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## Repeat Offenders in Organizational Wrongdoing: Do Recidivists Face Worse Penalties than One-Timers?

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**WORKING PAPER**

The licensing effects of stigma:

Declining reputational penalties as firms persist in organizational misconduct

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Research robustly the negative consequences of organizational misconduct. Firms that engage in misconduct—such as fraud, law-breaking, tax evasion, anti-trust violations, and misleading or misrepresentative accounting practices—experience significant losses in shareholder value and performance (Akhigbe, Kudla, & Madura, 2005; Baucus & Baucus, 1997; Davidson & Worrell, 1988; Harris, 2007; Palmrose, Richardson, & Scholz, 2004; Wu, 2003), diminished expectations of earnings and subsequent increases in the cost of capital (Farber, 2005; Hribar & Jenkins, 2004), and greater likelihoods of facing shareholder class action lawsuits (Lu, 2004). Given these consequences, it would be reasonable to think that after experiencing the outcomes of an initial act of misconduct, organizations would repent, change their practices, and operate legitimately again. Recent theory confirms this intuition (Pfarrer, DeCelles, Smith, & Taylor, 2008a). However, the only evidence we have about the persistence of corporate misconduct seems to demonstrate that organizations frequently and repeatedly engage in acts of wrongdoing. Available data suggest that more than half of firms violate the law, and about half of those firms will end up re-offending (Clinard & Yeager, 1980; Davidson, Worrell, & Lee, 1994). These two facts—that the consequences of misconduct are severe, but that a substantial proportion of firms repeatedly engage in misconduct—are not easily reconciled, and lead to the obvious question: if the consequences of organizational misconduct are so severe, why do organizations risk re-offending?

### **THE CONSEQUENCES OF AND ATONEMENT FOR REPEATED MISCONDUCT**

We know almost nothing about the persistence of organizational misconduct over time, even though researchers studying the ‘dark side’ of organizations have been vocal in their concern about the intractability of corporate corruption (Ashforth, Gioia, Robinson, & Treviño,

2008; Salinger, 2004). From the little we know about recidivism in organizational crime, it appears that a minority of firms engage a disproportionate percentage of law violations; one study of close to 600 of the largest publicly owned corporations in the U.S. found that 38 firms (13% of the sample) accounted for 52% of all violations charged during the 2-year study period (Clinard & Yeager, 1980). This fact highlights the importance of understanding recidivism in organizational misconduct: if we can understand the process of recidivism better, we have a better chance of discouraging a significant proportion of organizational misconduct generally.

However, the research that has examined the consequences of misconduct tends to examine misconduct as a one-time event (Agrawal, Jaffe, & Karpoff, 1999; Akhigbe et al., 2005; Bromiley & Marcus, 1989; Karpoff & Lott, 1993; Palmrose et al., 2004), sidestepping the questions of what firms do “after the fall” (Pfarrer et al., 2008a). In fact, the work that has noted recidivism at all has either drops recidivists from their samples as unnecessary complications (e.g., Arthaud-Day, Certo, Dalton, & Dalton, 2006; Harris, 2007; Harris & Bromiley, 2007) or uses recidivism as a control variable (e.g., Pfarrer, Smith, Bartol, Khanin, & Zhang, 2008b). The exception to this is a study of the longer-term performance implications of illegal behavior, which did find that the performance declines felt by firms after a first conviction apparently did not deter subsequent illegal behavior (Baucus & Baucus, 1997). Yet we know almost nothing about the potentially different consequences faced by organizations that become repeat offenders.

On the contrary, much of the criminological literature is focused on understanding recidivism precisely because a criminal record remains one of the best predictors of crime, and as such recidivists are responsible for the majority of criminal offences (Gendreau, Little, & Goggin, 1996; U.S. Department of Justice, 2000). Because of this, most western criminal justice systems are built on the premise of escalating penalties for repeat offenders: for both individual criminal

offenders and corporations, both penalties and enforcement norms are typically designed to punish recidivists more severely than first-time offenders (Dana, 2001).

### **How Is Repeated Misconduct Penalized?**

The question of whether organizations that persist in misconduct suffer more severe or lenient consequences than organizations for which misconduct is a one-time event is important, because unless the consequences for persistent misconduct do not remain at least as severe as they were for the first-time offender, firms might not have the motivation necessary to change their practices and return to legitimate modes of operating (Baucus, 1994). This concern leads to the main research question we were interested in pursuing in this paper: *do the penalties for engaging in misconduct persevere at the same level, escalate, or abate for firms engaging in repeated acts of misconduct, compared to one-time offenders?*

In investigating this research question the main answer we propose is counterintuitive: we suggest that the consequences to firms of engaging in misconduct actually abate—rather than escalate—across multiple incidents. We develop this hypothesis by drawing on the literature on stigma (starting with Goffman, 1963), and make the argument the stigma associated with firms upon an initial event of misconduct *unintentionally provides those firms license to continue engaging in similar illegitimate activities without facing additional penalties*. We claim that the devaluation of the firm associated with the first stigmatizing event of misconduct eliminates future expectations that the firm will again operate legitimately, and that these lowered expectations depress audience reactions to future acts of misconduct. Finding empirical support for this claim would provide an important explanation for the apparent irreconcilability of why so many corporations engage in repeated misconduct when the (original) consequences of

misconduct are so severe: because they do not suffer the same consequences the second time around.

### **Are Re-Legitimizing Efforts Effective for Stigmatized Firms?**

We also know very little about the efficacy of actions that firms undertake to restore their legitimacy once they have engaged in misconduct. Obviously, organizations are very committed to increasing positive perceptions of their firms and to mitigating the penalties they face for actions which might generate negative consequences, thus avoiding being categorized as illegitimate (Rao, 1994; Scott, 1987). Perhaps one of the reasons why firms risk re-offending is because they believe that actions they have undertaken to restore their legitimacy have worked. However, though the reasons behind re-legitimizing actions have been theorized (e.g., Pfarrer et al., 2008a; Wiesenfeld, Wurthmann, & Hambrick, 2008), to our knowledge their efficacy has never been tested. One of the most common rehabilitative measures undertaken by firms is to change their CEO (Arthaud-Day, Certo, Dalton, & Dalton, 2006; Hennes, Leone, & Miller, 2008), thereby signalling to stakeholders that the firm is committed to positive change. Therefore, the second research question we examine in this paper investigates the effectiveness of leadership change (Arthaud-Day et al., 2006; Hennes et al., 2008) and specifically considers whether this re-legitimizing effort effectively dampens the reputational penalties faced by firms which persist in misconduct. In other words: *are leadership changes effective in protecting firms against future reputational penalties faced by firms after already stigmatized by misconduct?* We suggest that a change in firm leadership prior to re-offending is able to further buffer a firm against the usual penalties because it fundamentally changes the way the activity is perceived. Whereas a first act of misconduct is commonly regarded as negative and stigmatizing, a second event may now be considered a by-product of the rehabilitation process, where the firm

effectively discloses further egregious acts as part of the process to ‘clean up’ the mess left by the former CEO. To our knowledge, this represents the first empirical examination of the effectiveness of rehabilitative actions firms undertake to re-establish legitimacy after misconduct.

However, this conversation would be richer if we could better explain *why* re-legitimizing efforts are effective. We therefore ask a third question, which probes into the different reasons why leadership change might be effective in protecting against future penalties faced by stigmatized firms: *are certain types of leadership change more likely than others to contribute to this protective effect?* Work on senior leadership change has long distinguished between insider and outsider succession (Gouldner, 1954 ; Grusky, 1960 ; Helmich & Brown, 1972), with a general (though not uncontested) view that outsider succession represents a greater commitment to change, while insider succession represents a greater commitment to the status quo. Thus, in the context of leadership change as a rehabilitative act, we examine whether bringing in a new outsider CEO is more positively regarded by external audiences—arguably a more sincere commitment to rehabilitation—than bringing in a new insider CEO.

We now follow by developing our main hypotheses. The first hypothesis addresses the consequences to firms of repeated misconduct by drawing on the literature on stigma (Crocker & Major, 1989; Goffman, 1963; Jones et al., 1984), building the main argument that the admission of misconduct is a stigmatizing act, and that this stigma unintentionally licenses organizations to continue similar behavior in future without confronting similar penalties. We draw additional theoretical support for this hypothesis using the literature on middle status conformity (Phillips & Zuckerman, 2001). The second set of hypotheses address the effectiveness of leadership change as a rehabilitative action prior to the announcement of second or later act of misconduct, and makes the claim that rehabilitative actions undertaken by firms already stigmatized by a first

act of misconduct will be effective in protecting against future firm penalties. We further probe why these rehabilitative actions might be effective by determining whether the protection offered by leadership change is more driven by outsider succession (representing a greater commitment to change) or insider succession (representing a lesser commitment to change).

In answering these two research questions, this paper strives to make a number of important contributions to the literature. Three contributions deserve particular notice. First, this paper focuses for the first time that we know of on a crucial subset of firms that engage in misconduct: recidivists. Since one of the reasons why it is important to better understand organizational misconduct is to develop better ways to discourage or prohibit it, determining the differences in the consequences of misconduct for one-time and repeat offenders is crucial. Second, this paper aims to contribute to the literature on stigma, by positing and empirically testing whether prior stigmatizing acts of misconduct operate to license the stigmatized firm to engage in similar acts of misconduct without suffering the same penalties in future. If so, this paper describes an unexpected potential benefit of being stigmatized: license to behave badly without suffering the same consequences as those who are not stigmatized. Third, this paper extends our empirical understanding of the effectiveness of rehabilitative actions undertaken to restore legitimacy, testing whether rehabilitative actions such as changing corporate leadership before announcing repeated misconduct effectively protects against the reputational penalties stemming from that admission. If so, this paper adds both depth and nuance to our limited understanding about what happens to organizations “after the fall” (Pfarrer et al., 2008a).

## PENALTIES FOR THE STIGMATIZED

Stigma, a “deeply discrediting” attribute (Goffman, 1963: 13) of an individual (Crocker et al., 1989; Goffman, 1963; Jones et al., 1984) or an organization (Paetzold, Dipboye, & Elsbach, 2008), represents a contamination of a social actor’s identity with many spillover effects to other aspects of that actor’s existence. Similar to the literature on misconduct, the stigma literature has focused primarily on negative consequences, and stigma has been shown to lead to many negative consequences including social exclusion, decreased well-being, and reduced opportunities for interaction with legitimate others (Carter & Feld, 2004; Elliott, Ziegler, Altman, & Scott, 1982; Gramling & Forsyth, 1987; Kurzban & Leary, 2001; Link & Phelan, 2001).

There have been a few efforts to uncover any potential benefits of stigma, and most of the efforts there have been have focused on the benefits society accrues through stigmatizing certain groups within it. For example, studies have examined the societal benefits of reducing overall levels of smoking through the stigmatization of smokers (Bayer & Stuber, 2006), and of criminal deterrents through the stigmatization of criminals (Rasmusen, 1996). Organizational scholars have argued that the stigma associated with illegitimate modes of operating is beneficial to society because it creates incentives for firms to function within social and legal norms (Paetzold et al., 2008), though whether these incentives work remains an empirical question. More importantly, all of these efforts to reveal the hidden benefits of stigma focus on the benefits of stigma to the non-stigmatized rather than to the stigmatized.

### **The Licensing Effects of Stigma**

However, there is one consequence of stigmatization which we argue comes with a very important side benefit, which we term the *behavioral licensing effect of stigma*. Stigma is also

often associated with lowered expectations for performance or success; the negative consequences of these lowered expectations have found their most comprehensive examination in the work on stereotype threat, which shows how lowered expectations of individual members of stigmatized social groups often leads their decreased performance (Steele, 1999; Steele & Aronson, 1995). While lowered behavioral expectations of stigmatized groups clearly have many negative repercussions for the stigmatized, it is also possible that these lowered expectations can work to benefit them: because it requires less effort to meet these lowered expectations. Goffman noted this potential advantage of stigma, and recognized that this advantage was familiar to most stigmatized individuals as well, when he wrote: “The stigmatized individual is likely to use his stigma for ‘secondary gains’, as an excuse for ill success that has come his way for other reasons” (Goffman, 1963: 21).

This licensing effect has been found among mental patients and the disabled, for whom the stigma of disability can release individuals from role expectations generally required of other societal members (Haber & Smith, 1971). In a study of formal mental patients, many claim that being released from normative behavioural expectations is a distinct benefit of being stigmatized as someone with a chronic mental illness (Herman & Miall, 1990). Stigmatizing disabilities have even been described as crutches which sufferers can come to depend on “not only as a reasonable escape from competition but as a protection from social responsibility” (Baker & Smith, 1939: 303). It is important not to conflate two important elements of the licensing effect of stigma: (1) at its core, what we propose is that having a stigma can be experienced as freedom to deviate from normative expectations of behavior; (2) however, this licensing effect works because of *how stigma affects how external stakeholders view ones expected behavior*. For example, one of the patients stigmatized by chronic mental illness in Herman and Miall’s study claims:

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... (1990: 258)

We argue that this licensing effect of stigma extends to organizations as well as individuals, such that the admission of an initial act of misconduct places them in a stigmatized category ("fraudulent firms"), perversely benefiting those firms in that key audiences of organizations then expect more deviant behavior from them, and thus penalize them less harshly than non-stigmatized firms when they repeat the misconduct.

*Hypothesis 1. The reputational penalties faced by organizations upon the admission of a repeated act of misconduct are less severe than upon the admission of an initial act of misconduct.*

### **Middle Status Conformity**

Though we have primarily relied on the stigma literature we have used to develop this hypothesis, ideas from the literature on middle status conformity (Phillips & Zuckerman, 2001) also provide support for our ideas. The literature on middle status conformity argues that agents at the high or low ends of status hierarchies conform to normative modes of behavior less than agents in the middle of status hierarchies (Phillips & Zuckerman, 2001). Thus, while both high and low status actors are freed from the pressure to conform, both their nonconformity and their reasons for it differ at either end of status hierarchies (Blau, 1960; Dittes & Kelley, 1956; Phillips et al., 2001). We focus here only on how being a member at the low end of a status hierarchy (i.e., a stigmatized firm) frees that firm from the otherwise strong pressures to conform to normative practice (i.e., operate legitimately from an accounting perspective), and argue that

one reason why low status firms are freed from these operational prescriptions is because key audiences (i.e. the stock market) do not punish low status firms as harshly for deviations from normative behavior.

This is a peripheral and new application of the theory behind middle status conformity. Most of the work on middle status conformity has focused, as its name suggests, on how and why actors in the middle of status hierarchies operate differently than agents at either of the ends of status hierarchies, in particular when it comes to conforming to normative practice in that context (Blau, 1960; Menzel, 1960; Phillips et al., 2001). When attention has been paid to the ends of the spectrum, the focus has tended to be on how agents with high status operate differently from those in the middle. For example, Hollander's work on idiosyncrasy credits is entirely focused on those at the high end of the status hierarchy—arguing that individuals demonstrating high performance, leadership emergence, or other indicators of high status accrue “idiosyncrasy credits” which can be spent behaving counter-normatively without reducing their influence over other group members (Hollander, 1958, 1964). In other words, *audiences react more leniently to the behavioral deviations of high status group members.*

However, less work has been done theorizing about why those at the low end of status hierarchies operate counter-normatively. Phillips and Zuckerman (2001) argue that low status actors have wider behavioral flexibility because, whatever their actions, they are likely to remain fixed at the lower ends of status hierarchies. We agree that the intransigent nature of being stigmatized as low status by operating illegitimately is likely to free those firms to continue to operate illegitimately. Yet this only explains why firms feel licensed to operate illegitimately; it does not explain why an audience's reaction to firms for operating illegitimately may be more lenient for low status firms than for others. We therefore apply the idea of “idiosyncrasy credits”

(Hollander, 1958, 1964) to low status firms (where low status is due to misconduct), and argue that a firm's previous misconduct liberates it in the eyes of the public to continue to operate illegitimately. We argue that the stigma associated with a first time offence provides low status firms with an alternate form of an "idiosyncrasy credit", liberating deviations from typical operational requirements expected of legitimately operating firms without commensurate penalties by external audiences.

### **REHABILITATIVE ACTIONS FOR THE STIGMATIZED**

Given the serious consequences that organizations face after misconduct, ways in which they can effectively dampen future penalties arising after misconduct could be crucial to firm survival. We know very little about whether rehabilitative actions are actually effective in re-establishing firms' legitimacy or in protecting firms against future penalties. In this section of the paper, we examine the effectiveness of one specific rehabilitative action—CEO change—in protecting firms against the penalties associated with misconduct.

After episodes of misconduct, senior leadership change is one of the most common actions that firms take to demonstrate rehabilitation and communicate their commitment to substantive changes in the way they operate (Arthaud-Day et al., 2006; Hennes et al., 2008). For example, recent research has found that CEO exit is twice as likely at firms that had filed a material restatement to their financial statements within the prior two years than at other comparable firms which had not done so (Arthaud-Day et al., 2006). To our knowledge, the effectiveness of leadership change in protecting firms from the penalties associated with misconduct has only been discussed theoretically (Pfarrer et al., 2008a; Wiesenfeld et al., 2008) but not been empirically tested. Our first hypothesis thus follows directly from this previous

theorizing about rehabilitative action, which is that taking a proactive measure, such as changing the public face of the organization, demonstrates commitment to positively changing the way a stigmatized firm operates, and effectively mitigates the negative effect of recurrent acts of misconduct.

*Hypothesis 2. CEO change prior to a repeated incident of misconduct protects against the reputational penalties faced by firms following those instances of misconduct.*

The perhaps more interesting question following from this hypothesis is to determine why these legitimizing efforts are effective. To begin to probe this question, we examine whether different types of CEO change offer different levels of protection from future reputational penalties. As we discussed in the introduction, work on senior leadership change has long distinguished between insider and outsider succession (Gouldner, 1954 ; Grusky, 1960; Helmich & Brown, 1972), with the general view that external succession represents a greater commitment to change, while insider succession represents a greater commitment to the status quo. One can develop an argument for either of these types of CEO change being more effective in protecting against reputational penalties for stigmatized firms.

One might think that outsider CEO change might be more effective in protecting against the penalties associated with misconduct. There are both symbolic reasons and substantive reasons why outsider CEO change would be more effective in protecting firms against future reputational penalties than insider CEO change. The symbolic message conveyed by a new outsider CEO is of a serious commitment to change (Friedman & Singh, 1989). The organization might hope that the appointment of an outsider CEO would help relieve the organization of the stigma of misconduct, more effectively placing the blame for the misconduct and thus the stigma

on the fired CEO (Wiesenfeld et al., 2008). The new CEO therefore symbolically represents bringing in new blood untainted by the stigma of misconduct. Outsider CEO appointments are more frequent after poor performance, of which the need for restatements is arguably demonstrative (Schwartz & Menon, 1985). Outsider CEO appointments are generally regarded as more favourable by the stock market, especially when the incumbent CEO has been forced to resign (Borokhovich, Parrino, & Trapani, 1996). Substantively outsider CEO succession represents bringing in a new team with new experience and without records of misconduct: a sincere “cleaning house”. Hypothesis 3a therefore tests the particular moderator of outsider CEO change as the driver of the protective effect of CEO change against the reputational penalties associated with misconduct.

*Hypothesis 3a. Outsider CEO change prior to a repeated incident of misconduct will be more protective against the reputational penalties faced by that firm following those instances of misconduct compared to no CEO change.*

If the appointment of an outsider CEO offers more protection against future reputational penalties than no CEO change, that would suggest that external audiences care about symbolic and substantive re-legitimization efforts in organizations after misconduct.

These arguments in favour of outsider CEO succession as effective protection against reputational penalties could imply that insider CEO change would offer worse protection against those penalties. As outsider CEO appointments are generally regarded favourably by the stock market, insider CEO appointments are often regarded negatively, especially when an insider appointment follows a chief executive resignation (Borokhovich et al., 1996). However, these negative returns are reactions to the appointment of the insider CEO, and as such are not reflective of how that appointment might lead to *future* protection against other triggered

reactions. Again, there are both symbolic reasons and substantive reasons why insider CEO change rather than outsider CEO change would offer better protection against the penalties associated with future admissions of misconduct.

Symbolically, while outsider CEO succession represents a more “deep cleaning” of the organization, insider CEO succession represents a more “surface cleaning” of the organization, since an insider CEO downplays the severity of the initial act of misconduct while still ensuring there is a target of blame for it (Wiesenfeld et al., 2008). Substantively, research has shown that insider CEOs are preferable in that they are more of a known entity than outsider CEOs (Zajac, 1990): they know where the skeletons are (have more company specific knowledge), board and analysts have better information on them, and they have an established internal network which may allow them to clean up the circumstances around the new admission of misconduct more quickly (they are efficient). If the appointment of insider CEOs offers more protection against future penalties compared to firms with no CEO change, that would suggest that external audiences value care more about maintaining the status quo and internal corporate stability than they do about substantive organizational change after misconduct.

*Hypothesis 3b. Insider CEO change prior to a repeated incident of misconduct will be more protective against the reputational penalties faced by that firm following those instances of misconduct compared to no CEO change.*

Though hypotheses 3a and 3b could be competing, since there are arguments for why both outsider and insider CEO change might be driving the protective effects of CEO change in general, we acknowledge that both of these explanations might be at play.

## METHODS

### Research Context

The context that we use to examine this phenomenon is to look at the consequences, in terms of how an important group of stakeholders (investors) react (in terms of abnormal stock market returns) to recurrent admissions on behalf of firms of misconduct (restatement announcements).

### Sample and Data

The sample of restating firms was drawn from two databases issued by the GAO covering consecutive periods totalling nine and a half years. The first report covers restatements initially announced between January 1, 1997 and June 30, 2002 (U.S. Government Accountability Office, 2003), and the second covers restatements initially announced between July 1, 2002 and June 30, 2006 (U.S. Government Accountability Office, 2006). The same methodology for identifying a comprehensive set of restatements was used in developing both databases for their respective time periods. Not all financial restatements are a result of organizational misconduct. However, the GAO excluded all restatements resulting from “routine” matters, and so the GAO only included records in their databases cases where the restatement was a result of 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. Government Accountability Office, 2002: 76). The firms included in this sample have therefore been identified by the U.S. Government as having engaged in misconduct and the restatement events that comprise the records identified by the U.S. Government as having been a result of suspicious if not

intentionally fraudulent behavior. Together the two databases comprised 2309 restatement announcements (919 from 1995 through mid-2002, and 1390 from mid-2002 through mid-2006).

To conduct the appropriate analyses to test our hypothesis, it was necessary to construct a matched set of single-restating firms and multiple-restating firms. Identifying a causal effect is a challenge when using observational data due to the possibility of selection bias. For these data there are likely systematic differences between firms who restate only once, and those who restate multiple times. For example, it is possible that firms who restate repeatedly have demonstrated worse performance than single restating firms, and therefore are more likely to risk restating their financial statements over and over again. That is, whether or not a firm restates multiple times is not determined randomly; instead, firms select into this condition.

We accordingly employ propensity score matching methods (Dehejia & Wahba, 2002; Rosenbaum & 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, & Lee, 1995; Daily & Schwenk, 1996; Zajac & Westphal, 1994) and are frequently used by researchers studying restatements (e.g., Agrawal et al., 1999; Arthaud-Day et al., 2006; Richardson, 2005). The two approaches are similar in that they both involve the researcher constructing a sample of non-treated (i.e. single-restating) firms to serve as a control for the treated (i.e. multiple restating) sample of firms. 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 treated firm and a control firm is feasible when the match is being made on only a few dimensions (for example, industry and geographic region). As the number of dimensions grows, so does the likelihood that no firm with the exact same attributes

exists (the so-called curse of dimensionality). Instead, firms can be matched on a propensity score based on their probability of belonging to the treatment group (Rosenbaum & Rubin, 1983).

There are two steps to propensity score matching. First, a propensity score is calculated for every firm in the sample. A propensity score represents the likelihood of that firm ending up in the treatment condition (in this case, the likelihood that the firm eventually restates more than once), based on observable pre-treatment covariates (our control variables). Therefore, firms who eventually restate more than once are matched to firms who only restate once based on the multiple restatement firms' characteristics before their second restatement (i.e. before they end up in the treatment condition). As Table 1 indicates, the pretreatment covariates between these two groups are not balanced. In other words, there exist significant differences between firms who only restate once in our sample, and those firms who go on to restate at least a second time. Accordingly we estimated propensity scores for our GAO database sample of first restatements (190 firms would go on to restate again, and 452 would not) using a logit model (the exact specification that we used is available from the authors upon request). After we created a new matched sample using the propensity scores, significant differences between single and multiple restating firms on these control variables no longer exist.

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Insert Table 1 about here  
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Second, once propensity scores have been calculated for each treatment and control firm, we created a matched sample using nearest neighbor matching with replacement, where single and multiple restaters were matched to their nearest neighbour on the propensity score (i.e., each multiple restating firm was matched with the single restating firm with the closest propensity score). Matching with replacement means that multiple restating firms were matched with their

closest counterpart, even if that single restating firm had already been used as a match. This means that for our control sample, multiple observations for the same firm could exist. In practice the majority of the firms in our control sample were matched once ( $n = 60$ ), ten firms were matched twice, another eleven firms were matched three times, and 2 firms were matched four times. This left us with a matched data set comprising 141 single restating control firms and 141 multiple restating firms. This matched sample of first restatements for the treated and control firms were then appended with the treated firm's second or later restatements. We dropped observations relating to second or later restatements when we did not have data on that firm's first observation. This final data set is comprised of 443 restatement events, where firms restate anywhere from one ( $n = 141$ ) to 7 times ( $n = 1$ ).

### **Measures**

These data were compiled from a number of sources. The dates of the restatement announcements, NYSE (New York Stock Exchange) listing, shares outstanding, the prompter of the restatement (the company, an auditor or the SEC) and the reason for the restatement were drawn from a database developed by the U.S. Government Accountability Office (GAO). Data relating to other attributes of the restatement and data that were missing from the GAO were coded from firm press reports and filings using the SEC's EDGAR database. CEO change information was calculated using information from Standard and Poors' Execucomp that summarizes executive data collected from the company's annual proxy. Information pertaining to a CEO's history prior to his or her appointment also came from filings in the EDGAR database. Financial data was collected from Compustat and stock market information from CRSP (Center for Research in Security Prices).

**Dependent variable. *Stakeholder reaction to restatement events.*** Consistent with prior work on reputational penalties (Agrawal & Chadha, 2005; Bromiley & Marcus, 1989; Davidson et al., 1994; Karpoff & Lott, 1993; Palmrose et al., 2004), we used cumulative abnormal stock returns (CARs) as our dependent variable to measure the penalties faced by firms after misconduct. More specifically, we used a model that estimates daily market-adjusted abnormal returns based on an equally-weighted CRSP index. Further, abnormal returns were standardized by weighting the stock by its standard deviation. These standardized daily abnormal returns are aggregated to determine the standardized CAR for a given period. CARs are a measure of the extraordinary (positive or negative) returns to a firm's stock over a time period defined by the researcher, after controlling for what would have been a normal trajectory of that stock's price given historical information on the stock and the value of the index for the exchange that the stock trades on. Computing a normal return (from which to base the determination of an abnormal return) involves choosing an estimation window prior and non-overlapping with the event window (McWilliams & Siegel, 1997). We set our estimation window at 46 to 255 trading days prior to our event window, and an event window of 3 days prior to 3 days post the restatement announcement.

**Independent variables. *Second or later restatement.*** We coded restatements in the GAO database '0' if it was the first restatement by the firm in the database and coded the restatement '1' if it was a second or later restatement by the firm. We collapse the multiple restatement events into one group because we find no significant difference between the effects of a second ( $n=135$ ) or third restatement ( $n=45$ ) on SCARs when compared to a first restatement. There were few restatement events within a firm greater than three (restatement events four or greater totalled 14).

**CEO change.** We created a dichotomous variable to assess CEO change. If a CEO was new to the firm either in the restatement calendar year or in the calendar year prior to the restatement, the variable was coded 1 and 0 otherwise. Using the executive biographical information from the firm's proxy statement we were able to differentiate between CEOs who were promoted internally and those who came from outside the company. To test hypothesis 3, we used three categories: *no CEO change* for firms that did not change their CEO prior to the restatement; *outsider CEO change* for new CEOs whose prior position were not within the restating firm; and *insider CEO change* for new CEOs whose prior position was within the restating firm. For these analyses no CEO change was the referent category.

**Control variables.** We include controls for *firm quality* and *firm reputation* to eliminate the alternative explanation that firms of high quality or with high reputations are protected against the reputational damage of second or later restatements. Firm quality variables include (1) *S&P500*, a dummy variable for whether the firm is included in the S&P500, and (2) *NYSE* a dummy variable for whether the company is listed on the New York stock exchange, with companies listed on NASDAQ or lower-status exchanges (such as the American Stock Exchange or the National Stock Exchange) as the referent category. *Firm reputation* was a dummy variable, with "1" representing that it had been included on the *Fortune* reputation survey in either of the prior two years (with 0 if the firm had not been included in either year). This survey asks over 8,000 executives, directors, and securities analysts to rate a list of the ten largest companies in their industries on the following dimensions: social responsibility, innovation, long-term investment value, use of corporate assets, employee talent, financial soundness, quality of product/service, and quality of management.

We include a number of controls for the *seriousness of the restatement* to ensure that our main independent variables are capturing only the variance attributable to the recidivism itself and not the seriousness of the re-offending (or original) misconduct. (1) *Amend 10K*, a dummy variable for whether the 10K was amended as part of the restatement, was included since restatements which involve the annual report have been found to trigger more severe reactions than restatements which involve only a quarterly report (10Q) (Wu, 2003). (2) *Quarters Restated*, a variable capturing the total number of fiscal quarters involved in the restatement, was included since restatements involving a longer time frame would likely be considered more serious than restatements involving a shorter time frame. (3) *Prompt SEC* and *Prompt Auditor*, two dummy variables capturing the body prompting the restatement (with firm-prompted restatements as the referent category) were included since firms which restate at the prompting of these two bodies suffer worse consequences than firms which restate voluntarily (Akhigbe et al., 2005; Wu, 2003). (4) *Reduced net income*, a dummy variable for whether the restatement resulted in an overall reduction in net income, was included since restatements resulting in an overall reduction in net income have been shown to have more severe penalties those which do not (Akhigbe et al., 2005). (5) *Net effect* represents, in dollars, the total amount the restatement affects total income of the company, which we include since the overall effect of the restatement on net income has been shown to affect the market response to restatement announcements (Feroz, Park, & Pastena, 1991). (6) We also included two dummy variables for particularly serious types of restatements: *Revenue recognition* and *error involving fraud*. Research has found that the reason for the restatement affects the penalties firms face, with restatements resulting from faulty revenue recognition and error involving fraud to be two which result in particularly adverse outcomes (Hennes et al., 2008; Palmrose et al., 2004; Pozner, 2007; Wilson, 2006). The referent category

for these two dummy variables are all other reasons firms have for restatements (such as errors in accounting for inventory, restatements in response to SEC guidance or accounting errors without involving fraud (Wilson, 2006). This comprehensive list of variables controls for the type of restatement. This information was drawn from annual reports, 10Ks, 10Qs, and proxy statements and other company filings (such as press releases) from the EDGAR database maintained by the SEC.

We also include controls for *firm size*, measured as (1) the logged *total assets* of the company in millions of dollars, lagged by one year, since size has been shown to affect reactions to financial information (Collins, Kothari, & Rayburn, 1987; Freeman, 1987), and (2) *total shares outstanding* in millions as well as (3) *firm performance*, measured as the return on assets, lagged by one year, since financial performance is an indicator of overall firm quality.

### **Analysis**

The unit of analysis in this study is the restatement event. We estimated the stock market penalty for restating using ordinary least squares (OLS) regression. Since half the firms in our sample restated more than once, there exist multiple observations that involve the same firm. As a result, residuals for observations involving the same firm could be correlated and we used accordingly used robust standard errors to correct for non-independence of observations.

We included a year fixed effect to control for systematic or environmental time-varying effects that might also influence the effect of a restatement on a firm's stock value. We had limited observations for 1996 and 1997 so these two years were grouped together into one category. We also included in all model specifications an industry fixed effect based on two-digit SIC (Standard Industrial Classification) codes to control for non time-varying industry attributes that might influence market reactions to a restatement. Since we had few firms in the categories

of Agriculture, Forestry and Fishing (01-09), Construction (15-17), and Non Classifiable Establishments (99), we grouped these firms into one category.

## RESULTS

Table 2 reports descriptive statistics and a correlation matrix for the variables used in our analysis.

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 Insert Table 2 about here  
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Table 3 presents OLS regression estimates for each of the models. The results of Model 1 provide support for Hypothesis 1, that firms are penalized less by stakeholders on a second or later restatement compared to a first restatement.

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 Insert Table 3 about here  
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Model 2 presents results from subsequent analyses where we dummy coded restatement events into four categories (first, second, third and fourth or later restatement) to assess whether the effects of a multiple restatement were constant across the restatement events. That is, we investigated whether this result was robust to an alternative specification of the restatement event. As indicated in Table 3, both second restatements and third restatements incur significantly less severe market penalties than first restatements. Though there is no significant effect of a fourth or later restatement compared to a first restatement, the low number of observations in that category ( $n= 14$ ) might simply mean that we do not have enough power to pick up significant changes for that category. Though the differences between the multiple restatement categories are not significant, it is interesting to note that the coefficients increase in size as the number of

restatements increases, suggesting that the penalties associated with misconduct abate across incidents. These results are graphed in Figure 1.

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 Insert Figure 1 about here  
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The hypothesized interaction between CEO change and a second or later restatement is tested in Model 3. The overall interaction is not significant; however, because we are interested in the specific difference between second or later restating events where there has been a change in CEO, and second or later restating events where there has not been a CEO change, we conducted further analyses to investigate this difference. A simple comparison between these two groups indicated that restating firms that changed their CEO prior to their second restatement are less penalized than second restating firms that did not change their CEO ( $t(409) = 1.81, p = 0.07$ ). This relationship is graphically displayed in Figure 2. There are no significant differences between firms that changed their CEO prior to their first restatement and firms that did not change their CEO prior to their first restatement ( $t(409) = -0.32, p = 0.75$ ).

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 Insert Figure 2 about here  
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Model 4 tests the effect of the type of CEO change (insider or outsider) on market reactions to a second or later restatement. The interaction between an insider CEO appointment and a second or later restatement is significant and positive, suggesting that firms are penalized less on a second or later restatement when the new CEO has been promoted from within. However, the interaction between a second or later restatement and an outsider CEO hire is not significant, suggesting that changing the public face of the firm does nothing to buffer that firm against the penalties of further misconduct. Further probing of these interactions using simple

comparison tests indicate that for a second or later restatement, firms are less penalized when they promote the new CEO from within when compared to firms that do not change their CEO ( $t(407) = -1.63, p = 0.10$ ). However, no difference exists between externally recruiting a new CEO and promoting an insider CEO ( $t(407) = -0.09, p = 0.93$ ), and no significant difference exists between firms who hire externally and those who do not change their CEO prior to a second or later restatement ( $t(407) = 1.16, p = 0.25$ ). As depicted in Figure 3, firms with an internally promoted new CEO are not only penalized less compared to firms that do not change their CEO, but actually incur moderately positive stock returns on the announcement of a second or later restatement.

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 Insert Figure 3 about here  
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## DISCUSSION

This paper reports on the consequences of repeated acts of organizational misconduct, and finds that the penalties faced by firms actually abate across incidents: upon the admission of a repeated acts of misconduct, firms actually confront smaller reputational penalties than when they admit an initial act of misconduct. This finding, which we believe to be the first time that the consequences of repeated acts of organizational misconduct have been studied empirically, suggests that one of the important functions that has been theorized as a reason why stigmatizing organizations is important—“to encourage socially or organizationally valued behaviors” (Paetzold et al., 2008: 190)—may not be effective. Counterintuitively, firms that persist in misconduct are penalized less by the audience which is supposed to determine their value the second (and later) time(s) around than they do the first time. This disadvantage, which we term

the licensing effect of stigma, occurs because the reputational hit on a firm that admits misconduct has the most substantial effect upon the initial admission of guilt. In fact, Rasmusen, a political economist studying stigma and crime, has acknowledged the likelihood that the negative effects of stigma probably abate over time:

The main disadvantage of stigma is perhaps that its effectiveness diminishes for recidivists. Stigma is a cheap and efficient punishment, but only for someone with a reputation to lose. The stigma from a first conviction is greater than from subsequent convictions, and after enough convictions the marginal effect is negligible (1996: 536).

We believe the finding that firms face declining penalties across increasing incidents of misconduct has both important practical implications for how to best discourage recidivism among corporate offenders, and theoretical implications for the behavioural licensing effect of stigma.

In addition to the theoretical implications these findings have for the literature on stigma, we also believe they can make a contribution to the literature on middle status conformity (Phillips & Zuckerman, 2001), as well as the literatures on moral licensing (Cain, Loewenstein, & Moore, 2005; Effron, Cameron, & Monin, 2009; Monin & Miller, 2001) and desensitization (Ashforth & Anand, 2003). In terms of the literature on middle status conformity, these results suggest that low status firms have their own version of “idiosyncrasy credits” (Hollander, 1958, 1964), and are liberated by external audiences from the typical operational requirements expected from legitimately operating firms because they are already viewed as intransigently lower status than firms at their initial admission of restatement.

The literature on moral licensing—a phenomenon which has recently been given increasing attention in the psychological literature, focuses on how individuals license their own bad behavior (Cain et al., 2005; Effron et al., 2009; Monin & Miller, 2001). This work focuses on how an earlier choice or action which establishes one's positive moral credentials (for example, by disagreeing with blatantly sexist statements), licenses one to engage in less morally desirable acts later on (for example, rejecting qualified job applicants who are members of stereotyped groups) (Monin & Miller, 2001). However, the literature on moral licensing is about how good behavior at Time 1 licenses bad behavior at Time 2. Our work is about how bad behavior at Time 1 licenses *similar* behavior at Time 2. Second, the work on moral credentials focuses on how individuals license *themselves* to behave badly. Our work focuses on external audiences, and shows how the diminishing expectations of external audiences license *others* to behave badly.

The literature on desensitization (Ashforth & Anand, 2003) discusses how continued exposure to similar behavior progressively weakens reactions to that behavior (2003: 13). Interestingly, desensitization has been not been studied empirically in contexts where the desensitization is to unethical behavior (Moore, 2009), but instead has focused on how individuals become desensitized to repeated requests to engage in pro-social behaviors such as volunteering or promoting socially worthy causes (e.g., Cialdini, Vincent, Lewis, Catalan, Wheeler, & Darby, 1975; Freedman & Fraser, 1966). In many ways, the argument we are making about stigma is a desensitization argument: when external audiences are continually exposed to similar episodes of misconduct at a firm, their reaction to that misconduct diminishes. However, like the literature on moral licensing, the literature on desensitization focuses on the *individual's responses*, in this case to behavioral repetition. We are clearly suggesting a process

similar to desensitization, in that audiences' reactions to repeated admissions of misconduct become diminished due to an expectation that the negative behavior will continue. However, we focus on external audiences, and how they become desensitized to repeated acts of the same behavior over time.

In other words, the psychological literatures on moral licensing and desensitization focus on the *internal, psychological* mechanisms that individuals feel liberate them to more freely engage in unethical behavior. We are instead focusing on *external, sociological* mechanisms that license deviant behavior. Yet we believe this paper adds depth to our general understanding of both of these phenomena, by demonstrating another route through which agents are liberated to function in morally undesirable ways (moral licensing) and how repeated exposure to negative stimuli weakens reactions to those stimuli (desensitization).

These results also represent a test of earlier theoretical statements about about what happens to firms after misconduct. Pfarrer and his colleagues argue that recidivist firms face greater stakeholder doubt and a harder time re-establishing legitimacy than first-time offenders (Pfarrer et al., 2008a). These results provide evidence that, instead, recidivist firms face fewer reputational penalties than their first-time offender associates. We acknowledge that the absence of similar reputational penalties for recidivists than for one-time offenders is not necessarily evidence that recidivist firms have an easier time re-establishing their legitimacy than first-time offenders. The data we have the strongest evidence for are more specifically about penalties than they are about the re-establishment of legitimacy. It is possible that this subset of recidivist firms simply no longer have legitimacy in any of the common understandings of that word, and that these results support Pfarrer and his colleagues' proposition. However, we believe our findings

represent an important empirical addition to the conversation started by Pfarrer and his colleagues.

We also argue that in the absence of escalating penalties for repeat offences, firms stigmatized by a first instance of misconduct become members of a (stigmatized) subculture where being acknowledged by external audiences as having legitimacy is less important than ensuring that external audiences do not continue to penalize them for persisting in misconduct. Work on illicit subcultures demonstrates that the illegitimacy associated with becoming labelled as criminal is less important *within* those illicit subcultures than those outside the subculture might think, because participation in the subculture provides new opportunities for competing in status hierarchies for prestige and other rewards commensurate with proper legitimacy (Matsueda, Gartner, Piliavin, & Polakowski, 1992).

These results also suggest that a common mechanism firms use to rehabilitate their image, and removal and replacement of the CEO, does act to further buffer the company from harm should it wish to reoffend. The findings about CEO change also have important implications. Though we did not find the main effect for CEO change protecting against the penalties associated with future misconduct, the follow-up tests did suggest that making changes to senior leadership are only effective in protecting against future penalties associated with misconduct when the new CEOs are organizational insiders rather than outsiders. This suggests that external audiences are less interested in substantive change to the way an organization operates than they are in a simple message that a firm's internal skeletons will be addressed by someone who knows.

Though they were not hypothesized, we find it interesting that firms that change their CEO prior to an initial act of misconduct face particularly severe penalties. Our findings suggest that changing the CEO prior to a first admission of restatement results in worse reputational

penalties than not changing the CEO—the opposite finding to this type of rehabilitative act after a second or later admission of misconduct.

It is important to make clear that we are not making the argument that being stigmatized is a positive condition. Clearly, decades of research confirms that stigma has multiple and wide-ranging negative implications (Crocker, Major, & Steele, 1998; Kurzban & Leary, 2001). However, we have shown that there is at least one positive effect of being stigmatized as a fraudulent firm, which is that you are less likely to be penalized for engaging in similar misconduct in the future.

TABLE 1

## Balance in Pretreatment Covariates when Matching on the Propensity Score

	Overall Sample			Matched Sample		
	Mean Treat.	Mean Control	<i>p</i> -value for Diff.	Mean Treat.	Mean Control	<i>p</i> -value for Diff.
S&P 500	0.32	0.19	0.00	0.30	0.26	0.43
NYSE	0.58	0.47	0.03	0.56	0.55	0.91
NASDAQ	0.40	0.48	0.12	0.42	0.43	0.81
Amend 10K	0.66	0.66	0.98	0.65	0.70	0.31
Quarters Restated	4.45	3.50	0.03	4.52	4.04	0.35
SEC Prompt	0.17	0.12	0.15	0.17	0.18	0.76
Auditor Prompt	0.10	0.09	0.67	0.10	0.07	0.40
Revenue Recognition	0.24	0.19	0.19	0.23	0.18	0.31
Fraud	0.11	0.06	0.02	0.11	0.09	0.56
Shares Outstanding (000 000s)	247.93	170.25	0.21	187.04	208.12	0.65
Reduces Net Income	0.66	0.65	0.86	0.65	0.59	0.27
Net Effect	13.39	12.62	0.20	13.28	12.31	0.22
Return on Assets	-2.51	-0.66	0.47	-2.44	-2.73	0.91
Total Assets	7.35	6.87	0.02	7.29	7.44	0.55
Firm Reputation	1.24	1.03	0.34	1.22	1.10	0.68
<i>Year</i>						
1998	0.04	0.02	0.16	0.04	0.02	0.48
1999	0.09	0.08	0.16	0.10	0.10	0.48
2000	0.07	0.06	0.51	0.08	0.09	1.00
2001	0.09	0.10	0.42	0.09	0.14	0.83
2002	0.23	0.07	0.80	0.21	0.17	0.20
2003	0.17	0.10	0.00	0.17	0.21	0.37
2004	0.09	0.20	0.05	0.09	0.09	0.45
2005	0.16	0.33	0.00	0.16	0.13	0.84
2006	0.01	0.03	0.00	0.01	0.01	0.61
<i>Industry</i>						
Mining	0.05	0.05	0.08	0.05	0.08	1.00
Manufacturing	0.33	0.36	0.95	0.34	0.25	0.33
Transport, Communication, Electric, Gas and Sanitary	0.14	0.11	0.52	0.13	0.17	0.09
Wholesale Trade	0.04	0.02	0.33	0.04	0.04	0.32
Retail Trade	0.12	0.17	0.24	0.13	0.13	1.00
Finance, Insurance, Real Estate Services	0.12	0.10	0.51	0.12	0.11	0.85
	0.18	0.17	0.78	0.18	0.21	0.55
<i>N</i>	190	452		141	141	



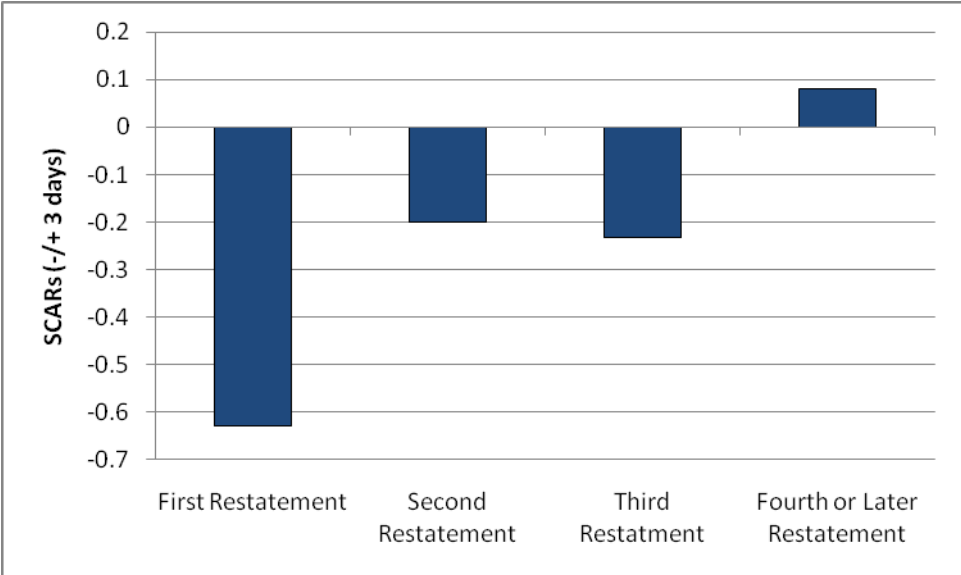
**TABLE 3**  
**OLS Regression Results**

Dependent Variable: SCARs (-/+ 3 Days)	(1)	(2)	(3)	(4)
S&P 500	0.32 (0.35)	0.32 (0.36)	0.35 (0.36)	0.31 (0.36)
NYSE	-0.11 (0.26)	-0.10 (0.26)	-0.11 (0.27)	-0.13 (0.27)
Amend 10K	0.10 (0.17)	0.10 (0.17)	0.13 (0.17)	0.14 (0.17)
Quarters Restated	0.01 (0.02)	0.01 (0.02)	0.00 (0.02)	0.00 (0.02)
SEC Prompt	0.02 (0.20)	0.02 (0.20)	0.00 (0.20)	0.01 (0.20)
Audit Prompt	-0.42 (0.39)	-0.42 (0.38)	-0.42 (0.39)	-0.45 (0.39)
Revenue Recognition	-0.31 (0.22)	-0.31 (0.22)	-0.32 (0.22)	-0.32 (0.22)
Fraud	-0.09 (0.29)	-0.10 (0.29)	-0.05 (0.29)	-0.01 (0.30)
Shares Outstanding (000 000)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Reduces Net Income	-0.34+ (0.18)	-0.33+ (0.19)	-0.37** (0.18)	-0.39** (0.19)
Net Effect	-0.01 (0.02)	-0.01 (0.02)	-0.01 (0.02)	-0.01 (0.02)
Return on Assets	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Total Assets	-0.05 (0.13)	-0.05 (0.13)	-0.05 (0.13)	-0.05 (0.13)
Firm Reputation	0.04 (0.05)	0.04 (0.05)	0.04 (0.05)	0.05 (0.05)
Second or Later Restatement	0.56* (0.18)		0.37+ (0.21)	0.39+ (0.22)
Second Restatement		0.53* (0.19)		
Third Restatement		0.57** (0.25)		
Fourth or Later Restatement		0.75 (0.54)		
CEO Change			-0.08 (0.24)	
CEO Change X Second or Later			0.50 (0.35)	
Outsider CEO Change				0.44 (0.35)
Insider CEO Change				-0.28 (0.28)
Outsider CEO X Second or Later				-0.03 (0.51)
Insider CEO X Second or Later				0.72+ (0.40)
Constant	-0.00 (1.03)	-0.04 (1.05)	-0.02 (1.04)	-0.17 (1.02)
Observations	443	443	443	443
$R^2$	0.13	0.13	0.13	0.14

*Note:* Robust standard errors in parentheses. Year and industry fixed effects are included but not reported due to space constraints. +  $p < 0.10$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$

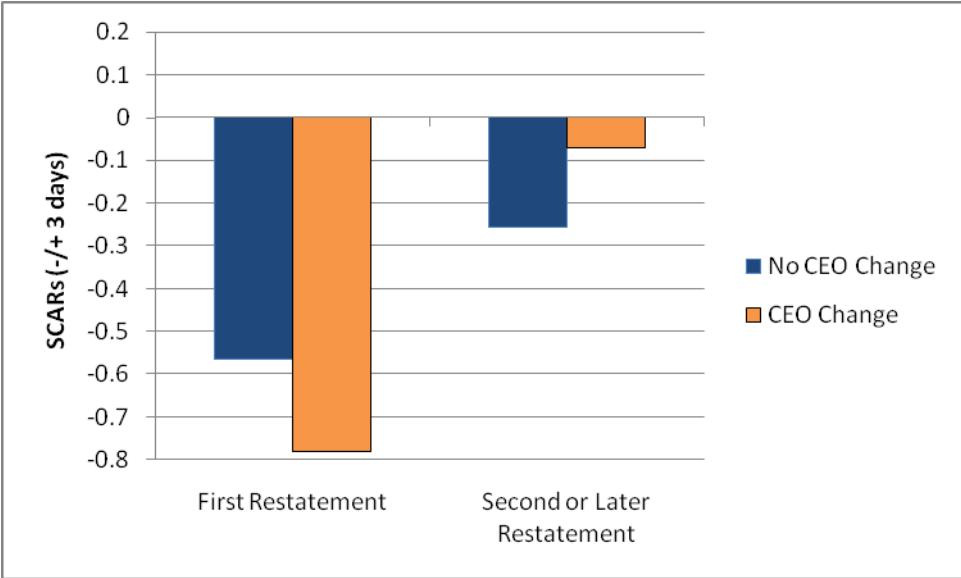
**FIGURE 1**

**Stock Market Reactions to Restatement Events**



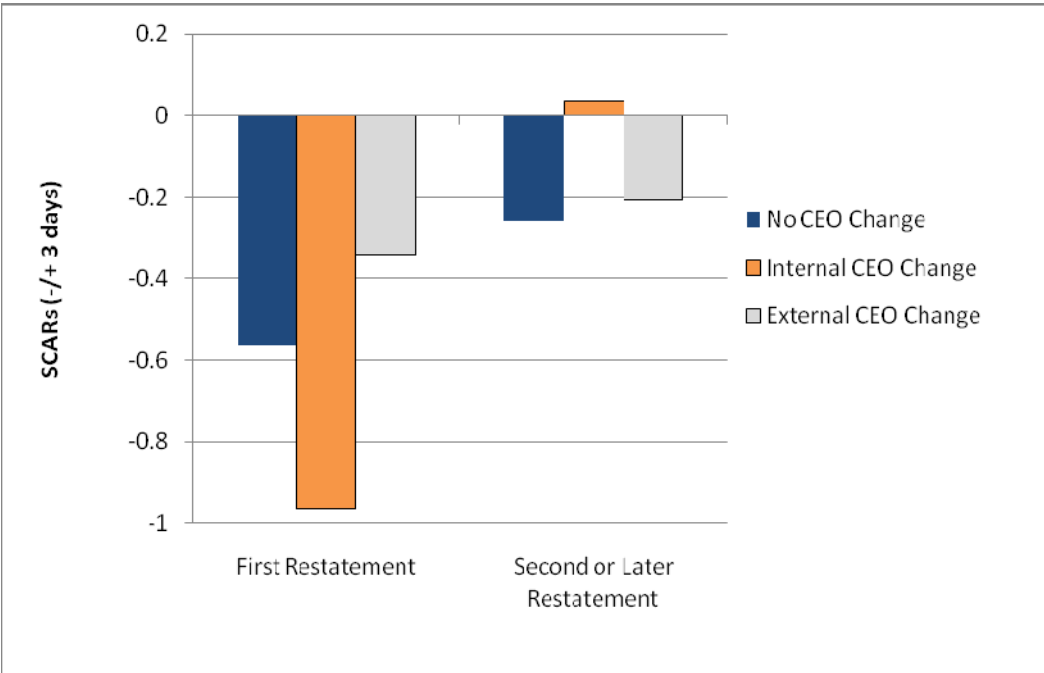
**FIGURE 2**

**Stock Market Reactions to Restatement Events  
With Prior CEO Change and No CEO Change**



**FIGURE 3**

**Stock Market Reactions to Restatement Events With Prior  
No CEO Change, Insider CEO Change and No CEO Change**



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