

Risky Business:
Do Disclosure and Shareholder Approval of
Corporate Political Contributions Affect
Stock Volatility and Value?[†]

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Abstract

We utilize a quasi-natural experiment to examine whether mandating greater disclosure and shareholder approval of political contributions reduces risk and increases firm value. In particular, we examine the Neill Committee Report (NCR), which led to the passage of the United Kingdom's Political Parties, Elections, and Referendums Act 2000 (PPERA). The NCR recommended, and PERPA put into law, stronger disclosure and shareholder approval of political contributions. Using a differences-in-differences methodology, we find that after the NCR's release, politically active firms saw an *increase* in risk, as proxied by stock return volatility, and a *decrease* in firm value, as proxied by Tobin's Q. These results present a challenge to arguments for mandating greater disclosure and shareholder oversight of corporate political activities.

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“These companies deserve credit for embracing transparency and reducing potential risk to shareholder value by disclosing direct and indirect contributions made with corporate funds.” — New York State Comptroller Thomas DiNapoli, announcing an agreement reached with five major corporations to disclose political spending.

1. Introduction

In part due to the US Supreme Court’s decision in *Citizens United v. FEC*, some politicians, interest groups, academics, and activist investors are pressuring publicly traded firms to disclose all of their political spending to shareholders and receive permission from those shareholders before making such expenditures. The pressure for greater transparency and shareholder oversight has taken many forms, including legislation,¹ disclosure ratings,² shareholder resolutions (Baloria et al. 2015), lawsuits (Frankel 2013), and a petition for SEC regulation (Committee on Disclosure of Corporate Political Spending 2011). Some proponents of a stricter regulatory regime view corporate political spending as risky, opaque, and harmful to shareholder value. According to this view, mandatory disclosure and shareholder approval of political spending will reduce risk and increase firm value.³ In this study, we provide evidence to the contrary, and show that such mandatory shareholder disclosure and approval policies could in fact *increase* long-term return volatility and *reduce* firm value.⁴

There are three interrelated arguments for why corporate political spending harms firm value and increases volatility: activism; agency concerns; and moral hazard. Some proponents of a stricter regulatory regime view corporate political spending as risky and harmful to

¹ See, for example, 2013 Congressional bills S.824 and H.R.1734, both known as the Shareholder Protection Act of 2013.

² See, for example, the CPA-Zicklin Index of Corporate Political Accountability and Disclosure, available at www.politicalaccountability.net.

³ There are many arguments for why shareholders ought to be informed about, and approve, corporate political spending, but in this paper we focus on the ones relevant to a firm’s financial success.

⁴ Risk as proxied by return volatility is central to asset pricing, portfolio management, financial contracting, and corporate risk management. High stock-return volatility can increase a firm’s cost of capital and make stock-based compensation schemes unattractive by reducing the informational content of an agent’s actions, as transmitted by stock prices (Diamond and Verrecchia 1991; Bushee and Noe 2000; Rajgopal and Venkatachalam 2011).

shareholders because it draws unwanted attention from activists, creating risks (reputational,⁵ legal liability, and business strategy misalignment)⁶ for the politically active firm. These risks can lead to increased volatility and may damage firm value.

The agency argument (Bebchuk and Jackson 2010; Aggarwal et al. 2012) holds that the interests of managers and directors diverge frequently and significantly from those of shareholders. Managers may use the firm's political spending to pursue a political agenda at odds with the interests of the firm, potentially exposing the firm to activist criticism or leading to a misaligned business strategy.⁷ Political spending may also lead to moral hazard, encouraging managers to take excess risks in the belief that they will be protected by government in the event that the bet goes bad (Kostovetsky 2015).⁸

By constraining managers, disclosure and shareholder approval would seem to be policies that offer many benefits with few costs. Yet, there is reason to think that greater disclosure or shareholder approval of political spending will not be beneficial to shareholders. Disclosure requirements may force the manager to make accommodations that move away from profit maximization (Matusaka and Ozbas 2014) and skew managerial decisions in favor of those which provide positive "hard" (verifiable) information over "soft" information (Edmans et al. 2016).

⁵ One prominent example is Target, which received negative publicity in 2010 when gay rights advocates protested the company because it contributed to a pro-business group called MN Forward. This group in turn had supported the pro-business Minnesota candidate for governor, Tom Emmer, who also opposed gay marriage.

⁶ This classification is due to the Conference Board, which argues that business strategy misalignment may lead to "significant costs or lost revenues" (2012, 5).

⁷ It is possible, as well, that managers may use political spending to affect policies that reduce shareholder's ability to address agency problems with respect to other corporate decisions. In such cases, managers may play it too safe and reduce firm risk below optimal levels (Low 2009).

⁸ The empirical literature on corporate political spending has focused primarily on the returns to spending, not the risks of such spending. This results in this literature are mixed, with some studies finding negative effects (e.g., Aggarwal et al. 2012; Hadani and Schuler 2013), positive effects (e.g., Cooper et al. 2010; Stratmann and Verret 2015), and no effects (e.g., Ansolabehere et al. 2004) of such spending. One weakness of this literature is endogeneity—namely, that the same factors that induce political activities are related to the inherent riskiness of the firm. For example, Cooper et al. (2010) suggest that political contributions could be capturing an omitted risk factor tied to abnormal returns.

Moreover, the release of proprietary information on a firm's political strategy creates a roadmap for its competitors and hostile interest groups to attack the firm. Managers, fearful of reputational harms, may alter political strategies in ways that reduce their effectiveness. For instance, managers engaged in rent seeking may be dissuaded from continuing to do so due to concerns about the appearance of seeking "favors" from government. Such disclosure may also reduce the marginal benefits of rent seeking if politicians become more wary of appearing to grant favors to politically connected firms.⁹

Shareholder approval also could expose a publicly traded firm to greater regulatory risk if it reduces its flexibility and agility in responding to a proposed regulatory change vis-à-vis privately held firms and other interest groups. Ultimately, the net effect of disclosure and approval of political spending on shareholder value and risk is an empirical question.

In this paper, we utilize a quasi-natural experiment to examine whether greater shareholder oversight of political spending does, in fact, increase value, as proxied by Tobin's Q and by abnormal returns, and reduce risk, as proxied by return volatility. Using a surprising report (the Neill Committee Report) that led ultimately to the passage of the United Kingdom's Political Parties, Elections and Referendums Act 2000 (PPERA), which mandated shareholder approval and greater and more centralized disclosure of campaign contributions, we implement a differences-in-differences methodology and find little evidence that the report reduced return volatility or increased value for politically active firms.

In fact, our results suggest the opposite effects. Using a differences-in-differences analysis, we find the release of the Neill Committee Report (NCR) *increased* risk by as much as 35% in some specifications for firms that were contributing to UK political parties prior to its

⁹ This may be a benefit for society as a whole, but it nonetheless may hurt shareholders, especially if not all firms cease rent seeking as a result of disclosure or shareholder approval.

release. Further, we find that while there was no immediate effect of the NCR on returns, the long-run effect was a decrease in firm value by 3-4% for firms that were contributing to UK political parties prior to its release. This market reaction foreshadowed the consequences of the PPERA's passage, as the law seems to have "chilled" the activities of politically active firms: after the PPERA was enacted, nearly all politically active firms in our sample stopped doing so. The regression results are robust to several sensitivity tests, including propensity score matching and the use of placebos.¹⁰

These findings call into question the claim that mandatory disclosure and shareholder approval of corporate political activity reduce risk and increase firm value, and in doing so, they contribute to several literatures. Our evidence is consistent with Watts and Zimmerman's (1978) theory of accounting standards, which focuses in part on political costs and argues that groups have incentives to lobby for policies that transfer wealth away from corporations. To counter the risk of government intrusion, corporations become involved in the political process. Firms may also use political spending to manage political risk and reduce their sensitivity to political uncertainty. Our results suggest that disclosure and shareholder approval of corporate political activities may do more harm to firms than good because these policies short-circuit this defensive posture.¹¹

Moreover, in addition to contributing to the literature on political connections, contributions, and lobbying (Milyo et al. 2000; Fisman 2001; Khwaja and Mian 2005; Faccio

¹⁰ The NCR included other provisions such as the establishment of an Election Commission and a cap on the amount a political party can spend in an election cycle. It is possible that these restrictions may have influenced the stock market or influenced contribution behavior, but we have no theoretical or empirical reason to believe this to be the case.

¹¹ Our results do not provide evidence regarding the NCR's impact on social welfare. It is possible, for instance, that the markets may be better off with disclosure if it prevents firms from seeking advantages in the political sector through rent seeking. A social welfare analysis would be much more complicated, however, as one would have to account for many other effects of the law, including that it affected public but not private firms. Moreover, one would have to consider the effects on the electoral process and for the public of greater restrictions on speech.

2006; Faccio et al. 2006; Fan et al. 2007; Goldman et al. 2009; Yu and Yu 2011; Fisman et al. 2012; Fang and Prabhat 2014; Kostovetsky 2015), we also contribute to the empirical research on disclosure.¹² The accounting literature argues that information asymmetries and conflicts of interest between managers and investors lead to demands for disclosure (Healy and Palepu 2001). Shareholders benefit from disclosure as it reduces information asymmetries, managerial appropriation, and estimation risk; attracts analysts; and helps reduce the cost of capital and return volatility (Amihud and Mendelson 1986; Diamond and Verrecchia 1991; Clarkson et al. 1996; Lang and Lundholm 1996; Botosan 1997; Rajgopal and Venkatachalam 2011).

As recently as 2001 researchers referred to empirical work on the regulation of disclosure as “virtually non-existent” (Healy and Palepu 2001, 412). There is more research today (Leuz and Wysocki 2008),¹³ but still, “there is surprisingly little evidence on the alleged costs and benefits of disclosure regulation, and the economic consequences of mandatory disclosures are theoretically far from clear and heavily debated” (Bushee and Leuz 2005).

Most notably, even though SEC disclosure regulations are viewed as a cornerstone of US capital markets, researchers find mixed evidence on the impact of the Securities Act of 1933 and the Exchange Act of 1934 on risk and return (Stigler 1964; Benston 1973; Jarrell 1981; Chow 1983). The evidence on the net costs and benefits of Regulation FD and the Sarbanes-Oxley Act is mixed, as well (Leuz and Wysocki 2008).

Our findings are in line with papers that find negative effects of disclosure through transmission of proprietary information to competitors (Darrough and Stoughton 1990), increased litigation risk (Rogers and Van Buskirk 2009), and reputational and political costs

¹² There is a large literature on the consequences of mandated changes in accounting standards, but, as Leuz and Wysocki (2008, 38) note, “these studies examine firms that are already subject to the US disclosure regime and generally focus on individual (accounting) rule changes, rather than broader changes in the disclosure regime.”

¹³ For instance, Christensen et al. (2013) find that more stringent accounting standards have had little effect on firm liquidity.

arising from non-shareholders taking actions that adversely affects the firms (Watts and Zimmerman 1978; Li et al. 1997; Cormier and Magnan 1999).

Our paper also contributes to the literature on shareholder approval and corporate governance. The existing literature has typically focused on shareholder activism related to executive compensation (Ertimur et al. 2011; Ng et al. 2011); approval of board members and mergers (Burch et al. 2004, Arena and Ferris 2007); and the value of shareholder voting, proxy contests, and the role played by institutional investors and proxy advisors (Mulherin and Poulsen 1998; Gillan and Starks 2000; Yermack 2010). We highlight the costs of shareholder approval of political activity. Our evidence is consistent with Karpoff and Rice (1989), who suggest that managers facing frequent shareholder votes might spend lot of time campaigning and end up compromising the firm's long-term interests. Similarly, Yermack (2010) argues that voting on social issues can create negative publicity for a firm's business practices, resulting in greater scrutiny by regulators and lawyers.

The paper proceeds as follows. First, we provide background information on the 1998 Neill Committee Report, which led to the Political Parties, Elections, and Referendums Act 2000. Then, we describe the construction of our dataset and our methodology. Next, we present our findings, including several robustness checks, and conclude by discussing the implications of our findings.

2. The Neill Committee Report and the Political Parties, Elections and Referendums Act 2000

Before 2000, campaign finance activities of political parties in the United Kingdom were lightly regulated, and parties were not required to report the sources of their funds. Even though political parties were not required to make their donor lists public, the UK's Companies Act of 1985 required covered companies to disclose political contributions over £200 in the Directors'

Report (the company's annual report). The Act also required corporations to disclose contribution amounts and recipient names (Adams and Hardwick 1998). Fisher (1994) examined contributions to the Conservative Party in the year 1991-1992 and found that of the top 4,000 companies ranked by revenue, 242 made political contributions. The mean was £16,085, and the median was £5,000.

In late 1997, Bernie Ecclestone donated £1 million to the Labour party, allegedly to influence the proposed ban on tobacco advertising in Formula 1 racing. In response, the Labour-controlled government returned the money to Ecclestone and asked the Committee on Standards in Public Life (the Neill Committee) to study party financing activities (Fisher 2001). The committee proposed a set of strong reforms in British party financing activities in October 1998. According to journalistic and scholarly accounts, many aspects of the report were leaked, but when the report was released, observers expressed surprise regarding how far it went, including that it called for shareholder approval of contributions (Eastham 1998; Rawnsley 1998; Fisher 2002). Fisher (2002, 392) wrote, "Given the abject failure of previous attempts to reform party finance during the last twenty-five years, the radicalism and comprehensiveness of the report caused genuine surprise." Despite fears that Labour would be hurt by the new rules, leaders relented and the proposals eventually became part of the Political Parties, Elections and Referendums Act 2000.

This UK legislation strengthened disclosure requirements, and it also required publicly listed companies in the United Kingdom to seek shareholder approval for corporate political spending. On disclosure, the act expanded the definition of political contributions and also provided a single source for the public to obtain contribution-related information for UK-incorporated firms in standard format—information that was already available, but scattered in

the annual reports of the companies. In addition, a publicly listed firm now had to seek shareholder consent before exceeding £5,000 in political spending in a given year.

Because the Neill Committee Report was exogenous to corporate risk taking and the stringency of its recommendations were surprising, we can treat the NCR as a quasi-natural experiment and analyze its effects on the riskiness and value of UK-listed firms. For completeness, we also examine the effects of the PPERA's passage, which was less surprising than the content of the NCR.

3. Data and Methodology

The initial sample of publicly listed firms in the United Kingdom is drawn from Datastream for the period October 1996 to December 2002. Financial data and stock prices are also from Datastream.¹⁴ The Labour Research Department (LRD) generously provided us with data on contributions to UK political parties.¹⁵ We supplement the LRD data by checking annual reports of publicly listed firms in the United Kingdom. To measure corporate governance, we obtain blockholder (a shareholder holding more than 5% of the company's common shares outstanding) data from BoardEx. We supplement the BoardEx data with hand-collected blockholder data from annual reports.¹⁶ In corporate governance models, blockholders typically exert governance through monitoring and direct intervention in a firm's operations (Edmans 2014). Firms without a blockholder are classified as those with weak corporate governance. We

¹⁴ We do not use data beyond 2002 in our regression analyses to avoid the effects of the Directors Remuneration Report (DRR) regulations of 2002. The DRR mandated that boards of directors at publicly listed companies in the UK seek an advisory shareholder vote and produce a comprehensive report on executive compensation. The DRR legislation provided more transparency in pay packages and gave shareholders a voice on pay.

¹⁵ The PPERA expanded the definition of political organizations to include entities concerned with policy review and legal reform, and broadened the definition of a contribution somewhat (e.g., sponsoring annual political party dinners became categorized as political contributions under the PPERA). This change does not affect our analysis, except for the discussion of the PPERA's effects on contribution behavior, and even in that case, the change does not meaningfully affect our findings.

¹⁶ The blockholder data is as of 2000.

utilize this corporate governance measure to estimate the differential impact of the NCR's release and the PPERA's enactment on firm risk and value.

We use three measures of risk. The first is the annualized volatility of daily stock returns (total risk). The second is systematic risk, defined as the annualized volatility of daily-expected returns, estimated from the Fama-French three-factor model, which builds on the traditional CAPM model (Fama and French 1992, 1993). The third is idiosyncratic or firm-specific risk, defined as the annualized volatility of the residuals from the Fama-French three-factor model.

3.1. Estimating Risk

Total risk (variance of daily stock returns) for the stock of firm i in month T is computed using the following equation:

$$Total\ Risk_{iT} = \frac{\sum_{t=1}^n (R_{iTt} - R_{iTag})^2}{n - 1}. \quad (1)$$

R_{iTt} is the daily return of the stock on day t in month T , n is number of return observations for the stock in month T , and R_{iTag} is the average of daily returns of the stock in month T .

To compute systematic and idiosyncratic risk, first we estimate the Fama-French three-factor model (Fama and French 1992, 1993) to predict expected returns:

$$R_{iTt} - rf_{Tt} = \alpha_{iT} + \beta_{iT}(RM_{Tt} - rf_{Tt}) + \gamma_{iT}SMB_{Tt} + \delta_{iT}HML_{Tt} + \varepsilon_{iTt}, \quad (2)$$

where T represents month, t represents day, and i represents the stock of firm i . R_{iTt} is the daily return of the stock, $RM_{Tt} - rf_{Tt}$ is the return of the market portfolio minus the risk-free rate, SMB is the difference between the return of a portfolio of small stocks and that of a portfolio of larger stocks, and HML is the difference between the return of a portfolio of high book-to-market stocks and that of a portfolio of small book-to-market stocks. Following Fu (2009), the idiosyncratic risk of a stock is computed as the variance of the regression residuals from equation

(2). The systematic risk of a stock is computed as the variance of the predicted return, where predicted return is computed from equation (2) as $\alpha_{iT} + \beta_{iT}(RM_{Tt} - rf_{Tt}) + \gamma_{iT}SMB_{Tt} + \delta_{iT}HML_{Tt}$. We annualize the daily variances by multiplying them by 252 (the number of trading days in a year).

Under the traditional CAPM model, all investors diversify idiosyncratic risk by holding the market portfolio. Consequently, only systematic risk is priced in equilibrium. Therefore, any change in idiosyncratic risk does not affect shareholder value. However, in reality, managers are not diversified because their human capital is tied to the firm. An increase in firm-specific uncertainty is costly for managers because managers may lose their jobs. Consequently, poorly diversified managers could cut back on a risky project if the uncertainty is high enough, even if the project uncertainty is firm-specific and the project increases shareholder value. Thus, an exogenous increase in firm-specific risk could result in managers not taking on those value-enhancing risky projects that they would have taken on otherwise. Therefore, an increase in idiosyncratic risk could reduce shareholder value even for well-diversified shareholders. Furthermore, Heaton and Lucas (2000) and Moskowitz and Vissing-Jørgensen (2002) find that investors hold large amounts of idiosyncratic risk in the form of human capital and private equity. Goyal and Santa-Clara (2003) argue that because of this, the relevant measure of risk for many investors is total risk and not systematic risk. Since systematic, idiosyncratic, and total risk are all relevant for investors, we estimate the effects of the NCR's release and PPERA's passage on total, systematic, and idiosyncratic risk.

3.2. Empirical Model for Examining the Effects of the NCR and PPERA on Risk

To test for the effects of the PPERA's enactment and the release of the NCR, we use a differences-in-differences technique (Bertrand et al. 2004) and exploit the fact that any regime

shift from a proposed or an actual change in campaign finance law primarily affects politically active firms. The differences-in-differences approach allows us to isolate the effect of these events (assuming that there were no confounding events around the same date, an issue we return to later). Our regression specification is

$$\begin{aligned} \text{Log}(\text{Risk}_{it}) = & \alpha X_{it} + \beta \text{Post}_{it} \times \text{Politically Active}_{it} + \text{time fixed effects} \\ & + \text{firm fixed effects} + \varepsilon_{it}, \end{aligned} \quad (3)$$

where $\text{Log}(\text{Risk}_{it})$ is the natural logarithm of firm risk. The methodology is similar to the one used by Low (2009) to examine whether managers' risk-taking behavior increases after an exogenous change in takeover protection case law in Delaware.

In the model, $\text{Politically Active}_{it}$ is equal to 1 if the firm contributed to a UK political party prior to the NCR's release (defined as calendar years 1992-1998), and 0 otherwise.

Post_{it} is equal to 1 if the year-month is after the event, and 0 otherwise. X_{it} represents the control variables, which we now describe.

Since managers have the ability to affect risk through leverage, capital expenditures, and research and development (R&D) expenditures (Coles et al. 2006; Low 2009), we control for the contemporaneous values of these firm policy variables. *Leverage* is defined as the book value of debt over the book value of assets. *Capital Expenditure* is defined as capital expenditure scaled by the book value of assets. *R&D Expenditure* is expenditure on research and development scaled by the book value of assets. We also control for *Market-to-book*, which measures growth opportunities and is defined as the market value of equity over the book value of equity; *Size*, defined as the book value of assets in thousands of US dollars; and *ROA* (profitability), defined as EBITDA over the book value of assets. All financial data, including data used to generate ratios or for scaling, is measured in thousands of US dollars.

We also account for firm fixed effects and time fixed effects. Firm fixed effects control for unobserved cross-sectional heterogeneity across firms. Year-month fixed effects control for market-wide fluctuations in volatility. Since the specifications include time and firm fixed effects, the non-interacted $Post_{it}$ or $Politically Active_{it}$ dummy variables drop out of the model. The coefficient of interest in the model is β , which approximates the percentage change in risk for politically active firms caused by proposed disclosure and shareholder approval regulations.¹⁷

In another specification, we control for industry and time fixed effects and include the non-interacted $Politically Active_{it}$ dummy variables in the model.

$$\begin{aligned} \text{Log}(\text{Risk}_{it}) = & \alpha X_{it} + \gamma \text{Politically Active}_{it} + \beta \text{Post}_{it} \times \text{Politically Active}_{it} \\ & + \text{time fixed effects} + \text{industry fixed effects} + \varepsilon_{it}, \end{aligned} \quad (4)$$

By using industry fixed effects, we are able to include the *Politically Active* variable in the analysis, allowing us to estimate the average difference in risk between politically active and inactive firms prior to the NCR's release (captured by γ). The main coefficient of interest in the model is β , which approximates the percentage change in risk for politically active firms caused by proposed disclosure and shareholder approval regulations. We cluster standard errors by firm in both models (3) and (4).

3.3. Empirical Model for Examining the Effects of the NCR and PPERA on Value

To test for the effects of the PPERA and NCR on value, we undertake two types of analyses. First, we use a differences-in-differences technique (Bertrand et al. 2004). Our regression specification is:

¹⁷ Because we are working with the log of risk, the percentage effect is calculated using the exponential of the coefficient and the formula $\exp(\beta)-1$.

$$\text{Log}(\text{Value}_{it}) = \alpha X_{it} + \beta \text{Post}_{it} \times \text{Politically Active}_{it} + \text{time fixed effects} + \text{firm fixed effects} + \varepsilon_{it}, \quad (5)$$

We follow Morck et al. (1988) and use Tobin's Q to proxy for firm value. We define Tobin's Q as the market value of equity plus book value of liabilities, scaled by book value of assets.

Second, we estimate one-day, three-day, and five-day cumulative abnormal return (CAR) for each firm on the NCR release date and regress CAR on the *Politically Active* dummy to infer the effect of the NCR on value. Our regression specification is

$$\text{CAR} = \alpha X_{it} + \beta \text{Politically Active}_{it} + \text{industry fixed effects} + \varepsilon_{it}, \quad (6)$$

To compute abnormal returns, first we use daily stock returns data to estimate the Fama-French three-factor model (Equation 2). The one-year estimation period ends 60 days prior to the event date. The abnormal return for each firm is computed as the difference between the actual return on the event date minus the predicted return for that date from the Fama-French three-factor model. One-day CAR is abnormal return on the event date. Three-day CAR is abnormal returns cumulated over three days, starting one day prior to the event date and ending one day after the event date. Five-day CAR is abnormal returns cumulated over five days, starting two days prior to the event date and ending two days after the event date.

3.4. Descriptive Statistics

Panel A of Table 1 reports descriptive statistics for all publicly traded firms in the United Kingdom with readily available financial data for the year 1997, a year prior to the NCR release year. We separate the firms into politically active and inactive firms, where politically active firms are those that contributed to UK political parties between 1992 and 1997 (prior to the

NCR's release); the remaining firms are defined as inactive. 147 firms in our sample are politically active while 1,507 are inactive. On average, politically active firms gave £46,802 in total contributions from 1992 through 1997 (the length of Great Britain's electoral cycle). The top 3 contributing industries in our sample are Construction, Financial Services, and Transportation. The small proportion of politically active firms in the UK is not an aberration. In the United States, an even smaller proportion of firms were active via political action committees (PACs) during the 1995-96 electoral cycle, which coincides with a presidential election.¹⁸

For the entire sample, active firms tend to be larger, more profitable, and have lower market-to-book ratios than their inactive counterparts; stocks of politically active firms, on average, also have lower firm-specific risk and total risk. Figure 1 plots the annualized idiosyncratic volatility of politically active and inactive firms for the range of our data, estimated monthly. The figure shows parallel trends in idiosyncratic risk for the active and the inactive firms prior to the NCR. After the Neill Committee releases its report in October 1998, the idiosyncratic risk of the active firms increases vis-à-vis that of the inactive firms. However, the risk levels of politically active and inactive firms do not show a parallel trend prior to the PPERA. Specially, the two diverge between August and November 2000, a period of fuel protests in the United Kingdom (Doherty et al. 2003).¹⁹

The differential trends in volatility for active and inactive firms prior to the enactment of PPERA, but not before the NCR, means that the period prior to the PPERA may include other events that affect active and inactive firms differentially (in this case, news related to the 2001

¹⁸ 790 of the 10,482 firms in COMPUSTAT disbursed funds from US PACs in the period 1995 to 1996. These firms did give more on average than firms in Great Britain; the average PAC disbursements of politically active firms in the US for the 1995-1996 election cycle was \$109,058.

¹⁹ Figures A1-A4 formally test for the presence of a trend prior to the event. Figures A1 and A2 show that there is no pre-trend prior to the NCR. In Figures A3 and A4, we drop the months of fuel protests from the sample. After dropping these months, we find that there is no pre-trend prior to the PPERA. Figures A3 and A4 also show that PPERA had no significant impact on risk, as proxied by return volatility.

elections and 2000 fuel protests). Moreover, the NCR's recommendations were surprising to political observers and formed the basis for the PPERA legislation. As a result, we focus on the NCR's release in October 1998 as our event of interest in what follows; we also report results for the PPERA's enactment in November 2000 for completeness, and we also conduct several placebo tests and robustness checks.

3.5. *The PPERA's Effect on Contribution Behavior: Suggestive Evidence*

Table 2 reports that of the 95 firms that contributed prior to the NCR release and which are present in the data for at least part of the period 2001-2006, only 2 continued to contribute after the PPERA while 93 stopped contributing.²⁰ These findings are consistent with those in Torres-Spelliscy (2012, 415-416), who finds that spending by 28 UK firms that had previously given at least £50,000 to the parties dropped precipitously in the wake of PPERA, and that aggregate corporate spending appears to have dropped, as well. In another study, Torres-Spelliscy and Fogel (2011, 558) find that 49 companies that made political expenditures in the 1990s *stopped entirely* after 2000. Further analysis reveals that publicly traded firms may be disadvantaged by this law relative to privately held firms; these authors find that spending by privately-held companies "rose dramatically" in the wake of PPERA, while remaining stable in the aggregate for publicly traded firms (2011, 558-559).

To be sure, this time period also coincided with a transfer in control of government from the Conservatives to Labour, and this shift in power may also have affected the behavior of firms. Nonetheless, this section provides at least suggestive evidence that the PPERA led to

²⁰ These results are striking because we are defining contributions in the post-PPERA period more broadly, following the legislative changes in the definition of a contribution (see footnote 15). If we restrict the post-PPERA contributions to those consistent with the pre-PPERA definition, the results are similar.

changes in the behavior of firms, and we now turn to the consequences of this law for shareholders.

4. Results

4.1. *What is the Effect of the NCR on Risk?*

We start by conducting multivariate tests to examine the impact of the Neill Committee Report on stock volatility, focusing on our sample of all publicly traded firms. We use 24 months of data around the NCR (October 1997 through October 1999) for the differences-in-differences analysis. Table 3 presents the findings. The coefficient of interest is for the *Post x Politically Active* variable, which is equal to 1 if the firm is politically active prior to the NCR and the year-month is after the NCR's release, and 0 otherwise. Columns (1) to (3) present the results for regressions with industry fixed effects. The results show that the NCR *increases* total risk by 30%, systematic risk by 27%, and idiosyncratic risk by 32% for firms that were contributing to UK political parties. The coefficients on the control variables are as expected. The coefficient on the *Politically Active* variables also indicates that politically active firms were less risky than the inactive firms prior to the NCR's release.

Columns (4) to (6) present the results for regressions with firm fixed effects. Consistent with the results in columns (1) to (3), the results in columns (4) to (6) show that the NCR *increases* total risk by 22%, systematic risk by 17%, and idiosyncratic risk by 25% for firms that were contributing to UK political parties. Since we are using firm fixed effects in columns (4) to (6), coefficients on many firm-specific variables that tend to be sticky are statistically insignificant.

One potential concern with our analysis in Table 3 is that firm size and political activity are highly correlated. If the NCR differentially affects larger firm vis-à-vis smaller firms, then

the coefficient on *Politically Active x Post* would suffer from omitted variable bias. To address this concern, we include *Log(Size) x Post* variable to in our empirical specification. Columns (4) to (6) of Table 4 show that after including *Log(Size) x Post* variable in the model, the NCR drives total risk up by 16% and firm-specific risk up by 19%, while it has no significant impact on systematic risk.

Another potential concern is that being politically active is not a random assignment but a choice a firm makes based on its characteristics, meaning that these characteristics could be driving the results. The regression methodology controls for observable firm characteristics. However, to the extent that there is a significant difference between the politically active and inactive firms in terms of financial characteristics, and most of the variation in the data comes from the inactive firms, the results may be biased. To address this concern, we follow Rosenbaum and Rubin's (1983) propensity score matching (PSM) method to match politically active firms and inactive firms based on their propensity to be politically active.

We estimate a firm's propensity to be politically active in 1997 as a function of several firm characteristics using a probit model. These firm characteristics are firm-level controls that we used in Table 3. For every politically active firm in 1997 we select a politically inactive firm from the same industry that is closest to the politically active firm in terms of its propensity to be politically active. To ensure good matches, the maximum allowed distance (caliper) between the propensity scores of the treatment group (politically active firms) and the control group (politically inactive firms) is .05. After matching, the mean and the median difference between propensity scores of the treatment and the control group is .009 and .003, respectively.

Panel B of Table 1 presents descriptive statistics for the matched sample of 117 politically active firms and 117 politically inactive firms (not all politically active firms could be

matched). The table shows that after matching, there is no statistically significant difference between politically active firm and inactive firms in terms of observable firm characteristics (firm-level controls). Consistent with our earlier findings for the entire sample, the table also shows that prior to the NCR's release, politically active firms, on average, are less risky than the inactive firms.

Table 5 presents the results for the propensity-matched sample. Columns (1) to (3) present the results for regressions with industry fixed effects. The results show that the NCR *increases* total risk by 32%, systematic risk by 26%, and idiosyncratic risk by 35% for firms that were contributing to UK political parties. Columns (4) to (6) present the results for regressions with firm fixed effects. Columns (4) to (6) show that the release of the NCR results in a 21% increase in total risk and a 23% increase in firm-specific risk. The impact on systematic risk is positive but statistically insignificant. Further, the results obtained using the matched sample are very similar to the results obtained using the unmatched sample (see Table 3). This suggests that bias arising from the differences in firm characteristics of the politically active and inactive firms is not driving our results.

Next, we analyze whether the results are concentrated around the NCR. If the change in risk is concentrated around the report, then it more likely that identification is coming from the report and not from some other event.²¹ Table 6 presents the results. Panel A reports results for all publicly traded firms in the UK. Panel B reports results for the propensity-matched sample. Our results are largely robust even if we restrict the sample to +/-3 months around the NCR (July 1998 to January 1999). Columns (1) to (3) of Panel A shows that if we choose the sample around

²¹ Regulations related to corporate governance were released in years 1998 and 1999: the Hampel Report was released in January 1998, Combined Code on Corporate Governance was released in June 1998, and the Turnbull Report was released in September 1999. By restricting sample to -/+3 months around the NCR (July 1998 to January 1999), we address concerns that corporate-governance-related events drive our results.

the NCR as July 1998 to January 1999, the NCR drives total risk up by 15% and idiosyncratic risk by 21% for politically active firms, while the impact of the NCR on systematic risk is not statistically different from zero. After controlling for firm fixed effects instead of industry fixed effects, we find that the NCR drives idiosyncratic risk by 17% for politically active firms, while the impact of the NCR on systematic risk and total risk is not statistically different from zero (see Columns (4) to (6) of Table 6). Total risk increases by 13% but the increase is statistically insignificant. The evidence is statistically weaker for the propensity-matched sample.

To further test whether the NCR has a long-term effect on risk, we change the sample to 24 months before the NCR's release and 24 months after the NCR's release (October 1996 to October 2000). Panel A of Table 7 reports results for all publicly traded firms in the UK. Panel B of Table 7 reports results for the propensity-matched sample. Columns (1) to (3) of Panel A show that the NCR produced a 30% increase in total risk, a 15% increase in systematic risk, and a 35% increase in firm-specific risk for the politically active firms. After controlling for firm fixed effects in columns (4) to (6), we find that the NCR increases total risk by 26% and idiosyncratic risk by 30% for politically active firms, while the impact of the NCR on systematic risk (12%) is not statistically different from zero. Panel B shows that our inferences are robust to using the propensity-matched sample.

4.2. *Placebo Test*

We conduct two placebo tests by setting the release date of the NCR to 12 months and 24 months before its actual release date and then re-running the analysis presented in Table 6. We choose the analysis from Table 6 because this enables us to avoid the potentially confounding effects of the May 1997 elections in the placebo tests. (There are too many confounding events in the interim between the NCR's release and the PPERA's passage to conduct post-NCR

placebo tests.) The placebo events have no effect on risk in any of the six specifications.²² This gives us greater confidence that the NCR is a meaningful event for firms and the results are not spurious.

4.3. *Does Weak Governance Matter?*

Recall that one argument for greater disclosure and shareholder approval of corporate political spending is the fear that managers will appropriate shareholder dollars to pursue pet political causes. To examine whether the NCR affected risk differently for firms where managers are more likely to spend corporate money on politics for personal benefits, we create two interaction terms, *WeakGov x Post* and *WeakGov x Post x Politically Active*. The former takes on a value of 1 if the firm does not have a shareholder controlling large block of shares (more than 5% of total shares outstanding) and the year-month is after the event; the latter takes on a 1 if *WeakGov x Post* = 1 and the firm contributed to a UK political party prior to the NCR's release. These variables are then incorporated into the analysis. The results are presented in Table 8, based on the specification from Table 3. The coefficients on the interaction term *WeakGov x Post x Politically Active* are statistically insignificant, suggesting that the NCR did not have a differential impact on the riskiness of active firms that are weakly governed.

4.4. *What Is the Effect of the PPERA's Enactment on Risk?*

Next, we conduct multivariate tests to examine the impact of the PPERA's enactment on shareholder risk. Confounding events (news related to the 2001 national election and the UK fuel protests of 2000) near the enactment of the PPERA, and the fact that its passage was expected,

²² The results are available upon request; we do not report them in a table for space considerations.

make identification more difficult here.²³ However, for completeness, in Table 9 we present selected analyses. We avoid the short-term increase in volatility arising from the UK fuel protests by dropping year-month observations (August 2000 to November 2000) affected by the protests. In Panel A, the sample starts 12 months before the beginning of UK fuel protests (August 1999 to July 2000) and ends 12 months after the PPERA's passage (December 2000 to November 2001). In Panel B, the sample starts 3 months before the beginning of UK fuel protests (May 2000 to July 2000) and ends 3 months after the PPERA's passage (December 2000 to February 2001). In Panel C, the sample starts 24 months before the beginning of UK fuel protests (August 1998 to July 2000) and ends 24 months after the PPERA's passage (December 2000 to November 2002). The table shows that the coefficient on *Post x Politically Active* is not statistically different from zero for any measure of risk, and the coefficients are substantively much smaller, as well.

To deal with confounding events in another way and assess the net effects of the NCR and PPERA (and therefore allow for “learning” by investors in between the period when the NCR was released and the PPERA was passed), we now define the pre-event period as the period prior to the NCR (October 1996 to September 1998) and the post-event period as the period after the enactment of the PPERA (December 2000 to November 2002). We drop all observations from October 1998 to November 2000. As shown in Columns (1) to (3) of Table 10, the net impact of the regime shift is a 21% increase in total risk and a 23% increase in firm-specific risk. The net impact on systematic risk is positive but statistically insignificant. After controlling for firm fixed effects in Columns (4) to (6), we find that the net impact of the regime shift is a 14% increase in firm-specific risk. The net impact of the regime shift on total and

²³ Pastor and Veronesi (2012) argue that political uncertainty is associated with return volatility and systematic risk. Election-related news would affect political uncertainty, and politically active firms are likely to be more sensitive to political uncertainty. Therefore, identification is more difficult for the impact of the PPERA than for the NCR.

systematic risk is positive but statistically insignificant. Taken together with the NCR results, the evidence suggests that greater disclosure to and oversight of corporate political spending by shareholders does not reduce risk, and for at least some types of risk, increases it.

4.5. *What is the Effect of the NCR on Value?*

Finally, we examine the effect of the Neill Committee Report on firm value. Table 11 presents the findings for multiple event windows (1997-1999 and 1996-2000). Panel A reports results for all publicly traded firms in the United Kingdom. Panel B reports results for the propensity-matched sample. The coefficient of interest is for the *Post x Politically Active* variable, which is equal to 1 if the firm is politically active prior to the NCR and the year is after the NCR's release, and 0 otherwise. Panel A shows that the NCR reduces firm value by 3.2%, while Panel B shows that the NCR reduces firm value by 4.5%. Overall, the evidence shows that the NCR hurts firms that were contributing politically prior to the NCR release.

We also conduct an event study on the day the NCR was released. Table 12 presents the results. We do not find a significant association between abnormal returns and politically active dummy. This lack of an association may have occurred for several reasons, including that investors took time to "learn" about the effects of the NCR and that investors wanted to wait to act on the news to see how elected officials reacted to the report.²⁴

5. **Conclusion**

In this paper, we show that greater oversight of corporate political behavior hurts rather than helps shareholders. This result is robust to several sensitivity checks and finding runs

²⁴ An event study for the PPERA is not justifiable because the PPERA's passage was in no way a surprise. Not surprisingly, then, when we perform an event study around PPERA's passage, we find no effect of the PPERA on stock prices.

counter to the conventional wisdom that disclosure and shareholder democracy, including the PPERA, are beneficial.

Torres-Spelliscy and Fogel (2011), as well as Bebchuk and Jackson (2010), view the effects of the PPERA favorably, and Bebchuk and Jackson (2010) even argue that the law does not go far enough in giving shareholders a say on spending. Why is the UK experience so at odds with these views? We argue that the typical justification for shareholder approval and disclosure is incomplete, as it only focuses on the risk that managers may misuse corporate funds (either to pursue their own political ends or in excessively risky ways). It does not, however, acknowledge that activists may have ideological motivations that are not in the best interests of shareholders, and that the shareholder approval process provides them with an easy avenue to attack the corporation.²⁵ In other words, the preferences of some shareholders may not be aligned with the goal of maximizing shareholder value. Even if these attacks never occur, to the extent that managers change their behavior in light of these fears, the firm may be less adept at responding to political threats, and as a result, volatility may increase.

Our paper is the first to quantify the costs to firms as a result of the PPERA, in the form of increased stock volatility and lower Tobin's Q, and offers good reason to be much more cautious about the advisability of implementing similar rules in the United States. In fact, the effects of a similar law in the United States could be even worse for firms than what UK firms have experienced. Verret (2011) argues that politically motivated shareholders, like unions and pension funds controlled by politicians, are more prevalent in the United States, and are likely to use shareholder approval as a political weapon. Recent empirical work by Matsusaka and Ozbas (2015) lends credence to this argument; these authors show that labor unions use their

²⁵ Moreover, mandating a single policy applying to all firms may actually short-circuit the existing equilibrium under which shareholders can pressure the corporation to make changes in disclosure or approval processes, and corporations and/or shareholders can determine whether to accept these requests (Baloria et al. 2015).

shareholder proposal rights as bargaining chips during contract talks. The political activism of shareholders in the United States is likely to exacerbate, not mitigate, the problems facing managers, suggesting that the findings in our paper may understate the effect of such changes in the United States.

This paper points to the need for more attention to identification (in the statistical sense) in assessing how legal restrictions on corporate political strategy affect managerial decision making as well as shareholder value. Studies that have attempted to measure the returns to corporate political spending have faced the challenge that variation in spending is often not exogenous. Using changes in laws, especially unanticipated changes, allows us to indirectly assess how corporate political spending affects shareholder value.

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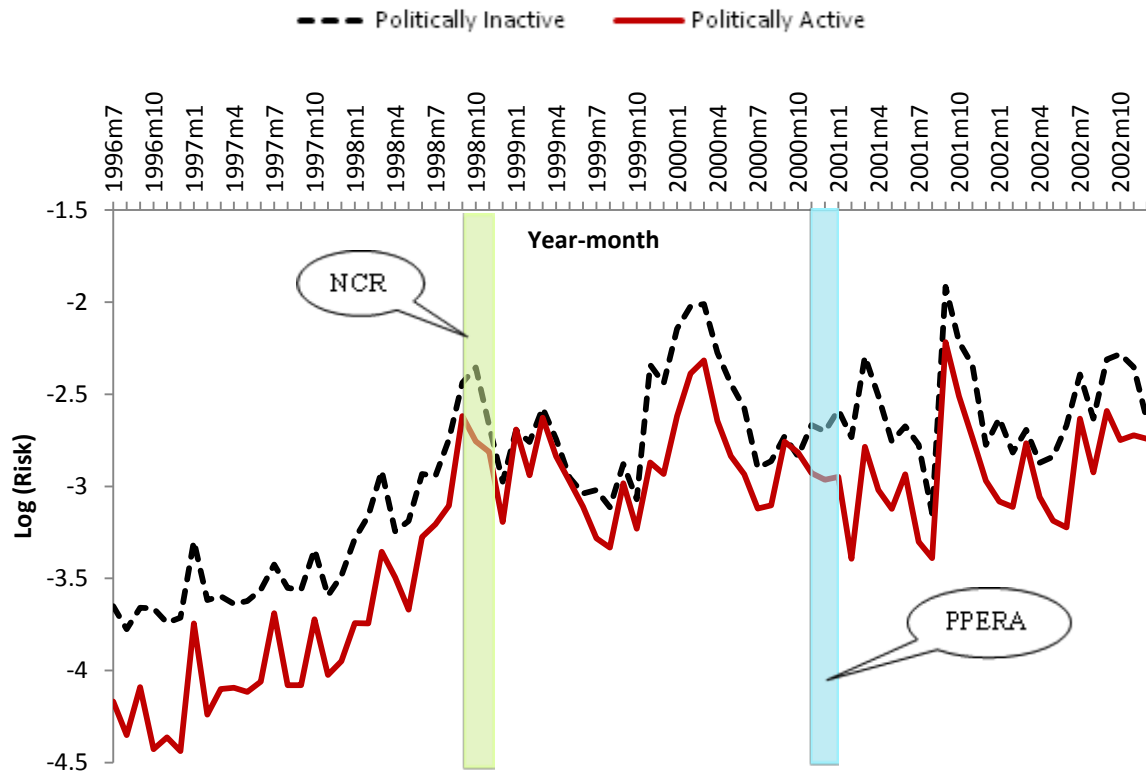
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Figure 1
Log Monthly Idiosyncratic Volatility Around the Neill Committee Report (NCR) and the PPERA



The figure plots logged values of monthly idiosyncratic volatility around the Neill Committee Report (NCR) of October 1998 and the Political Parties, Elections and Referendums Act (PPERA) enacted in November 2000. The Y-axis represents logged values of monthly idiosyncratic volatility. Ymn on the X-axis denotes month n of year Y.

Table 1
Descriptive Statistics (1997)

Panel A: All Firms	A	B	B-A
	<i>Politically Inactive Firms (N=1507)</i>	<i>Politically Active Firms (N=147)</i>	<i>Diff</i>
Risk			
Log (Total Risk)	-2.54	-3.07	-0.53***
Log (Systematic Risk)	-5.44	-5.38	0.055
Log (Idiosyncratic Risk)	-2.64	-3.23	-0.59***
Controls			
Log (Size)	10.89	12.75	1.86***
Market-to-book	0.46	0.25	-0.20***
ROA	0.092	0.13	0.039**
Firm policy			
Leverage	0.18	0.19	0.018
R&D Expenditure	0.0089	0.0071	-0.0019
Capital Expenditure	0.069	0.057	-0.013
Panel B: Propensity-Matched Sample			
	A	B	B-A
	<i>Politically Inactive Firms (N=117)</i>	<i>Politically Active Firms (N=117)</i>	<i>Diff</i>
Risk			
Log (Total Risk)	-2.84	-3.12	-0.28**
Log (Systematic Risk)	-5.43	-5.54	-0.11
Log (Idiosyncratic Risk)	-2.96	-3.27	-0.32***
Controls			
Log (Size)	12.03	12.22	0.19
Market-to-book	0.23	0.25	0.021
ROA	0.13	0.14	0.0075
Firm policy			
Leverage	0.20	0.19	-0.003
R&D Expenditure	0.0060	0.0064	0.00039
Capital Expenditure	0.058	0.060	0.0020

Note: Panel A reports the mean for all publicly traded firms in the United Kingdom in the year 1997. Panel B reports descriptive statistics for the propensity-matched sample. All financial data, including data used to generate ratios or for scaling, is measured in thousands of US dollars. All variables are measured in the year 1997. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 2
Contributions to Political Parties by Publicly Traded Firms: The NCR and the PPERA

	Post-PPERA Contributor	Post-PPERA Non-contributor
Pre-NCR Contributor	2	93
Pre-NCR Non-contributor	10	982

Note: The sample consists of firms that are present in both 1997 and for at least part of the period 2001 to 2006. Because some firms are no longer in existence and/or in the Datastream database in the post-PPERA time period, the number of firms is smaller than those in subsequent tables.

Table 3
Full Sample: Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1997 to October 1999

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.26*** (0.064)	0.24*** (0.069)	0.28*** (0.064)	0.20*** (0.062)	0.16** (0.069)	0.22*** (0.063)
Politically Active	-0.16** (0.078)	-0.10 (0.08)	-0.19** (0.078)			
Log (Size)	-0.025* (0.014)	0.063*** (0.016)	-0.050*** (0.014)	-0.051 (0.055)	-0.021 (0.062)	-0.059 (0.056)
Market-to-book	0.073*** (0.026)	0.13*** (0.028)	0.055** (0.025)	0.015 (0.029)	0.035 (0.032)	0.010 (0.029)
ROA	-1.53*** (0.13)	-1.42*** (0.13)	-1.58*** (0.13)	-0.37** (0.15)	-0.23 (0.17)	-0.41*** (0.15)
Leverage	0.25* (0.15)	0.089 (0.16)	0.30** (0.15)	0.15 (0.21)	0.14 (0.22)	0.18 (0.21)
R&D Expenditure	0.60 (0.47)	0.99* (0.52)	0.48 (0.47)	-0.91** (0.41)	-0.68 (0.50)	-1.06** (0.42)
Capital Expenditure	-0.21 (0.36)	0.017 (0.38)	-0.24 (0.35)	-0.030 (0.31)	0.10 (0.36)	-0.052 (0.32)
Constant	-3.18*** (0.16)	-6.56*** (0.17)	-3.07*** (0.16)	-2.35*** (0.64)	-4.74*** (0.72)	-2.46*** (0.64)
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	30,952	30,952	30,952	30,952	30,952	30,952
Adj. R ²	0.16	0.18	0.16	0.08	0.13	0.07

Note: The sample starts 12 months before the NCR's release and ends 12 months after the NCR's release. *Post* is equal to 1 if the year-month is November 1998 to October 1999, and 0 otherwise. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 4
Full Sample with Robustness to Firm Size: Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1997 to October 1999

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.23*** (0.068)	0.18** (0.075)	0.25*** (0.069)	0.15** (0.064)	0.096 (0.070)	0.17*** (0.065)
Politically Active	-0.12 (0.082)	-0.053 (0.090)	-0.14* (0.082)			
Post x Log Firm Size	0.023** (0.010)	0.034*** (0.012)	0.021** (0.010)	0.032*** (0.0092)	0.040*** (0.011)	0.031*** (0.0094)
Constant	-3.25*** (0.17)	-6.50*** (0.19)	-3.15*** (0.17)	-3.29*** (0.13)	-5.09*** (0.73)	-2.73*** (0.65)
Firm-level controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	30,952	30,952	30,952	30,952	30,952	30,952
Adj. R ²	0.17	0.18	0.17	0.08	0.13	0.07

Note: This regression is identical to the analysis in Table 3, except for the addition of an interaction term for the post-NCR period and firm size. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 5
Propensity-Matched Sample: Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1997 to October 1999

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.28*** (0.100)	0.23** (0.11)	0.30*** (0.10)	0.19* (0.10)	0.14 (0.11)	0.21** (0.10)
Politically Active	-0.21** (0.088)	-0.14 (0.096)	-0.24*** (0.089)			
Constant	-4.36*** (0.53)	-8.28*** (0.55)	-4.01*** (0.51)	-1.31 (1.91)	-3.45 (2.34)	-1.93 (1.96)
Firm-level controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	4,569	4,569	4,569	4,569	4,569	4,569
Adj. R ²	0.24	0.27	0.22	0.13	0.17	0.11

Note: This regression is identical to the analysis in Table 3, except that the sample is now restricted to the propensity-matched firms. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 6
Narrowing the Event Window: Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, July 1998 to January 1999

Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.14* (0.083)	0.070 (0.098)	0.19** (0.084)	0.12 (0.088)	0.050 (0.11)	0.16* (0.089)
Politically Active	-0.13 (0.087)	-0.089 (0.10)	-0.17** (0.086)			
Constant	-1.82*** (0.19)	-4.38*** (0.21)	-1.82*** (0.19)	-3.62** (1.68)	-4.53** (1.81)	-4.21** (1.76)
Firm-level controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	8,694	8,694	8,694	8,694	8,694	8,694
Adj. R ²	0.17	0.18	0.16	0.06	0.12	0.04

Panel B: Propensity-Matched Sample	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.21 (0.14)	0.10 (0.17)	0.26* (0.14)	0.16 (0.15)	0.083 (0.18)	0.19 (0.15)
Politically Active	-0.18* (0.098)	-0.12 (0.11)	-0.22** (0.10)			
Constant	-4.52*** (0.54)	-8.42*** (0.62)	-4.12*** (0.54)	-5.46 (8.53)	-13.0 (12.9)	-4.36 (7.87)
Firm-level controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	1,314	1,314	1,314	1,314	1,314	1,314
Adj. R ²	0.24	0.28	0.21	0.08	0.14	0.06

Note: The samples start 3 months before the NCR's release and ends 3 months after the NCR's release. *Post* is equal to 1 if the year-month is November 1998 to January 1999, and 0 otherwise. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 7
Widening the Event Window: Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1996 to October 2000

Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.26*** (0.069)	0.14* (0.077)	0.30*** (0.069)	0.23*** (0.066)	0.11 (0.075)	0.26*** (0.066)
Politically Active	-0.19** (0.076)	-0.12 (0.09)	-0.22*** (0.075)			
Constant	-1.84*** (0.18)	-4.35*** (0.19)	-1.90*** (0.17)	-3.36*** (0.41)	-7.00*** (0.47)	-3.10*** (0.41)
Firm-level controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	59,667	59,667	59,667	59,667	59,667	59,667
Adj. R ²	0.21	0.20	0.20	0.13	0.14	0.11

Panel B: Propensity-Matched Sample	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.33*** (0.11)	0.30** (0.12)	0.34*** (0.10)	0.19* (0.10)	0.14 (0.12)	0.20** (0.10)
Politically Active	-0.34*** (0.083)	-0.32*** (0.093)	-0.36*** (0.082)			
Constant	-5.11*** (0.56)	-8.51*** (0.67)	-4.93*** (0.53)	-0.45 (1.44)	-4.23*** (1.57)	-0.27 (1.45)
Firm-level controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	8,704	8,704	8,704	8,704	8,704	8,704
Adj. R ²	0.26	0.27	0.24	0.17	0.17	0.16

Note: The samples start 24 months before the NCR's release and ends 24 months after the NCR's release. *Post* is equal to 1 if the year-month is November 1998 to October 2000, and 0 otherwise. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 8
Robustness to Corporate Governance: Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1997 to October 1999

Panel A: Full Sample	(1)	(2)	(3)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.26*** (0.089)	0.26** (0.10)	0.26*** (0.090)
Weak Governance x Post	0.055 (0.043)	0.049 (0.050)	0.057 (0.044)
Weak Gov. x Post x Politically Active	-0.11 (0.12)	-0.19 (0.14)	-0.079 (0.12)
Constant	-2.39*** (0.64)	-4.77*** (0.71)	-2.51*** (0.64)
Firm-level controls	Y	Y	Y
Firm fixed effects	Y	Y	Y
Year-month fixed effects	Y	Y	Y
N	30,952	30,952	30,952
Adj. R ²	0.08	0.13	0.07

Panel B: Propensity-Matched Sample	(1)	(2)	(3)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.25* (0.14)	0.23 (0.15)	0.26* (0.14)
Weak Governance x Post	0.097 (0.15)	-0.0074 (0.17)	0.13 (0.15)
Weak Gov. x Post x Politically Active	-0.13 (0.21)	-0.15 (0.23)	-0.12 (0.21)
Constant	-1.31 (1.92)	-3.19 (2.30)	-2.02 (2.00)
Firm-level controls	Y	Y	Y
Firm fixed effects	Y	Y	Y
Year-month fixed effects	Y	Y	Y
N	4,569	4,569	4,569
Adj. R ²	0.13	0.17	0.11

Note: Panel A(B) is identical to the analysis in Table 3(5) except for the addition of variables related to firm governance. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 9

Full Sample: Differences-in-Differences Regressions Estimating the Effect of the PPERA's Enactment on Monthly Return Volatility for UK Firms, Various Time Periods

Panel A Pre-Period: Aug. 1999 – July 2000; Post-Period: Dec. 2000 – Nov. 2001			
	(1)	(2)	(3)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	-0.037 (0.076)	0.017 (0.083)	-0.061 (0.079)
Constant	-2.98*** (0.67)	-6.01*** (0.81)	-2.97*** (0.66)
N	25,118	25,118	25,118
Adj. R ²	0.096	0.14	0.071

Panel B Pre-Period: May 2000 – July 2000; Post-Period: Dec. 2000 – Feb. 2001			
	(1)	(2)	(3)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	-0.063 (0.13)	-0.054 (0.17)	-0.053 (0.13)
Constant	-4.71*** (1.14)	-7.33*** (1.63)	-4.95*** (1.21)
N	6,095	6,095	6,095
Adj. R ²	0.03	0.04	0.02

Panel C Pre-Period: Aug. 1998 – July 2000; Post-Period: Dec. 2000 – Nov. 2002			
	(1)	(2)	(3)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	-0.097 (0.069)	-0.076 (0.075)	-0.10 (0.071)
Constant	-2.64*** (0.46)	-5.44*** (0.51)	-2.68*** (0.46)
N	50,130	50,130	50,130
Adj. R ²	0.07	0.11	0.06

Note: In all panels, the months spanning the UK fuel protests (August 2000 to November 2000) have been dropped to avoid this confounding event which caused an increase short-term volatility. *Post x Politically Active* is equal to 1 if the firm contributed to a UK political party prior to the NCR's release and the year-month is after November 2000. Standard errors clustered by firm are in parentheses. All panels include the firm-level controls from Table 3, firm fixed effects, and year-month fixed effects. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 10
Full Sample: Differences-in-Differences Regressions Estimating the Net Effect of the Neill Committee Report and the PPERA's Enactment on Monthly Return Volatility for UK Firms, 24 months Pre-NCR and 24 months Post-PPERA

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>	<i>Total Risk</i>	<i>Systematic Risk</i>	<i>Idiosyncratic Risk</i>
Post x Politically Active	0.19** (0.080)	0.13 (0.095)	0.21*** (0.079)	0.11 (0.081)	0.022 (0.095)	0.13* (0.081)
Politically Active	-0.22*** (0.075)	-0.17** (0.083)	-0.23*** (0.074)			
Constant	1.16*** (0.14)	-1.53*** (0.16)	1.21*** (0.14)	-2.18*** (0.43)	-5.25*** (0.48)	-2.19*** (0.43)
Firm-level controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	Y	Y	Y
Industry fixed effects	Y	Y	Y	N	N	N
Year-month fixed effects	Y	Y	Y	Y	Y	Y
N	54,126	54,126	54,126	54,126	54,126	54,126
Adj. R ²	0.23	0.22	0.22	0.14	0.16	0.12

Note: *Post* is equal to 1 if the year-month is December 2000 through November 2002, and 0 otherwise. The pre-event period is October 1996 to September 1998 and the post-event period is December 2000 to November 2002. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 11

Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Firm Value (Tobin's Q) for UK Firms, Various Time Periods

Panel A: Full Sample	(1)	(2)
	1997-1999	1996-2000
Post x Politically Active	-0.036** (0.017)	-0.032** (0.017)
Constant	0.39 (0.47)	0.82*** (0.22)
Firm-level controls	Y	Y
Firm fixed effects	Y	Y
Year fixed effects	Y	Y
N	4,415	7,101
Adj. R ²	0.14	0.22

Panel B: Propensity-Matched Sample	(1)	(2)
	1997-1999	1996-2000
Post x Politically Active	-0.021 (0.020)	-0.046** (0.023)
Constant	0.31 (0.68)	0.54 (0.36)
Firm-level controls	Y	Y
Firm fixed effects	Y	Y
Year fixed effects	Y	Y
N	619	986
Adj. R ²	0.25	0.39

Note: *Post* is equal to 1 if the year is 1998 or afterwards. Standard errors clustered by firm are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Table 12**Regressions Estimating the Effect of the Neill Committee Report Announcement on Announcement Returns, Various Time Windows**

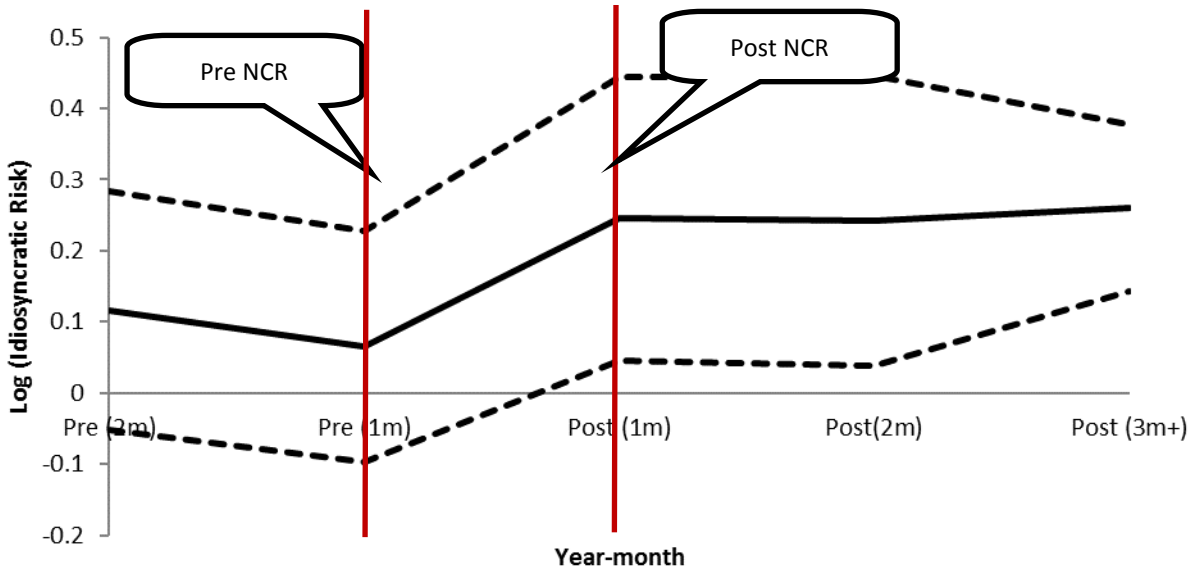
Panel A: Full Sample	(1)	(2)	(3)
	AR[0]	AR[-1,+1]	AR[-2,+2]
Politically Active	-0.00037 (0.0017)	0.0027 (0.0058)	0.0065 (0.0068)
Constant	-0.018* (0.010)	-0.031 (0.022)	-0.069** (0.027)
Firm-level controls	Y	Y	Y
Industry fixed effects	Y	Y	Y
N	1,375	1,375	1,375
Adj. R ²	0.07	0.06	0.06

Panel B: Propensity-Matched Sample	(1)	(2)	(3)
	AR[0]	AR[-1,+1]	AR[-2,+2]
Politically Active	-0.0019 (0.0028)	0.00040 (0.0080)	0.0069 (0.010)
Constant	-0.015 (0.014)	-0.048 (0.030)	-0.074* (0.041)
Firm-level controls	Y	Y	Y
Industry fixed effects	Y	Y	Y
N	216	216	216
Adj. R ²	0.30	0.30	0.31

Note: In Column (1), the dependent variable is one-day announcement returns. In Column (2), the dependent variable is three-day announcement returns. In Column (3), the dependent variable is five-day announcement returns. Standard errors clustered by industry are in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

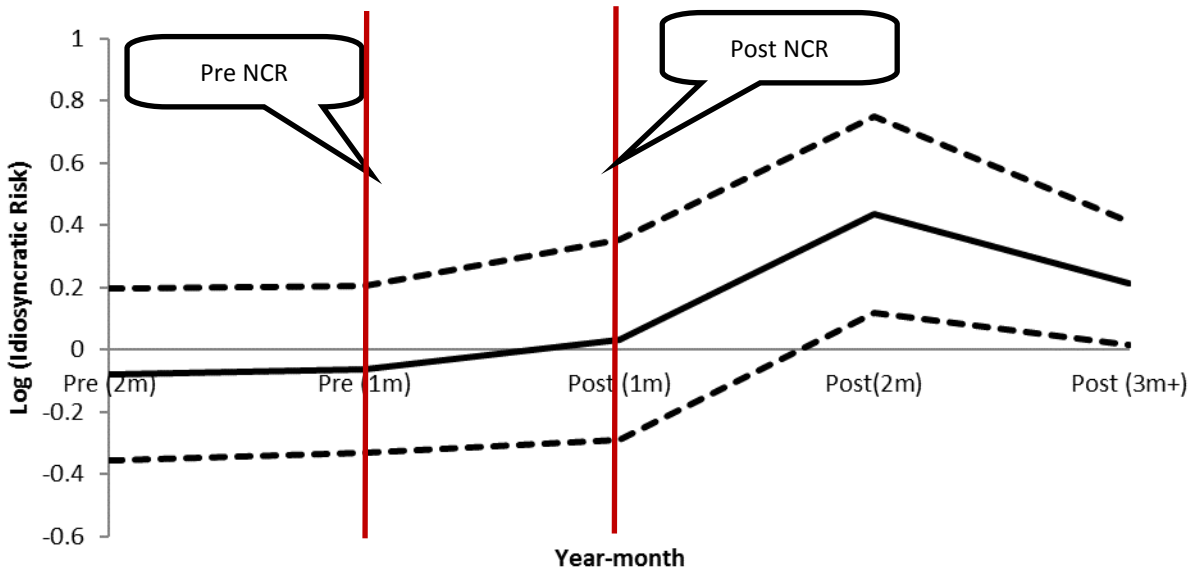
Appendix

Figure A1. Coefficient Dynamics for the Full Sample: Log Monthly Idiosyncratic Volatility Around the Neill Committee Report (NCR) , October 1997 to October 1999



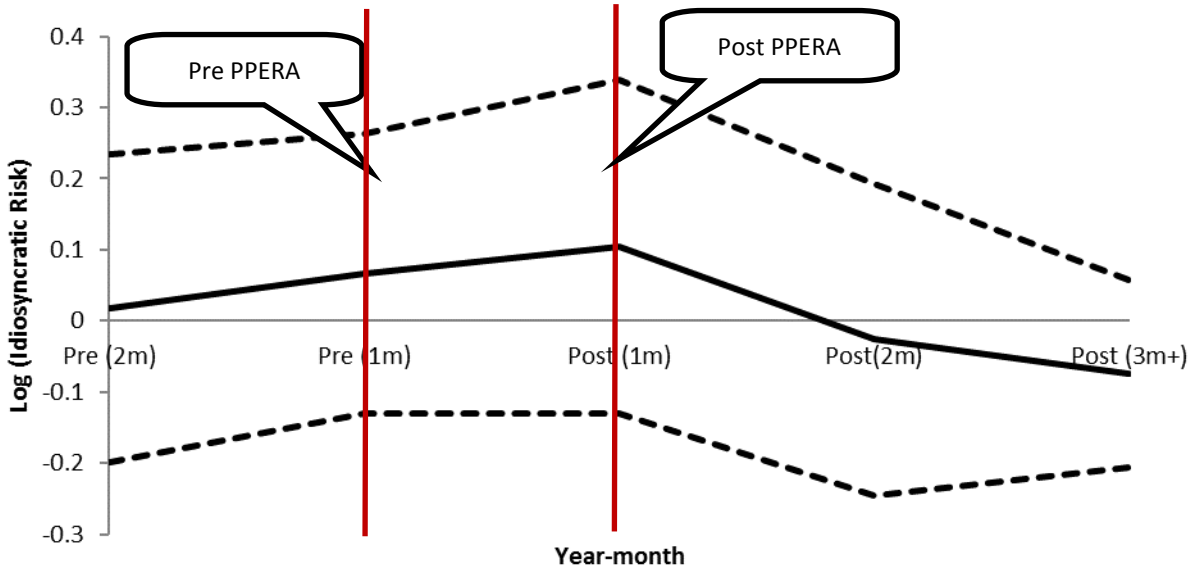
The figure plots coefficient dynamics for the effects of the NCR on logged values of monthly idiosyncratic volatility. Each point estimate can be interpreted relative to the year-months 3 months and prior to the NCR release. The dashed lines show the 90% confidence interval. The sample covers the period October 1997 to October 1999.

Figure A2. Coefficient Dynamics for the Propensity-Matched Sample: Log Monthly Idiosyncratic Volatility Around the Neill Committee Report (NCR), October 1997 to October 1999



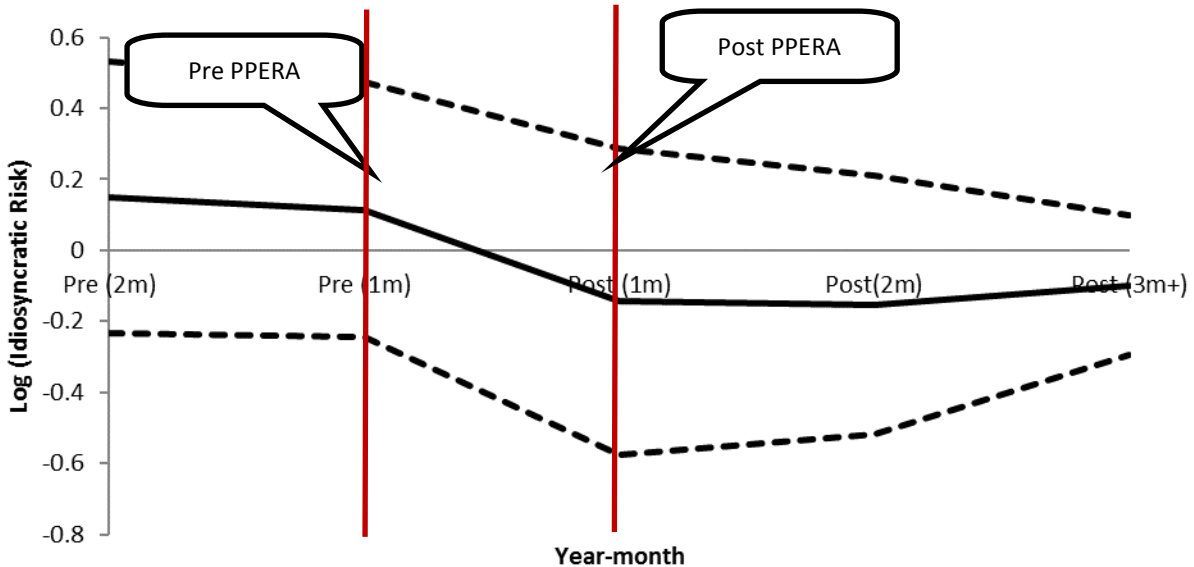
The figure plots coefficient dynamics for the effects of the NCR on logged values of monthly idiosyncratic volatility. Each point estimate can be interpreted relative to the year-months 3 months and prior to the NCR release. The dashed lines show the 90% confidence interval. The sample covers the period October 1997 to October 1999.

Figure A3. Coefficient Dynamics for the Full Sample: Log Monthly Idiosyncratic Volatility Around the PPERA, Pre-Period: Aug. 1999 – July 2000; Post-Period: Dec. 2000 – Nov. 2001.



The figure plots coefficient dynamics for the effects of the PPERA on logged values of monthly idiosyncratic volatility. Each point estimate can be interpreted relative to the year-months 3 months and prior to the PPERA. The sample covers the period August 1999 to November 2001. Year-months corresponding to the UK fuel protests (August 2000 to November 2000) are dropped from the sample. The dashed lines show the 90% confidence interval.

Figure A4. Coefficient Dynamics for the Propensity-Matched Sample: Log Monthly Idiosyncratic Volatility Around the PPERA, Pre-Period: Aug. 1999 – July 2000; Post-Period: Dec. 2000 – Nov. 2001.



The figure plots coefficient dynamics for the effects of the PPERA on logged values of monthly idiosyncratic volatility. Each point estimate can be interpreted relative to the year-months 3 months and prior to the PPERA. The sample covers the period August 1999 to November 2001. Year-months corresponding to the UK fuel protests (August 2000 to November 2000) are dropped from the sample. The dashed lines show the 90% confidence interval.