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WORKING PAPER

Avoiding the consequences of repeated misconduct:

Stigma's licence and stigma's transferability

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ABSTRACT

Contributing to the literature on stigma and re-legitimation, this paper examines two ways organizations may dampen the penalties associated with admissions of misconduct. Using a matched sample of firms reporting earnings restatements (141 single restating firms and 141 multiple restating firms), we find (1) that the stigma associated with a first admission of misconduct licences firms to engage in additional acts of misconduct without incurring the same penalties the second time around, and (2) that firms can effectively regain legitimacy after engaging in misconduct by replacing their CEO prior to publicly announcing the misconduct, but only when the CEO is replaced with an outsider. We argue the first finding is representative of a "licensing effect of stigma," an unexpected positive outcome from a predominantly negative labelling process. We argue that the second finding is representative of how stigma can be successfully transferred from firms to associated organizational elites. We suggest that doing so permits the firm to re-establish its legitimacy even in the face of misconduct admissions because the leadership change re-frames new admissions of misconduct in a more positive light. Both findings help us understand why corporate misconduct may be so persistent and intractable.

Research robustly documents 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 likelihood of facing shareholder class action lawsuits (Lu, 2004). Given these consequences, it would be reasonable to think that after an initial act of misconduct, organizations would repent, change their practices, and operate legitimately again. Although recent theory supports this intuition (Pfarrer, DeCelles, Smith, & Taylor, 2008a), evidence regarding the persistence of corporate misconduct seems to demonstrate that organizations engage in acts of wrongdoing both frequently and repeatedly. Available data suggest that more than half of all firms violate the law, and about half of those firms 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?*

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). It is possible that one of the reasons why firms risk re-

offending is because they believe that actions they have undertaken to restore their legitimacy have worked, although the efficacy of theoretical re-legitimizing actions (e.g., Pfarrer et al., 2008a; Wiesenfeld, Wurthmann, & Hambrick, 2008) has never been tested. One of the most common rehabilitative measures undertaken by firms is to change the CEO (Arthaud-Day, Certo, Dalton, & Dalton, 2006; Hennes, Leone, & Miller, 2008), thereby signalling to stakeholders that the firm is committed to positive change. Whether the signalling event of new leadership actually positively influences how external audiences react to future actions of the firm, however, is not known. Therefore, in this paper we also investigate the effectiveness of the rehabilitative action of leadership change (Arthaud-Day et al., 2006; Hennes et al., 2008) and specifically considers the question: *are rehabilitative actions undertaken by firms which have engaged in misconduct effective in protecting them against misconduct's associated penalties?*

Our answer to the first of these questions draws on the literature on stigma (Crocker & Major, 1989; Goffman, 1963; Jones et al., 1984). We argue that the admission of misconduct is a stigmatizing act, and that stigma deriving from being labelled as a firm which has engaged in misconduct unintentionally licences organizations to engage in similar behaviour in future without confronting the same level of penalties again. Our answer to the second of these questions addresses the effectiveness of leadership change as a rehabilitative action prior to a misconduct announcement. We make the general claim that, while bringing in new leadership from outside the organization will help protect any firm from the penalties associated with the announcement of misconduct, it will be particularly effective when undertaken by firms already stigmatized by an initial act of misconduct, because the act of CEO change effectively transfers the stigma of misconduct from the firm to the outgoing leader.

In addressing our 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 reputation after misconduct, testing whether rehabilitative actions such as changing corporate leadership before the announcement of misconduct effectively protects against the penalties which stem 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), and whether the stigma associated with a firm after engaging in misconduct can be effectively transferred to an outgoing CEO (Wiesenfeld et al., 2008), improving external audiences' reactions to announcements of further acts of misconduct.

THE CONSEQUENCES OF 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 commit a disproportionate percentage of 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 better understand the reasons behind recidivism, we have a better chance of discouraging a significant proportion of organizational misconduct.

Interestingly, the research that has examined the consequences of misconduct has tended to look at 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)— including whether firms persist in misconduct, what the consequences of recidivism are, and whether any re-legitimizing actions they may take are effective. In fact, the few papers that have noted the persistence of misconduct among certain firms has either dropped recidivists from their samples as unnecessary complications (e.g., Arthaud-Day, Certo, Dalton, & Dalton, 2006; Harris, 2007; Harris & Bromiley, 2007) or used recidivism as a control variable (e.g., Pfarrer, Smith, Bartol, Khanin, & Zhang, 2008b). The one exception to this we can find is a study of the longer-term performance implications of illegal behaviour, which did conclude that the performance declines suffered by firms after a first conviction apparently did not deter subsequent illegal behaviour (Baucus & Baucus, 1997). We know almost nothing, however, about the potentially different consequences faced by organizations that become repeat offenders compared to the consequences organizations suffer at their first offence.

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 future criminal activity, and as such recidivists are responsible for the majority of criminal offences (Gendreau, Little, & Goggin, 1996; U.S. Department of Justice, 2000). Consequently, most western criminal justice systems intentionally escalate 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).

The question of whether organizations that persist in misconduct suffer more severe or more 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). However, drawing on the literature on stigma (starting with Goffman, 1963), we counter-intuitively hypothesize corporate corruption may persist precisely because the consequences for repeated admissions misconduct are less severe than those experienced upon a first admission. We develop this hypothesis by arguing that admitting misconduct is a stigmatizing act to firms (Devers, Dewett, Mishina, & Belsito, 2009; Sutton & Callahan, 1987; Wiesenfeld et al., 2008), leading to lower stakeholder performance and behavioural expectations; stakeholders react less intensely to subsequent stigma-creating actions because such behaviour conforms to their already lowered expectations.

Thus, we argue that the stigma associated with firms upon an initial event of misconduct *unintentionally provides those firms license to engage in similar illegitimate activities without facing more substantial penalties*. We claim that the devaluation of the firm associated with the

first stigmatizing event of misconduct (cf. Wiesenfeld et al., 2008) eliminates future expectations that the firm will return to legitimate modes of behaviour, 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 paradox of why so many corporations engage in repeated misconduct: because they do not suffer as severely the second time around.

Organizational Misconduct as a Stigmatizing Act

Stigma, a "deeply discrediting" attribute (Goffman, 1963: 13) of an individual (Crocker et al., 1989; Goffman, 1963; Jones et al., 1984) or an organization (Devers et al., 2009; 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. To date, the majority of the research on stigma has focused on its effects on individuals (e.g., Chen & Kenrick, 2002; Goldstein & Johnson, 1997; Kurzban & Leary, 2001b; Lyons, 2006). A growing body of literature has examined stigma at the organizational level, however, and has shown that multiple types of misconduct and failure result in labelling organizations as stigmatized (Devers et al., 2009; Pozner, 2008; Sutton & Callahan, 1987; Wiesenfeld et al., 2008).

The literature on misconduct as well as the literature on stigma have focused primarily on the negative consequences to organizations (Paetzold et al., 2008), 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). Similarly negative consequences, including withdrawal by key stakeholders, negative reputational effects, and devaluation of the others associated with the stigmatized target, have also been shown to accrue at the organizational level (Hudson & Okhuysen, 2009; Pozner, 2008; Sutton & Callahan, 1987).

There have been few efforts to uncover the potential benefits of stigma, and most of those have been have focused on the benefits to society from stigmatizing deviant groups. 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 psychological literature does show that, under certain circumstances, benefits can accrue to stigmatized individuals as well. Stigma can allow individuals to make external attributions about the source of their social devaluation and thus protect their self-esteem (Crocker & Major, 2003), and provide a basis for groups of stigmatized individuals to band together, resulting in collective empowerment (Goffman, 1963; Ragins, 2008; Shih, 2004). Although this research focuses on how stigma can be used to benefit the stigmatized, it requires the stigmatized actor to actively engage with his negative identity and use it to create positive outcomes for himself. Still, no research has been undertaken to investigate the potentially beneficial effects of stigmatization on organizations.

The Licensing Effects of Stigma

We argue that there is an additional consequence of stigmatization with a very important side benefit, which we term the *behavioural licensing effect of stigma*. Stigma is often associated with lowered expectations for performance or success. The negative consequences of

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 behavioural expectations clearly have many negative repercussions for the stigmatized, it is also possible that these lowered expectations can work to benefit them, inasmuch as it requires less effort to meet these lowered expectations. Goffman (1963: 21) recognized this side-effect of stigma, which he noted was already familiar to most stigmatized individuals, 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."

This form of behavioural licensing has long 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 former mental patients, many claimed that being released from normative behavioural expectations was 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).

The licensing effect of stigma we propose here does not depend on the stigmatized actor actively engaging with the stigmatized identity to create positive outcomes. Instead, this effect is experienced as freedom to continue engaging in the stigmatizing behaviour, with the positive outcome for the stigmatized actor being a reduction in *penalties imposed by the non-stigmatized*. In other words, this licensing effect works because of how stigma affects *external stakeholders'*

expectations of stigmatized firms. As an example from the individual-level literature on stigma, one of the patients with chronic mental illness in Herman and Miall's (1990) 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 causes key audiences of organizations to then expect less legitimate behaviour from them, and thus penalize them less harshly than non-stigmatized firms when they repeat the misconduct.

> *Hypothesis 1. The penalties faced by organizations upon the admission of a repeated act of misconduct will be less severe than those faced upon the admission of an initial act of misconduct.*

RE-LEGITIMATION AFTER MISCONDUCT

The second question we wanted to address in this paper involves the effectiveness of re-legitimizing actions firms take after misconduct. Given the serious consequences that organizations face after misconduct, finding ways to dampen future penalties arising after misconduct could be crucial to firm survival. Yet very little is known about whether rehabilitative actions are actually effective in re-establishing firms' legitimacy or in protecting firms against the consequences of misconduct. 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. Our next argument follows directly from previous theorizing about rehabilitative actions (Pfarrer et al., 2008a; Wiesenfeld et al., 2008), which argues 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 may effectively mitigate the negative effect of admissions 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 has not been empirically tested.

We suggest that a change in firm leadership prior to the admission of misconduct will buffer a firm against the usual penalties because it fundamentally changes the way misconduct is perceived, and that this buffering effect will be stronger for repeat offenders. 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.

One of the main arguments for why CEO change will effectively protect firms against future penalties associated with misconduct has to do with the transferability of stigma. Goffman (1963: 43) writes that stigma is transferred by association—called 'courtesy stigma'—that people who associated with stigmatized actors are "all obliged to share some of the discredit of

the stigmatized person to whom they are related." Stigma transferability has been demonstrated at both the individual level (e.g., Goldstein & Johnson, 1997; Hebl & Mannix, 2003; Mehta & Farina, 1988) as well as at the organizational level, such that firms in the same category as a firm which has suffered a legitimacy loss through misconduct will also suffer (Jonsson, Greve, & Fujiwara-Greve, 2009).

Stigma by association can be particularly dangerous for corporate elites when a firm is the stigmatized actor, because observers look for individuals on whom to blame corporate failures (Meindl, Ehrlich, & Dukerich, 1985). Someone has to end up "paying" for poor performance (Boeker, 1992), and someone—typically the chief executive—often does (Arthaud-Day et al., 2006; Hennes et al., 2008). Corporate elites in control of firms might use CEO change not only as a way of signalling that someone has taken the fall for the misdeeds of the corporation, but also in an attempt to transfer the stain of the stigma from the organization to the departed individual (Wiesenfeld et al., 2008). Thus, we argue that a leadership change prior to the admission of misconduct will dampen the penalties associated with that admission because some of the stigma will have been effectively transferred to the outgoing leader.

We do not argue that all types of leadership changes will have the same effect on the consequences of admitting misconduct, however. 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, bringing in a new outsider CEO should be more positively regarded by external audiences—arguably a more sincere commitment to rehabilitation—than appointing a new insider CEO.

There are both symbolic and substantive reasons why outsider CEO change should be more effective in protecting firms against future penalties associated with misconduct than insider CEO change. The symbolic message conveyed by a new outsider CEO is one 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, and performance which requires recently came financial records certainly qualifies as poor performance (Schwartz & Menon, 1985). Outsider CEO appointments are also generally regarded more favourably 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 2. The penalties faced by organizations who appoint a new outside CEO prior to the admission of misconduct will be less severe than those faced by organizations with no CEO change or insider 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.

More importantly for our line of inquiry, this dampening effect should be stronger for organizations admitting repeated acts of misconduct than for those admitting to misconduct for the first time. Because CEO changes are so commonly seen around admissions of misconduct (Arthaud-Day et al., 2006; Hennes et al., 2008), external stakeholders may read the change as

part of the misconduct admission "script," more a symbolic gesture than a substantive act. When not yoked to an initial admission of misconduct, however, external CEO change may be seen as an even stronger expression of commitment to change, and hence a more substantive act. In fact, admitting to additional misconduct subsequent to the appointment of a new external CEOs may enhance the credibility of the firm, as it enables the new regime to claim to have cleared the books and to make a public commitment to start fresh.

Hypothesis 3. External CEO change will be more effective in dampening the penalties associated with misconduct prior to the admission of a repeated act of misconduct than prior to the admission of an initial act of misconduct.

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 of firm 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 behaviour. 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.

*** Insert Table 1 about here ***

Second, once propensity scores have been calculated for each treatment and control firm, we created a matched sample using nearest neighbor matching with replacement (Dehejia & Wahba, 2002) , 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 restatement. We included first, second, third and forth restatement events for firms in our matched sample; examples of more than four restatements were extremely rare (less than 0.05% of all observations). Since we were not able to confirm with certainly that they were comparable the majority of restatements in our sample, restatement past a firm's fourth were not included in the final sample for these analyses. This final data set is comprised of 437 restatement events, where firms restate anywhere from one (n = 141) to 4 times (n =8).

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.-These daily abnormal returns are aggregated to determine the 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 20 to 200 trading days prior to our event window, and an event window of 2 days prior to 1 day 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 CARs when compared to a first restatement.

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).

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 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

Computing abnormal returns

Average daily abnormal stock returns (ARs) in response to a restatement were computed using event study methodology (Brown & Warner, 1985). The event date, t_0, is the date of the restatement announcement. Abnormal returns for restating firm i on day t in the event period t_{-2} to t_{+1} were calculated as:

$$AR_{it} = R_{it} - (\alpha + \beta R_{mt})$$

where AR_{it} is the daily abnormal return for firm i, R_{it} is the daily return for firm i, and R_{mt} is the daily return on the CRSP equally weighted index. The parameters α and β are OLS values obtained from the market model, estimated using daily returns from t_{-220} days to t_{-20} days prior to the restatement date. ARs are then calculated for each day and aggregated over several event windows. Following Brown and Warner (1985), t-statistics are used to test for statistical significance of the cumulative average ARs.

Cross-sectional analysis of restated firm valuation effects

Cross-sectional analyses controlling for the aforementioned firm, industry and restatement attributes were conducted to test if (a) the effect of a first earnings restatement on a firm's valuation is different from the effect of a second or later restatement; and (b) the appointment of an organizational outsider as CEO influences the effect of a restatement on firm valuation. The unit of analysis was the restatement event. The effects of a restatement and CEO change on firm valuation were estimated using ordinary least squares (OLS) regression. As half the firms in the sample restated more than once, there exist multiple observations that involve the same firm. Consequently, residuals for observations involving the same firm could be correlated and accordingly standard errors were cluster by firm to correct for non-independent observations.

Year fixed effects 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. There were limited observations for 1996 and 1997 so these two years were grouped together into one category. We also included in all specifications industry fixed effects based on two-digit SIC (Standard Industrial Classification) codes to control for non time-varying industry attributes that might influence stock market reactions to a restatement. Since there were few firms in the categories of Agriculture, Forestry and Fishing (01-09), Construction (15-17), and Non Classifiable Establishments (99), these firms were grouped into one category.

RESULTS

Valuation effects on firms that restated earnings more than once

The cumulative average abnormal returns of firms that restated their earnings are listed in Table 2. For a first restatement, the four day CAR beginning on Day t_{-2} is -5.32%, which is negative and statistically significant. Similarly when a firm is restating earnings on a second or

later occasion, the four day CAR beginning on Day t_{-2} is negative and significant, however the response was less negative (-1.95%) compared to the CAR for a first restatement. These results provide preliminary evidence suggesting that restating firms are penalized less for repeated acts of misconduct.

*** Insert Table 2 about here ***

Table 3 shows the distribution of CARs for a firm's first and second or later restatement for a four day event period. While overall there is a negative response to restatement announcements, there exists substantial variation in market reactions suggesting that attributes of the firm or the restatement offset the negative valuation effects of a restatement announcement (e.g., Akhigbe et al., 2005).

*** Insert Table 3 about here ***

Cross-sectional analysis

To test our hypotheses that firms suffer fewer penalties upon a second or later restatement announcement and that are able to offset the penalties associated with restatements through external CEO change, we conducted a cross-sectional analysis of the valuation effects for the sample of single and multiple restating firms. Table 4 reports descriptive statistics and a correlation matrix for the variables used in our analysis and Table 5 presents OLS regression estimates for each of the model specifications.

*** Insert Table 4 about here ***

The first model presented in Table 5 reports the results of regressing the four-day CAR for all firms in the matched sample on firm and restatement related controls. Note that it is not particularly surprising that few of the control parameters are significant given that the control sample was selected explicitly based on a combination of these same parameters using

propensity score matching. However, these controls are included for all model specifications since there is no comparable control sample for the second or later restatement observations.

*** Insert Table 5 about here ***

Hypothesis 1—that restating firms would incur a less negative share price response upon the announcement of a second or later restatement compared to a first restatement—is tested in Model 2 of Table 5. The model indicates a positive and significant effect of a second or later restatement on firm valuation, providing support for Hypothesis 1. Compared to a first restatement, a firm's expected 4-day CAR is 3.6% higher upon the announcement of a second or later restatement, holding all other variables constant.

Hypothesis 2—that firms are able to offset the negative effects of an earnings restatement by appointing a CEO external to the company prior to the restatement announcement—is tested in Model 3 of Table 5. The model indicates a positive and significant effect of an external CEO appointed prior to a restatement announcement on firm valuation. The appointment of an external CEO prior to a restatement increased a firm's expected 4-day CAR by 4.8% compared to a firm that has did not change their CEO. We also considered the effect that an internal CEO appointment might have on a firm's valuation at the time of a restatement, in the case that *any* type of CEO change may act to offset a negative stock market reaction to an earnings restatement. Model 3 indicates that the appointment of an internal CEO had no significant effect on a firm's expected 4-day CAR.

While an external CEO should offset a negative stock market response to a restatement announcement, Hypothesis 3 suggests that this effect will be stronger for a second or later restatement compared to a first restatement announcement. By interacting the external CEO measure with the dummy variable for a second or later restatement, we can examine the impact

of the appointment of a new external CEO prior to a second or later restatement on a firm's valuation. As in Model 3, we also separately estimate the interaction between an internal CEO appointment and a second-or-later restatement. Note that the meaning of the second-or-later restatement, internal CEO change and external CEO change coefficients are different across Models 3 and 4. Whereas the coefficient for a second or later restatement refers to the difference in CARs between a first and a second-or-later restatement in Model 3, in Model 4 it refers to the difference between the two types of restatements only when there is no CEO change (the omitted CEO change category). Similarly, external CEO in Model 4 refers to the difference between a new external CEO and no CEO change only for a first restatement, whereas in Model 3 it refers to the difference between external and no CEO change for both types of restatement (first and second-or-later).

In Model 4, the external CEO X second-or-later restatement interaction represents the effect of an external CEO change (compared to no CEO change) for a second- or-later restatement. However, since we are interested in the specific difference between CEO change prior to a first restatement compared to prior to a second restatement, we conducted further analyses to investigate the difference between these two groups. While a simple comparison between no CEO change and external CEO change for a second-or-later restatement indicates a positive and significant difference ($F(223) = 3.80$, $p = 0.05$), we found no evidence suggesting that external CEO change prior to a second restatement more strongly offsets negative stock market reactions compared to a new external CEO appointment prior to a first restatement ($F(223) = 2.32$, $p = 0.14$). While there was no significant difference for an external CEO appointment between a first and second-or-later restatement, the predicted values for these two categories trended in the hypothesized direction. We suspect our sample lacks adequate power to

adequately tease out this effect since it only yielded 23 observations where the firm appointed an external CEO prior to a second or later restatement.

Figure 1 plots the predicted values of firm CARs for first restatement by CEO change type and second-or-later restatement by CEO change type. As previously demonstrated, firms incur significant and less severe penalties for second-or-later restatement compared to a first restatement. Additionally, for both first and second or later restatements, firms incur significant and less severe negative reactions when they appointed an external CEO prior to the restatement announcement. These predicted values suggest that not only are firms that externally appointed a new CEO before a second-or-later restatement penalized less compared to firms that appoint an external CEO prior to a first, but these firms actually incur moderately positive CARs upon the announcement of a second or later restatement.

*** Insert Figure 1 about here ***

DISCUSSION

This paper investigates 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 act of misconduct, firms actually confront smaller penalties from stakeholders than when they admit misconduct initially. This finding, which we believe to be the first time that the consequences to firms of repeated acts of organizational misconduct have been studied empirically, suggests that one of the important reasons that has been why stigmatizing organizations is important—"to encourage socially or organizationally valued behaviours" (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 advantage, which we term the

licensing effect of stigma, occurs because the stigma associated with a firm upon the first admission of misconduct has the most substantial effect on stakeholder reactions; subsequent admissions of misconduct from stigmatized firms, as more aligned with stakeholder expectations (Rhee & Haunschild, 2006), provoke less substantial reactions.

In fact, Rasmusen (1996: 536), 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.

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, they also 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). Regarding middle status conformity, our 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.

Our work also extends the literature on moral licensing, a phenomenon which has recently been given increasing attention in the psychological literature, which focuses on how

individuals license their own bad behaviour (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), licences 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). The literature on moral licensing studies how good behaviour at Time 1 licenses bad behaviour at Time 2, whereas our work investigates how bad behaviour at Time 1 licenses *similar* behaviour at Time 2. Similarly, whereas 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. Hence this study represents a significant extension of well-studied social psychological phenomena.

In addition, this study represents an extension to the literature on desensitization (Ashforth & Anand, 2003), which investigates how continued exposure to similar behaviour progressively weakens reactions to that behaviour (2003: 13). Interestingly, desensitization has been not been studied empirically in contexts where the desensitization is to unethical behaviour (Moore, 2009), but instead has focused on how individuals become desensitized to repeated requests to engage in pro-social behaviours such as volunteering or promoting socially worthy causes (e.g., Cialdini, Vincent, Lewis, Catalan, Wheeler, & Darby, 1975; Freedman & Fraser, 1966). Like the literature on moral licensing, the literature on desensitization focuses on the *individual's response*s, in this case to behavioural repetition. In many ways, our argument 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. Inasmuch as we

focus on how external audiences become desensitized to repeated acts of the same behaviour over time, our paper represents an important contribution to and extension of this literature.

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 behaviour. In contrast, we focus on *external, sociological* mechanisms that license deviant behaviour. In so doing, our study adds depth to our general understanding of both of these phenomena, by demonstrating another route though 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).

Our results also represent a test of earlier theoretical statements about what happens to firms after misconduct. Pfarrer and his colleagues (2008a) argue that recidivist firms face greater stakeholder doubt and a harder time re-establishing legitimacy than first-time offenders. Our results provide evidence that, instead, recidivist firms face fewer penalties than their first-time offender associates. We acknowledge that the absence of similar penalties for recidivists as for one-time offenders is not necessarily evidence that recidivist firms have an easier time re-establishing their legitimacy than first-time offenders, as our data provide evidence more specifically about penalties than about the re-establishment of legitimacy. It is possible that this subset of recidivist firms simply no longer have legitimacy in the eyes of external audiences, and that these results support Pfarrer and his colleagues' (2008a) proposition. Nevertheless, we believe our findings represent an important empirical addition to this burgeoning conversation.

We also argue that in the absence of escalating penalties for repeat offences, 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 penalization for persistent 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 and provides some evidence that the stigma of misconduct can be transferred to outgoing organizational elites (Wiesenfeld et al., 2008). Although our findings are relatively weak in this regard, our preliminary evidence demonstrates that the appointment of an external CEO prior to the admission of misconduct further licenses organizations to reoffend. These findings have important implications, as they suggest that external audiences are more 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 a trusted insider someone who knows. This is an important refinement of the CEO succession literature. Subsequent research should investigate the limits of such an effect, as well as the efficacy of additional rehabilitative measures, such as board changes, CFO changes, and the adoption of new corporate governance practices.

It is important to make clear that we do not argue that being stigmatized is a positive condition. Decades of research confirms that stigma has multiple and wide-ranging negative implications (Crocker, Major, & Steele, 1998; Kurzban & Leary, 2001a). We have shown, however, that there is at least one positive effect of being stigmatized as a misconduct firm: a decreased likelihood of being 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	*p*-value for Diff.	Mean Treat.	Mean Control	*p*-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
Year						
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
Industry						
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	0.12	0.10	0.51	0.12	0.11	0.85
Services	0.18	0.17	0.78	0.18	0.21	0.55
N	190	452		141	141	

TABLE 2

Average cumulative abnormal returns in response to earnings restatements

	First Restatement (*N*=282)			Second or Later Restatement (*N*=155)			All Restatements (*N*=437)		
Interval	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 3

Distribution of cumulative abnormal returns for restated firms (-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 4: 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	Amend 10K	0.668	0.471	-0.003	-0.038	0.042					
5	Quarters Restated	4.629	4.981	0.001	0.031	-0.029	0.246				
6	SEC Prompt	0.172	0.377	0.060	0.163	0.054	0.089	0.266			
7	Auditor Prompt	0.092	0.289	-0.164	-0.038	0.005	0.072	0.017	-0.144		
8	Revenue Recognition	0.227	0.419	-0.117	-0.093	-0.134	-0.048	0.024	0.015	-0.001	
9	Fraud	0.094	0.292	-0.033	-0.043	-0.004	0.060	0.183	0.041	0.034	0.013
10	Shares Outstanding (000 000s)	220.090	459.828	0.104	0.467	0.207	-0.130	0.031	0.107	-0.022	0.014
11	Reduces Net Income	0.627	0.484	-0.039	-0.016	0.014	0.029	0.109	-0.051	0.114	0.090
12	Net Effect	13.065	6.444	0.001	0.113	0.010	0.009	0.122	0.097	0.067	0.000
13	Return on Assets	-3.976	22.966	0.138	0.194	0.207	0.014	-0.001	0.073	-0.011	-0.052
14	Total Assets (000 000s)	7.373	-2.130	0.145	0.496	0.472	-0.049	-0.025	0.007	0.033	-0.201
15	Firm Reputation	1.157	2.268	0.119	0.567	0.373	-0.056	-0.036	0.075	-0.065	-0.022
16	Second or Later Restatement	0.355	0.479	0.126	0.008	0.029	-0.016	0.095	-0.020	0.030	0.056
17	External New CEO	0.112	0.316	0.015	-0.076	0.003	-0.058	0.152	-0.008	0.063	0.068
18	Internal New CEO	0.220	0.415	-0.038	0.015	-0.017	-0.013	0.165	0.037	-0.073	0.017

		9	10	11	12	13	14	15	16	18
10	Shares Outstanding (000 000s)	-0.003								
11	Reduces Net Income	0.135	-0.024							
12	Net Effect	0.098	0.127	0.621						
13	Return on Assets	-0.016	0.087	-0.038	0.032					
14	Total Assets (000 000s)	-0.025	0.321	0.056	0.149	0.226				
15	Firm Reputation	0.011	0.505	-0.014	0.009	0.137	0.449			
16	Second or Later Restatement	-0.042	0.066	0.018	0.057	-0.082	0.007	-0.002		
17	External New CEO	0.060	-0.037	0.064	-0.016	-0.202	-0.055	-0.069	0.100	
18	Internal New CEO	0.095	0.014	0.021	0.045	0.027	-0.012	0.069	0.023	-0.189

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

TABLE 5

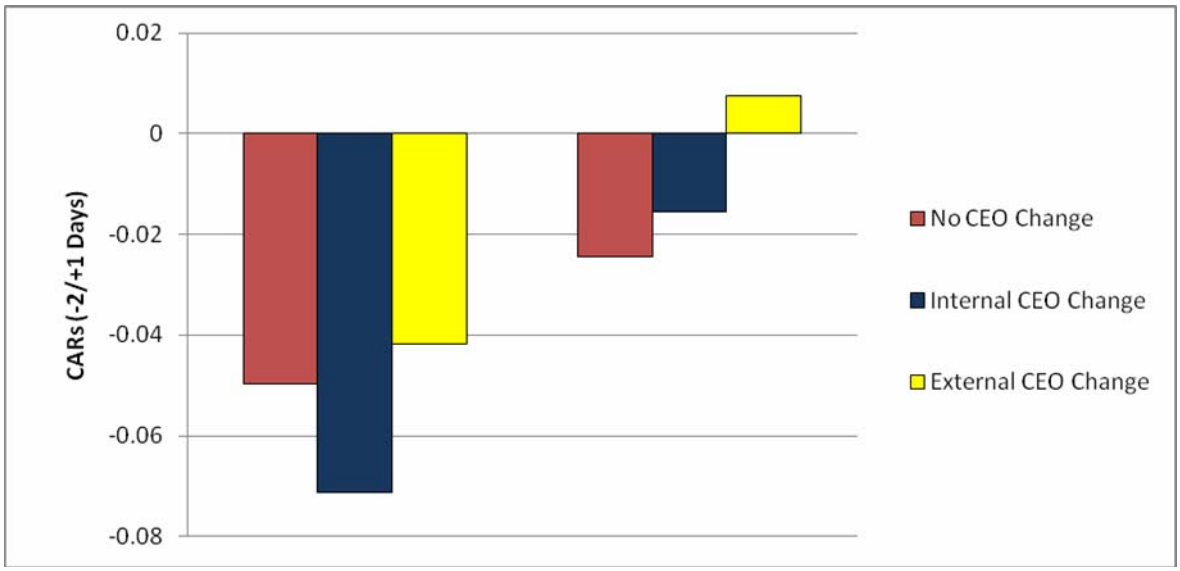
OLS Regression Results

Dependent Variable: CARs (-2/+1 Days)	(1)	(2)	(3)	(4)
S&P 500	0.028	0.027	0.028	0.029
	(0.018)	(0.018)	(0.018)	(0.018)
NYSE	-0.004	-0.005	-0.008	-0.008
	(0.014)	(0.014)	(0.014)	(0.014)
Amend 10k	-0.005	-0.002	0.000	0.000
	(0.014)	(0.013)	(0.013)	(0.014)
Quarters Restated	0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
SEC Prompt	0.009	0.010	0.010	0.009
	(0.016)	(0.016)	(0.016)	(0.017)
Auditor Prompt	-0.054	-0.057+	-0.060+	-0.060+
	(0.034)	(0.034)	(0.035)	(0.035)
Revenue Recognition	-0.022	-0.024	-0.026	-0.026
	(0.019)	(0.019)	(0.019)	(0.020)
Fraud	0.004	0.005	0.004	0.006
	(0.029)	(0.029)	(0.028)	(0.028)
Shares Outstanding (000 000s)	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Reduces Net Income	-0.002	-0.000	-0.002	-0.004
	(0.015)	(0.015)	(0.015)	(0.015)
Net Effect	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Return on Assets	0.000	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Total Assets (000 000s)	0.003	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Firm Reputation	-0.002	-0.002	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Second or Later Restatement		0.036*	0.035*	0.027
		(0.015)	(0.014)	(0.018)
Internal New CEO			-0.008	-0.018
			(0.015)	(0.021)
External New CEO			0.048*	0.041+
			(0.020)	(0.022)
Internal CEO X Second or Later				0.026
				(0.030)
External CEO X Second or Later				0.018
				(0.035)
Constant	-0.043	-0.041	-0.064	-0.062
	(0.051)	(0.050)	(0.047)	(0.047)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	437	437	437	437
Number of Firms	233	233	233	233
R-squared	0.124	0.140	0.154	0.155

Note: Robust firm clustered standard errors in parentheses.
*$p<0.05$, +$p<0.1$

FIGURE 1

Stock market reactions to earnings restatements with no prior CEO change, internal CEO change and external CEO change



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