HOW DO INVESTORS REACT TO CORPORATE POLITICAL SPENDING DISCLOSURE?

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ABSTRACT

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My dissertation consists of three main parts. In the first part, I provide a literature review discussing the history of campaign finance law in the United States, the literature examining the determinants of corporate political spending, the literature examining the influence of political concerns on investing decisions, and the literature applicable to the disclosure of corporate political spending. This part is intended to provide the reader with a broad base of information to better understand the context of my experimental study in the second part.

In the second part, I present an experimental study examining how investors react to the disclosure of corporate political spending. In the ongoing debate over whether public companies should be required to disclose their political spending, one frequent argument against requiring disclosure is that investors do not consider political spending information relevant for their decisions. However, I predict and find in my experiment that investors whose political identities are aligned with a company's political spending assess the company as more attractive and invest more in the company than investors whose political identities are misaligned with the political spending. Interestingly, this effect stems from the negative reactions of misaligned investors. The attractiveness assessments and investment amounts of misaligned investors are significantly lower than those of investors in a control condition with no political spending disclosure, but those of aligned investors are not significantly different from those in the control condition. My results also provide evidence that investors use political spending information consciously rather than because

of a subconscious bias. These findings have implications for regulators because, contrary to the argument that political spending information is irrelevant to investors, my results suggest that investors consciously use political spending information in their investment decisions.

In the third part, I discuss future research possibilities on corporate political spending disclosure. I outline research questions that would extend my experimental study, and I identify research questions related to managerial decision-making about corporate political spending and its disclosure. Finally, I note the importance of also considering the effects of corporate political spending disclosure on other stakeholders such as suppliers and customers.

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PREFACE

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1.0 INTRODUCTION

Corporate political spending is a topic that has only recently been broached by accounting researchers (Lu et al. 2014, Baloria et al. 2016) but that has been debated vigorously in the legal field since the Citizens United decision in 2010 (Bebchuk and Jackson 2012). In this dissertation, I aim to inform the reader about corporate political spending and its disclosure, to provide evidence that disclosing information about corporate political spending can affect investors' decisions, and to discuss future research opportunities involving corporate political spending and its disclosure.

In chapter 2, I provide a broad literature review pertaining to corporate political spending and its disclosure. I start with a history of campaign finance law in the United States to describe the context in which the current corporate political spending situation developed. Then, I discuss the literature on the determinants of corporate political spending, which is dominated by two explanations: strategic investment and agency problems. Next, I review the literature that has examined political influences on investor decision-making. Finally, I discuss the literature on disclosure as it pertains to corporate political spending.

In chapter 3, I present my experimental study providing evidence that investors use corporate political spending information when it is available and that their use of this information depends on the alignment or misalignment of their political identities with the politics being funded by the corporation. I predict and find in my experiment that investors whose political identities are aligned with a company's political spending assess the company as more attractive and invest more in the company than investors whose political identities are misaligned with the political spending. This effect stems from the negative reactions of misaligned investors. I also provide evidence that investors use the political spending information consciously rather than having a subconscious bias.

In chapter 4, I discuss opportunities for future research in the area of corporate political spending and its disclosure. I elaborate upon a few additional research questions that could be addressed by extending my study from chapter 3. Additionally, I present questions regarding managers' decision-making on corporate political spending and its disclosure, and I consider what the effects of corporate political spending disclosure might be on other stakeholders such as suppliers and customers.

2.0 LITERATURE REVIEW

2.1 HISTORY OF CAMPAIGN FINANCE LAW IN THE UNITED STATES

The history of political spending in the United States pre-dates the formation of the country as an independent nation. In his 1755 race for the Virginia House of Burgesses, George Washington refused to buy what was then a customary offering of food and alcohol for the voters on election day. He lost that election, and subsequently in the 1757 election, he provided 160 gallons of alcoholic beverages to a group of 391 voters. He won that time, but the House of Burgesses went on to outlaw the practice of wining and dining voters for votes shortly thereafter (Toedtman 2012, Brusoe 2016).

Although that could be seen as just a humorous anecdote, the influence of money in American politics has been present since those pre-revolutionary days and continued in ebbs and flows in the centuries that followed. In 1828, Andrew Jackson became the first presidential candidate to run what modern Americans would recognize as a campaign, with two campaign offices and the distribution of pamphlets. After his election, he also made sure that some of his supporters found roles in the federal government ("Money-in-Politics Timeline" 2017). Later, prior to his successful presidential campaign, Abraham Lincoln went so far as to purchase a newspaper in his home state of Illinois and contracted with its publisher terms stating that, "the paper must publish weekly and support the Republican Party." After being elected, Lincoln sold the paper and gave its publisher a role in government as "special consul to Vienna" (Brusoe 2016).

Despite these questionable practices, it was not until 1867 that Congress passed the first law pertaining to federal campaign finance. In the Naval Appropriations Bill, federal officials were prohibited from seeking political contributions from naval yard workers ("Money-in-Politics Timeline" 2017). Then, in 1881, James Garfield was assassinated by a man whom he had rejected for a government position, and by 1883, the Civil Service Reform Act extended the prior prohibition to cover soliciting money from "any civil service workers" and made it illegal to grant civil service positions for reasons other than merit (Fuller 2014). With kickback schemes outlawed, corporations became an even more important player in elections with the 1896 McKinley campaign raising more than $6 million of its $16 million in contributions from corporations that had been persuaded by its pro-business platform. This corporate support of politics continued through the 1904 presidential election with Theodore Roosevelt benefiting from and then subsequently denouncing corporate political contributions. He called on Congress to ban them, and in 1907 the Tillman Act was passed banning all campaign contributions from corporations and national banks ("Money-in-Politics Timeline" 2017).

That legislation was followed in 1910 by the Federal Corrupt Practices Act (FCPA) that required campaign finance disclosure from all members of the House of Representatives. The FCPA was then amended in 1911 to extend the required disclosure to all members of the Senate, as well as to candidates in congressional primary races. Per member spending limits were also introduced with the amendment (Fuller 2014). It did not take long, however, before the Act was challenged. In 1918, Henry Ford lost a primary race to be the Republican senatorial candidate from Michigan and accused his opponent Truman Handy Newberry of violating the FCPA. Newberry was convicted of the violation and appealed the decision to the Supreme Court. In 1921, in Newberry v. United States, the Court held that Congress could not regulate primaries, exonerating Newberry ("Money-in-Politics Timeline" 2017, "The Election Case" 2017). In the wake of the Teapot Dome scandal, the FCPA was amended once more in 1925 to broaden filing requirements, make filings quarterly, and require the disclosure of any contributions over $100. However, the law continued to be ineffective as, “there were no penalties for failing to file” (Fuller 2014, “Money-in-Politics Timeline” 2017). In reality, Congress did not collect the filings required under this amendment until 1967, and even then, the Department of Justice failed to take action on reported violations of the law (“Important Dates” 2014).

More groups were excluded from making political contributions as time passed. In 1935, the Public Utilities Holding Act prevented public utilities from giving to political campaigns (“Money-in-Politics Timeline” 2017). In 1943, the Smith-Connally Act banned labor unions from giving to political campaigns. This move led the Congress of Industrial Organizations, a labor union, to create the first political action committee, or PAC. With a PAC, the union could subvert the new law by having a separate entity collect voluntary donations from members whose union dues could no longer be used toward political contributions (Fuller 2014, “Money-in-Politics Timeline” 2017). Then, in 1947, the Taft-Hartley Act “made permanent the ban on contributions to federal candidates from unions, corporations, and interstate banks, and extended the prohibition to include primaries as well as general elections” (“Important Dates” 2014). The Act also outlawed “independent expenditures” by these entities, precluding them from political spending altogether (Fuller 2014).

In 1971, Congress finally passed legislation intended to address federal campaign finance as a whole. With the Federal Election Campaign Act, FECA, most of the FCPA was superseded, and an array of new rules came into place. The FECA applied to “primaries, runoffs, general elections, and conventions” (“Important Dates” 2014). It increased the scope and frequency of required reporting and introduced spending limits for some kinds of campaign advertising. It also explicitly permitted “corporations and unions to use their own treasury funds to establish, operate, and solicit voluntary contributions for PACs" ("Money-in-Politics Timeline" 2017). Concurrently, Congress also passed the 1971 Revenue Act which established a "public campaign fund for eligible presidential candidates" by including a check-box on federal income tax returns such that taxpayers could direct $1 to the fund ("Important Dates" 2014).

At first, enforcement of the new rules under FECA was distributed among several different government offices, but the aftermath of the Watergate scandal brought an amendment to FECA in 1974 creating the Federal Election Commission, or FEC, to take over that role ("Money-in-Politics Timeline" 2017). The 1974 amendment also expanded the public financing of presidential campaigns and introduced more spending limits on federal election campaigns ("Important Dates" 2014). However, the new law did not stand for long before facing a challenge. Just two years later in *Buckley v. Valeo*, the Supreme Court held that some contribution limits were justifiable but spending limits were a violation of First Amendment rights ("Money-in-Politics Timeline" 2017). Furthermore, the Court provided an opening to corporations and unions by allowing them to sponsor "issue advocacy" advertising as long as it did not explicitly mention the election or defeat of a particular candidate (Beatty 2007).

This served as a harbinger of things to come with two more Supreme Court cases questioning what corporations could do with respect to election advertising. In 1986, the FEC brought a case against a 501(c)(4) not-for-profit corporation called Massachusetts Citizens for Life, Inc, or MCFL. The group had published a "voter's guide" to endorse specific pro-life candidates, ostensibly violating the rules against corporations paying for explicit election advertising. However, the Court held that, "the law in question was unconstitutional as applied to MCFL because the organization was created in order to disseminate political ideas, wasn't a for-profit corporation and didn't accept contributions from for-profit corporations" ("Money-in-

Politics Timeline" 2017). With this exception established, the Michigan Chamber of Commerce, another not-for-profit corporation, challenged the applicability of an election advertising ban to its efforts. In 1990, the Supreme Court considered the case of *Austin v. Michigan Chamber of Commerce* and held that the exception granted to MCFL did not apply there because the Michigan Chamber of Commerce collected dues from for-profit corporations ("Money-in-Politics Timeline" 2017).

Then, in 2002, the Bipartisan Campaign Reform Act, BCRA, also known as the McCain-Feingold Act, was passed. Although the main thrust of the legislation was to ban the "soft money" raised by political parties outside the oversight of the FEC, BCRA also tightened restraints on political advertising by corporations and unions that had been exploiting the letter of the law from the *Buckley v. Valeo* decision (Jones 2017). Corporations and unions had strongly implied their political endorsements through the issue-related advertising they were allowed, and BCRA put an end to these "electioneering communications" that endorsed or denounced a federal candidate (Jones 2017). BCRA was "immediately challenged" in *McConnell v. FEC*, but in 2003 the Supreme Court upheld the law's major provisions ("Money-in-Politics Timeline" 2017). However, by 2004, Wisconsin Right to Life, a not-for-profit corporation, ran advertisements that appeared to oppose the re-election of Russ Feingold, who ironically had co-sponsored BCRA. A lawsuit followed, and in 2007 the Supreme Court considered the case of *FEC v. Wisconsin Right to Life*. The Court ruled in favor of Wisconsin Right to Life with the majority opinion specifying that the only advertisements that corporations and unions could be prohibited from running would be those "susceptible of no reasonable interpretation other than as an appeal to vote for or against a specific candidate" (Beatty 2007).

Even this lax version of the ban on political advertising by corporations and unions only lasted a few years. In 2010, the Supreme Court's decision in *Citizens United v. FEC* dismantled much of the restrictions that had been placed on corporate and union political spending. Citizens United, a non-profit corporation, had produced a movie, "Hillary: The Movie," that the FEC deemed to be an electioneering communication, a violation of BCRA (Hamilton 2010). On appeal to the Supreme Court, Citizens United prevailed. The majority opinion did not deny that the movie was an electioneering communication but instead struck down the section of BCRA that had prohibited them stating that the prohibition had infringed upon the First Amendment right to freedom of speech. Furthermore, the decision in the earlier *Austin v. Michigan Chamber of Commerce* case was overturned (Hamilton 2010). The Court effectively granted corporations and unions the right to spend unlimited amounts on political advertising as long as they did so independently of the candidates and political parties ("Money-in-Politics Timeline" 2017). Nonetheless, the Court did maintain the ban on "direct corporate contributions to candidates" (Hamilton 2010). In a related case a few months later, *Speechnow.org v. FEC*, the D.C. Circuit Court of Appeals cited the recent Supreme Court decision in deciding that "unlimited independent expenditures" could not be linked to corruption risk (Fuller 2014). Thus, contributions to groups such as PACs and 501(c) organizations could not be limited, creating the legal room for SuperPACs that can receive and spend unlimited amounts (Liptak 2010).

These decisions also created the possibility of untraceable "dark money" political spending through 501(c) groups, which may have been an unintentional consequence. "Some experts believe that the Supreme Court did not realize that lax interpretation of the laws governing independent expenditures by the FEC and political activity of tax-exempt organizations by the IRS meant that transparency would not be guaranteed" ("Money-in-Politics Timeline" 2017). Nonetheless, the

Court saw a challenge to its *Citizens United v. FEC* decision the very next year in *American Tradition Partnership, Inc. v. Bullock* and held to its position, this time extending the recent precedent to cover state elections as well ("Money-in-Politics Timeline" 2017).

Most recently, the Supreme Court held in 2014 in the case of *McCutcheon v. FEC* that the limit on individuals' aggregate contributions across all candidates, parties, and PACs was unconstitutional, again citing the First Amendment ("Money-in-Politics Timeline" 2017). Although this limit did not constrain many donors, its removal does clear the path for those wealthy donors to spend more on elections especially through PACs, which can be created without limit (Fuller 2014). However, also in 2014, the Supreme Court declined to hear the case of *Iowa Right to Life Committee v. Tooker*, in which a lower court had held that Iowa's ban on direct corporate contributions to candidates and committees should stay in place (Hurley 2014). In rejecting the case, the Court allowed the federal ban on such direct corporate contributions to continue as well.

2.2 THE DETERMINANTS OF CORPORATE POLITICAL SPENDING

I now discuss the literature as it pertains to the determinants of corporate political spending. Two general positions on corporate political spending have been put forth in the literature. One holds that corporate political spending is strategic and has benefits for the firms that engage in it. The other regards corporate political spending as being symptomatic of agency problems with corporate executives using the firm's money to support their own interests rather than the best interests of the firm. I first discuss the studies arguing that corporate political spending is strategic and then discuss the studies arguing that it is the result of an agency problem.

2.2.1 Corporate Political Spending as a Strategic Investment

Prior research has shown that firms with politically connected directors experience benefits. Although none of these studies specifically examines corporate political spending, I mention them first because they may relate to corporate political spending through a path I discuss in the next paragraph. Goldman, Rocholl, and So (2008) examine the stock market performance of firms with directors who have political connections. First, the authors show that the addition of a politically connected director to a firm's board results in positive abnormal returns for the firm's stock. Second, they show that after the US presidential election in 2000 firms with directors who have Republican connections gain in market value while firms with directors who have Democratic connections decline in market value (Goldman et al. 2008). Consistent with that finding, Goldman, Rocholl, and So (2013) show that after the 1994 Congressional elections, which resulted in the Republicans taking control of the House and the Senate from the Democrats, firms with Republican-connected directors gained government procurement contracts while firms with Democrat-connected directors lost government procurement contracts. Also, companies with politically connected directors get lower interest rates and better terms on bank loans (Houston, Jiang, Lin, and Ma 2014).

Lu, Shailer, and Wilson (2016) show that in Australia director networks influence firms' political spending. In Australia, there is no limit on how much money a corporation can donate to political parties, and all political donations beyond a relatively low threshold are required to be reported. In this setting, the authors confirm that directors' professional networks, through corporate board interlocks, and non-professional networks, through non-profit board interlocks, affect their firms' political spending. Specifically, firms spend more on political donations if their directors are connected with directors of other firms that spend on political donations (Lu et al. 2016). Although this result has not yet been shown for US firms, where much of firms' political spending may not be disclosed, it is plausible that a similar effect could exist in the US. Thus, if directors exert some control over their firms' political spending, the benefits documented in the studies of politically connected directors may be due in part to their firms' political spending, which they have influenced. I may be the first to explicitly propose this mediating path between politically connected directors and the benefits their firms receive.

Moving on to research showing direct relationships between political spending and benefits to firms, Cooper, Gulen, and Ovtchinnikov (2010, p. 687) analyze data "of firm-level contributions to U.S. political campaigns from 1979 to 2004." The authors examine firm PAC contributions to political candidates and create a measure of the sum of the candidates that a firm supported through its PAC during a rolling five-year window. They find "a strong and robust correlation between this contribution measure and a firm's future abnormal returns" (Cooper et al. 2010, p. 718). That is, firms that support more political candidates generally have higher abnormal stock returns. This relationship is found to be particularly strong for firms that support more candidates in the states in which they are headquartered, for firms that support more candidates for House seats, and for firms that support more Democrats. There is also a significant positive relationship between the political contribution measure and firms' future profitability such that a larger number of political candidates supported predicts a higher future return on equity for the firm. Further analyzing the abnormal stock returns, the authors note that if one only considers the firms' PAC contributions, which averaged $23,471 per year over the sample period, this relationship implies "an extremely high rate of return," with firms having an average abnormal increase in shareholder wealth of $163.8 million per year (Cooper et al. 2010, p. 719). However, the authors speculate that the actual costs of currying political favor may be much higher considering that lobbying expenses are "20 to 60 times more" than PAC contributions and firms may also take other costly actions to build their political relationships (Cooper et al. 2010, p. 719).

Another study considers firm PAC contributions and comes to a similar conclusion about the effect on firm value through a novel approach. Akey (2015) examines the relationship between firm PAC contributions and abnormal equity returns in the context of close-call congressional elections in the US. Specifically, the author limits his analysis to congressional elections that were won by a margin of 5% or less, compared to the median election during the 1998-2010 period being won by 33% of the vote. Elections in this sample are thought to be more likely to have been uncertain ex-ante, thus increasing the chance of a postelection stock market reaction. Akey (2015, p. 3220) first looks at special elections, held to replace a member of Congress who has left before the end of his or her term, and finds "a causal effect of connectedness on firm value that is larger than estimates previously reported in the literature." The author's use of the word "connectedness" denotes his view that firm PAC contributions are just a proxy for how connected to a politician the firm is (Akey 2015). In a similar analysis using general elections to provide a larger sample, the effect size appears to be smaller, but the general finding holds: "On election days, the market reacts positively if a firm is connected to winning politicians and negatively if it is connected to losing politicians, but the magnitudes are smaller than for special elections" (Akey 2015, p. 3220). The study rules out politicians favoring particular industries or regions as an explanation for the effect. Furthermore, the study shows that stock market reactions are even stronger for the portion of contributions that went to "senior politicians' leadership PACs," what the author calls "indirect connections" (Akey 2015, p. 3220). These are firm PAC contributions that went to a more senior politician's PAC before going to support the candidate in the election. The author argues that the stronger effect makes sense due to the discretion these leadership PACs have over allocating funds to candidates. Simply put, "senior politicians may be able to influence party members in ways that firms cannot" (Akey 2015, p. 3220). Finally, similar to the Cooper et al. (2010) finding regarding return on equity, Akey (2015) finds that firms' fundamentals improve as a result of their political connections with future sales being positively affected by the firms' PAC contributions to winning candidates.

Along with these studies documenting positive correlations between corporate political spending and stock returns, other studies document more overt benefits to corporate political spending in the form of diminished government regulation. Correia (2014) examines SEC enforcement actions and penalties as a function of firm PAC contributions and lobbying expenses. Using a sample of firm-years during which firms have had to restate their financial statements, she finds that firms are less likely to face enforcement actions from the SEC in proportion to their history of PAC contributions. She finds a similar result for firms' lobbying expenses affecting the likelihood of enforcement, but interestingly her analyses of the effects on penalties for firms that come under enforcement actions show that only PAC contributions lead to lower penalties. In other words, firms with "long-term PAC contributions" appear to have developed political relationships that lower their chances of facing enforcement actions and lower the penalties associated with these actions if they arise (Correia 2014, p. 259). Consistent with the political relationship story, the study also notes that focusing on firm PAC contributions to politicians who are in better positions to pressure the SEC, such as being in the majority party or being on certain committees, yields even lower likelihoods of enforcement actions and lower penalties. In explaining the study's results, the author notes two possibilities. The firms' political contributions could lead to the politicians intervening on behalf of the firms, or they could just signal the firms' "willingness to fight the SEC's enforcement decision" (Correia 2014, p. 259). Either way, the study shows that "the SEC is influenced by considerations other than the merits of the case" (Correia 2014, p. 259).

Similarly, there is evidence that corporate political spending may decrease firms' tax burdens. Brown, Drake, and Wellman (2015) show that firms' long-term political spending can benefit them through reduced tax rates. They explicitly position their study as "providing an economic link for the observed contribution-return relation documented in Cooper, Gulen, and Ovtchinnikov (2010)" (Brown et al. 2015, p. 69). The study uses measures of firm PAC contributions similar to those used in Cooper et al. (2010) except that these measures only focus on contributions to members of congressional tax-writing committees. The authors find that firms that provide PAC contributions to tax-writing members of Congress have lower future effective tax rates (ETRs) than firms that do not, and this relationship is strengthened by the number of candidates supported and the length of time over which the support is consistently provided (Brown et al. 2015). Further, limiting the sample to only firms that have PACs, the number of candidates supported and the length of the support given both continue to have negative relationships with both future cash and GAAP ETRs. As an example, "supporting approximately 5.25 additional tax-writing members of Congress, is associated with a 1.69 percent lower future cash ETR and a 1.66 percent lower future GAAP ETR (approximately $33 million in tax savings)" (Brown et al. 2015). Using the same independent measures, the authors also document a negative relationship with volatility in firms' future cash ETRs. Essentially, firms benefit in proportion to the number of candidates they support and the length of time they support them by receiving not only lower tax rates but also more consistent taxation year to year. Drawing on theory about a "relational approach to political strategy" to explain their main results, the authors predict and find significant negative interactions between their two measures of political support and a measure of lobbying activity on both future cash and GAAP ETRs (Brown et al. 2015). That is, the study shows that the effects of firms' PAC contributions and firms' lobbying activities have a complementary relationship in regard to lowering firms' tax rates. The authors argue that this is consistent with a story wherein the firm uses its political spending to increase access for its lobbying efforts (Brown et al. 2015).

In studies that complement Brown et al. (2015) well, Kim and Zhang (2016) and Baloria and Klassen (2017) report evidence that firms behave strategically regarding their political connections and tax choices. Kim and Zhang (2016) use three different measures of firm political connections, including whether a firm had a PAC, to show that tax aggressiveness is greater among politically connected firms, regardless of the measures used for both political connections and tax aggressiveness. The authors control for "other determinants of tax aggressiveness, industry and year fixed effects, and the endogenous choice of being politically connected," and they ultimately conclude that their results are consistent with several possible explanations in which firms benefit, or at least expect to benefit, from their political connections (Kim and Zhang 2016, p. 78). Related, Baloria and Klassen (2017, p. 1) focus on the behavior of "firms that contributed to congressional candidates who favor reductions in the U.S. corporate statutory tax rate." Using data from the 2012 election, the authors find that these firms managed their effective tax rates upward by an average of 3% in the two quarters prior to the election. They find evidence consistent with the idea that the increases in ETRs were intended "to improve their candidate's prospects by avoiding the release of damaging information" (Baloria and Klassen 2017, p. 18). Specifically, firms engaged in greater

ETR management when they had stronger relationships with their supported candidates, as proxied by geographic co-location and same party affiliation, and when their supported candidates were in more competitive races (Baloria and Klassen 2017).

Taken together, the studies arguing for corporate political spending as a strategy provide evidence that firms' political contributions benefit them both in terms of stock market reaction and underlying economic realities. Furthermore, it appears that some of the economic benefits to these firms happen through straightforward regulatory channels, with firms having less reason to fear the SEC and IRS. However, other studies have shown negative results for firms engaged in corporate political spending, and I discuss that literature next.

2.2.2 Corporate Political Spending as an Agency Problem

Coates (2012) suggests that much, but not all, corporate political activity is a value-destroying misuse of executive power. The author acknowledges early in the paper that in industries subject to government regulation and/or reliant on government spending, "it seems hard to imagine that shareholders of any given firm would benefit from unilateral political disarmament" (Coates 2012, p. 658). He then argues that these industries may drive the findings of Cooper et al. (2010), which he asserts did not properly control for industry in some analyses. Using data from S&P 500 firms and measures of corporate political activity based on firms' PAC spending and lobbying expenses, Coates (2012) goes on to document a variety of concerning relationships involving corporate political activity, or CPA. The study shows that CPA is negatively correlated with both ownership concentration and shareholder rights, suggesting that firms in which shareholders have relatively weak control spend more on CPA. CPA is positively correlated with CEOs' personal use of corporate jets and with CEOs retiring to assume a political appointment or run for political office, suggesting that CPA is at least correlated with agency costs and may be an agency cost itself. Notably, the author clarifies that the relationships just mentioned "are weakest (or even reversed) in heavily regulated or government-dependent industries, and strongest in other industries" (Coates 2012, p. 688). Similarly, CPA is negatively associated with firm value, but this effect is "weakest (or even positive)" in those regulated or government-dependent industries (Coates 2012, p. 688). Worryingly, the study finds that after the *Citizens United* decision CPA increased most strongly among firms that are not in heavily regulated or government-dependent industries. As an example, while firms' PAC spending went up overall after *Citizens United*, the PAC spending of firms in heavily regulated industries declined. The author finds this trend to be indicative of an increase in the problematic, agency cost type of CPA after *Citizens United*. To that end, he shows that:

> Firms that were politically active in 2008 experienced an average 8 percent lower increase in their industry-relative shareholder value from their crisis-era lows when compared to firms that were politically inactive in 2008, consistent with *Citizens United* inducing an increase in unobservable political activity by previously politically active firms, with a significant attendant drag on shareholder value. (Coates 2012, p. 688)

In another study concerning corporate political spending as an agency problem, Aggarwal, Meschke, and Wang (2012, p. 0) "find virtually no support for the hypothesis that donations represent an investment in political capital." This study differs from most studies in the literature by examining corporate soft money donations and corporate donations to §527 organizations, both of which came directly from corporate treasuries. As can be seen in this review, most of the corporate political spending literature relies on firms' PAC spending and/or the personal political spending of executives. However, the *Citizens United* decision allows corporations to spend freely on politics from their own funds, so Aggarwal et al. (2012) choose to study the political spending that most closely resembles the spending which is now essentially unlimited after *Citizens United*. They find that firms' political spending is associated with lower stock returns, and "this reduction in shareholder value far outstrips the dollar value of the donations" (Aggarwal et al. 2012, p. 1). Regarding the underlying mechanism for this value destruction, the study shows that firms that engage in these types of political spending have lower R&D and investment spending, despite having more free cash flow, and are more likely to acquire other firms. Importantly, the acquisitions made by these firms "have significantly lower cumulative abnormal announcement returns than non-donating firms," suggesting that these acquisitions are questionable from a shareholder value perspective (Aggarwal et al. 2012, p. 0). Furthermore, higher levels of political spending are correlated with poorer corporate governance, but the difference in corporate governance does not fully explain the relationship between firms' political spending and lower stock returns. That is, corporate political spending appears to have a negative effect on firms' stock returns that is separate from the effect of the governance problems which may be facilitating the spending. (Aggarwal et al. 2012).

Finally, Hadani and Schuler (2013) obtain results similar to the main findings of Coates (2012) but they do so in a broader sample of 943 firms from the S&P 1500 for which complete financial data were available for the years 1998-2008. Using a factor the authors call "corporate political investments," developed from an exploratory factor analysis of firms' lobbying expenses, PAC spending, soft money expenses, and donations to §527 groups, Hadani and Schuler (2013, p. 165) show that firms' corporate political investments in a given year "are negatively associated with market performance and cumulative political investments [sum of a firm's corporate political investments in prior years and the current year] worsen both market and accounting performance." However, consistent with the findings in Coates (2012), these results do not hold for firms in regulated industries. In fact, cumulative corporate political investments are significantly positively associated with firm value for firms in regulated industries. The authors express surprise over their main findings, having predicted positive associations ex-ante, and conclude that agency theory provides the best explanation of their results (Hadani and Schuler 2013).

2.2.3 Conclusion

Clearly, the literature on the determinants of corporate political spending is conflicted. Probably the only conclusion that almost all researchers would agree upon is that corporate political spending may be beneficial, and thus likely strategic, for firms in regulated industries. Outside of that, it may be important to note some differences in these studies. Cooper et al. (2010) and Brown et al. (2015) both use measures of the number of candidates supported as their main proxies for corporate political spending, and they both find significant benefits to firms who support more candidates. None of the studies that find significant negative effects of corporate political spending consider how many candidates a firm supported. Additionally, Brown et al. (2015) and Akey (2015) both examine limited settings in which one may be more likely to see a positive effect of corporate political spending. Brown et al. (2015) only consider spending that supported candidates for the tax-writing committees of the U.S. Congress. Akey (2015) only examines political spending that supported candidates in competitive races where the margin of victory was less than 5%. In contrast, Coates (2012), Aggarwal et al. (2012), and Hadani and Schuler (2013) all examine relatively broad samples without many restrictions. Of note, the only major subsample analyses in Coates (2012) and Hadani and Schuler (2013) are those of firms in regulated industries, and both studies find that the negative effects of corporate political spending do not hold for those firms. Thus, much of the apparent disagreement between these studies may be resolved with future research considering the measures used and settings examined.

As to the debate over whether corporate political spending is a strategic investment or a wasteful agency problem, the evidence in aggregate would seem to indicate that it may be either depending on the situation. That is, some firms are clearly using political spending to their advantage (Cooper et al. 2010, Correia 2014, Akey 2015, Brown et al. 2015) while other firms are spending to their detriment (Aggarwal et al. 2012, Coates 2012, Hadani and Schuler 2013). However, an explanation that seems to be underappreciated in this literature, as I do not recall seeing it in much detail, is that some managers may be better than others at implementing corporate political spending as an investment and, perhaps, most managers are quite bad at it. This explanation could reconcile the differences mentioned above; good political spending managers may know to support a large number of candidates and to target candidates who are on particular committees or who are facing hotly-contested elections. Conversely, bad political spending managers may be more inclined to spend more freely but without a coherent plan, which could help to explain the generally negative relationships documented between amounts of political spending and firm value. This would be especially true if there were a correlation between being a bad political spending manager and poor managerial ability in general. As such, it may be proper to conclude that corporate political spending is motivated by either well-intentioned strategy or self-serving managerial discretion, but even well-intentioned strategy may not yield the desired benefits.

2.3 POLITICAL INFLUENCES ON INVESTING

I will now discuss the existing literature as it pertains to political considerations in investor decision-making. While not directly addressing corporate political spending, a number of studies have shown that individual investors, fund managers, and analysts are influenced by their own politics and/or by the politics of firms' management in their investment considerations. Starting with individual investors, Kaustia and Torstila (2011, p. 98) show that at least in Finland, "left-wing voters and politicians are less likely to invest in stocks." Their results indicate that compared to a member of a moderate right party a member of a moderate left party is 17% to 20% less likely to participate in the stock market. This effect is seen consistently across four different data sets, one of which consists of members of parliament who share similarly high levels of income and education. Kaustia and Torstila (2011, p. 99) attribute this "stock market aversion" effect to the "generalized antipathy towards capital markets" that is often associated with left-wing politics. They specify that the results are consistent with a "value-expressive hypothesis" wherein a left-wing individual applies his political values to his economic choices (p. 99). The authors note that this makes sense in the framework provided by Fama and French (2007, p. 668) where investors may have "tastes for assets as consumption goods" independent of the assets' potential economic payoffs. Finally, the authors also rule out alternative explanations including, "wealth effects, risk aversion, reverse causality, return expectations, safety net expectations, social capital, or trust" (Kaustia and Torstila 2011, p. 110).

Another study involving individual investors was performed by Bonaparte and Kumar (2013). The study finds that political activism in general is linked to a higher likelihood of stock market participation. That is, "politically active individuals are 9-25% more likely to participate in the stock market" (Bonaparte and Kumar 2013, p. 760). Individuals are classified as politically active if they are registered voters who report participating in most elections. The finding holds regardless of the individual's personal politics, and the authors theorize that the effect is due to a lowered cost of information gathering. They note that politically active people spend more time following the news, roughly 30 minutes more per day, than those who are not politically active, and they infer that, "This interest in following political news has positive spillover effects on their efforts to gather stock market-related news, which increases their probability of participating in the stock market" (Bonaparte and Kumar 2013, p. 762). Furthermore, the study uses instrumental variables to test for the proposed causality and concludes that "greater political activism *causes* higher participation rates" (Bonaparte and Kumar 2013, p. 780). This relationship appears in multiple samples from the United States and also seems to hold in Europe when comparing country-level data.

Perhaps most interesting and potentially concerning regarding individual investors' consideration of politics is a study showing that investors are "more optimistic and perceive markets to be less risky and more undervalued when their preferred party is in power" (Bonaparte, Kumar, and Page 2017, p. 69). Drawing on two national surveys and brokerage data from a large discount broker in the US, the authors first demonstrate that investors' attitudes toward the economy shift when there is a change in the party in power. Specifically, investors "become more optimistic about the stock market and the overall economy" when their party comes to power and become less optimistic when the party they oppose comes to power (p. 71). The decline in optimism is stronger for Democrats when Republicans come to power, but the authors note that this could be an artifact of asymmetry in their sample because it covers the later years of the Clinton administration, during which Republicans controlled Congress, and the George W. Bush administration when Republicans controlled both the White House and Congress (Bonaparte et al.

2017). The authors go on to show that investors also make real changes to their investments when a different party comes to power. During periods wherein their preferred party is in control, investors increase their exposures to systematic risk by investing more of their wealth in stocks, bonds, and mutual funds. Additionally, investors whose party is in power shift their portfolios to include more stocks with higher volatility as measured by market beta (Bonaparte et al. 2017). This shift toward higher risk investments does earn investors whose party is in power higher raw returns and higher market-adjusted returns. However, the study finds that, "the improvement in risk-adjusted performance is economically small" (Bonaparte et al. 2017, p. 92).

Individual investors' politics thus play a role in whether and how they invest. However, the effects of political considerations on investment are not limited to non-professionals. Chin and Parwada (2009) find that institutional fund managers are also swayed by their political preferences. Comparing the portfolios of money managers who gave contributions primarily to Democrats versus those who gave primarily to Republicans in the lead-up to the 2000 US Presidential election, the authors find that fund managers favor stocks that are perceived to have better prospects if their chosen candidate wins the election. That is, Democrat fund managers overweighted the stocks of companies widely perceived to benefit under a Gore presidency, and Republican fund managers overweighted the stocks of companies widely perceived to benefit under a Bush presidency (Chin and Parwada 2009). Interestingly, "politically-motivated trades perform significantly better than non-politically driven trades" (p. 6) during the period studied, and "post sell returns are positive, indicating both successful profit taking and forgone value from selling too early" (Chin and Parwada 2009, p. 29). This means that overweighting due to political preferences was actually beneficial for both Democrat and Republican money managers during the run-up to the 2000 election. Framing their study as examining "whether real world investors take into account the stock market effects of Presidential Election cycles," Chin and Parwada argue that the overweighting was strategic and provide some evidence that money managers adjusted their portfolios across quarters in tune to new information about their preferred candidate's chances of winning (2009, p. 28). However, the authors do not explicitly address why these Democrat and Republican money managers only exploited the politically-motivated trades affiliated with their preferred candidates. If the overweighting were purely strategic, money managers either failed to realize that politically-motivated trades would outperform on both sides or they were averse to investing in stocks perceived to do well under the candidate they opposed.

In another study of fund managers, Democrat fund managers are found to underweight companies in the "politically sensitive industries" of tobacco, guns and defense, and natural resources, as well as companies with low scores on a measure of corporate social responsibility (Hong and Kostovetsky 2012, p. 3). Although Democrat managers are also more likely to run socially responsible investment (SRI) funds, the authors exclude SRI funds from their analyses, so their results reflect a tendency by Democrat fund managers to underweight "socially irresponsible" stocks despite not having any mandate to do so (Hong and Kostovetsky 2012, p. 18). The study does not make a strong assertion as to why Democrat fund managers avoid these stocks, but it does note that there is a negligible difference in fund performance between Democrat and Republican managers. Also, the underweighting by Democrat managers is not only in mutual funds but is also seen in a sample of hedge funds where managers are more likely to have a substantial portion of their own wealth invested in their funds and receive performance-based fees. The implication is that, at the least, Democrat managers do not appear to be sacrificing returns to avoid the "socially irresponsible" stocks (Hong and Kostovetsky 2012, p. 3). Overall, the magnitude of the underweighting of these stocks by Democrat managers in non-SRI funds is greater than half of the underweighting seen in SRI funds. The authors suggest this could result in price effects, contrary to popular belief: “Considering that many professional managers are already practicing ‘closet SRI,’ it is unlikely that they will provide the contrarian positions needed to stabilize prices in markets” (Hong and Kostovetsky 2012, p. 19).

Chin and Parwada (2009) and Hong and Kostovetsky (2012) both show that professional investment managers exhibit preferences for or against certain kinds of firms depending on their personal political preferences. More recent literature suggests that investment managers and analysts also exhibit preferences for and against firms based on the politics of the firms’ managers. That is, investment professionals’ judgments about firms are influenced by the match or mismatch between their own political preferences and the political preferences of the firms’ management (Jannati, Kumar, Niessen-Ruenzi, and Wolfers 2016; Wintoki and Xi 2017). Wintoki and Xi (2017, p. 0) find that, “mutual fund managers are more likely to allocate assets to firms managed by executives and directors with whom they share a similar political partisan affiliation.” This result still holds when controlling for the underweighting by Democrat fund managers of politically sensitive industries identified in Hong and Kostovetsky (2012). Wintoki and Xi (2017) attribute the effect to in-group favoritism by ruling out two alternative explanations. First, prior literature has shown that information can flow to investment professionals through their social ties with firms’ directors (Cohen, Frazzini, and Malloy 2008; Akbas, Meschke, and Wintoki 2016). However, if this were the reason that fund managers overweight the stocks of companies run by their politically-affiliated peers, the fund managers should benefit from the information flow and experience higher returns as a result. On the contrary, Wintoki and Xi (2017, p. 5) find that fund managers who allocate more of their portfolios to these “politically similar firms” underperform fund managers who do not. Second, the authors speculate that their effect could be due to the "familiarity" of fund managers with executives who share their political beliefs (p. 3). Essentially, they could simply be investing more in the firms managed by people whom they know. Wintoki and Xi (2017) leverage the finding from Bonaparte et al. (2017) to rule out this explanation. If the overallocation effect were due to familiarity with executives rather than in-group favoritism based on political affiliation, Wintoki and Xi (2017) argue that the effect should not vary when political power changes hands because the familiarity that has already been established would not change. Nonetheless, they find that mutual fund managers were more likely to over-allocate to the firms of politically similar executives when their party held the presidency. This holds true for both Democrat and Republican fund managers. Thus, Wintoki and Xi (2017) conclude that it is likely that the overallocation effect is due to in-group favoritism wherein fund managers perceive executives who share their political preferences to be of higher ability than others. They also note that this "partisan bias" is similar in effect size to other known investor biases, including the home state bias shown in Pool, Stoffman, and Yonker (2012) (Wintoki and Xi 2017, p. 6).

While Wintoki and Xi (2017) document political in-group favoritism in fund managers, Jannati et al. (2016) document a very similar effect in equity analysts. They find that analysts have in-group biases "based on gender, ethnicity, and political attitudes" (Jannati et al. 2016, p. 0). Male analysts expect lower earnings from companies with female CEOs, and domestic analysts expect lower earnings from companies with foreign CEOs. Most pertinent to the present discussion though, "earnings forecasts of Republican analysts are lower for firms headed by Democrat CEOs" (Jannati et al. 2016, p. 0). Because most equity analysts are male, domestic, and Republican, these in-group biases have a systematic effect on consensus earnings forecasts and lead to significantly larger positive earnings surprises for firms with female or foreign CEOs and significantly smaller negative earnings surprises for firms with female, foreign, or Democrat CEOs. Firms with

Democrat CEOs also appear to have larger positive earnings surprises, but that effect is not statistically significant at conventional levels. Similarly, analysts, overall, give significantly fewer buy recommendations and significantly more sell recommendations for firms with female CEOs or foreign CEOs. This pattern in stock recommendations is directionally the same for Republican analysts evaluating firms with Democrat CEOs, but it is not statistically significant at conventional levels. Nonetheless, the results for Republican analysts regarding firms of Democrat CEOs in Jannati et al. (2016) are consistent with the in-group favoritism effect seen in fund managers in Wintoki and Xi (2017).

Taken together, the literature regarding political considerations in investor decision-making tells us that individuals' political preferences influence whether and how they invest. Studies have shown that individual investors are less likely to invest in stocks if they vote for left-wing political parties (Kaustia and Torstila 2011), more likely to invest in stocks if they vote regularly (Bonaparte and Kumar 2013), and likely to invest more in financial assets and shift to riskier stocks when their preferred political party is in power (Bonaparte et al. 2017). Professional investors are also influenced by their political preferences. Fund managers invest more in companies perceived to do well if their preferred Presidential candidate wins an election (Chin and Parwada 2009). Democrat fund managers shy away from companies in politically sensitive industries and companies that have low corporate social responsibility ratings (Hong and Kostovetsky 2012). Fund managers invest more in firms whose executives and directors are politically aligned with them (Wintoki and Xi 2017). Republican equity analysts give lower earnings estimates to firms led by Democrat CEOs (Jannati et al. 2016). The reasons behind these effects are not yet entirely clear, but suggested reasons include the expression of personal values (Kaustia and Torstila 2011), optimism tied to political outcomes (Chin and Parwada 2009, Bonaparte et al. 2017), and in-group bias (Jannati et al. 2016, Wintoki and Xi 2017). My study adds to this literature by identifying another situation in which political considerations influence investor decision-making and by attempting to shed additional light on the reasons behind that influence.

2.4 DISCLOSURE AND POLITICAL SPENDING

2.4.1 Theory Regarding Mandatory Versus Voluntary Disclosure

It is not clear how financial statement users may respond to a disclosure differently whether it is mandatory versus voluntary. There is a substantial literature about disclosure in economics and accounting (Beyer et al. 2010). Yet, few studies pertain to differences in investors' responses to information that is disclosed by mandate or by choice. We know that firms generally have incentives to disclose their privately-held information (Akerlof 1970) and that many models predict the "unraveling result" in which firms voluntarily disclose all their private information (Grossman and Hart 1980, Grossman 1981, Milgrom 1981, Milgrom and Roberts 1986). However, these models rely on simplifying assumptions that are not always valid in practice. Two in particular seem unlikely to hold with respect to corporate political spending information. First, the disclosure models predicting the unraveling result presume that, "all investors interpret the firms' disclosure in the same way and firms know how investors will interpret that disclosure" (Beyer et al. 2010, p. 300-301). Politics being as divisive as they are, it is hard to fathom that almost any disclosure of corporate political spending would be interpreted identically across all investors and even less plausible that firms would be able to anticipate investors' interpretation with certainty.

Second, the disclosure models predicting the unraveling result presume that, "disclosures are costless" (Beyer et al. 2010, p. 300). Although the actions necessary to disclose corporate political spending information are inexpensive, the disclosure itself may cause a firm to incur costs as has been seen in politically-motivated customer boycotts of Target and L.L. Bean (Montopoli 2010, Victor 2017).

Violating the assumption about uniformity of investors' interpretations lead to different predictions about firms' disclosure policy. If managers believe that investors have their own private information, managers could have difficulty predicting what investors' responses to disclosure will be (Beyer et al. 2010). Dutta and Trueman (2002) create a model in which investors have private information that affects how they will interpret management's disclosure. Managers must rely on their beliefs about what investors' interpretations of the disclosure will be, and thus the unraveling result no longer holds. This could map to the corporate political spending setting in which all investors presumably hold private information about their own political preferences. As such, managers would need to assess investors' politics in deciding what to disclose about their firm's political spending, and this could explain why we do not see the unraveling result in practice with regard to the disclosure of corporate political spending.

Similarly, violating the assumption that disclosure is costless can also lead to managers being selective about their disclosures (Verrecchia 1983, Jorgensen and Kirschenheiter 2003). This may partly explain why we do not see the unraveling result in the disclosure of corporate political spending. As mentioned before, there is anecdotal evidence that political spending associated with a firm can trigger customer boycotts (Montopoli 2010, Victor 2017), and as mentioned directly above, it is also unlikely that investors would respond to corporate political spending information uniformly. It seems plausible that managers consider these potential costs when deciding what to disclose about their firms' political spending.

As Koonce, Seybert, and Smith (2011) point out, there is good reason to believe that investors may interpret disclosures differently depending on whether they are mandatory or voluntary. Correspondent inference theory specifies that, "individuals typically rely on three factors–choice, expectations, and intent," in deciding how to attribute the cause of a single outcome (Koonce et al. 2011, p. 213). The choice component could matter in how investors interpret disclosures because "behavior that is freely chosen is generally attributed to the person more than if that same behavior was coerced" (Koonce et al. 2011, p. 213). This means that investors might attribute the content of a voluntary disclosure to company managers more than if it were part of a mandatory disclosure. That is, a voluntary disclosure may be interpreted as expressing information about management's preferences or beliefs, whereas a mandatory disclosure is less likely to be interpreted that way.

This potential difference in interpretation is also predicted by signaling theory in economics. Spence (1973) introduces the idea that an individual may choose to undertake a costly action just to signal his value to a potential employer. In the Spence setting, the costly action is acquiring an education, but the notion could generalize to other settings. Bhattacharya & Ritter (1983) expand on the signaling model to show that firms may be willing to voluntarily disclose private information even when doing so is costly to their competitive position in order to secure financing from investors. What follows naturally from these models is that costly disclosure choices convey information beyond the information contained in the disclosure. In Spence (1973), the choice to obtain an education does not just convey that the individual now has an education but that the individual was more capable of getting one relative to his competitors. In Bhattacharya and Ritter (1983), the choice to disclose R&D information does not just convey that the company has that technological know-how but that management believes the company can follow through on developing it to a point that will justify having disclosed the information publicly, enabling competitors, to obtain financing. Essentially, the disclosure choice is information about the discloser in addition to whatever information is contained in the voluntary disclosure. Contrasting this with a mandatory disclosure where the discloser must reveal a particular set of information, a voluntary disclosure by its nature can convey information about the discloser that cannot be inferred from a mandatory disclosure.

2.4.2 Empirical Work on Corporate Political Spending Disclosure

I now discuss the literature as it pertains directly to the disclosure of corporate political spending. This literature is relatively nascent because prior to the *Citizens United* ruling in 2010 corporations in the US were generally prohibited from using their own funds for political purposes and, to my knowledge, all corporate political spending that did occur prior to *Citizens United*, either with corporate funds or with executive/employee funds, was subject to FEC and/or IRS reporting requirements. As such, U.S. corporations have only had the legal capacity to spend anonymously on political activity for about seven years. I exclude law review articles and other legal or political essays from this section as I am most interested in relating what has been discovered empirically about corporate political spending disclosure.

Baloria, Klassen, and Wiedman (2015) examine the disclosure of corporate political spending in the broader context of shareholder proposals for voluntary disclosures. They note that, "there are virtually no firms initiating disclosure of political spending in the absence of a shareholder proposal" (Baloria et al. 2015, p. 2). Using "a sample of 541 proposals submitted to

S&P 500 firms between 2004 and 2012," the authors find that about 20% of proposals get implemented, but that most proposals that get implemented have been withdrawn prior to vote (Baloria et al. 2015, p. 2). That is, among proposals that go up for a shareholder vote, only 8% are implemented, while 56% of proposals that have been withdrawn are subsequently implemented. The authors provide evidence suggesting that withdrawn proposals are the results of negotiations with firms' management. Furthermore, the study shows that activist investors may have different reasons for seeking disclosure. Pension funds are significantly more likely to submit disclosure proposals to firms that have a higher proportion of their PAC spending directed to Republicans, and pension funds do not appear to specifically target firms with signs of agency problems. All other activist investors are significantly more likely to target firms with signs of agency problems, but they do not appear to consider the firms' political ideologies. Interestingly, the authors provide evidence that both firms' management and investors are aware of these different motivations. Proposals brought by pension funds are less likely to be withdrawn before a vote, and thus they are also less likely to be implemented. This implies that firms' management are less likely to negotiate with pension fund activists compared to other activists. Moreover, "investors respond negatively, on average, to firms announcing the implementation of political spending-related proposals. In a cross-sectional analysis, we find that investor reaction is positively related to the extent of firms' agency problems and negatively related to the existence of a pension fund proposal sponsor" (Baloria et al. 2015, p. 4). So, shareholder activism is at least somewhat effective in bringing about voluntary disclosure of corporate political spending, especially at the firms that may be suffering from agency problems. Also, investors' reactions suggest that they pay attention to why the voluntary disclosure has been initiated and respond accordingly.

In another study, DeBoskey, Li, Lobo, and Luo (2017) examine the transparency of firms' corporate political disclosure (CPD) and their effect on firms' cost of debt. They use the CPA-Zicklin index, a measure of firms' disclosure transparency about their "overall political activities, including spending, policies, and oversight" (DeBoskey et al. 2017, p. 4). The authors find that more transparent disclosure about firms' political activities, including political spending, is associated with lower costs of debt. The effect is "more pronounced for firms that are more sensitive to government economic policy, have entrenched CEOs, and are smaller" (DeBoskey et al. 2017, p. 5). The relationship remains significant even when controlling for measures of financial reporting quality and other non-financial disclosure quality. According to the authors, "This suggests that CPD represents a distinct dimension of a firm's non-financial disclosure that reduces creditors' political risk uncertainty" (DeBoskey et al. 2017, p. 6). They also suggest that their findings could be because more transparent CPD leads to reduced agency costs by increasing accountability for corporate political spending. (DeBoskey et al. 2017)

Finally, Prabhat and Primo (2017) examine the mandatory disclosure of corporate political spending that has been law in the United Kingdom since 2000. The Political Parties, Elections, and Referendums Act 2000 (PPERA) requires disclosure of corporate political spending and also requires shareholder approval before spending occurs. The authors find that politically active firms now have greater stock return volatility than before the mandate, and they find that while politically active firms did not immediately lose value after the announcement of the new policy, they have lost value in the ensuing years. Prabhat and Primo (2017) interpret their findings as an indictment of the idea that mandatory disclosure of corporate political spending is beneficial to shareholders. In discussing the US proponents of such a law, they note that these activists focus too much on the potential agency costs of corporate political spending and fail to recognize that "the preferences of some shareholders may not be aligned with the goal of maximizing shareholder value" (Prabhat and Primo 2017, p. 20). They suggest that the law in the UK may have changed managers' behavior regarding useful political spending and as such "the firm may be less adept at responding to political threats" (Prabhat and Primo 2017, p. 21).

Synthesizing a coherent narrative from these studies may at first seem difficult, but at least two findings seem consistent here. First, some activist investors are seeking disclosure for political reasons. Baloria et al. (2015) find that pension funds target Republican-leaning firms, and Prabhat and Primo (2017) suggests that politically-driven activists may be scaring managers in the UK away from beneficial political spending. Second, both Baloria et al. (2015) and Prabhat and Primo (2017) find some evidence that equity markets generally respond negatively to the disclosure of corporate political spending. This finding would seem to align well with the studies supporting benefits of corporate political spending, presuming that disclosure discourages such spending. However, the finding in DeBoskey et al. (2017) is curious in this context, suggesting that creditors appreciate increased political spending disclosure while the other studies suggest that investors do not. Further research in this area could disentangle the factors underlying these effects. One possibility that comes to mind is that creditors are much more sensitive to the potential agency costs of corporate political spending, which could increase credit risk, and much less sensitive to the potential benefits of corporate political spending, which can increase returns for investors but are unlikely to affect returns for creditors.

3.0 STUDY OF HOW INVESTORS REACT TO CORPORATE POLITICAL SPENDING DISCLOSURE

3.1 INTRODUCTION

Do investors care about the political spending of the firms in which they invest? This is the primary question behind the ongoing debate regarding whether public companies should be required to disclose their political spending. In its *Citizens United* decision in 2010, the United States Supreme Court ruled that corporations and unions could spend unlimited amounts on political advertising. Although this allows corporations to openly sponsor political advertising, it also allows corporations to instead fund certain §501(c) groups that then sponsor political advertising with no requirement to disclose the original funding source (Palmer and Phillip 2012). These groups are often referred to as "dark money" groups because they allow corporations to influence political outcomes without their shareholders or the public knowing how much they are spending in support of particular candidates or causes.[1]

As a result, some argue that the SEC should require corporations to disclose their political spending as a matter of investor protection, arguing that investors should be informed of corporations' political activity (Bebchuk and Jackson 2013). However, others argue that investors "will likely not find this additional information useful," partly because the amount of political spending by any individual firm is unlikely to be financially material (Bainbridge et al. 2012, p.

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[1] The specific §501(c) groups that can be used for this purpose are defined by the Internal Revenue Code and include §501(c)(4) Social Welfare Organizations, §501(c)(5) Labor and Agricultural Organizations, and §501(c)(6) Business Leagues. Further information on why these groups are referred to as "dark money" can be found at https://www.opensecrets.org/outsidespending/nonprof_summ.php.

1). Thus, the main purpose of my study is to provide empirical evidence for regulators on whether investors find corporate political spending information useful and use it in their investment decisions.

I conduct an experiment to examine whether and how investors use financially immaterial disclosed corporate political spending information when making investment decisions and whether it matters if the disclosure is mandatory rather than voluntary as it is currently. Further, I examine whether investors' use of disclosed political spending information depends upon their beliefs regarding the effect of the disclosed spending on the firm's financial performance. I predict and find that investors whose personal political identities are aligned with a company's disclosed political spending assess the company as more attractive as an investment and invest more in the company than investors whose political identities are misaligned with the political spending. Interestingly, this finding is primarily driven by the negative reactions of misaligned investors. Misaligned investors assess the company as significantly less attractive as an investment and invest significantly less in the company than investors in a control condition with no political spending disclosure. However, aligned investors' attractiveness assessments and investment amounts are not significantly different from those in the control condition. This suggests that any disclosure of corporate political spending may lead to a negative overall effect on investment in a company and may explain why business groups have opposed disclosure.

I also provide evidence that investors use disclosed political spending information consciously rather than because of a subconscious bias and that some investors use the information in their investment decisions even though they do not believe the disclosure will affect the firm's financial performance while others use it because they expect financial implications. That is, some investors appear to simply have investing tastes (Fama and French 2007) directed against political spending that conflicts with their personal political identities, while other investors appear to believe that political spending that conflicts with their personal political identities portends poorer financial performance. Finally, I find no significant effects of mandatory versus voluntary disclosure, suggesting that mandating disclosure of corporate political spending would not change how investors use it.

My findings provide evidence for regulators regarding whether to require corporations to disclose their political spending. The finding that investors *consciously* use disclosed corporate political spending information when making investment decisions indicates that they consider such information useful. The finding that some investors use the information without even considering whether there could be financial implications suggests that such investment decisions are based on taste rather than any potential financial effects and that financially immaterial political spending can still affect investors' choices. Combining these findings with the additional finding that investors use the information in the same way regardless of whether the firm provides it voluntarily or in a required disclosure suggests that investors could benefit from mandatory disclosure of corporate political spending.

My study also extends the finance literature on the effect of "tastes" for assets on investment decisions (Fama and French 2007). Although prior studies show that individual investors become more optimistic when their preferred party is in power (Bonaparte et al. 2017) and money managers invest in specific industries or types of companies based on their political preferences (Chin and Parwada 2009, Hong and Kostovetsky 2012), my study is the first to document that the nature of the disclosed political spending of an individual firm can affect how investors view that firm as an investment. Furthermore, I provide evidence that investors use political spending information in different ways, with some investors forming expectations of financial implications for the firm and other investors making decisions simply by their tastes.

Finally, my study extends prior results showing that investment decisions are more affected by negative political considerations than by positive political considerations. Consistent with Hong and Kostovetsky, who find that Democrat money managers make less investment in “socially irresponsible” firms, and with Kaustia and Torstila (2011), who find that left-wing voters and politicians in Finland are less likely to participate in the stock market based on their negative views of capitalism, I find that investors whose political identities are misaligned with the political spending of a company assess that company as a less attractive investment and invest less in the company. Further, I find that this negative reaction to political spending that conflicts with their personal political identities holds not only for investors with progressive political identities, as seen in prior studies, but also for investors with conservative political identities.

The next section provides background on corporate political spending and its disclosure. Section III develops my hypotheses and research question, and Section IV describes my research method. Section V presents my results, and Section VI concludes the paper with a discussion of the implications of my findings.

3.2 BACKGROUND

In the majority opinion of the *Citizens United* case, Justice Kennedy wrote, “With the advent of the Internet, prompt disclosure of expenditures can provide shareholders and citizens with the information needed to hold corporations and elected officials accountable for their positions and supporters.” This suggests that the majority of the Supreme Court anticipated that corporations would disclose information about their newly allowed political spending. However, such disclosure has not been forthcoming from most companies (Blumenthal 2015).

Although companies may now sponsor political advertising directly and publicly, most choose to spend on elections using either independent expenditure-only committees, better known as Super PACs, or using certain not-for-profit groups falling under §501(c) of the Internal Revenue Code. In either case, the companies themselves are not required to disclose their political spending. However, funds given to Super PACs are ultimately disclosed by the Super PAC pursuant to Federal Election Commission (FEC) regulations.[2] For example, in 2010, Target gave $150,000 to a conservative Super PAC in Minnesota supporting an anti-gay marriage gubernatorial candidate and then faced a boycott by gay rights activists after its contribution was disclosed by the Super PAC (Torres-Spelliscy 2010).

In contrast to Super PACs, the dark money §501(c) groups are not subject to the same FEC regulations and thus are not required to publicly disclose their donors if political action is not their "primary activity."[3] In the 2012 election cycle, spending by these dark money groups and by groups that received considerable support from dark money groups exceeded $300 million and accounted for almost 30% of outside spending (Center for Responsive Politics 2016b).[4] Using such dark money groups corporations can influence political outcomes without their shareholders or the public knowing how much they are spending to support particular candidates or causes.

[2] Super PACs must disclose their donors, so all money flowing into a Super PAC can be traced at least one level back. However, §501(c) groups may contribute to Super PACs without disclosing their donors. In such a case, the §501(c) group would be disclosed as a donor by the Super PAC, but the original source of the contributed funds would remain unknown to the public.

[3] An explanation of the IRS's policies on §501(c)(4), §501(c)(5), and §501(c)(6) organizations regarding political activity can be found at https://www.irs.gov/pub/irs-tege/eotopicl03.pdf.

[4] Outside spending includes "political expenditures made by groups or individuals independently of, and not coordinated with, candidates' committees" (Center for Responsive Politics 2016a).

In response to the concern about dark money, a petition to the SEC in 2011 by a committee of ten prominent legal scholars proposed that the agency should require disclosure of corporate political spending, arguing that investors should be aware of corporations' political activity (Bebchuk and Jackson 2013). The petition has drawn the most comment letters in the history of the SEC with over 1.2 million, "an overwhelming majority" of which support the petition (Bebchuk and Jackson 2015).[5] The petition is also supported by many prominent individuals, including former SEC chairmen William Donaldson and Arthur Levitt.[6]

Conversely, others argue that requiring disclosure of political spending falls outside the SEC's mission and is unnecessary for investor protection. In a comment letter on the petition to the SEC, a group of scholars led by Stephen Bainbridge of UCLA offers several arguments opposing the creation of a rule mandating disclosure. Among other things, these scholars contend that investors "will likely not find this additional information useful," noting that political spending amounts are unlikely to be financially material for most companies (Bainbridge et al. 2012, p. 1).[7]

To date, the SEC has not passed any rules requiring disclosure of corporate political spending. After former SEC chairwoman Mary Jo White was pressured by House Republicans to dismiss the petition requesting mandatory disclosure of corporate political spending, the SEC removed the issue from its agenda in late 2013 (ElBoghdady 2013a, ElBoghdady 2013b). Furthermore, the federal budget for 2016 contained a provision precluding the SEC from issuing

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[5] All the comment letters are available at: https://www.sec.gov/comments/4-637/4-637.shtml
[6] Their comment letter can be seen at: https://www.sec.gov/comments/4-637/4637-3105.pdf
[7] Bainbridge et al. (2012) also raise other objections to the proposed disclosure rule. These include assertions that corporate political spending is already disclosed in other ways, that a rule mandating disclosure would "burden political expression," and that making such a rule would destroy the SEC's nonpartisan integrity. Bebchuk and Jackson (2013) respond to many of these objections and argue that they are invalid based on matters of institutional or legal fact.

any rule during 2016 that would require the disclosure of corporate political spending (Bebchuk and Jackson 2015).

Therefore, disclosure of corporate political spending currently remains voluntary. Baloria et al. (2015) document a number of cases wherein shareholder proposals have led companies to disclose their political spending. Some other companies may elect to disclose the information without such prompting. In either case, it is difficult to determine how complete these disclosures are.[8] Given that disclosure is not required in the United States, there are no reliable archival data available domestically to examine how investors react to mandatory versus voluntary disclosure of corporate political spending. The UK does have a law mandating disclosure of corporate political spending, and there is evidence that investors reacted negatively to the passage of that law (Prabhat and Primo 2017). However, I have not seen any studies specifically examining investor reaction with UK firms that previously disclosed voluntarily, so it is hard to say if mandatory disclosure changed how investors reacted to corporate political spending. Certainly, the implication in Prabhat and Primo (2017) is simply that investors found politically active firms to be less valuable once they had to disclose their spending, which could be due to an assumption that the firms will no longer be able to curry political favor as well as they did prior. So, prior to issuing any rule on the disclosure of corporate political spending, it would be helpful for the SEC and other standard setters to better understand whether investors find such disclosures useful.

[8] Some information about individual companies' spending on dark money groups may be found at http://politicalaccountability.net/what-does-your-company-spend. However, many companies' disclosures are limited to policies regarding such spending and/or the portion of such spending that is specifically allocated to lobbying expenses and is therefore non-deductible for tax purposes. As such, the full amounts spent on dark money groups are rarely disclosed.

3.3 DEVELOPMENT OF HYPOTHESES AND RESEARCH QUESTION

I rely on prior findings in the finance literature to predict how the disclosure of corporate political spending is likely to affect investor behavior. First, prior theoretical research suggests that investors' decisions do not necessarily depend only on their prospective payoffs and the associated risks. Rather, some investors appear to exhibit "tastes" for assets as though they were consumption goods, and these tastes can create lasting price effects in equity markets (Fama & French 2007). Second, some evidence suggests that investors could have tastes based on their political identities. Kaustia and Torstila (2011) provide evidence that left-wing voters and politicians in Finland are less likely than others to participate in the stock market. They interpret this as "value-expressive" behavior, in which left-leaning individuals have more negative views of the capitalist system and thus choose not to invest in stocks.

Similarly, Chin and Parwada (2009) and Hong and Kostovetsky (2012) find that fund managers make investment decisions based on their party affiliations. Chin and Parwada (2009) use fund holdings data during the U.S. presidential election in 2000 to show that fund managers traded stocks in accordance with their party affiliations. That is, fund managers who were Republican (Democrat) overweighted the stocks of companies that were expected to perform better under a Bush (Gore) presidency. Hong and Kostovetsky (2012) find that fund managers who make more contributions to the political campaigns of Democrats than those of Republicans invest smaller portions of their funds in firms within industries like tobacco and gun-making that are likely viewed as socially irresponsible by Democrats. Additionally, these Democrat-supporting managers tend to avoid investing in firms with low scores on corporate social responsibility.

Alternatively, rather than demonstrating investors' political tastes, the findings described above could also result from investors holding different financial expectations because of their politics. There is evidence that investors in the U.S. are more optimistic about financial markets when their favored party is in power (Bonaparte et al. 2017). Unlike the findings above that could be the result of political tastes, the effect seen in Bonaparte et al. is clearly not a taste for an asset but a difference in expected financial outcomes. Investors whose party is in power tend to have lower perceptions of risk and higher confidence in the financial markets and the economy. Such perceptions lead these investors to allocate higher amounts to risky assets during periods when their party is in power (Bonaparte et al. 2017). This suggests another reason in addition to political tastes for why investors' political identities could influence their judgments. If a company's management directs its corporate political spending in a manner with which investors agree, the investors may genuinely expect the company to have a better financial outcome. Because it is hard to tell in much of the existing literature whether tastes or financial expectations are responsible for the influence of politics on investor behavior, I designed my study to identify which of these reasons drives investors' reactions to corporate political spending.

My first hypothesis follows from the results described above showing that tastes and/or financial expectations related to political identities can cause investors to favor some investments over others. Specifically, if investors can have tastes that affect how they value firms (Fama and French 2007) and if those tastes are related to their political identities (Chin and Parwada 2009, Kaustia and Torstila 2011, Hong & Kostovetsky 2012), investors are likely to value firms differently depending on the type of political spending they disclose. Likewise, if investors form different financial expectations based on their political identities (Bonaparte et al. 2017), they also are likely to value firms differently depending on the type of political spending they disclose. That is, an investor could value a firm more positively or negatively depending on whether its political spending is aligned or misaligned with the investor's personal political identity. Therefore, my first hypothesis is:

H1: Investors will value firms that disclose political spending that is aligned with the investors' political identities higher than firms that disclose political spending that is misaligned with their political identities.

Additionally, I hypothesize that investors will *consciously* behave as predicted in H1. I base this hypothesis on two arguments. First, the existence of socially-responsible investment (SRI) funds indicates that some investors consciously select firms based on personal tastes. Such investors purposefully buy shares in firms whose business activities and practices are not in conflict with their personal values. Although these SRI funds are not specifically political in nature, some of the issues that determine how they invest, such as firms' carbon emissions, are politically charged. Second, the petition to the SEC requesting it require the disclosure of corporate political spending information has been supported by some investors who express concern that corporate political spending could run counter to their personal views.[9] This shows that at least some investors explicitly consider a company's political spending when making investment decisions.

Alternatively, investors may not be fully aware of their politically-derived tastes and/or financial expectations and could subconsciously value firms differently based on the firms' disclosed political spending. Although this is possible, it appears unlikely given the recognition by some investors that firms could have political spending with which they disagree and would act against if given the opportunity. This leads to my second hypothesis:

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[9] As an example, in a comment letter dated May 6, 2015, "Brian Arbogast, et al." state, "Shareholders cannot determine whether corporate political expenditures are supporting individuals or groups that engage in advocacy on other issues to which they object, and therefore cannot exercise their ownership rights by attempting to restrict such spending or by selling their stakes in the company." https://www.sec.gov/comments/4-637/4637-3020.pdf

H2: Investors will *consciously* value firms that disclose political spending that is aligned with the investors' political identities higher than firms that disclose political spending that is misaligned with their political identities.

The hypotheses above make general predictions that investors will use corporate political spending information when making investment decisions. However, they do not address whether investors might use such information differently if the disclosure is mandatory rather than voluntary. Mandatory versus voluntary disclosure of corporate political spending information may not affect how investors use such information if investors find it equally salient in both cases. That is, if investors believe such information is relevant for their decisions, they would be expected to use it whether the company disclosed it voluntarily or because it was required to disclose it under reporting standards.

Alternatively, mandatory disclosure could cause investors to view the political spending information as more important or less important, depending on their perceptions of required disclosures. Some investors could believe that any information that is required to be disclosed must be important or disclosure wouldn't be required. For such investors, mandating disclosure could cause them to weight the information more heavily than they otherwise would. Conversely, some investors could believe required disclosures are provided simply to fulfill a requirement and therefore may not be very important. For such investors, mandating disclosure may have little, if any, effect on their investment decisions.

The potential influence of voluntary disclosure on investors' use of corporate political spending information could be similarly complicated. Some investors could believe that any information that a company voluntarily discloses must be an important signal because the company is going beyond its legal duty in providing information. This would be consistent with theory related to signaling in the economic literature (Spencer 1973, Bhattacharya and Ritter 1983). This would especially apply to political spending information because its divisive nature could subject the company to criticism and potential costs. Investors who believe voluntarily disclosed political spending is a signal would weigh it more heavily. However, investors could alternatively believe that a company disclosing political spending information voluntarily has such uncontroversial spending that it is not risking potential criticisms and costs. In that case, investors would not perceive the spending as a signal and would not weigh it more heavily.

Because it is difficult to predict whether investors will use political spending information differently when provided voluntarily as compared to when it is a required disclosure, I test the following research question:

RQ: How, if at all, does mandatory versus voluntary disclosure affect investors' reactions to corporate political spending information?

3.4 METHOD

3.4.1 Participants

Prior research has supported the recruitment of non-professional investors through Amazon Mechanical Turk (mTurk) and has shown that these participants behave similarly to other non-professional investors (Rennekamp 2012, Owens 2014, Krische 2015). As such, I recruited 162 non-professional investor participants via mTurk, restricting participation to those located in the United States with an approval rating on previous tasks of at least 95%. Participants were paid $2.25 each and took an average of slightly less than 19 minutes to complete the study.

To limit the incentive for participants to misidentify themselves as investors, I did not screen for this attribute prior to participation in the study. Instead, I included a question in the post-experimental questionnaire related to investing experience and identified non-professional investors by using their responses to this question. Of 331 participants who completed the study in accordance with the requirements, 162 (49%) report having invested in stocks. These 162 participants are the non-professional investors (hereafter, referred to simply as investors) that I use in my analyses.[10]

Of the 162 investor participants, 153 (94%) indicate they have read financial statements, 144 (89%) report currently holding investments, and 91 (56.2%) report that their current investment holdings exceed $10,000. Further, participants' average age is 38 years; 65% are male; 59% hold a Bachelor's degree or higher; and 99% report being U.S. citizens (all but two who are non-citizen residents), suggesting that they should be familiar with, and interested in, American politics.[11]

3.4.2 Design

I use a 1 x 3 experimental design to test H1 and H2. I collect both between-participants and within-participant data for the three conditions. The three conditions are Aligned, Misaligned, and Control. In the Aligned condition, investors' political identities are aligned with the company's disclosed political spending. In the Misaligned condition, investors' political identities are misaligned with the company's disclosed political spending. In the Control condition, no political spending information is disclosed. To form the Aligned and Misaligned conditions, I manipulate

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[10] 350 participants completed the study, but 19 of these participants were removed because they had re-entered the study after either answering the manipulation check question incorrectly or failing to answer it at all.
[11] Using pre-screens available on Amazon Mechanical Turk, I restricted the availability of my study to U.S. residents. I believe citizens and non-citizen residents are likely to be familiar with and interested in American politics. Regardless, if any of my participants lacks an interest in American politics, this should only serve to bias against finding support for my predictions.

the type of political spending by the company (Progressive versus Conservative) and measure the political identities of the investors (Progressive versus Conservative). Investors are included in the Aligned condition when their measured political identity is aligned with the manipulated disclosed political spending of the company; they are included in the Misaligned condition when their measured political identity is not aligned with the disclosed political spending of the company. Next, I describe the procedures related to the conditions used to test H1 and H2. This is followed by a separate section that describes the additional conditions and procedures used to test my RQ.

3.4.3 Procedures related to H1 and H2

I conduct my study using the Qualtrics survey platform. First, investors are randomized into one of three corporate political spending disclosure manipulations: progressive spending, conservative spending, or no spending disclosure.[12] These manipulations allow me to form my 1 x 3 design with the Aligned, Misaligned, and Control conditions. A summary of these conditions is shown in Panel A of Table 1.

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[12] The progressive spending and conservative spending manipulations are also randomly treated with one of three disclosure manipulations. The Mandatory and Voluntary disclosure manipulations are achieved by adding an introductory clause to the disclosure explicitly stating that it is or is not required. The Unspecified disclosure manipulation does not have an introductory clause.

Table 1. Variable Names and Descriptions

Panel A: Primary Independent Variable (Conditions)

Name	Description
Aligned	Investor's political identity is aligned with company's political spending
Misaligned	Investor's political identity is **not** aligned with company's political spending
Control	No disclosure of political spending

Panel B: Primary Dependent Variables[1,2]

Name	Description
$Attractiveness_{bt}$	Between-participant measure of the company's attractiveness as an investment on a scale of 1 (Very Unattractive) to 11 (Very Attractive)
$Investment_{bt}$	Between-participant measure of how much of a $1,000 industry allocation, in $100 increments starting from $0, an investor would choose to invest in the company
$Attractiveness_{C}$	Within-participant measure that is identical to $Attractiveness_{bt}$, except that investors are told to assume the company had not disclosed any political spending
$Investment_{C}$	Within-participant measure that is identical to $Investment_{bt}$, except that investors are told to assume the company had not disclosed any political spending
$Attractiveness_{wi}$	Within-participant measure that is identical to $Attractiveness_{bt}$, except that investors are told to assume political spending had been the opposite of what it was for $Attractiveness_{bt}$[3]
$Investment_{wi}$	Within-participant measure that is identical to $Investment_{bt}$, except that investors are told to assume political spending had been the opposite of what it was for $Investment_{bt}$[3]

[1] Investors in the between-participant Control condition only respond to $Attractiveness_{bt}$ and $Investment_{bt}$. They do not complete the other measures.

[2] Investors respond to $Attractiveness_{bt}$ and $Investment_{bt}$, then $Attractiveness_{C}$ and $Investment_{C}$, and finally $Attractiveness_{wi}$ and $Investment_{wi}$.

[3] Investors who initially saw the company spend on The Fund for a Progressive America (The Fund for a Conservative America) are told to respond to $Attractiveness_{wi}$ and $Investment_{wi}$ assuming that the company had instead spent on The Fund for a Conservative America (The Fund for a Progressive America).

Investors read a description of the company, Great Grocer, which includes information about its business strategy and financial performance, along with an income statement for the most recent year, and a disclosure of political spending information if applicable (see Appendix A for an example). Investors then provide responses for my primary, between-participant (designated by the $_{bt}$ subscript) dependent variables: $Attractiveness_{bt}$ and $Investment_{bt}$. For $Attractiveness_{bt}$, investors indicate their "assessment of the attractiveness of Great Grocer as an investment" on an 11-point scale with endpoints of "Very Unattractive"(1) and "Very Attractive"(11), and a midpoint of "Neither Attractive nor Unattractive." For $Investment_{bt}$, investors are given some assumptions about their hypothetical portfolio and the company's valuation and are then asked to indicate "how much of your $1,000 investment in this industry you would choose to invest in Great Grocer" on a scale from $0 to $1,000 in $100 increments. Panel B of Table 1 describes these variables and the other primary dependent variables in this study.

Investors who saw either type of political spending disclosures in the between-participants conditions (that is, those not in the between-participants Control condition) next move to the within-participant portion of the study by reading the following statement: "Thank you for your previous responses. Now, please respond to the following two questions assuming that Great Grocer had not disclosed any political spending." Investors then respond to the same two items they did earlier, giving a 1 to 11 assessment of attractiveness and a $0 to $1,000 choice for investment. These measures provide the within-participants control condition data, so I refer to them as $Attractiveness_{c}$ and $Investment_{c}$. Next, investors respond to the same two items one final time after being told to assume that Great Grocer disclosed political spending of the other type that they had not seen initially. Specifically, investors who initially saw the company spend on The Fund for a Progressive America (The Fund for a Conservative America) were now told to respond assuming that the company instead had political spending to The Fund for a Conservative America (The Fund for a Progressive America). I refer to these measures as $\text{Attractiveness}_{wi}$ and Investment_{wi}[13] Investors then answer questions regarding their perceptions of the effect of the firm's political spending on its future financial performance and on other investors' reactions.

To identify investors' political identities, which was necessary to classify them into the Aligned or Misaligned conditions, participants responded to the question, "If you decided to contribute to one of the two organizations mentioned in this study, which one would it be?" Investors chose between the Fund for a Progressive America and the Fund for a Conservative America. As explained earlier, the response to this question determines an investor's political identity as progressive or conservative which is then compared to the political spending they saw earlier (progressive or conservative) to classify them as Aligned or Misaligned with the types of spending they saw earlier.

Finally, all investors, including those in the between-participant Control condition, complete a post-experimental questionnaire answering questions regarding their impressions of corporate political spending, their political beliefs, and their demographic characteristics.

3.4.4 Additional conditions and procedures related to my RQ

To test my RQ, in addition to my primary conditions of Aligned, Misaligned, and Control, I also manipulate how political spending information is disclosed. Specifically, I vary a brief clause at

[13] $\text{Attractiveness}_{wi}$ and Investment_{wi} are the measures for the within-participant Misaligned (Aligned) conditions for investors who were in the Aligned (Misaligned) conditions when providing $\text{Attractiveness}_{bt}$ and Investment_{bt}. $\text{Attractiveness}_{bt}$ and Investment_{bt} are also included in the within-participant data such that every investor who was in the between-participants Aligned or Misaligned conditions has responses for the within-participant Aligned, Misaligned, and Control conditions.

the beginning of the political spending disclosures in the conditions where progressive spending or conservative spending is disclosed as mentioned above.[14] For Voluntary disclosure, the introductory clause "Although not required to do so by any reporting standard," is added before the disclosure (see Appendix A for an example of Voluntary disclosure). For Mandatory disclosure, the introductory clause "Citing a legal requirement to disclose its political spending," is added before the disclosure. For Unspecified disclosure, there is no introductory clause to indicate whether the disclosure was required or made voluntarily. This manipulation is not used in the between-participants Control condition because no political spending is disclosed.

The manipulation of the introductory clause creates the three disclosure conditions—Mandatory, Voluntary, and Unspecified—used to test my research question, which asks whether mandatory versus voluntary disclosure of corporate political spending differentially affects investors' use of such information. The introductory clause is the only difference across the Mandatory, Voluntary, and Unspecified conditions; all other aspects of the experiment are identical to those described earlier, and the Aligned and Misaligned conditions are formed within each of the Mandatory, Voluntary, and Unspecified conditions in the same manner as described earlier.

[14] As mentioned before, these spending conditions ultimately translate into the Aligned or Misaligned conditions, depending on the political identity of the individual investor.

3.5 RESULTS

Before describing the tests of my hypotheses, I briefly describe the main result relating to my research question, which asks whether voluntary versus mandatory disclosure differentially affects investors' use of disclosed corporate political spending information. As reported in more detail later, my results for H1 and H2 hold when controlling for voluntary versus mandatory disclosure.[15] Therefore, I combine the data from the Voluntary, Mandatory, and Unspecified disclosure conditions to test H1 and H2.

3.5.1 Hypothesis 1

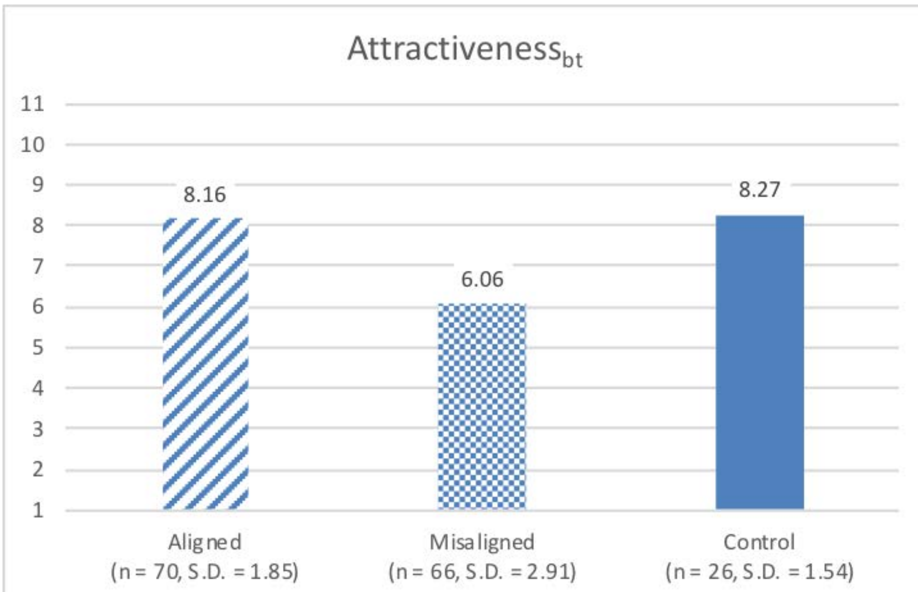
H1 predicts that investors will value a company higher as an investment when its political spending is aligned with the investors' political identities than when its political spending is misaligned with the investors' political identities. Panels A and B of Figure 1 show the means for the *between-participants* dependent variables $\text{Attractiveness}_{bt}$ and Investment_{bt} for the Aligned, Misaligned, and Control conditions. The pattern of these means provides initial support for H1. Panels A and B of Figure 2 show disaggregation of the means of these variables within the Aligned and Misaligned conditions by investors' political identities and the company's political spending. Notably, the disaggregated means do not differ significantly within the Aligned condition and within the Misaligned condition. That is, the pattern of means for both dependent variables is

[15] In untabulated analyses, I control for possible main effects of Voluntary and Mandatory disclosure as well as interaction effects of Voluntary X Alignment and Mandatory X Alignment. None of these effects is statistically significant.

unaffected by investors' political identities, ruling out concerns that support for H1 could be driven by the reactions of progressive investors only or conservative investors only.

Figure 1. **Means of Between-Participants Dependent Variables**

Panel A: Attractiveness



Panel B: Investment

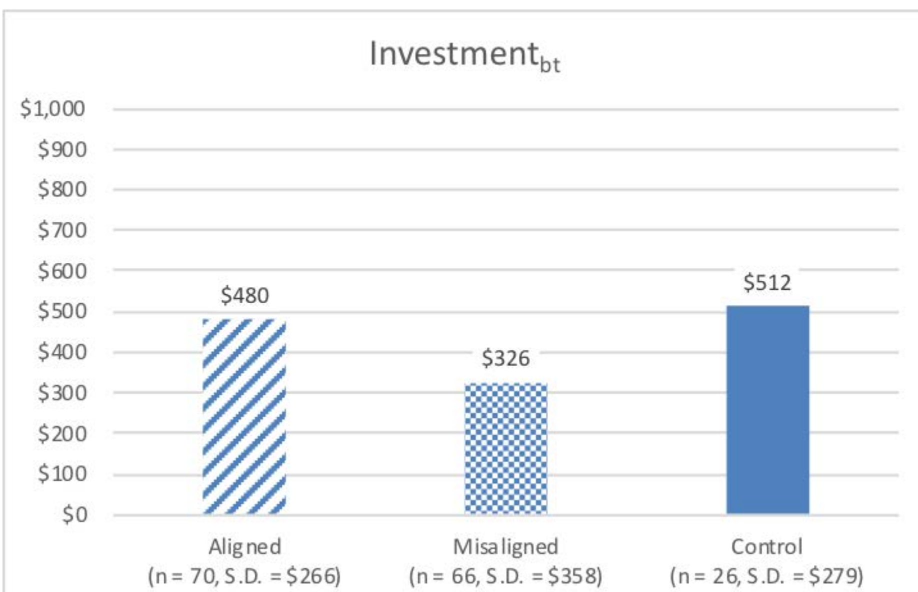
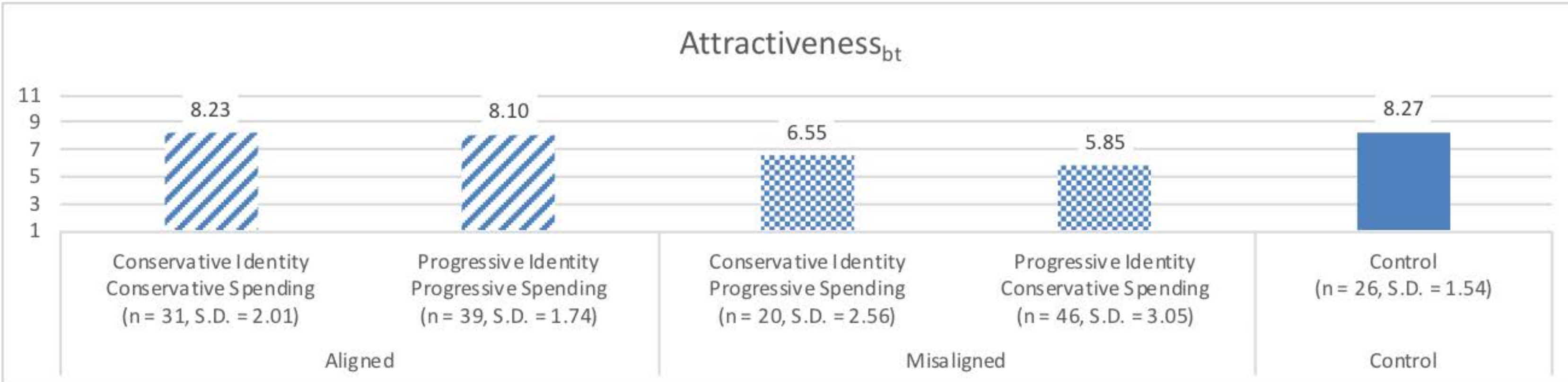
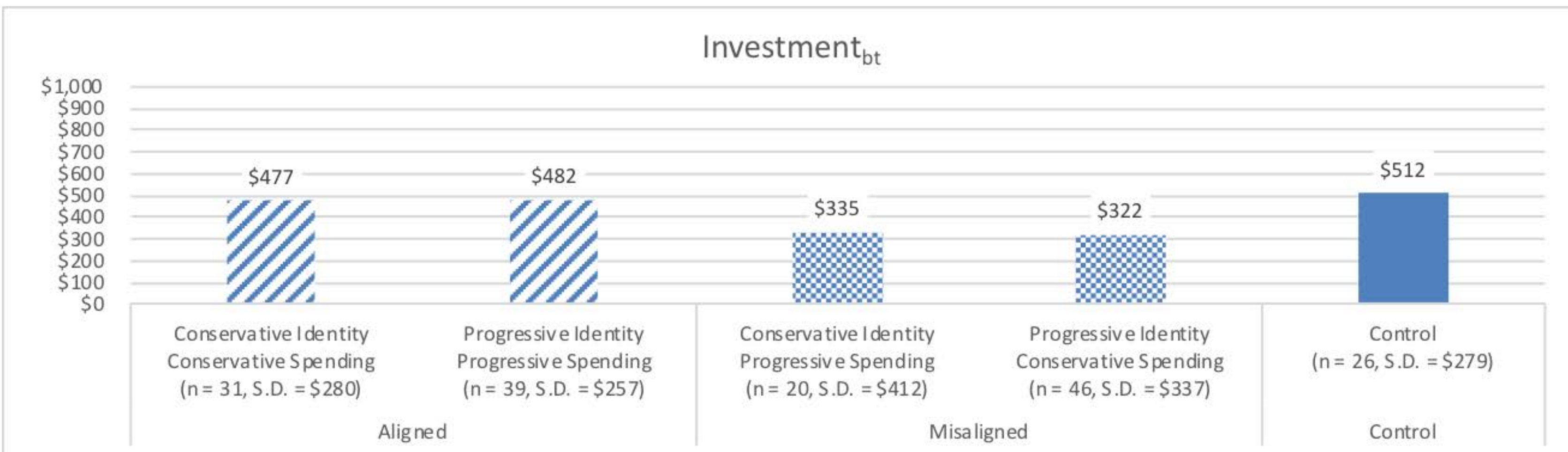


Figure 2. **Means of Between-Participants Dependent Variables by Political Identity and Spending Type**

Panel A: Attractiveness



Panel B: Investment



I begin my formal analysis by conducting separate 1 x 3 ANOVAs for $\text{Attractiveness}_{bt}$ and Investment_{bt} comparing across the between-participant Aligned, Misaligned, and Control conditions. As reported in Table 2, Panel A, the results show that there are significant differences across the three conditions for both $\text{Attractiveness}_{bt}$ ($F = 16.61$, $p < .0001$) and Investment_{bt} ($F = 5.54$, $p < .005$). To test H1, I conduct a planned pairwise comparison of the Aligned and Misaligned conditions for both $\text{Attractiveness}_{bt}$ and Investment_{bt}.[16] As reported in Table 2, Panel B, I find that the means for both $\text{Attractiveness}_{bt}$ and Investment_{bt} are significantly higher ($p < .001$ and $p = .012$, respectively) in the Aligned condition than in the Misaligned condition. Thus, consistent with H1, investors whose political identities are aligned with the company's political spending assessed the company as a more attractive investment and chose to invest more in the company compared to investors whose political identities are misaligned with the company's political spending.

[16] I report all pairwise comparison results using the Bonferroni correction.

Table 2. Analyses for Hypothesis 1

Panel A: Analyses of Variance

Dependent Variable	Source of Variation	Sum of Squares	d.f.	Mean Square	*F*-Statistic	*p*-value[a]
Attractiveness	*Condition*	177.16	2	88.58	16.61	< 0.0001
	Error	848.14	159	5.33		
Investment	*Condition*	1055249.42	2	527624.71	5.54	0.0047
	Error	15144750.60	159	95250.00		

Panel B: Planned Pairwise Comparisons

Compared Conditions	Mean Difference	*p*-value[a,b]
Aligned versus Misaligned		
Attractiveness	2.10	< 0.001
Investment	$154	0.012
Aligned versus Control		
Attractiveness	-0.11	1.00
Investment	-$32	1.00
Misaligned versus Control		
Attractiveness	-2.21	< 0.001
Investment	-$186	0.031

[a] All reported p-values are two-tailed.

[b] I report all pairwise comparison results using the Bonferroni correction.

Next, I examine whether the results for H1 are due to positive effects on investors' decisions in the Aligned condition, negative effects on investors' decisions in the Misaligned condition, or both. I conduct planned pairwise comparisons between the Aligned and Control conditions and between the Misaligned and Control conditions for both $\text{Attractiveness}_{bt}$ and Investment_{bt}. If investors' decisions are affected both positively by being politically aligned and negatively by being politically misaligned, I would expect that the means of both $\text{Attractiveness}_{bt}$ and Investment_{bt} in the Control condition would lie between those of the Aligned and Misaligned conditions. However, as can be seen in Figure 2, Panel A, the means of both $\text{Attractiveness}_{bt}$ and Investment_{bt} in the Control condition are very similar to those in the Aligned condition, but noticeably higher than those in the Misaligned condition.

The results of the pairwise comparisons reported in Table 2, Panel B, confirm the pattern noted above. That is, there are no significant differences for either $\text{Attractiveness}_{bt}$ ($p = 1.00$) or Investment_{bt} ($p = 1.00$) between the Aligned and Control conditions, but both $\text{Attractiveness}_{bt}$ ($p < .001$) and Investment_{bt} ($p = .031$) are significantly lower in the Misaligned condition than in the Control condition. These results show that the reason H1 is supported is that the attractiveness assessments and investment amounts are lower for investors whose political identities are misaligned with the company's political spending than for investors in the Control condition who were not provided with any political spending disclosure. That is, the effect of political spending disclosure is negative and only present among investors whose political identities are misaligned with the company's political spending.

3.5.2 Hypothesis 2

H2 predicts investors will *consciously* value a company higher when its political spending is aligned with the investors' political identities than when its political spending is misaligned with the investors' political identities. In other words, H2 predicts that the results reported for H1 reflect investors' conscious decisions rather than subconscious biases. If this is the case, the pattern of responses seen in the between-participants data supporting H1 should continue in the within-participant data. That is, investors should continue to make lower attractiveness assessments and invest smaller amounts when the company's political spending is misaligned with their political identities than when the company's political spending is aligned with their political identities or when no political spending is disclosed. These differences across conditions should persist because in the within-participant case each investor responds to all three conditions and can see clearly that only the type or presence of political spending changes across conditions. Therefore, if investors consciously use information about the company's political spending they will respond in the same way they did in the between-participants conditions.

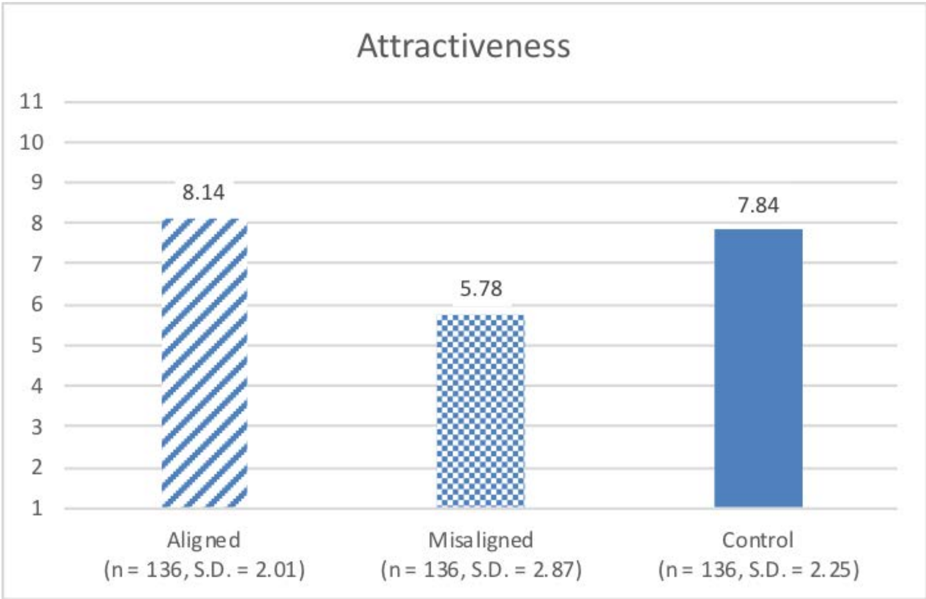
Figure 3 shows the means for the within-participant dependent variables, Attractiveness and Investment, for the Aligned, Misaligned, and Control conditions.[17] The pattern of results in Figure 3 for the within-participant data is the same as that for the corresponding Figure 1 for the between-participants data. Likewise, the pattern in Figure 4, which shows disaggregation of the means of the within-participant dependent variables by investors' political identities, is the same

[17] The within-participant dependent variables for the Aligned and Misaligned conditions include the between-participant dependent variables, $\text{Attractiveness}_{bt}$ and Investment_{bt}. To compose the Aligned and Misaligned conditions for each participant in the within-participant case, I must use participants' responses to $\text{Attractiveness}_{bt}$ and Investment_{bt} as well as $\text{Attractiveness}_{wi}$ and Investment_{wi}. The within-participant Control condition is comprised of participants' responses for $\text{Attractiveness}_{C}$ and Investment_{C}.

as that in the corresponding Figure 2 for the between-participants data. The fact that the patterns are the same for the between-participant and within-participant data suggests that the investors are consciously using the disclosed political spending information in their investment decisions.

Figure 3. **Means of Within-Participants Dependent Variables**

Panel A: Attractiveness



Panel B: Investment

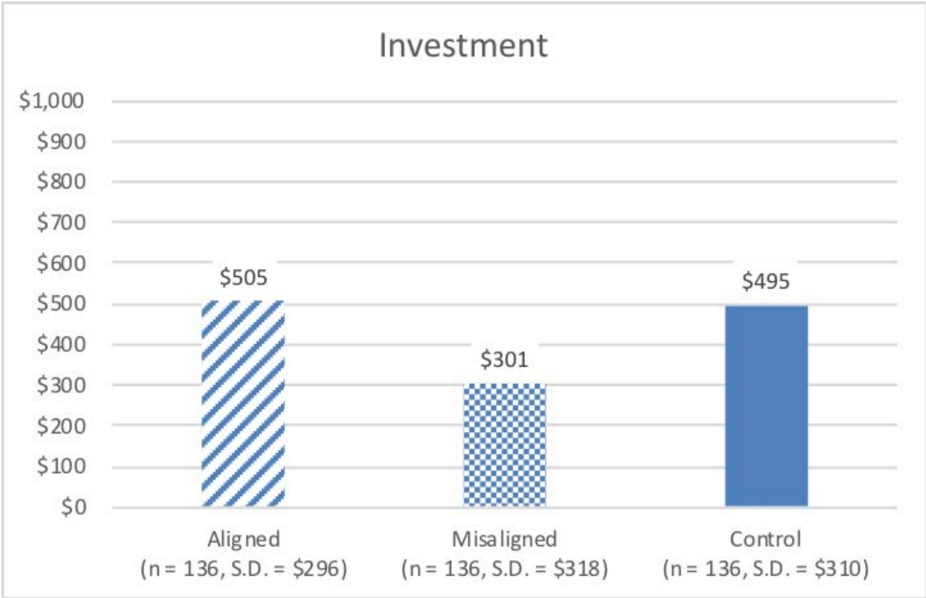
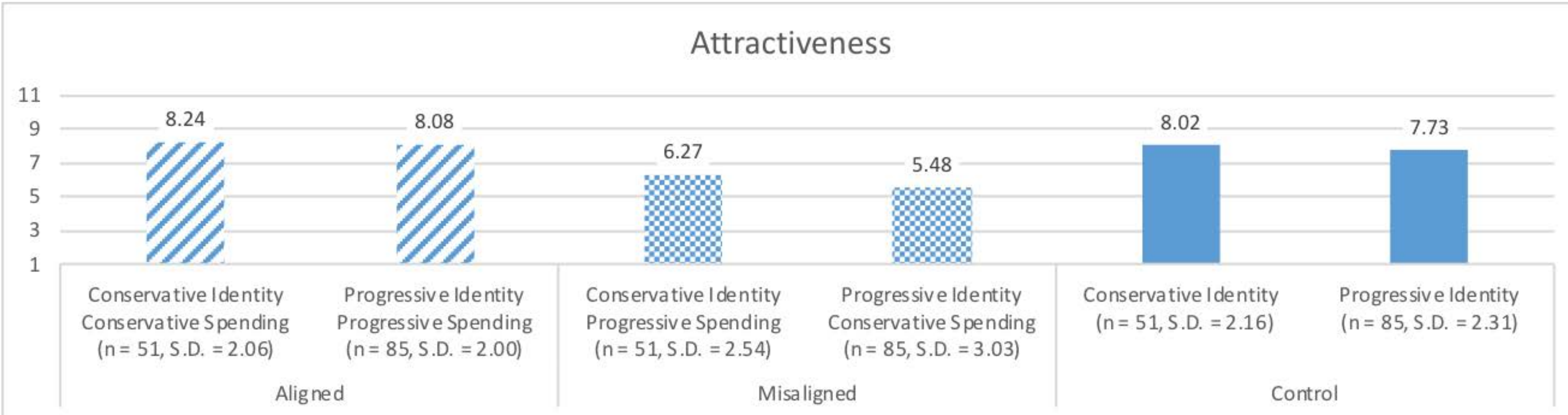
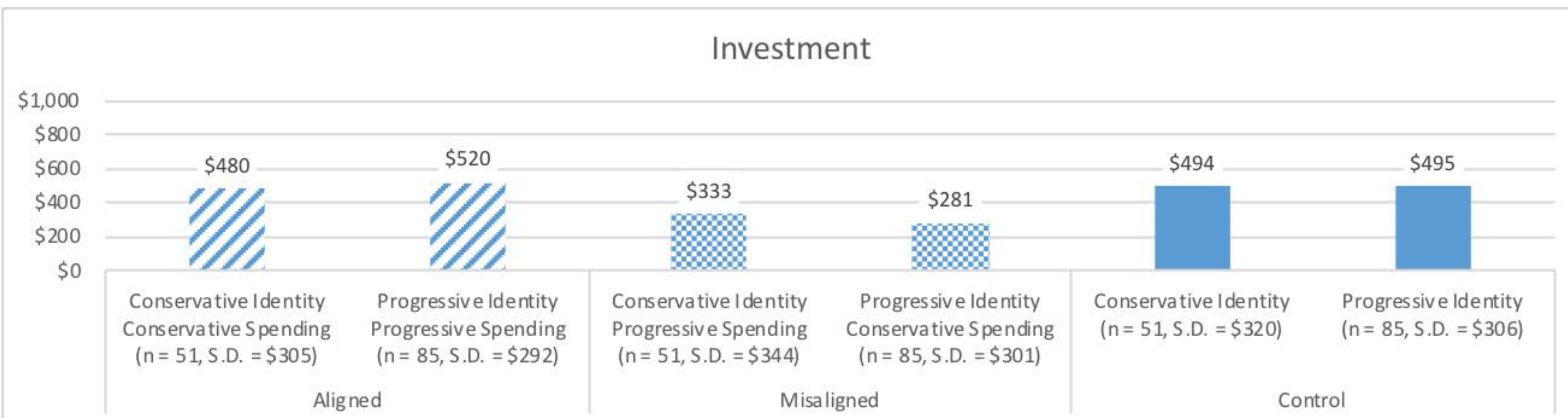


Figure 4. **Means of Within-Participants Dependent Variables by Political Identity and Spending Type**

Panel A: Attractiveness



Panel B: Investment



To formally test H2, I conduct the same analyses I used to test H1, except that now I use repeated-measures ANOVAs because I am testing within-participant data.[18] I again conduct separate 1 x 3 ANOVAs of Attractiveness and Investment comparing across the Aligned, Misaligned, and Control conditions. The results of the repeated measures ANOVAs reported in Table 3, Panel A, show that, as for the between-participant data, there are significant differences across the three conditions for both Attractiveness ($F = 68.9$, $p < .001$) and Investment ($F = 57.7$, $p < .001$).[19] Consistent with the between-participants results reported earlier for H1, a planned pairwise comparison reported in Table 3, Panel B, shows that the means for both Attractiveness ($p < .001$) and Investment ($p < .001$) are significantly higher in the Aligned condition than in the Misaligned condition. This suggests that, consistent with H2, investors *consciously* value a company higher when its political spending is aligned with the investors' political identities than when its political spending is misaligned with the investors' political identities.

Also, consistent with the between-participant results for H1, means for Attractiveness ($p = .164$) and Investment ($p = 1.00$) are not significantly different between the Aligned and Control conditions, and means for Attractiveness ($p < .001$) and Investment ($p < .001$) are significantly lower in the Misaligned condition than in the Control condition. Thus, all results for the within-participant data parallel the results for H1 using the between-participant data. These results are consistent with investors consciously using the company's political spending information when making their attractiveness assessments and choosing their investment amounts.

[18] The 26 participants in the between-participant Control condition were not asked to assess the company with political spending of any kind. Thus, only the 136 participants from the between-participant Aligned and Misaligned conditions are included in the within-participant analyses.

[19] The significance of these results does not differ when using various corrections for data that violates the assumption of sphericity, as mine does.

Table 3. **Analyses for Hypothesis 2**

Panel A: Repeated Measures Analyses of Variance

Dependent Variable	Source of Variation	Sum of Squares	d.f.	Mean Square	*F*-Statistic	*p*-value[a,b]
Attractiveness	*Condition*	448.83	2	224.41	68.92	< 0.001
	Error	879.17	270	3.26		
Investment	*Condition*	3607254.91	2	1803627.45	57.70	< 0.001
	Error	8439411.77	270	31257.08		

Panel B: Planned Pairwise Comparisons

Compared Conditions	Mean Difference	*p*-value[a,c]
Aligned versus Misaligned		
Attractiveness	2.36	< 0.001
Investment	$204	< 0.001
Aligned versus Control		
Attractiveness	0.30	0.164
Investment	$10	1.000
Misaligned versus Control		
Attractiveness	-2.06	< 0.001
Investment	-$194	< 0.001

[a] All reported p-values are two-tailed.

[b] Statistical significance does not change if Greenhouse-Geisser, Huynh-Feldt, or Lower-bound corrections are used.

[c] I report all pairwise comparison results using the Bonferroni correction

3.5.3 Research Question

My research question asks whether and how voluntary versus mandatory disclosure affects how investors use information about corporate political spending. As mentioned above, the manipulations I use to address this question do not have a statistically significant effect on my results for H1 and H2, so I report the previous analyses having collapsed across the disclosure conditions of Voluntary, Mandatory, and Unspecified.[20]

Figure 5, Panel A, shows the means of the between-participants dependent variables $\text{Attractiveness}_{bt}$ and Investment_{bt} for the Aligned and Misaligned conditions in each of the disclosure conditions of Voluntary, Mandatory, and Unspecified.[21] In each of the disclosure conditions, the pattern of means is consistent with H1. This is preliminary evidence that regardless of whether disclosure is voluntary, mandatory, or unspecified, investors whose political identities are aligned with the company's political spending value the company higher than investors whose political identities are not aligned with the company's political spending.

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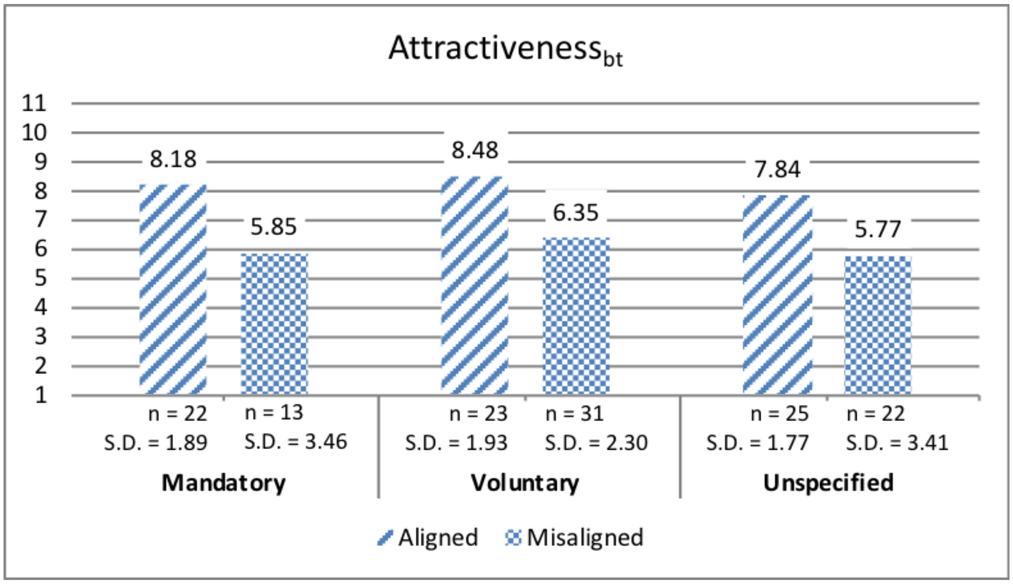
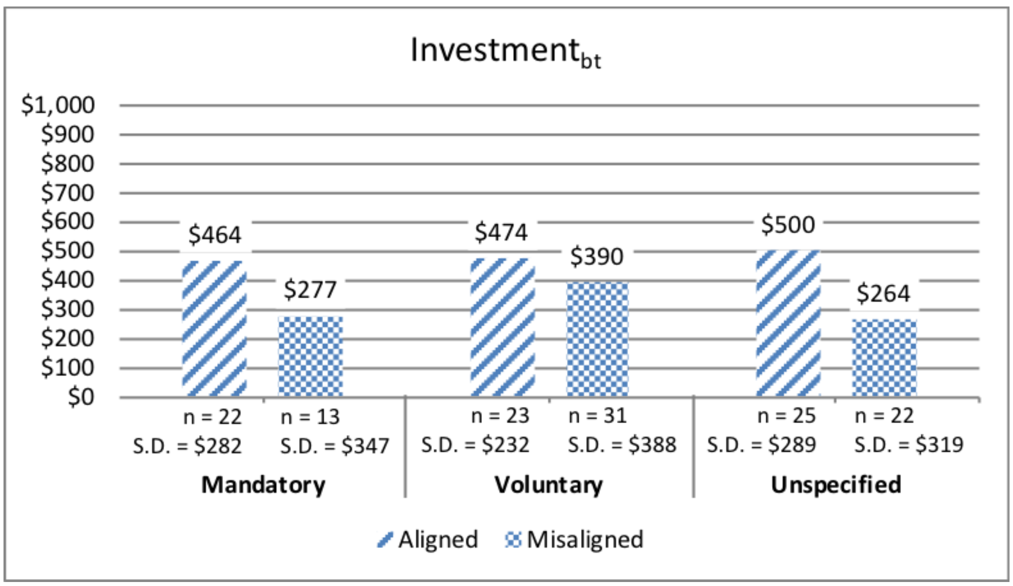
[20] In separate analyses (untabulated), I consider whether H1 and H2 hold within each disclosure condition independently. All results for H1 and H2 remain statistically significant and directionally consistent except for the results for H1 regarding Investment in the Voluntary condition. That result remains directionally consistent but is not statistically significant.

[21] The disclosure condition manipulations are not present in the between-participants Control condition because there was no disclosure of political spending.

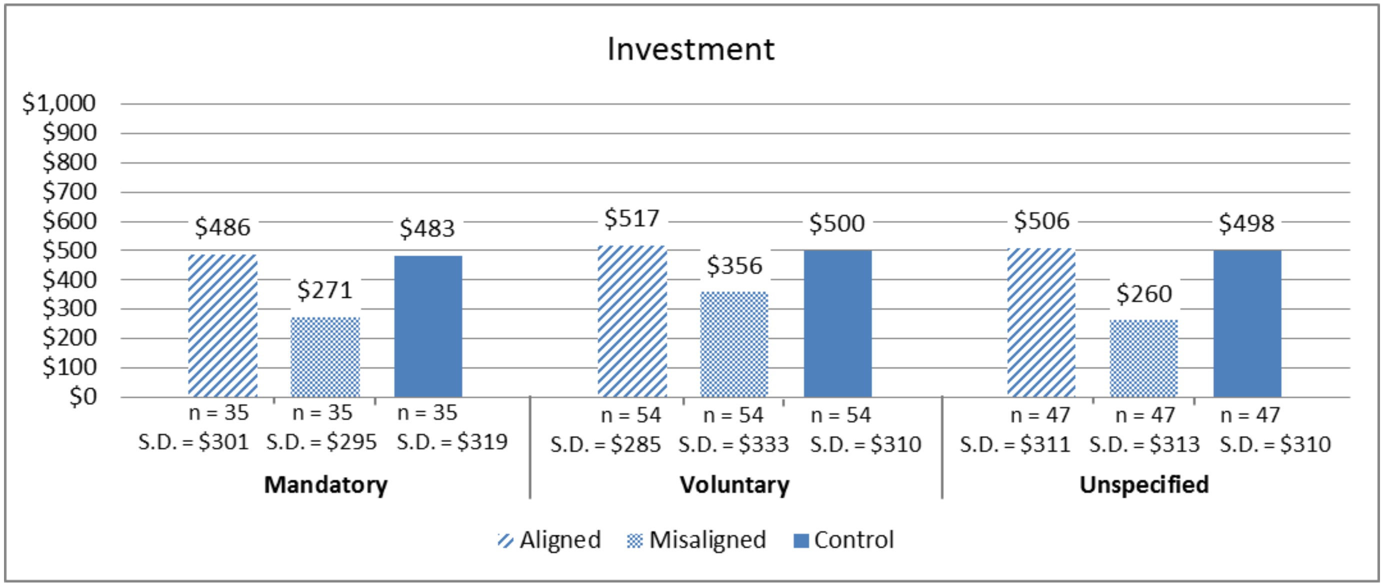
Figure 5. **Means by Disclosure Condition for Research Question**

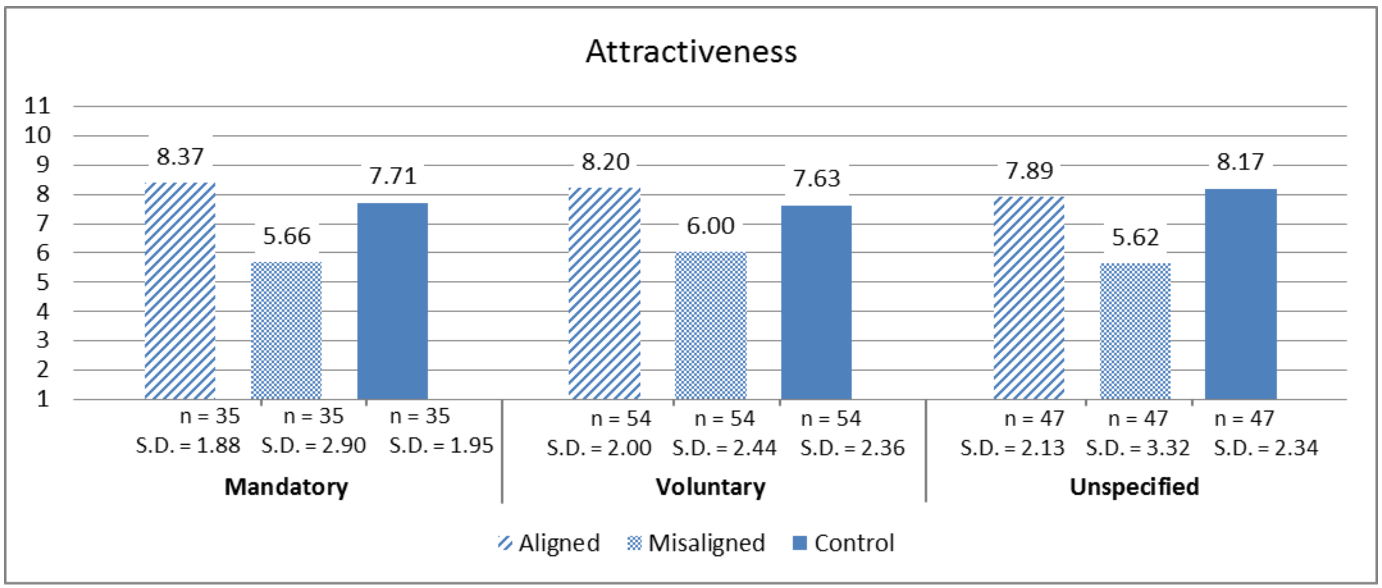
Panel A: Means of Between-Participant Dependent Variables





Panel B: Means of Within-Participant Dependent Variables





To formally test whether voluntary versus mandatory disclosure affects investors' use of corporate political spending information, I first conduct the same ANOVAs for $\text{Attractiveness}_{bt}$ and Investment_{bt} as I did in testing H1 except that I now include the disclosure conditions as another factor. Because these disclosure conditions were not present in the between-participants Control condition, where there was no disclosure of political spending, the Control condition is not included in the ANOVAs. Panels A and B of Table 4 show the results of these ANOVAs for $\text{Attractiveness}_{bt}$ and Investment_{bt}, respectively.

Table 4. Analyses for Hypothesis 1 Controlling for Disclosure Condition

Panel A: Analysis of Variance, Dependent Variable is $\text{Attractiveness}_{bt}$

Source of Variation	Sum of Squares	d.f.	Mean Square	*F*-Statistic	*p*-value[a]
Intercept	6361.86	1	6361.86	1061.64	< 0.001
Alignment	150.21	1	150.21	25.07	< 0.001
Disclosure	9.57	2	4.79	0.80	0.452
Alignment x Disclosure	0.37	2	0.19	0.03	0.970
Error	779.03	130	5.99		

Panel B: Analysis of Variance, Dependent Variable is Investment_{bt}

Source of Variation	Sum of Squares	d.f.	Mean Square	*F*-Statistic	*p*-value[a]
Intercept	19781702.00	1	19781702.00	198.79	< 0.001
Alignment	905290.42	1	905290.42	9.10	0.003
Disclosure	98705.98	2	49352.99	0.50	0.610
Alignment x Disclosure	150838.89	2	75419.44	0.76	0.471
Error	12936339.71	130	99510.31		

[a] All reported p-values are two-tailed.

Neither ANOVA shows a significant main effect nor a significant interaction effect of the disclosure conditions. For $Attractiveness_{bt}$, the difference between the Aligned and Misaligned conditions remains significant ($F = 25.07$, $p < .001$), while the main effect ($F = 0.80$, $p = .452$) and interaction effect ($F = .03$, $p = .970$) of the disclosure conditions are not significant. Likewise, for $Investment_{bt}$, the difference between the Aligned and Misaligned conditions remains significant ($F = 9.10$, $p < .01$), while the main effect ($F = 0.50$, $p = .610$) and interaction effect ($F = 0.76$, $p = .471$) of the disclosure conditions are not significant. These results suggest that voluntary versus mandatory disclosure does not affect how investors use corporate political spending information.

Figure 5, Panel B, shows the means of the within-participant dependent variables of Attractiveness and Investment across the within-participant Aligned, Misaligned, and Control conditions in each of the disclosure conditions. In each of the disclosure conditions, the pattern of means is consistent with H2. This is preliminary evidence that regardless of whether disclosure is voluntary, mandatory, or unspecified, investors whose political identities are aligned with the company's political spending *consciously* value the company higher than investors whose political identities are not aligned with the company's political spending.

To formally test whether voluntary versus mandatory disclosure affects whether investors consciously use corporate political spending information, I now conduct the same repeated-measures ANOVAs for the within-participant dependent variables of Attractiveness and Investment as I did in testing H2 except that I now include the disclosure conditions as a between-participants factor. Panels A and B of Table 5 show the results of these ANOVAs for Attractiveness and Investment, respectively.

Table 5. **Analyses for Hypothesis 2 Controlling for Disclosure Condition**

Panel A: Repeated Measures Analysis of Variance, Dependent Variable is Attractiveness

Source of Variation	Sum of Squares	d.f.	Mean Square	*F*-Statistic	*p*-value[a,b]
Aligned/Misaligned/Control (AMC)	446.66	2	223.33	68.92	< 0.001
AMC x Disclosure	17.21	4	4.30	1.33	0.260
Error	861.97	266	3.24		

Panel B: Repeated Measures Analysis of Variance, Dependent Variable is Investment

Source of Variation	Sum of Squares	d.f.	Mean Square	*F*-Statistic	*p*-value[a,b]
Aligned/Misaligned/Control (AMC)	3612705.01	2	1806352.50	57.92	< 0.001
AMC x Disclosure	142962.59	4	35740.65	1.15	0.335
Error	8296449.17	266	31189.66		

[a] All reported p-values are two-tailed.

[b] Statistical significance does not change if Greenhouse-Geisser, Huynh-Feldt, or Lower-bound corrections are used.

Neither repeated-measures ANOVA shows a significant interaction effect of the disclosure conditions with the Aligned, Misaligned, and Control conditions. For Attractiveness, the differences among the within-participant Aligned, Misaligned, and Control conditions remain significant ($F = 68.92$, $p < .001$), while the interaction effect ($F = 1.33$, $p = .260$) of the disclosure conditions is not significant. Likewise, for Investment, the differences among the Aligned, Misaligned, and Control conditions remain significant ($F = 57.92$, $p < .001$), while the interaction effect ($F = 1.15$, $p = .335$) of the disclosure conditions is not significant. Separate, untabulated tests of the between-participants effects of the disclosure conditions in the within-participant data also show non-significant effects on Attractiveness ($F = 0.01$, $p = .991$) and Investment ($F = 0.35$, $p = .706$). Taken together, these results suggest that voluntary versus mandatory disclosure does not affect whether investors consciously use corporate political spending information. In summary, I find no evidence that voluntary versus mandatory disclosure has an effect on how investors use corporate political spending information.

However, my manipulation of disclosure condition was subtle, and it is possible that investors did not consider whether the political spending information was being disclosed voluntarily or not. Unfortunately, I did not include a manipulation check for disclosure condition in my study, so it is impossible to tell whether investors noticed the introductory clauses in the disclosures indicating voluntary or mandatory disclosure. To address this concern, I have run a subsequent trial of my study using only the Voluntary and Mandatory disclosure conditions and including a manipulation check to determine whether participants notice the disclosure condition manipulation.[22] In a sample of 19, I find that 15 participants, or 79%, correctly identify whether the disclosure was required or not.[23] In this trial, I included both investors and non-investors because I am only concerned with whether they notice the disclosure condition. All four participants who answered the manipulation check question incorrectly indicated that they did not think information about whether the disclosure was required had been provided. Although the sample in this trial is small, it does show a majority of participants understood whether or not the disclosure was required after receiving the same information as the participants in the main study.

[22] All other aspects of my experimental design remain the same except that I omit the Unspecified disclosure condition from the trial because there is no mention of disclosure regime for participants to notice. I also omit the between-participants Control condition from the trial as there is no disclosure in that condition.

[23] The trial included 20 participants, but one was excluded for re-entering the study after failing the initial manipulation check question regarding spending type. This is consistent with the screening protocol I used in my main study.

3.5.4 Additional Analysis

In the development of H1, I explained that previously documented effects of investors' politics on their investment choices could be evidence of politically-derived tastes for assets or could be evidence of different financial expectations resulting from investors' personal political identities. To explore this issue, I designed my experiment to help sort out which of these underlying reasons drives my findings regarding investors' reactions to disclosed corporate political spending information.

Specifically, I asked participants if they had considered whether the disclosed political spending would affect the future financial performance of the company.[24] 60% (81/136) replied, "Yes;" 40% (55/136) replied, "No." Controlling for the response to this question in additional analyses for H1 and H2 does not change the significance of the predicted effects, and the control variable is not significant at conventional levels in any of the analyses (untabulated).[25,26] That is, H1 and H2 both hold regardless of whether participants considered any future financial impact of the disclosed political spending. This suggests that some investors acted only on their tastes regarding the political spending while others may have had differing financial expectations for the company because of its political spending.

To ascertain whether participants who responded, "Yes," to the question above actually held different financial expectations for the company based on its disclosed political spending, I

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[24] The 26 participants in the between-participants control condition were not asked this question because they did not see any disclosure of political spending.

[25] To re-test H1 with this question as a control variable, I ran the ANOVAs previously used for H1 but with an additional, dichotomous factor coded "0" for "No" and "1" for "Yes." As noted in footnote 21, the between-participants control condition did not have this question, so these ANOVAs only test the Aligned and Misaligned conditions against each other.

[26] To re-test H2 with this question as a control variable, I ran the repeated-measures ANOVAs previously used for H2 but with an additional, dichotomous between-participants factor coded "0" for "No" and "1" for "Yes."

asked those participants to provide their assessment of how they believed the future financial performance of the company would be affected when it disclosed political spending to The Fund for a Conservative America and when it disclosed political spending to The Fund for a Progressive America. Participants responded on an 11-point scale from "Very Negatively" (1) to "Very Positively" (11) with a midpoint of "No Effect" (6).

Interestingly, 17% (14) of the 81 participants who had said, "Yes," to the prior question indicated that they expected "No Effect."[27] Adding these participants to those who said, "No," to the prior question suggests that roughly half of the participants acted only on their tastes when valuing the company lower if it had political spending misaligned with their political identities. Nonetheless, the other half of participants indicate that they did expect an effect of the disclosed political spending on the future financial performance of the company.

3.6 CONCLUSION

Tests of my hypotheses provide evidence that investors consciously use disclosed corporate political spending information in their investment decisions. Specifically, investors whose political identities are not aligned with a company's political spending assess the company to be less attractive and invest less in the company than investors whose political identities are aligned with the company's political spending or who do not receive information about the company's political spending. Investors continue to use political spending information in this manner even after they

[27] These data are for the "Fund" participants saw first, between-participants. The data for this question regarding the "Fund" participants saw second, within-participant, is similar with 14% (11) of the 81 participants indicating "No Effect."

see that a change in the presence or type of political spending is the only change across scenarios, providing evidence that investors consciously use the information rather than using it unintentionally because of a subconscious bias. My results also provide evidence that investors' use of political spending disclosures in their decisions is unaffected by whether the disclosure is voluntary or mandatory.

Further, my results suggest that investors use disclosed corporate political spending information in different ways. Some investors expect that the political spending will have financial implications for the company, while other investors do not consider any financial impact of the spending but use the information in their decisions nonetheless. Thus, it appears that the negative effect of being misaligned with a company's political spending could stem from different rationales in different investors, with some perceiving an economic rationale for their choices and others acting only on their tastes.

These findings provide evidence for regulators regarding whether to require corporations to disclose their political spending. The finding that investors *consciously* use disclosed corporate political spending information when making investment decisions indicates that they consider such information useful. Combining this finding with the additional finding that investors use the information in the same way regardless of whether disclosure is voluntary or mandatory suggests that investors could benefit from mandatory disclosure of corporate political spending. Furthermore, corporate political spending disclosure decreases investment by investors with misaligned political identities and does not increase investment by investors with aligned political identities. As such, corporations have an incentive to avoid disclosing their political spending unless a regulator requires it.

My study also extends the literature on the effects of political influences on investment decisions. Although prior studies show that individual investors become more optimistic when their preferred party is in power (Bonaparte et al. 2017) and money managers invest in specific industries or types of companies based on their political preferences (Chin and Parwada 2009, Hong and Kostovetsky 2012), my study is the first to document that the nature of the disclosed political spending of an individual firm can affect how investors view that firm as an investment.

Finally, my study extends prior results showing that investment decisions are more affected by negative political considerations than by positive political considerations. Consistent with Hong and Kostovetsky, who find that Democrat money managers make less investment in "socially irresponsible" firms, and with Kaustia and Torstila (2011), who find that left-wing voters and politicians in Finland are less likely to participate in the stock market based on their negative views of capitalism, I find that investors whose political views are misaligned with the political spending of a company assess that company as a less attractive investment and invest less in the company. Also, I find that this effect holds not only for investors with progressive political identities, as seen in prior studies, but also for investors with conservative political identities.

My study has several limitations that could be addressed in future research. First, I conduct my experiment with non-professional investors and find strong evidence that these investors consciously use corporate political spending information in their decisions. However, although prior studies suggest that professional investors also exhibit some political preferences in their decisions, my results cannot be directly extrapolated to professional investors who may behave differently for a variety of reasons. Therefore, a follow-up study using financial analysts and/or fund managers to see whether they behave similarly to non-professional investors would be useful.

Second, my study is limited to investors' reactions to corporate political spending information and does not address the managerial decisions behind such spending. It is possible, and intuitive, that managers would anticipate investors' use of corporate political spending information and adapt strategically. That is, managers could make political spending and disclosure choices based on their perceptions of their firm's investors. Also, managers may make different corporate political spending choices under a mandatory disclosure rule. Further research on the managerial decision-making behind firms' political spending would thus help regulators and standard setters better understand the consequences of any potential disclosure rules.

Finally, my experiment does not take into account many factors that exist in the real-world. I manipulate corporate political spending as either progressive or conservative, but firms often spend on both sides. My results cannot specifically address how investors may react to more complex disclosures with political spending supporting many different groups. Also, investors' reactions to corporate political spending may vary based on firm characteristics that I held constant in my study. I chose to use a grocery store chain in my experiment to minimize the influence of the company's industry on investors' decisions. However, investors could react differently to the political spending of a major defense contractor, for example. In that same vein, clear links between a company's political spending and its business interests could change how investors evaluate that spending. Thus, there remains much to be learned about how investors use corporate political spending information, and future research could explain nuances not captured in my study.

4.0 FUTURE RESEARCH ISSUES RELATED TO CORPORATE POLITICAL SPENDING

In my study, I examined investor reaction to corporate political spending (CPS) by a single firm that either had conservative or progressive political spending. Also, my participants were non-professional investors, and participants' investment decisions were not linked to actual economic outcomes in my experiment. Although my design allowed me to establish that investors react to the disclosure of CPS, there are a number of important open questions that could be addressed in future research.

First, it is not uncommon for firms to have CPS that spans both sides of the aisle. For example, Exxon Mobil has supported both Republicans and Democrats with its CPS in recent years.[28] A natural extension of my study would be to run an additional condition wherein the firm spends on both conservative and progressive groups. The results of my study suggest that investors' reactions to CPS are limited to negativity toward CPS with which they disagree, so it would be interesting to see if that negative effect is still present when a firm also has CPS with which the investor agrees. If the negative effect is still present, it would provide evidence that the results of my dissertation study are robust to more complex CPS scenarios. If the negative effect is no longer present, it would suggest that CPS with which investors agree helps to ameliorate their negative reactions to CPS with which they disagree. These findings would have implications for

[28] For some details on Exxon Mobil's CPS, see http://www.trackyourcompany.org/company-details.html?Contributions_Org_ID=415.

regulators in their consideration of disclosure requirements and would also be relevant for firm managers when making CPS decisions.

Second, my study showed the reactions of non-professional investors to CPS, but much of the trading volume in stock markets is directed by professional investors who possess greater knowledge about investing and may behave differently than the casual investor. So, it would be interesting to examine professional investors' reactions to CPS. I could do this with an experiment similar to my prior study but including the additional condition described in the preceding paragraph. It may be difficult to recruit professional investors for this study, but using master's students who have already completed at least level 1 of the CFA exam and have experience in investing would provide a more sophisticated set of participants. If the findings of the study do not differ from my prior study, the question would then be whether disclosures of CPS would have price effects in financial markets. Alternatively, if professional investors do not react to the disclosure of CPS, we could infer that they do not find CPS to be informative about firm value and that disclosures of CPS probably would not affect stock prices because professional investors would trade against non-professional investors and set prices without considering CPS.

Third, a follow-up experiment using a laboratory financial market could show whether investors' reactions to CPS affect their investing behavior and could also provide more information about the reasons for their reactions. Comparing bid and ask data across conditions would reveal if investors use CPS information, and comparing prices across conditions would reveal if the disclosure of CPS affects price. In the experimental market, I could create assets that have payoffs to the investors who hold them and also have payoffs to a political group. If investors' negative reactions in my prior study were affective responses to the CPS, investors should have similar feelings in the experimental market and offer lower bids for assets that have payoffs to political groups with which the investors disagree. Alternatively, if investors' negative reactions in my prior study resulted from reasoning about the effects of CPS on firm value, investors would have no reason to bid lower for assets that have payoffs to political groups with which they disagree unless those payoffs affected the investors' payoffs. I could manipulate whether the political payoffs affect investors' payoffs and examine investor behavior between-participants. If investors do not lower their bids in response to assets that have disagreeable political payoffs when those payoffs do not affect their own payoffs, it would support a financial motivation for the negative effect in my prior study.

If investors lower their bids even when their own payoffs are unaffected in the study described in the previous paragraph, it would support an affective argument for the negative effect in my dissertation study. Then I could introduce another manipulation to test the robustness of the effect in the presence of financial incentives. If I provide a mix of high- and low-paying assets such that in some markets only the high-paying assets have payoffs to one of the political groups, conservative or progressive, some investors will be forced to choose between buying low-paying, politically-neutral assets or buying high-paying assets that are linked to a political group with which they disagree. The difference in the yields investors are willing to accept between these assets would provide some evidence about the strength of the negative effect of misaligned CPS.

Fourth, there could be an interaction effect between firm type and CPS such that investors may not react in the same way across firms. Coates (2012) and Hadani and Schuler (2013) both provide evidence that CPS is not detrimental, and may be beneficial, to firms reliant on government regulation and/or spending. Considering this, it would be useful to re-examine the effect from my prior study in the context of different industries. This could be achieved by running a similar experiment to the one in my study but with firms in the defense and utilities industries. This manipulation could also be implemented in the experimental market study mentioned in the previous paragraph. The effect from my prior study may be diminished or absent in scenarios where a firm stands to benefit from CPS through reduced regulations or increased government spending.

Fifth, the existing literature is mixed regarding managers' motivations for CPS, with some studies supporting the idea that CPS is a strategic investment and other studies condemning CPS as an agency cost. For example, Cooper et al. (2010), Correia (2014), Akey (2015) and Brown et al. (2015) all find evidence of firms benefiting from CPS, whereas Coates (2012), Aggarwal et al. (2012), and Hadani and Schuler (2013) find evidence that CPS lowers firm value. My dissertation study does not address managerial decisions about CPS, but examining these decisions could be useful to regulators and investors. Both regulators and investors would likely welcome information about managers' intentions concerning CPS, as the agency cost explanation for CPS would make it unacceptable. Furthermore, if managers conduct CPS to benefit their firms, regulators would probably also want to understand how the firms derive benefits from CPS and whether those benefits occur through legal and ethical pathways. Related, there are also mixed results regarding how the disclosure of CPS is received by the market. Baloria et al. (2015) and Prabhat and Primo (2017) show negative effects of CPS disclosure on stock returns, but DeBoskey et al. (2017) shows that firms with more transparent disclosure of CPS enjoy lower costs of debt. To address this, I could conduct an experiment to examine managers' motivations for CPS and how their decisions about CPS may change if disclosure is required or expected.

Sixth, it is unclear how other stakeholders like suppliers and customers react to CPS and whether and how managers consider their reactions when making decisions about CPS. Anecdotally, customers have responded very negatively to disclosed CPS by Target and to the personal political spending by one of the owners of L.L. Bean (Torres-Spelliscy 2010, Victor 2017). With events like these being publicly recognized in the popular press, it is likely that managers at least consider how their firms' CPS would be viewed by current and potential customers before deciding on CPS matters.

In all, CPS and its disclosure are interesting and pertinent topics for accounting research as the legal environment in the United States continues to allow corporations to spend their funds to influence political outcomes without requiring disclosure of that spending. Many questions remain about how investors would use disclosed CPS information if it were available and about why and how managers decide to engage in CPS. My dissertation study provides a first step toward acknowledging that investors use CPS disclosures in their decisions and also opens a number of additional questions.

APPENDIX: EXAMPLE FROM EXPERIMENTAL INSTRUMENT

Example of Company Information (Progressive Spending, Voluntary Disclosure)

Today you will be making investment decisions regarding a company. You will have access to information regarding the company's business as well as its most recent income statement. You will rate the attractiveness of the company as an investment and indicate how much of a hypothetical investment portfolio you would invest in the company.

The Great Grocer Company owns a chain of grocery stores throughout the United States. With stores in 35 states and over 2,700 locations, Great Grocer has been in the grocery business for over a century and has a loyal customer base in many of its markets. It has historically positioned itself as a low-cost provider of food and basic household items, but recently it has renovated some of its stores in higher-income areas to compete with the larger, upscale chains that have become more popular in those areas.

Sales have grown consistently in the past five years with an annual average growth rate of six percent. Sales at existing stores grew at an annual average of nearly four percent over the same period, while the Company's newer stores performed well and accounted for the remainder of its sales growth. Although there were increased costs associated with the new store openings and the renovations of some older stores, Great Grocer has increased its net income by an annual average of about four percent over the past five years.

Great Grocer Company's income statement for the most recent year is presented below (in millions of dollars):

	(In millions)
Sales Revenue	$109,830
Cost of Goods Sold	85,496
Gross Profit	24,334
Operating Expenses	20,758
Operating Income	3,576
Interest Expense	482
Income Before Tax	3,094
Income Tax Expense	1,045
Net Income	$ 2,049

Although not required to do so by any reporting standard, Great Grocer explains in the notes to its financial statements that $400,000 of its $20,758 million in operating expenses reflects political spending in the form of contributions it made to the Fund for a Progressive America.

According to its website, “The Fund for a Progressive America is a 501(c)(4) not-for-profit organization that advocates on behalf of Americans with progressive views. We support issues important to our members such as fair trade policies and actively-engaged government. We also support candidates for elected office who understand that progressive policies are vital for the country’s enduring success.”

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