (log of firm assets), *performance* (return on assets), *media visibility* (logarithm of total number of articles covering a firm's activities), *reputation* (position in each of the SIPRI rankings' quartiles), *market share in military business* (ratio of focal firms' military sales to industry military sales), *stigma dilution* (the ratio of coverage of the firm's commercial activities over the coverage of the firm's total activities) (Vergne, 2012), *nonmilitary disapproval* (number of audiences' criticism of firm's actions and behaviors unrelated to their military business), *disapproval of peers* (total number of audiences criticism of other firms in the arms industry), and *capital expenditures* (log transformed). All the control variables were lagged by 1 year relative to the dependent variable. We also included year dummies to account for temporal heterogeneity throughout our observation window.

In Table 4, Models 1 through 3, we find results consistent with the predictions made in Hypothesis H1a, Hypothesis H1b, and Hypothesis H2. In Model 1, we regress firms' CARs on the control variables and a dummy variable that takes the value of 1 when political actors shamed arms producers' behavior as illegal and 0 when other actors did the same. The coefficient of this dummy variable (i.e., illegality-based stigmatizing labels by political actors vs. nonpolitical actors) is negative and significant ( $p = .024$ ), supporting H1a. Similarly, in Model 2, the coefficient of the dummy variable that takes the value of 1 when civil society actors shamed arms producers' behavior as harmful and 0 when other actors did the same is negative and significant ( $p = .011$ ), lending support to H1b. Finally, in Model 3, and once again in support of the prediction made in H2, the coefficient of the dummy variable that takes the value of 1 when financial actors shamed affiliations with third parties as inappropriate and 0 when other actors did the same is negative and significant ( $p = .035$ ). We repeated these analyses with alternative windows of  $(-1,1)$ ,  $(2,1)$ ,  $(0,2)$ , and  $(-1,3)$  and found support for our hypotheses. For example, for the  $(-1, 2)$  window, the coefficients of dummy variables for illegality stigmatization by political actors versus nonpolitical actors, harmful stigmatizing labels by civil society actors versus noncivil society actors, and affiliate shaming by financial actors versus non-financial actors were negative and different from zero with  $p$ -values equal to 0.055, 0.010, and 0.002 respectively (results for alternative windows are reported in Table A9 in the Appendix).

## 5.4 | Robustness checks

To ensure that our findings are not spurious and that the negative market reaction results from audiences' stigmatizing labels, we conducted several tests to check the robustness of our results.



TABLE 4 Regression analyses of hypotheses H1a, H1b, and H2 with (0,1) window

	<b>1</b>	<b>2</b>	<b>3</b>
	b/se/p	b/se/p	b/se/p
Firm size	0.00163 (.00426) (.7091)	0.00849 (.00278) (.0042)	0.00321 (.00347) (.3609)
Disapproval of peers	-0.00004 (.00031) (.9051)	0.00031 (.00014) (.0352)	0.00001 (.00015) (.9313)
Past disapproval	0.00003 (.00068) (.9704)	0.00058 (.00112) (.6052)	0.00052 (.00073) (.4755)
Scandal	0.00799 (.00978) (.4301)	0.00835 (.00646) (.2047)	-0.01440 (.01547) (.3571)
Media visibility	0.00205 (.00123) (.1220)	-0.00065 (.00280) (.8182)	0.00053 (.00225) (.8133)
Firm reputation	-0.00222 (.00264) (.4160)	-0.00496 (.00165) (.0048)	-0.00092 (.00164) (.5771)
Firm performance	0.13673 (.08320) (.1262)	-0.00252 (.04575) (.9564)	-0.01317 (.00149) (.0000)
Market share	-0.07429 (.05093) (.1703)	-0.00432 (.10864) (.9685)	-0.15145 (.06639) (.0277)
Stigma dilution	0.00868 (.01098) (.4445)	-0.01656 (.01162) (.1630)	0.01663 (.01259) (.1934)
Nonmilitary disapproval	-0.00084 (.00066) (.2235)	0.00033 (.00023) (.1716)	0.00037 (.00037) (.3283)
Capital expenditures	-0.00084 (.00368) (.8231)	-0.00560 (.00394) (.1636)	-0.00518 (.00393) (.1953)
Illegality content: political actors versus nonpolitical actors	<b>-0.00798</b> <b>(.00308)</b> <b>(.0236)</b>		



TABLE 4 (Continued)

	1	2	3
Harm content: civil society versus noncivil society actors		<b>-0.01171</b> (.00436) (.0108)	
Stigmatizing labeling by association: financial versus nonfinancial actors			<b>-0.00681</b> (.00312) (.0348)
Constant	-0.05133 (.03005) (.1133)	-0.02400 (.01862) (.2055)	-0.03386 (.04303) (.4358)
Observations	196	167	119
R <sup>2</sup>	0.169	0.195	0.550
Adjusted R <sup>2</sup>	0.023	0.024	0.417

Note: Standard errors and *p*-values in parentheses.

#### 5.4.1 | Alternative asset pricing models and placebo event windows

We examined the robustness of our base line finding to the choice of asset pricing models (Table A5), as well as the choice of placebo event windows (Table A6).

#### 5.4.2 | Alternative event windows

Following the standard practice of testing results with alternative windows in event studies, we examined the robustness of our results to several choices of alternative windows. Especially, to capture any leakage of information to investors before an article appears in the media, and to gauge the (relatively) longer-term consequences of stigmatizing labels on stock market reactions, we decided to repeat all of our main analyses with an *alternative long window* (i.e.,  $(-1,3)$ ). The results of these analyses are also reported in Tables A7 to A9 in Appendix and are consistent with our predictions for the baseline hypothesis as well as with H1a, H1b, and H2.

#### 5.4.3 | Accounting for precipitating events

One potential concern inherent to our empirical setup is that stigmatizing labels can occasionally be associated with the occurrence of certain precipitating events, so that the negative reaction of investors may be driven by these, rather than by stigmatizing judgment itself. For example, the breaking out of an armed conflict in which an arms manufacturer's products are used may serve as the precipitating event for the stigmatization of the company by various audiences. To address this potential confounding effect in our analysis, we combed through all the



instances of stigmatizing labels in our sample to see if any precipitating events could be identified. Out of 534 stigmatizing labels, 56 instances did not have any precipitating event. An example of such instance, Raytheon was shamed as “merchant of death” in the *Sunday Times*, but the article made no reference to any specific event to justify the use of this label. Out of the remaining data points for which a precipitating event could be identified (i.e., 478 stigmatizing labels), we then excluded those instances for which the time difference between the precipitating event and the stigmatizing label is less than a month (a total of 45 instances).<sup>10</sup> As the market reaction to any significant event usually dissipates within a few days, the choice of a one-month window between precipitating and focal stigmatizing labeling events represents a conservative approach. We reran all the previous analyses with the same event window (i.e., [0,1]) even though the approach described above left us with fewer observations. Results of the baseline hypothesis as well as hypotheses H1a, H1b, and H2 are reported in Tables A7, A8, and A9 and are very similar to what we found in our main analysis, supporting all our predictions.

#### 5.4.4 | Stigmatization volume

We also carried out separate regression analyses to rule out alternative explanations for our baseline hypothesis in more depth. Since stigmatizing events produce a negative market reaction, it is plausible that this effect may become stronger when these instances of stigmatizing labels appear in a greater number of media outlets (Piazza & Perretti, 2015). In Model 1 of Table 5, therefore, we tested this claim by including a variable for *stigmatization volume*—defined as the number of media outlets carrying a given stigmatizing judgment—as the explanatory variable and controlling for several other factors while also including year dummies. Consistent with our baseline hypothesis, the coefficient of stigmatization volume is negatively associated with CARs in Model 1 ( $p = .020$ ). In Table A10 in the Appendix, we repeat these analyses with alternative windows and observe similar results for this robustness test. For example, in the (-1,3) and (-1,1) event windows, the coefficient of the stigmatization volume is negative and different from zero with  $p$ -values respectively equal to 0.008 and 0.000.

#### 5.4.5 | Stigmatizing labels simultaneously targeting multiple arms producers

In our analysis, we noticed that some stigmatizing labels appeared to target multiple firms simultaneously; we therefore split our sample to account for these two types of stigmatizing labels—that is, targeting a single arms producer or multiple arms producers—separately. 371 instances stigmatizing labels fell within the former category with a 0.38% average decrease in firms' CARs ( $p = .008$ ). As for the latter category, we accounted for 163 instances, with an average drop in CARs of -0.27% ( $p = .097$ ). However, we did not find any statistical difference between the CARs of these two types of stigmatizing labels ( $p = .605$ ). Results are reported in Model 2 of Table 5. We then repeated this analysis by including control variables and creating a dummy variable that takes the value of 1 if a stigmatizing label targets multiple firms and

<sup>10</sup>For example, the firms' presence in Iraq during the Iraq war, was a precipitating event for activists' shaming of firms such as Halliburton. However, in many cases, the date that Halliburton started its activities in Iraq (i.e., 2004) was far from the date that the firm was shamed by activists because of its participation (i.e., 2006). Because of this long window between the precipitating event and the stigmatizing judgment, we did not remove such instances from our sample.



TABLE 5 Supplementary analyses for the (0,1) window

	1 stigmatization volume	2 stigmatization of peers	3 yearly time trend	4 decade time trend	5 9/11 influence	6 Iraq controversy influence
Firm size	0.00020 (.00408) (.9631)	-0.00097 (.00381) (.7999)	-0.00018 (.00298) (.9515)	-0.00017 (.00299) (.9536)	0.01286 (.01158) (.2700)	-0.00017 (.00588) (.9779)
Disapproval of peers	-0.00004 (.00004) (.2966)	-0.00004 (.00018) (.8420)	-0.00006 (.00008) (.4556)	-0.00006 (.00008) (.4150)	-0.00008 (.00075) (.9157)	-0.00007 (.00012) (.5550)
Past disapproval	0.00034 (.00024) (.2107)	0.00040 (.00050) (.4268)	0.00036 (.00041) (.3778)	0.00037 (.00042) (.3826)	-0.00129 (.00207) (.5355)	0.00068 (.00012) (.0002)
Scandal	-0.00459 (.00174) (.0462)	-0.00088 (.00483) (.8555)	-0.00477 (.00467) (.3076)	-0.00465 (.00470) (.3224)	-0.00795 (.01389) (.5689)	-0.00354 (.00298) (.2593)
Media visibility	0.00166 (.00193) (.4277)	0.00162 (.00131) (.2173)	0.00174 (.00126) (.1687)	0.00171 (.00125) (.1708)	-0.00059 (.00684) (.9311)	0.00280 (.00090) (.0097)
Firm reputation	-0.00172 (.00072) (.0616)	-0.00166 (.00134) (.2188)	-0.00174 (.00108) (.1101)	-0.00172 (.00108) (.1132)	-0.00689 (.00391) (.0816)	-0.00248 (.00131) (.0849)
Firm performance	-0.05402 (.05894) (.4015)	-0.06512 (.04911) (.1889)	-0.05788 (.03534) (.1020)	-0.05851 (.03521) (.0971)	-0.07155 (.04231) (.0945)	0.03766 (.04006) (.3674)
Market share	-0.08992 (.04231) (.0869)	-0.10276 (.04595) (.0283)	-0.08682 (.06542) (.1850)	-0.08739 (.06564) (.1837)	30.58389 (75.40279) (.6861)	-0.01971 (.06221) (.7572)
Stigma dilution	0.00239 (.01048) (.8289)	0.00245 (.00829) (.7682)	0.00213 (.00687) (.7563)	0.00192 (.00683) (.7794)	-0.01429 (.02880) (.6211)	0.00840 (.00757) (.2906)
Nonmilitary disapproval	0.00009 (.00015) (.5651)	0.00001 (.00020) (.9725)	0.00008 (.00017) (.6512)	0.00008 (.00017) (.6456)	0.00018 (.00078) (.8179)	-0.00057 (.00032) (.1018)
Capital expenditures	-0.00075 (.00320) (.8250)	0.00050 (.00354) (.8887)	-0.00037 (.00280) (.8944)	-0.00037 (.00281) (.8941)	-0.01605 (.01116) (.1541)	0.00178 (.00525) (.7412)



TABLE 5 (Continued)

	1 stigmatization volume	2 stigmatization of peers	3 yearly time trend	4 decade time trend	5 9/11 influence	6 Iraq controversy influence
Stigmatization volume	<b>-0.00491</b> (.00136) (.0152)					
Peers also targeted versus peers not targeted		<b>0.00452</b> (.00278) (.1085)				
Time trend (yearly)			<b>-0.00005</b> (.00027) (.8607)			
Decade dummy				<b>-0.00005</b> (.00222) (.9805)		
Sep 11 dummy					<b>-0.00437</b> (.01051) (.6783)	
Iraq controversy dummy						<b>-0.00464</b> (.00224) (.0620)
Constant	0.00431 (.02359) (.8622)	0.00011 (.02337) (.9961)	0.00663 (.01562) (.6713)	0.00640 (.01598) (.6891)	0.00803 (.04274) (.8514)	-0.01500 (.02267) (.5217)
Observations	532	532	532	532	97	271
R <sup>2</sup>	0.044	0.072	0.048	0.048	0.215	0.084
Adjusted R <sup>2</sup>	0.023	0.018	0.026	0.026	0.103	0.042

Note: Standard errors and *p*-values in parentheses.

0 otherwise. As reported in Model 2 of Table 5, the coefficient for this dummy variable is not separable from zero ( $p = .109$ ), showing, while investors react negatively to both types of stigmatizing labels, there is no meaningful difference between how investors react to said labels targeting multiple firms vis-à-vis a single firm.

#### 5.4.6 | Time trends

To check whether our results are contingent on time trends or time-dependent industry-level shocks, we conducted several robustness checks reported in Models 3 and 4 of Table 5. All

models are reported with control variables. In Model 3, we tested the effect of time trends on stock market reactions (CARs) by means of a linear time variable (i.e., year as a numeric variable). The coefficient of the time trend variable was not separable from 0 ( $p = .861$ ), suggesting that the effect of stigmatizing labeling on CARs does not vary over time. We repeated the analysis with a decade dummy which took the value of 0 for the years before 2008<sup>11</sup> and 1 for the years after. As shown in Model 4, the coefficient is not statistically separable from 0 ( $p = .981$ ), suggesting no significant differences in investors' reaction to stigmatizing labels were observed across different decades.

#### 5.4.7 | Significant controversies

We also tested whether the occurrence of September 11 attacks had an effect on market reactions to stigmatizing labels. Research has showed that after 9/11, the stigma associated with arms production diminished to some extent (Vergne, 2012). Hence, it is plausible that investors may have become less sensitive to audiences' stigmatization of arms' manufacturers—thus becoming inclined to punish firms less severely—in the aftermath of 9/11, compared to before. We thus created a dummy variable that took the value of 1 for stigmatizing labeling events happening in the months of 2001 prior to the attack and in 2000, and 0 for those happening after the attack in 2001 and 2002. The coefficients of this dummy variable in Model 5 of Table 5 are negative and different from 0 ( $p = .089$ ), confirming that stigmatizing labels before September 11 yielded more negative reactions by investors.

Finally, the Blackwater controversy in Iraq was another event that received negative publicity from different actors and brought many private military contractors under heightened scrutiny (Phillips, 2007). Here, too, it is plausible to assume that this event may have magnified negative perceptions of arms producers. We thus created a dummy variable that took the value of 1 for stigmatizing labels taking place in the months after to the controversy (in 2007) and those in 2008, and 0 for instances happening following the controversy in 2007 and those in 2006. The coefficient of this dummy variable in Model 6 is negative and different from 0 ( $p = .062$ ), suggesting that stigmatizing labels after the scandal yielded more negative reactions by investors.

#### 5.4.8 | Event study in multiple-country settings

Previous research has indicated that the use of market model for event study in multiple-country setting may lead to biased coefficients because market model does not account for (a) the possibility of financial market integration across countries and (b) the currency exchange rates among countries (Park, 2004). To make sure that this potential bias has not distorted our results, we followed the methodology suggested by Park (2004) and calculated CARs for each firm by using a world market model. Accordingly, we adjusted the abnormal returns for global market movements by adding *Financial Times Stock Exchange World Index* as the world market index and for changes in currency exchange rates by using the exchange rates between the U.S. dollar and the respective local currencies (Li, 2022). After repeating our regression analyses with the adjusted CARs, our results stayed the same (see Table A11 of the Appendix).

<sup>11</sup>Given that our sample covers 1998–2016, we considered 1998–2007 as the first decade and 2008–2016 as the second.



## 6 | DISCUSSION

Stigma and its consequences represent a burgeoning (and important) topic of interest in organizational studies (e.g., Aranda et al., 2021; Pollock, Lashley, Rindova, & Han, 2019; Zhang et al., 2021). Despite broad consensus about the negative economic externalities of stigma, little attention has been paid to the conditions under which such consequences materialize and what determines their magnitude. To our knowledge, this paper represents one of the first attempts to explicitly flesh out the relationship between stigma targeting the firm or its affiliates and the economic penalties associated with it. We theorized that stigmatizing labels translate into economic sanctions because they threaten the firm's legitimacy. Moreover, we have argued that the content of stigmatizing labeling, as well as the specific audiences articulating them, are also relevant, both because different types of stigmatizing labels jeopardize different facets of the firm's legitimacy and because specific audiences are associated with distinct domains of expertise and institutionalized roles of authority within society. We examined these assertions empirically, building on a mixed-methods study of global arms manufacturers between 1998 and 2016. In so doing, we were able to examine the economic consequences of stigma both at scale and at a much greater level of detail than previously possible, offering important nuance to our understanding of the relationship between media coverage of audiences' stigmatizing labels and the sanctioning of firms (e.g., Graffin, Bundy, Porac, Wade, & Quinn, 2013; Pollock & Rindova, 2003).

Overall, our work contributes to the organizational stigma literature along several distinct dimensions. First, our study provides an initial framework for future studies on the conditions under which the economic consequences commonly associated with stigma are likely to materialize. With the exception of Devers et al. (2009), organizational scholars have largely refrained from explicitly theorizing about the mechanisms through which the consequences of stigma manifest, instead relying on the established sociological literature (e.g., Goffman, 1963) for the theoretical heavy lifting. This represents a missed opportunity, inasmuch as there is a great deal to gain from unpacking *who* stigmatizers are and *how* stigma varies in terms of the accusations levied (Helms & Patterson, 2014). In this regard, we have argued—and found support for—the idea that stigmatizing labels by audiences who are in the position of authority to express those labels are taken as valid cues by market actors leading to stronger economic sanctioning and depressed valuations. Our findings also build empirical support for prior theory on the role of audiences in the emergence and impact of stigma (Aranda, Helms, Patterson, Roulet, & Hudson, 2023). While prior studies have focused upon the content of stigmatizing labels in its origins and negative effects (e.g., Piazza & Perretti, 2015), our results point to a more refined view of the effects of stigma which, in addition to the content of stigmatizing labels, is shaped by the type of stigmatizer audience, consistent with Helms, Patterson, & Hudson's (2019): p. 7 characterization of stigma as "a perception-driven label held by particular audiences" that "is not inherent to targets". In so doing, our work also shifts the prevailing analytical focus of this body of research from the perspective of the stigmatized to that of stigmatizers (Ferns, Lambert, & Gunther, 2022; Kvåle & Murdoch, 2021; Toubiana & Ruebottom, 2022).

Second, we advance knowledge on how stigma dynamics play out in stigmatized industries. Our empirical analysis examined the arms industry, which is arguably characterized by core stigma (Hudson, 2008). But whereas most prior studies of core-stigmatized organizations and industries treated stigma simply as a backdrop against which organizational action unfolds, or as a result of which negative consequences materialize, our empirical setup allowed us to: (a) observe the stigmatization process taking place through condemnation by audiences; and

(b) link the occurrence of stigmatization to negative economic consequences clearly and unambiguously. At the same time, the fact that our study empirically focuses on a single industry inevitably limits the scope of our arguments and findings. For example, the characteristics of stigmatizing labels used to vilify targets might be substantially different in other settings, and stigma may in principle target a wide range of firms and practices that are not represented here. Additionally, future work should explore the applicability of our arguments to settings where stigma is not as pervasive. These include noncore-stigmatized settings where stigma may only arise as a result of traumatic events, as explored by work on event stigma (e.g., Frandsen & Morsing, 2021; Hudson, 2008; McDonnell, Odziemkowska, & Pontikes, 2021), or in contexts where firms can engage in stigmatized and nonstigmatized activities simultaneously (Piazza & Perretti, 2015). Because event stigma is known for its less permanent nature, the accepted institutional role of an audience may matter in determining whether event stigma ultimately wanes or, conversely, becomes permanent. Along similar lines, the economic penalties associated with stigma may be a function of whether—and to what degree—organizations are able to engage in stigma dilution (Vergne, 2012). Because everything core-stigmatized organizations do is subject to stigma, these organizations have “nowhere to flee” and are likely to suffer greater consequences than organizations with the option of diversifying towards less stigmatized activities.

Finally, our findings also suggest that audiences have—at least in theory—some degree of strategic control over the economic consequences that organizations suffer as a result of stigmatizing labels. Sophisticated interest groups—such as activists, or the media—could thus seek to arbitrage these differences *strategically*, fine-tuning their condemnation to inflict economic harm on their targets. Civil society actors, for example, could strategically choose to frame their vilification attempts in terms of harm, rather than illegality, in order to inflict greater economic penalties, while market actors could direct their attention to the exchange partners of firms engaged in controversial activities, thereby pushing them to withdraw from transactions with the focal firm due to the risk of stigma by association (Jensen, 2006).

## 6.1 | Limitations and directions for future research

While we believe our study makes important contributions to the literature, as outlined above, we must also acknowledge its limitations. First and foremost, we have focused on a specific negative externality of stigma, conceptualized as a stigma-induced withdrawal from transactions resulting in economic devaluation, and measured as the cumulative abnormal returns (CARs) to arms producers. In this way, our study hardly does justice to the many ways in which stigma-associated penalties may materialize. Our dependent variable is arguably a proxy for informal ostracism, which represents one of the main theoretical mechanisms through which stigma manifests in practice, as fewer and fewer individuals and organizations alike are willing to transact—or even associate with—“tainted” firms. However, stigma can bring about other negative externalities, including overt hostility from political actors and regulatory bodies (Augustine & Piazza, 2022) as well as organized opposition from activists determined to punish firms for behavior they find objectionable (King & Soule, 2007). Future work would ideally expand upon our findings by broadening the scope of inquiry to encompass the diverse forms of sanctioning associated with stigma, as well. Event studies are also best suited for examining short-term outcomes, but cannot account for longer-term consequences, which would also warrant further investigation. Finally, and along similar lines, the negative consequences of stigma are arguably a function not only of stigmatizing labels, but also to the degree to which the target



firms enjoy support from various audiences (Helms & Patterson, 2014; Hudson & Okhuysen, 2009)—such as the general public, elected officials, civil society, and financial actors—as such support may mitigate the negative externalities associated with public vilification (Piazza & Augustine, 2022). While we were unable to account for such forms of support due to the design of our study, a more comprehensive theoretical model of the consequences of stigma would encompass these, as well.

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## DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the first author upon reasonable request.

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## APPENDIX

TABLE A1 Keywords used to extract articles containing stigmatizing labeling

Initial keywords from Vergne (2012) and Durand and Vergne (2015)	Extended keywords extracted from Oxford languages dictionary
Condemn*	Censure, criticize, castigate, attack, denounce, deplore, decry, revile, inveigh against, chastise, berate, upbraid, reprimand, rebuke, reprove, reprehend, take to task, find fault with, deprecate, disparage, slam, hammer, lay into, cane, blast, slate, slag off, slash, reprobate, excoriate, vituperate, arraign, objurgate, anathematize
Protest*	Express opposition, raise objections, object, dissent, take issue, make/take a stand, put up a fight, complain, express disapproval, disagree, express disagreement, demur, remonstrate, expostulate, make a fuss, cry out, speak out, rail, inveigh, fulminate, oppose, challenge, allegation, probe, question, investigate
Inappropriate*	Unsuitable, unfitting, ill-suited, unseemly, unbecoming, unprofessional, unfit, unbefitting, indecorous, improper, incongruous, wrong, amiss, inapposite, injudicious, ill-advised, ill-judged, ill-considered, infelicitous, unfortunate, regrettable, misguided, ill-timed, inopportune, tactless, poor, undesirable
Complain*	Grumble, moan, whine, bleat, speak out against, rail at, oppose, lament, gripe, grizzle, chunter, chirp, natter
Blame*	Hold responsible, hold liable, accuse, inculpate, guilt, accountability, liability, onus, culpability, recrimination
Critic*	Censure, denounce, arraign, attack, disapprove, disparage, denigrate, deprecate, malign, vilify, besmirch, slur, slam, hammer, blast, pull apart, pressure, hit, savage, crucify, monster, slate, rubbish, sledge, excoriate, against
Unethical	Immoral, amoral, unprincipled, dishonorable, wrong, dishonest, deceitful, disreputable, unconscionable, fraudulent, dirty, unfair, devious, wicked, evil, sinful, iniquitous, depraved, unprofessional, improper, unseemly, unworthy, negligent
Corrupt*	Dishonest, dishonorable, amoral, untrustworthy, deceitful, disreputable, discreditable, shameful, scandalous, bribable, fraudulent, swindling, grafting, criminal, lawless, felonious, villainous, malfeasant, crooked, shady, tricky, dirty, rascally, scoundrelly, bent, dodgy, bribe, buy off, contaminate, taint, fiddle
Violat*	Breach, infringe, infract, break, transgress, overstep, not comply with, disobey, defy, disregard, ignore, invade, intrude, trespass, disturb, disrupt, blasphem, degrade, damage, vandalize, deface, destroy, trash
Bad	Unacceptable, deficient, faulty, negligent, dreadful, awful, terrible, disgraceful, miserable, pathetic, lousy, appalling, rubbish, crap, unpleasant, disagreeable, unwelcome, unfavorable, adverse, nasty

**TABLE A2** Textual patterns used to code stigmatizing label focus, with exemplars

Stigma focus	Patterns extracted from the content analysis	Exemplars of shamed organizations or affiliates from media articles
Target stigma	<ul style="list-style-type: none"> <li>• <i>Organization + audiences'</i> stigmatizing labels targeting a firm's specific deviant behavior or action</li>   <li>• <i>Organization + audiences'</i> stigmatizing labels targeting a firm's specific problematic product(s)</li>   <li>• <i>Organization + audiences'</i> stigmatizing labels targeting the organization by using stigmatizing nicknames</li> </ul>	<p>Meanwhile in St. James's Square, demonstrators amassed outside Lockheed Martin, chanting, "War criminals."</p> <p><i>Elbit Systems and Israel Aerospace Industries Ltd.</i> commit war crimes and violate human rights.</p> <p>Human rights groups say that the attacks by the drones manufactured by <i>Cobham plc</i>- which specializes in defense and communications electronics, including satellites violate international law.</p> <p>Amnesty International criticizes <i>Chemring</i> for exporting tear gas, apparently under license, to the Kenyan paramilitary police.</p>
Stigma by association	<p><i>Exchange partner + audiences'</i> stigmatizing labels targeting the specific exchange partner having ties with the target organizations</p> <ul style="list-style-type: none"> <li>• <i>Exchange relationship + audiences'</i> stigmatizing labels about having ties with the target organizations</li> </ul>	<p>In a letter sent to the head of TD's U.S. operations this week, the Chicago Mayor urged the bank to stop providing financial services to <i>Smith &amp; Wesson</i>.</p> <p>"Charities should not be linking with <i>BAE</i> because their support provides it with an element of legitimacy", tells the research co-ordinator at the charity Campaign Against Arms Trade.</p>

TABLE A3 Textual patterns used to code stigmatizing label content, with exemplars

Stigma content	Keywords and patterns extracted from the content analysis	Exemplars of content from media articles
<p>Harm (target) stigma</p> <p>Accounts based on the perceived physical and social harm to individuals or society associated with an organization's core activities (Helms &amp; Patterson, 2014)</p>	<p><i>Harm-related keywords</i></p> <ul style="list-style-type: none"> <li>Kill*, died, harm*, hurt*, damage*, slaughter*, devastat*, (promot*, nurtur*, involve*) and (conflict*, insecurity or war*), (endanger*, loss) and (peace*, live*, security), threat* to, death*, (violat*, undermin*, jeopardiz*, threaten*) and right*, war crime*, criminal, evil, murder, tortur*</li> </ul> 	<p>A major British defense company is supplying the deadly US drone programme, which has killed scores of civilians.</p>
<p>Illegality (target) stigma</p> <p>Accounts based upon the perceptions that a targeted organization's activities are lawless (do not follow rules) and should be illegal (Helms &amp; Patterson, 2014; Schwartz &amp; Skolnick, 1962)</p>	<p><i>Illegality-related keywords</i></p> <ul style="list-style-type: none"> <li>illegal, allegat*, accus*, against law, allegations, against rules, violat* and (rule*, law*, legal code), fraud*, corrupt* and (influenc*, lobby*, practice*, impact*, act*, behavior, activit*), (break* or bend* or circumvent* and law*), (shady or questionable) and (activit*, practic*), bribe*, bribing, kickback*, (contract* or bid*) and (defraud*, rigg*, collusion, collud*, overcharg*, infring*, breach*), export* and repressive regime*, leak* and (secret*, data, information, files), charges, wrongdo*</li> </ul> 	<p>The arms firm (<i>Chemring</i>) is increasingly using overseas production deals to avoid controls on exports to repressive regimes.</p>



TABLE A3 (Continued)

Stigma content	Keywords and patterns extracted from the content analysis	Exemplars of content from media articles
		million contract to supply warplanes to the Dominican army.
	<ul style="list-style-type: none"> <li>• <i>Organization + audiences'</i> stigmatizing labels targeting an alleged violation of a generally accepted rule (e.g., corporate transparency)</li> </ul>	<p><i>Dassault Aviation</i>, a key partner for Britain in the development of unmanned fighter aircraft, is among the worst in disclosing what it does to prevent corrupt practices, said Transparency International in its second anti-corruption report.</p>
Unethicality (affiliates) stigma	Keywords related to <i>unethical affiliations</i>	
(Goffman, 1963; Pontikes et al., 2010)	<ul style="list-style-type: none"> <li>• unethical, immoral, illegitimate, (against, violat*, question*, contradict*, undermin*, ignor*, transgress*, defy, breach*) and (ethics, ethical, moral, norms, values, beliefs, legitimacy), (critic*, sever, abandon, divest, cut, terminat*, stop*) and (deal*, invest*, tie*, contribut*, sponsor*, collaborat*, link*, connection*, service*)</li> </ul> 	
	<ul style="list-style-type: none"> <li>• <i>Exchange partner (or exchange relationships) + audiences'</i> stigmatizing labels targeting the problematic nature of investing in the target organization</li> </ul>	<p>Lloyds TSB has signed up to a multi-million dollar bond deal with the arms manufacturer <i>Lockheed Martin</i> despite growing condemnation of the British banking industry's continued investments in cluster bomb manufacturers.</p>
	<ul style="list-style-type: none"> <li>• <i>Exchange partner (or exchange relationships) + audiences'</i> stigmatizing labels targeting the problematic nature of financial contributions from the target organization (e.g., contributions to museums)</li> </ul>	<p>The Royal British Legion has become the target of criticism for accepting an undisclosed sum from <i>BAE systems</i> maker of combat aircraft, missiles.</p>
	<ul style="list-style-type: none"> <li>• <i>Exchange partner (or exchange relationships) + audiences'</i> stigmatizing labels targeting the problematic nature of sponsorship ties with a target organization (e.g., renting a physical facility for a product fair, library openings)</li> </ul>	<p>The London Transport Museum is facing calls to sever its links with a major defense company. Activists have picketed the museum over complaints that it should not have entered into sponsorship deals with <i>Thales</i>, which draws 60 percent of its revenue from defense equipment.</p>



TABLE A4 Exemplars of different audiences stigmatizing arms producers and their affiliates

Audiences	Specific audiences derived from the content analysis	Exemplars of different audiences conveying stigma from media articles
Civil society actors	<p>Stigma revolving around a firm's deviant behavior or problematic product(s), or having ties with the target organization by:</p> <ul style="list-style-type: none"> <li>• <i>Journalists</i></li> </ul>	<p>The Guardian has discovered a secret vault in Switzerland, where <i>BAE</i>, Britain's largest arms company, is alleged to have hidden filing cabinets full of evidence of covert payments to foreign politicians.</p>
Financial actors	<p>Stigma revolving around a firm's deviant behavior or problematic product(s), or having ties with the target organization by:</p> <ul style="list-style-type: none"> <li>• <i>Current investors</i></li> </ul>	<p>Peace campaigners are to stage a demonstration outside the Science Museum in London on Saturday afternoon in protest at sponsorship by <i>Airbus</i>, the aerospace firm, which sells weapons to a number of regimes implicated in human rights abuses</p>
	<ul style="list-style-type: none"> <li>• <i>Financial analysts</i></li> </ul>	<p>The US Presbyterian Church on Friday announced that it will exert pressure on <i>four US corporations</i> to cease supplying military equipment and technology to Israel for use in the occupation of the Palestinian territories</p>
	<ul style="list-style-type: none"> <li>• <i>Potential investors or customers</i></li> </ul>	<p>Directors of <i>GKN</i> faced hostile questions from shareholders at its last AGM over the company's sale of armored vehicles to Indonesia.</p>
		<p>The <i>Saab</i> has been under fire from analysts in view of the devastation that the Oct 8 quake caused. Besides the loss of more than 73,000 lives, the quake also almost flattened the cities of Muzaffarabad, Baagh and Balakot.</p>
		<p>The Local Authority Pension Fund Forum (LAPFF), which represents 40 funds with invested assets totalling pounds 70bn, is drawing up a series of questions, including allegations of bribery to put to defense companies (e.g., <i>BAE</i>) as it seeks to encourage responsible investment by pension funds.</p>
		<p>Or</p>



TABLE A4 (Continued)

Audiences	Specific audiences derived from the content analysis	Exemplars of different audiences conveying stigma from media articles
Political actors	<p>Stigma revolving around a firm's deviant behavior or problematic product(s), or having ties with the target organization by:</p> <ul style="list-style-type: none"> <li>• <i>Elected officials</i></li> <li>• <i>Political parties</i></li> </ul>	<p>Aviva, one of the world's largest insurance companies, has created a "stop list" of 12 arms manufacturers which includes the US defense giants <i>Lockheed Martin</i>, <i>Textron</i> and <i>Alliant Techsystems</i>. All three have received hundreds of millions of pounds in investment from British high street banks.</p>



**TABLE A5** Additional models testing the effect of stigmatizing labels on firms' CARs with alternative asset pricing models for (0,1) window

	Mean CAR	CSect test	Patell Z	CDA test	CtdCs Z
Market-adjusted model	-0.32%	-3.14 (.002)	-2.86 (.002)	-2.89 (.004)	-2.86 (.004)
Fama–French three factor model	-0.40%	-3.52 (.000)	-3.67 (.000)	-3.22 (.001)	-3.61 (.000)
Fama–French plus momentum	-0.38%	-3.30 (.001)	-3.82 (.000)	-3.19 (.001)	-3.71 (.000)

Note: All tests are two-tailed except for *Patell Z*. *p*-values in parentheses.

**TABLE A6** Additional models testing the effect of stigma on firms' CARs in distant windows (before and after) relative to the stigmatizing events

	Mean CAR	CSect test	Patell Z	CDA test	CtdCs Z
Window before the event: (-10, -5)	+0.20%	0.90 (.368)	1.02 (.154)	1.18 (.238)	0.84 (.400)
Window after the event: (6, 11)	-0.22%	-1.35 (.179)	-1.08 (.140)	-0.83 (.217)	-0.76 (.447)

Note: All tests are two-tailed except for *Patell Z*. *p*-values in parentheses.

**TABLE A7** Robustness checks: The effect of stigmatizing labels on firms' CARs (H0)—(0,1) window with dropped observations, long window (-1,3), and alternative windows of (-1,1), (-1,2), and (0,2).

Window	Percentage mean	Numbers (-/+)	CSect test
(0,1) with dropped observations	-0.44%	489: 272/217	-3.28 (.001)
Long window (-1,3)	-0.32%	534: 284/250	-1.83 (.068)
(-1,1)	-0.34%	534: 294/240	-2.74 (.014)
(-1,2)	-0.43%	534: 305/229	-2.76 (.006)
(0,2)	-0.36%	534: 286/248	-2.10 (.036)

Note: *p*-values in parentheses.

TABLE A8 Robustness checks: The interactive effects of audiences and stigmatizing labels on firms' CARs (H1a, H1b, and H2)—(0,1) with dropped observations, long window (−1,3), and alternative windows of (−1,1), (−1,2), and (0,2)

(0,1) with dropped observations			Long window (−1,3)			(-1,1)			(-1,2)			(0,2)				
Percentage mean and numbers	Percentage mean and numbers	Percentage mean and numbers	CSEct test	(−/+)	CSEct test	(−/+)	CSEct test	(−/+)	CSEct test	(−/+)	CSEct test	(−/+)	Percentage mean and numbers	Percentage mean and numbers	CSect test	(−/+)
Illegality/political actors	-0.39%	-2.08	-0.93%	-2.36	-1.07%	-3.71	-0.81%	-2.05	-0.70%	-2.01						
	30/25	(.082)	33/24	(.056)	35/22	(.003)	32/25	(.062)	30/27	(.056)						
Illegality/nonpolitical actors	0.10%	0.50	0.17%	0.50	-0.09%	-0.46	-0.07%	-0.33	-0.34%	-1.21						
	58/72	(.628)	66/73	(.626)	68/71	(.652)	74/65	(.750)	71/68	(.235)						
Harm/civil society actors	-0.88%	-7.21	-0.32%	-2.20	-0.42%	-3.99	-0.44%	-2.70	-0.76%	-7.72						
	78/38	(.000)	73/62	(.070)	75/60	(.016)	80/55	(.036)	79/56	(.000)						
Harm/other actors	-0.30%	-0.95	0.49%	1.42	-0.15%	-0.84	-0.05%	-0.52	0.24%	0.59						
	11/16	(.398)	16/17	(.229)	13/20	(.432)	16/17	(.629)	14/19	(.587)						
By association/financial actors	-1.06%	-2.31	-1.25%	-2.28	-0.91%	-2.83	-1.30%	-2.50	-1.01%	-2.02						
	38/12	(.025)	36/17	(.027)	41/12	(.007)	38/15	(.016)	35/18	(.048)						
By association/nonfinancial actors	-0.42%	-1.26	-0.50%	-1.24	-0.22%	-0.74	-0.43%	-1.18	-0.49%	-1.26						
	57/54	(.211)	60/57	(.218)	62/55	(.215)	65/52	(.242)	57/54	(.211)						

Note: *p*-values in parentheses.

TABLE A9 Robustness checks: Regression analyses of hypotheses H1a, H1b, and H2 with (0,1) window with dropped observations, the long window (-1,3), and alternative windows of (-1,1), (-1,2), and (0,2)

		Long window (-1,3)						(-1,1)						(-1,2)						(0,2)					
		Long window (-1,3)			(-1,1)			(-1,2)			11	12	13	14	15	11	12	13	14	15	11	12	13	14	15
		1	2	3	4	5	6	7	8	9	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p
Firm size	0.00110 (0.00338)	0.00482 (0.00405)	-0.00504 (0.00441)	-0.00382 (0.00534)	0.00955 (0.00224)	-0.00335 (0.00738)	0.00584 (0.00253)	0.00774 (0.00462)	-0.00294 (0.00407)	0.00531 (0.00579)	0.00588 (0.00477)	-0.00656 (0.00477)	0.00311 (0.00104)	0.00849 (0.00583)	-0.00384 (0.00583)										
	(0.7502) (0.2678)	(0.2794) (0.00008)	(0.4789) (0.00002)	(0.00028) (0.00035)	(0.4849) (0.00034)	(0.2894) (0.00034)	(0.0154) (0.00034)	(0.5382) (0.00035)	(0.2167) (0.00035)	(0.1016) (0.00035)	(0.2840) (0.00035)	(0.5268) (0.00031)	(0.00000) (0.00031)	(0.5268) (0.00031)	(0.00000) (0.00016)										
Disapproval of peers	0.00008 (0.00035)	0.00030 (0.00007)	0.00002 (0.00015)	-0.00024 (0.00043)	0.00035 (0.00013)	-0.00006 (0.00017)	-0.00003 (0.00040)	0.000034 (0.00015)	-0.00005 (0.00043)	0.00008 (0.00015)	0.000035 (0.00015)	0.00012 (0.00016)	-0.00021 (0.00015)	0.000031 (0.00003)	-0.00031 (0.00015)	-0.00016 (0.00015)									
Past disapproval	-0.00040 (0.00070)	-0.00027 (0.00043)	0.00036 (0.00083)	-0.00144 (0.00102)	0.00103 (0.00121)	-0.00011 (0.00203)	-0.00055 (0.00062)	0.00009 (0.00112)	-0.00036 (0.00103)	-0.00098 (0.00061)	-0.00017 (0.00101)	-0.00022 (0.00127)	-0.00055 (0.00075)	-0.00055 (0.00077)	0.00047 (0.00152)	0.00047 (0.3148)									
Scandal	0.01488 (0.00923)	0.0183 (0.00442)	-0.01666 (0.01335)	0.00799 (0.00649)	0.00813 (0.00558)	-0.01284 (0.01582)	0.01312 (0.00621)	0.01070 (0.00507)	-0.01597 (0.01539)	0.02002 (0.00532)	0.01161 (0.00540)	-0.01828 (0.01450)	0.00835 (0.00700)	-0.01122 (0.00509)	0.00835 (0.01486)	-0.01122 (0.7625)									
Media visibility	0.00024 (0.00086)	0.00146 (0.00160)	0.00060 (0.00253)	-0.00219 (0.00266)	-0.00180 (0.00192)	-0.00126 (0.00360)	0.00061 (0.00144)	0.00040 (0.00218)	-0.00101 (0.00105)	-0.00120 (0.00143)	-0.00030 (0.00283)	-0.00092 (0.00225)	0.00050 (0.00200)	-0.00065 (0.00225)	0.00026 (0.00292)	-0.00026 (0.4689)									
Firm reputation	-0.00060 (0.00212)	-0.00433 (0.00095)	0.00295 (0.00149)	0.00302 (0.00311)	-0.00267 (0.00138)	0.00349 (0.00185)	-0.00155 (0.00250)	-0.00127 (0.00128)	0.00143 (0.00131)	-0.00034 (0.00172)	-0.00204 (0.00102)	0.00441 (0.00156)	-0.00321 (0.00262)	-0.00496 (0.00074)	0.00203 (0.00169)	-0.00203 (0.4677)									
Firm performance	0.09923 (0.09145)	0.00736 (0.01491)	-0.14786 (0.05385)	-0.06084 (0.13045)	0.00077 (0.06995)	-0.16168 (0.04393)	0.07107 (0.07571)	0.02599 (0.03880)	-0.14296 (0.05472)	0.03357 (0.05198)	0.01065 (0.04722)	-0.19498 (0.15724)	-0.00252 (0.04964)	-0.11546 (0.04895)	-0.11546 (0.04895)										
Market share	-0.08360 (0.07270)	0.07899 (0.06136)	-0.17502 (0.07861)	-0.21338 (0.12326)	0.27891 (0.04674)	-0.18887 (0.04748)	0.39833 (0.03961)	-0.08778 (0.03187)	-0.24335 (0.05627)	0.36223 (0.02372)	-0.18271 (0.06287)	-0.27203 (0.06298)	-0.00432 (0.07252)	-0.17818 (0.06035)	-0.17818 (0.06035)										

TABLE A9 (Continued)

(0.1) with dropped observations										(0.2)									
Long window (-1,3)					(-1,1)					(-1,2)					(-1,2)				
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p
Sigma dilution	0.00638 (0.00834)	-0.00897 (0.00815)	0.03125 (0.01140)	-0.01172 (0.01712)	-0.02714 (0.01242)	0.03005 (0.00912)	-0.00869 (0.00867)	-0.01849 (0.00874)	0.00931 (0.00882)	-0.01099 (0.01156)	-0.01955 (0.01160)	0.02862 (0.01159)	-0.00468 (0.01160)	-0.01656 (0.01160)	0.03268 (0.01295)				
Nonmilitary disapproval	-0.00064 (0.00047)	0.00026 (0.00016)	0.00061 (0.00040)	0.000070 (0.00022)	0.00044 (0.00019)	0.00030 (0.00043)	0.00040 (0.00019)	0.00031 (0.00043)	0.00049 (0.00015)	0.00060 (0.00040)	0.00037 (0.00015)	0.00049 (0.00010)	0.00192 (0.00016)	0.00033 (0.00016)	0.00042 (0.00039)				
Capital expenditures	0.00137 (0.1978)	-0.00307 (0.1452)	-0.00256 (0.1554)	0.00928 (0.7263)	-0.00571 (0.5044)	-0.00239 (0.6085)	-0.00060 (0.1399)	-0.00540 (0.2767)	-0.01974 (0.1974)	-0.00208 (0.0225)	0.00161 (0.2413)	-0.00318 (0.0923)	-0.00185 (0.0706)	0.00316 (0.3165)	-0.00560 (0.0543)				
Illegality content: political actors versus nonpolitical actors	-0.00736 (0.6391)	-0.01000 (0.5552)	-0.01000 (0.3286)	-0.01000 (0.1126)	-0.01000 (0.0696)	-0.01000 (0.7329)	-0.01000 (0.9011)	-0.01000 (0.6974)	-0.01000 (0.6292)	-0.01000 (0.5833)	-0.01000 (0.4263)	-0.01000 (0.7378)	-0.01000 (0.4751)	-0.00950 (0.5801)					
Sigma by financial versus nonfinancial actors	0.00443 (0.0368)	0.00445 (0.0369)	0.00443 (0.0369)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)	0.00443 (0.0366)					
Harm content: civil society versus noncivil society actors	-0.01615 (0.01133)	-0.01615 (0.0133)	-0.01615 (0.0133)	-0.01615 (0.0133)	-0.01615 (0.0133)														
Sigma by financial versus nonfinancial actors	0.00422 (0.0340)	0.00422 (0.0340)	0.00422 (0.0340)	0.00422 (0.0340)	0.00422 (0.0340)														



TABLE A9 (Continued)

(0.1) with dropped observations			Long window (-1,3)			(-1,1)			(-1,2)			(0.2)			
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p
Constant	-0.06155 (0.02353)	-0.02808 (0.01954)	0.06178 (0.05905)	-0.04623 (0.03583)	-0.02893 (0.03038)	0.08036 (0.02433)	-0.07147 (0.02110)	-0.04275 (0.02773)	0.04722 (0.01869)	-0.08168 (0.02074)	0.09971 (0.0506)	-0.03301 (0.03383)	0.09971 (0.01941)	-0.03203 (0.02383)	0.04243 (0.06742)
Observations	185	191	196	167	169	196	169	196	197	169	196	169	196	169	169
R <sup>2</sup>	0.151	0.199	0.517	0.110	0.240	0.428	0.159	0.201	0.490	0.175	0.196	0.527	0.281	0.044	0.391
Adjusted R <sup>2</sup>	0.003	0.029	0.374	0.046	0.080	0.258	0.059	0.032	0.329	0.020	0.387	0.128	0.018	0.210	

Note: Standard errors and p-values in parentheses.



**TABLE A10** Robustness checks: Regression analyses of the effect of stigmatizing labels' volume on firms' CARs ( $H_0$ ) in (0,1) with dropped observations, long window (-1,3), and alternative windows of (-1,1), (-1,2), and (0,2)

	1 <b>(0,1) with dropped observations b/se/p</b>	2 <b>Long window (-1,3) b/se/p</b>	3 <b>(-1,1) b/se/p</b>	4 <b>(-1,2) b/se/p</b>	5 <b>(0,2) b/se/p</b>
Firm size	-0.00283 (.00320) (.3761)	-0.00693 (.00642) (.3293)	0.00128 (.00569) (.8315)	-0.00120 (.00477) (.8111)	-0.00593 (.00446) (.1998)
Disapproval of peers	-0.00000 (.00010) (.9806)	0.00002 (.00016) (.8935)	-0.00005 (.00006) (.4685)	0.00001 (.00005) (.8511)	-0.00003 (.00011) (.7602)
Past disapproval	-0.00010 (.00040) (.8081)	-0.000050 (.00052) (.3766)	-0.00016 (.00027) (.5894)	-0.00064 (.00017) (.0141)	-0.00093 (.00028) (.0032)
Scandal	-0.00167 (.00523) (.7491)	0.00028 (.00331) (.9369)	-0.00160 (.00259) (.5648)	0.00157 (.00140) (.3140)	0.00276 (.00421) (.5202)
Media visibility	0.00131 (.00159) (.4119)	-0.00063 (.00101) (.5577)	0.00017 (.00118) (.8931)	-0.00029 (.00109) (.8040)	-0.00046 (.00100) (.6500)
Firm reputation	-0.00115 (.00129) (.3728)	0.00124 (.00136) (.4029)	-0.00063 (.00094) (.5298)	0.00004 (.00095) (.9687)	-0.00005 (.00120) (.9670)
Firm Performance	-0.09764 (.04352) (.0253)	-0.15165 (.12433) (.2769)	-0.08781 (.06598) (.2407)	-0.12600 (.08466) (.1968)	-0.16091 (.10415) (.1388)
Market share	-0.16440 (.09607) (.0876)	-0.03993 (.08419) (.6553)	-0.09997 (.04053) (.0568)	-0.17558 (.03689) (.0051)	-0.21410 (.05117) (.0005)
Stigma dilution	0.00321 (.00806) (.6904)	-0.01549 (.00612) (.0525)	-0.01183 (.00412) (.0350)	-0.01051 (.00628) (.1553)	-0.01430 (.00814) (.0950)
Nonmilitary disapproval	0.00020 (.00024) (.4176)	0.00082 (.00021) (.0102)	0.00030 (.00021) (.2010)	0.00040 (.00031) (.2613)	0.00048 (.00027) (.0887)



TABLE A10 (Continued)

	<b>1 (0,1) with dropped observations b/se/p</b>	<b>2 Long window (-1,3) b/se/p</b>	<b>3 (-1,1) b/se/p</b>	<b>4 (-1,2) b/se/p</b>	<b>5 (0,2) b/se/p</b>
Capital expenditures	0.00279 (.00301) .3528) -0.00527 (.00311) (.0907)	0.00650 (.00465) .2206) -0.00728 (.00083) (.0003)	-0.00044 (.00492) .9324) -0.00623 (.00067) (.0002)	0.00256 (.00391) .5417) -0.00585 (.00248) (.0647)	0.00765 (.00339) .0361) -0.00706 (.00359) (.0643)
Constant	0.01478 (.01842) .4228)	0.04929 (.04530) .3262)	0.00291 (.02893) .9239)	0.01015 (.03068) .7542)	0.03461 (.03527) (.3388)
Observations	532	532	532	532	532
R <sup>2</sup>	0.061	0.101	0.051	0.061	0.072
Adjusted R <sup>2</sup>	0.039	0.050	0.030	0.039	0.050

Note: Standard errors and *p*-values in parentheses.



**TABLE A11** Robustness checks: Accounting for cross country market integration and currency exchange rates in regression analyses of hypotheses H1a, H1b, and H2 with (0,1) window

	1 b/se/p	2 b/se/p	3 b/se/p
Firm size	0.01107 (.01382) (.4494)	0.00014 (.00389) (.9720)	0.00652 (.00391) (.1461)
Disapproval of peers	0.00040 (.00026) (.1698)	0.00021 (.00013) (.1676)	0.00025 (.00042) (.5813)
Past disapproval	0.00034 (.00126) (.7955)	-0.00008 (.00121) (.9513)	-0.00153 (.00378) (.6997)
Scandal	0.00029 (.00760) (.9705)	0.01611 (.01297) (.2606)	-0.00937 (.01597) (.5786)
Media visibility	-0.00668 (.00717) (.3824)	0.00525 (.00645) (.4467)	-0.00356 (.00582) (.5634)
Firm reputation	-0.25399 (.25027) (.3399)	-0.06637 (.10571) (.5476)	-0.06896 (.05384) (.2411)
Firm performance	0.00660 (.22199) (.9771)	0.04777 (.11672) (.6965)	0.03283 (.09191) (.7332)
Market share	-9.81928 (24.54365) (.7010)	2.56011 (12.63814) (.8462)	32.51457 (31.08539) (.3359)
Stigma dilution	-0.02230 (.04019) (.5962)	0.00413 (.03559) (.9115)	0.01067 (.02122) (.6329)
Nonmilitary disapproval	0.00085 (.00308) (.7897)	0.00020 (.00064) (.7675)	0.00008 (.00076) (.9213)
Capital expenditures	0.00222 (.02017) (.9154)	0.00082 (.00693) (.9092)	-0.00618 (.00640) (.3713)
Illegality content: political actors versus nonpolitical actors		<b>-0.01789</b> <b>(.00693)</b> <b>(.0363)</b>	

TABLE A11 (Continued)

	<b>1</b> <b>b/se/p</b>	<b>2</b> <b>b/se/p</b>	<b>3</b> <b>b/se/p</b>
Harm content: civil society versus noncivil society actors		<b>-0.00841</b> (.00267) (.0197)	
Stigma by association: financial versus nonfinancial actors			<b>-0.01177</b> (.00400) (.0258)
Constant	-0.03581 (.03102) (.2863)	-0.04684 (.04131) (.3002)	0.00671 (.01611) (.6915)
Observations	196	167	169
R <sup>2</sup>	0.143	0.124	0.209
Adjusted R <sup>2</sup>	0.017	0.012	0.051

Note: Standard errors and *p*-values in parentheses.