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WORKING PAPER

AVOIDING THE CONSEQUENCES OF MISCONDUCT:
BECOMING LICENSED BY AND INSULATED FROM STIGMA

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ABSTRACT

Recent work on organizational stigma considers corporate misconduct to be a stigmatizing act, and has confirmed many negative consequences that follow from it. This paper uses theory on organizational stigma in a new way to explore two contexts in which the penalties for engaging in a stigmatizing act such as corporate misconduct can be avoided, and in so doing informs our understanding of why misconduct is so persistent. Specifically, a firm's existing stigma, elicited by a prior history of misconduct, may license a firm to reoffend without incurring escalated penalties. Moreover, actions taken in advance of a stigmatizing disclosure—such as bringing in new leadership from outside the organization—may insulate firms from stigma's negative consequences. We test these possibilities by examining investor responses to 437 earnings restatement announcements identified as fraudulent between 1997 and 2006, and find that firms with a prior history of misconduct and firms who have recently brought in new leaders from outside the organization are less harshly penalized for their misconduct than firms without a history of misconduct or firms without a recent leadership change. CEO change appears particularly effective in dampening penalties for repeat offenders.

A growing literature on organizational stigma (Devers et al. 2009; Paetzold, Dipboye and Elsbach 2008; Pozner 2008) suggests that multiple types of corporate misconduct and failures, including involvement in illegal activities (Elsbach and Sutton 1992; Sullivan, Haunschild and Page 2007), corporate scandal (Jonsson, Greve and Fujiwara-Greve 2009), and bankruptcy (Sutton and Callahan 1987), elicit an organizational stigma, that is: “a label that evokes a collective stakeholder group-specific perception that an organization possesses a fundamental and deep-seated flaw” (Devers et al. 2009: 155). Unlike other social evaluation constructs, such as reputation or legitimacy, stigma triggers strong negative affective reactions that amplify behavioral responses from the non-stigmatized (Devers et al. 2009). Stakeholders’ reactions to stigmatizing actions are therefore both substantive and universally negative, including withdrawal by key stakeholders, losses in shareholder value, diminished earnings expectations, and increases in the cost of capital and the likelihood of facing shareholder class-action lawsuits (Akhigbe, Kudla and Madura 2005; Baucus and Baucus 1997; Davidson and Worrell 1988; Elsbach and Sutton 1992; Harris 2007; Hribar and Jenkins 2004; Jonsson, Greve and Fujiwara-Greve 2009; Lu 2004; Palmrose, Richardson and Scholz 2004; Sullivan, Haunschild and Page 2007; Sutton and Callahan 1987; Wu 2003).

Given these negative consequences, it is perhaps surprising that the majority of firms engage in at least some form of stigmatizing activity. For example, available evidence suggests that more than half of all firms violate the law, and about half of that group does so repeatedly (Clinard and Yeager 1980; Davidson, Worrell and Lee 1994). Yet the literature has been slow to offer any reasons to explain the uneasy co-existence of these two facts: the consequences of stigma-eliciting actions are grave, but a majority of firms engage in some types of these behaviors, with a substantial proportion of those offending repeatedly. The aim of this paper is to make some headway toward reconciling these two facts, by asking the following questions: What if the consequences of stigma-eliciting actions aren’t consistently negative? What if the negative consequences of these actions are at least partially avoidable?

In their recent paper on organizational stigma, Devers and colleagues assert that the consequences of stigma are contextually determined (2009). In part, this is because highly negative corporate events

such those that would elicit a stigma can be complex and difficult to understand (Hambrick and D'Aveni 1988), which leaves individual stakeholders more reliant on contextual cues when forming judgments of wrongdoing (Wiesenfeld, Wurthmann and Hambrick 2008). Similarly, Greve and his colleagues suggest that those who are in a position to penalize firms for misconduct may not impose those penalties in a consistent manner (Greve, Palmer and Pozner 2010). Drawing on the literature on stigma in a new way (Devers et al. 2009; Goffman 1963; Paetzold, Dipboye and Elsbach 2008), this paper explores why the penalties for organizational misconduct may be inconsistently applied by external stakeholders, and why at times misconduct may not be as harshly punished as one might expect. Specifically, we examine two specific contextual cues—a firm's prior history of misconduct, and the firm's recent experiences with leadership change—that might lead stakeholders to treat similar instances of misconduct more leniently, facilitating the (at least partial) avoidance of some of stigma's consequences.

The possibility that either of these two contexts reduce the penalties firms face when making a stigma-eliciting disclosure has important implications for society. Unless the consequences for repeated misconduct remain at least as severe as they were for first-time offenses, firms may not be motivated to change their practices and return to legitimate modes of operating (Baucus 1994; Pfarrer et al. 2008a). Moreover, if specific types of leadership change can dampen the penalties associated with misconduct, firms may choose to change leaders rather than ceasing the activity that triggers stakeholders to impose penalties in the first place. The paper aims to contribute to our understanding of why misconduct remains so intractable even in the face of its predominantly negative consequence, by showing that, in certain contexts, the full extent of those consequences can be avoided.

We now provide a more detailed discussion regarding each of the two contexts that might dampen the penalties imposed on organizations by stakeholders: a firm's prior history of misconduct, and a change in leadership prior to the revelation of misconduct. Since stigma is a label applied by external audiences (Goffman 1963), and because reactions to a potentially stigmatizing event is likely to vary across groups (Devers et al. 2009), we focus on a specific and important firm audience to study stigma's

consequences: investors. We test our hypotheses using data on investor responses to announcements of fraudulent, non-routine earnings restatements between 1997 and 2006 investigating penalties stemming from an isolated act of misconduct within a clearly identified audience.

The licensing effect of stigma

In the literature on organizational misconduct, the topic of recidivism has interestingly received almost no attention in the literature, although a considerable proportion of corruption can be attributed to repeat offenders (Ashforth et al. 2008; Clinard and Yeager 1980). Empirical work on misconduct has almost exclusively studied first time offenses (Agrawal, Jaffe and Karpoff 1999; Akhigbe, Kudla and Madura 2005; Bromiley and Marcus 1989; Karpoff and Lott 1993; Palmrose, Richardson and Scholz 2004), with the few papers that have noted its persistence among certain firms either dropping recidivists from their samples (Arthaud-Day et al. 2006; Harris 2007; Harris and Bromiley 2007) or using recidivism as a control variable (e.g., Pfarrer et al. 2008b).

Extant research on organizational stigma similarly focuses on first-time offenders, exploring the consequences for firms that accrue at the moment of becoming stigmatized—that is, at the point at which firms go from being untainted to tainted, due to a firm action that triggers this negative label such as announcing corporate scandal (Jonsson, Greve and Fujiwara-Greve 2009), bankruptcy (Sutton and Callahan 1987), or revealing involvement in illegal activities (Elsbach and Sutton 1992; Sullivan, Haunschild and Page 2007). That the consequences of a stigmatizing act may be different for an organization already stigmatized by their prior activities has been neglected in the research to date. In particular, we know little about whether stakeholders might apply penalties to recidivist firms announcing a repeated act of misconduct differently from how they might penalize first time offenders.

Most of what is known about criminal recidivism suggests that recidivists would be more harshly penalized than first-time offenders. Western criminal justice systems intentionally escalate penalties for both individuals and organizations that repeatedly offend (Dana 2001; Zimring, Hawkins and Kamin 2001). Traditional sociological theory also assumes that repeated acts of misconduct will be at least as

severely punished as first-time offenses, since consistent punishment for deviations from normative behavior maintains social cohesion and supports dominant group norms (Durkheim 1969). The literature on stigma, however, opens up the reverse possibility. Specifically, the stigma stemming from organizational misconduct (Devers et al. 2009) may function to license firms to re-engage in similar acts without encountering similarly harsh penalties from external stakeholders, due to the lower expectations they hold of the stigmatized firm.

Work on stigma at the individual level confirms that diminished expectations of performance constitute an important outcome of the stigma-labeling process. Though these lowered expectations often cause decreased performance (Steele 1999; Steele and Aronson 1995), they also seem to license stigmatized individuals to perform poorly relative to their untainted peers, insofar as less effort is required to meet the lower standards. This license has been previously documented among the mentally ill and the disabled, who report feeling permitted to behave outside normative boundaries without suffering the same penalties as would be imposed on non-stigmatized individuals for the same actions (Haber and Smith 1971). As one mental patient noted: “Being crazy does have its benefits. Like people don’t expect the same things from you. You don’t have to perform up to certain standards like you would if you hadn’t had the breakdown. You don’t have to be as responsible, to meet certain obligations...” (Herman and Miall 1990: 258). Goffman recognizes this side-effect of stigma as well (1963), and notes it as already familiar to most stigmatized individuals, who can come to depend on stigma “not only as a reasonable escape from competition but as a protection from social responsibility” (Baker and Smith 1939: 303).

Indeed, it is not only the stigmatized themselves that feel the burden of social expectations to be lifted; external audiences also treat the stigmatized differently from those that have not repeatedly transgressed. Labeling theory (Becker 1963; Lemert 1951) focuses on how external audiences construct the meaning of deviance, and aligns nicely with Goffman’s (1963) original understanding of stigma as a negative label imposed by external audiences. Recognizing also that penalties for misconduct will vary across contexts, Becker proposes that the deviant label changes the way external audiences respond to an

individual's behavioral choices. Specifically, "being...branded as deviant has important consequences for one's further social participation. He has been revealed to be a different kind of person from the kind he was supposed to be.... and treated accordingly" (Becker 1963: 31-32). By extension, therefore, the presence of stigma may well cause external audiences to react to a further stigma-eliciting behavior more leniently, because the prior stigma has already triggered the label of "a different kind" of actor.

While labeling theory focuses on how external audiences treat individuals differentially depending on how they are labeled, a similar process is also likely at the organizational level. In both cases, prior stigma categorizes the actor, whether individual or organizational, as "a different kind"—in other words, the kind who would engage in such stigma-eliciting behavior. The negative reaction this is likely to evoke will not be as strong, because stigma-eliciting behavior is already expected from that actor. Rasmusen (1996), a political economist studying stigma and crime, acknowledged the likelihood that the negative consequences of stigma probably abate across each incidence that elicits it. Applying the same logic to organizations, stakeholder responses to already stigmatized firms will be less severe than responses to firms without a prior stigma. An initial act of misconduct serves as a catalyst that results in a stigma label being applied within a stakeholder group (Devers et al., 2009), and revelations of subsequent misconduct are consistent with the group's already lowered expectations, reinforcing the organization's membership in that category. Holding the severity of the act constant, stakeholder responses to organizational recidivists will therefore be less negative and result in less severe penalties compared to first time offenders. Existing stigma thereby licenses firms to repeat their deviations from normative standards without accruing the same, or increasingly negative, penalties from stakeholders.

H1: *When firms disclose a repeated act of misconduct, the penalties imposed by stakeholders will be less severe than the penalties imposed when disclosing an initial act of misconduct.*

Insulation from stigma

Just as external stakeholder reactions will be affected by prior negative actions, such as their history of misconduct, they will also be affected by more positively regarded actions. One salient context

that is likely to affect how stakeholders interpret misconduct and minimize potential penalties for it is the firm's recent history of leadership change. CEOs are frequently targeted for denigration following corporate failures (Wiesenfeld, Wurthmann and Hambrick 2008), just as they are singled out for kudos following corporate successes (Hayward, Rindova and Pollock 2004). It is therefore common for firms to announce a change in senior leadership following episodes of misconduct (Arthaud-Day et al. 2006; Hennes, Leone and Miller 2008): firms that have filed a material restatement to their financial statements within the last two years are twice as likely to replace their CEOs than other comparable firms (Arthaud-Day et al. 2006). Perhaps due to its frequency, CEO change has been proposed as an important rehabilitation act firms can take after misconduct (Pfarrer et al. 2008a; Wiesenfeld, Wurthmann and Hambrick 2008). Changing the public face of the organization is argued to demonstrate a commitment to positive change and thus mitigate the consequences that follow misconduct admissions.

To date, most of the research on managing the consequences of negative firm actions focuses on what firms do or ought to do "after the fall" (Ashforth and Lee 1990; Elsbach 1994; Marcus and Goodman 1991; Pfarrer et al. 2008a). CEO change as a rehabilitative act after misconduct is likely to work because it allows the stigma of the misconduct to be transferred onto the outgoing CEO (Wiesenfeld, Wurthmann and Hambrick 2008). Goffman discusses how stigma can be transferred from stigmatized actors to those associated with them, a phenomenon he calls 'courtesy stigma' (Goffman 1963). This type of stigma-by-association has been demonstrated at both the individual (Halter 2008; Ostman and Kjellin 2002; Parfene, Stewart and King 2009), and organizational levels (Jonsson, Greve and Fujiwara-Greve 2009). In light of stigma-by-association, CEO change can be viewed as a rehabilitative action, or an attempt by firms to cleanse themselves from stigma's taint and re-establish legitimacy (Elsbach and Sutton 1992) by decoupling themselves from potential scapegoats such as leaders or business units (Devers et al. 2009). Moreover, leadership change is an important "ritual act" that appeases stakeholders and provides hope for future success (Brown 1982; Gamson and Scotch 1964), particularly when outsiders are looking for individual targets to blame for corporate failures (Boeker 1992; Meindl, Ehrlich

and Dukerich 1985). Organizations may therefore decouple from scapegoats at the highest level (i.e., fire the CEO) to signal that someone has taken the fall for organizational misdeeds (Wiesenfeld, Wurthmann and Hambrick 2008), and to transfer the stigma from the organization to the departed leader (Semadeni et al. 2008; Wiesenfeld 1993).

Nevertheless, it may actually be the actions firms take “before the fall” that are most effective in mitigating misconduct’s consequences. In addition to benefiting from CEO change after misconduct, firms may also be able to insulate themselves from the worst consequences of misconduct by changing their leadership *prior to* the disclosure of a potentially stigmatizing act. In other words, rather than *transferring* an existing stigma from the organization to the outgoing leader, firms are *inoculated* against the application of the stigma label. Given how serious the consequences of misconduct can be, if choices firms make prior to admitting misconduct mitigate those penalties, such choices may prove crucial to firm survival. Though little theory or empirical work has explored whether and how these anticipatory strategies might work, Elsbach, Sutton and Principe’s (1998) study of the strategic management of hospital billing practices identifies several ways that undesirable stakeholder responses can be averted in advance, and notes several contexts in which firms do proactively anticipate and avert the potential fallout from the disclosure of negative information. We examine CEO change as one such anticipatory strategy.

Anticipatory strategies may be particularly important when an organization’s legitimacy is at stake (Elsbach and Sutton 1992; Sutton and Callahan 1987). Because they can influence how potentially negative information is received, interpreted, and reacted to by external audiences (Elsbach, Sutton and Principe 1998), anticipatory strategies may be easier to execute than re-legitimation efforts that are made after an organizational threat (Ashforth and Gibbs 1990). A leadership change that occurs prior to an admission of misconduct may positively influence external audiences’ responses to that admission by facilitating attributions that the organization is actively dealing with the root cause of the misconduct. CEO change prior to a restatement announcement can thus be considered a symbolic act (Pfeffer 1981) with both expressive and instrumental dimensions (Daft 1983): expressive in that the organization has

taken an action about the problem that will be positively perceived, and instrumental in that the organization has anointed a new leader with the authority to undertake the clean-up. Another advantage of anticipatory CEO change—unlike some other strategies that firms may take prior to a controversial or negative announcement (Higgins and Snyder 1989)—is that its positive influence on how the future misconduct announcement is viewed is accomplished without drawing specific direct attention to the negative act itself. Thus, leadership change may allow firms to avert some of the misconduct's negative consequences, insulating the firm from the brunt of the stigma that is typically triggered by such disclosures.

Not all leadership change is likely to be effective in this regard. Work on senior leadership change has long distinguished between insider and outsider succession (Gouldner 1954; Grusky 1963), with a general (though not uncontested) view that outsider succession signals a greater commitment to change (Borokhovich, Parrino and Trapani 1996), while insider succession represents a greater commitment to the status quo. Thus, outsider CEO appointments occur more frequently after poor performance (Schwartz and Menon 1985) and are generally regarded more favorably by the financial markets, especially when the incumbent CEO has been forced to resign (Friedman and Singh 1989). By bringing in a new organizational leader untainted by the firm's prior misconduct, outsider CEO succession represents a more sincere effort to "clean house," while symbolically bringing in new blood untainted by the stigma of misconduct.

H2: Organizations that appoint a new outsider CEO prior to an admission of misconduct will face less severe penalties than organizations with no CEO change or insider CEO change.

Whereas new outside leadership may protect any firm from the penalties associated with misconduct, this insulation might work particularly well for recidivist firms. A CEO appointed prior to a repeated announcement of misconduct joined the organization in the aftermath of an earlier wrongdoing. As such, the later misconduct is more easily interpreted by external audiences as a by-product of the rehabilitation process, where the firm effectively discloses further egregious acts as part of the process of

cleaning up the mess left by the former CEO. The existing stigma of the prior misconduct makes any re-legitimation attempt tricky (Ashforth and Gibbs 1990), but in the context of an intervening CEO change, a new admission of misconduct may be considered a tidying up of loose ends rather than the beginning of a longer descent into scandal.

Consider Tyco's CEO Dennis Kozlowski, who resigned in the midst of both personal and organizational scandal. The press described Kozlowski's replacement by former Motorola executive Ed Breen as an effort to "clean house" (Bloomberg News 2003), and labeled Breen a "very straightforward" executive, his appointment the "centerpiece of a plan by Tyco and its board to restore investor confidence" (Sorkin 2002). Arguably, the announcements of misconduct that occurred after Breen's arrival at Tyco could be perceived more positively than disclosures of misconduct made while Kozlowski was still at the helm. In fact, when Tyco again restated earnings within a year of Breen's appointment, an analyst at Wells Fargo suggested that while the discovery of new accounting issues was a lingering risk, it was a risk that "appears to be dissipating" (Bowe 2003). Admitting to additional misconduct subsequent to the appointment of an outsider CEO may even enhance the credibility of the firm, as it allows the new regime to reassert its public commitment to rehabilitation. For example, media coverage of the Tyco restatement under Breen cited the company's recently launched internal review to investigate accounting practices used by "Mr. Kozlowski's management" (Bowe 2003).

H3: Organizations that appoint an outsider CEO prior to an admission of repeated misconduct will face less severe penalties than organizations that appoint an outsider CEO prior to an initial admission of misconduct.

METHODS

To test our hypotheses, we investigate stock market penalties that accrue when publicly-traded corporations restate previously-filed financial statements to test two contexts under which anticipated penalties for organizational misconduct may not hold: when firms have already been stigmatized by a prior restatement, and when firms have made an outsider CEO appointment prior to a restatement

announcement. This empirical setting allows us to observe the consequences of an isolated act of misconduct within a clearly identified audience. Focusing on a specific audience is necessary to study stigma's consequences, since stigma is a label applied to devalued actors by external audiences (Goffman 1963), and because reactions to a potentially stigmatizing event is likely to vary across groups (Devers et al. 2009). Thus, we examine the penalties imposed by a specific and influential stakeholder of publicly traded firms: investors.

Devers et al. (2009) identifies earnings restatements as engendering organizational-level conduct stigma resulting from "specific actions and choices of organizational members" (2009: 158). A restatement is evidence that the organization and auditor failed to detect and/or correct a material error in original financial statements prior to filing with the Securities and Exchange Commission (SEC) and is a strongly discrediting event in that it suggests failure in both internal and external financial control mechanisms (Arthaud-Day et al. 2006). Our sample of restating firms was drawn from two databases issued by the U.S. Government Accountability Office (GAO), an independent and non-partisan agency that works to improve the accountability and performance of the U.S. federal government. The first report covers restatements announced between January 1, 1997 and June 30, 2002 (U.S. GAO, 2003), and the second covers restatements initially announced between July 1, 2002 and June 30, 2006 (U.S. GAO, 2006). By searching Lexis-Nexis using variations of the keyword "restate" and other related terms, the GAO identified a comprehensive set of restatement announcements over both periods (see U.S. GAO, 2006 for a more detailed methodological description). The GAO excluded all restatements resulting from routine corrections that might be driven by a change in accounting practices or bookkeeping errors (Arthaud-Day et al. 2006). As such, the GAO database only includes cases resulting from accounting irregularities due to "so-called 'aggressive' accounting practices, intentional and unintentional misuse of facts applied to financial statements, oversight or misinterpretation of accounting rules, and fraud" (U.S. GAO, 2002: 76). In other words, these firms have been deemed by the U.S. Government and considered by a wide range of other researchers to have engaged in misconduct (Arthaud-Day et al. 2006; Harris and

Bromiley 2007; Pfarrer et al. 2008b; Prechel and Morris 2010). Together the data comprised 2309 restatement announcements, covering a nine and a half year period. Of this overall sample, complete data on all the study variables were available for 452 single-restating firms and 190 multiple-restating firms.

To test our hypotheses, we constructed a matched set of single-restating firms and multiple-restating firms. One challenge with identifying a causal effect using these data is the possibility of selection bias, as there are likely systematic differences between firms that restate only once and those that restate multiple times. We accordingly employed propensity score matching methods (Dehejia and Wahba 2002; Rosenbaum and Rubin 1983) to construct a control group of single-restating firms. This approach is similar to traditional matching methods that are commonly used to study rare events (Cannella, Fraser and Lee 1995; Daily and Schwenk 1996; Zajac and Westphal 1994) and are frequently used by researchers studying restatements (Agrawal, Jaffe and Karpoff 1999; Arthaud-Day et al. 2006; Richardson 2005). The advantage of propensity score matching over traditional matching is that it allows matches to be made on more than a few dimensions. Finding an appropriate match between a treatment firm and a control firm is feasible when the match is made on only one or two dimensions (e.g., industry and geographic region). However, as the number of dimensions grows, so does the likelihood that no firm with the exact same attributes exists (the “curse of dimensionality”). Propensity score matching instead matches firms based on their probability of belonging to the treatment group (Rosenbaum and Rubin 1983), in this case the likelihood that the firm eventually restates more than once, based on observable pre-treatment covariates.

We estimated propensity scores for our GAO database sample of first restatements using a logit model and created a matched sample using nearest-neighbor matching with replacement (Dehejia and Wahba 2002), where each multiple-restating firm was matched with a single-restating firm with the most similar propensity score. This matching process resulted in a data set comprising 141 single restating control observations and 141 multiple-restating firms (because firms were matched with replacement, the final sample contained 233 unique firm observations). After matching, significant differences between

single and multiple-restating firms on these control variables no longer existed (additional details on the matching process, including the exact matching specification, is available from the authors upon request). The multiple restating firm's second, third, and fourth restatements were then appended to this matched sample. As they were extremely rare (less than 0.05 percent of all matched sample observations), instances of more than four restatement announcements and were dropped from the data set, because we were not able to confirm with certainty that they were comparable with the majority of restatements in our sample. The final data set comprised 437 restatement events, with firms restating between one ($n = 141$) and 4 times ($n = 8$).

Consistent with prior research on stigma and organizational misconduct, we use an event study methodology to investigate deviations from expected market reactions (CARs) upon the announcement of an earnings restatement (e.g., Kang 2008; Karpoff, Lee and Vondracik 1999). Deviations from expected market returns are an appropriate measure of firm penalties for three reasons. First, consistent with the understanding that stigma is a label imposed by external audiences on devalued actors, deviations from expected market returns captures how an organizational actor is differentially valued by an important organizational audience in the aftermath of a stigmatizing announcement. Second, this approach permits observation of audience reactions that are temporally proximal to the announcement of a stigmatizing act, minimizing the lag between the disclosure of that act and audience evaluations. Compared to other types of organizational evaluations, such as earnings forecasts or analyst recommendations, analyzing investor reactions increases measurement accuracy and reduces the likelihood that changes in audience penalties are due to an unrelated intervening event. Finally, this approach permits the measurement of very small changes in reactions, whereas the above measures of audience reactions are less fine-grained and sensitive to new information.

Measures

Our data were compiled from a number of sources. The dates of the restatement announcements, New York Stock Exchange (NYSE) listing, shares outstanding, the prompter of the restatement (the

company, an auditor or the SEC) and the reason for the restatement were drawn from the GAO database. Data relating to other attributes of the restatement and data missing from the GAO database were coded from press releases and filings contained in the SEC's EDGAR database. CEO change information was drawn from the company's annual proxy statements via Standard and Poor's Execucomp database. Information pertaining to a CEO's biographical history prior to his or her appointment also came from filings in the EDGAR database. Financial data were collected from Compustat and stock market information from the Center for Research in Security Prices (CRSP).

Stakeholder response to restatement events. Consistent with prior work (Agrawal and Chadha 2005; Bromiley and Marcus 1989; Davidson, Worrell and Lee 1994; Karpoff and Lott 1993; Palmrose, Richardson and Scholz 2004), we used cumulative abnormal stock returns (CARs) to measure penalties in response to a restatement announcement. CARs are a measure of the extraordinary (positive or negative) returns to a firm's stock over a given time period, after controlling for what would have been a normal trajectory of that stock's price, given historical information on the stock and a market portfolio of shares. Using a measure of deviation from expected market reactions is an appropriate dependent variable to test our hypotheses because it accurately reflects how situational contexts, such as recidivism and anticipatory CEO change, affect changes in audience assessments of public firms, controlling for baseline expectations for that firm. CARs measure proportional deviations from expected returns and values can be compared across firms regardless of the absolute change in stock price. A primary element of a financial event study is the calculation of average daily abnormal stock returns (ARs), which is the difference between the firm's actual return and expected (or normal) return had the event of interest not taken place on day, t . Abnormal returns for restating firm i on day t were calculated as:

$$AR_{it} = R_{it} - E(R_{it})$$

where AR_{it} is the daily abnormal return for firm i , R_{it} is the daily return for firm i , and $E(R_{it})$ is the normal return for firm i on day t had the event not occurred. The normal return is calculated using a statistical model that calculates the return of a share relative to the return of a specified market portfolio:

$$E(R_{it}) = \alpha_i + \beta_i R_{mt}$$

where α_i is the intercept, β_i is the non-diversifiable risk of firm i , and R_{mt} is the rate of return of the chosen market portfolio on t . The parameters α_i and β_i of the market model were estimated using an equally-weighted CRSP index as the market portfolio. Computing a normal return (from which abnormal returns are determined) involves choosing an estimation window prior to and non-overlapping with the event window (McWilliams and Siegel 1997). We set our estimation window from t_{-220} days to t_{-20} days prior to the event date t_0 , and then aggregated the daily ARs to arrive at the CAR for an event window spanning the event period t_{-2} to t_{+1} . We chose a four-day window around the event because there was significant information leakage 2-days prior to an announcement (see Table 1), and included 1-day following the announcement because some firms made the announcement after the financial markets closed. Following Brown and Warner (1985), t -statistics are used to test the statistical significance of the CARs.

Second or later restatement. We coded each restatement “1” if it was a second or later restatement by the firm and “0” otherwise. We collapsed multiple-restatement events into one group, because we found no significant difference between the effects of a second, third or fourth restatement on CARs when compared to a first restatement.

CEO change. We used three categories to assess CEO change: *no CEO change*, *outsider CEO change* and *insider CEO change*. Using the executive biographical information from the firm’s proxy statement, we were able to differentiate between CEOs who were promoted as insiders and those who were appointed as outsiders of the company. As such, we coded a CEO as new if (s)he took office in the calendar year of the focal restatement announcement. New CEOs whose prior positions were not within the restating firm were coded as firm outsiders, and new CEOs whose prior positions were within the restating firm, and had been a member of that firm for at least a year prior to starting in the CEO position were coded as firm insiders.

Organizational controls. We include controls for firm quality and firm reputation to rule out the alternative explanation that firms of high quality or with positive reputations are protected from being

penalized for restatements. One firm quality measure was *S&P500*, a dummy variable for whether the firm is included in the S&P500. Additionally, *NYSE* is a dummy variable for whether the company is listed on the New York stock exchange, and *NASDAQ* similarly indicates whether the company's stock is traded on the NASDAQ. Lower-status exchanges (such as the American Stock Exchange or the National Stock Exchange) serve as the referent category. *Firm reputation* is a dummy variable, with "1" representing that the firm had been included in the *Fortune* magazine reputation survey in either of the prior two years, and "0" if the firm had not been included in either year. We also include controls for *firm size*, as size has been shown to affect reactions to financial information (Collins, Kothari and Rayburn 1987; Freeman 1987). These are: (1) the logged *total assets* of the company in millions of dollars, lagged by one year; and (2) *total shares outstanding* in millions. *Return on assets* (ROA), lagged by one year, was measured by a firm's net income divided by total assets and is a common indicator of firm performance (e.g., Arthaud-Day et al. 2006).

Restatement controls. We included six measures of the seriousness of the restatement to ensure that our main independent variables are capturing only the variance attributable to the act of restating and not the seriousness of restatement. (1) *Amend 10K*, a dummy variable for whether the 10K was amended as part of the restatement, was included because restatements that involve changes to an annual report to trigger more severe reactions than restatements which involve only a 10Q quarterly report (Wu 2003). (2) *Quarters Restated*, a variable capturing the total number of fiscal quarters involved in the restatement, was included because restatements involving a longer time frame might be considered more serious than restatements involving a shorter time frame. (3) *Prompt SEC* and *Prompt Auditor*, two dummy variables indicating who prompted the restatement, are included because firms prompted to restate by external bodies suffer worse consequences than firms that restate voluntarily (Akhigbe, Kudla and Madura 2005; Wu 2003). Firm-prompted restatements serve as the reference category. (4) *Reduced net income*, a dummy variable for whether the restatement resulted in an overall reduction in net income, was included because restatements resulting in an overall reduction in net income elicit more severe penalties than

those that do not (Akhigbe, Kudla and Madura 2005). (5) *Net effect* represents the effect of the restatement on firm net income, logged in dollars, that we include because the overall effect of a restatement on net income affects market responses to restatement announcements (Feroz, Park and Pastena 1991). (6) We also included two dummy variables for particularly serious types of restatements: *revenue recognition* and *error involving fraud* because restatements resulting from faulty revenue recognition and error involving fraud result in particularly adverse outcomes (Hennes, Leone and Miller 2008; Palmrose, Richardson and Scholz 2004; Wilson 2006). The reference category for these dummy variables includes all other reasons for the restatement. This information was drawn from restatement filings, annual reports, 10Ks, 10Qs, proxy statements and other filings from the SEC's EDGAR database.

ANALYSIS AND RESULTS

We predict whether recidivist firms and firms that have brought in an outsider CEO will experience less negative market reactions to restatement announcements, compared to first-time offenders and firms that did not appoint an outside CEO. In all models the firm's four-day CAR is the dependent variable and the unit of analysis is the restatement event. Two analytic techniques are used to test our hypotheses. We first used a financial event study to assess whether the penalties associated with an earnings restatement abate for second or later announcements, and then estimate the effects of a restatement and CEO change on abnormal returns using ordinary least squares (OLS) regression. Year dummies were included to control for systematic or environmental time-varying effects that might also influence the effect of a restatement on a firm's stock market valuation, such as variation in the severity of SEC enforcement and media coverage of firm misconduct over this period (Kang 2008). We also included industry dummies based on two-digit SIC (Standard Industrial Classification) codes to control for stable industry attributes that might influence stock market reactions to a restatement. Three categories with few observations—Agriculture, Forestry and Fishing (01-09), Construction (15-17), and Non Classifiable Establishments (99)—were grouped into one category. Standard errors were clustered by firm to correct for non-independence among observations.

Financial Event Study

The CARs for firms that restated their earnings are listed in Table 1. For a first restatement, the four-day CAR is -5.32 percent, and is statistically significant. For a firm restating its earnings on a second or later occasion, the market reaction remains negative and significant; however, the response was smaller in magnitude (-1.95 percent) compared to a first restatement. Figure 1 plots the average CARs for single and multiple restating firms from 12 days before to 12 days after the restatement announcement. The graph indicates that single and multiple restating firms experience similar CARs in the days leading up to a restatement announcement, but that the CARs for multiple restating firms are visibly less negative than those for single restating firms in the days immediately following a restatement announcement.

*** Insert Table 1 about here ***

*** Insert Figure 1 about here ***

Table 2 presents the distribution of CARs for a firm's first and second or later restatement announcement. While overall the response to a restatement announcement was negative, there exists substantial variation in market reaction between first and second or later announcements, as well as within these two categories. The market response tends to be more negative for a first compared to a second or later restatement, but there was also substantial within-category variation in the CARs incurred by a firm, ranging from -19.54 percent to 6.07 percent for a first restatement and -12.28 percent to 8.96 percent for a second or later restatement. This wide range of values suggests that attributes of the firm or the circumstances surrounding the restatement possibly offset the negative valuation effects of the announcement (e.g., Akhigbe, Kudla and Madura 2005), two reasons for which we investigate here.

*** Insert Table 2 about here ***

Regression Analyses

To test our hypotheses that firms suffer less severe penalties upon a second or later restatement announcement and are able to offset the penalties associated with restatements through outsider CEO change, we conducted a cross-sectional analysis of the valuation effects for the matched sample of single

and multiple-restating firms. Table 3 reports variable descriptive statistics and correlations, and Table 4 presents OLS regression estimates for each of the model specifications. Few of the control parameters are significant because the control sample was matched explicitly based on a combination of these same parameters. We still include these controls for all model specifications, because unlike a first restatement, there is no control sample for the second or later restatement observations.

*** Insert Table 3 about here ***

In Table 4, Model 1 reports the results of regressing the four-day CAR for all firms in the matched sample on firm and restatement related controls. In Model 2, the coefficient of the second or later restatement variable is both positive and significant, indicating that a second or later restatement is associated with less negative market penalties compared to a first restatement and consistent with Hypothesis 1. Compared to first restatements, the four-day CAR is 3.6 percent higher upon the announcement of a second or later restatement, holding all other variables constant.

To explore whether the less negative return for second or later restatements could be explained by a floor effect (meaning share prices for recidivist firms are so low that there is little room for the price to drop further), we collected additional data on firm share prices the trading day prior to the restatement announcement. Share prices (in dollars) were indeed lower for a second or later restatement ($M=18.58$, $SD=1.17$) compared to a first restatement ($M=24.24$, $SD=1.44$); however they were not so low to suggest that floor effects caused less negative CARs. Further, 90% of share prices prior to a second restatement were above \$3.13, suggesting that there was room for most share prices to still lose substantial value. A number of firms prior to a first restatement also had low share prices, as 10% of the prices before a first restatement were below \$3.44.

Model 3 adds two variables that indicate whether a new CEO was an outsider or an insider of the company prior to the restatement announcement. Consistent with Hypothesis 2, which predicts that firms can offset the negative effects of an earnings restatement by appointing an outsider CEO, the appointment of an outsider CEO prior to a restatement increased the four-day CAR by 4.7 percent compared to firms

that did not change their CEO. Model 3 also indicates no difference on a firm's four-day CAR between appointing an insider CEO compared to no CEO change.

While an outsider CEO offsets stock market penalties associated with a restatement announcement, Hypothesis 3 suggests that this effect will be stronger for a second or later restatement compared to a first restatement announcement. We estimated two additional models to test this hypothesis. Models 4 and 5 differ from Models 1 to 3 in that a firm fixed effect is included. Because Hypothesis 3 is concerned with a phenomenon that only applies to multiple-restating firms, a fixed effect model allows us to address the question "Does outsider CEO change offset the penalties associated with a restatement announcement significantly more in advance of a firm's second or later restatement?" In these models firm level controls, the industry dummies and NASDAQ control, that are stable within a firm are not directly estimated. Model 4 regresses the four-day CAR for multiple restating firms only on firm and restatement related controls, and Model 5 adds a series of dummy variables to test whether the effect of an outsider CEO on the four-day CAR is less negative when it comes before a repeated admission of misconduct. We coded six categories to capture all cases of CEO change (no change, outsider, insider) for first and second restatements. The reference case is outsider CEO change prior to a second restatement. In support of Hypothesis 3—that a new outsider CEO will be more effective in reducing penalties for repeated misconduct than an initial act of misconduct—the coefficient for the *first restatement, outsider CEO change* term is negative and significant. Results also indicate that, compared to an outsider CEO change prior to a second restatement, all other CEO change categories incurred a more negative four-day CAR.

*** Insert Table 4 about here ***

Figure 2 plots the predicted values of firm CARs by restatement and CEO change type from Model 5. As previously reported, firms incur less severe penalties for second or later restatement compared to a first restatement. Additionally, for both first and second or later restatements, firms incur less severe penalties when they appoint an outsider CEO prior to the restatement announcement. These predicted values suggest that not only are firms that appoint a new outsider CEO before a second or later restatement

penalized less compared to firms that appoint an outsider CEO prior to a first, but also that these firms actually incur positive CARs upon the announcement of a second or later restatement.

*** Insert Figure 2 about here ***

DISCUSSION

We apply the literature on stigma to build theory about two contexts that dampen the penalties imposed by external audiences for announcing misconduct: when a firm becomes a repeat offender and when a new outsider CEO has been brought in prior to the disclosure. We find that investors penalize firms less when they admit a repeated act of misconduct than when they admit misconduct for the first time. We suggest that this effect is due to what we term the licensing effect of stigma: the first admission of misconduct lowers stakeholders' expectations of a firm, thus dampening stakeholders' future reactions to similar types of admissions (Rhee and Haunschild 2006). Our findings, which we believe are the first to address directly the differential consequences of repeated acts of organizational misconduct, imply that one proposed purpose of organizational stigma—"to encourage socially or organizationally valued behaviors" (Paetzold, Dipboye and Elsbach 2008: 190)—may not be effective. On the contrary, the diminished penalties that accrue to repeated acts of misconduct may actually act to enable future bad behavior, and help to explain the persistence of organizational misconduct.

We also find evidence that firms may insulate themselves from potential stigma and its associated penalties through actions they take in advance of disclosing misconduct. Specifically, we find that stakeholder-imposed penalties for misconduct are diminished when there has been a recent appointment of a new outsider CEO. This act distances the organization from a scapegoat (i.e., the outgoing CEO) towards whom the stigma triggered by the misconduct has been redirected (Elsbach and Sutton 1992; Wiesenfeld, Wurthmann and Hambrick 2008). The dampening effect of CEO change on penalties is also stronger for recidivists than for first-time offenders. These results are consistent with the idea that the stigma of misconduct can be redirected from firms to outgoing organizational elites, even when the leadership change has happened in advance of the stigmatizing announcement. This susceptibility of

admissions of misconduct to reframing in light of prior CEO change suggests that firms may be able to both prevent stigma from arising before an initial admission of misconduct, as well as mitigate the consequences of stigma when the CEO change is made prior to a subsequent admission of misconduct.

These two effects—that stigma can license misconduct and that firms can insulate themselves against stigma’s effects—both lower the penalties imposed by external stakeholders for admissions of organizational misconduct. These effects have dramatic implications for society, through helping us better understand the reasons behind organizational recidivism, as well as for firms, which we show here have at least one way to manage the potential stigma associated with misconduct and its consequences. This paper also offers two new perspectives on organizational stigma, by showing that being stigmatized does not universally result in worse outcomes for firms, and by showing that stigma—and the prospect of stigma—can be managed proactively by firms in anticipation of a stigmatizing announcement

From an empirical perspective, this paper is the first to focus on a crucial subset of firms that engage in misconduct: recidivists. This fact highlights the general importance of better understanding recidivism in organizational contexts. By focusing on recidivists as our population of interest, we enhance both theory and our practical understanding of misconduct by demonstrating why it might persist in this crucial subset of offending firms. Second, this paper also extends our empirical understanding of how preemptive actions undertaken by firms can influence the consequences of disclosing behavior that elicits stigma. Finding that new leadership changes how stigmatizing announcements are perceived (and thus penalized) by external stakeholders provides important information for firms wanting to mitigate potential negative consequences when misconduct has occurred and will need to be made public. This finding also speaks to some of the conflicting findings of the CEO succession literature, as it suggests that organizational stakeholders value the appointment of outsiders, who can bring a fresh perspective to a firm following misconduct, rather than leaving the problems to be managed by a trusted insider.

Though the primary contribution of this work is to the growing literature on organizational stigma, these results also speak to the literature on rehabilitative processes around misconduct (Pfarrer et al.

2008a), and the literature on desensitization as a key part of corruption processes (Ashforth and Anand 2003). Theory on rehabilitative actions had focused on what happens to stigmatized firms “after the fall” (Pfarrer et al. 2008a). In testing what firms can do “before [and between] the fall,” we contribute to understanding how firms may manage the consequences of misconduct in a proactive way in advance of disclosing misconduct, as well as in a rehabilitative way after such a disclosure. In addition, this study broadens the literature on desensitization, which discusses how continued exposure to a certain type of behavior progressively weakens reactions to that behavior (Ashforth and Anand 2003). Desensitization to unethical behavior has been not been studied empirically (Moore 2009); however, these results indicate that when external audiences are repeatedly exposed to similar admissions of misconduct for a given firm, penalties for that misconduct commensurately diminish, representing an important extension of this literature.

We are curious about whether, in the absence of escalating penalties for repeat offenses, firms stigmatized by a first instance of misconduct may become members of a stigmatized subculture in which being acknowledged by external audiences as legitimate is less critical than preventing further penalties for persistent misconduct. Work on illicit subcultures has noted that the illegitimacy associated with the label “criminal” is more important to those outside illicit subcultures, because the subculture provides its own opportunities to compete in status hierarchies for prestige and other rewards (Matsueda et al. 1992). Our findings suggest that a similar phenomenon may occur within organizational populations, which also gives rise to a possible alternative explanation for our findings: firms’ investors have heterogeneous preferences, and investors who care about firm misconduct may sell their shares after a first admission. Thus, the abnormal returns for a second or later restatement might capture penalties from a different set of investors than a first restatement. We cannot rule out with these data that the investors in a recidivist firm are different than those of a first-time offender, pointing to an important future research direction investigating the stability of communities that penalize organizational misconduct. While appraisals of and penalties for misconduct vary considerably across audiences (Devers et al. 2009), they also might

change temporally within an audience, particularly when group membership is highly fluid. It could be useful for future research to track investor movements within these firms over time to get a better sense of how stakeholder groups change and vary their responses to misconduct.

This study focuses on large, publicly traded firms, and the penalties imposed by a very specific type of audience: investors. Doing so allows us to analyze fine-grained responses to misconduct by a specific group, while minimizing the temporal lag between the misconduct and the penalty. We are limited, however, in our ability to generalize these findings to smaller or privately held organizations. As prior research has identified (Karpoff, Lee and Vondracik 1999), more powerful or larger firms may incur less severe market penalties due to a lack of investor alternatives (Greve, Palmer and Pozner 2010). In other words, size, which leads to a lack of investor alternatives, may enable these firms to more easily offset penalties associated with misconduct. Further research is needed to verify the generalizability of our findings to other types of firms, yet we expect that the same underlying mechanisms, licensing and insulation, will still be at play for a variety of organizational types.

An additional limitation of this study is that there are likely restatements that occurred prior to 1997 that are not recorded in our data. If this were the case, some restatements would be misclassified as a first restatement, instead of a second or later restatement. While this is plausible, over the past decade the number of reported restatements has grown exponentially, which reduces the likelihood that a disproportionate number of the restatements in these data are misclassified. By 2006 there were a total of 2309 restatements; however, of those only 92 were recorded in 1997. Moreover, if some restatements were indeed coded incorrectly, it makes our test for how recidivists are penalized more conservative, because the inclusion of a second restatement (where the market penalties are lower) would bias the effect of a first restatement downward, reducing the difference between first and repeated restatements.

It is important to clarify that we do not argue that being stigmatized, or the prospect of being stigmatized, is a positive condition. Decades of research confirms that stigma has multiple and wide-ranging negative implications (Crocker, Major and Steele 1998; Kurzban and Leary 2001). We have

shown, however, that stigma triggered by misconduct offers has least one positive consequence: lower penalties for engaging in additional misconduct. We have also shown that stakeholder responses to misconduct can be positively affected by symbolic acts such as bringing in a new outsider CEO in advance of making that misconduct public. By presenting two distinct paths through which the penalties of engaging in misconduct can be dampened, we have advanced our understanding of organizational stigma, and why misconduct may be so intractable.

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TABLE 1: Average cumulative abnormal returns in response to earnings restatements

Interval	First Restatement (N=282)			Second or later Restatement (N=155)			All Restatements (N=437)		
	CAR	t-statistic	% Positive	CAR	t-statistic	% Positive	CAR	t-statistic	% Positive
[-12, -3]	-1.24%	-1.357 +	43%	-1.31%	-1.247	43%	-1.26%	-1.833 *	43%
[-2, 0]	-3.49%	-6.985 ***	35%	-1.65%	-2.884 **	45%	-2.84%	-7.524 ***	38%
[0, +1]	-4.19%	-10.270 ***	37%	-1.04%	-2.215 *	46%	-3.07%	-9.968 ***	40%
[-2, +1]	-5.29%	-9.165 ***	35%	-1.95%	-2.942 **	42%	-4.11%	-9.418 ***	38%
[+2, +11]	0.05%	0.052	69%	0.74%	0.975	53%	0.29%	0.427	50%

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$

Notes: The reported *t*-statistics are used to test for statistical significance of the cumulative average abnormal returns, where the null hypothesis is no cumulative average abnormal return for the event period. ‘% Positive’ refers to the percentage of firm CARs that were positive for the event period.

TABLE 2: Distribution of cumulative abnormal returns for restatement announcements (-2/+1 days)

Percentile	First Restatement CARs	Second or later Restatement CARs	All Restatements
90%	6.07%	8.96%	7.08%
75%	1.27%	2.40%	1.77%
50% (Median)	-2.34%	-1.13%	-1.64%
25%	-7.86%	-5.44%	-6.73%
10%	-19.54%	-12.28%	-16.54%

TABLE 3: Variable Descriptive Statistics and Correlations

	Mean	Std. Dev	1	2	3	4	5	6	7	8
1 CARs (-2/+1 Days)	-0.041	0.127								
2 S&P 500	0.279	0.449	0.136							
3 NYSE	0.568	0.496	0.100	0.379						
4 NASDAQ	0.412	0.493	-0.087	-0.355	-0.959					
5 Amend 10K	0.668	0.471	-0.003	-0.038	0.042	-0.052				
6 Quarters Restated	4.629	4.981	0.001	0.031	-0.029	0.003	0.246			
7 SEC Prompt	0.172	0.377	0.060	0.163	0.054	-0.036	0.089	0.266		
8 Auditor Prompt	0.092	0.289	-0.164	-0.038	0.005	-0.008	0.072	0.017	-0.144	
9 Revenue Recognition	0.227	0.419	-0.117	-0.093	-0.134	0.114	-0.048	0.024	0.015	-0.001
10 Fraud	0.094	0.292	-0.033	-0.043	-0.004	0.002	0.060	0.183	0.041	0.034
11 Shares Outstanding (000 000s)	220.090	459.828	0.104	0.467	0.207	-0.191	-0.130	0.031	0.107	-0.022
12 Reduces Net Income	0.627	0.484	-0.039	-0.016	0.014	-0.018	0.029	0.109	-0.051	0.114
13 Net Effect	13.065	6.444	0.001	0.113	0.010	0.007	0.009	0.122	0.097	0.067
14 Return on Assets	-3.976	22.966	0.138	0.194	0.207	-0.194	0.014	-0.001	0.073	-0.011
15 Total Assets (000 000s)	7.373	2.130	0.145	0.496	0.472	-0.441	-0.049	-0.025	0.007	0.033
16 Firm Reputation	1.157	2.268	0.119	0.567	0.373	-0.361	-0.056	-0.036	0.075	-0.065
17 Second or later Restatement	0.355	0.479	0.126	0.008	0.029	-0.037	-0.016	0.095	-0.020	0.030
18 CEO Change	0.330	0.471	-0.016	-0.046	-0.017	0.017	-0.044	0.250	0.030	-0.020
19 New Outsider CEO	0.112	0.316	0.015	-0.076	0.003	-0.003	-0.058	0.152	-0.008	0.063
20 New Insider CEO	0.220	0.415	-0.038	0.015	-0.017	0.016	-0.013	0.165	0.037	-0.073

	9	10	11	12	13	14	15	16	17	18	19
10	0.013										
11	0.014	-0.003									
12	0.090	0.135	-0.024								
13	0.000	0.098	0.127	0.621							
14	-0.052	-0.016	0.087	-0.038	0.032						
15	-0.201	-0.025	0.321	0.056	0.149	0.226					
16	-0.022	0.011	0.505	-0.014	0.009	0.137	0.449				
17	0.056	-0.042	0.066	0.018	0.057	-0.082	0.007	-0.002			
18	0.063	0.108	-0.012	0.057	0.026	-0.112	-0.051	0.017	0.081		
19	0.068	0.060	-0.037	0.064	-0.016	-0.202	-0.055	-0.069	0.100	0.491	
20	0.017	0.095	0.014	0.021	0.045	0.027	-0.012	0.069	0.023	0.757	-0.189

Notes: $N = 437$ (matched sample). Correlations greater than and equal to 0.095 are significant at $p < 0.05$.

TABLE 4: Abnormal stock market returns by restatement announcement and CEO change

Dependent Variable: CARs (-2/+1 Days)	(1)	(2)	(3)	(4)	(5)
S&P500	0.028 (0.018)	0.027 (0.018)	0.028 (0.018)	0.062 (0.044)	0.065 (0.043)
NYSE	0.032 (0.032)	0.031 (0.031)	0.027 (0.030)	-0.025 (0.060)	-0.016 (0.061)
NASDAQ	0.038 (0.033)	0.038 (0.032)	0.037 (0.031)		
Amend 10k	-0.004 (0.014)	-0.001 (0.013)	0.001 (0.014)	0.012 (0.024)	0.024 (0.024)
Quarters Restated	0.001 (0.001)	-0.000 (0.001)	-0.000 (0.001)	0.000 (0.002)	0.001 (0.002)
SEC Prompt	0.008 (0.016)	0.009 (0.016)	0.009 (0.016)	0.008 (0.032)	-0.007 (0.032)
Auditor Prompt	-0.054 (0.034)	-0.057 (0.034)	-0.060+ (0.035)	-0.038 (0.038)	-0.040 (0.039)
Revenue Recognition	-0.022 (0.020)	-0.023 (0.020)	-0.025 (0.020)	-0.008 (0.043)	-0.002 (0.040)
Fraud	0.004 (0.029)	0.005 (0.029)	0.004 (0.028)	-0.004 (0.034)	0.005 (0.033)
Shares Outstanding (000 000s)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Reduces Net Income	-0.001 (0.015)	0.001 (0.015)	-0.001 (0.015)	-0.020 (0.032)	-0.024 (0.031)
Net Effect	-0.000 (0.001)	-0.001 (0.001)	-0.000 (0.001)	-0.000 (0.002)	-0.000 (0.002)
ROA	0.000 (0.000)	0.001 (0.000)	0.001 (0.000)	-0.000 (0.001)	0.000 (0.001)
Total Assets (000 000s)	0.003 (0.004)	0.003 (0.004)	0.003 (0.004)	-0.003 (0.020)	-0.005 (0.020)
Firm Reputation	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	-0.002 (0.010)	0.005 (0.012)
Second or later Restatement		0.036* (0.015)	0.035* (0.014)	0.035+ (0.021)	
New Insider CEO			-0.009 (0.015)		
New Outsider CEO			0.047* (0.020)		
First restatement, no CEO change					-0.138** (0.052)
First restatement, insider CEO change					-0.139* (0.068)
First restatement, outsider CEO change					-0.090+ (0.047)
Second restatement, no CEO change					-0.110* (0.049)
Second restatement, insider CEO change					-0.116+ (0.066)
Constant	-0.080 (0.060)	-0.078 (0.057)	-0.099+ (0.054)	-0.017 (0.168)	0.150 (0.176)
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	No	No	No	Yes	Yes
Restatement observations	437	437	437	296	296
Firm observations	233	233	233	141	141
R-squared	0.126	0.141	0.155	0.102	0.139

Note: Robust firm clustered standard errors in parentheses. ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Second restatement, outsider CEO change is the omitted category for model 5.

FIGURE 1: Cumulative Market-Model Abnormal Return for Earnings Restatements

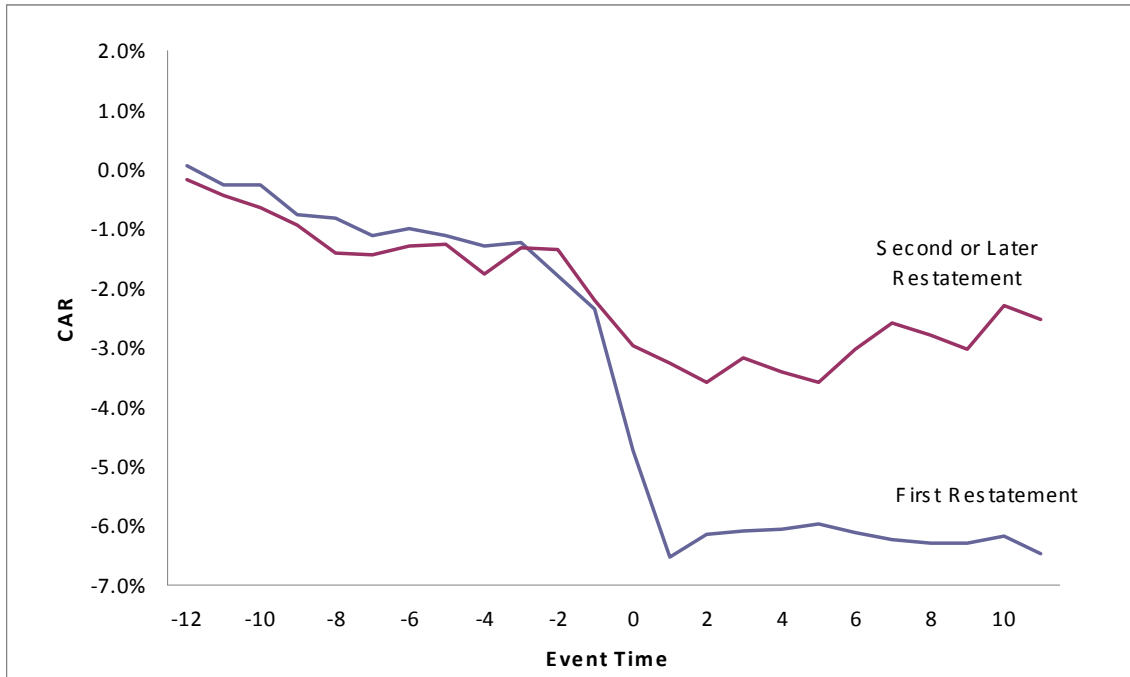


FIGURE 2: Stock market reactions to earnings restatements with no prior CEO change, insider CEO change and outsider CEO change

