

# Risky Business: Do Disclosure and Shareholder Approval of Corporate Political Contributions Affect Stock Volatility and Value?<sup>†</sup>

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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, as proponents of such policies claim. 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 PPERA put into law, stronger disclosure and shareholder approval of political contributions. Using a differences-in-differences methodology combined with propensity score matching and quantile regressions, we find no consistent evidence that shareholders benefitted from these mandates. Instead, we find that after the NCR's release, higher-risk politically active firms suffered an increase in risk, as proxied by stock return volatility, hurting precisely the firms that such rules are thought to help. Moreover, this effect eventually filtered down to the entire distribution of politically active firms. While we do not find evidence that the value of politically active firms was immediately harmed by the NCR, we do find evidence that politically active firms suffered a decline in value, as proxied by Tobin's Q, in the years immediately following the NCR's release. These results present a challenge to arguments for mandating greater disclosure and shareholder oversight of corporate political activities.

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## 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 firms whose stock is publicly traded in the United States to disclose all of their political spending to shareholders and receive permission from shareholders before making such expenditures. The pressure for greater transparency and shareholder oversight has taken many forms, including legislation,<sup>1</sup> disclosure ratings,<sup>2</sup> 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.<sup>3</sup> In this study, we provide evidence to the contrary, and show that such mandatory shareholder disclosure and approval policies could in fact *increase* return volatility and *reduce* firm value.<sup>4</sup>

There are three interrelated arguments for why corporate political spending harms firm value and increases volatility: activism; agency concerns; and moral hazard. Specifically, some proponents of a stricter regulatory regime view corporate political spending as risky because (a) it draws unwanted attention from activists, creating risks (reputational,<sup>5</sup> legal liability, and

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<sup>1</sup> See, for example, 2017 US House of Representatives bill H.R. 376, known as the Shareholder Protection Act of 2017.

<sup>2</sup> See, for example, the CPA-Zicklin Index of Corporate Political Accountability and Disclosure, available at [www.politicalaccountability.net](http://www.politicalaccountability.net).

<sup>3</sup> 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.

<sup>4</sup> 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).

<sup>5</sup> 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.

business strategy misalignment)<sup>6</sup> and a potential loss in value for the politically active firm, (b) managers may use the firm's political spending to pursue a political agenda at odds with the interests of the firm, reducing firm value and potentially exposing the firm to activist criticism (Bebchuk and Jackson 2010; Aggarwal et al. 2012),<sup>7</sup> and (c) it may also lead to moral hazard, encouraging managers to take excessive risks in the belief that they will be protected by the government in the event that the bet goes bad (Kostovetsky 2015).<sup>8</sup>

By constraining managers' ability to use corporate funds for political purposes, disclosure and shareholder approval would seem to be policies that offer many benefits with few costs. Yet, there are several reasons to think that these policies will not be beneficial to shareholders. One, 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, creating the potential for negative media coverage, reputational risks, and business disruption for the politically active firms. Second, managers fearful of reputational harms due to such disclosures may spend less money on political activities due to concerns about the appearance of seeking "favors" from government and to prevent an attack by hostile interest groups, leading to an increased risk of unfavorable regulatory or legislative changes.<sup>9</sup> Third, shareholder approval could expose a publicly traded

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

<sup>7</sup> Managers may also use political spending to affect policies that reduce investors' ability to address agency problems with respect to other corporate decisions (Werner and Coleman 2015). The effects of these actions can be surprising; for instance, Low (2009) finds that the passage of an anti-takeover statute in Delaware led managers may play it too safe and reduce firm risk below optimal levels (Low 2009).

<sup>8</sup> 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 drive 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.

<sup>9</sup> Such disclosures may also reduce the marginal benefits of rent seeking if politicians become wary of appearing to grant favors to politically connected firms. This may benefit 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.

firm to greater regulatory risk if it reduces the firm's flexibility and agility in responding to a proposed regulatory change vis-à-vis privately held firms and other interest groups, or if the fear of losing a shareholder vote dissuades managers from spending money on political activities. More generally, firms use political spending to manage political risk and reduce their sensitivity to political uncertainty. 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 methodology combined with propensity score matching and quantile regressions, we find no consistent evidence that shareholders benefitted from these mandates. Instead, we find that after the NCR's release, higher-risk politically active firms suffered an *increase* in risk, as proxied by stock price volatility, hurting precisely the firms that such rules are thought to help. Moreover, this effect eventually filtered down to the entire distribution of politically active firms. Further, we find that while there was no immediate effect of the NCR on returns, the longer-run effect was a decrease in firm value by 3-5% for firms that were contributing to UK political parties prior to its release

(as proxied by Tobin's Q).<sup>10</sup> 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.

This paper contributes to the literature on political connections, contributions, and lobbying (Milyo et al. 2000; Fisman 2001; Khwaja and Mian 2005; Faccio 2006; Faccio et al. 2006; Fan et al. 2007; Goldman et al. 2009; Yu and Yu 2011; Fisman et al. 2012; Kostovetsky 2015; Akey, 2015; Fang and Prabhat 2017) by providing evidence that calls into question the claim that mandatory disclosure and shareholder approval of corporate political activity reduce risk and increase firm value. 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 both offensive and defensive political strategies.<sup>11</sup>

Our paper also contributes to the literature on shareholder rights 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; Matsusaka and Ozbas 2017). Our paper highlights 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

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<sup>10</sup> 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.

<sup>11</sup> 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.

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. Matsusaka and Ozbas (2017) show how shareholder approval rights have limited (if any) benefit for shareholder value, and how managers might react to shareholder proposal rights by moving firm policies away from those that pursue profit maximization.

Finally, we contribute to the understanding of corporate disclosure's effects. The case for disclosure typically centers on information asymmetries and conflicts of interest between managers and investors (Healy and Palepu 2001). Our findings are in line with empirical papers that find negative effects of disclosure due to transmission of proprietary information to competitors (Darrrough and Stoughton 1990); increased litigation risk (Rogers and Van Buskirk 2009); and reputational and political costs 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 complements Werner (Forthcoming), who uses a natural experiment—the inadvertent disclosure of corporate political spending—and finds that these disclosures negatively affected the returns of those firms that were already the target of shareholder resolutions regarding political spending disclosure, but increased the returns of other politically active firms (which presumably were not as vulnerable to activists).

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, the 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, some 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. 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, make identification far less plausible for the PPERA itself.<sup>12</sup>

### **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.<sup>13</sup> The Labour Research Department (LRD) generously provided us with

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<sup>12</sup> 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.

<sup>13</sup> 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.



data on contributions to UK political parties.<sup>14</sup> We supplemented the LRD data by checking annual reports of publicly listed firms in the United Kingdom.

We use three measures of risk. The first is total risk, defined as the annualized volatility of daily stock returns. 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. Measuring Risk

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

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

$R_{iTt}$  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  $R_{iTavg}$  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,

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<sup>14</sup> 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.

*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

may all be relevant for investors, we estimate the effects of the NCR's release on all three measures.

### 3.2. Statistical Model for Examining the Effects of the NCR on Risk

To test for the effects of the NCR's release on risk, 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.

$\text{Post}_{it}$  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<sub>it</sub>* or *Politically Active<sub>it</sub>* dummy variables drop out of the model. The coefficient of interest in the model is  $\beta$ , which approximates the percentage change in risk for politically active firms caused by proposed disclosure and shareholder approval regulations.<sup>15</sup>

In another specification, we control for industry and time fixed effects and include the non-interacted *Politically Active<sub>it</sub>* dummy variable 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  $\gamma$ ). The main coefficient of interest in the model is  $\beta$ , which approximates the percentage change in risk for politically active firms caused

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<sup>15</sup> Because we are working with the log of risk, the percentage effect is calculated using the exponential of the coefficient and the formula  $\exp(\beta)-1$ .

by proposed disclosure and shareholder approval regulations. We cluster standard errors by firm in both models (3) and (4).

Finally, we estimate model (4) using quantile regressions to assess whether the risk for politically active firms has increased differentially across the quantiles (10%, 20%, 30%, 40%, 50%, 60%, 70%, 80%, 90%) after the NCR release. Quantile regressions estimate quantiles of the conditional distribution of the dependent variable as a function of observed covariates, while OLS estimates the conditional mean of the dependent variable as function of observed covariates. The quantile regression results help us assess whether a change in risk varies across the quantiles.

### *3.3. Statistical Models for Examining the Effects of the NCR on Value*

To test for the effects of the NCR on value, we undertake two types of analyses. First, we estimate one-day, three-day, and five-day cumulative abnormal returns (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

$$CAR = \alpha X_{it} + \beta Politically\ Active_{it} + industry\ fixed\ effects + \varepsilon_{it}. \quad (5)$$

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 the abnormal return on the event date. Three-day CAR is the abnormal return cumulated over three days, starting one day prior to the event date and ending one day

after the event date. Five-day CAR is the abnormal return cumulated over five days, starting two days prior to the event date and ending two days after the event date.

Second, to allow for learning about the consequences of the NCR to occur, we consider a longer event window and use a differences-in-differences technique (Bertrand et al. 2004) to analyze the effects on value. Our regression specifications are

$$\text{Log}(\text{Value}_{it}) = \alpha X_{it} + \gamma \text{Politically Active}_{it} + \beta \text{Post}_{it} \times \text{Politically Active}_{it} + \text{time fixed effects} + \text{industry fixed effects} + \varepsilon_{it} \quad (6)$$

and

$$\text{Log}(\text{Value}_{it}) = \alpha X_{it} + \beta \text{Post}_{it} \times \text{Politically Active}_{it} + \text{time fixed effects} + \text{firm fixed effects} + \varepsilon_{it} \quad (7)$$

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. The calculation of Tobin's Q requires accounting data that is available on a yearly basis, so the analyses utilizing this variable look at changes in Tobin's Q on a year-over-year basis.

### 3.4. Descriptive Statistics (All Firms)

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's release year. We separate the firms into politically active and inactive firms, where politically active firms are defined as 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 coincided with a presidential election.<sup>16</sup>

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. There is no meaningful difference in our measure of value, Tobin's Q, or on measures of abnormal returns. The parallel trends assumption holds for the dependent variables in the analyses for both the full sample and the propensity-matched sample, to which we now turn.<sup>17</sup>

### *3.5. Descriptive Statistics (Propensity-Matched Sample)*

A potential methodological concern for our analysis 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 the firm-level controls

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<sup>16</sup> 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.

<sup>17</sup> It is not necessary to test for parallel trends for the CAR regressions, however.

that we use in our main analysis. 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 the 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, with no meaningful difference in value or abnormal returns.

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

We focus on the NCR's release in October 1998 as our event of interest in our regression analyses, as the NCR's recommendations were surprising to political observers and formed the basis for the PPERA legislation. First, however, we provide some suggestive evidence regarding the PPERA's impact.

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.<sup>18</sup> 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 suggestive evidence that the PPERA led to 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 begin 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 first use three months of data around the NCR (July 1998 through January 1999) for the differences-in-differences analysis in order to minimize the likelihood of other events affecting our analysis. If the change in risk is concentrated around the report, then it more likely that identification is

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<sup>18</sup> 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 14). If we restrict the post-PPERA contributions to those consistent with the pre-PPERA definition, the results are similar.

coming from the report and not from some other event.<sup>19</sup> Panel A reports the results for all publicly traded firms in the UK. Panel B reports the findings 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-month is after the NCR's release, and 0 otherwise. Columns (1) to (3) present the results for regressions with industry fixed effects, and columns (4) to (6) present the results with firm fixed effects. The most robust of the findings in Panel A is that idiosyncratic risk spikes in the wake of the NCR. Importantly, we find no evidence that risk levels dropped due to the NCR.

When we perform the same analysis on the propensity-matched sample (Panel B), the magnitude of the coefficients is comparable, but the statistical significance of our findings is weakened. We still find evidence for an increase in idiosyncratic risk, but only with industry-fixed effects and not when we implement firm fixed effects. We still find no evidence that risk levels dropped due to the NCR.

It is possible, however, that the average effects estimated by the regressions mask important differences across the risk distribution. To examine whether the entire distribution of volatility has changed after the NCR release, we perform quantile regressions on both the full sample and the propensity-matched sample. The results from quantile regressions are presented in Panels A to C of Table 4 (full sample) and Table 5 (propensity-matched sample), where each column presents the result from estimating equation (4) for a particular quantile of the dependent variable.<sup>20</sup> It is clear from these results that the initial effects of the NCR were concentrated on

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<sup>19</sup> Regulations related to corporate governance were released in years 1998 and 1999: the Hampel Report was released in January 1998, the Combined Code on Corporate Governance was released in June 1998, and the Turnbull Report was released in September 1999. By restricting the sample to  $-/+3$  months around the NCR (July 1998 to January 1999), we address concerns that corporate-governance-related events drive our results.

<sup>20</sup> A quantile regression with firm fixed effects failed to converge, which we believe is due to the computational demands of quantile regressions.

higher-risk firms. In none of the six analyses do we find evidence that firms through the 40<sup>th</sup> percentile saw an increase in risk, but we do find that firms toward the top end of the risk distribution were affected—regardless of which risk measure we use. This suggests that in the short run, the NCR hurt the firms that are thought to benefit from stricter oversight of political spending—higher-risk politically active firms.

While a tight time window around the NCR’s release gives us high confidence that identification is coming from the event, the negative is that it does not allow time for investors to learn about the likely consequences of the NCR. A one-year window around the NCR’s release (from October 1997 to October 1999) allows for such learning to take place, at the methodological expense of introducing the possibility that other events may also be influencing risk levels.

Table 6 presents differences-in-differences regressions for the full sample (Panel A) and for the propensity-matched sample, and Tables 7 and 8 present quantile regressions for the full sample and propensity-matched sample, respectively. In Table 6, we find very strong evidence that all types of risk for politically active firms increased as a result of the NCR. Tables 7 and 8 show that firms across the risk distribution were affected. One possibility for this evolution is that investors realized that the ability of firms to effectively manage regulatory issues—thereby encouraging stability—was especially threatened by the NCR.

#### *4.2. What is the Effect of the NCR on Firm Value?*

We examine the effect of the Neill Committee Report on firm value in two ways. First, we conduct an event study on the day the NCR was released. Table 9 presents the results. We do not find a statistically significant association between abnormal returns and the politically active indicator variable. This lack of an association may be due to 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. Also, as we noted earlier, parts of the report leaked in advance of its release, making it difficult to establish a clear breakpoint for the analysis.

To allow for learning by investors and to mitigate concerns about leaks, we conduct a differences-in-difference regression focused on Tobin's Q for multiple event windows (1997-1999 and 1996-2000). Tobin's Q is measured once per year and allows us to take a longer-run perspective on the effect of the NCR. Panel A of Table 10 reports results for the entire sample, and Panel B reports the results for the propensity-matched sample.

The negative coefficients on *Politically Active* in the industry fixed effects specifications in columns (1) and (2) of both Table 10 panels show that prior to the NCR's release, politically active firms were less valuable than politically inactive firms, consistent with their lower volatility.

As we noted earlier, the proposed regulations in the NCR had the potential to reduce a firm's flexibility and agility in responding to proposed regulatory changes vis-à-vis other firms. The firms may become more likely to face adverse regulatory changes and less likely to push for value enhancing regulatory changes, causing the return distribution to become more negatively skewed. This could be assessed using the third moment of the return distribution. However, the skewness measure does not satisfy the parallel trends assumption using the NCR as the breakpoint, so we cannot address this possibility directly. We can, however, use the firm fixed effects regressions in columns (3) and (4), combined with the volatility findings presented earlier, to indirectly assess this possibility. Specifically, does firm value decline as volatility increases—consistent with the idea that negative shocks are now more likely?

In a word: yes. We see that the NCR made matters worse for politically active firms, reducing firm value in addition to increasing volatility. Panel A shows that the NCR reduced firm value for politically active firms by 3-4%, while Panel B shows that the NCR reduced firm value for politically active firms by 3-5%.

## 5. Conclusion

In this paper, we find that greater oversight of corporate political behavior appears to hurt rather than help shareholders by increasing stock volatility, especially for higher-risk firms, and we find some evidence that it also reduces firm value as measured by Tobin's Q (though not when we look at announcement returns). These results run counter to the conventional wisdom that disclosure and shareholder democracy, including the PPERA law spawned by the NCR, are beneficial to shareholders.

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.<sup>21</sup> 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

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<sup>21</sup> 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).

that managers change their behavior in light of these fears (i.e., reduce their involvement in the political process), 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 NCR and PPERA, in the form of increased stock volatility and lower Tobin's Q, and offers good reason to be 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 et al. (2017) lends credence to this argument; these authors show that labor unions use their 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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**Table 1**  
**Descriptive Statistics: Means (1997)**

<b>Panel A: All Firms</b>	<b>(1)</b>	<b>(2)</b>	<b>(2) – (1)</b>
	<i>Politically Inactive Firms (N=1,507)</i>	<i>Politically Active Firms (N=147)</i>	<i>Diff</i>
<b><i>Risk, Returns, and Value</i></b>			
Log (Total Risk)	-2.54	-3.07	-.53***
Log (Systematic Risk)	-5.44	-5.38	.055
Log (Idiosyncratic Risk)	-2.64	-3.23	-.59***
AR[0]	-.00013	-.0012	.000062
AR[-1,+1]	-.0072	-.0015	.0057
AR[-2,+2]	-.0095	-.0000017	.0095
Log (Tobin's Q)	.69	.63	-.059
<b><i>Firm-level controls</i></b>			
Log (Size)	10.89	12.75	1.86***
Market-to-book	.46	.25	-.20***
ROA	.092	.13	.039**
Leverage	.18	.19	.018
R&D Expenditure	.0089	.0071	-.0019
Capital Expenditure	.069	.057	-.013
<hr/>			
<b>Panel B: Propensity-Matched Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(2) - (1)</b>
	<i>Politically Inactive Firms (N=117)</i>	<i>Politically Active Firms (N=117)</i>	<i>Diff</i>
<b><i>Risk, Returns, and Value</i></b>			
Log (Total Risk)	-2.84	-3.12	-.28**
Log (Systematic Risk)	-5.43	-5.54	-.11
Log (Idiosyncratic Risk)	-2.96	-3.27	-.32***
AR[0]	.00073	-.0011	-.0018
AR[-1,+1]	-.0031	-.0019	.0012
AR[-2,+2]	-.0070	.0015	.0085
Log (Tobin's Q)	.64	.60	-.04
<b><i>Firm-level controls</i></b>			
Log (Size)	12.03	12.22	.19
Market-to-book	.23	.25	.021
ROA	.13	.14	.0075
Leverage	.20	.19	-.003
R&D Expenditure	.0060	.0064	.00039
Capital Expenditure	.058	.060	.0020

Note: The number of firms for which we have announcement return information is less than the number of firms for which we have accounting, risk, and value data due to unavailable pricing data around the announcement day, which in part is due to illiquidity of some stocks. N for the AR[] variables is as follows: Panel A, Col (1): 1,266; Panel A, Col. (2): 108; Panel B, Cols. (1) and (2): 108. 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**  
**Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, July 1998 to January 1999**

<b>Panel A: Full Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>	<b>(6)</b>
	<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	.14* (.083)	.070 (.098)	.19** (.084)	.12 (.088)	.050 (.11)	.16* (.089)
Politically Active	-.13 (.087)	-.089 (.10)	-.17** (.086)			
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 <sup>2</sup>	.17	.18	.16	.06	.12	.04

  

<b>Panel B: Propensity-Matched Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>	<b>(6)</b>
	<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	.21 (.14)	.10 (.17)	.26* (.14)	.16 (.15)	.083 (.18)	.19 (.15)
Politically Active	-.18* (.098)	-.12 (.11)	-.22** (.10)			
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 <sup>2</sup>	.24	.28	.21	.08	.14	.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 4**

**Quantile Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, July 1998 to January 1999 (Full Sample)**

<b>Panel A (Total Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	-.044 (.16)	-.040 (.11)	-.022 (.091)	.12 (.098)	.17 (.10)	.28*** (.11)	.33*** (.062)	.30*** (.066)	.18* (.095)
Politically Active	.011 (.063)	.036 (.08)	-.033 (.044)	-.11** (.052)	-.10** (.050)	-.14** (.061)	-.17*** (.042)	-.19*** (.056)	-.17*** (.057)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	8,694	8,694	8,694	8,694	8,694	8,694	8,694	8,694	8,694

  

<b>Panel B (Systematic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	-.097 (.24)	-.15 (.17)	-.17 (.10)	.039 (.16)	.0047 (.14)	.18* (.10)	.20** (.10)	.33*** (.11)	.21*** (.069)
Politically Active	-.023 (.17)	.021 (.080)	.025 (.060)	-.022 (.075)	.012 (.059)	-.062 (.059)	-.052 (.067)	-.13** (.055)	-.077* (.041)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	8,694	8,694	8,694	8,694	8,694	8,694	8,694	8,694	8,694

  

<b>Panel C (Idiosyncratic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.052 (.10)	.046 (.10)	.056 (.095)	.10 (.095)	.10 (.088)	.24*** (.090)	.25*** (.089)	.33*** (.070)	.22** (.10)
Politically Active	-.079 (.062)	-.033 (.045)	-.077 (.057)	-.13** (.059)	-.12*** (.047)	-.19*** (.046)	-.18*** (.034)	-.26*** (.058)	-.12 (.088)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	8,694	8,694	8,694	8,694	8,694	8,694	8,694	8,694	8,694

Note: The sample starts 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. Percentages in parentheses for each column denote the percentile on which the quantile regression was performed. Standard errors clustered by firm are in parentheses. \*\*\*, \*\*, and \* denote significance at 1%, 5%, and 10%, respectively.

**Table 5**

**Quantile Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, July 1998 to January 1999 (Propensity-Matched Sample)**

<b>Panel A (Total Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	-.073 (.15)	-.025 (.12)	-.031 (.11)	.22 (.14)	.34*** (.13)	.24** (.10)	.17 (.11)	.221* (.119)	.23** (.094)
Politically Active	.063 (.099)	-.013 (.071)	-.054 (.070)	-.16*** (.058)	-.25*** (.058)	-.19*** (.052)	-.24*** (.073)	-.268*** (.0700)	-.30*** (.059)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314

  

<b>Panel B (Systematic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.085 (.28)	-.19 (.18)	-.065 (.17)	-.066 (.16)	.17 (.17)	.31* (.18)	.37** (.15)	.18 (.13)	.20** (.090)
Politically Active	-.024 (.19)	.083 (.11)	-.019 (.10)	-.027 (.095)	-.13 (.10)	-.25*** (.084)	-.30*** (.068)	-.20*** (.066)	-.23*** (.055)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314

  

<b>Panel C (Idiosyncratic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.21 (.15)	.14 (.10)	.10 (.11)	.15 (.13)	.31** (.13)	.25*** (.089)	.28** (.13)	.097 (.14)	.16 (.15)
Politically Active	-.15* (.085)	-.13* (.077)	-.16*** (.056)	-.15** (.075)	-.27*** (.057)	-.30*** (.054)	-.29*** (.052)	-.21*** (.056)	-.17*** (.050)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314

Note: The sample starts 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. Percentages in parentheses for each column denote the percentile on which the quantile regression was performed. Standard errors clustered by firm are in parentheses. \*\*\*, \*\*, and \* denote significance at 1%, 5%, and 10%, respectively.



**Table 6**  
**Widening the Event Window: Differences-in-Differences Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1997 to October 1999**

<b>Panel A: Full Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>	<b>(6)</b>
	<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	.26*** (.064)	.24*** (.069)	.28*** (.064)	.20*** (.062)	.16** (.069)	.22*** (.063)
Politically Active	-.16** (.078)	-.10 (.08)	-.19** (.078)			
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 <sup>2</sup>	.16	.18	.16	.08	.13	.07

  

<b>Panel B: Propensity-Matched Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>	<b>(6)</b>
	<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	.28*** (.100)	.23** (.11)	.30*** (.10)	.19* (.10)	.14 (.11)	.21** (.10)
Politically Active	-.21** (.088)	-.14 (.096)	-.24*** (.089)			
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 <sup>2</sup>	.24	.27	.22	.13	.17	.11

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 7**

**Quantile Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1997 to October 1999 (Full Sample)**

<b>Panel A (Total Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.27*** (.062)	.30*** (.066)	.21*** (.050)	.23*** (.047)	.28*** (.048)	.28*** (.044)	.25*** (.038)	.20*** (.045)	.15*** (.041)
Politically Active	-.071 (.050)	-.098** (.045)	-.065 (.042)	-.11*** (.033)	-.14*** (.031)	-.14*** (.031)	-.14*** (.031)	-.15*** (.039)	-.14*** (.033)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	30,952	30,952	30,952	30,952	30,952	30,952	30,952	30,952	30,952

  

<b>Panel B (Systematic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.34*** (.089)	.21** (.089)	.15** (.070)	.22*** (.078)	.22*** (.066)	.23*** (.045)	.20*** (.065)	.20*** (.056)	.26*** (.041)
Politically Active	-.19** (.083)	-.058 (.064)	-.035 (.053)	-.069 (.048)	-.034 (.045)	-.067* (.037)	-.041 (.044)	-.074** (.030)	-.14*** (.036)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	30,952	30,952	30,952	30,952	30,952	30,952	30,952	30,952	30,952

  

<b>Panel C (Idiosyncratic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.31*** (.075)	.28*** (.057)	.27*** (.058)	.27*** (.056)	.30*** (.047)	.26*** (.044)	.21*** (.038)	.22*** (.045)	.16*** (.043)
Politically Active	-.11*** (.034)	-.096** (.039)	-.12*** (.045)	-.15*** (.035)	-.18*** (.035)	-.19*** (.035)	-.16*** (.030)	-.17*** (.032)	-.17*** (.034)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	30,952	30,952	30,952	30,952	30,952	30,952	30,952	30,952	30,952

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. Percentages in parentheses for each column denote the percentile on which the quantile regression was performed. Standard errors clustered by firm are in parentheses. \*\*\*, \*\*, and \* denote significance at 1%, 5%, and 10%, respectively.

**Table 8**

**Quantile Regressions Estimating the Effect of the Neill Committee Report on Monthly Return Volatility for UK Firms, October 1997 to October 1999 (Propensity-Matched Sample)**

<b>Panel A (Total Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.33*** (.078)	.32*** (.075)	.24*** (.066)	.28*** (.067)	.20*** (.053)	.28*** (.058)	.24*** (.062)	.18*** (.059)	.17*** (.058)
Politically Active	-.24*** (.066)	-.25*** (.054)	-.17*** (.042)	-.18*** (.041)	-.15*** (.036)	-.19*** (.032)	-.15*** (.042)	-.13*** (.040)	-.13*** (.039)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	4,569	4,569	4,569	4,569	4,569	4,569	4,569	4,569	4,569

  

<b>Panel B (Systematic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.58*** (.098)	.19* (.099)	.080 (.099)	.14 (.091)	.20** (.084)	.16** (.068)	.19** (.081)	.16* (.081)	.18** (.080)
Politically Active	-.34*** (.070)	-.16*** (.063)	-.071 (.076)	-.10* (.056)	-.078 (.061)	-.087** (.038)	-.071 (.057)	-.036 (.036)	-.16*** (.045)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	4,569	4,569	4,569	4,569	4,569	4,569	4,569	4,569	4,569

  

<b>Panel C (Idiosyncratic Risk)</b>									
	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
Post x Politically Active	.30*** (.078)	.31*** (.066)	.32*** (.058)	.31*** (.069)	.28*** (.062)	.27*** (.058)	.29*** (.059)	.21*** (.061)	.21*** (.069)
Politically Active	-.26*** (.059)	-.28*** (.049)	-.26*** (.041)	-.24*** (.045)	-.23*** (.040)	-.26*** (.032)	-.24*** (.042)	-.19*** (.033)	-.15*** (.052)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	N	N	N	N	N	N	N	N	N
Industry fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	4,569	4,569	4,569	4,569	4,569	4,569	4,569	4,569	4,569

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. Percentages in parentheses for each column denote the percentile on which the quantile regression was performed. Standard errors clustered by firm are in parentheses. \*\*\*, \*\*, and \* denote significance at 1%, 5%, and 10%, respectively.

**Table 9**  
**Regressions Estimating the Effect of the Neill Committee Report Announcement on Announcement Returns, Various Time Windows**

<b>Panel A: Full Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>
	AR[0]	AR[-1,+1]	AR[-2,+2]
Politically Active	-.00037 (.0017)	.0027 (.0058)	.0065 (.0068)
Constant	-.018* (.010)	-.031 (.022)	-.069** (.027)
Firm-level controls	Y	Y	Y
Industry fixed effects	Y	Y	Y
N	1,375	1,375	1,375
Adj. R <sup>2</sup>	.07	.06	.06

  

<b>Panel B: Propensity-Matched Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>
	AR[0]	AR[-1,+1]	AR[-2,+2]
Politically Active	-.0019 (.0028)	.00040 (.0080)	.0069 (.010)
Constant	-.015 (.014)	-.048 (.030)	-.074* (.041)
Firm-level controls	Y	Y	Y
Industry fixed effects	Y	Y	Y
N	216	216	216
Adj. R <sup>2</sup>	.30	.30	.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.

**Table 10**

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

<b>Panel A: Full Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>
	1997-1999	1996-2000	1997-1999	1996-2000
Post x Politically Active	-.014 (.017)	.0057 (.018)	-.036** (.017)	-.032** (.017)
Politically Active	-.061** (.061)	-.073*** (.026)		
Firm-level controls	Y	Y	Y	Y
Industry fixed effects	Y	Y	N	N
Firm fixed effects	N	N	Y	Y
Year fixed effects	Y	Y	Y	Y
N	4,415	7,101	4,415	7,101
Adj. R <sup>2</sup>	.30	.40	.14	.22

  

<b>Panel B: Propensity-Matched Sample</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>
	1997-1999	1996-2000	1997-1999	1996-2000
Post x Politically Active	-.0039 (.023)	-.025 (.026)	-.032* (.020)	-.049** (.021)
Politically Active	-.090*** (.032)	-.078** (.031)		
Firm-level controls	Y	Y	Y	Y
Industry fixed effects	Y	Y	N	N
Firm fixed effects	N	N	Y	Y
Year fixed effects	Y	Y	Y	Y
N	619	986	619	986
Adj. R <sup>2</sup>	.55	.51	.25	.38

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.