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ESSAYS IN CORPORATE FINANCE AND GOVERNANCE

A DISSERTATION APPROVED FOR THE
MICHAEL F. PRICE COLLEGE OF BUSINESS

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I dedicate this dissertation to my mother and best friend, Connie Newton.

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Abstract

This dissertation is a collection of essays that investigate the intersection of managerial behaviors, corporate governance quality, and financial outcomes. Chapter 1 explores the relationship between chief executive compensation, financial performance, and corporate governance quality for large U.S. nonprofits – an organizational form that does not have owners. Strong corporate governance helps to establish reasonable levels of managerial pay and perquisite consumption, and to mitigate excessively high levels of executive compensation that tend to accompany poor organizational performance. These findings demonstrate the positive impact of good governance on managerial behaviors and corporate outcomes in a setting where monitoring mechanisms are inherently weak. Chapter 2 examines whether nonprofit hospitals use earnings management to enhance their charitable image. Nonprofit hospitals whose earnings deviate from a low profit benchmark shift costs from non-patient-centered to patient-centered activities to project the appearance that they are better fulfilling a charitable mission and patient-centered focus. This effect is magnified for hospitals with greater normative pressures, lower regulatory oversight, and greater reliance on external financing through donations. These findings highlight the importance of looking beyond bottom-line net income when assessing the quality of accounting earnings. Chapter 3 investigates the impact of corporate political connections on firm value and financial policies in the context of a landmark Supreme Court ruling, *Citizens United v. Federal Election Commission* ("*Citizens United*"). Historically politically connected firms realized an abnormal

price drop of −0.475% on the date the *Citizens United* decision was announced, and a cumulative abnormal loss of −1.219% five days after the announcement date. In contrast, historically non-politically connected firms enjoyed positive returns on announcement date on the order of 0.240%. Additionally, the corporate cash holdings of historically politically connected firms increase in response to the *Citizens United* decision, particularly when corporate governance is weak. These findings support a link between agency costs and political connections.

Chapter 1

Executive Compensation, Organizational Performance, and Governance Quality in the Absence of Owners

I. Introduction

Stark differences in the organizational forms of nonprofit and for-profit entities have elicited an assortment of concerns about the role and responsibilities of nonprofit managers. Specifically, nonprofit organizations lack many of the external monitoring mechanisms native to for-profit entities, thereby allotting nonprofit managers discretion about whether and how to prioritize their organization's goals and objectives or, alternatively, their own personal interests. However, despite the prominence of the nonprofit sector in the U.S. economy – specifically, encompassing 5.5% of U.S. GDP and 9% of the nation's labor force (in 2010)[1] – many of these concerns remain unaddressed by both researchers and regulators. Indeed, of particular importance is the nonprofit organization's absence of shareholders and the implications such lack of oversight has on the motives and actions of nonprofit managers. Given that the managers of nonprofits are not held accountable to (and are not themselves) owners of the organization, nonprofit entities are exposed to agency conflicts that are arguably more prevalent and of greater severity than those facing for-profit organizations.[2] These concerns draw special attention to nonprofit

[1] See Roeger, Blackwood, and Pettijohn (2013).

[2] Clearly, the term "agency conflicts" in the nonprofit context does not carry the same meaning as in for-profits because nonprofits do not have residual claimants – just a nebulous set of "stakeholders." I use the term "agency conflicts" here in the broader sense of managers pursuing personal goals instead of those of the organization.

governance quality since implementing strong governance mechanisms should help to ward off undesirable consequences that are likely to accompany agency conflicts, such as high levels of executive pay and/or poor organizational performance. This paper presents evidence targeted at answering four main research questions: (1) What factors determine CEO-to-employee relative pay and, separately, executive perquisites at nonprofits?, (2) What factors explain organizational performance?, (3) What is the relationship (if any) between CEO-to-employee relative pay and organizational performance?, and (4) What role does governance quality play with respect to the relations described in (1), (2), and (3)?

I investigate the intersection between CEO-to-employee relative pay, organizational performance, and governance quality in the context of a large and diverse sample of nonprofit organizations over the period 2008-2010, utilizing nonprofit financial and governance data drawn from the significantly redesigned Internal Revenue Service (IRS) Form 990, *Return of Organization Exempt from Income Tax*, which large nonprofits are required to file beginning in tax year 2008. By most accounts, the most notable additions to the new form were questions related to governance practices. The new section, entitled "Governance, Management, and Disclosure", was touted as the revamped form's "crown jewel" by Steven T. Miller, former Commissioner of the Tax Exempt and Government Entities Division (Miller 2008). These new data allow for a comprehensive analysis of nonprofit governance quality that was previously impossible to conduct. I investigate how nonprofit

relative pay and measures of nonprofit performance relate to a nonprofit governance quality index that I construct from this relatively new dataset.

Also of interest in the present study is the relation between nonprofit CEO-to-employee relative pay and nonprofit performance.[3] A talented and very hard-working CEO who operates in the best interests of the nonprofit may be rewarded for achieving optimal organizational performance (Pay-for-Performance Hypothesis). Controlling for other factors, this hypothesis implies a positive relation between CEO compensation and measures of nonprofit performance. In contrast, abnormally high CEO compensation may arise if governance mechanisms are inadequate such that some CEOs are able to use their power, influence, and freedom from monitoring to extract high levels of compensation and/or to minimize their efforts (Agency-Governance Hypothesis), implying a negative relation between relative pay and performance. While the former example highlights an optimal relation between relative pay and performance, the latter profiles a case of sub-optimally high relative pay and low organizational performance. To disentangle these hypotheses, it is important to account for governance quality since both CEO-to-employee relative pay and organizational performance should be impacted by board monitoring and

[3] As a testament to the relevance of pay-for-performance in the nonprofit setting, it is worth noting that several large nonprofits explicitly state that executive pay is tied to organizational performance. For example, the compensation committee charter of the American Cancer Society states that the committee must identify the measures and levels of performance to be used when setting the CEO's annual incentive plan goals (http://www.cancer.org/aboutus/whoweare/governance/acspc-041378). Dignity Health states that a substantial portion of executive pay is linked to organizational performance (http://capsules.kaiserhealthnews.org/wp-content/uploads/2013/06/Dignity-Health-statement.pdf). In a 2014 job posting for Chief of Staff, Bellweather Education Partners describes a benefits package that is based in part on firm performance (http://www.bridgespan.org/Nonprofit_Jobs/Position_Details.aspx?jobId=11556).

governance. Specifically, when governance quality is high, we might expect a positive relationship between executive pay and nonprofit performance. In practice, however, any quantitative measure of governance (including the one employed here) provides a mere *proxy* for true governance quality since we cannot observe individual board member characteristics, such as how capable they are or the effort they expend. Therefore, a finding of a negative relation between CEO-to-employee relative pay and nonprofit performance after accounting for the governance measure, would constitute additional evidence of serious governance issues at many nonprofits. Hence, this paper tests whether, if at all, there exists a link between CEO compensation and nonprofit performance that is of a positive (Pay-for-Performance Hypothesis) or negative (Agency-Governance Hypothesis) variety, and how any such relation depends on nonprofit governance quality. Clearly both hypotheses imply that CEO-to-employee relative pay and organizational performance are endogenous since both are likely impacted by unobservable variables such as executive ability, motivation and talent. I address this likely endogeneity between relative pay and organizational performance using instrumental variables.

Pay of both executives and non-executives likely differs considerably from one nonprofit to another. For instance, salaries at hospitals or major universities tend to exceed those in preschools and local theatre groups. To partially account for this, I utilize a "relative pay ratio," defined as (the natural log of) the ratio of CEO compensation to average non-CEO employee compensation. Nonprofit performance is captured through four measures, including the ratios of program expenses to total

expenses, program expenses to total assets, net fundraising revenue to total fundraising revenue, and (the log of) total revenue per employee (e.g., Desai and Yetman 2006). As detailed below, governance quality is defined as an index of numerous mechanisms that illustrate the efficacy of the nonprofit's governing body, governing policies, compensation policies, and accountability and transparency.

I begin the analysis by examining the determinants of the CEO-to-employee relative pay ratio. Controlling for other factors such as industry, nonprofit size, age, and other firm characteristics, as well as CEO tenure and gender, and controlling for endogeneity with instrumental variables, I find that relative pay is negatively related to both the index of governance quality and nonprofit performance. The statistically significant negative association between the relative pay ratio and nonprofit performance is robust to including several factors that could impact both the relative pay ratio and performance, including size, donations growth, liquidity, and asset tangibility. These results support the view that both nonprofit relative CEO pay and organizational performance are subject to agency conflicts, i.e., that at poorly governed nonprofits, insufficiently monitored CEOs are apparently awarded high levels of relative pay while minimizing their efforts.

Next, I explore the determinants of four measures of nonprofit performance while controlling for traditional factors such as firm size, leverage, liquid assets, and asset tangibility. The relation between performance and the governance index is weak and varies depending on the performance measure. However, as previously observed, the negative relation between relative pay and performance is strong and

consistent across all four performance measures indicating that both relative pay and performance are subject to agency and governance problems at nonprofits. Additional analyses using a changes specification lend further support to the Agency-Governance Hypothesis.

I also investigate how other types of executive compensation relate to organizational performance and governance quality. One result of ineffective monitoring mechanisms could be an over-consumption of executive perquisites. Consistent with this theory, I find that weak governance quality (i.e., ineffective monitoring mechanisms) is associated with a higher likelihood of executive perquisites, such as first-class travel, discretionary spending accounts, and housing allowances. Moreover, inferior performance (also suggestive of ineffective monitoring mechanisms) is associated with a greater prevalence of executive perquisites. Taking the opposing view, what can be said for an organization that is characterized by *strong* governance quality and *superior* organizational performance? I find that, although well-governed and high-performing nonprofits are *individually* less likely to award CEOs with perquisites, good governance quality is associated with an incremental but significant *increase* in the likelihood of perquisites among high-performing entities. Collectively, these results suggest that while the Agency-Governance Hypothesis is applicable to the average nonprofit organization, the Pay-for-Performance Hypothesis is supported for nonprofits that are both well-governed and high-performing.

In addition to a general investigation of how expected CEO-to-employee relative pay and organizational performance at nonprofits are impacted by governance quality, I investigate how especially high relative pay and especially poor performance are related to governance quality. While the results of this paper clearly indicate that relative pay at the average nonprofit is inversely related to the governance index, it is possible that extremely high relative pay is even more strongly related to the governance index. Using quantile regressions to infer whether extremely high CEO-to-employee relative pay arises due to poor nonprofit governance, I find that the governance index has a much stronger impact on the 80th quantile than on the conditional mean. Thus, while relative pay at the average firm is inversely related to governance quality, it appears that very high relative pay is even more strongly associated with poor governance. In other words, good governance appears especially effective at restraining extremely high relative pay within nonprofit organizations.

Finally, I analyze whether the relation between nonprofit relative pay, organizational performance, and governance quality differs as a function of nonprofits' revenue structures. Specifically, nonprofits constrained by reliance on a single source of funding likely differ from their peers along dimensions that could plausibly impact the pay-performance-governance intersection. I find that the negative pay-for-performance relation is generally robust to nonprofit revenue structures. I also find that internal governance quality plays a more prominent role in moderating the relative pay levels of nonprofits relying on multiple funding sources

relative to those with a more focused revenue structure. Stronger competitive forces accompanying the focused revenue structures may act as an inherent (external) disciplinary force that precludes the need for strong internal governance.

This paper contributes unique evidence on the alignment of managers' and owners' interests. Specifically, systematic differences in the external monitoring mechanisms of nonprofits relative to those of their for-profit counterparts create unique opportunities by which managers can extract corporate resources or manipulate corporate outcomes for their personal gain (see, for example, the earnings management literature of Krishnan, Yetman, and Yetman 2006 and Keating, Parsons, and Roberts 2008). These distinctive attributes join together to make the nonprofit sector a highly desirable research setting, and one that despite its economic importance – with about 20% of U.S. corporations operating as nonprofits – has been largely neglected in existing corporate finance literature (Adelino, Lewellen, and Sundaram 2014). Existing studies (described below) fall short of providing a complete picture of the potential consequences of (nonprofit) managers without owners. In an attempt to fill this gap, I use relatively new data for a large cross-section of nonprofits to study the relationship between executive compensation, organizational performance, and governance quality.

Regarding extant literature of executive pay, relevant studies of the nonprofit labor market profile the intrinsic motives of nonprofit workers both from theoretical (e.g., Frey 1997) and empirical (e.g., Jobome 2006) standpoints. Consistent with the labor donation theory hypothesis of Preston (1989), Steinberg (1990) concludes that

high wages can undermine the morale of (intrinsically motivated) nonprofit workers. Nevertheless, modern anecdotal evidence has revealed numerous instances of grossly overcompensated CEOs (e.g., Buettner 2011; Hancock 2013), casting doubt on the true motives and reward structures of nonprofit executives. Related literature on nonprofit executive pay reveals a positive link between executive pay and nonprofit size (e.g., Aggarwal, Evans, and Nanda 2012; Frumkin and Keating 2010; Hallock 2002) that varies across industries and ownership types (Ballou and Weisbrod 2003; Eldenburg, Hermalin, Weisbach, and Wosinska 2004).

This paper also relates to existing studies that link executive compensation to firm performance. In the context of the nonprofit sector, difficulty in quantifying nonprofit performance implies potentially few performance-based incentives for nonprofit executives (e.g., Erus and Weisbrod 2003). Empirical evidence on the matter reveals mixed insights on any relation between nonprofit performance and executive pay, ranging from a weak but positive relation (Baber, Daniel, and Roberts 2002) to a negative association (Garner and Harrison 2013) to no significant relation at all (O'Connell 2005; Sedatole, Swaney, Yetman, and Yetman 2014). Relating CEO-to-employee relative pay to multiple measures of nonprofit performance, I find a strong and consistent *negative* relation.

This paper also contributes to existing studies of governance quality and its impact on both executive pay and firm performance. An examination of the importance of corporate governance mechanisms in nonprofit organizations is particularly essential, given the prevalence of weak governance quality in nonprofits

relative to their for-profit counterparts (Fama and Jensen 1983; Glaeser 2003). Inherent challenges to establishing an effective system of nonprofit governance stem from the sector's nondistribution constraint on residual income, the absence of a market for corporate control, and the uniqueness of nonprofit objectives (e.g., Brody 1996). Extant empirical literature demonstrates that poor governance quality alienates donors (Hansmann 1980), while improvements in governance are associated with more efficient operations (Alexander and Lee 2006; Callen, Klein, and Tinkelman 2003). Related studies of the nonprofit governing board – a monitoring mechanism described as a critical input to achieving the organization's charitable and performance goals (Hillman and Dalziel 2003; Miller-Millesen 2003) – reveals links between larger boards and decreased monitoring incentives (O'Regan and Oster 2005) as well as between insider boards and higher compensation levels (Brickley, Van Horn, and Wedig 2010; Cardinaels 2009).

While these findings are informative, the extant literature fails to offer a definitive prediction as to how executive pay should relate to nonprofit performance nor does it test how nonprofit executive pay is related to governance measures. Using governance data newly made available on the IRS Form 990, this paper explores the impact of nonprofit governance quality on both executive compensation and organizational performance.

The next section presents a discussion of relevant literature and develops my hypotheses regarding executive compensation, organizational performance, and corporate governance quality. Section III describes the sampling procedure and

section IV reports the results of the empirical analyses. Conclusions are offered in section V.

II. Nonprofit Compensation, Performance, & Governance

The nonprofit sector is a substantial and increasingly important component of the U.S. economy. In 2012, there were 1.6 million nonprofit entities registered with the IRS. As of 2010, the nonprofit sector encompassed 5.5% of U.S. GDP and 9% of the nation's labor force. At this time, public charities reported an aggregate $1.5 trillion in revenues, $1.45 trillion in expenses, and $2.71 trillion in total assets. Gifts from individual foundations, corporations, and bequests reached approximately $290.89 billion.[4] As of year 2010, the median CEO pay among 282 large charities was $475,192 – a 2.7% increase over the prior year (Gose and López-Rivera 2012).

A. The Nonprofit Labor Market

A large proportion of extant nonprofit research has explored the motives, roles, and responsibilities of the nonprofit worker. While for-profit workers are predominantly motivated by tangible monetary incentives, nonprofit workers tend to be intrinsically motivated and grateful to work for the greater good (Frey 1997; Gallagher and Einhorn 1976; Mirvis and Hackett 1983). The altruistic nature of

[4] Interestingly, while many other sectors of the economy struggled during the 2007-2009 financial crisis, most nonprofit industries thrived, reporting an increase in both employment and wage levels in 2010 relative to 2000 on the order of 17% and 29%, respectively (inflation-adjusted). This positive pattern runs counter to that of the for-profit business sector, which endured a drop in both employment and wage levels over the same period of –6% and –1%, respectively (Roeger, Blackwood, and Pettijohn 2013) .

nonprofit worker preferences has been formalized by labor donation theory (Preston 1989), which predicts that nonprofit workers select into their roles based on the charitable mission of the nonprofit rather than its propensity to distribute monetary benefits. For example, according to Hansmann (1980), the nonprofit sector can better attract intrinsically motivated workers by establishing lower pay levels relative to the for-profit sector. Preyra and Pink (2001) use the multitask agent model of Holmstrom and Milgrom (1991) to demonstrate that conservative pay levels can motivate a nonprofit manager to focus on the core (often unobservable) activities of a charitable organization. In a review of related research, Steinberg (1990) concludes that high pay levels can actually undermine the morale of intrinsically motivated workers. Handy and Katz (1988) find that committed nonprofit workers are willing to accept a lower salary in exchange for working in the nonprofit sector. Weisbrod (1983) finds that lawyers working for the public interest accept lower pay relative to attorneys of private practice. The notion that nonprofit workers donate their labor is also consistent with the empirical results of Jobome (2006) for a sample of U.K. charities, Narcy (2011) for a sample of French nonprofits, and Salamon and Sokolowski (2005) when finding an 11% wage differential between nonprofit and for-profit workers in the U.S.

Related research on the topic of nonprofit labor markets provides insights on variation due to gender, skill, and industry. For example, in a study of the U.S. labor market, Preston (1990) finds that the wage gap between lesser-paid nonprofit workers and higher-paid for-profit workers is less pronounced for females relative to

males. Based on U.S. Census data, Preston and Sacks (2010) find that the average nonprofit worker earns about the same as a less-skilled for-profit worker. Using data for Great Britain, France, and Italy for year 1998, Lucifora and Meurs (2006) find that, on average, highly skilled workers receive higher pay in the private sector relative to the public sector, while just the opposite is true for low-skill types. They find that these pay differences are larger for (1) females when compared to males, and (2) less regulated environments (e.g., Great Britain) relative to more stringent regulatory environments (e.g., France and Italy). Segal and Weisbrod (2002) find that volunteers will select into industries that align well with their skills and preferences, which creates inter-industry variation in volunteer supply.

Extant research also demonstrates that the type – and not just amount – of incentives impacts managerial (and organizational) performance. Specifically, nonprofit organizations are known to reward managers with perquisites such as elaborate offices, shorter workdays, and additional vacation time (Glaeser and Shleifer 2001). Relatedly, when donor monitoring is weak, Preston (1988) contends that excess donations may be used to fund perquisites. Additional insights into the importance of pay composition are provided in related for-profit literature (e.g., Ryan and Wiggins 2004).

B. Nonprofit Executive Compensation

As a matter of U.S. law, nonprofit organizations "must not be organized or operated for the benefit of private interests…" (IRC § 501(c)(3)(d)(ii)). Even so,

numerous cases of high nonprofit executive pay have been documented. For example, Philip Levy and Joel Levy (brothers), two top executives of a Medicaid-financed nonprofit organization serving the developmentally disabled in New York, faced significant scrutiny for earning nearly $1 million a year, driving state-funded luxury cars, and passing their children's tuition bills onto the nonprofit group (Buettner 2011). As another example, in mid-2013, Senator Charles Grassley – a long-time critic of nonprofit hospitals – voiced serious concerns about nonprofit boards rewarding CEOs with large monetary bonuses in return for their increasing the quantity, not the quality, of services provided (Hancock 2013). One of the many other heavily scrutinized cases of apparent abuse of nonprofits' tax-exempt status is that of American Bureau of Shipping, a company advertising "to promote the security of life & property on the seas" (American Bureau of Shipping 2011), and "which lavished its executives with multimillion-dollar pay packages and perks; and purchased an offshore hedge fund [in the Cayman Islands]" (Evans 2012).[5]

While these and other well-documented cases of high executive nonprofit pay raise obvious concerns regarding the recruitment procedures and governance standards in the nonprofit sector, questions on the origins of variation in nonprofit executive pay schemes – both within firms (relative to non-executives) and across firms (relative to competing organizations) – have not been rigorously explored heretofore. Existing evidence supports a positive association between executive pay and organizational size (e.g., Aggarwal, Evans, and Nanda 2012; Frumkin and

[5] For other examples involving allegations of excessive nonprofit executive pay, see Dennison (2011), Doyle (2011), Fries (2013), Kruesi (2011), Pogrebin and Taylor (2010), and Sataline (2010).

Keating 2010; Hallock 2002), systematic differences in pay levels across industries and ownership types (Ballou and Weisbrod 2003; Eldenburg, Hermalin, Weisbach, and Wosinska 2004), a positive association between excess executive pay and excess liquid asset levels (Core, Guay, and Verdi 2006), and an inverse association between pay levels and donation inflows (Balsam and Harris 2014; Galle and Walker 2014). However, the literature fails to offer conclusive evidence on many important issues involving nonprofit compensation, such as how executive pay relates to organizational performance and corporate governance.

C. Nonprofit Performance and Executive Pay

Studying the relation between nonprofit performance and any other variable is complicated by the fact that there is no universally accepted measure of nonprofit performance. Structural differences in the profit motives and pricing structures of for-profit and nonprofit hospitals (e.g., Hoerger 1991) imply that performance measures are generally not interchangeable between the two sectors. Because it is difficult to quantify the performance of nonprofit organizations, the compensation structure of nonprofit executives includes fewer performance-based incentives relative to that of for-profit executives (Erus and Weisbrod 2003; Roomkin and Weisbrod 1999; Wilding, Collins, Jochum, and Wainwright 2004). Accordingly, Jobome (2006) posits that any relation between nonprofit performance and managerial pay should be weak, though this prediction may vary based on the nonprofit's financing mix. For example, Rose-Ackerman (1987) illustrates that a

nonprofit heavily relying on private donations – especially in lieu of other funding sources – will be held more accountable to its donors, which could affect managerial behaviors and performance.[6] A predicted weak pay-for-performance relation in nonprofits stands in stark contrast to the repeatedly documented positive pay-for-performance link in for-profit entities (e.g., Jensen and Murphy 1990; Conyon and He 2011),[7] which stems from a desire to align the manager's actions with the company's profit-maximizing goals.

Empirical studies in the nonprofit realm have yielded mixed evidence on the pay-for-performance relation. For a sample of U.S. liberal arts colleges, O'Connell (2005) finds an insignificant relation between chief executive pay and endowment yield. In a cross-sectional study of charities based in the state of Maryland for the period 1996-1997, Baber, Daniel, and Roberts (2002) find a weak but positive relation between CEO pay and nonprofit performance, where the latter captures changes in program spending. Related studies, all based on the U.S. nonprofit sector, contribute additional and varied conclusions: Sedatole, Swaney, Yetman, and Yetman (2014) find no significant relation between executive pay and program spending; Garner and Harrison (2013) document a negative association between executive pay and program spending levels when the CEO is the only executive listed on the Form 990; while Brickley and Van Horn (2002) find that executive pay

[6] A sample of the related literature demonstrating the economic impact of nonprofits' funding mix includes Andreoni and Payne (2003) for the U.S., Anheier, Toepler, and Sokolowski (1997) for Germany, and both Posnett and Sandler (1989) and Wolk (2013) for the U.K.

[7] An exception is Brick, Palmon, and Wald (2006) who find an inverse relation between firm performance and the excess amount of compensation to a for-profit's CEO and directors, suggestive of cronyism.

is positively related to return on assets. It bears repeating that studies of nonprofit performance share a common weakness, that being the absence of a universally accepted and applicable measure of nonprofit performance. In an attempt to address this deficiency, I use multiple measures of performance in the main analyses of this paper.

D. Nonprofit Governance

A particularly important difference between the organizational forms of for-profit and nonprofit entities lies in their ownership structures. Specifically, as noted by Jensen and Meckling (1976), one of the primary avenues through which agency conflicts arise within for-profits is between their agents (managers) and primary principals (shareholders), particularly when management fails to fulfill its obligation to maximize shareholder wealth. In stark contrast, nonprofit organizations are bound by a non-distribution constraint that prohibits the organization from distributing residual income to any individual with an ownership interest in the firm (Weisbrod 1988). Moreover, who has property rights to this residual income is not clear (Sloan 1988). According to Weisbrod (1989), "The principal source of [the nonprofit sector's] theoretical justification is also the source of its principal liability – the nondistribution constraint. This legal restriction on distributing profit to anyone who has control over the organization has the adverse effects of reducing managerial incentives to minimize costs, seek out new markets, and innovate" (p. 545). In an attempt to offset these adverse effects, a nonprofit might establish a system of

corporate governance protections;[8] however, available evidence suggests that the governance structures of nonprofits are much weaker relative to their for-profit counterparts (Glaeser 2003).

A core component of any corporate governance system is the board of directors. The role of a nonprofit director is particularly complex. Brody (1996) attributes this complexity to the sector's non-distribution constraint, the absence of a market for corporate control, and the uniqueness of each nonprofit's objective. Consistent with "resource dependence theory" (Pfeffer and Salancik 1978), multiple studies have described the board as a key resource (e.g., Brown 2005; Miller-Millesen 2003). Hillman and Dalziel (2003) point to board capital, defined as human capital (e.g., expertise) and relational capital (e.g., social ties), as being a key resource that the nonprofit organization depends on for achieving its charitable and performance goals. Nonprofits may also structure their audit committees to reflect demands for monitoring, which are heightened when a nonprofit receives financing from the government (Vermeer, Raghunandan, and Forgione 2006). Fama and Jensen (1983) provide an account of what nonprofit boards should entail: board members that are primarily if not entirely external to the organization, most if not all of whom meaningfully contribute to the organization through their wealth or time, and all of which are prepared to serve without pay. Middleton (1987) identifies other important properties: nonprofit boards exist to ratify (not formulate) policy, retain ultimate decision-making authority even though their decisions are often based on

[8] For formal definitions of "corporate governance," see Anheier (2005), Shleifer and Vishny (1997), and Tirole (2001).

information supplied by management, and tend to dodge controversial issues in the interest of preserving harmony and peace among its members. For an extensive synthesis of existing empirical work on nonprofit boards – and specifically on the balance of power between nonprofit boards and management, see Ostrower and Stone (2006).

These issues beg the question of what types of governance mechanisms nonprofits utilize, and whether there exists a "best practices" system of nonprofit governance. In a survey questionnaire of nonprofits based in the state of North Carolina, Iyer and Watkins (2008) provide evidence on nonprofits' voluntary adoption of governance provisions resembling those that had been mandated of for-profit entities by the Sarbanes-Oxley Act of 2002. Examples include independent audit committees, codes of conduct, certifications of financial documents, and internal controls. They find that many nonprofits had implemented at least some of these protections, with notable exceptions including code of conduct and whistleblower policies. In a study of nongovernmental development organizations in Spain, Andrés-Alonso, Cruz, and Romero-Merino (2006) find that one of the most effective governance protections is an active institutional donor. With such a large stake in the entity, these donors have good reasons to carefully monitor the activities of managers. As to whether there exists a one-size-fits-all "best" set of governance provisions, existing evidence would suggest otherwise. Notably, Andrés-Alonso, Azofra-Palenzuela, and Romero-Merino (2010) contend that the optimal governance system (e.g., board size, number of independent board members, etc.) is

organization-specific, and depends on factors such as the character and expertise of its directors.

E. The Economic Importance of Nonprofit Governance

Studies on the economic impact of nonprofit governance have yielded a diverse yet largely fragmented set of findings.[9] As highlighted by Lynk (1995) for a sample of California nonprofit hospitals, there is no one-size-fits-all objective for the nonprofit sector. Specifically, while for-profit entities are established for the purpose of maximizing profits (as their name implies), nonprofits are formed for a purpose much greater than to simply *not* make a profit, though that purpose will vary from nonprofit to nonprofit. The consequence of these unique and difficult-to-value objectives is that many of the corporate outcomes realized in the for-profit sector are not applicable to nonprofits. For example, according to Lynk (1995), mergers of for-profit and nonprofit entities distinctly differ: while for-profit mergers result in a net *increase* in output prices, mergers of private nonprofit hospitals result in a net *decrease* in output prices.

Despite these challenges, many studies of nonprofit organizations have attempted to link nonprofit governance quality to economic variables, such as executive compensation. For example, both Cardinaels (2009) and Brickley, Van Horn, and Wedig (2010) provide evidence of a link between high executive pay levels and boards composed of a large proportion of "insider" (i.e., non-independent)

[9] See Jegers (2009) for a comprehensive survey of nonprofit research drawn from the economics, health economics, management, and accounting disciplines, and their relation to principal-agent theory.

directors. Existing evidence suggests that this result also holds for for-profits (e.g., Core, Holthausen, and Larcker 1999). Fisman and Hubbard (2005) find that for nonprofits having poor governance, more excess funds are distributed as managerial pay than are retained by the nonprofit to fund future expenditures. Eldenburg and Krishnan (2003) find that CEOs of municipal district hospitals receive less pay than CEOs of private nonprofit hospitals, principally because the boards of municipal district hospitals are elected by the public and their governing policies are subject to substantial scrutiny.

Related literature has investigated the association between nonprofit governance and organizational performance. These studies commonly find that stronger governance is associated with better nonprofit performance, though drawing such inferences is complicated by difficulties in objectively and accurately measuring both governance quality and nonprofit performance (e.g., Herman and Renz 1997, 1999, 2004). To measure governance quality, Willems, Huybrechts, Jegers, Weijters, Vantilborgh, Bidee, and Pepermans (2012) develop a 26-item governance quality index to assess matters such as the board's responsibilities and the nonprofit's willingness to stay abreast of trends in management and governance practices. Another approach to establish a link between board performance and organizational performance is that of Gill, Flynn, and Reissing (2005) who developed a Governance Self-Assessment Checklist, an instrument designed to inform board members of their own contributions to organizational effectiveness. For a sample of Arizona and Florida hospitals, Kalodimos (2014) finds that stronger

internal governance significantly improves the prospects of heart attack survival. Related work on the governance-performance link of U.S. nonprofits includes Callen, Klein, and Tinkelman (2003), who find a positive association between the ratio of expenses spent on charitable purposes and the proportion of major donors on its board; Alexander and Lee (2006), who document more efficient operations for nonprofits whose boards conform to the corporate model of a governing board; and Desai and Yetman (2006), who find organizational performance improvements pursuant to *state-imposed* governance provisions. Additionally, O'Regan and Oster (2005) provide a variety of inferences on the economic impact of *director-level* characteristics.

F. The Intersection of Nonprofit Executive Pay, Organizational Performance, and Governance

Additional and more sophisticated research on the intersection of executive compensation, organizational performance, and governance quality in nonprofit entities is warranted for several reasons. First and foremost, extant literature fails to provide a definitive directional prediction as to how nonprofit CEO pay should relate to organizational performance, particularly after controlling for governance quality. One reason for this deficiency is simply that the volume and depth of governance data were lacking prior to the 2008 introduction of a redesigned and much more detailed IRS Form 990. By using data originating from the redesigned Form 990, my study overcomes governance-related data limitations.

A second motivation for further study of the relation between nonprofit pay, performance, and governance is that different empirical studies utilize different measures of performance, which further complicates any attempt to make meaningful conclusions on the nonprofit pay-for-performance link. Uncertainty underlying how to best measure nonprofit performance – particularly relative to for-profit performance – has long been recognized as problematic (Roomkin and Weisbrod 1999). I alleviate these ambiguities by making use of multiple measures of nonprofit performance in the primary analyses of this paper.

Lastly, the link between executive pay, performance, and governance has become increasingly important in response to better quality compensation disclosures already implemented in the U.S. (via the Dodd-Frank Act) and to those being seriously considered around the world (see, for example, Gumbel (2013) in the case of Europe; Shecter (2013) in the case of Canada; and Vina (2011) in the case of the United Kingdom). In the for-profit executive compensation literature, researchers have responded to CEO-to-employee pay ratio disclosure mandates by relating firm performance to measures of relative CEO pay. Examples include Faleye, Reis, and Venkateswaran (2013) who utilize a "CEO-employee relative pay [ratio]," defined as the ratio of CEO pay to average employee pay (in log form), as well as Bebchuk, Cremers, and Peyer (2011) who study the "CEO pay slice," computed as the ratio of CEO pay to total pay to the top 5 executives. To test for the possibility of high CEO compensation in the nonprofit sector, I utilize a measure of CEO compensation relative to average employee compensation. Nonprofit

organizations are keenly aware of the fact that similar disclosure requirements, such as a ratio of CEO-to-employee relative pay, are likely forthcoming.[10] By using a definition of relative CEO pay, my paper offers new and timely insights into nonprofit compensation structures.

III. Sample

Data for this study are based on year 2008, 2009, and 2010 filings of IRS Form 990, *Return of Organization Exempt from Income Tax*, the annual information return required of all large tax-exempt organizations.[11] The form underwent a significant revision for tax year 2008, which included the addition of a new section entitled, "Governance, Management, and Disclosure." Because governance data were recently added (2008), and are presently available for tax years 2008, 2009, and 2010 only, this study's data are large in the cross-section but relatively short in the time-series. Inherent disadvantages of a cross-sectional research design such as that employed in this study and much of the related literature include (1) less ability to draw causal inferences, and (2) the possibility that empirical results will vary when analyzed across different time periods. I use instrumental variables estimations and a changes specification to address these concerns.

[10] Linking a relative measure of pay addresses requests from regulators of the nonprofit sector including that of Massachusetts Attorney General Martha Coakley when indicating that "these kinds of comparisons might moderate the increasing pay disparity between executives and others in the workforce, and increase public confidence in the appropriateness of CEO compensation" (Office of Attorney General Martha Coakley, p. 38).

[11] Private foundations file a Form 990-PF, and small nonprofits (other than private foundations that have gross receipts less than $100,000 and total assets less than $250,000) file a 990-EZ. For the Form 990 and related tax documents, see http://www.irs.gov/uac/Form-990,-Return-of-Organization-Exempt-From-Income-Tax- .

As alluded to previously, on December 19, 2007, a dramatically redesigned Form 990 was issued, about which Acting Commissioner of the IRS, Kevin Brown, stated the following: "We need a Form 990 that reflects the way this growing sector operates in the 21st century. The new 990 aims to give both the IRS and the public an improved window into the way tax-exempt organizations go about their vital mission" (Internal Revenue Service 2007). In its redesigned state, the Form 990 spans eleven pages, not including the many (sixteen) supplemental schedules. Meeting the increased number and complexity of requirements is sure to require additional time and effort on the part of nonprofits, the disdain for which is perhaps no better expressed than through the sentiment of one executive director of a large public charity: "If this is the annual return we have to file, we don't want to be tax-exempt anymore" (Hopkins 2009, p. 115).

A large sample of Form 990 returns are provided in the Internal Revenue Service Statistics of Income (SOI) microdata files, which are publicly and freely available. For this study, SOI data are retrieved from the National Center for Charitable Statistics (NCCS), which cleans and distributes nonprofit data provided by the IRS. The SOI sample is weighted towards larger organizations and as a result, in the aggregate, encompasses over 90 percent of all nonprofit assets and revenues (Yetman and Yetman 2012). To capture data provided in the redesigned Form 990, the sample considered for this study includes all U.S.-based nonprofit organizations in the 2008, 2009, and 2010 tax year SOI databases.[12] CEO identity, compensation,

[12] The SOI database has been utilized in much of the extant, related literature (e.g., Balsam and Harris 2014; Core, Guay, and Verdi 2006; Garner and Harrison 2013; Keating, Parsons, and Roberts

and tenure are gathered from the Guidestar Premium database which, similar to the NCCS, disseminates Form 990 information into a digitized, machine-readable form. I exclude observations involving either non-positive executive compensation (Carroll, Hughes, and Luksetich 2005) or CEO turnovers. It is important to note that since the 2008 tax year marked the first year in which the redesigned Form 990 was filed, not all 501(c)(3) nonprofits were required to use the new form. Specifically, for tax year 2008 (2009) [2010], only those organizations with total assets of at least $2.5 million ($1.25 million) [$500,000] or gross receipts of at least $1 million ($500,000) [$200,000] were required to use the redesigned Form 990. As such, I restrict the sample to only those nonprofits *required* to file the redesigned form since voluntary filers might tend to be well-governed. This restriction biases the sample towards larger nonprofits, but more importantly eliminates the possibility of sample selection bias. Eliminating apparent coding inconsistencies (as in Core et al. 2006, Aggarwal et al. 2012, Baber et al. 2002, and others), observations that are classified as "out-of-scope" by the NCCS, and requiring sufficient data to estimate the main statistical models results in a maximum sample of 10,186 firm-year observations and 5,287 unique nonprofits. To mitigate the effect of outliers and any remaining data errors, all continuous variables are winsorized at the 1st and 99th percentiles.

2008; Yetman and Yetman 2012). With the exception of Yetman and Yetman (2012), these studies rely on data drawn from the old Form 990.

IV. Empirical Analysis

Table 1 provides descriptive statistics of variables used in subsequent empirical analyses. All variables are defined in Appendix A. The mean (median) values of total assets, total revenue, and number of employees are $198.4 ($59.4) million, $93.0 ($23.4) million, and 879 (308), respectively. The mean (median) CEO compensation is $327,212 ($222,326), and the mean (median) CEO-to-employee relative pay ratio is 11.36 (8.06).[13] Comparing relative pay ratios across major industries reveals that the highest values belong to the "Education" sector (mean = 14.88, median = 14.17) and lowest values to the "International" sector (mean = 4.65, median = 3.72).[14] As for the primary measure of organizational performance utilized in this paper, the mean (median) proportion of program expenses to total expenses is 84.3% (86.7%). Comparisons across major industries reveal that the public benefit sector has the highest ratio of program expenses to total expenses (mean = 87.57%, median = 90.24%) while the arts sector has the lowest ratio (mean = 77.97%, median = 81.05%). In subsequent statistical tests, I account for differences across industries by including industry fixed effects for each of the 26

[13] For a group of for-profit entities between $10 million to $150 million in total assets (a range comparable to the 25th and 75th percentiles of the current sample's total assets), the mean (median) relative pay ratio is 32 (11), per the author's calculations. Although relative pay levels are notably smaller for nonprofits relative to their for-profit peers, the estimates of nonprofit relative pay provided in this paper are in line with existing calculations. For example, a 2005 *Bloomberg Businessweek* article notes the following: "In 1998, chief executives [of nonprofits] earned 4.4 times as much as the average nonprofit worker, while in 2003 they earned 4.8 times as much. If the trends in executive and worker pay continue, by 2018, chief executives will be making six times what other employees are paid" (Lipman 2005). These forward-looking projections are consistent with more recent examples of nonprofits seeking to establish an upper bound on the ratio of CEO to lowest full-time employee pay: 6-to-1 in the case of San Francisco's city-supported nonprofits (Takagi 2008) and 10-to-1 in the case of St. Mary's College in Maryland (Pyke 2014).

[14] See the *NTEE-10 Industry Classifications* and *NTEE-26 Industry Classifications* variable definitions in Appendix A for industry classifications.

major nonprofit industry groupings (as identified by the National Taxonomy of Exempt Entities (NTEE)).

The governance index consists of sixteen components assessing several aspects of nonprofit governance quality, including its governing board, governing policies, compensation policies, as well as transparency and accountability, and is described in detail in Panel 2 of Appendix A. Most index components consist of "Yes"/ "No" questions coded as 0-1 indicator variables, where a value of 1 signifies the response hypothesized to indicate better governance quality. Each component was chosen from the redesigned Form 990 (primarily the "Governance, Management, and Disclosure" section) on account of: (1) its possible relevance to either CEO compensation or nonprofit performance based on the for-profit and nonprofit literatures, and (2) the likelihood that the source question would be answered honestly.[15] The Form 990 includes several questions in which virtually all nonprofits answer in the same direction; as such, to keep the governance index manageable, questions with especially similar responses across organizations (specifically, those in which at least 95% of organizations agree in their responses) are eliminated. Also, in an effort to assign higher weights to components associated with more diverse responses, I weight each component by its cross-sectional

[15] For example, the governance index includes an indicator variable for whether at least two-thirds of the organization's board of directors is independent. There is a large stream of literature documenting the impact of board independence on executive pay (e.g., Brickley et al. 2010 for nonprofits; Core, Holthausen, and Larcker 1999 for for-profits). As another example, the governance index also includes indicator variables for whether the nonprofit abides by strong governing policies related to conflict of interest, whistleblower, and document retention matters. The importance of having strong governing policies in place has been repeatedly stressed for its potential to curb self-interested managers' tendencies (see, for example, the Sarbanes-Oxley Act and recommendations of the Panel of the Nonprofit Sector).

standard deviation (see Appendix B for each component's respective weight; and Van Bruggen, Lilien, and Kacker (2002) as well as Behrman, Mitchell, Soo, and Braco (2010) on the importance of adopting a survey aggregation method that produces more accurate inferences). For each firm-year observation, the governance index is defined as the average of the values of four sub-indices (*Governing Body, Governing Policies, Compensation Policies, Accountability & Transparency*), where each sub-index is defined as the ratio of the weighted sum of the individual components as a proportion of the observation-specific total number of possible responses, such that governance quality is increasing in the magnitude of the index. The mean (median) standard deviation weighted governance index is 0.363 (0.356), with the public benefit sector having the highest governance index scores (mean = 0.380, median = 0.392) and education sector being associated with the lowest values (mean = 0.322, median = 0.312).

The measure of governance utilized in this paper improves upon related literature in several respects. First, the new Form 990 data provide a comprehensive picture of nonprofit governance mechanisms and precludes having to rely on one or a few metrics such as donor presence on boards of directors (Callen et al. 2003), board independence (e.g., Brickley et al. 2010), or board size (Aggarwal et al. 2012). Second, the governance index is primarily composed of "Yes" / "No" questions; this standardized format facilitates a more objective measure of governance – an otherwise difficult hurdle to overcome (e.g., Herman and Renz 1997; 1999; 2004). Finally, the inferences of nonprofit governance quality presented in this paper are not

restricted to a single state (as in Iyer and Watkins 2008) or a single industry (as in Brickley et al. 2010).

Table 2 provides correlations among CEO compensation, its possible determinants, and measures of organizational performance and nonprofit governance. As expected, (the log of) the CEO-to-employee relative pay ratio is positively related to both (the log of) the number of employees and (the log of) total assets, with correlations of 0.68 and 0.42, respectively. Moreover, the correlation between these two measures of organizational size is also quite high (0.53). Other noteworthy correlations include that between nonprofit age and the number of employees (0.42), suggesting that more mature nonprofits tend to employ a larger number of employees; and a mechanically-induced large negative correlation between the performance measure *Log(Revenue per Employee)* and the proportion of total revenue received by the government (–0.52). All other correlations are less than 0.40 in absolute value.

A. Contrasting Views of the Relationship between Nonprofit Governance, Pay, and Performance

The main focus of my empirical analyses is to investigate the intersection of nonprofit CEO pay, organizational performance, and governance quality. Specifically, it may be the case that CEOs are rewarded for achieving optimal organizational performance, whether it be for maximizing charitable spending (in lieu of administrative or fundraising spending), raising a significant amount of donations in a cost efficient manner, or ensuring their organization is productive as

evidenced by a large ratio of total revenue per employee. I label this prediction the Pay-for-Performance Hypothesis. In contrast, in poorly governed nonprofits, CEOs may be free to prioritize their own best interests rather than those of the organization, leading poorly monitored CEOs to minimize their efforts and/or neglect to optimize organizational performance. I title this prediction the Agency-Governance Hypothesis.

To disentangle these predictions about the nature of the relationship between the relative pay ratio and nonprofit performance, I begin by estimating the following two equations:

$$\begin{aligned} Log(Relative\ Pay)_{it} = {} & \beta_0 + \beta_1\, Organizational\ Performance_{it} + \\ & \beta_2\, Governance\ Index_{it} + \\ & \textstyle\sum_{j=3}^{n} \beta_j\, Other\ Determinants_{it} + \\ & \textstyle\sum_{k=1}^{n} \gamma_k\, Industry_k + \sum_{t=1}^{n} \delta_t\, Year_t + \varepsilon_{it} \end{aligned} \tag{1}$$

$$\begin{aligned} Organizational\ Performance_{it} = {} & \alpha_0 + \alpha_1\, Log(Relative\ Pay)_{it} + \\ & \alpha_2\, Governance\ Index_{it} + \\ & \textstyle\sum_{k=3}^{n} \alpha_k\, Other\ Determinants_{it} + \\ & \textstyle\sum_{k=1}^{n} \gamma_k\, Industry_k + \sum_{t=1}^{n} \delta_t\, Year_t + \varepsilon_{it} \end{aligned} \tag{2}$$

where industries are based on the NTEE's 26 major groupings. Both models include year effects and, consistent with the empirical recommendations of Gormley and Matsa (2014), both specifications include industry effects.[16] *Organizational*

[16] Gormley and Matsa (2014) demonstrate that the fixed effects estimator produces consistent estimates and is empirically superior to alternative approaches such as industry-adjusting (e.g., deducting the mean from) the dependent variable.

Performance is one of four measures of performance: (1) the ratio of program expenses to total expenses ("Program Spending Ratio"), (2) the ratio of program expenses to total assets ("Program Expenses to Assets"), (3) the ratio of net fundraising revenue (fundraising revenue net of fundraising expenses) to total fundraising revenue ("Fundraising Ratio"), and (4) (the log of) the ratio of total revenue per employee ("Log(Revenue per Employee)").[17] The Program Spending Ratio has been utilized as a measure of organizational performance in multiple related studies (e.g., Aggarwal et al. 2012; Baber et al. 2002). The ratios Program Expenses to Assets and Fundraising Ratio are based on performance metrics utilized by Desai and Yetman (2006). Log(Revenue per Employee) builds upon extant nonprofit literature (e.g., Pauly and Redisch 1973; Shen 2003) as well as a study in the for-profit literature by Faleye et al. (2013).

The models include control variables that, based on prior literature, have the potential to impact executive compensation, organizational performance, or the relation therein. For example, both models control for organizational size (the number of employees and total assets) which is known to affect both executive pay (e.g., Aggarwal et al. 2012) and organizational performance (e.g., Garner and Harrison 2013). Both models also control for liquidity, as an abundance of liquid resources can complicate nonprofit manager-stakeholder relations (e.g., Core, Guay,

[17] On the Form 990, a nonprofit can choose to allocate compensation expense to three classifications: (1) Program Service Expenses, (2) Management & General Expenses, and (3) Fundraising Expenses. Because some of these expense totals are included in the performance variables (e.g., Program Service Expenses in the *Program Spending Ratio* and *Program Expenses to Assets* measures, as well as Fundraising Expenses in the *Fundraising Ratio* measure), I avoid including compensation expense in both the dependent and independent variables of the regression models by presenting all performance variables net of CEO compensation expense. See Appendix A for detailed definitions.

and Verdi 2006); government funding for its ability to serve as a managerial monitoring mechanism (e.g., O'Regan and Oster 2002); the size of the board of directors since smaller boards may make more effective monitors (e.g., Yermack 1996); to control for the entity's growth prospects, the age of the nonprofit (e.g., Garner and Harrison 2013); the proportion of fixed assets to total assets as a measure of asset tangibility and the entity's strategic flexibility (e.g., Gopalan, Milbourn, and Song 2010); donations growth to account for the impact of donor influence on both pay and performance (e.g., Balsam and Harris 2014); and an indicator variable for whether at least 90% of the nonprofit's revenues originate from program service activities (e.g., Aggarwal et al. 2012). Additional control variables exclusive to equation 1 include CEO tenure, since longer-standing CEOs may receive greater compensation (e.g., Cheng 2004); CEO gender to control for any pay differential across males and females (e.g., Brick, Palmon, and Wald 2006); and median state-level household income to control for regional differences in pay levels (e.g., Deng and Gao 2013). Additional control variables exclusive to equation 2 include two variables for financial reporting quality: an indicator variable for whether the nonprofit reported zero fundraising expenses (e.g., Krishnan, Yetman, and Yetman 2006; nonprofits have a tendency to shift fundraising expenses to program expense classifications), and the ratio of total liabilities to total assets (e.g., Vermeer, Edmonds, and Asthana 2014; higher leverage incentivizes nonprofits to misreport their financial statements in an effort to appease lenders). Detailed definitions of all variables are provided in Appendix A.

Regarding equation 1 (2), β_1 (α_1) measures the relationship between nonprofit performance and CEO pay. The Pay-for-Performance Hypothesis implies a positive β_1 coefficient. The Agency-Governance Hypothesis predicts a negative relationship between organizational performance and CEO pay, i.e., a negative β_1. In the context of equation 1, the Agency-Governance Hypothesis also implies a negative relationship between relative CEO pay and governance quality, though imperfect measurement of true governance quality is likely to produce a β_2 coefficient that is approximately zero. [18, 19] To summarize, the Pay-for-Performance (Agency-Governance) Hypothesis implies a positive (negative) pay-performance relationship.

B. Endogeneity of CEO Pay and Nonprofit Performance

As implied by the model specifications in equations 1 and 2, both relative pay and organizational performance are likely endogenous. Indeed, according to the Agency-Governance Hypothesis, both are impacted by governance quality and, according to the Pay-for-Performance Hypothesis, both are impacted by CEO skill or effort. For example, according to the Agency-Governance Hypothesis, unobservable

[18] Note that these predictions acknowledge the imperfect nature of the governance index (arising due to non-quantifiable aspects of governance quality); if instead the governance index measured nonprofit governance quality perfectly and sufficiently, the Agency-Governance Hypothesis would imply a β_1 coefficient that is approximately zero and a β_2 coefficient that is negative in value, as the governance index would capture the full effect of governance on relative pay. However, realistically speaking, any governance index is imperfect and, given the Agency-Governance Hypothesis's expectation of higher relative pay in poorly governed nonprofits, the prediction becomes a negative β_1.

[19] An alternative prediction about the relationship between relative CEO pay and governance quality stems from an empirical finding in the for-profit literature. The argument states that intense monitoring is associated with lower executive pay levels since shareholders will see less of a need to compensate a CEO who is already held accountable by way of the company's strong governance mechanisms (Acharya and Volpin 2010). These employment conditions would be viewed negatively by the CEO, and could encourage him to bargain for higher compensation, implying a potentially positive link between relative CEO pay and governance quality.

aspects of governance quality, such as the quality and motivation of individual board members, may affect both executive compensation and organizational performance in that one could expect good governance quality to mitigate high levels of CEO pay *and* promote organizational performance. Relatedly, under the Pay-for-Performance Hypothesis, the talent and motivation levels of a CEO likely impact both pay and organizational performance.

In the context of equation 1 where nonprofit performance is treated as endogenous, I use as an instrumental variable (IV) the analogous performance measures at nonprofits that are similar in size, industry, and location. Similarly, for equation 2 and treating CEO compensation as endogenous, I utilize as an IV the executive compensation levels of nonprofits that are similar in size, industry, and location. Unobservables, such as the quality and motivation of the CEO and board members, should impact both pay and performance of the nonprofit in question but have little impact on other nonprofits. At the same time, pay and performance at individual nonprofits should be correlated with those of similar sized nonprofits in the same industry and geographic area.

Literature on information transmission among nonprofit organizations suggests that social norms and institutional pressures prompt nonprofits to pattern their behaviors after those of local peer firms (Zorn, Flanagin, and Shoham 2011). An emerging body of empirical evidence supporting behavioral clustering, although currently confined to for-profit entities, attributes the tendency to greater ease and reduced cost in acquiring valuable information (e.g., Dougal, Parsons, and Titman

2015; Kedia and Rajgopal 2009; Pirinsky and Wang 2006). With regard to spillovers in organizational performance, Engelberg, Ozoguz, and Wang (2013) find stronger co-movement in the earnings and investment levels of companies located in the same industry and geographic area. As for executive compensation, Engelberg, Gao, and Parsons (2013) document a positive relationship between the compensation to a CEO relative to their local competitors. The importance of geography in establishing executive compensation levels is also demonstrated by Bouwman (2013). In constructing the IVs, the number of sample splits on size (industry) [region] is 3 (8) [9], as detailed in Panel 3 of Appendix A. In each case the size-industry-region IV is calculated excluding nonprofit *i*.[20]

C. Determinants of CEO-to-Employee Relative Pay

Table 3 reports IV estimates of equation 1 where (the log of) the CEO-to-employee relative pay ratio is regressed on its traditional determinants, measures of nonprofit performance (with instruments as described above), and the governance quality index, in addition to industry and year fixed effects. The table includes separate regressions for each of the four different measures of organizational performance considered. For all instrumental variables estimations of equations 1 and 2, the Hausman tests reject the hypothesis of exogeneity (p-values < 0.05). Of primary importance among Table 3 results is the negative relation between all four measures of organizational performance and the CEO-to-employee relative pay ratio (p-values < 0.0001 in three of the four specifications). These results lend support to

[20] Least squares estimations of equations 1 and 2 are available from the author upon request.

the Agency-Governance Hypothesis in that poor performing nonprofits are also characterized by high levels of relative pay, implying weak governance and ineffective monitoring mechanisms.

Table 3 also provides evidence that good governance quality may alleviate high levels of executive pay, with statistically significant negative coefficients documented in two of the four specifications (p-values < 0.05). In terms of economic significance, from column I, a single standard deviation improvement in the governance index (0.096) is associated with a relative pay ratio that is 1.3% smaller in magnitude. Additionally, the results indicate a non-linear relation between the relative pay ratio and board size, suggesting that CEOs of nonprofits having larger boards are associated with higher relative pay ratios. The coefficients on board size (positive) and its squared term (negative) imply that the increase in relative pay as a function of board size occurs at a decreasing rate but is positive for all reasonable board sizes.

Table 3 identifies several other important determinants of relative CEO pay. For example, the relative pay ratio is positively related to (the log of) the number of employees, (the log of) total assets, and (the log of) CEO tenure, and is negatively related to government revenue, (the log of) median state-level household income, and an indicator variable set to 1 if the CEO is a female. The positive association between CEO pay and organizational size supports a widely documented finding in the related nonprofit literature (e.g., Aggarwal et al. 2012; Frumkin and Keating

2010; Hallock 2002).[21] The result that CEO pay is increasing in CEO tenure suggests that entrenched, long-standing CEOs may be able to extract higher compensation or, alternatively, could represent additional pay for experience. One possible explanation for the negative association between relative pay and government revenue is that greater reliance on the government for resources corresponds to closer monitoring, and thus more reasonable CEO compensation levels. The negative association between relative pay and median household income is likely mechanical: holding CEO pay constant, as average worker pay (and income) increases, the relative pay ratio decreases. The (female) gender indicator variable enters with a negative and significant coefficient, possibly because women are more willing to accept lower paying positions when operating in a charitable environment or that female CEOs are more generous than other staff members.

In sum, the results in Table 3 support the Agency-Governance Hypothesis, particularly the negative and significant associations between CEO pay and both nonprofit performance and governance quality. These results are consistent with agency and governance problems in many nonprofits.

D. Determinants of Nonprofit Performance

The results presented in Table 3 indicate that highly-compensated nonprofit CEOs are associated with both inferior organizational performance and poor

[21] To ensure that the size-pay relationship is properly modeled, I estimate several variants of equations 1 and 2 by replacing the logarithmic size variables *Log(No. of Employees)* and *Log(Total Assets)* with (1) total assets and its squared term along with employees and its squared term, and (2) total assets and its inverse along with employees and its inverse. The main models with logarithmic size variables provide the highest explanatory power of all alternative specifications.

governance quality. The beneficial impact of good governance quality in moderating high compensation levels motivates an important follow-up question: do better-governed nonprofits out-perform their poorly-governed peers?

Table 4 provides IV estimates corresponding to equation 2 where four different measures of nonprofit performance are regressed on their traditional determinants, (the log of) the relative pay ratio, as well as the governance index, in addition to industry and year fixed effects. Similar to Table 3, this table includes a separate column for each of the four different measures of organizational performance. A statistically negative relationship is observed between the relative pay ratio and all four measures of organizational performance (p-values < 0.01). In terms of economic significance, a one standard deviation increase in (the log of) the relative pay ratio (0.856) is associated with a 19.2% smaller Program Spending Ratio. In sum, the results provided in Tables 3 and 4 demonstrate a strong and consistent inverse relation between pay and performance, suggesting that both CEO compensation and nonprofit performance are subject to agency and governance problems. I acknowledge that even after instrumental variables estimations, some endogeneity likely remains in that governance and pay or performance depend on the same unobservable variables though most sources of endogeneity imply a positive bias while I find evidence of a negative relation.

For the remainder of the paper, I focus on a single measure of performance: the program spending ratio. One reason to prioritize this performance metric over competing alternatives is for its conceptually appealing qualities. Specifically,

nonprofit organizations are expected to fulfill a charitable mission, the core expenses for which are captured in program expense categories. Thus, it is reasonable to conclude that a nonprofit devoting a high proportion of its total spending towards charitable causes (relative to administrative or fundraising alternatives) is directing its resources towards activities that will further the organization's charitable mission, although spending practices do not necessarily imply a high level of (non-financial) organizational effectiveness and efficiency. Another reason to focus on the program spending ratio is a matter of precedence, given that it is a widely-accepted organizational performance metric utilized in related literature (e.g., Aggarwal et al. 2012; Baber et al. 2002).

E. The Impact of Governance on Relative Pay surrounding Changes in Organizational Performance

The empirical results presented thus far suggest that CEO pay is negatively related to nonprofit performance, yet good governance quality helps to offset high relative pay ratios. Next, I examine the relation between changes in relative pay and changes in charitable spending for different levels of governance quality. That is, I analyze whether governance quality counteracts the negative relation between relative pay and organizational performance. To offset the loss of observations accompanying a changes specification, CEO turnover years are included and their effect captured by a *CEO Turnover Dummy* indicator variable. Table 5 reports the results. Although changes in program spending and changes in the relative pay ratio are negatively related, I find that – relative to their poorly-governed peers – CEOs of

well-governed nonprofits realize incrementally and significantly higher relative pay in response to increases in charitable spending levels (p-value < 0.0001). This finding lends further credibility to the notion that adopting strong governance mechanisms translates into a more efficient pay-performance relation.

F. Executive Perquisites, Nonprofit Performance, and Governance

The results presented in this paper support a negative relationship between CEO pay and both governance quality and organizational performance. How do other types of executive compensation relate to nonprofit performance and governance? In this section, I analyze the relationship between executive perquisites, organizational performance, and governance quality. One aspect of the revised Form 990 is more detailed information on executive compensation. For example, the first question on Schedule J ("Compensation Information") asks whether the organization provided perquisites to any officer, director, trustee, key employee, or highly compensated employee. Among the eight listed perquisite categories are first-class or charter travel, travel for companions, a discretionary spending account, and housing allowances, to name a few. Note that the Form 990 does not require information on the dollar value of such perquisites, which inhibits any analysis on the costs attached to certain perquisites (such as first-class travel) relative to others or their evolvement through time (Yermack 2006).

To begin, I estimate a multivariate logit model predicting the probability of at least one type of perquisite being awarded to an executive in a given year.

Subsequently, I consider the probability that at least one executive is awarded first-class or charter travel – a specific and luxurious type of perquisite that is conceivably highly sensitive to governance quality. In both models, the independent variables of interest include (1) an indicator variable for superior nonprofit performance (specifically, reporting a program spending ratio above the annual industry median), (2) governance quality, captured by the governance index, and (3) the interaction of the variables described in (1) and (2). The models control for traditional determinants of the relative pay ratio, where determinants are those referred to in equation 1 with baseline results in column I of Table 3.

Columns I and II of Table 6 present the results of the logit regression predicting the probability that at least one type of perquisite is awarded, while columns III and IV report results predicting the probability of first-class or charter travel. The direction and significance of the coefficients of interest are comparable between the two models. The indicator variable capturing superior nonprofit performance enters with a negative and statistically significant coefficient in three of four specifications (p-values < 0.05). This result suggests that superior organizational performance (and effective monitoring mechanisms) is inversely related to the likelihood of receiving executive perquisites, which supports the earlier result of a negative relation between nonprofit performance and reportable compensation. The governance index enters with a negative and significant coefficient in all four specifications (p-value < 0.0001), indicating that well-governed, effectively monitored firms are less apt to award perquisites to managing

executives. Of particular importance is the interaction term between these two variables, which enters with a positive and highly statistically significant coefficient (p-value < 0.0001). This latter result suggests that, although high-performing and well-governed firms are *individually* less likely to award executive perquisites, good governance quality is associated with an *incremental* but significant *increase* in the likelihood that executives of high-performing nonprofits will be rewarded with perquisites (relative to their poorly-governed peers).

I also investigate the relationship between the *variety* of executive perquisites, nonprofit performance, and governance quality. Specifically, I estimate a multivariate ordered probit model relating the number of perquisites awarded during the year (i.e., the sum of perquisite indicators) to the same independent variables. Results, reported in columns V and VI of Table 6, include coefficients that enter with signs and statistical significance levels that are very similar to those corresponding to the logit models. Specifically, I find that the variety of executive perquisites decreases in cases of superior nonprofit performance and in cases of good governance quality, but that a well-governed nonprofit is associated with an incremental but significant *increase* in a high-performing nonprofit organization's variety of executive perquisites (relative to their poorly-governed counterparts). In other words, the negative perquisite-performance relation breaks down in the presence of strong governance. Collectively, the results in Table 6 further support a negative association between managerial compensation and nonprofit performance that is incrementally impacted by the organization's governance quality.

G. The Impact of Governance Quality on Extreme Values of Relative Pay and Nonprofit Performance

To summarize, the results of this paper demonstrate a strong negative relation between nonprofit CEO pay and organizational performance. Opposite to the Pay-for-Performance Hypothesis and most evidence from the for-profit literature, higher relative pay is associated with poorer, not better performance. This suggests that both high pay and poor performance are associated with agency issues which governance at nonprofits fails to correct. As further support of this view, I find that relative pay is also negatively associated with an index of nonprofit governance quality. To this point the analysis has employed standard regressions which measure how the *conditional means* of pay and performance are impacted by the governance index and other factors. If the aforementioned negative relation between pay and performance is associated with agency concerns, it is reasonable to expect the impact of governance quality on both pay and performance to be incrementally greater at extreme values of these variables. In other words, one would expect governance quality to play a particularly important role in reducing extremely high CEO compensation and in avoiding extremely poor nonprofit performance. To test this implication, I re-estimate equations 1 and 2 using quantile regressions with instrumental variables constructed as before. Quantile regressions represent a convenient method of estimating conditional quantile models, as opposed to least squares estimations which estimate the conditional mean (e.g., Koenker and Hallock 2001).

To measure the impact of governance mechanisms on extremely high CEO pay levels, I re-estimate equation 1 using a quantile regression specification where the effects of organizational performance and governance quality on compensation are estimated at the 80th percentile of the distribution of the CEO-to-employee relative pay ratio. In an effort to alleviate endogeneity concerns, instrumental variables are employed throughout the analyses. Results, presented in Table 7, indicate a negative pay-for-performance link for these extreme values of CEO compensation. More interesting is the incrementally larger impact of governance quality on CEO compensation, with coefficients that are larger in both magnitude and significance when compared to the corresponding instrumental variables results reported in Table 3. For instance, in the regression where performance is measured using the program spending ratio, the coefficient of the governance index is –0.433 in Table 7 versus –0.138 in Table 3. Therefore, while strong governance appears somewhat effective at restraining compensation at the average nonprofit, it appears much more effective at restraining extremely high compensation levels.

In quantifying the impact of governance mechanisms on extremely low nonprofit performance levels, I re-estimate equation 2 using a quantile regression specification where the effects of CEO pay and governance quality on nonprofit performance are estimated at the 20th percentiles of each of the four distributions of organizational performance. As before, these regressions are estimated using IVs. Results, provided in Table 8, again support a link between high levels of pay and low levels of performance.

It may prove informative to analyze which one or more of the four sub-indices of the governance index may be driving the results documented in Tables 7 and 8. To address this question, I re-estimate the quantile regression specifications corresponding to equations 1 and 2 replacing the *Governance Index* with its sub-indices, i.e., *Governing Body*, *Governing Policies*, *Compensation Policies*, and *Accountability & Transparency* (described in detail in Panel 2 of Appendix A). Results are provided in Table 9 (estimates of equation 1) and Table 10 (estimates of equation 2). While the coefficients of all four governance sub-indices are virtually all negative in the four regressions in Table 9, the *Governing Board* and *Accountability & Transparency* sub-indices appear especially important in constraining very high levels of relative pay. Turning to Table 10 where organizational performance at the 20th percentile is modeled as a function of the governance sub-indices, the results provide relatively little conclusive evidence on how each governance sub-index relates to nonprofit performance, with the *Compensation Policies* entering with a positive and significant coefficient in two of the four specifications and both the *Governing Board* and *Accountability & Transparency* sub-indices entering with negative and significant coefficients in three of the four performance measures. Taken as a whole, the empirical results in this section provide evidence that the negative pay-for-performance link stands firm at extreme values of both CEO pay and organizational performance, with governance quality playing a particularly important role. Collectively, these findings lend support to the Agency-Governance Hypothesis.

H. Nonprofit Executive Pay, Organizational Performance, and Governance as a Function of Nonprofit Revenue Structures

It is reasonable to expect the relation between nonprofit pay, performance, and governance to differ as a function of a nonprofit's revenue structure. Specifically, a nonprofit that is constrained by relying on a single source of income will likely operate in a more competitive environment and, in response, attempt to be a better steward of available resources. This shift in behavior could plausibly affect the nature of the relationship between relative pay and organizational performance. To address this possibility, I re-estimate the main empirical specifications of this paper for sub-samples of "commercial" and "traditional" nonprofit organizations. "Commercial" nonprofits are defined as those with at least 90% of revenues from program services (e.g., Aggarwal et al. 2012; Steinberg 2004).[22] In contrast, "traditional" nonprofits operate on a greater variety of revenue sources such as donations, government grants, and rental income. The commercial versus traditional nonprofit classification tends to cluster along industry lines. For example, the hospital industry is dominated by commercial nonprofits (87%), while traditional nonprofits dominate the arts and public benefit industries (95% and 81%, respectively). On average, commercial nonprofits tend to be larger compared to traditional nonprofits in terms of the number of employees (mean for commercial = 1,303; mean for traditional = 450), total revenue (mean for commercial = $138.9 million; mean for traditional = $46.8 million), and total assets (mean for commercial

[22] Formally, this metric is defined as the ratio of Program Services Revenue to the sum of Program Services Revenue and Donations Revenue.

= $225.0 million; mean for traditional = $172.6 million). As for compensation, CEOs of commercial nonprofits benefit from a higher average salary (mean = $419,317; median = $278,241) relative to the average pay for CEOs of traditional nonprofits (mean = $234,715; median = $180,664).

Earlier regression specifications incorporated an indicator variable, *Commercial Dummy*, to control for potential differences in the revenue structures of nonprofit organizations. As another way to check whether the negative pay-for-performance relation holds for commercial versus traditional nonprofits, I re-estimate equations 1 and 2 after splitting the sample into commercial and traditional sub-groups. Instrumental variables are again used in all estimations. Results, provided in columns I and II (equation 1) and III and IV (equation 2) of Table 11 demonstrate a consistently negative association between the relative pay and program spending ratios, and one that is consistently statistically significant for commercial nonprofits. Additionally, governance appears to play a more prominent role in moderating the pay levels of traditional nonprofits' CEOs. This result may be driven by differences in the competitiveness of commercial and traditional nonprofits; specifically, the higher level of competition native to commercial nonprofits may discipline the actions of managers, making internal governance relatively less important. In sum, the results suggest that the negative pay-for-performance relation applies to commercial and traditional nonprofits alike, lending further support to the Agency-Governance Hypothesis. [23]

[23] In untabulated robustness checks, I find that the results associated with equations 1 and 2 are robust to (1) using *excess* relative pay in place of its level and (2) incorporating state fixed effects, given

I. Limitations

1. Proper Measurement of Managerial Incentives

A limitation native to all empirical studies of managerial incentives is that compensation is merely the output of a remuneration system which we cannot directly observe. To the extent that remuneration is contingent on financial performance, observed pay should be positively related to performance. Thus, the negative pay-performance link documented in this paper indicates an even more negative relation between a priori pay and performance.

A potential concern specific to studies utilizing a relative compensation measure (such as the CEO-to-employee relative pay ratio used in this paper) is on the interpretation of changes in relative pay as a function of changes in financial performance. Specifically, a negative pay-performance relation could arise if the CEO's pay is fixed but ordinary workers' compensation depends on performance. However, the majority of existing literature (e.g., Brickley and Van Horn 2002; Jobome 2006) and anecdotal evidence (e.g., Hancock 2013; Pogrebin and Taylor 2010) focuses on how executive compensation (and not that of ordinary workers) is contingent on organizational performance, thereby inducing a positive pay-performance relation in most cases.

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that some U.S. states have instituted mandatory governance standards (Desai and Yetman 2006). Note that in (2), the inclusion of household income as an independent variable in equation 1 measures the time series impact of variations in state income. Instrumental variables are used for these estimations. Additionally, I find qualitatively similar results when using quantile regressions estimated at the median. Results are available upon request.

2. Proper Measurement of Nonprofit Performance

An inevitable complication of any empirical study of the nonprofit sector is that there is no measure of performance that is uniformly applicable to a broad set of nonprofit organizations. This impediment is particularly relevant for inter-industry settings such as the sample employed in this paper. In an attempt to address this concern, I use multiple measures of (financial) performance, including the charitable spending ratio which is commonly used in related literature (e.g., Aggarwal et al. 2012; Baber et al. 2002; Sedatole et al. 2013). The reliability of these financial performance measures depends in part on financial reporting quality; specifically, a nonprofit manager's decision to misreport financial statements can render financial performance measures misleading (by, for example, recording zero fundraising expenses; see Krishnan et al. 2006). As a means to address this concern, in untabulated tests I find qualitatively comparable results when re-estimating the main models (equations 1 and 2) for observations where the Form 990 was audited by an independent financial accountant.

3. Proper Measurement of Governance Quality

The measure of governance quality used in this paper is defined as an index of mechanisms pertaining to the nonprofit's governing body, governing policies, compensation policies, and accountability and transparency. As mentioned previously, quantitative measures of governance serve as proxies for *true* corporate governance quality. Moreover, inferring the hypothesized impact of some

governance provisions requires assumptions that may not hold for all nonprofits or in all circumstances; for example, the decision to delegate managerial duties to an outside company may weaken the governance quality of some nonprofits, though in this paper it is assumed to strengthen governance for the majority of organizations. Although these weaknesses are not specific to my setting, in untabulated sensitivity tests I address the external validity of the governance index by examining its relation to additional measures of accountability and transparency culled from organizations' websites by Charity Navigator. These metrics include whether the nonprofit's website provides a donor privacy policy, audited financial statements, its most recent Form 990, a list of its board members, and a list of its staff members – all metrics which should indicate better governance if answered in the affirmative. I find that the mean governance index score is higher for four of the five metrics, with the difference being statistically significant in three of the four cases.

4. Other Inefficient Pay-Performance Arrangements

The primary hypotheses of this paper offer a set of competing predictions on the directional link between nonprofit executive pay and organizational performance: (1) positive, where hard-working managers are rewarded for their optimizing organizational performance; or (2) negative, where managers extract high levels of relative pay while realizing inferior organizational performance. Moreover, these predictions are proposed from the standpoint of their interaction with agency concerns and corporate governance quality. However, if nonprofit managers are on

average motivated by utility gained through altruism rather than that arising from compensation, one could imagine that high pay levels could actually discourage an executive from fulfilling the organization's performance goals. Thus, for a nonprofit manager that is highly intrinsically motivated, less compensation is better than more. This case predicts a positive (negative) pay-performance link for low (high) levels of pay, but for a different reason than that proposed by the Pay-for-Performance (Agency-Governance) Hypothesis. Although altruistic motives are difficult to observe or to quantify, an abundance of anecdotal evidence (discussed earlier) pointing to nonprofit executives' desires to maximize the amount of pecuniary benefits (e.g., Buettner 2011; Evans 2012; Hancock 2013) runs counter to this alternative explanation.

V. Conclusion

Allegations of grossly overpaid nonprofit CEOs have become increasingly common, yet whether high executive pay levels are typically established in an optimal or sub-optimal manner remains an unanswered question. Specifically, a capable and hard-working nonprofit CEO may be rewarded for achieving optimal organizational performance. In contrast, in a nonprofit marred by agency conflicts and poor governance, an incompetent, lazy, or dishonest CEO may prioritize their own interests before those of their organization, resulting in both poor organizational performance and high relative pay. In an effort to disentangle these predictions, I investigate whether high levels of nonprofit CEO pay are associated with superior or

inferior nonprofit performance. In addition, I examine how CEO pay and nonprofit performance relate to an index of nonprofit governance based on new data.

After accounting for other determinants of pay and performance, I document a statistically significant and economically meaningful negative relation between CEO-to-employee relative pay and organizational performance at large nonprofits. I further find that relative pay is negatively related to an index of the quality of the nonprofit's governance. Similarly, management consumption of perquisites is negatively related to both performance and governance quality after controlling for other factors. I also find that extremely high relative pay is strongly associated with poor governance, and that governance quality plays a more prominent role in moderating the relative pay levels of nonprofits that are not bound by the disciplinary forces accompanying intense product market competition. Finally, I show that although changes in the relative pay ratio and changes in charitable spending are negatively related in general, the CEO of a nonprofit that is well-governed realizes as incremental but statistically significant increase in relative pay pursuant to increases in charitable spending levels. These results point to serious agency conflicts and governance issues at large nonprofits such that weak governance tends to lead to both poor performance and high relative pay.

The results in this paper demonstrate the potential for good governance quality to alleviate the undesirable consequences that tend to accompany cases of ineffective and/or insufficient monitoring mechanisms. Although this conclusion is based on a sample of organizations particularly susceptible to especially severe

agency conflicts, it is not unique to the nonprofit organizational form. Rather, the beneficial properties of good governance quality documented in this paper extend to for-profits and nonprofits alike, thus providing a basis for all organizational types to implement strong governance mechanisms as a means to ward off the undesirable consequences of firm-specific agency conflicts**.**

Chapter 2

Cost Shifting in Nonprofit Hospitals[24]

I. Introduction

Over the past several years there has been growing concern over the affordability of health care in the United States. Health care costs have risen dramatically and some question the quality of care offered to those that are uninsured or underinsured. Despite recent efforts to address these concerns (e.g., the Patient Protection and Affordable Care Act of 2010), the sustainability of government-related health care programs remains uncertain (e.g., Alonso-Zaldivar 2010, Kaiser Family Foundation 2008).

Nonprofit hospitals play an interesting role in the health care debate. By receiving tax-exempt status through their nonprofit status, they save billions in tax-related costs. These savings are expected by politicians and other groups to result in greater community benefits, thereby addressing the high-cost health care crisis. However and perhaps surprisingly, nonprofit hospitals are often more profitable than their for-profit counterparts but provide only a similar level of community benefits through charity care. As a result, many question whether the cost of their tax-exempt status outweighs the realized benefits. In fact, to the extent that their tax-exempt status represents an inefficient allocation of scarce resources (e.g., excessive CEO

[24] This chapter is based on collaborative work with Wayne B. Thomas.

salaries), nonprofit hospitals may further exacerbate the problems associated with rising health care costs.

Given the intense scrutiny from the media, donors, regulators, politicians, and third-party payers, nonprofit hospitals reporting seemingly high profits may wish to mask their allocation of resources. One means by which managers of nonprofit hospitals can attempt to disguise these profits, and thereby avoid such scrutiny, is shifting costs from non-patient-centered (hereafter, "non-core") to patient-centered (hereafter, "core") activities. By shifting costs in this manner, core profitability decreases while non-core expenses decrease. Thus, the hospital creates the appearance that it is better fulfilling its charitable mission and patient-centered focus, while providing services at affordable prices. We expect that when a nonprofit hospital reports bottom-line earnings well above the zero-profit benchmark, it is more likely to shift costs to core activities directly related to patient services.

We also examine hospitals' cost shifting behavior when they report bottom-line earnings below the zero-profit benchmark. Similar to for-profit organizations, when a nonprofit hospital reports earnings below the zero-profit benchmark, it risks facing the inability to continue as a going concern. Such a situation likely creates both compensation and employment concerns for managers. Losses may also create additional criticisms from the community that managers are using resources inefficiently by spending excessively on non-patient-related expenses. Thus, hospitals reporting below the zero-profit benchmark may have similar incentives to

shift expenses from non-core activities to core activities, reducing profits on patient-related services and reducing costs of non-patient-related services.

Given these reporting pressures faced by nonprofit hospitals, it is not surprising that prior research has detected earnings management in nonprofit hospitals through the management of accruals and use of real activities to avoid reporting earnings that fall below or well above the zero-profit benchmark (e.g., Leone and Van Horn 2005; Eldenburg, Gunny, Hee, and Soderstrom 2011). We consider that hospital managers may also engage in an additional form of earnings management through classification shifting (e.g., McVay 2006; Fan, Barua, Cready, and Thomas 2010; Barua, Lin, and Sbaraglia 2010). Studies on classification shifting provide evidence that for-profit managers shift expenses *from* core activities to non-core activities to inflate core profitability. In contrast, we seek to provide evidence of whether nonprofit hospitals deflate core profitability by shifting expenses *to* core activities. Specifically, we examine whether nonprofit hospitals that report bottom-line net income below or well above the zero-profit benchmark are more likely to shift expenses from non-core to core classifications.

To examine cost shifting behavior, we consider nine types of nonprofit hospitals that are expected to be more likely to engage in this type of earnings management. These include hospitals that are rural, have a church affiliation, are part of a system, provide high charity care, have low Medicare utilization, have low Medicaid utilization, are not audited, have low external fiscal fees, or have high donations. These hospitals are more likely to shift because they face greater

normative pressures from the community (rural, church-related, system, or high charity care), have lower regulatory oversight (low Medicare or low Medicaid), have fewer accounting controls (no audit or low external fiscal fees), or rely more on external financing through donations (high donations).

We employ a sample of nonprofit, California hospitals over the period 2002 to 2010 and find evidence generally consistent with our expectations. When bottom-line net income is well above the zero-profit benchmark, nonprofit hospitals suspected of having more incentives and ability to shift costs from core expenses to non-core expenses tend to do so (p-value < 0.0001 for a two-tailed test). When profits are below the zero-profit benchmark, we find marginal evidence of shifting (p-value = 0.075 for a one-tailed test).

The tests above are based on an aggregate measure of the general tendency of hospitals to shift. To provide a potentially more informative test of the specific types of hospitals engaging in cost shifting, we examine individual hospital characteristics. This test has the advantage of offering more specific conclusions about cost shifting behavior but the disadvantage of individual measures potentially providing lower-power tests. When earnings are well above the zero-profit benchmark, we find statistically significant evidence of shifting costs from non-core to core activities for hospitals that are rural, provide high charity care, have low Medicaid utilization, or have high donations. When bottom-line net income is below the zero-profit benchmark, hospitals that have low Medicare utilization, have low Medicaid utilization, or have high donations engage in cost shifting.

Our results provide an interesting perspective on cost shifting behavior. As discussed briefly above, research in this area (e.g., McVay 2006; Fan, Barua, Cready, and Thomas 2010; Barua, Lin, and Sbaraglia 2010) typically motivates shifting behavior on incentives to *increase* core profitability. While these incentives are clear for for-profit organizations, we identify a situation where organizations face incentives to *decrease* core profitability. At both ends of the performance spectrum – when profits are below or well above the zero-profit benchmark – nonprofit hospitals face incentives to decrease core profitability (and decrease non-core expenses) by shifting non-core expenses to core expenses. Thus, we provide a non-linear prediction on cost shifting behavior in our setting that has not been investigated in prior research.

We also expect our findings to have broad relevance, given the relatively limited research on healthcare-related reporting issues. Health care is the largest industry in the U.S. but one that faces significant social, political, and economic challenges. In recent years, health care costs have risen dramatically, while more Americans become uninsured or underinsured. Some argue that health care coverage should be a right guaranteed to all by the government because doing so would improve public health, reduce overall health care costs, promote economic growth (by allowing more flexible employment and reducing the risk of personal bankruptcy), and increase social goods to the less fortunate. However, the sustainability of government-related health care programs is in serious question. Given that nearly one-third of the population is expected to need government-

assisted health care by 2030, the importance of understanding the performance and operating efficiency of health care providers is critical, yet remains relatively unexplored by accounting researchers.

Nonprofit hospitals are directly relevant to the health care debate because of their prominence in the health care industry and their tax-exempt status. Of the 630,000 beds in Medicare-certified community hospitals in the United States in 2003, 68 percent were located in nonprofit hospitals.[25] Thus, the operating activities of nonprofit hospitals have a material impact on our nation's health care system. Further exacerbating the impact of nonprofit hospitals on the nation's health care system is their exemption from various federal, state, and local taxes. Congress' Joint Committee on Taxation estimated tax benefits of nonprofit hospitals to equal nearly $13 billion in 2002 (Congressional Budget Office 2006b). To the extent that the tax savings are used to provide benefits to the community, this resource allocation may be justified as the best approach to maximizing social welfare. However, many politicians and other groups question whether, in fact, nonprofit hospitals adequately and efficiently provide community benefits sufficient to justify their tax-exempt status.

Nonprofit hospitals are most commonly criticized because of their seemingly high profits, extravagant upgrades to facilities, high executive compensation, and large expenditures on potentially unproductive non-core activities (e.g., research). Carreyrou and Martinez (2008) report that the combined net income of the 50 largest

[25] Sixteen percent were located in for-profit hospitals, and 15 percent were located in government (nonfederal) facilities (Congressional Budget Office 2006a).

nonprofit hospitals jumped nearly eight-fold to $4.27 billion between 2001 and 2006, and that 77% of all nonprofit hospitals are profitable while only 61% of for-profit hospitals are profitable. At the same time, the amount of charity care provided by nonprofit hospitals is virtually the same as that provided by for-profit hospitals, leading many to question where the tax savings are being spent. Some nonprofit hospitals have engaged in questionable reporting practices to justify their tax-exempt status. For example, St. Louis-based BJC HealthCare claims to have provided $1.8 billion in benefits to various communities in 2004. Closer examination reveals that BJC counted the salaries of its employees as community benefits. These salaries, including the CEO's $1.8 million compensation, accounted for $937 million of total community benefits, while actual charity care represented a meager $35 million.

To the extent that cost shifting occurs, nonprofit hospitals can mask inefficient allocation of scarce resources and mislead financial statement users, regulators, politicians, hospital donors, and others into believing the nonprofit hospital's tax-exempt status is warranted. Moreover, the effectiveness of existing legislation (e.g., the Patient Protection and Affordable Care Act) and all future legislation (e.g., on the sustainability of Medicare) at least partially depends on the (reported) financial performance of hospitals. We expect our findings to yield clarity on the quality of nonprofit hospitals' earnings by looking beyond bottom-line net income.[26]

[26] Zismer and Proeschel (2009, p. 88) write, "'Wall Streeters frequently comment on the quality of earnings for public companies. The quality of the earnings affects investors' and credit markets' perspectives on the value of companies. … Similarly, community-based, not-for-profit health care system management and boards should analyze the quality of earnings as they evaluate

In the next section we discuss reporting incentives of nonprofit hospitals, as well as prior research in this area. Section III presents our hypothesis related to cost shifting behavior when bottom-line net income is below or well above the zero-profit benchmark. In section IV, we outline the sampling procedure and present the research design of estimating the relation between unexpected core expenses and unexpected non-core expenses, which is our test of cost shifting behavior. The results are reported in section V, and conclusions are offered in section VI.

II. Background

A. Incentives to Manage Earnings in Nonprofit Hospitals

It is well-documented that the earnings benchmark for a nonprofit hospital is commonly thought to be zero (e.g., Eldenburg et al. 2011, Leone and Van Horn 2005). One obvious reason that nonprofit hospitals wish to avoid reporting repeated losses is that it will risk facing the inability to continue as a going concern. In addition, related to CEO compensation and job stability, Brickley and Van Horn (2002) find that both the compensation and turnover of nonprofit hospital managers are significantly related to the hospital's financial performance. Specifically, they find that the manager's compensation will be higher when profits are zero or slightly positive (instead of negative), while his risk of termination will be higher when profits are consistently negative. Moreover, if hospital donors become aware of such mismanagement of resources via the media or otherwise, they may withhold their

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performance. The reason? The bottom line alone (operating margin) does not tell the whole story."

donations.[27] The incentives of nonprofit hospitals to avoid losses are similar to those faced by for-profit organizations.

However, unlike for-profit organizations, nonprofit hospitals also have incentives to avoid high profits. High profits can generate a host of scrutiny from the media, hospital donors, regulators, politicians, and third-party payers. Media scrutiny on the basis of a nonprofit hospital's excess profits can devalue the entity's reputation. Hospital donors may deem a hospital unworthy of contributions and regulators may question the worthiness of a nonprofit hospital's IRS 501(c)(3) tax-exempt status should the hospital realize an excessively positive stream of profits. Some nonprofit hospitals have lost their tax-exempt status because of excessive profits and failure to report sufficient charity care.[28] Finally, third-party payers will also have leverage to place price pressure on nonprofit hospitals and negotiate reimbursement terms that are less favorable to the entity in cases of excessive profits. With the number and importance of these incentives in mind, we next present evidence of earnings management in nonprofit hospitals.

B. Evidence of Earnings Management in Nonprofit Hospitals

Perhaps the two studies most closely related to ours because of their focus on nonprofit hospitals' ability to manage earnings to maintain the zero-profit benchmark

[27] Frank, Salkever, and Mitchell (1990) document that when making donation decisions, donors take into account the level of the nonprofit hospital's earnings and also the amount of charity care being provided.

[28] For example, in *Provena Covenant Medical Center v. Department of Revenue*, the Illinois Supreme Court ruled in 2010 that because Provena Covenant Medical Center in Urbana provided charity care equal to less than one percent of its revenues, it should lose its tax-exempt status.

are Leone and Van Horn (2005) and Eldenburg et al. (2011). These studies focus on accrual manipulation and real activities management. We discuss these studies next.

First, consistent with findings in Hoerger (1991), Eldenburg et al. (2011) find that nonprofit hospitals manage earnings through altering real activities.[29] Specifically, they find that when nonprofit hospitals have pre-managed earnings below (well above) zero, they tend to decrease (increase) expenditures related to non-operating activities, while expenditures for core patient care activities remain unchanged. Thus, managers are willing to alter discretionary spending but not at the expense of patient care when performance is below expectations. They also find that when profits are well above zero, nonprofit hospitals tend to report fewer asset sales for gains, implying that managers are saving these gains for future periods.[30] Overall, their results are consistent with nonprofit hospitals managing their expenditures to better achieve the zero-profit benchmark.

Second, beyond real activities management, Leone and Van Horn (2005) predict and find that nonprofit hospital managers also use discretionary accruals (e.g., adjustments to the third-party-allowance, and allowance for doubtful accounts) to lower earnings towards zero. In so doing, nonprofit hospitals can avoid the stigma and associated costs of reporting "too much" income. They further find that these managers also manage accruals upward to avoid reporting losses. However, these

[29] Hoerger (1991) documents that relative to for-profit hospitals, the earnings of nonprofit hospitals are less variable and also less responsive to exogenous factors such as changes in government policy. He suggests that this evidence is consistent with hospitals' willingness to adjust discretionary spending to report earnings at (or just above) zero.

[30] An additional interesting analysis in their study is the incremental impact that a manager's pay-for-performance has on his willingness to alter reported performance through discretionary spending.

managers do not appear to manage accruals to avoid earnings decreases, which seems reasonable given that repeated earnings increases could place the hospital's tax-exempt status in jeopardy.

We consider that managers of nonprofit hospitals may engage in a third form of earnings management – cost shifting. That is, managers of nonprofit hospitals may have incentives to reclassify one expense as another type of expense (i.e., shift reported costs). While such earnings management does not alter reported bottom-line net income, it potentially affects financial statement users' perceptions of the hospital. The purpose of our study is to examine the extent to which managers of nonprofit hospitals shift expenses when bottom-line net income deviates from the zero-profit benchmark. To help motivate the likelihood of such earnings management behavior, we rely on studies which provide general evidence of cost shifting in nonprofit hospitals, although these studies do not investigate the zero-profit setting.

Krishnan and Yetman (2011) evaluate whether nonprofit hospital managers inflate program service expenses relative to fundraising and management & general expenses, where program service expenses can be described as those dedicated to any "activity of an organization that accomplishes its exempt purpose" (Internal Revenue Service 2010). Notably, they find more shifting in hospitals that face high normative pressures such as church affiliation, but low regulative pressures such as low Medicare revenues as a proportion to total revenue.[31] Eldenburg and Vines

[31] Krishnan and Yetman (2011) examine the cost shifting behavior of California nonprofit hospitals on the Form 990. As discussed in more detail later, we examine California nonprofit hospitals' Office

(2004) find that in response to a 1990 accounting rule change pertaining to the disclosure of uncompensated care, nonprofit hospital managers reclassified bad debt to charity care, a decision that forwent the hospital's ability to collect on the receivable but one that also potentially improved the hospital manager's reputation. This effect appeared to be most pronounced in high cash, low operating margin hospitals. We take the findings in these studies as evidence in favor of nonprofit hospital managers' incentive, ability, and tendency to shift expenses and hope to build on these results by examining the shifting behaviors of those hospitals whose earnings deviate from the zero-profit benchmark.

III. Hypothesis Development

In direct contrast with for-profit entities, when nonprofit hospitals perform well and realize significantly positive profits, nonprofit hospital managers have incentives to manage earnings downward. As discussed previously, some reasons for this decision include a motivation to avoid scrutiny from the media, politicians, and donors, to present at least the appearance of providing sufficient charity care that warrants 501(c)(3) tax-exempt status, and to avoid providing third-party payers the opportunity to negotiate reimbursement terms that are less favorable to the hospital.

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of Statewide Health Planning and Development (OSHPD) reports. We believe understanding cost shifting behavior on the OSHPD report will be particularly important. The OSHPD reports are the primary reports analyzed by California state auditors for assessing nonprofit hospitals' operations. In fact, examination of the Form 990 is not common and IRS audits of this form are extremely rare (United States Government Accountability Office 2002). Thus, regulators' focus on the OSHPD reports allows us to offer evidence of direct relevance to policy makers. Krishnan and Yetman (2011) make the explicit assumption that OSHPD reports will *not* be manipulated, thus motivating their examination of Form 990s. However, we find evidence of cost shifting behavior on the OSHPD reports in our zero-profit constraint setting.

At the same time, the nonprofit hospital cannot continue as a going concern should it report losses on a consistent basis.

To convince the users of financial statements that resources were used wisely and in conformity with the entity's charitable mission and patient-centered focus, we hypothesize that nonprofit hospital managers will "micro-manage" their income statement. In particular, we expect that the nonprofit hospital manager may choose to reclassify non-core expenses (e.g., general and administrative services) to core expenses (i.e., patient care, ambulatory, and ancillary services), thereby portraying reduced profitability in direct patient-related services. For hospitals well above the zero-profit benchmark, such cost shifting helps the hospital's appearance of providing affordable health care (i.e., not charging patients too much). For hospitals with profits below the zero-profit benchmark, reclassification of expenses from non-core activities to core activities allows managers to blame poor performance on the willingness of the hospital to provide high-quality (i.e., high expense) health care at low prices, instead of inefficiently using resources on non-core activities that tend to be unrelated to direct patient care. Thus, cost shifting by hospitals reporting earnings below or well above the zero-profit benchmark portrays the appearance that the hospital is achieving its overall mission of high-quality, affordable health care to the community, while using resources efficiently. Such shifting is also important personally to the manager for maintaining her favorable reputation (both professionally and within the community).

In summary, we predict that nonprofit hospitals whose earnings deviate from the zero-profit benchmark will engage in more cost shifting relative to those hospitals that meet the zero-profit benchmark, because those that deviate will want to project at least the appearance to financial statement users that the deviation occurred for "good reason" (i.e., for reasons in line with the nonprofit hospital's charitable mission and patient-centered focus).

We also have expectations on *which* hospitals are more likely to shift expenses. Below we discuss nine hospital factors, and in section IV we discuss how our research design incorporates these factors to provide potentially more informative tests of hospitals' cost shifting behavior when profits are below or well above the zero-profit benchmark.

A. Rural Hospitals

Rural hospitals serve a critical role as health care providers to less populated communities and, as a result, are highly visible. Rural hospitals have also proven especially susceptible to financial distress pursuant to regulatory changes, such as during the transition to Medicare's prospective payment system when the increase in costs was so extensive that some rural hospitals were forced to close (Li, Schneider, and Ward 2009). Moreover, the visibility of a rural hospital attracts the attention of donors, regulators, the media, and other stakeholders and results in greater pressure to meet expectations (e.g., Goodstein 1994; Julian, Ofori-Dankwa, and Justis 2008). Thus, we predict that rural hospitals are more likely to engage in cost shifting.

B. Church Hospitals

A church hospital must uphold religious-oriented standards in order to maintain its church affiliation. For example, Catholic Healthcare West (CHW), the largest Catholic-affiliated health care system in the western U.S., must comply with the Ethical and Religious Directives for Catholic Health Care Services as outlined by the United States Conference of Catholic Bishops (2001). CHW identifies "serving and advocating for our sisters and brothers who are poor and disenfranchised" as one of the three key areas for which the hospital dedicates its resources (Catholic Healthcare West 2011). Hence, compliance with church hospital directives and expectations results in added scrutiny. Moreover, church hospitals are also expected to operate efficiently and be of equal competitors with non-church hospitals (Scott and Davis 2007). Thus, we predict that church-related hospitals are more likely to engage in cost shifting.

C. System Hospitals

System hospitals are typically held accountable to many constituencies across multiple layers of horizontal and vertical integration. System hospitals appear to recognize and attempt to fulfill these normative expectations (Alexander, Weiner, and Succi 2000) while also maintaining a high level of community orientation (Proenca , Rosko, and Zinn 2000). Moreover, the higher reputation accompanying system hospital membership is accompanied by higher costs (Dranove and Shanley

1995) and generally no enhancement of financial performance (Tennyson and Fottler 2000). Thus, we predict that system hospitals are more likely to engage in cost shifting.

D. Charity Care Provision

Just as rural hospitals fulfill a critical role to less populated communities, hospitals that provide charity care offer access to those patients who would otherwise be unable to afford care. As such, the more charity care a hospital provides, the more it will be valued by the community and the more pressure it will face to operate efficiently. However, high levels of charity care will also place financial burdens on the hospital (Rosko 1999). As a result, communities will advocate for more charity care when a hospital is performing well financially, while physicians will prefer to invest residual profits in technology and not charity care (Eldenburg, Hermalin, Weisbach, and Wosinska 2004). While we predict that normative pressures associated with high levels of charity care will induce more cost shifting in hospitals who depart from the zero-profit benchmark, we acknowledge the existence of mitigating factors that might induce lesser or even the absence of shifting.

E. Medicare and Medicaid Utilization

Medicare is a national program that provides insurance to qualified elderly and disabled individuals. Medicaid (termed Medi-Cal in California) is a state-based program that provides insurance to those individuals and families with limited

means. Both the Medicare and Medicaid programs are administered by the Centers for Medicare & Medicaid Services. Hospitals that provide services to Medicare and/or Medicaid patients may be audited by the Office of Audit Services (OAS), a division of the U.S. Department of Health & Human Services. Moreover, Chang, Steinbart, and Tuckman (1993) find that a nonprofit hospital faces a higher probability of an OAS audit as the proportion of Medicare patients to total patients increases. If performed with appropriate diligence, these audits would have the effect of reducing the ability of hospitals to misclassify expenses for the purpose of cost shifting. Thus, we predict that hospitals with lower levels of Medicare and Medicaid utilization (and therefore subject to less regulatory oversight) are more likely to engage in cost shifting.

F. Audit and Professional Fees for Accounting Services

In the nonprofit industry, the use of an outside accountant has been shown to reduce the likelihood of expense misreporting (Krishnan, Yetman, and Yetman 2006). We propose that the lack of an independent financial audit for financial reports will be associated with more shifting. Similarly, we predict that lower fees paid to external professionals for fiscal services (e.g., general ledger accounting, credit and collection services) will be associated with more shifting. In both cases, we expect that the nonprofit hospital manager's ability to influence financial reports will be greater and hence, there will be more opportunity for cost shifting.

G. Donations Revenue

In many cases, nonprofit hospitals depend on public donations for sustainability and at times, even survival. Harvey and McCrohan (1988) find that donors are more willing to contribute to the nonprofit hospital if they perceive that it is operating efficiently. Moreover, because the financial statements of hospitals receiving donation revenue will generally be subject to additional scrutiny (United States GAO 2002, Weisbrod and Dominguez 1986), managers have more incentives to manage reported performance. By shifting non-core expenses to core expenses, the manager creates the appearance to the hospitals' donors that resources have been devoted to patient care at affordance prices, while costs related to non-patient activity have been minimized (i.e., the hospital is operating efficiently in achieving its charitable mission). Reducing core profitability may also facilitate fundraising by portraying the need for additional donations to maintain the hospital's current level of patient care without raising prices. We predict that hospitals with higher levels of donations are more likely to engage in cost shifting.

Our summary hypothesis is stated below:

Hypothesis: Nonprofit hospitals suspected of having greater incentives and ability to reclassify non-core (non-revenue-generating) expenses as core (revenue-generating) expenses will do so when their profits are below or well above the zero-profit benchmark.

By "suspected," we mean hospitals that are characterized as rural, church-related, part of a system, high charity care, low Medicare, low Medicaid, not audited, low external fiscal fees, or high donations.

IV. Sample and Research Design

A. Sample

All data are retrieved from California's Office of Statewide Health Planning and Development (OSHPD) website.[32,33] Financial statement data filed with the OSHPD must reconcile with hospitals' financial reports, which are prepared under U.S. Generally Accepted Accounting Principles. The income statement included in the OSHPD's Hospital Annual Financial data is reproduced in summary form in Figure 1. We note that the richness of OSHPD data has elicited their use in multiple prior analyses of the hospital manager's earnings management behavior (e.g., Eldenburg et al. 2011, Elshafie 2007, Krishnan and Yetman 2011). The sample includes church-owned and community hospitals registered as tax-exempt entities. An initial screen reveals 1,868 nonprofit hospital-year observations during the 2002-2010 period. We impose several exclusions which are generally consistent with Eldenburg et al. (2011) and Krishnan and Yetman (2011). First, we exclude specialty hospitals, psychiatric hospitals, and substance abuse hospitals due to their differing patient mix, services provided, and reimbursement structures relative to general hospitals. Next, we exclude hospitals that filed non-comparable financial reports as identified by the OSHPD. This set consists of Kaiser hospitals, long-term care emphasis hospitals, Shriner's hospitals, and state hospitals, among others. We also eliminate hospitals with fewer than 50 licensed beds due to the unique economic nature of small-bed hospitals (e.g., Abraham, Gaynor, and Vogt 2005; Balakrishnan

[32] See http://www.oshpd.ca.gov/HID/DataFlow/HospMain.html .

[33] As of October 2011, the OSHPD freely provides annual financial data report files dating back to fiscal years ending in 2003, with prior years' data dating back to 1976 available for purchase.

and Soderstrom 2006; Krishnan, Joshi, and Krishnan 2004). Finally, we eliminate 22 observations due to insufficient data. The final sample consists of 1,282 hospital-year observations and is detailed in Table 12.

B. Research Design

We propose a test of nonprofit hospitals shifting non-core expenses (*NONCORE_EXP*) to core expenses (*CORE_EXP*) when overall profitability is below or well above the zero-profit benchmark. To provide such a test, we need to estimate unexpected core expenses and unexpected non-core expenses. We first estimate expected *CORE_EXP* and expected *NONCORE_EXP* using the following models (hospital subscripts omitted).

$$\begin{aligned} CORE_EXP_t = {} & \alpha_0^C + \alpha_1^C CORE_EXP_{t-1} + \alpha_2^C ATO_t + \alpha_3^C \Delta REV_t + \\ & \alpha_4^C NEG_\Delta REV_t + \alpha_5^C CURRENT_t + \alpha_6^C DAYS_CASH_t + \\ & \alpha_7^C TEACHING_t + \alpha_8^C LnAGE_t + \alpha_9^C LnASSETS_t + \\ & \alpha_{10}^C CMI_t + \alpha_{11}^C HHI_t + \varepsilon_t \end{aligned} \quad (3)$$

$$\begin{aligned} NONCORE_EXP_t = {} & \alpha_0^{NC} + \alpha_1^{NC} NONCORE_EXP_{t-1} + \alpha_2^{NC} ATO_t + \alpha_3^{NC} \Delta REV_t + \\ & \alpha_4^{NC} NEG_\Delta REV_t + \alpha_5^{NC} CURRENT_t + \alpha_6^{NC} DAYS_CASH_t + \\ & \alpha_7^{NC} TEACHING_t + \alpha_8^{NC} LnAGE_t + \alpha_9^{NC} LnASSETS_t + \\ & \alpha_{10}^{NC} CMI_t + \alpha_{11}^{NC} HHI_t + \varepsilon_t \end{aligned} \quad (4)$$

For each hospital *i*, we estimate both models using all other hospitals in the same fiscal year. Coefficient estimates times actual amounts for hospital *i* are used

to calculate expected expenses for hospital *i*. Actual expenses less expected expenses equal unexpected expenses (*U_CORE_EXP* and *U_NONCORE_EXP*).

$$U_CORE_EXP_t = CORE_EXP_t - E(CORE_EXP_t) \quad (5)$$

$$U_NONCORE_EXP_t = NONCORE_EXP_t - E(NONCORE_EXP_t) \quad (6)$$

For precise data definitions of variables used in models (3)-(6), see Appendix C. Variables in models (3) and (4) are meant to control for natural business factors which affect a hospital's level of *CORE_EXP* and *NONCORE_EXP*. Lagged core expenses ($CORE_EXP_{t-1}$) and non-core expenses ($NONCORE_EXP_{t-1}$) control for the expected persistence of these expenses. Asset turnover (ATO_t), the percentage change in gross patient revenue (ΔREV), and the percentage change in gross patient revenue if ΔREV is less than 0 ($NEG_\Delta REV$) are chosen based on similar variables used in prior research to control for expected performance (McVay 2006; Fan et al. 2010). The current ratio (*CURRENT*) and days cash on hand ratio (*DAYS_CASH*) are included to account for the impact of liquidity on nonprofit hospitals' level of core and non-core expenses (Eldenburg and Vines 2004; Krishnan and Yetman 2011). An indicator variable for teaching hospitals (*TEACHING*), log of the number of years since the hospital was originally licensed based on OSHPD Utilization data files (*LnAGE*), and log of total assets (*LnASSETS*) are included to control for the academic status, age and reputation, and size of the hospital, respectively. To control for the acuity of patients served, we include case mix index based on OSHPD Case

Mix Index files (*CMI*). Finally, the Herfindahl-Hirschman Index (*HHI*) controls for the level of local competition (Krishnan and Yetman 2011).

It is important to note that in the presence of cost shifting, the predicted relation between *U_CORE_EXP* and *U_NONCORE_EXP* is *negative*. If hospital managers reclassify non-core expenses as core expenses, the level of *U_CORE_EXP* (*U_NONCORE_EXP*) in year *t* is increasing (decreasing) in the amount of reclassification that occurs. Empirically, however, it is quite possible to observe a *positive* relation between *U_CORE_EXP* and *U_NONCORE_EXP*, even in the presence of shifting. The positive relation could occur to the extent we measure unexpected expenses with error. That is, a hospital may incur higher than usual overall expenses in a given year for any number of valid economic conditions. If core and non-core expenses are similarly affected by unforeseen economic forces (i.e., those factors not included in our expectations models), *U_CORE_EXP* and *U_NONCORE_EXP* will tend to occur in the same direction. Thus, shifting induces a negative relation between *U_CORE_EXP* and *U_NONCORE_EXP*, while measurement error in our expectations models induces a positive relation. Given that nonprofit hospitals' expense expectations models are not well-developed in the literature and the inherent difficulty in measuring (unobservable) unexpected expenses, it is reasonable to assume that measurement error exists in our tests.

To address this measurement error, we compare the relation between *U_CORE_EXP* and *U_NONCORE_EXP* for hospitals expected to engage in cost shifting to that of other hospitals. We predict that hospitals suspected of engaging in

cost shifting will show a more negative (or less positive) relation between *U_CORE_EXP* and *U_NONCORE_EXP*. For each hospital-year, we define a variable ($SUSPECT^A$) that equals the summation of nine indicator variables based on factors hypothesized in section III. Each of the nine indicator variables is coded as one if the hospital has the particular hospital characteristic and reports earnings well above the zero-profit benchmark (e.g., $RURAL^A$ = 1 if the hospital is located in a rural area and reports earnings well above the zero-profit benchmark, 0 otherwise). $SUSPECT^A$ ranges from 0 to 9 and represents the increasing likelihood that the hospital will engage in cost shifting. For easier interpretation of the results, we scale $SUSPECT^A$ by 9 so that the variable ranges from 0 to 1. The differential relation between *U_CORE_EXP* and *U_NONCORE_EXP* for suspect hospitals versus non-suspect hospitals is measured using the following model.

$$U_CORE_EXP_t = \beta_0 + \beta_1 SUSPECT_t^A + \beta_2 U_NONCORE_EXP_t + \beta_3 U_NONCORE_EXP_t \times SUSPECT_t^A + \varepsilon_t \quad (7)$$

β_3 tests our hypothesis. We expect the *incremental* relation between *U_CORE_EXP* and *U_NONCORE_EXP* for suspect hospitals to be negative. Thus, our hypothesis test of cost shifting becomes $\beta_3 < 0$.[34] β_2 measures the relation between *U_CORE_EXP* and *U_NONCORE_EXP* for non-suspect hospitals.

For our test of cost shifting for hospitals below the zero-profit benchmark, we follow the same research design. We define $SUSPECT^B$ as the summation of the

[34] Note that this test purposely excludes hospitals that report earnings below the zero-profit benchmark. Tests for cost shifting by these hospitals are detailed below.

nine indicator variables (scaled by 9) for hospitals reporting earnings below the zero-profit benchmark (e.g., $RURAL^B$ = 1 if the hospital is located in a rural area and reports earnings below the zero-profit benchmark, 0 otherwise). After substituting $SUSPECT^B$ into equation (7), we again expect $\beta_3 < 0$. As hospitals' incentive and ability to shift expenses increase, the relation between *U_CORE_EXP* and *U_NONCORE_EXP* is expected to decrease. These results would be consistent with cost shifting behavior for hospitals below the zero-profit benchmark.

The final dimension of our research design is classifying hospitals relative to the zero-profit benchmark. We classify hospitals as below the benchmark if they report net income less than zero. Classifying hospitals above the benchmark is more subjective. Our underlying hypothesis suggests that hospitals which are "well above" zero earnings have incentives to shift non-core expenses to core expenses, and those "just above" zero have less (or no) incentive. The point at which profits are "well above" zero for a nonprofit hospital is subjective and likely occurs along a continuum. Therefore, we seek to clearly distinguish those hospitals that are well above the benchmark from those that are just above to provide a more reliable test. To do this, we classify hospitals with net income (scaled by lagged total assets) greater than 0.10 as hospitals that are well above the benchmark. Benchmark hospitals are classified as those reporting net income (scaled by lagged total assets) greater than or equal to zero but less than 0.04 (Eldenburg et al. 2011). Hospitals that fall within the 0.04 to 0.10 range are not included in either the above-benchmark group or the benchmark group, although we report sensitivity analyses on this

sample later. Our classification system results in about 24% (27%) of the sample observations being classified as above (below) the benchmark, and 23% being classified as benchmark hospitals. The remaining 26% of the possible hospitals have net income greater than the benchmark hospitals but less than the above-benchmark hospitals. See Figure 2.

V. Results

A. Calculation of Unexpected Expenses

Table 13 provides the distributions of variables used to calculate unexpected expenses in models (3) and (4). Core expenses are only approximately 65% greater than non-core expenses. Thus, non-core expenses provide managers a material amount of expenses to shift in an attempt to alter external parties' perceptions of the resources devoted to core activities. The amounts reported for the explanatory variables seem reasonable. Also as expected, the means of unexpected core and non-core expenses are close to zero. This would be true almost by definition based on our estimation procedure. We are not necessarily interested in the *amount* of unexpected expenses for certain hospitals. Instead, our tests rely on the *relation* between *U_CORE_EXP* and *U_NONCORE_EXP* being lower for hospitals expected to engage in greater cost shifting when they deviate from the zero-profit benchmark.

Table 14 provides coefficient estimates of a pooled model which includes all 1,282 hospital-year observations in our sample. Note that we show coefficients from a pooled model to provide descriptive evidence of the overall relations between (core

and non-core) expenses and their explanatory variables. However, when estimating expected expenses of hospital *i* in year *t*, we estimate a model that includes only hospitals other than hospital *i* in the same fiscal year. Coefficient estimates from these models times actual amounts for hospital *i* in year *t* are used to calculate expected expenses for hospital *i* in year *t*.

There is a strong positive relation between current expenses and lagged expenses. The level of expenses (scaled by gross patient revenue) is negatively related to revenue growth (*ΔREV*) but more so for hospitals with revenue declines (*NEG_ ΔREV*). Asset turnover (*ATO*), the current ratio (*CURRENT*), and the Herfindahl-Hirschman Index based on total discharges (*HHI*) are positively related to core expenses. The days cash on hand ratio (*DAYS_CASH*) is positively related to non-core expenses. The remaining variables are insignificant. Both models show high fit, with adjusted R^2s of 0.907 and 0.874.

Table 15 provides correlations among *U_CORE_EXP* or *U_NONCORE_EXP* and our partitioning variables. The first result to note is that the correlation between *U_CORE_EXP* and *U_NONCORE_EXP* is positive, whereas our prediction in the presence of cost shifting would be negative. To us, this suggests that normal economic events and hospital characteristics beyond those controlled for in models (3) and (4) jointly affect the level of *CORE_EXP* or *NONCORE_EXP*, creating unexpected amounts that occur in the same direction.[35] However, this is not critical to our research design, but it does necessitate our choice of comparing *SUSPECT*

[35] It is also the case that cross-sectional variation in coefficient estimates or nonlinear relations between expected expenses and explanatory variables would induce measurement error, even if the expectations models included all relevant variables.

hospitals to other hospitals. We expect that the correlation between *U_CORE_EXP* and *U_NONCORE_EXP* will be less positive (or negative) for hospitals more likely to shift. These include hospitals with certain characteristics discussed in section III and that report earnings below or well above the benchmark level of earnings. We test this in the next section. The remainder of the correlations among the partitioning variables are relatively low with one exception. Not surprisingly, hospitals with high Medicare utilization tend not to be hospital with high Medicaid utilization. While nonprofit hospitals perhaps can afford to provide a proportionally high amount of services to one of either Medicare or Medicaid patients, doing so for both would be disadvantageous. Specifically, Medicare and Medicaid programs are known to provide lower reimbursement rates relative to private payers and, as documented by Kramer and Santerre (2010), a payer mix that entails a higher proportion of public to private payers is associated with lower CEO pay.

B. Tests of Hypothesis

Panel A (B) of Table 16 provides our test of the hypothesis by examining the incremental relation between *U_CORE_EXP* and *U_NONCORE_EXP* for hospitals well above (below) the zero-profit benchmark based on $SUSPECT^{A}$ ($SUSPECT^{B}$) scores. In Panel A, we first note the positive coefficient on *U_NONCORE_EXP*. The coefficient suggests that for non-suspect hospitals, the relation between *U_CORE_EXP* and *U_NONCORE_EXP* is significantly positive. While one might predict no relation between these two variables in the absence of cost shifting,

finding a positive relation likely suggests measurement error in expected expenses being positively correlated for core and non-core expenses. However, for hospitals that engage in cost shifting, we expect the relation between unexpected expenses to become smaller (or even negative). The coefficient on the interaction of *U_NONCORE_EXP* and $SUSPECT^{A}$ provides our test of the incremental relation between *U_CORE_EXP* and *U_NONCORE_EXP* for hospitals expected to have greater incentives and ability to shift costs that report earnings well above the zero-profit benchmark. Consistent with our hypothesis, the incremental relation between *U_CORE_EXP* and *U_NONCORE_EXP* is significantly negative for $SUSPECT^{A}$ hospitals.

In Panel B we test our hypothesis for suspect hospitals that report earnings below the zero-profit benchmark. Similar to Panel A, we find that non-suspect hospitals have a significantly positive relation between *U_CORE_EXP* and *U_NONCORE_EXP*. For $SUSPECT^{B}$ hospitals, the incremental relation is negative, as expected, but not significant at conventional levels for a two-tailed test. From the results in Table 16, we conclude that the strongest evidence of shifting occurs for hospitals that are more likely to have incentives and ability to shift and that report earnings well above the zero-profit benchmark.

To further interpret the results, we split each sample into those with positive and negative *U_NONCORE_EXP*. The reason for doing so follows. In Table 16, we document that the incremental relation between *U_CORE_EXP* and *U_NONCORE_EXP* is significantly negative for $SUSPECT^{A}$ hospitals. However,

finding this negative relation does not necessarily indicate that expenses are being shifted to core activities from non-core activities. In fact, one would observe this same negative relation between *U_CORE_EXP* and *U_NONCORE_EXP* if cost shifting were occurring in the opposite direction (i.e., from core to non-core activities). To the extent that cost shifting is occurring in the hypothesized direction, *U_CORE_EXP* should be positive (i.e., core expenses are greater than expected) while *U_NONCORE_EXP* should be negative (i.e., non-core expenses should be less than expected). In other words, *U_CORE_EXP* and *U_NONCORE_EXP* should show the most evidence of a negative relation when *U_NONCORE_EXP* is negative. If, however, we observe that the negative relation between *U_CORE_EXP* and *U_NONCORE_EXP* occurs primarily when hospitals report positive *U_NONCORE_EXP*, then it is more difficult to believe that hospitals are shifting expenses in the manner we hypothesize. Thus, splitting the sample into those observations with positive and negative *U_NONCORE_EXP* allows for stronger conclusions.

Panel A (B) of Table 17 provides these additional tests for hospitals well above (below) the zero-profit benchmark. In Panel A, we observe that the incremental relation between *U_CORE_EXP* and *U_NONCORE_EXP* for $SUSPECT^{A}$ is both highly negative and statistically significant only when *U_NONCORE_EXP* is negative. For observations with positive *U_NONCORE_EXP*, the incremental relation is close to zero and is not statistically significant. Further, an F-test of equality of coefficients across samples indicates

that the incremental relation when *U_NONCORE_EXP* is negative is statistically different from when *U_NONCORE_EXP* is positive (p-value < 0.0001).[36] In Panel B, we find that when *U_NONCORE_EXP* is negative the incremental relation is negative but not significant at conventional levels for a two-tailed test (significant at only the 0.075 level for a one-tailed test). An F-test of equality of coefficients across samples reveals that the incremental relation when *U_NONCORE_EXP* is negative does not differ statistically from when *U_NONCORE_EXP* is positive. Thus, we continue to conclude that evidence of cost shifting is greater for hospitals well above the zero-profit benchmark but marginal, at best, for hospitals below the benchmark.

The more significant findings when profits are well above versus below the zero-profit benchmark seem reasonable when considering the sentiment surrounding nonprofit hospitals. Nearly all of the negative media publicity and critical comments by regulators, politicians, and community groups occur when nonprofit hospitals report excessive profits. Thus, empirically demonstrating that shifting is more likely to occur when profits are well above the zero-profit benchmark gives us greater confidence in our research design and conclusions.

[36] To compute an F-test of equality of coefficients, we estimate a combined model of positive and negative unexpected non-core expenses. Specifically, the model we use is as follows:

$$U_CORE_EXP_t = \beta_0^{neg} + \beta_0^{pos} + \beta_1^{neg} SUSPECT_t^A + \beta_1^{pos} SUSPECT_t^A + \beta_2^{neg} U_NONCORE_EXP_t + \beta_2^{pos} U_NONCORE_EXP_t + \beta_3^{neg} U_NONCORE_EXP_t \times SUSPECT_t^A + \beta_3^{pos} U_NONCORE_EXP_t \times SUSPECT_t^A + \varepsilon_t$$

The coefficients β_k^{neg} and β_k^{pos} (k = 0, 1, 2, 3) correspond to negative and positive unexpected non-core expenses, respectively. Our F-test becomes $\beta_3^{neg} = \beta_3^{pos}$.

C. Tests of Individual Hospital Characteristics

Our next tests consider detailed examinations of which hospital characteristics contribute to the more negative relation between *U_CORE_EXP* and *U_NONCORE_EXP* when suspect hospitals deviate from the zero-profit benchmark. To do this, we separate $SUSPECT^{A}$ into nine indicator variables using the hospital characteristics discussed in section III. This allows for nine separate tests. This analysis has the advantage of providing more specific conclusions about which hospitals are likely engaging in cost shifting. At the same time, using individual measures likely provides lower-power tests because of the smaller sample size.

In Panel A of Table 18, we test for cost shifting when earnings are well above the benchmark by again separately investigating observations with positive and negative *U_NONCORE_EXP*. In Panel A, when *U_NONCORE_EXP* is negative, we find that five of the nine characteristics are negative but only four are significant. Hospitals that are rural, provide high charity care, have low Medicare utilization, or have high donations exhibit more evidence of shifting non-core expenses to core expenses when net income is well above the benchmark. Doing so allows these hospitals to portray less profitable core operations, thereby seemingly projecting performance more in line with their charitable mission and patient-centered focus.

When *U_NONCORE_EXP* is positive, only low Medicaid hospitals display a significant incremental negative relation. Furthermore, this negative relation approximately equals that of low Medicaid hospitals when *U_NONCORE_EXP* is

negative. Thus, our previous conclusions related to these hospitals' shifting behavior is confounded by these results.

In Panel B of Table 18, we provide analyses for cost shifting when earnings are below the zero-profit benchmark. When *U_NONCORE_EXP* is negative, we find that hospitals with low Medicare, low Medicaid, or high donations show the most evidence of cost shifting. We further note that the coefficients for these hospitals are also more negative than those of hospitals with positive *U_NONCORE_EXP*, reinforcing our conclusions that costs are being shifted from non-core to core activities. Thus, while our aggregate measure of cost shifting for hospitals below the zero-profit benchmark ($SUSPECT^B$) does not provide strong evidence, tests of individual hospital characteristics are consistent with some hospitals acting in this manner.

D. Alternative Benchmark Definitions

As discussed earlier, it is somewhat subjective to define what constitutes earnings "well above" the zero-profit benchmark. To address this, we perform two sensitivity tests on our results in Table 18. First, we redefine the benchmark group to include net income (scaled by lagged total assets) greater than or equal to 0.00 and less than or equal to 0.10. Untabulated results reveal very similar findings using this alternative definition of the benchmark group. Specifically, when *U_NONCORE_EXP* is negative, five (four) of nine coefficients are in the predicted direction (i.e., negative) for the above-benchmark (below-benchmark) group with

four (three) of these being at least marginally statistically significant. Moreover, the coefficients in the sample split where *U_NONCORE_EXP* is negative are more negative or less positive than coefficients arising from when *U_NONCORE_EXP* is positive in five (three) of nine cases for above-benchmark (below-benchmark) hospitals.

Second, we redefine the above-benchmark group to include net income (scaled by lagged total assets) greater than or equal to 0.04. Untabulated results again support our hypothesis, with five of nine coefficients being negative for each of the above-benchmark and below-benchmark groups, three (four) of which are at least marginally statistically significant for the above-benchmark (below-benchmark) group. For negative *U_NONCORE_EXP*, five (four) of nine coefficients are more negative or less positive than the corresponding positive *U_NONCORE_EXP* coefficients in the above-benchmark (below-benchmark) group. Therefore, we find our results to be robust to inclusion of nonprofit hospital earnings greater than or equal 0.04 but less than or equal to 0.10.

We also redefine the benchmark group according to different upper bounds. Holding the below-benchmark and above-benchmark definitions constant (i.e., as defined in section IV), we redefine benchmark hospitals as those reporting net income (scaled by lagged total assets) greater than or equal to zero but less than 0.02 and 0.03, respectively. Untabulated results using these alternative benchmark group definitions again support our hypothesis and are very similar to results presented.[37]

[37] Specifically, by defining the upper bound of the benchmark profit range to be 0.03 and when *U_NONCORE_EXP* is negative, five coefficients are negative and four are at least marginally

Therefore, our results are robust to a more restrictive definition of what constitutes meeting the zero-profit benchmark.

Finally, we redefine the below-benchmark group by excluding those observations with very small losses. It may be the case that reporting very small losses is "close enough" to zero that the hospital faces no incentive to shift expenses. When we exclude observations with net income (scaled by lagged total assets) between zero and −0.01, results are similar to those reported.[38] In summary, our overall conclusions are robust to minor modifications in the classification of hospitals falling short of, meeting, or exceeding the zero-profit benchmark.

VI. Conclusion

While the profit motive of nonprofit hospitals differs from for-profit institutions, similar earnings management techniques are used to achieve this objective. Specifically, instead of being charged with maximizing profits as in the case of for-profits, nonprofit hospitals are expected to fulfill a zero-profit benchmark (Hoerger 1991, Leone and Van Horn 2005). If a nonprofit hospital realizes repeated

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statistically significant for each of the above-benchmark and below-benchmark groups. The coefficients corresponding to *U_NONCORE_EXP* that is negative are more negative or less positive than coefficients arising from when *U_NONCORE_EXP* is positive in six (four) of nine cases for above-benchmark (below-benchmark) hospitals. By defining the upper bound of the benchmark profit range to be 0.02 and when *U_NONCORE_EXP* is negative, five (four) coefficients are negative for the above-benchmark (below-benchmark) group and four of these coefficients are at least marginally statistically significant for each of the above-benchmark and below-benchmark groups. The coefficients corresponding to *U_NONCORE_EXP* that is negative are more negative or less positive than coefficients arising when *U_NONCORE_EXP* is positive in six (four) of nine cases for above-benchmark (below-benchmark) hospitals.

[38] After eliminating small losses and when *U_NONCORE_EXP* is negative, five (four) coefficients are negative and four (three) are at least marginally statistically significant for the above-benchmark (below-benchmark) group. The coefficients corresponding to *U_NONCORE_EXP* that is negative are more negative or less positive than coefficients arising when *U_NONCORE_EXP* is positive in five of each of the above-benchmark and below-benchmark groups.

losses, it may face the inability to continue as a going concern, and hospital managers suffer lower compensation or higher unemployment risk. If a nonprofit hospital reports excessively positive profits, it will likely face scrutiny from the media, donors, regulators, politicians, and third-party payers. Prior research indicates that nonprofit hospitals manage accruals (e.g., Leone and Van Horn 2005) and real activities (e.g., Eldenburg et al. 2011) to accomplish the zero-profit objective. Moreover, preliminary evidence has confirmed that cost shifting is used to inflate core expenses devoted to the hospital's charitable mission and patient-centered focus (e.g., Krishnan and Yetman 2011). Our study combines existing knowledge in these areas by providing evidence of cost shifting in nonprofit hospitals whose earnings are below or well above the zero-profit benchmark relative to those hospitals who meet this profit objective.

We expect that nonprofit hospitals whose bottom-line net income is below or well above the zero-profit benchmark have incentives to shift non-core (i.e., non-patient-centered) expenses to core (i.e., patient-centered) expenses. By shifting costs to core expenses, core profitability decreases, creating the appearance that the hospital is better fulfilling its charitable mission and patient-centered focus, while providing services at affordable prices. At the same time, shifting expenses from non-core activities may reduce criticisms from the community and others that managers are using resources inefficiently by spending excessively on activities not directly related to patient care.

To test cost shifting behavior, we first estimate unexpected core expenses and unexpected non-core expenses. If nonprofit hospitals shift expenses from non-core to core activities, we should observe a negative relation between unexpected core and unexpected non-core expenses. We compare the relation between unexpected core expenses and unexpected non-core expenses for nonprofit hospitals suspected to engage in cost shifting versus non-suspect hospitals.

We find that nonprofit hospitals with greater normative pressures, lower regulatory oversight, and greater reliance on external financing through donations tend to shift costs when reporting bottom-line net income that deviates from the zero-profit benchmark. Specifically, when bottom-line net income is well above the zero-profit benchmark, hospitals that are rural, provide high charity care, have low Medicaid utilization, or have high donations tend to shift expenses from non-core activities to core activities. When bottom-line net income is below the zero-profit benchmark, hospitals that have low Medicare utilization, have low Medicaid utilization, or have high donations engage in cost shifting.

Beyond providing additional evidence of manipulative reporting behavior by hospitals, our study may also provide information directly relevant to regulators of nonprofit hospitals. These hospitals receive tax-exempt status, and their right to do so represents an implicit allocation of scarce government resources. Given the growing concerns over the sustainability of government-assisted health care, regulators (and others) need to better understand the performance and cost efficiency of health care providers. To the extent that cost shifting occurs, nonprofit hospitals

may mask inefficient allocation of scarce resources and mislead external parties into believing the nonprofit hospital's tax-exempt status is warranted. This misleading financial reporting may also hinder regulators' ability to identify the real issues plaguing the health care industry, further aggravating the effectiveness of current and future legislation. We hope that our findings shed light on the existence of a relatively unknown form of earnings management in nonprofit hospitals.

Chapter 3

The Impact of Political Connectedness on Firm Value and Corporate Policies: Evidence from *Citizens United*[39]

> "At bottom, the Court's opinion [in *Citizens United*] is thus a rejection of the common sense of the American people, who have recognized a need to prevent corporations from undermining self-government since the founding, and who have fought against the distinctive corrupting potential of corporate electioneering since the days of Theodore Roosevelt. It is a strange time to repudiate that common sense. While American democracy is imperfect, few outside the majority of this Court would have thought its flaws included a dearth of corporate money in politics."
>
> – Dissenting opinion of Supreme Court Justice John Paul Stevens, Citizens United v. FEC

I. Introduction

Do corporate political connections enhance or destroy firm value? Existing insights into political connectedness as it relates to firm value are inconclusive, likely owed at least in part to endogeneity concerns.[40] This paper exploits an exogenous enhancement in the impact of political connections on firm value and corporate policies that accompanied a landmark Supreme Court ruling, *Citizens United v. Federal Election Commission* (hereafter, "*Citizens United*") which lifted long-standing limits on corporate political contributions. In doing so, we are able to help

[39] This chapter is based on collaborative work with Vahap B. Uysal.

[40] Notably, in response to endogeneity concerns, prominent researchers have called for more careful inspection of the relationship between corporate political activism and firm value. For example, Cooper, Gulen, and Ovtchinnikov (2010) state the following: "[…] do we document evidence of a causal link from firm PAC contributions to future stock prices? Answering this question in the affirmative requires resolving potential endogeneity problems with our data… Our hope is that future work… can further analyze the issue of causality and the related topic of whether the correlations between contributions and returns arise from mispricing or risk" (2010, p. 690).

fill a gap in the literature with regard to the value implications of corporate political connectedness.

Political connectedness may destroy shareholder value if the political connectedness of a company is driven by management's political agenda – an agenda that diverges from that of the company's stakeholders. Specifically, connected firms place their resources in jeopardy of being exploited by politicians (Fan, Wong, and Zhang 2007; Caprio, Faccio, and McConnell 2013), and are marred by agency conflicts (Aggarwal, Meschke, and Wang 2012; Boubakri, El Ghoul, and Saffar 2013). Under this view, *Citizens United* amplifies the value-destroying effect of political connectedness through its lifting of restrictions on corporate political contributions; thus, historically politically connected firms are likely to realize an unfavorable market reaction surrounding the *Citizens United* decision (Agency Cost Hypothesis).

A competing view states that political connectedness enhances shareholder value (e.g., Cooper, Gulen, and Ovtchinnikov 2010). Specifically, politically connected firms can derive a benefit (1) from preferential access to financing (Faccio, Masulis, and McConnell 2006; Claessens, Feijen, and Laeven 2008), (2) through less risk and an accompanying lower cost of capital (Boubakri, Guedhami, Mishra, and Saffar 2012), (3) when a politically connected individual joins the corporation's board (Goldman, Rocholl, and So 2009), or (4) when a businessperson enters politics (Faccio 2006). As *Citizens United* amplified the value implications of political connectedness, this view predicts that connected firms will enjoy a

favorable market reaction surrounding the announcement of *Citizens United* (Value Enhancing Hypothesis).

To address the value implications of political connectedness, we exploit an exogenous and landmark Supreme Court ruling, *Citizens United*, as a quasi-natural experiment. Decided in January 2010, *Citizens United* lifted long-standing limits on corporate political contributions. However, the verdict was unanticipated and did not come easily, as the issue was accompanied by considerable disagreement (e.g., Bravin 2010; Barnes 2010b; Biskupic 2009). The 5-4 vote in favor of its passing further verified division amongst members of the Court.[41]

The controversial and uncertain nature of *Citizens United* offers an ideal research setting to examine the effect of political connectedness on both firm value and corporate policies. Specifically, much of the extant, related literature relies on a simple, cross-sectional research design that is subject to endogeneity concerns. Since predicting the likely outcome of *Citizens United* was highly infeasible (e.g., Barnes 2010a; Biskupic 2009; Eggen 2010), it is difficult to argue that either investors or corporations were able to anticipate its favorable vote and proactively

[41] Soon after the ruling, President Barack Obama voiced vigorous criticism, declaring the decision "a green light to a new stampede of special interest money" (Barnes and Eggen 2010). The majority vote adamantly defended their belief that corporate political contributions are a form of free speech and, as such, constitutional under the First Amendment. Not surprisingly, the months following passage of *Citizens United* have been marked by controversy and turmoil, with some lawmakers and investors urgently pleading that the Supreme Court reconsider the expansive and profuse provisions accompanying *Citizens United*, in addition to urging the Securities and Exchange Commission to consider adopting disclosure requirements for corporate political contributions. Notably, in June 2012, the Supreme Court refused a request to reconsider its *Citizens United* decision (Bravin 2012). As of mid-2013, the SEC was continuing to consider petitions by advocates of campaign finance reform calling for stark improvements in the disclosure of corporate political contributions (SEC 2011).

adjust their behaviors accordingly. The surprise nature of *Citizens United* lends to its credibility as a valid quasi-natural experimental setting.

A primary channel through which corporations become politically connected is through campaign contributions to candidates for office. We follow previous studies (Aggarwal et al. 2012; Claessens et al. 2008; Goldman et al. 2009) in basing our definition of political connectedness on corporate campaign contributions, as reported in the Center for Responsive Politics. Specifically, for companies belonging to the S&P 500, we define a firm as politically connected if its median firm-level, pre-*Citizens United* political contributions (scaled by net total assets) falls in the top quartile.

By operationalizing the political connectedness measure in the quasi-natural experiment of *Citizens United*, this paper provides novel evidence on the impact of political activism on firm value. Specifically, after estimating abnormal returns using a seemingly unrelated regression (SUR) that accounts for cross-correlated error terms – a distinction and empirical improvement relative to related papers – we find that political connections destroy shareholder value. Notably, historically politically connected firms realized an abnormal price drop of –0.475% on the date the *Citizens United* decision was announced, and a cumulative abnormal loss of –1.219% five days after the announcement date. In contrast, historically non-politically connected firms enjoyed positive returns on announcement date in the order of 0.240%. The difference between the announcement date reactions of politically connected firms and non-politically connected firms is also highly statistically significant. In

multivariate analyses that control for firm characteristics, we continue to find a negative and significant effect of political connectedness on shareholder value. Furthermore, the negative effect of political connectedness is significant only for the subsample of firms with weak corporate governance. Collectively, these findings support the Agency Cost Hypothesis.

Given our finding that political connections reduce firm value, we exploit differences in corporate policies as a means to further investigate whether political connectedness and agency conflicts are positively related. Since corporate policies and political connectedness are jointly determined, we use the quasi-natural experimental setting of *Citizens United* to overcome endogeneity limitations. This improvement over related, existing literature offers an opportunity to draw more sound conclusions on the relationship between corporate policies and political connectedness.

We study this relationship in the context of cash management policies. Building upon previous studies that show that greater agency conflicts are associated with higher cash holdings (e.g., Jensen 1986; Stulz 1990; Harford 1999), we compare the cash holdings of politically connected firms to their less-connected counterparts. Specifically, we utilize a difference-in-differences approach to capture differences between firms of differing political connectedness and during different periods of time (i.e., pre- versus post-*Citizens United*). After controlling for traditional determinants of cash holdings, we find that switching from non-politically connected status to politically connected status results in an incremental increase in

corporate cash holdings of almost 20% following passage of *Citizens United*. Poor corporate governance quality also exacerbates the agency problems inherent to politically connected firms. Specifically, politically connected firms with entrenched managers, busy boards of directors, and overcompensated CEOs retain even more cash relative to their well-governed counterparts in the post-*Citizens United* period. We also find that the mere entertaining of a shareholder proposal that would restrict political contributions acts as a monitoring mechanism and, by extension, reduces the cash holdings of politically connected firms. Further, our results reveal that within-firm political tension, defined as divergence in management's political preferences from those of his employees, is associated with incrementally higher cash holdings. Collectively, these findings support the existence of agency costs in politically connected firms.

We conduct several sensitivity tests to validate our findings. Specifically, we provide evidence that the parallel trends assumption required for difference-in-differences estimation is not violated in our research setting. Additionally, our conclusions regarding the value implications of corporate political connections hold after accounting for potentially confounding events as well as in a placebo analysis, in which we compare politically connected companies to a group of "placebo-politically-neutral" firms. Our findings related to the agency conflicts of politically connected firms are robust to an analysis of *excess* cash holdings. Finally, we demonstrate that our empirical findings remain intact after excluding politically exposed firms belonging to the defense, energy, and utilities industries. These

robustness checks lend further support to the value-destroying effects of corporate political connections and a positive association between political connectedness and agency costs.

This paper adds to emerging literature on the value-decreasing effects of political connections on stock prices. In a study of the effects of *Citizens United*, Coates (2012) finds that political connectedness is negatively related to Tobin's Q ratio, suggesting agency problems in politically connected firms.[42] Using the event study methodology, we document an unfavorable (favorable) capital market reaction to *Citizens United* for politically (non-politically) connected firms. Our paper fulfills a request repeatedly stated in related literature to explore the ramifications of *Citizens United* on corporate political activism, particularly since the landmark decision is expected "to greatly increase the use of corporate funds for political donations" (Aggarwal, Meschke, and Wang 2012, p. 2). Through a study of *Citizens United* and its exogenous enhancement in the value implications of political connectedness, we are better able to provide a causal link between political connections and changes in firm value.

This paper is also related to extant literature that examines the impact of political connectedness on corporate policies. Relevant studies have shown that political connectedness is significantly and positively related to executive compensation (Aslan and Grinstein 2012), leverage (Boubakri, Cosset, and Saffar

[42] Previous studies show that Tobin's Q ratio is correlated with several other factors (e.g., growth opportunities, capital structure) which do not fully reflect shareholder value (e.g., Anderson and Reeb 2003; Hail and Leuz 2009). We offer a more direct examination of shareholder value by making use of an event study methodology.

2012; Faccio 2010; Hutton, Jiang, and Kumar 2013), and liquidity (Boubakri, El Ghoul, and Saffar 2013). In a cross-country study of corporate cash holdings, Boubakri, El Ghoul, and Saffar (2013) find that politically connected firms hold more cash relative to non-politically connected firms, suggesting that politically connected firms face acute governance problems known to correlate with cash hoarding behaviors (e.g. Jensen 1986; Stulz 1990). These excessive cash reserves can then be exploited by politicians in an attempt to advance political and social agendas at the expense of shareholders. In contrast, a study of political *lobbying* expenditures by Hill, Fuller, Kelly, and Washam (2014) demonstrates an inverse relation between cash holdings and lobbying costs. The authors attribute this finding to the liquidity-related benefits of political connections (such as better access to external financing), and the known tendency for firms to hold smaller cash balances when the cost of illiquidity is low (e.g., Opler, Pinkowitz, Stulz, and Williamson 1999). Our contribution to this stream of literature is two-fold. First, we demonstrate that political connectedness is a measure of agency problems and is distinct from both traditional determinants of cash holdings and governance measures. While the majority of previous studies rely on inferences drawn from cross-sectional data, the primary focus and findings of our paper revolve around a quasi-natural experiment, thus significantly alleviating endogeneity concerns. Second, our results shed light on the dynamics of internal and external corporate governance mechanisms. Specifically, by showing significant effects of political connectedness and its

interaction with corporate governance measures, this study suggests that political connectedness further exacerbates agency conflicts within the firm.

The remainder of the paper is organized as follows. A discussion of *Citizens United* is presented in the next section. Section III presents our hypotheses and related literature. Section IV describes the sampling procedure, and our empirical analysis is offered in section V. We conclude in section VI.

II. Background

Historically, corporations were prohibited from actively campaigning on behalf of politicians through donations of independent expenditures, which were strictly forbidden during the period from World War II through 2010.[43, 44] This ban was challenged by the nonprofit organization Citizens United through their 2008 release of a conservative-inspired documentary attacking then-senator Hillary Rodham Clinton's record and instilling doubts as to her qualifications to be president. The Federal Election Commission restricted Citizens United on their advertisements of the movie, a limitation that Citizens United CEO David Bossie – a long-time adversary of Senator Clinton – claimed to violate his First Amendment

[43] Independent expenditures are defined as funds "expressly advocating the election of or defeat of a clearly identified candidate who is not made in cooperation, consultation, or concert with, or at the request or suggestion of, a candidate, a candidate's authorized committee, or their agents, or a political party or its agents" (11 CFR 100.16(a)). An example of an independent expenditure would be a corporation's decision to finance television commercials endorsing the candidate of their choosing.

[44] The first explicit ban on the use of general treasury funds for campaign expenditures was delivered through a provision in the Taft-Hartley Act (1947). Subsequent court rulings (e.g., Federal Election Commission Act (1971, 1974), *Austin vs. Michigan Chamber of Commerce* (1990), Bipartisan Campaign Reform Act (2002)) reinforced this ban. For a comprehensive review of legal decisions leading up to *Citizens United*, see Briffault (2011).

and right to free speech (Rucker 2010). The response to Citizens United's challenge was a landmark Supreme Court decision in January 2010, where prohibitions on certain forms of corporate political activism were rendered unconstitutional under the First Amendment.

Some aspects of the regulatory environment surrounding corporate political activism did remain unchanged following *Citizens United*. Notably, corporations are still prohibited from contributing corporate funds directly to a political candidate. Instead, corporations derive a large proportion of their political connectedness through Political Action Committee (PAC) contributions. Managers, employees, and shareholders can contribute to their company's PAC, and subsequently the PAC will channel the said funds to a political candidate. Interestingly, corporations are permitted to use internal funds to finance the fundraising efforts of PACs. Despite these regulatory consistencies, a logical yet relatively unexplored implication of *Citizens United* is an increase in corporate political activism through all means available, and not just through channels that were previously off limits.

Election spending reached new highs following passage of *Citizens United*. The estimated $6 billion spent on the year 2012 election represents a new record and exceeds the cost of the second most expensive election by more than $700 million (Center for Responsive Politics 2012). While corporations have never been barred from lobbying to reform *policies*, passage of *Citizens United* essentially opened the floodgates for corporations to take an active, direct, and economically meaningful role in campaigning for preferred political *candidates*. Moreover, given the

previously-documented complementary nature of the various types of political activism, *Citizens United* stimulated corporate political involvement of all forms, even those types that were allowed to be used prior to its passage (Coates 2012).[45,46]

A distinction of this paper is its focus on *Citizens United*, a powerful and controversial decision but one whose effects are limited to the United States. That is, our focus differs from cross-country studies (e.g., Boubakri, El Ghoul, and Saffar 2013; Faccio 2006) and country-specific studies (Bunkanwanicha and Wiwattanakantang 2009; Ferguson and Voth 2008; Fisman 2001; Johnson and Mitton 2003; Ramalho 2007; Roberts 1990) in that we concentrate on how a timely, exogenous shock (*Citizens United*) affects the political connectedness landscape of corporations located within the United States. Restricting our focus to an unanticipated change in corporate political contributions policy in the United States provides an ideal setting to test the implications of corporate political contributions on firm value and corporate policies while holding important within-country factors

[45] For additional evidence on the repeatedly documented and widely-accepted complementary nature of different forms of corporate political activity, see, for example, Ansolabehere, Snyder, and Tripathi (2002), Lord (2000), Schuler, Rehbein, and Cramer (2002), and Wright (1990).

[46] It is important to recognize that many forms of corporate campaign activity need not be disclosed. This reality complicates studies of political connectedness, since corporations can choose to strategically disguise their political activism. For example, a firm can avoid disclosure altogether by channeling political contributions through a separate entity (i.e., a "conduit", or "independent" organization). In the case that the independent entity subsequently contributes said funds to a political campaign, it may be required to disclose the identity of its donors. However, if the entity restricts its contributions to independent expenditures, no disclosure is required. These strategic yet unobservable channels of political activism were utilized even more in the post-*Citizens United* period. Specifically, the identities of donors who sourced more than 50 percent of the $266.4 million contributed by outside groups in 2010 remain unknown (Public Citizen 2010). The unobservable nature of some forms of corporate political activity results in conservative estimates of political contributions and should only bias against our ability to document a link between political connectedness and corporate policies.

constant, such as cultural values, political views, regulations, and government structure.

III. Hypothesis Development

A. Agency Cost Hypothesis

As illustrated next, there exist several arguments for why the political motives of managers create or further exacerbate agency conflicts and are thus associated with a reduction in firm value. The potential negative consequences in this context are perhaps best identified through the testimony of Columbia law professor John C. Coffee, Jr. before the U.S. House Subcommittee on Capital Markets, Insurance, and Government Sponsored Enterprises, who noted the following:

> "…The goal, however, has to be not only to increase transparency and disclosure, but to give shareholders an effective remedy by which to challenge decisions of which they disapprove, because this is a world in which shareholder and managerial interests are not well aligned. There may be perfectly legitimate corporate contributions, but for every dollar contributed by a corporation that maximizes shareholder wealth, there are other dollars that are contributed to pursue the personal, political, or ideological agenda of senior managers, all of that is hidden… [*Citizens United*] assumes that shareholders have practical remedies by which to contest decisions of managers to make contributions. In fact, they have very few rights." (Corporate Governance after *Citizens United* 2010)[47]

[47] Relatedly, in his 2010 report to the shareholders of the New York City Retirement Systems, John C. Liu, Comptroller of the City of New York, indicated that, "In the wake of *Citizens United*, it is even more important that boards provide the oversight and disclosure necessary to ensure that any political spending ultimately benefits shareholders, not the managers who control the corporate purse strings" (Comptroller of the City of New York 2010, p. 3).

More recent headlines directly tied to *Citizens United* involve incidents in which top-level corporate executives use their stature to influence the political leanings and voting decisions of lower-level employees, consistent with the presence of agency problems. For example, an October 2010 *New York Daily News* article revealed that the owner of several Ohio-based McDonald's restaurants sent Republican advertisements to employees as a means to strongly encourage them to vote for GOP candidates in the mid-term election. The owner, Paul Siegfried, went so far as to threaten the employees with pay and benefit cuts should they choose to not comply (Shahid 2010). These instances of political coercion are only becoming more common in the post-*Citizens United* period, as the freedoms imparted by the law have left many managing directors to believe that money is not the only unrestricted resource to be used for political means, but that employees can be used, too (e.g., Paarlberg 2012; Charles 2012; McCarthy 2012).[48]

Given their overarching negative influence, corporate political connections may harm shareholder value. Fan, Wong, and Zhang (2007) show that politicians

[48] Examples of agency conflicts within politically connected organizations are abundant. For instance, consider Roland Arnall, founder of Ameriquest Mortgage Co. and the single biggest contributor to President George W. Bush during the 2002-2004 time frame. During the 2002-2007 period, Mr. Arnall and his wife contributed at least $12.25 million to the Bush campaign – an investment that would generate a very high rate of return. For example, in several states, Ameriquest benefited from the relaxing of stringent regulations that would have adversely affected the company's subprime lending practices. Moreover, President Bush later appointed Mr. Arnall to the prestigious position of ambassador to the Netherlands. Lastly, in exchange for their generosity, the Arnalls were invited to the inaugural dinner in January 2005, along with numerous other balls, receptions, and galas (Simpson 2007; Esdall and Bimbaum 2005). As a more recent example, a March 2012 *Associated Press* report revealed that "more than half of President Barack Obama's most generous campaign fundraisers have visited the White House at least once for meetings with top advisers, holiday parties, or state dinners… [and] scores have made multiple visits" (Gillum 2012). As another example, a December 2010 meeting hosted by President Obama involved 20 of the most politically active CEOs in corporate America. Collectively, these 20 executives had made $8.2 million in political contributions over the prior 20 years and were described by the Center for Responsive Politics as "a pretty friendly crowd" (Riley 2010).

may attempt to extract a politically connected firm's resources to advance their own agendas. Shleifer and Vishny (1994) theoretically demonstrate this tendency as one where politicians compel corporate donors to pursue their social policy goals. Caprio, Faccio, and McConnell (2013) use a country-specific measure to demonstrate that a company's propensity to shelter assets increases when the potential of political extraction is high. Aggarwal, Meschke, and Wang (2012) and Boubakri, El Ghoul, and Saffar (2013) find that politically connected firms are marred by agency conflicts. In a study of changes in congressional committee chairmanships, Cohen, Coval, and Malloy (2011) demonstrate that increases in state-level government spending deter (i.e., "crowd out") private-sector investment of geographically proximate corporations. If political connections *decrease* firm value, the announcement of new regulations that effectively approve of and promote corporate political activism should be accompanied by a *negative* reaction to historically politically connected firms. Therefore, the argument for political connections having value-destroying implications predicts a negative market reaction to politically connected firms surrounding the *Citizens United* decision. Moreover, as agency costs are associated with higher cash holdings, the Agency Cost Hypothesis predicts that politically connected firms are more likely to hoard cash relative to their less-connected counterparts.[49]

[49] A direct relationship between cash holdings and agency problems has garnered support from much of the relevant, extant literature. See, for example Jensen (1986), Stulz (1990), Harford (1999), Faleye (2004), Dittmar, Mahrt-Smith, and Servaes (2003); and Chen, Chen, Schipper, Xu, and Xue (2012).

B. Value Enhancing Hypothesis

Political connections may instead extend value-enhancing benefits to shareholders. In an international study, Faccio, Masulis, and McConnell (2006) find that politically connected firms are significantly more likely to be bailed out by the government in times of distress relative to non-politically connected firms. Relatedly, Claessens, Feijen, and Laeven (2008) document politically connected firms' preferential access to financing. As an example, Solyndra, a former manufacturer of solar panels, is thought to have reaped significant financial benefits in response to its political connections. Specifically, in 2009, the company received a $535 million loan guarantee from the Department of Energy. In August 2011, Solyndra filed for bankruptcy, with the government projected to recoup a mere 19 cents on the dollar (Bathon 2012). A twist in this story originates in George Kaiser, an extremely wealthy Oklahoma oilman whose foundation owned one-third of the company. Kaiser is reported to have raised between $50,000 and $100,000 for President Barack Obama's 2008 campaign for presidency. One might conclude that Kaiser's loyalty to the President paid off in a big way; namely, some have suggested that the Solyndra plant built in Kaiser's hometown of Tulsa, Oklahoma, was secured through his generous political contributions (Mildenberg and Robison 2011).

Another instance of politicians catering to their donors arises through the combined efforts of congressional representatives Brian Bilbray, Erik Paulsen, and Jim Gerlach, who in June 2012 voted against a 2.3% excise tax on medical devices (H.R. 436) that went on to pass by a vote of 270-146. An examination of the

legislators' contribution inflows reveals clear political motives, with PACs of leading health care companies such as Abbott Laboratories, Life Technologies Corporation, and the Medical Device Manufacturers Association topping Bilbray's list of major donors (Walters 2012). A politician's decision to promote the profitability of his donors (in this case, through tax breaks) represents a clear channel through which firms can use political contributions to enhance their financial stability and, by extension, firm value. Furthermore, a politically connected firm may derive value-enhancing benefits from a reduction in risk and lower cost of capital (Boubakri, Guedhami, Mishra, and Saffar 2012; Goldman, Rocholl, and So 2009; Faccio 2006).

If political connections *increase* firm value, the announcement of new regulations that effectively approve of and promote corporate political activism should be accompanied by a *positive* reaction to historically politically connected firms. As such, the notion that political connections are value-enhancing predicts a positive market reaction to politically connected firms surrounding the passage of *Citizens United*. Moreover, the value-enhancing effects of political connections may also be derived from liquidity and soft-budget constraints that effectively free up the flow of cash (e.g., Boubakri, Cosset, and Saffar 2012). Thus, according to the Value Enhancing Hypothesis, politically connected firms respond to their financially *un*constrained position by holding *less* cash relative to their less-connected counterparts.

IV. Sample

We obtain political contributions data from the Center for Responsive Politics (CRP). The CRP provides numerous data sets that can be used to gauge political activism, including campaign finance, lobbying, and the personal financial conditions of members of Congress and has been used repeatedly in related literature (e.g., Aslan and Grinstein 2012; Belo, Gala, and Li 2013; Goldman et al. 2009). The focus of this paper is on corporate campaign finance activity, as gathered from the CRP and originating through Federal Election Commission (FEC) records. Major subsets of the campaign finance data include individual contributions and (separately) political action committee (PAC) contributions, both to candidates and to committees. Any individual contributing more than $200 is required by law to report the contribution, as well as to disclose their employer and occupation to the FEC. The stringency of this requirement sheds light on employee-level contributions as being an important component of a company's political activism.

To compute a measure of corporate campaign finance activity, we gather contribution-level observations for contributions made to candidates by PACs and by employees, both those tied to parent and to wholly-owned subsidiary companies. Subsequently, we pool all contributions at the parent-level each fiscal year. PAC committee names and identifying information are gathered from the FEC and then matched to contribution-level observations in the CRP data. Employee contributions are identified by company name within the CRP data.[50] Consistent with Goldman, Rocholl, and So (2009) and Coates (2012), among others, we restrict the sample to

[50] We manually reviewed all search results for actual matches and coded them accordingly.

S&P 500 firms. Financial statement data are gathered from the Compustat Fundamentals Annual file, stock returns are collected from CRSP, and names of subsidiary companies are retrieved from Mergent Online. We pull governance data from the RiskMetrics Directors database, RiskMetrics Shareholders Proposals file, and Execucomp Annual Compensation database. Our sample spans federal election cycle years 2006, 2008, 2010, and 2012, or fiscal years from 2005 to 2011.[51] To be included in the sample, a firm must appear in the data set in at least one pre-*Citizens United* year (2005, 2006, 2007, or 2008), and at least one post-*Citizens United* year (2010 or 2011).

V. Empirical Analysis

Table 19 provides descriptive statistics of political contributions on aggregate (Panel A) and firm-level (Panel B) bases. The mean (median) of firm-level political contributions is $142,448 ($57,225) across all firms, $24,703 ($4,800) for historically non-politically connected firms, and $297,735 ($169,170) for historically politically connected firms. When considering the sample in the aggregate, PAC contributions represent about 53% of the 373,393 contribution-level observations in our data set, but 67% of the sample-wide total contributions of slightly more than $441 million dollars. Non-executive employee contributions represent 35% of all contributions by count and 23% by dollar value, while executive contributions constitute 12% of all contributions by count and 9% by dollar value.

[51] To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. The main results of the paper are qualitatively very similar if this restriction is not imposed.

A. Tests of Hypothesis

January 21, 2010 marked a significant day of change in corporate political activism. Specifically, the Supreme Court's controversial decision in favor of *Citizens United* lifted long-standing limits on corporate political contributions. We exploit the announcement of *Citizens United* as a means to assess the effect of political connectedness on firm value. However, evaluating the market reaction to *Citizens United* is complicated by the fact that all sample firms have the same event date (January 21, 2010). Event clustering in calendar time induces cross-correlation in estimates of abnormal returns which creates downward-biased standard errors and upward-biased test statistics. As such, rather than use standard event study methodology, we instead estimate each sample firm's reaction to the ruling through a seemingly unrelated regression (SUR). The SUR framework simultaneously estimates a set of firm-specific equations that include cross-correlated error terms:

$$R_i = \alpha_i + \beta_i R_m + \delta_i Event + \varepsilon_i \quad (8)$$

where R_i is the return series for individual firm i, R_m is the return series for the CRSP value-weighted index (including dividends), and *Event* is a dummy variable that equals 1 on days included in the event window (0 otherwise). For example, for the (0,+2) window, *Event* is set to one on January 21, 2010 (Thursday), January 22, 2010 (Friday), and January 25, 2010 (Monday), and is zero otherwise. Daily returns are measured between April 1, 2008, and March 31, 2010, and are retrieved from the

CRSP daily returns file. The SUR methodology was developed by Schipper and Thompson (1983) and has since become increasingly utilized in corporate finance research (e.g., Doidge and Dyck 2015; Fernandes, Lel, and Miller 2010). This method enables us to measure the overall stock market reaction to *Citizens United*, while also accounting for cross-correlation in abnormal returns. Our main interest is in firm-specific estimates of $\hat{\delta}_i$ and, in particular, whether (1) the estimates jointly and significantly differ from zero, and (2) the estimates significantly differ across firms having varying degrees of political connectedness.

Table 20 reports the market reaction to *Citizens United* for our sample of historically politically connected firms ("POLITICAL") and historically non-politically connected firms ("NEUTRAL"). Our measure of historical political connectedness is based on median firm-level pre-*Citizens United* contributions (scaled by net total assets), where POLITICAL captures firms in the fourth quartile and NEUTRAL captures firms in the first quartile as well as historically non-politically active firms.[52, 53] Since $\hat{\delta}_i$ represents the average abnormal return for firm *i*, we multiply the average value of $\hat{\delta}_i$ estimates in each event window by increments of 100% for each day in the event window to obtain the cumulative abnormal return

[52] Using this definition, an example of a historically politically connected company in our data set is Corning Inc. A 2006 *New York Times* article declared Corning to be one of then-senator Hillary Clinton's largest sources of campaign contributions (McIntire and Hernandez 2006). Notably, in response to the announcement of *Citizens United*, shareholders of Corning Inc. realized a cumulative abnormal loss of −0.79% five days after the announcement date of *Citizens United*.

[53] Consistent with much of the existing, related literature (e.g., Aggarwal et al. 2012; Aslan and Grinstein 2012; Belo et al. 2013; Coates 2012), we rely on monetary political contributions to classify companies as being politically connected or not. However, we acknowledge the existence of a variety of other definitions of political connections. For example: Faccio and Parsley (2009) define a politically connected company as one that is headquartered in a politician's hometown; Goldman, Rocholl, and So (2009) capture political connectedness through political ties to a company's board of directors.

(CAR). For example, CAR(0,+2) is computed as the average value of $\hat{\delta}_i$ estimates resulting from a SUR regression where *Event* = 1 on days 0, +1, and +2, multiplied by 300% for the three days in the event window. Panel A provides descriptive statistics of abnormal returns for all sample firms and degrees of historical political connectedness. The mean (median) abnormal return on the date of the *Citizens United* decision is –0.08% (–0.12%) with a standard deviation of 1.59%. The mean (median) cumulative abnormal return for the (0,+5) window surrounding *Citizens United* is –0.48% (–0.61%) with a standard deviation of 3.65%. The average firm in our sample of POLITICAL and NEUTRAL firms has total assets (net of cash) of approximately $18.2 billion and market-to-book ratio of 2.25.

Panel B of Table 20 provides univariate tests of CARs. Results indicate a negative market reaction to historically politically connected firms persisting for five days beyond the announcement date, with a statistically significant negative reaction noted for all event windows considered [(0, 0), (0,+2), and (0,+5)]. On the announcement date, the average abnormal return to politically connected firms is –48 basis points (p-value = 0.028), with an average cumulative loss of 122 basis points five days after announcement date (p-value = 0.028). In contrast, historically *non*-politically connected firms exhibit a positive but insignificant market reaction over all event windows studied. For example, on the announcement date, this "neutral" subsample realized an average abnormal return of 0.240% (p-value = 0.173), and an average cumulative abnormal return of 0.123% five days after announcement date (p-value = 0.728). Tests for differences in means are statistically significant for all

three event windows (p-values of 0.010, 0.007, and 0.041 for the (0, 0), (0,+2), and (0,+5) windows, respectively). Nonparametric test results, provided in Panel B of Table 20, also support this result, with approximately 72%, 77%, and 68% of the POLITICAL group's CARs being negative for the (0, 0), (0,+2), and (0,+5) windows, respectively, all of which are statistically significant. Overall, these results suggest that politically connected firms face an unfavorable market reaction to *Citizens United*, and provide preliminary evidence that political connections destroy shareholder value. [54]

As a formal test of our prediction that the market reacted differently to *Citizens United* based on whether the firm was politically connected, we examine how the estimated CARs (average $\hat{\delta}_i$ from equation 8, expressed as percentage returns) differ with regard to a measure of historical political connectedness. To do so, we estimate the following regression model using ordinary least squares:

$$\hat{\delta}_i = \beta_0 + \beta_1 \text{Political Dummy}_i + \beta_2 \mathbf{X_i} + \varepsilon_i \quad (9)$$

[54] Contrary to our and Coates's (2012) findings, Werner (2011) finds that *Citizens United* constituted a "nonevent," posing no impact to firm value. Our research design differs from the Werner (2011) study in several respects. First, we use a seemingly unrelated regression design to account for the fact that all sample firms have the same event date. Second, consistent with Goldman et al. (2009), Coates (2012), and others, we utilize a sample of S&P 500 firms. The S&P 500 should yield more representative results than Werner's sample of Fortune 500 firms. Third, we classify firms into politically and non-politically connected sub-groups based on their *median* firm-level pre-*Citizens United* political contributions. Using *total* firm-level pre-*Citizens United* contributions as Werner does could bias the classification methodology when firms entered or exited the Fortune 500 during his sample period. This bias is likely more severe in a study of Fortune 500 firms than of S&P 500 firms, given the high degree of year-to-year turnover in the Fortune 500 (for a list of Fortune 500 exits in 2011, see http://money.cnn.com/magazines/fortune/fortune500/2011/movers/exits.html). Fourth, we follow Aggarwal et al. (2012), Claessens, Feijen, and (2008), Goldman et al. (2009), among others, in basing our definition of political connectedness on campaign contributions rather than the lobbying expenditures utilized in Werner (2011). Lastly, we provide a much more expansive study of *Citizens United*'s impacts, spanning the areas of firm value, corporate policies, and governance implications.

where $\hat{\delta}_i$ originates from the SUR estimation in equation 8, Political Dummy is our measure of historical political activism, and $\mathbf{X_i}$ is a vector of control variables measured in fiscal year 2009. For precise definitions of variables, see Appendix D.

Table 21 reports the coefficient estimates corresponding to equation 9. After controlling for size, the market-to-book ratio, and leverage, we find that the coefficient on Political Dummy is negative and statistically significant for all three event windows studied (p-values of 0.007, 0.012, and 0.042, for the (0, 0), (0,+2), and (0,+5) windows, respectively). In terms of economic significance, the coefficient on Political Dummy suggests that the day of the *Citizens United* decision resulted in the market penalizing historically politically connected firms by 75 basis points compared to historically non-politically connected firms. Considering the cumulative effect of *Citizens United* spanning the announcement day through the five days following, historically politically active firms realized a 133 basis points loss relative to their less-connected counterparts.[55, 56] The evidence shown in Tables 20 and 21 lend support to the main prediction of this paper in favor of the Agency Cost Hypothesis.[57]

[55] The analysis of the market reaction to *Citizens United* is a gauge of the decision's *short-term* impact on shareholder value. However, we conjecture that by lifting limits on political contributions, the decision meaningfully impacted the present value of all future contributions. In doing so, we also expect the *Citizens United* ruling to offer far-reaching effects on other corporate policies – a hypothesis that we formally test in the next section.

[56] In untabulated results, we investigate whether the negative reaction of politically connected firms holds using an alternate definition of political connectedness. Specifically, we create a rank variable based on median firm-level pre-*Citizens United* contributions (scaled by Net Total Assets). The results remain intact when we conduct the multivariate market reaction regression on this alternative political connectedness measure.

[57] In untabulated results, we examine the potential price reversals of politically connected firms in the long-run. Specifically, we construct monthly, equally-weighted portfolios that are long in

B. Corporate Policies and Citizens United

Given our finding that political connections destroy firm value, we next investigate whether the cash management practices of historically politically connected firms significantly differed from those of historically non-politically connected firms following passage of *Citizens United*. Table 22 provides univariate tests of cash holdings for firms exhibiting varying degrees of pre-*Citizens United* political connectedness. Panel A of Table 22 demonstrates that the most politically connected firms (upper quartile) hold significantly larger cash balances than less politically connected firms (lower quartile) in both the pre- and post-*Citizens United* periods. Comparing cash holdings levels in the pre- and post-*Citizens United* periods within each quartile, we find that most politically connected firms report larger cash balances (as a proportion of net total assets) in the post-*Citizens United* period. That is, politically connected firms significantly increased their cash holdings from the pre- to post-*Citizens United* periods, whereas the difference in cash holdings (pre- versus post-*Citizens United*) of the least politically connected firms is not statistically significant.[58] This initial evidence supports the existence of agency costs in politically connected firms.

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politically connected firms and short in neutral firms in the post-*Citizens United* period (March 2010-December 2012). We regress the returns to this portfolio on the four factors from the Fama and French (1992) and Carhart (1997) models. The intercept term of this regression is insignificant, suggesting that there is no price reversal in the long run.

[58] The mean (median) annual contributions-to-assets ratio across all firm-years is approximately 0.7% (0.5%). Although we do not claim that the amount of campaign contributions will be especially large for all firms in all fiscal years, it is important to recognize that their cumulative impact over time is indeed economically meaningful.

We also examine the association between traditional determinants of cash holdings and political connections. From Panel A of Table 22, we find no significant difference between the most politically connected firms (upper quartile) and the least politically connected firms (lower quartile, ignoring non-politically active firms) on important, traditional determinants of cash holdings. Specifically, in the post-*Citizens United* period, politically connected firms do not significantly differ from non-politically connected firms on the basis of size (either in terms of Net Total Assets or Sales), leverage, profitability, or the propensity to pay dividends. Moreover, as demonstrated in Panel B of Table 22, in the post-*Citizens United* period, these groups of firms do not significantly differ on the basis of any of the key governance metrics studied. Additionally, within quartiles of historical political connectedness, very few firm characteristics or measures of governance quality significantly differ in the post-*Citizens United* period relative to the pre-*Citizens United* period. Collectively, these results suggest that political connectedness is distinct from both traditional determinants of cash holdings and governance measures.

Did historically politically connected firms indeed increase their campaign contributions following the *Citizens United* decision? To investigate how political contributions changed in the post-*Citizens United* period and as a function of *historical* political connectedness, we estimate the following model during the post-*Citizens United* period (2010-2011):

$$\text{Contributions/NTA}_{i,t} = \beta_0 + \beta_1 \text{Political Dummy}_i + \beta_2 \mathbf{X_{i,}}_{t-1} + \varepsilon_{i,t} \quad (10)$$

where Political Dummy is our measure of historical political activism, and $\mathbf{X_i}$ is vector of control variables. We use lagged values of control variables to overcome a potentially endogenous relationship between contemporaneous political contributions and cash holdings. A positive (negative) β_1 coefficient would suggest that historically politically connected firms increased (decreased) their political contributions following the *Citizens United* decision. We note that our main interest is not in the amount in which historically politically connected firms contribute following *Citizens United*, but rather in their cash management policies surrounding this exogenous shock.

Table 23 provides the results of this equation estimated just for fiscal years 2010-2011. The positive and statistically significant coefficient on the Political Dummy (our measure of historical political activism) suggests that politically connected firms increased their political contributions following *Citizens United*.

To examine how cash holdings changed as a function of the change in political contributions in the post-*Citizens United* period relative to the pre-*Citizens United* period for our S&P 500 firms, we estimate the following changes specification:

$$\Delta\text{Log(Cash/NTA)}_i = \beta_0 + \beta_1 \Delta\text{Contributions/NTA}_i + \beta_2 \Delta \mathbf{X_i} + \varepsilon_i \qquad (11)$$

where Contributions/NTA is our measure of political involvement, $\mathbf{X_i}$ is vector of traditional determinants of cash holdings, and changes are computed as the value in 2010 (post-*Citizens United*) relative to the value in 2008 (pre-*Citizens United*). If the

level of corporate cash holdings is increasing in the degree of political connectedness (suggesting the *existence* of agency costs), we expect to find a positive β_1 coefficient. In modeling the determinants of cash holdings, we control for several firm characteristics. Specifically, consistent with Opler, Pinkowitz, Stulz, and Williamson (1999), Harford, Mansi, and Maxwell (2008) and others, we control for firm size, leverage, growth opportunities, contemporaneous cash flows, the standard deviation of cash flows, net working capital, R&D expenditures, capital expenditures, and the propensity to pay dividends. Although this model does not consider historical political activism, it provides a preliminary look into whether and, if so, how cash holdings and political contributions differed in 2010 relative to 2008.

Table 24 reports coefficient estimates corresponding to the simple difference regression (post- relative to pre-*Citizens United*) specified in equation 11. We find a positive and significant coefficient on ΔContributions/NTA (p-value = 0.043). That is, as political contributions increased from the pre- to post-*Citizens United* periods, so did corporate cash holdings. This evidence is consistent with the notion that politically connected firms are marred by agency conflicts.

To formally investigate whether the cash holdings levels of politically connected firms significantly differed from that of their less-connected counterparts following the *Citizens United* decision, we use a difference-in-differences research design. In this test, the differences stem from (1) a measure of *historical* political activism, and (2) pre- versus post-*Citizens United* periods. We model cash holdings

as a function of historical political activism in the pre- versus post-*Citizens United* periods:

$$\begin{aligned}\text{Log(Cash/NTA)}_{i,t} = {} & \beta_0 + \beta_1 \text{Political Dummy}_i + \\ & \beta_2 \text{Post-}\textit{Citizens United}\text{ Dummy}_{i,t} + \\ & \beta_3 (\text{Political Dummy}_i \times \text{Post-}\textit{Citizens United}\text{ Dummy}_{i,t}) + \\ & \beta_4 \mathbf{X_{i,}}_{t-1} + \varepsilon_{i,t}\end{aligned} \quad (12)$$

where Post-*Citizens United* Dummy is set to 1 for fiscal years 2010 and 2011 (0 otherwise), and all other variables are as defined previously. Regarding equation 12, β_1 measures the relation between cash holdings and political connectedness, while β_3 measures the *differential* relation between cash holdings and political connectedness during the post-*Citizens United* period. The primary empirical advantage of a difference-in-differences estimation is the ability to control for permanent differences between and time-varying differences within treatment and control groups (e.g., Roberts and Whited 2013). In our context, Political Dummy controls for permanent differences between POLITICAL and NEUTRAL firms (such as the tendency for politically connected firms to [on average] hold larger cash balances, as evidenced by Table 20), while Post-*Citizens United* Dummy accounts for trends among POLITICAL and NEUTRAL firms (such as the tendency for both groups to [on average] increase their cash holdings from the pre- to post-*Citizens United* period, as illustrated in Table 20). The remaining variation is the change in cash holdings levels (the outcome variable of interest) experienced by POLITICAL firms relative to the change in cash experienced by NEUTRAL firms, post- relative to pre-

Citizens United. The test of our hypothesis becomes $\beta_3 \neq 0$. That is, in reference to equation 12, $\beta_3 < 0$ (> 0) would suggest that political connections motivate a firm to hold less (more) cash relative to their less-connected counterparts following passage of *Citizens United* compared to before its passage.

Table 25 provides coefficient estimates and p-values based on standard errors clustered by firm corresponding to equation 12.[59] We find a positive association between political connections and cash holdings. Specifically, the relation between cash holdings and our measure of historical political connectedness significantly increased during the post-*Citizens United* period relative to the pre-*Citizens United* period (p-value = 0.026). In contrast, cash holdings of historically non-politically connected firms fell following *Citizens United*, although this finding is not statistically significant. In terms of economic significance, switching from non-politically connected status to politically connected status results in an incremental increase in corporate cash holdings of 19.84% following passage of *Citizens United*.[60] Taken as a whole, these findings are consistent with the view that politically connected firms suffer from agency problems (indicated by the retention of high cash holdings), but their less politically connected counterparts do not.

Coefficient estimates on traditional determinants of cash holdings are also largely consistent with previous studies (Opler et al. 1999; Harford et al. 2008). Specifically, current year cash holdings are positively associated with lagged values

[59] Petersen (2009) explains that fewer than ten time clusters is not sufficient to cluster standard errors by time. Thus, we report standard errors clustered by firm. Regardless, in untabulated analysis, results remain intact when we cluster standard errors by both firm and time.

[60] The incremental and economic effect of politically connected status in the post-*Citizens United* period = $e^{0.181} - 1 = 0.1984$.

of cash holdings, the market-to-book ratio, cash flow, cash flow volatility, and R&D expense. Current year cash holdings are negatively related to size, leverage, net working capital, capital expenditures, and the propensity to pay dividends. We also note that the adjusted R^2 of 79.8% provided in Table 25 is consistent with prior studies utilizing similar specifications that include the lagged value of cash holdings as an explanatory variable (e.g., Harford, Mansi, and Maxwell 2008; Agrawal and Nasser 2012).

C. Corporate Governance Quality and Political Connections

The evidence presented so far in this paper is consistent with the hypothesis that politically connected firms are characterized by agency problems. In this section, we examine whether corporate governance quality incrementally affects this overall finding. Existing evidence suggests that poor corporate governance quality magnifies agency conflicts between managers and stakeholders and diminishes the value of cash holdings (Dittmar, Mahrt-Smith, and Servaes 2003; Dittmar and Mahrt-Smith 2007; Harford, Mansi, and Maxwell 2008). Building upon the finding provided in Table 22 that political connectedness is distinct from traditional measures of corporate governance, we next investigate whether corporate governance quality moderates the effect of political connectedness on cash holdings. Specifically, we test whether poorly-governed, historically politically connected firms have higher cash holdings relative to well-governed and/or historically non-politically connected firms.

We capture governance quality through four measures. The first measure is the managerial entrenchment index (E-Index) proposed by Bebchuk, Cohen, and Ferrell (2009). Firms that facilitate the entrenchment of managers, characterized by staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments, are of inferior governance quality. The second measure is the external involvement of board members. Busy boards, defined here as the proportion of independent directors that serve on three or more boards, tend to become distracted and are less effective monitors because their attention is diverted away from the "home" corporation (Fich and Shivdasani 2006). The third measure of governance quality is defined in terms of excess CEO compensation. Excessive CEO compensation is more common in corporations with poor corporate governance structures (Core, Holthausen, and Larcker 1999). Since governance quality is decreasing in all three of these measures, we predict that the agency conflicts plaguing politically connected firms will be amplified when managerial entrenchment levels, the proportion of busy board members, and level of excess CEO compensation are high.

Our fourth measure of corporate governance quality addresses the role of investor activism as a monitoring mechanism. In the context of corporate political contributions, we search the RiskMetrics Shareholder Proposals database for politically-oriented shareholder proposals. We are not necessarily interested in determining the fate of these proposals, but instead consider the mere knowledge that a proposal is being considered to act as a monitoring mechanism and, as such,

impose restraints on a manager's tendency to hoard cash. For our POLITICAL (NEUTRAL) subgroup, 54.7% (65.5%) of firms entertained a politically oriented shareholder proposal during our sample period.

To test our predictions, we augment the cash holdings difference-in-differences model (equation 12) by incorporating measures of corporate governance:

$$\begin{aligned}\text{Log(Cash/NTA)}_{i,t} = {} & \beta_0 + \beta_1 \text{Political Dummy}_i + \\ & \beta_2 \text{Post-}\textit{Citizens United}\ \text{Dummy}_{i,t} + \beta_3 \text{Governance}_{i,t} + \\ & \beta_4(\text{Political Dummy}_i \times \text{Post-}\textit{Citizens United}\ \text{Dummy}_{i,t}) + \\ & \beta_5(\text{Political Dummy}_i \times \text{Governance}_{i,t}) + \\ & \beta_6(\text{Post-}\textit{Citizens United}\ \text{Dummy}_{i,t} \times \text{Governance}_{i,t}) + \\ & \beta_7(\text{Political Dummy}_i \times \text{Post-}\textit{Citizens United}\ \text{Dummy}_{i,t} \times \text{Governance}_{i,t}) + \\ & \beta_8 \mathbf{X_{i,}}_{t-1} + \varepsilon_{i,t} \end{aligned} \quad (13)$$

where Governance is one of the four measures of corporate governance quality mentioned previously. Since we take governance quality to be worsening in the busy-ness of directors, excessive compensation to CEOs, the magnitude of managerial entrenchment, and a lack of investor activism in politically-oriented matters, we posit that the agency problems (as captured by a build-up of cash) of historically politically connected firms in the post-*Citizens United* period are more severe when the degree of managerial entrenchment, the proportion of busy board members, and excessive compensation to the CEO are high, and when the firm is not required to entertain a politically-oriented shareholder proposal. That is, for these four proxies of governance quality, our prediction becomes $\beta_7 > 0$.

Table 26 provides results of our tests of the impact of governance quality on the agency implications of corporate political connections. Columns I, II, and III provide estimates of equation 13 using the first three proxies of governance quality. The results are largely consistent with agency costs of politically connected firms. Specifically, we find that the busy-ness of boards is associated with larger cash holdings for historically politically connected firms in the post-*Citizens United* period (p-value = 0.065). Similarly, an excessively compensated CEO is associated with an incremental enhancement of agency problems in historically politically connected firms following the *Citizens United* decision (p-value = 0.028). Additionally, we find that higher managerial entrenchment levels are associated with a build-up of cash among politically connected firms following the *Citizens United* decision, though this finding is not significant at conventional levels (p-value = 0.114). Taken together, the results provided in the first three columns of Table 26 are largely consistent with the prediction that weak governance exacerbates agency problems in politically connected firms.

The remaining three columns of Table 26 provide the results of estimating equation 13 using politically-oriented shareholder proposals as the measure of governance quality (columns IV, V, and VI). Consistent with expectations, we find that failing to entertain a politically-oriented shareholder proposal during either the 2007-2008 (pre-*Citizens United*) or 2010-2011 (post-*Citizens United*) periods results in an incremental increase in the cash holdings of historically politically connected firms relative to historically non-politically connected firms in the post-*Citizens*

United period relative to the pre-*Citizens United* period (p-value = 0.035). We further find that the incremental increase in agency problems (and cash holdings) is much greater for politically-oriented shareholder proposals entertained during the post-*Citizens United* period (column VI of Table 26; p-value = 0.062). This result supports the intuition that the passage of a rule that lifted long-standing restrictions on corporate political activism only amplified existing agency conflicts of politically connected firms. Taken together, these findings support a positive association between political connections and agency costs.

D. A Clash of Colors: Red versus Blue Corporate Contributions

The results of this paper suggest that political connections are value-destroying, and that connected firms are marred by agency conflicts. Agency conflicts arise when a manager's motives diverge from that of his stakeholders. It is rational to expect agency problems to be heightened by a divergence in the party affiliation of political contributions made by executive employees and those made by *non*-executive employees. To capture the degree of political tension within a corporation, we calculate the absolute deviation between the proportion of executive employee contributions to Republicans and the proportion of non-executive employee contributions to Republicans for historically politically active firms. We compute the median firm-level deviation during the pre-*Citizens United* period, and sort firms into deciles. To examine for the existence of agency costs in the post-*Citizens United* period, we estimate the following model:

$$\text{Log(Cash/NTA)}_{i,t} = \beta_0 + \beta_1 \text{Political Deviation}_i + \beta_2 \text{Post-}\textit{Citizens United}\ \text{Dummy}_{i,t} + \beta_3 (\text{Political Deviation}_i \times \text{Post-}\textit{Citizens United}\ \text{Dummy}_{i,t}) + \beta_4 \mathbf{X_{i,\,t-1}} + \varepsilon_{i,t} \quad (14)$$

where Political Deviation is a continuous variable ranging from 0.10 (bottom decile of deviation) to 1.00 (top decile of deviation) and $\mathbf{X_i}$ is a vector of lagged control variables as defined previously. Equation 14 is estimated for firms in the upper quartile of pre-*Citizens United* contributions and for years in which these firms report both positive executive contributions and positive non-executive contributions. We predict agency conflicts to be increasing in the extent to which the party affiliation of executive contributions deviates from that of non-executive contributions. Moreover, we expect *Citizens United* to only exacerbate the agency problems of firms with historically high degrees of political tension. Given our predictions, we expect to find a positive β_3.

Table 27 depicts the impact of within-firm political tension on cash holdings. The results suggest that as the degree of deviation between the party affiliation of executive and non-executive political contributions increases, corporate cash holdings increase in the post-*Citizens United* period (p-value = 0.011). These results lend further support to agency conflicts as a plausible explanation for the value-destroying effects of corporate political connections.

E. Alternative Explanations & Robustness Checks

1. Alternative Explanations

We next address several alternative explanations of our findings. First, we acknowledge the possibility that *Citizens United* transformed the competitive landscape to a focus on corporate political activism (and its associated impact on cash holdings). To account for this possibility, it is important that we incorporate metrics whose reliability is not affected by *Citizens United*. Our analysis of corporate governance quality achieves this goal. That is, by focusing on widely-accepted governance metrics whose dependability was not affected by *Citizens United* (Table 22), we are better able to exploit the exogenous enhancement in firm value and corporate policies that accompanied *Citizens United*.

Second, we recognize that the media scrutiny and related negative attention accompanying *Citizens United* may have made it more difficult for all companies to use political involvement as a means to enhance firm value. To address this concern, we have utilized a measure of political connectedness that is based on *historical* campaign contributions. Specifically, our finding that politically connected firms realized a negative market reaction to the *Citizens United* decision is based on their political activism in the pre-*Citizens United* period (Tables 20 and 21). Most importantly, this result does not depend on the amount, cost, and potentially unfavorable consequences of their post-*Citizen United* political activity.

Third, we revisit our presumption that *Citizens United* more meaningfully impacted historically politically active firms relative to their politically *in*active

counterparts. A counter-argument states that because *Citizens United* lifted restrictions on certain forms of previously barred political activities, the decision gave the upper hand to historically non-politically active firms. If true, we would expect to observe a significant positive market reaction on the part historically non-politically active firms. Instead, our results reveal that *Citizens United* was accompanied by an insignificant (though positive) market reaction to non-politically active firms (Table 20 Panel B), thus disputing this counter-argument.

Lastly, we address the conjecture that the negative reaction to historically politically active firms surrounding the *Citizens United* decision was driven by a higher cost of political connections. That is, *Citizens United* opened up a new channel through which corporations could funnel their political contributions. We propose that the negative market reaction to politically connected companies is driven by the agency costs of political connections and, specifically, self-interested managers' tendencies. A counter-argument states that adding a new mode of corporate political activity will compel politically connected firms to contribute larger sums of money to political causes in order to maintain an equivalent (pre-*Citizens United*) level of political connectedness. We do not dispute this possibility and, in fact, provide empirical evidence to support this idea (see Table 23). However, if dollar-for-dollar political connections enhance firm value (Value Enhancing Hypothesis), the incrementally higher contributions of politically connected companies should be viewed favorably by market participants. If instead an additional $1 of political contributions destroys firm value (Agency Cost

Hypothesis), the now larger level of contributions should be viewed *un*favorably by market participants. Indeed, the premise and implications of our hypotheses stand independent of this alternative explanation.

2. Parallel Trends Assumption

To infer causality from difference-in-differences (DID), the parallel trends assumption must hold: in the absence of treatment, historically politically connected firms (treated group) and historically non-politically connected firms (untreated group) should exhibit a common trend. To test this assumption, we mechanically shift the post-*Citizens United* event window backwards by 1, 2, and 3 years (e.g., Amore and Bennedsen 2013; Irani and Oesch 2013). That is, we consider the change in cash holdings of historically politically connected firms relative to their non-connected counterparts surrounding fictitious *Citizens United* events occurring on January 21st of years 2009, 2008, and 2007. If our result of higher cash holdings by historically politically connected firms in response to *Citizens United* is not the result of overall trends, we expect to find an insignificant coefficient on the interaction term corresponding to equation 12. Results, provided in Table 28, support this conjecture and corroborate the reliability of inferences drawn from DID estimates in our research setting.

3. Potential Confounding Events

A fundamental concern underlying event studies is that other events occurred during the event window. These confounding events can interfere with our ability to draw accurate inferences about the impact of the event of interest. We repeat our analysis of the market reaction to *Citizens United* after excluding firms that announced earnings or other significant corporate events, as identified in company press releases and newswire reports in the *LexisNexis Academic* database. Of the combined, initial sample of POLITICAL and NEUTRAL firms (133 observations), the number of firms with confounding events ranged from 8 (for the –1, 0 window) to 43 (for the –1, +5 window). Results of this sensitivity check, provided in section I of Tables 29 and 30, are consistent with earlier event study results (Tables 20 and 21). For example, after excluding firms with potential confounding events, section I of Table 29 demonstrates that historically politically connected firms realized an abnormal price drop of –0.592% on announcement date (p-value = 0.002) and a cumulative abnormal loss of –0.785% five days after announcement date (p-value = 0.090). Thus, our finding that political connections are value-decreasing is robust to controlling for confounding events.

4. Politically Exposed Industries

Companies are inherently motivated to influence the legislative process in order to achieve more favorable corporate outcomes (Grossman and Helpman 1994). Moreover, as suggested by Stigler (1971), this incentive is likely more prevalent in

regulated industries as regulated firms tend to exhibit a heightened sensitivity to protecting their private interests and, as such, are more willing to join forces when lobbying for or against legislative proposals. Firms belonging to the defense, energy, and utilities industries face significantly greater political exposure relative to other industries. One might expect that politically exposed companies would be unduly harmed by *Citizens United* since they are, by definition, compelled to contribute to political campaigns. In particular, it is possible that the value-destroying effects of political connections documented in this paper are primarily driven by companies whose political contributions are largely involuntary and, as such, most likely not attributable to agency conflicts.

To ensure that these politically exposed industries are not driving the results, we repeat our analysis of the market reaction to *Citizens United* as well as the primary difference-in-differences test used to evaluate whether political connections are positively associated with agency costs. First, similar to Goldman, Rocholl, and So (2009), we repeat our analysis after excluding the defense, energy, and utilities industries. Second, we redefine our measure of historical political connectedness (Political Dummy) based on a firm's median firm-level pre-*Citizens United* contributions relative to the industry median. Results of sensitivity tests on the market reaction results are provided in sections II and III of Table 29 (univariate) and Table 30 (multivariate); results for the primary difference-in-differences results are provided in columns I and II of Table 31. Collectively, these results demonstrate

that our empirical findings are qualitatively unchanged after accounting for varying degrees of political exposure.[61]

5. Placebo Analysis

A question native to all empirical analyses of quasi-natural experiments is whether the documented results are truly driven by the purported effect or are instead a mere artifact of the data. As it applies in our setting, a reader may question whether the negative market reaction to politically connected companies surrounding the *Citizens United* decision can be explained by some factor other than corporate political connections. To address this concern, we conduct a "placebo analysis" in which we compare the market reaction of politically connected companies to a group of otherwise similar companies ("placebo-neutral" firms). A group of "placebo-neutral" firms is defined as companies that match to politically connected firms on industry (two-digit SIC) and size (Log of Total Assets) within the same fiscal year, where potential matches are drawn from the entire universe of Compustat firms. We re-estimate the impact of *Citizens United* by comparing the market reactions of historically politically connected companies to their "placebo-neutral" counterparts.

[61] In untabulated results, we evaluate the market reaction and cash holdings implications of *Citizens United* after defining an indicator variable, Exposed Dummy, to capture the defense, energy, and utilities industries. Specifically, our multivariate test of the market reaction to *Citizens United* becomes $\hat{\delta}_i = \beta_0 + \beta_1$Political Dummy$_i$ $+ \beta_2$Exposed Dummy$_i$ $+ \beta_3$(Political Dummy$_i$ × Exposed Dummy$_{i,t}$) $+ \beta_4 X_i + \varepsilon_i$. The resulting β_3 coefficient is statistically *in*significant for all three event windows (i.e., p-values of 0.833, 0.846, and 0.448 for the (0, 0), (0,+2), and (0,+5) windows, respectively), lending further support to our prediction that politically exposed firms are not driving the results. To evaluate the impact of *Citizens United* on the cash holdings of politically exposed firms, we adopt a difference-in-difference-in-differences specification. This specification parallels equation 6, with Exposed Dummy taking the place of the Governance term. The main coefficient of interest, β_7, is statistically *in*significant (p-value = 0.919), again favoring the notion that our findings are robust to controlling for the degree of corporate political exposure.

Results, provided in section IV of Table 29, support the earlier univariate results (Table 20 Panel B). Notably, the negative market reaction of politically connected companies is in stark contrast to a positive market reaction on the part of "placebo-neutral" companies. For example, while the announcement-date reaction of politically connected companies is –0.475%, the average reaction realized by "placebo-neutral" firms is +0.609%, the difference of which continues to be statistically significant (p-value = 0.0003). The results of this placebo study support our conclusion that the negative market reaction of politically connected companies to *Citizens United* is in fact driven by their political connections.

6. Market Reaction Implications of Corporate Governance Quality

To this point, our primary interest in multivariate market reaction tests has been the direction and significance of the coefficient on our measure of historical political connectedness, the Political Dummy. In this sensitivity test, we split the Political Dummy into its components, POLITICAL (Political Dummy = 1) and NEUTRAL (Political Dummy = 0), and subsequently interact these variables with a measure of weak corporate governance quality (as before, the E-Index, busy board members, or excessive CEO compensation). If politically connected companies' adverse reaction to *Citizens United* is attributable to agency problems, we expect the negative reaction to also hold for politically connected companies that are poorly governed (and inherently more susceptible to agency problems). Results, provided in section IV of Table 30, provide support for this conjecture. Specifically,

politically connected companies that are characterized by entrenched managers or excessively high CEO compensation levels realize a significant negative market reaction to *Citizens United* (p-values of 0.005 and 0.027, respectively). This evidence lends support to an agency-based explanation for politically connected companies' negative market reaction to *Citizens United*.

7. Excess Cash

Our main test of whether politically connected firms are marred by agency conflicts utilized a difference-in-differences research design, where the dependent variable was defined as the level of corporate cash holdings (scaled by net total assets). To more explicitly control for industry and time series patterns in cash holdings, we adjust the continuous variables in equation 12 by their annual industry medians. This approach enables us to account for both cross-sectional and time-series variation in variables of interest. Column III of Table 31 reports the results from regressing this measure of excess cash on industry-adjusted traditional determinants and measures of political connectedness. We continue to find a positive and significant effect of political connectedness on cash holdings, demonstrating that our empirical conclusions do not depend on variation either within industry or across time.

8. Infrequent Contributors

Rather than support a broad range of political candidates, a company may tend to contribute to just one congressional office – suppose, a candidate for state senator. Our definition of political connectedness as defined over the pre-*Citizens United* period captures two congressional elections (2006 and 2008), each instance of which encompassed the re-election of all U.S. House seats but only one-third of U.S. Senate seats. Given that approximately one-third of U.S. Senate seats were not subject to re-election during either the 2006 or 2008 elections, a company making infrequent contributions – particularly, by exclusively (though to a potentially large extent) supporting a candidate for the U.S. Senate – would be classified as politically *in*active by our measure. To address this concern, we redefine our proxy for historical political connectedness after omitting contributions to candidates for the U.S. Senate. In unreported sensitivity analyses, we find qualitatively similar results with regard to the direction, magnitude, and significance of the market reaction to *Citizens United* and its implications on corporate policies, both for politically and non-politically connected firms. Therefore, our results do not depend on the frequency of political contributions.

9. Financial Firms

Financial firms are also known to systematically differ from other sectors of the economy along key corporate policy and governance dimensions. In untabulated sensitivity tests, we exclude financial firms (SIC codes 6000-6999) and find

qualitatively similar results in the context of our market reaction and difference-in-differences analyses.[62] Thus, financial firms do not drive the results of the paper.

VI. Conclusion

Political connections may enhance or harm shareholder value. However, existing insights attempting to address the impact of corporate political connectedness on shareholder value are inconclusive. In an effort to test for the existence of a causal link between political connections and changes in shareholder value, we pose our research questions in the context of a quasi-natural experiment. Specifically, we focus on an exogenous enhancement in the value implications of political connectedness that accompanied the landmark Supreme Court case, *Citizens United.*

The findings in this paper support a negative relation between political connections and firm value. Using an event study methodology and a seemingly unrelated regression design, we find that politically connected firms realized significant negative abnormal returns following the announcement of the *Citizens United* decision. Specifically, historically politically connected firms realized an abnormal price drop of –0.475% on announcement date and a cumulative abnormal loss of –1.219% five days after announcement date, while historically non-politically

[62] Information regarding a decision in the *Citizens United* case hit the newswires at approximately 10:00 a.m. EST on January 21, 2010 (see, for example, Tedford 2010). At 11:34 a.m. EST, President Obama delivered a speech in which he publicly endorsed the Volcker Rule (The White House, Office of the Press Secretary 2010). Given that the financial sector reacted negatively to the Volcker Rule and its endorsement, it is possible that financial firms were reacting to the Volcker Rule endorsement rather than to the *Citizens United* decision. The fact that our results are robust to the exclusion of financial firms provides assurance that this overlap in events does not materially affect the findings of this paper.

connected firms enjoyed positive returns on announcement date in the order of 0.240%. Furthermore, the negative effect of political connectedness is significant for the subsample of firms with weak corporate governance. Collectively, these findings are consistent with a positive association between agency costs and political connectedness.

Given the negative implications of political connectedness on firm value, we investigate whether the corporate policy decisions of politically connected firms are suggestive of agency conflicts. Building upon previous studies that document a positive association between agency problems and cash holdings (e.g., Jensen 1986; Stulz 1990; Harford 1999), we evaluate whether the corporate cash holdings of connected firms significantly differ from those associated with their less-connected counterparts.

The findings in this paper reveal a connection between political activism and corporate policies. Using a difference-in-differences research design that controls for relevant firm characteristics, we find that politically connected firms increased their cash holdings following passage of *Citizens United relative* to before its passage. Poor corporate governance quality, captured by managerial entrenchment, busy board members, excessive remuneration to the CEO, and investor inactivism in the context of politically-oriented shareholder proposals also aggravates the existing agency conflicts within politically connected firms, as evidenced by an incrementally and significantly greater level of cash holdings. Finally, political tension within a firm, defined as divergence in the party affiliation of political contributions of

executive employees relative to that of *non*-executive employees, is associated with incrementally greater cash holdings. Collectively, our findings are consistent with the existence of agency problems in politically connected firms.

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Appendix A: Chapter 1 Variable Definitions

This table provides definitions of all variables in Chapter 1 and Tables 1 through 11. See Panel 1 for definitions of the main variables, Panel 2 for definitions of governance index variables, and Panel 3 for details on the construction of instrumental variables. Numbers in brackets denote the corresponding Revised Form 990 section, line, and column numbers.

Panel 1: Main variables.

Age	The Age of the firm, calculated relative to the Year of Formation [Page 1, L]
Asset Tangibility	The ratio of Land, Buildings, and Equipment, Net [X.10c.B] to Total Assets [X.16.B]
Board Size	The number of voting members of the governing body [VI.A.1a]
CEO Tenure	The number of years the CEO has held the title of CEO excluding the year of his or her appointment
CEO Turnover Dummy	Set equal to 1 when the CEO in year *t–1* is not the CEO in year *t*, and 0 otherwise
Commercial Dummy	Set equal to 1 when the ratio of Program Services Revenue to (Program Services Revenue + Donations Revenue) is at least 90%, and 0 otherwise ([VIII.2g.A] / [VIII.2g.A] + [VIII.1h.A])
Donations Growth	The change in the ratio of Net Contributions to Total Revenue [VIII.12.A] in year *t–1* relative to year *t–2* , where Net Contributions = Total Contributions [VIII.1h] – Government Revenue [VIII.1e]
First-class Flights Indicator	Set to 1 if the organization indicates that first-class or charter travel was provided to an executive during year *t* [Schedule J, I.1a]
Fundraising Dummy	Set equal to 1 when Total Fundraising Expenses [IX.25.D] are zero, and is 0 otherwise
Fundraising Ratio	The ratio of (Net Contributions – Net Fundraising Expenses) to Net Contributions, where Net Fundraising Expenses = Total Fundraising Expenses – Compensation allocated to Fundraising Expenses ([IX.25.D] – [IX.5.D] – [IX.6.D] – [IX.7.D]) and Net Contributions = Total Contributions – Government Revenue ([VIII.1h] – [VIII.1e])
Gender Dummy	Set equal to 1 if the CEO is a female, and is 0 otherwise
Governance Index	See "Governance Index" in Appendix A, Panel 2; defined as the average of four governance sub-indices (*Governing Body, Governing Policies, Compensation Policies, Accountability & Transparency*), where each sub-index is defined as the ratio of the sum of the indicator variables as a proportion of the total number of possible responses for each firm-year observation where each response is weighted by its annual cross-sectional standard deviation
Government Revenue	The ratio of Government Grants [VII.1e] to Total Revenue [VIII.12.A]
High Program Spending Dummy	Set equal to 1 when the *Program Spending Ratio* exceeds the annual industry median value, and is 0 otherwise
Household Income	Median household income (as reported by the U.S. Census Bureau's American Community Survey); computed for each state and fiscal year pair
Liquid Assets/Total Expenses	The ratio of Liquid Assets to Net Total Expenses, where Liquid Assets = Cash + Savings and Temporary Cash Investments + Pledges and Grants Receivable, Net + Accounts Receivable, Net, and Net Total Expenses = Total Expenses – Compensation Expense ([X.1.B] + [X.2.B] + [X.3.B] + [X.4.B]) / ([IX.25.A] – [IX.5.A] – [IX.6.A] – [IX.7.A])

Appendix A (Continued)

No. of Employees	The Number of Employees [I.5], i.e. the number of compensated employees (full-time or part-time) listed on the entity's W-3, "Transmittal of Wage and Tax Statements"
NTEE-10 Industry Classifications	A set of 10 groups defined by the National Taxonomy of Exempt Entities (NTEE): Arts, Culture, and Humanities; Education; Environment and Animals; Health; Human Services; International, Foreign Affairs; Public, Societal Benefit; Religion Related; Mutual/Membership Benefit; and Unknown, Unclassified
NTEE-26 Industry Classifications	A set of 26 groups defined by the National Taxonomy of Exempt Entities (NTEE): Animal-Related; Arts, Culture, and Humanities; Civil Rights, Social Action, Advocacy; Community Improvement, Capacity Building; Crime, Legal Related; Diseases, Disorders, Medical Disciplines; Education; Employment, Job Related; Environmental Quality, Protection, and Beautification; Food, Agriculture, and Nutrition; Health; Housing, Shelter; Human Services; International, Foreign Affairs, and National Security; Medical Research; Mental Health; Mutual/Membership Benefit Organizations; Philanthropy, Voluntarism, and Grantmaking Foundations; Public Safety; Public, Society Benefit; Recreation, Sports, Leisure, Athletics; Religion Related; Science and Technology Research Institutes; Social Science Research Institutes; Youth Development; Unknown
Perquisites Indicator	Set to 1 if the organization indicates that at least one type of perquisite was provided to an executive during year *t* [Schedule J, I.1a] Note: The eight possibilities listed are first-class or charter travel, travel for companions, tax indemnification and gross-up payments, discretionary spending account, housing allowance or residence for personal use, payments for business use of personal residence, health or social club dues or initiation fees, and personal services (e.g., maid, chauffeur, chef).
Program Expenses to Assets	The ratio of Net Program Service Expenses to Total Assets, where Net Program Service Expenses = Total Program Expenses – Compensation allocated to Program Service Expenses ([IX.25.B] – [IX.5.B] – [IX.6.B] – [IX.7.B]) / [X.16.B]
Program Spending Ratio	The ratio of Net Program Service Expenses to Net Total Expenses, where Net Program Service Expenses = Total Program Expenses – Compensation allocated to Program Service Expenses and Net Total Expenses = Total Expenses – Compensation Expense ([IX.25.B] – [IX.5.B] – [IX.6.B] – [IX.7.B]) / ([IX.25.A] – [IX.5.A] – [IX.6.A] – [IX.7.A])
Relative Pay Ratio	The ratio of CEO Compensation to Average Non-CEO Employee Pay (CEO Compensation as identified in [VII.1a.D] divided by the sum of ([IX.5.A] + [IX.6.A] + [IX.7.A] – CEO Compensation) scaled by the number of employees [I.5]), where CEO Compensation is "reportable compensation from the organization" (essentially, that which is reported on a W-2; in accordance with Internal Revenue Service guidelines, businesses are required to generate a W-2 for all employees for which they compensate as part of an employment relationship)
Revenue per Employee	The ratio of Net Total Revenue to the Number of Employees [I.5], where Net Total Revenue = Total Revenue [VIII.12.A] – Total Contributions [VIII.1h]
Total Assets	Total Assets [X.16.B]
Total Revenue	Total Revenue [VIII.12.A]
Total Liabilities/Total Assets	The ratio of Total Liabilities [X.26.B] to Total Assets [X.16.B]

Appendix A (Continued)

Panel 2: Components of the governance index.

	Governing Body	
Board Independence	Is at least two-thirds of the organization's board of directors is independent? [VI.A.1b / VI.A.1a]	= 1 if Yes = 0 if No
Inside Business Relationship	Did any person who is a current or former officer, director, trustee, or key employee have a direct business relationship with the organization [IV.28a], have a family member who has a direct or indirect business relationship with the organization [IV.28b], or serve as an officer, director, trustee, key employee, partner, or member of an entity doing business with the organization [IV.28c]?	= 1 if No = 0 if Yes
Outside Management	Did the organization delegate control over management duties customarily performed by or under the direct supervision of officers, directors or trustees, or key employees to a management company or other person? [VI.A.3]	= 1 if Yes = 0 if No
Stockholders	Does the organization have members or stockholders [VI.A.6] who may elect one or more members of the governing body [VI.A.7a] and approve the decisions of the governing body [VI.A.7b]?	= 1 if Yes = 0 if No
Form 990 to Board	Was a copy of the Form 990 provided to the organization's governing body before it was filed? [VI.A.10]	= 1 if Yes = 0 if No
	Governing Policies	
Conflict of Interest Policy	Does the organization have a written conflict of interest policy [VI.B.12a] that is regularly and consistently monitored and enforced [VI.B.12c], and that requires officers, directors or trustees, and key employees to disclose annually interests that could give rise to conflicts [VI.B.12b]?	= 1 if Yes = 0 if No
Whistleblower Policy	Does the organization have a written whistleblower policy? [VI.B.13]	= 1 if Yes = 0 if No
Document Retention Policy	Does the organization have a written document retention and destruction policy? [VI.B.14]	= 1 if Yes = 0 if No
Grant to Officer	Did the organization provide a grant or other assistance to an officer, director, trustee, key employee, or substantial contributor, or to a person related to such an individual? [IV.27]	= 1 if No = 0 if Yes
	Compensation Policies	
CEO Compensation Policy	Indicate the number of ways that the organization uses to establish the compensation of the organization's CEO/Executive Director [Schedule J, I.3]. Note: The six possibilities listed are a compensation committee, independent compensation consultant, Form 990 of other organizations, written employment contract, compensation survey or study, and approval by the board or compensation committee.	Continuous variable
Non-CEO Compensation Policy	Did the process for determining compensation of the non-CEO officers or key employees of the organization include a review and approval by independent persons, comparability data, and contemporaneous substantiation of the deliberation and decision? [VI.B.15b]	= 1 if Yes = 0 if No

Appendix A (Continued)

Executive Reimbursement Policy	If the organization provided fringe benefits to any person listed in the Schedule of Compensation to Officers, Directors, Trustees, Key Employees, and Highest Compensated Employees [Schedule J, I.1a], did the organization follow a written policy regarding payment or reimbursement or provision of all of the associated expenses [Schedule J, I.1b]?	= 1 if Yes = 0 if No
Executive Substantiation Policy	If the organization provided fringe benefits to any person listed in the Schedule of Compensation to Officers, Directors, Trustees, Key Employees, and Highest Compensated Employees [Schedule J, I.1a], did the organization require substantiation prior to reimbursing or allowing the associated expenses [Schedule J, I.2]?	= 1 if Yes = 0 if No
	Accountability & Transparency	
Tax Forms on Website	Does the organization make its Form 1023 (or 1024 if applicable), 990, and 990-T available for public inspection on its own website? [VI.C.18]	= 1 if Yes = 0 if No
Audited Financial Statements	Were the organization's financial statements compiled, reviewed, or audited by an independent accountant? [XI.2a-b] (If audited, the audit must not be a Circular A-133 audit [i.e., "No" to XI.3a])	= 1 if Audited = 0.50 if Reviewed or Compiled = 0 if No
Audit Committee	If the organization's financial statements are compiled, reviewed, or audited by an independent accountant [XI.2a-b], does the organization have a committee that assumes responsibility for oversight of the audit, review, or compilation of its financial statements and selection of an independent accountant [XI.2c]?	= 1 if Yes = 0 if No

Panel 3: Construction of instrumental variables.

Instrumental variables estimations incorporate instruments for (1) organizational performance, and (2) relative pay. For organizational performance, the instrument is defined as the mean performance of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. For relative pay, the instrument is defined as the mean value of *Log(Relative Pay)* of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Groups are formed according to the following criteria:

- Size: three splits (small, medium, large) based on total revenue
- Industry: eight splits (Arts, Education, Environment, Health, Human Services, International, Public and Societal Benefit, Religion) [b]
- Geographical region: nine splits in accordance with the U.S. Census Bureau's census codes (New England, Middle Atlantic, North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, Pacific)
- Year (fiscal)

Groups are required to entail a minimum of 5 observations to be included in instrumental variables estimations.

[a] Industry classifications are based on the 26 NTEE major groupings.

[b] Industry classifications are based on the 10 NTEE major groupings, excluding the "Mutual Benefit" (for which there are no observations) and "Other" industries.

Appendix B: Chapter 1 Governance Index Weights

This table provides the cross-sectional standard deviations of each of the sixteen governance variables listed in Panel 2 of Appendix A. These values are used as weights in forming the governance index, as described in the *Governance Index* definition in Panel 1 of Appendix A.

Governing Body	
Board Independence	0.500
Inside Business Relationship	0.433
Outside Management	0.266
Stockholders	0.378
Form 990 to Board	0.396

Governing Policies	
Conflict of Interest Policy	0.404
Whistleblower Policy	0.442
Document Retention Policy	0.429
Grant to Officer	0.244

Compensation Policies	
CEO Compensation Policy	0.315
Non-CEO Compensation Policy	0.480
Executive Reimbursement Policy	0.346
Executive Substantiation Policy	0.273

Accountability & Transparency	
Tax Forms on Website	0.236
Audited Financial Statements	0.409
Audit Committee	0.299

Appendix C: Chapter 2 Variable Definitions

This table provides definitions of all variables in Chapter 2, Tables 12 through 18, and Figures 1 and 2. See Panel 1 for definitions of the expectations model variables and Panel 2 for definitions of partitioning variables. Numbers in parentheses denote the corresponding OSHPD Hospital Annual Financial data page number, column number, and line number.

Panel 1: Expectations model variables.

$CORE_EXP_t$	[Daily Hospital Services Expense (8, 1, 146) + Ambulatory Services Expense (8, 1, 151) + Ancillary Services Expense (8, 1, 156)] / Gross Patient Revenue (8, 1, 30), measured in year *t*
$NONCORE_EXP_t$	[Research Costs (8, 1, 161) + Education Costs (8, 1, 166) + General Services (8, 1, 171) + Fiscal Services (8, 1, 176) + Administrative Services (8, 1, 181) + Unassigned Costs (8, 1, 186)] / Gross Patient Revenue (8, 1, 30), measured in year *t*
ATO_t	Operating Asset Turnover, defined by the OSHPD as Total Operating Revenue (8, 1, 140) / [Total Current Assets (5, 1, 55) + Net Property, Plant & Equipment (5, 1, 200)]
ΔREV_t	Percentage Change in Gross Patient Revenue (8, 1, 30), calculated as [Gross Patient Revenue$_t$ – Gross Patient Revenue$_{t-1}$] / Gross Patient Revenue$_{t-1}$
$NEG_\Delta REV_t$	Percentage Change in Gross Patient Revenue (ΔREV_t) if ΔREV_t is less than 0, and 0 otherwise
CURRENT	Total Current Assets (5, 1, 55) / Total Current Liabilities (5, 3, 60)
DAYS_CASH	Days Cash on Hand measured in year *t*, calculated as [Cash (5, 1, 5) + Marketable Securities (5, 1, 10)] / [[Total Operating Expenses (8, 1, 200) + Depreciation & Amortization Expense (9, 1, 15)]/ Days in Period], where Days in Period is the difference between the Report End Date (0, 1, 39) and the Report Begin Date (0, 1, 38)
TEACHING	1 if the hospital is identified as a teaching hospital in year *t* based on OSHPD Annual Financial data pivot profiles, and 0 otherwise
LnAGE	The logarithm of the number of years since facility was originally licensed (based on OSHPD Utilization Data File, 1.21.3) as of year *t*
LnASSETS	The logarithm of Total Assets (5, 1, 270) measured in year *t*
CMI	Case Mix Index in year *t* based on OSHPD Case Mix Index files (http://www.oshpd.ca.gov/HID/Products/PatDischargeData/CaseMixIndex/default.asp)
HHI	Herfindahl-Hirschman Index, calculated as Total Discharges (4, 12, 150) as a proportion of Total Discharges in the County of hospital residence, measured in year *t*

Appendix C (Continued)

Panel 2: Partitioning variables.

RURAL$^{A(B)}$	1 if the hospital was a rural hospital in year *t* based on OSHPD Annual Financial data pivot profiles and reports earnings well above (below) the zero-profit benchmark, 0 otherwise
CHURCH$^{A(B)}$	1 if the hospital was a church hospital in year *t* (1, 2, 5) and reports earnings well above (below) the zero-profit benchmark, and 0 otherwise
SYSTEM$^{A(B)}$	1 if the hospital was a system hospital in year *t* based on OSHPD Annual Financial data pivot profiles and reports earnings well above (below) the zero-profit benchmark, 0 otherwise
HIGH_CHARITY$^{A(B)}$	1 if the hospital provided Charity Care above the annual sample median and reports earnings well above (below) the zero-profit benchmark, 0 otherwise, where Charity Care is defined as [Charity Discounts-Hill Burton (8, 1, 350) + Charity Discounts-Other (8, 1, 355) + Contractual Adjustments-County Indigent Programs-Traditional (8, 1, 330) + Contractual Adjustments-County Indigent Programs-Managed Care (8, 1, 335) + Provision for Bad Debts (8, 1, 300)] / Gross Patient Revenue (8, 1, 30), measured in year *t*
LOW_MEDICARE$^{A(B)}$	1 if the hospital's reported proportion of inpatient Medicare days to total inpatient days is above the annual sample median and reports earnings well above (below) the zero-profit benchmark, 0 otherwise, where the proportion is defined as [Patient Days-Medicare-Traditional-Total (4.1, 1, 35) + Patient Days-Medicare-Managed Care-Total (4.1, 2, 35)] / Total Patient Days (4.1, 11, 35) as measured in year *t*
LOW_MEDICAID$^{A(B)}$	1 if the hospital's reported proportion of inpatient Medicaid days to total inpatient days is above the annual sample median and reports earnings well above (below) the zero-profit benchmark, 0 otherwise, where the proportion is defined as [Patient Days-Medicaid-Traditional-Total (4.1, 1, 35) + Patient Days-Medicaid-Managed Care-Total (4.1, 2, 35)] / Total Patient Days (4.1, 11, 35) as measured in year *t*
HIGH_DONATIONS$^{A(B)}$	1 if Donations Revenue is above the annual sample median and reports earnings well above (below) the zero-profit benchmark, 0 otherwise, where Donations are defined as Unrestricted Contributions (8, 1, 510) / Gross Patient Revenue (8, 1, 30) measured in year *t*
NO_AUDIT$^{A(B)}$	1 if the hospital's OSHPD Annual Financial data report did not undergo an independent financial audit prior to submission (0, 1, 40) and reports earnings well above (below) the zero-profit benchmark, 0 otherwise
LOW_FISCAL_FEES$^{A(B)}$	1 if the hospital's spending on Professional Fees for Total Fiscal Services (18, 4, 200) as a proportion of Gross Patient Revenue (8, 1, 30) is below the annual sample median and reports earnings well above (below) the zero-profit benchmark, 0 otherwise

Appendix D: Chapter 3 Variable Definitions

This table provides definitions of all variables in Chapter 3 and Tables 19 through 31. See Panel 1 for definitions of political connectedness variables, Panel 2 for traditional determinants for corporate cash holdings, and Panel 3 for corporate governance variables. Numbers in parentheses denote Compustat Fundamentals Annual variable names.

Panel 1: Political connectedness variables.

Contributions/NTA	Annual Political Contributions / [Total Assets (AT) – Cash & Cash Equivalents (CHE)] computed at the parent-level and measured in year *t*, where political contributions include donations made by PACs and employees associated with parent and wholly-owned subsidiary companies, unless indicated otherwise
Post-*Citizens United* Dummy	An indicator variable equal to 1 for fiscal years 2010 and 2011, 0 otherwise
Political Dummy	An indicator variable to capture the degree of historical political connectedness as determined by median firm-level pre-*Citizens United* contributions (scaled by Net Total Assets), set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms
Political Deviation	A ranked variable to capture the degree of historical within-firm divergence in political beliefs among historically politically active firms (i.e., where Political Dummy = 1), determined by the median firm-level absolute difference in the proportion of executive employee contributions to Republicans and the proportion of non-executive employee contributions to Republicans as observed during the pre-*Citizens United* period; ranging from 0.10 (bottom decile) to 1.00 (top decile)

Panel 2: Traditional determinants of corporate cash holdings.

Log(Cash/NTA)	The logarithm of Cash & Cash Equivalents (CHE) / [Total Assets (AT) – Cash & Cash Equivalents (CHE)], measured in year *t*
Log(Net Total Assets)	The logarithm of [Total Assets (AT) – Cash & Cash Equivalents (CHE)], measured in year *t*
Debt/NTA	[Long-term Debt (DLTT) + Debt in Current Liabilities (DLC)] / [Total Assets (AT) – Cash & Cash Equivalents (CHE)], measured in year *t*
Market-to-Book	[Market Value of Equity (CSHO × PRCC_F) + Total Liabilities (LT)] / [Total Assets (AT) – Cash & Cash Equivalents (CHE)], measured in year *t*
Cash Flow/NTA	[Operating Income before Depreciation (OIBDP) – Interest Expense (DP) – Taxes (TXT) – Common Dividends (DVC)] / [Total Assets (AT) – Cash & Cash Equivalents (CHE)], measured in year *t*
Std(Cash Flow/NTA)	The trailing volatility of cash flow (OIBDP), computed over a minimum (maximum) of 5 (10) years ending in year *t–1*
Net Working Capital/NTA	[Working Capital (WCAP) – Cash & Cash Equivalents (CHE)] / [Total Assets (AT) – Cash & Cash Equivalents (CHE)], measured in year *t*

Appendix D (Continued)

R&D/Sales	Research and Development Expenditures (XRD) / Sales (SALE), measured in year *t* (and set to 0 if missing)
R&D Missing Dummy	An indicator variable equal to 1 if the firm does not report Research & Development Expenditures (XRD) in year *t*, 0 otherwise
Capital Expenditures/NTA	Capital Expenditures (CAPX) / [Total Assets (AT) – Cash & Cash Equivalents (CHE)], measured in year *t*
Dividend Dummy	An indicator variable equal to 1 if the firm paid common dividends (DVC) in year *t*, 0 otherwise

Panel 3: Corporate governance variables.

Busy Directors	The proportion of independent directors who serve on three or more boards in year *t*
CEO Compensation Dummy	An indicator variable equal to 1 when a CEO's excess compensation in year *t* (defined as the residual from a regression of the logarithm of total CEO compensation on lagged Log(Total Assets), the lagged Market-to-Book ratio, industry effects based on two-digit SIC code classifications, and year fixed effects (Cheng, Hong, and Scheinkman 2010), where total compensation is set equal to TDC1 in the Execucomp database) is positive; 0 otherwise
E-Index	An index of managerial entrenchment, ranging from 0 to 6, where higher values indicate a greater degree of managerial entrenchment (Bebchuk et al. 2009)
Shareholder Inactivism Dummy	An indicator variable equal to 1 when a firm does *not* entertain a shareholder proposal pertaining to political contributions in year *t*, 0 otherwise

Table 1. Descriptive statistics, Chapter 1.

Panel A: Compensation variables.

Variable	N	Mean	Std. Dev.	Lower Quartile	Median	Upper Quartile
CEO Compensation	10,186	327,212	428,679	135,620	222,326	367,746
Relative Pay	10,186	11.356	10.591	4.644	8.062	14.425
Log(Relative Pay)	10,186	2.086	0.856	1.536	2.087	2.669

Panel B: Organizational performance variables.

Program Spending Ratio	10,186	0.843	0.114	0.795	0.867	0.919
Program Expenses to Assets	10,186	0.401	0.499	0.119	0.257	0.475
Fundraising Ratio	9,315	0.904	0.178	0.886	0.963	1.000
Log(Revenue per Employee)	9,711	10.704	1.473	10.067	10.803	11.541

Panel C: Determinants of relative pay and organizational performance.

Governance Index	10,186	0.363	0.096	0.295	0.356	0.443
Board Size	10,186	21	13	12	17	26
No. of Employees	10,186	879	1,542	62	308	1,017
Total Assets	10,186	198,405,661	821,149,254	15,476,609	59,423,104	145,761,043
Total Revenue	10,186	92,971,905	254,105,250	7,329,995	23,448,859	77,139,389
Age	10,186	60	43	28	46	86
Liquid Assets/Total Expenses	10,186	0.814	1.132	0.270	0.468	0.875
Government Revenue	10,186	0.105	0.231	0.000	0.001	0.050
Donations Growth	10,168	–0.001	0.196	-0.018	0.000	0.012
Asset Tangibility	10,186	0.363	0.263	0.105	0.375	0.560
Household Income	10,186	52,828	7,073	47,847	51,351	57,934
CEO Tenure	10,186	8.770	3.847	6.000	8.000	12.000
Gender Dummy	10,134	0.274	0.446	0.000	0.000	1.000
Commercial Dummy	10,186	0.367	0.482	0.000	0.000	1.000
Fundraising Dummy	10,185	0.358	0.479	0.000	0.000	1.000
Total Liabilities/Total Assets	10,186	0.397	0.370	0.127	0.333	0.578

Panel D: Number of organizations by NTEE major industry classification.

	Arts	Education	Health	Human Services	Other
No. of Observations	639	1,945	3,186	2,579	1,837

Descriptive statistics are computed for a maximum of 10,186 nonprofit-year observations spanning tax years 2008-2010. Variable definitions are provided in Appendix A.

Table 2. Correlations between the CEO-to-employee relative pay ratio, organizational performance, governance variables, and other determinants.

		2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.
1.	*Log(Relative Pay)*	−0.03	−0.08	−0.09	−0.08	−0.02	0.26	0.68	0.42	0.35	−0.17	−0.16	0.01	0.23	−0.02	0.02	−0.17	0.23	−0.05	0.12
2.	*Program Spending Ratio*		0.28	0.13	0.07	0.05	−0.01	0.04	0.06	−0.02	−0.15	0.07	−0.01	−0.05	0.02	0.06	0.02	0.06	0.04	0.07
3.	*Program Expenses to Assets*			0.05	−0.10	0.04	−0.13	0.11	−0.24	−0.10	−0.25	0.24	0.00	−0.09	0.03	0.00	0.02	0.14	0.18	0.25
4.	*Fundraising Ratio*				0.01	0.03	−0.02	−0.08	−0.01	−0.09	0.02	0.00	0.04	−0.08	−0.05	0.02	0.01	−0.14	0.36	−0.02
5.	*Log(Revenue per Employee)*					0.05	−0.14	−0.06	0.39	−0.05	−0.02	−0.52	−0.06	−0.17	0.01	0.00	−0.10	0.36	0.25	0.11
6.	*Governance Index*						0.02	0.01	0.06	−0.07	0.00	0.00	0.00	−0.10	0.08	0.04	0.03	0.00	−0.01	0.04
7.	*Board Size*							0.17	0.21	0.33	0.02	−0.03	0.00	−0.04	−0.04	−0.04	−0.03	−0.26	−0.36	−0.14
8.	*Log(No. of Employees)*								0.53	0.42	−0.33	−0.08	−0.01	0.32	0.00	−0.01	−0.20	0.44	0.05	0.24
9.	*Log(Total Assets)*									0.28	−0.04	−0.28	0.00	−0.09	0.00	−0.02	−0.21	0.16	0.00	0.08
10.	*Log(Age)*										−0.16	−0.10	0.00	0.11	0.02	−0.01	−0.09	0.03	−0.22	−0.10
11.	*Liquid Assets/Total Expenses*											0.03	−0.02	−0.18	0.02	0.01	0.06	−0.21	−0.06	−0.16
12.	*Government Revenue*												−0.03	0.02	0.00	0.02	0.09	−0.21	0.01	0.01
13.	*Donations Growth*													−0.03	0.02	0.02	0.00	−0.04	0.00	−0.01
14.	*Asset Tangibility*														−0.04	0.00	−0.04	0.25	0.03	0.24
15.	*Log(Household Income)*															0.06	0.03	0.02	−0.04	0.05
16.	*Log(CEO Tenure)*																0.01	0.05	0.04	0.00
17.	*Gender Dummy*																	−0.12	−0.08	−0.09
18.	*Commercial Dummy*																		0.43	0.38
19.	*Fundraising Dummy*																			0.27
20.	*Total Liabilities/Total Assets*																			

See Appendix A for variable definitions.

Table 3. Determinants of the log of the CEO-to-employee relative pay ratio at nonprofits – Instrumental variables estimations.

Performance measure:	Program Spending Ratio	Program Expenses to Assets	Fundraising Ratio	Log(Revenue per Employee)
	(I)	(II)	(III)	(IV)
Organizational Performance $_{it}$	–1.9280***	–0.3674***	–2.7893	–0.3405***
(endogenous)	(0.000)	(0.000)	(0.112)	(0.000)
Governance Index $_{it}$	–0.1383**	–0.1482**	–0.1351	–0.0744
	(0.046)	(0.025)	(0.128)	(0.300)
Board Size $_{it}$ (÷ 10)	0.1202***	0.1046***	0.0560	0.0947***
	(0.000)	(0.000)	(0.195)	(0.000)
Board Size Squared $_{it}$ (÷ 10^3)	–0.1008***	–0.0909***	–0.0346	–0.0898***
	(0.000)	(0.000)	(0.526)	(0.000)
Log(No. of Employees)$_{it}$	0.3138***	0.3500***	0.3183***	0.1402***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Total Assets)$_{it}$	0.0368***	–0.0232**	0.0330***	0.1958***
	(0.000)	(0.025)	(0.000)	(0.000)
Log(Age)$_{it–1}$ (÷ 10^2)	–1.8924*	–3.1272***	–3.3993	–0.1154
	(0.059)	(0.002)	(0.138)	(0.909)
Liquid Assets/Total Expenses $_{it–1}$ (÷ 10^2)	–1.6365	–1.4492*	–0.9874	–3.3432***
	(0.121)	(0.072)	(0.439)	(0.001)
Government Revenue $_{it–1}$	–0.2856***	–0.2199***	–0.4424***	–1.0463***
	(0.000)	(0.000)	(0.000)	(0.000)
Donations Growth $_{it–1}$ (÷ 10^2)	2.4153	1.2829		–7.0284*
	(0.509)	(0.712)		(0.082)
Asset Tangibility $_{it–1}$	0.0347	–0.1010**	0.0798**	0.0088
	(0.224)	(0.012)	(0.026)	(0.765)
Log(Household Income) $_{it–1}$	–0.0982*	–0.1144**	–0.3379**	–0.1371***
	(0.051)	(0.015)	(0.016)	(0.006)
Log(CEO Tenure)$_{it}$	0.0993***	0.0715***	0.0813***	0.0816***
	(0.000)	(0.000)	(0.000)	(0.000)
Gender Dummy $_{it}$	–0.0419***	–0.0588***	–0.0509***	–0.0738***
	(0.006)	(0.000)	(0.005)	(0.000)
Commercial Dummy $_{it}$	–0.0286	–0.0291	–0.2914**	0.2714***
	(0.169)	(0.108)	(0.029)	(0.000)
Intercept	2.0860***	1.7501***	5.8610*	2.5685***
	(0.001)	(0.001)	(0.065)	(0.000)
N	9,465	9,465	8,948	9,046
Adj. R^2	0.507	0.528	0.415	0.498
Industry Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes

The results represent pooled regressions of *Log(Relative Pay)* on its traditional determinants, governance quality, and a measure of organizational performance: *Program Spending Ratio* in column I, *Program Expenses to Assets* in column II, *Fundraising Ratio* in column III, and *Log(Revenue per Employee)* in column IV. The regressions utilize an instrumental variables estimation in which nonprofit *i*'s performance is instrumented with the mean performance of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. *Donations Growth* is excluded from column III to avoid a potentially highly collinear relationship between the *Fundraising Ratio* and *Donations Growth*. See Appendix A for variable definitions including a detailed explanation of how the instrument is constructed. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Table 4. Determinants of nonprofit performance – Instrumental variables estimations.

Performance measure:	Program Spending Ratio	Program Expenses to Assets	Fundraising Ratio	Log(Revenue per Employee)
	(I)	(II)	(III)	(IV)
Log(Relative Pay)$_{it}$	–0.2246***	–2.1722***	–0.2041***	–3.7095***
(endogenous)	(0.000)	(0.000)	(0.008)	(0.000)
Governance Index $_{it}$	–0.0225	–0.3928**	–0.0376	–0.4428
	(0.277)	(0.014)	(0.184)	(0.109)
Board Size $_{it}$ (÷ 10^2)	0.2904***	2.4496***	0.0264	4.1576***
	(0.000)	(0.000)	(0.797)	(0.000)
Board Size Squared $_{it}$ (÷ 10^4)	–0.2191***	–2.1471***	0.0517	–3.8862***
	(0.006)	(0.000)	(0.634)	(0.000)
Log(No. of Employees)$_{it}$ (÷ 10)	0.7075***	7.7377***	0.6518***	6.5710***
	(0.000)	(0.000)	(0.007)	(0.000)
Log(Total Assets)$_{it}$ (÷ 10)	0.1100***	–0.8067***	0.0824***	5.9726***
	(0.000)	(0.000)	(0.003)	(0.000)
Log(Age)$_{it–1}$ (÷ 10)	–0.0445	–0.4273**	–0.0995***	0.1913
	(0.102)	(0.043)	(0.003)	(0.607)
Liquid Assets/Total Expenses $_{it–1}$ (÷ 10)	–0.1102***	–0.3603**	–0.0392	–0.7825***
	(0.000)	(0.016)	(0.107)	(0.003)
Government Revenue $_{it–1}$	–0.0389**	–0.4348***	–0.0997***	–3.3960***
	(0.038)	(0.003)	(0.002)	(0.000)
Donations Growth $_{it–1}$ (÷ 10)	0.0772	0.0529		–2.4413*
	(0.438)	(0.945)		(0.082)
Asset Tangibility $_{it–1}$	–0.0050	–0.3619***	0.0135	0.1026
	(0.535)	(0.000)	(0.280)	(0.353)
Fundraising Dummy $_{it–1}$ (÷ 10)	0.0802*	0.8814**		1.8465***
	(0.078)	(0.012)		(0.003)
Total Liabilities/Total Assets $_{it–1}$ (÷ 10)	0.1360**	3.8156***	0.2331***	1.2296
	(0.047)	(0.000)	(0.009)	(0.185)
Commercial Dummy $_{it}$	0.0036	–0.1162***	–0.0955***	0.7008***
	(0.529)	(0.009)	(0.000)	(0.000)
Intercept	0.6990***	1.6859***	0.8898***	2.9565***
	(0.000)	(0.000)	(0.000)	(0.000)
N	9,409	9,409	9,005	8,993
Adj. R^2	0.038	0.054	0.044	0.213
Industry Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes

The results represent pooled regressions of *Organizational Performance* on its traditional determinants, governance quality, and (the log of) the CEO-to-employee relative pay ratio. Performance definitions considered include the *Program Spending Ratio* in column I, *Program Expenses to Assets* in column II, *Fundraising Ratio* in column III, and *Log(Revenue per Employee)* in column IV. The regressions utilize an instrumental variables estimation in which nonprofit *i*'s relative pay ratio is instrumented with the mean value of the relative pay ratio of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. *Donations Growth* and the *Fundraising Dummy* are excluded from the *Fundraising Ratio* regression (column III) to avoid a potentially spurious relationship between the dependent and independent variables. See Appendix A for variable definitions including a detailed explanation of how the instrument is constructed. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Table 5. The relation between changes in the log of the CEO-to-employee relative pay ratio and changes in charitable spending for varying degrees of governance quality.

	Dependent variable: Δ *Log(Relative Pay)*$_{it}$	
	(I)	(II)
Δ *Program Spending Ratio* $_{it}$	–0.0008	–0.0138***
	(0.349)	(0.000)
Δ *Governance Index* $_{it}$	0.0051	
	(0.182)	
Governance Index $_{it}$ ($\div 10^2$)		0.0555
		(0.304)
Δ *Program Spending Ratio* $_{it}$ $\times$ *Governance Index* $_{it}$		0.0349***
		(0.000)
Δ *Board Size* $_{it}$ ($\div 10^2$)	–0.0228	–0.0216
	(0.286)	(0.314)
Δ *Board Size Squared* $_{it}$ ($\div 10^4$)	0.1729*	0.1674
	(0.097)	(0.108)
Δ *Log(No. of Employees)*$_{it}$	0.8949***	0.8949***
	(0.000)	(0.000)
Δ *Log(Total Assets)*$_{it}$ ($\div 10$)	–0.0615**	–0.0642**
	(0.032)	(0.025)
Δ *Log(Age)*$_{it}$	–0.0069***	–0.0069***
	(0.000)	(0.000)
Δ *Liquid Assets/Total Expenses* $_{it}$ ($\div 10^3$)	–0.0045	–0.0041
	(0.506)	(0.549)
Δ *Government Revenue* $_{it}$	–0.0015**	–0.0016**
	(0.016)	(0.013)
Δ *Donations Growth* $_{it}$ ($\div 10^2$)	0.0078	0.0090
	(0.603)	(0.548)
Δ *Asset Tangibility* $_{it}$	–0.0010	–0.0010
	(0.108)	(0.102)
Δ *Log(Household Income)* $_{it}$	0.0005	0.0005
	(0.710)	(0.731)
Δ *Log(CEO Tenure)*$_{it}$	0.0025***	0.0025***
	(0.000)	(0.000)
Δ *Commercial Dummy* $_{it}$ ($\div 10^2$)	–0.0180	–0.0187
	(0.384)	(0.367)
CEO Turnover Dummy $_{it}$	–0.0024***	–0.0024***
	(0.000)	(0.000)
Intercept	–0.0001	–0.0003
	(0.405)	(0.199)
N	6,737	6,737
R^2	0.217	0.219
Industry Fixed Effects?	No	No
Year Fixed Effects?	Yes	Yes

The results represent pooled regressions where the change in *Log(Relative Pay)* is modeled as a function of changes in its traditional determinants, governance quality, and the *Program Spending Ratio*, where changes are computed as the difference between year *t* and year *t–1* values. Column II includes the contemporaneous value of the *Governance Index* to account for the impact of governance quality on the relation between changes in the relative pay ratio and in charitable spending. To offset the loss of observations accompanying a changes specification, CEO turnover years are included and their effect captured by a *CEO Turnover Dummy* indicator variable. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. See Appendix A for variable definitions. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Table 6. Executive perquisites, organizational performance, and governance quality.

	Logit Model *Perquisites Indicator*$_{it}$		Logit Model *First-class Flight Indicator*$_{it}$		Ordered Probit Model *Number of Perquisites*$_{it}$	
	(I)	(II)	(III)	(IV)	(V)	(VI)
High Program Spending Dummy $_{it}$	–0.0628	–12.3296***	–0.2595**	–3.9097***	–0.0404	–4.4609***
	(0.373)	(0.000)	(0.013)	(0.000)	(0.175)	(0.000)
Governance Index $_{it}$	–20.5963***	–29.8282***	–8.4361***	–11.3089***	–9.0526***	–12.3788***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
High Program Spending Dummy $_{it}$		4.4778***		1.3590***		1.6186***
× *Governance Index* $_{it}$		(0.000)		(0.000)		(0.000)
Board Size $_{it}$	0.0472***	0.0390***	–0.0429***	–0.0441***	0.0158***	0.0139***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Board Size Squared $_{it}$	–0.0005***	–0.0003**	0.0006***	0.0006***	–0.0002***	–0.0001***
	(0.000)	(0.015)	(0.000)	(0.000)	(0.001)	(0.007)
Log(No. of Employees)$_{it}$	0.3287***	0.3540***	0.3050***	0.3157***	0.1739***	0.1939***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Total Assets)$_{it}$	0.6554***	0.7022***	0.5963***	0.5807***	0.3278***	0.3151***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Age)$_{it–1}$	0.2511***	0.2165***	–0.2588***	–0.2770***	0.0788***	0.0655***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.005)
Liquid Assets/Total Expenses $_{it–1}$	–0.1393***	–0.1365***	–0.1078*	–0.0789	–0.0716***	–0.0626***
	(0.001)	(0.003)	(0.090)	(0.225)	(0.000)	(0.002)
Government Revenue $_{it–1}$	–0.9426***	–0.8648***	–0.6838*	–0.6588*	–0.5799***	–0.5637***
	(0.000)	(0.001)	(0.054)	(0.066)	(0.000)	(0.000)
Donations Growth $_{it–1}$	0.0713	0.2751	–0.0670	–0.0786	0.0250	0.0707
	(0.725)	(0.219)	(0.840)	(0.813)	(0.788)	(0.462)
Asset Tangibility $_{it–1}$	–0.9624***	–1.0459***	–1.2675***	–1.2354***	–0.4363***	–0.4372***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Household Income) $_{it–1}$	0.1222	0.3564	–1.1179***	–1.0496**	–0.0757	–0.0292
	(0.660)	(0.257)	(0.007)	(0.011)	(0.514)	(0.807)
Log(CEO Tenure)$_{it}$	–0.1616**	–0.1663**	0.0156	0.0222	–0.0756***	–0.0718**
	(0.019)	(0.033)	(0.875)	(0.825)	(0.008)	(0.014)
Gender Dummy $_{it}$	–0.4267***	–0.4740***	–0.0403	–0.0562	–0.2185***	–0.2311***
	(0.000)	(0.000)	(0.777)	(0.694)	(0.000)	(0.000)
Commercial Dummy $_{it}$	–0.1934*	–0.1474	–0.3620**	–0.3510**	–0.1226***	–0.1135***
	(0.051)	(0.185)	(0.010)	(0.013)	(0.002)	(0.006)
Intercept	–10.2940***	–9.4701***	2.6630	3.3176		
	(0.001)	(0.008)	(0.565)	(0.476)		
N	8,005	8,005	7,975	7,975	8,005	8,005
Pseudo R^2	0.509	0.605	0.251	0.264	0.299	0.342

Table 6 (Continued)

Columns I and II model the probability of at least one perquisite being awarded to an executive; columns III and IV model the probability of first-class or charter travel being awarded to an executive; and columns V and VI model the probability of the variety of executive perquisite types. Columns I, II, III, and IV represent multivariate logit models, while columns V and VI are multivariate ordered probit models. Each specification is modeled as a function of traditional determinants, governance quality, and an indicator variable for above industry median charitable spending (*High Program Spending Dummy*). Of the 8,005 observations where disclosure of executive perquisites was required, 3,363 reported providing at least one type of perquisite and 498 provided first-class or charter travel. Of those nonprofits that provided at least one perquisite, the mean (median) number of types provided was 1.93 (2.00). See Appendix A for variable definitions. All models include industry and year fixed effects, where industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test. For specification 5, cutpoints are 3.7, 4.7, 5.4, 6.0, 6.6, 7.2, and 8.2. For specification 6, cutpoints are 2.6, 3.8, 4.5, 5.1, 5.6, 6.2, and 7.3. All are statistically significant at the 0.10 level or better.

Table 7. Determinants of the log of the CEO-to-employee relative pay ratio – Quantile regressions at the 80th percentile.

Performance measure:	Program Spending Ratio	Program Expenses to Assets	Fundraising Ratio	Log(Revenue per Employee)
	(I)	(II)	(III)	(IV)
Organizational Performance $_{it}$	–2.1223***	–0.2976***	–3.8554**	–0.3062***
	(0.000)	(0.001)	(0.037)	(0.000)
Governance Index $_{it}$	–0.4329***	–0.4684***	–0.4223***	–0.4319***
	(0.000)	(0.000)	(0.000)	(0.000)
Board Size $_{it}$ (÷ 10)	0.0630***	0.0429**	–0.0136	0.0330**
	(0.000)	(0.043)	(0.766)	(0.039)
Board Size Squared $_{it}$ (÷ 10^3)	–0.0103	0.0087	0.0715	0.0134
	(0.640)	(0.754)	(0.212)	(0.518)
Log(No. of Employees)$_{it}$	0.3013***	0.3280***	0.3118***	0.1455***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Total Assets)$_{it}$	0.0529***	0.0019	0.0486***	0.1932***
	(0.000)	(0.892)	(0.000)	(0.000)
Log(Age)$_{it–1}$ (÷ 10^2)	–6.8165***	–7.4413***	–9.2777***	–5.0326***
	(0.000)	(0.000)	(0.000)	(0.000)
Liquid Assets/Total Expenses $_{it–1}$ (÷ 10^2)	–1.1298	–0.2448	–1.3504	–2.4810**
	(0.311)	(0.827)	(0.323)	(0.010)
Government Revenue $_{it–1}$	–0.3326***	–0.3035***	–0.5248***	–1.0465***
	(0.000)	(0.000)	(0.000)	(0.000)
Donations Growth $_{it–1}$ (÷ 10^2)	5.6230	2.8085		–5.8620
	(0.151)	(0.565)		(0.136)
Asset Tangibility $_{it–1}$	–0.0085	–0.1173**	0.0461	–0.0408
	(0.786)	(0.034)	(0.248)	(0.157)
Log(Household Income) $_{it–1}$	–0.0592	–0.0832	–0.3351**	–0.1070**
	(0.267)	(0.200)	(0.029)	(0.024)
Log(CEO Tenure)$_{it}$	0.1163***	0.0881***	0.0901***	0.0938***
	(0.000)	(0.000)	(0.000)	(0.000)
Gender Dummy $_{it}$	–0.0865***	–0.1049***	–0.0872***	–0.1165***
	(0.000)	(0.000)	(0.000)	(0.000)
Commercial Dummy $_{it}$	–0.0169	–0.0325	–0.3778***	0.2407***
	(0.451)	(0.204)	(0.007)	(0.000)
Intercept	2.4537***	1.6523**	7.9173**	2.9818***
	(0.000)	(0.026)	(0.022)	(0.000)
N	9,465	9,465	8,948	9,046
Pseudo R^2	0.348	0.348	0.357	0.344
Industry Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes

The results represent quantile regressions of *Log(Relative Pay)* on its traditional determinants, governance quality, and a measure of organizational performance: *Program Spending Ratio* in column I, *Program Expenses to Assets* in column II, *Fundraising Ratio* in column III, and *Log(Revenue per Employee)* in column IV. Each specification is estimated at the 80th percentile of the distribution of the CEO-to-employee relative pay ratio. All regressions utilize an instrumental variables estimation in which nonprofit *i*'s endogenous variable is instrumented with the mean analogous variable of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. *Donations Growth* is excluded from column III to avoid a potentially highly collinear relationship between the *Fundraising Ratio* and *Donations Growth*. See Appendix A for variable definitions. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Table 8. Determinants of nonprofit performance – Quantile regressions at the 20th percentile.

Performance measure:	Program Spending Ratio	Program Expenses to Assets	Fundraising Ratio	Log(Revenue per Employee)
	(I)	(II)	(III)	(IV)
Log(Relative Pay) $_{it}$	–0.2769***	–0.6943***	–0.2655***	–3.1873***
	(0.000)	(0.000)	(0.000)	(0.000)
Governance Index $_{it}$	–0.0247	–0.0716***	–0.0461**	–0.4541***
	(0.185)	(0.000)	(0.036)	(0.000)
Board Size $_{it}$ (÷ 10^2)	0.4327***	0.9379***	0.0893	3.1554***
	(0.000)	(0.000)	(0.233)	(0.000)
Board Size Squared $_{it}$ (÷ 10^4)	–0.3562***	–0.8507***	–0.0249	–2.9360***
	(0.000)	(0.000)	(0.769)	(0.000)
Log(No. of Employees)$_{it}$ (÷ 10)	0.9201***	2.7867***	0.8471***	6.0137***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Total Assets)$_{it}$ (÷ 10)	0.1194***	–0.5097***	0.1125***	5.0121***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Age)$_{it–1}$ (÷ 10)	–0.0194	–0.0528***	–0.1035***	0.0927
	(0.448)	(0.005)	(0.000)	(0.457)
Liquid Assets/Total Expenses $_{it–1}$ (÷ 10)	–0.1724***	–0.1001***	–0.0006	–0.7575***
	(0.000)	(0.000)	(0.978)	(0.000)
Government Revenue $_{it–1}$	–0.0431**	–0.1992***	–0.1065***	–3.7103***
	(0.013)	(0.000)	(0.000)	(0.000)
Donations Growth $_{it–1}$ (÷ 10)	–0.0179	0.0071		–3.9409***
	(0.855)	(0.919)		(0.000)
Asset Tangibility $_{it–1}$	0.0114	–0.0070	0.0175*	0.2046***
	(0.113)	(0.216)	(0.051)	(0.000)
Fundraising Dummy $_{it–1}$ (÷ 10)	0.0888**	0.1821***		1.2674***
	(0.030)	(0.000)		(0.000)
Total Liabilities/Total Assets $_{it–1}$ (÷ 10)	0.0080	0.6235***	0.2817***	1.3704***
	(0.899)	(0.000)	(0.000)	(0.000)
Commercial Dummy $_{it}$	0.0099*	–0.0120***	–0.1046***	0.6722***
	(0.063)	(0.003)	(0.000)	(0.000)
Intercept	0.4992***	0.7149***	0.7776***	2.7713***
	(0.000)	(0.000)	(0.000)	(0.000)
N	9,409	9,409	9,005	8,993
Pseudo R^2	0.070	0.239	0.080	0.469
Industry Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes

The results represent quantile regressions of *Organizational Performance* on the governance index and traditional determinants of organizational performance. Performance definitions considered include the *Program Spending Ratio* in column I, *Program Expenses to Assets* in column II, *Fundraising Ratio* in column III, and *Log(Revenue per Employee)* in column IV. Each specification is estimated at the 20th percentile of the distribution of the respective measure of performance. All regressions utilize an instrumental variables estimation in which nonprofit *i*'s endogenous variable is instrumented with the mean analogous variable of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. *Donations Growth* and the *Fundraising Dummy* are excluded from the *Fundraising Ratio* regression (column III) to avoid a potentially spurious relationship between the dependent and independent variables. See Appendix A for variable definitions. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Table 9. Determinants of the log of the CEO-to-employee relative pay ratio – Quantile regressions at the 80th percentile, modeled as a function of governance sub-indices.

Performance measure:	Program Spending Ratio	Program Expenses to Assets	Fundraising Ratio	Log(Revenue per Employee)
	(I)	(II)	(III)	(IV)
Organizational Performance $_{it}$	–0.1387*	–0.1362***	–0.1481***	–0.0979***
	(0.061)	(0.000)	(0.001)	(0.000)
Governing Board Index $_{it}$	–0.3212***	–0.3574***	–0.3965***	–0.3644***
	(0.010)	(0.004)	(0.004)	(0.001)
Governing Policies Index $_{it}$	–0.1284	–0.1246	–0.0710	0.0078
	(0.201)	(0.206)	(0.521)	(0.932)
Compensation Policies Index $_{it}$	–0.0646*	–0.0527	–0.0815**	–0.0504
	(0.074)	(0.138)	(0.037)	(0.127)
Accountability & Transparency Index $_{it}$	–0.1461**	–0.1766**	–0.1395*	–0.1362**
	(0.040)	(0.012)	(0.070)	(0.035)
Board Size $_{it}$ (÷ 10)	0.0648***	0.0519***	0.0590***	0.0463***
	(0.001)	(0.005)	(0.003)	(0.007)
Board Size Squared $_{it}$ (÷ 10^3)	–0.0198	–0.0047	–0.0157	–0.0044
	(0.424)	(0.848)	(0.547)	(0.845)
Log(No. of Employees)$_{it}$	0.2940***	0.3044***	0.2972***	0.2537***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Total Assets)$_{it}$	0.0433***	0.0255***	0.0393***	0.0846***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Age)$_{it–1}$ (÷ 10^2)	–5.6633***	–5.8516***	–4.2217***	–4.7326***
	(0.000)	(0.000)	(0.001)	(0.000)
Liquid Assets/Total Expenses $_{it–1}$ (÷ 10^2)	1.7298**	0.9448	1.0278	1.2375
	(0.040)	(0.258)	(0.260)	(0.107)
Government Revenue $_{it–1}$	–0.4304***	–0.3967***	–0.4707***	–0.6135***
	(0.000)	(0.000)	(0.000)	(0.000)
Donations Growth $_{it–1}$ (÷ 10^2)	2.6875	2.4415		–1.3719
	(0.541)	(0.572)		(0.739)
Asset Tangibility $_{it–1}$	0.0309	–0.0285	0.0778**	0.0118
	(0.354)	(0.405)	(0.034)	(0.696)
Log(Household Income) $_{it–1}$	–0.0868	–0.0842	–0.0773	–0.1146**
	(0.137)	(0.142)	(0.222)	(0.031)
Log(CEO Tenure)$_{it}$	0.0933***	0.0893***	0.0819***	0.0752***
	(0.000)	(0.000)	(0.000)	(0.000)
Gender Dummy $_{it}$	–0.1060***	–0.1106***	–0.0847***	–0.1107***
	(0.000)	(0.000)	(0.000)	(0.000)
Commercial Dummy $_{it}$	–0.0542***	–0.0442**	–0.0937***	0.0165
	(0.010)	(0.033)	(0.000)	(0.428)
Intercept	1.3476**	1.5613**	1.3274*	2.0899***
	(0.043)	(0.017)	(0.066)	(0.001)
N	9,465	9,465	8,948	9,046
Pseudo R^2	0.347	0.350	0.357	0.348
Industry Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes

The results represent quantile regressions of *Log(Relative Pay)* on its traditional determinants, sub-indices of the governance index, and a measure of organizational performance: *Program Spending Ratio* in column I, *Program Expenses to Assets* in column II, *Fundraising Ratio* in column III, and *Log(Revenue per Employee)* in column IV. Each specification is estimated at the 80th percentile of the distribution of the CEO-to-employee relative pay ratio. All regressions utilize an instrumental variables estimation in which nonprofit *i*'s endogenous variable is instrumented with the mean analogous variable of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. *Donations Growth* is excluded from column III to avoid a potentially highly collinear relationship between the *Fundraising Ratio* and *Donations Growth*. See Appendix A for variable definitions. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Table 10. Determinants of nonprofit performance – Quantile regressions at the 20th percentile, modeled as a function of governance sub-indices.

Performance measure:	Program Spending Ratio	Program Expenses to Assets	Fundraising Ratio	Log(Revenue per Employee)
	(I)	(II)	(III)	(IV)
Log(Relative Pay) $_{it}$	–0.2588***	–0.6646***	–0.2540***	–3.0941***
	(0.000)	(0.000)	(0.000)	(0.000)
Governing Board Index $_{it}$	–0.0617**	–0.0843***	–0.0608*	–0.5090***
	(0.035)	(0.001)	(0.070)	(0.000)
Governing Policies Index $_{it}$	0.0230	0.0203	–0.0075	0.2518**
	(0.303)	(0.295)	(0.772)	(0.015)
Compensation Policies Index $_{it}$	0.0136	0.0378***	0.0132	0.1643***
	(0.102)	(0.000)	(0.161)	(0.000)
Accountability & Transparency Index $_{it}$	0.0066	–0.0926***	–0.0326*	–0.7219***
	(0.694)	(0.000)	(0.083)	(0.000)
Board Size $_{it}$ (÷ 10^2)	0.4212***	0.8920***	0.0738	3.0151***
	(0.000)	(0.000)	(0.345)	(0.000)
Board Size Squared $_{it}$ (÷ 10^4)	–0.3488***	–0.8136***	–0.0140	–2.8607***
	(0.000)	(0.000)	(0.875)	(0.000)
Log(No. of Employees)$_{it}$ (÷ 10)	0.8620***	2.6703***	0.7989***	5.5862***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Total Assets)$_{it}$ (÷ 10)	0.1093***	–0.5274***	0.1052***	4.8783***
	(0.000)	(0.000)	(0.000)	(0.000)
Log(Age)$_{it–1}$ (÷ 10)	–0.0132	–0.0401*	–0.0981***	0.0883
	(0.631)	(0.082)	(0.001)	(0.486)
Liquid Assets/Total Expenses $_{it–1}$ (÷ 10)	–0.1704***	–0.1015***	–0.0012	–0.6454***
	(0.000)	(0.000)	(0.958)	(0.000)
Government Revenue $_{it–1}$	–0.0357*	–0.1979***	–0.1058***	–3.7618***
	(0.061)	(0.000)	(0.000)	(0.000)
Donations Growth $_{it–1}$ (÷ 10)	–0.0237	–0.0053		–3.6769***
	(0.824)	(0.950)		(0.000)
Asset Tangibility $_{it–1}$	0.0111	–0.0094	0.0177*	0.2294***
	(0.156)	(0.192)	(0.064)	(0.000)
Fundraising Dummy $_{it–1}$ (÷ 10)	0.0816*	0.1934***		1.2966***
	(0.064)	(0.000)		(0.000)
Total Liabilities/Total Assets $_{it–1}$ (÷ 10)	0.0113	0.5899***	0.2858***	0.9948***
	(0.868)	(0.000)	(0.000)	(0.002)
Commercial Dummy $_{it}$	0.0106*	–0.0110**	–0.1044***	0.6791***
	(0.063)	(0.027)	(0.000)	(0.000)
Intercept	0.6469***	1.0686***	0.8900***	5.5138***
	(0.000)	(0.000)	(0.000)	(0.000)
N	9,409	9,409	9,005	8,993
Pseudo R^2	0.070	0.239	0.080	0.471
Industry Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes

The results represent quantile regressions of *Organizational Performance* on sub-indices of the governance index and traditional determinants of organizational performance. Performance definitions considered include the *Program Spending Ratio* in column I, *Program Expenses to Assets* in column II, *Fundraising Ratio* in column III, and *Log(Revenue per Employee)* in column IV. Each specification is estimated at the 20th percentile of the distribution of the respective measure of performance. All regressions utilize an instrumental variables estimation in which nonprofit *i*'s endogenous variable is instrumented with the mean analogous variable of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. *Donations Growth* and the *Fundraising Dummy* are excluded from the *Fundraising Ratio* regression (column III) to avoid a potentially spurious relationship between the dependent and independent variables. See Appendix A for variable definitions. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Table 11. The relation between CEO-to-employee relative pay and nonprofit performance – Commercial versus traditional nonprofits.

	Dependent variable: *Log(Relative Pay)*$_{it}$		Dependent variable: *Program Spending Ratio* $_{it}$	
	Commercial vs. Traditional		Commercial vs. Traditional	
	(I)	(II)	(III)	(IV)
Program Spending Ratio $_{it}$	–3.2048***	–0.9116		
(endogenous)	(0.001)	(0.164)		
Log(Relative Pay)$_{it}$			–0.1102***	–0.4835***
(endogenous)			(0.000)	(0.007)
Governance Index $_{it}$	0.1082	–0.2559***	0.0288	–0.1217*
	(0.316)	(0.006)	(0.131)	(0.078)
Board Size $_{it}$ (÷ 10)	0.1003***	0.0984***	0.6649	5.9584***
	(0.001)	(0.000)	(0.292)	(0.006)
Board Size Squared $_{it}$ (÷ 10^3)	–0.0615	–0.0750***	0.4239	–4.5853**
	(0.201)	(0.004)	(0.627)	(0.024)
Log(No. of Employees)$_{it}$	0.3430***	0.3065***	0.4163***	1.4753***
	(0.000)	(0.000)	(0.000)	(0.008)
Log(Total Assets)$_{it}$	0.0553***	0.0116	0.0657***	0.0948**
	(0.000)	(0.199)	(0.002)	(0.012)
Log(Age)$_{it–1}$ (÷ 10^2)	–0.3904	–1.9280	0.0263	–0.1109
	(0.791)	(0.181)	(0.340)	(0.116)
Liquid Assets/Total Expenses $_{it–1}$ (÷ 10^2)	1.2020	–2.1033	–0.0403	–0.1941***
	(0.485)	(0.115)	(0.162)	(0.000)
Government Revenue $_{it–1}$	–0.1890***	–0.3993***	–0.0065	–0.1640*
	(0.003)	(0.000)	(0.614)	(0.057)
Donations Growth $_{it–1}$ (÷ 10^2)	–20.5074	2.7035	–0.0186	0.1258
	(0.195)	(0.447)	(0.950)	(0.490)
Asset Tangibility $_{it–1}$	–0.1207**	0.1374***	–0.0368***	0.0679*
	(0.016)	(0.000)	(0.000)	(0.052)
Log(Household Income) $_{it–1}$	0.0242	–0.1917***		
	(0.761)	(0.005)		
Log(CEO Tenure)$_{it}$	0.1570***	0.0431**		
	(0.000)	(0.014)		
Gender Dummy $_{it}$	–0.0500**	–0.0458**		
	(0.034)	(0.027)		
Fundraising Dummy $_{it–1}$ (÷ 10)			0.0516	0.3374**
			(0.212)	(0.046)
Total Liabilities/Total Assets $_{it–1}$			0.2173***	–0.1467
			(0.001)	(0.493)
Intercept	1.4015	2.6754***	0.7386***	0.7284***
	(0.127)	(0.002)	(0.000)	(0.000)
N	4,943	4,522	4,931	4,478
Adj. R^2	0.429	0.546	0.029	0.016
Industry Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes

Columns I and II present pooled regressions of *Log(Relative Pay)* on its traditional determinants, governance quality, and the *Program Spending Ratio* for sub-samples of commercial and traditional nonprofits, where commercial nonprofits are defined as those with at least 90% of revenues drawn from program services. Columns III and IV present pooled regressions of the *Program Spending Ratio* on its traditional determinants, governance quality, and (the log of) the CEO-to-employee relative pay ratio for sub-samples of commercial and traditional nonprofits. All regressions utilize an instrumental variables estimation in which nonprofit *i*'s endogenous variable is instrumented with the mean analogous variable of similar-sized organizations within the same industry-region-year excluding nonprofit *i*. Scaling of coefficients (where applicable) is denoted in parentheses to the right of variable names. See Appendix A for variable definitions. Industry classifications are based on the 26 NTEE major groupings. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed test.

Figure 1. Income Statement based on OSHPD Hospital Annual Financial data.
Income Statement (worksheet 8, section 1)

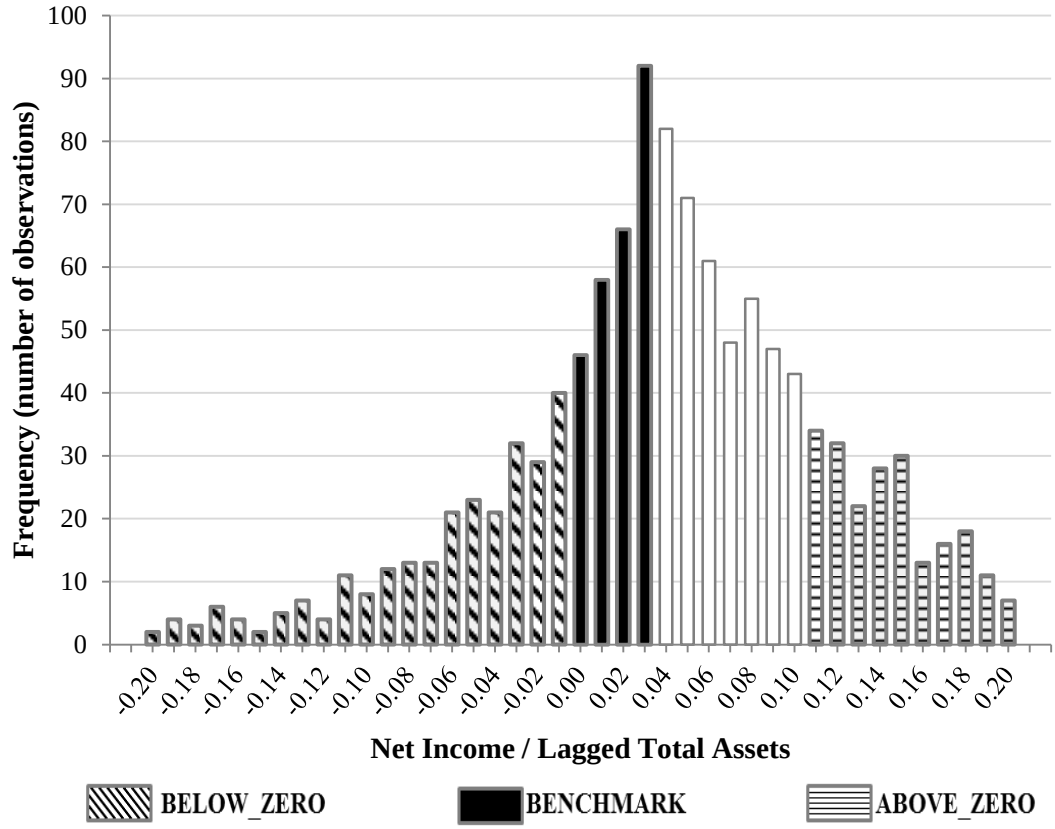
OSHPD Data Item	(line no.)
Operating Revenues:	
Daily Hospital Services	(5)
Ambulatory Services	(10)
Ancillary Services	(15)
Gross Patient Revenue	(30)
Deductions from Revenue	(105)
Capitation Premium Revenue	(107)
Net Patient Revenue	
Total Other Operating Revenue	(135)
Total Operating Revenue	(140)
Operating Expenses:	
Daily Hospital Services*	(146)
Ambulatory Services*	(151)
Ancillary Services*	(156)
Research Costs#	(161)
Education Costs#	(166)
General Services#	(171)
Fiscal Services#	(176)
Administrative Services#	(181)
Unassigned Costs#	(186)
Purchased Inpatient Services	(190)
Purchased Outpatient Services	(195)
Total Operating Expenses	(200)
Net from Operations	(205)
Net Non-Operating Revenue and Expense	(210)
Net Income before Taxes and Extraordinary Items	(215)
Provision for Current Income Taxes	(220)
Provision for Deferred Income Taxes	(225)
Net Income before Extraordinary Items	(230)
Extraordinary Items	(235)
Net Income	(245)

* Core (revenue-generating) expenses
# Non-core (non-revenue-generating) expenses

Figure 2: Frequency Plot of Net Income / Lagged Total Assets

Fiscal years 2002-2010; 1,282 hospital-year observations

Bin widths of 0.01



The horizontal axis has been truncated to [−0.20, 0.20], which encompasses approximately 90% of all observations. BELOW_ZERO hospitals are defined as those with net income$_t$ / total assets$_{t-1}$ less than 0.00. ABOVE_ZERO hospitals have net income$_t$ / total assets$_{t-1}$ greater than 0.10. Hospitals with net income$_t$ / total assets$_{t-1}$ greater than or equal to 0.00 and less than 0.04 are defined as BENCHMARK hospitals. No hypothesis was made on observations falling in the range [0.04, 0.10].

Table 12. Sample selection procedures, Chapter 2.

All church and nonprofit hospital-year observations during FY 2002-2010	1,868
Less: Specialty, psychiatric, and substance abuse hospital-year observations	(195)
Less: Non-comparable report[a] hospital-year observations	(223)
Less: Hospital-year observations with fewer than 50 licensed beds[b]	(146)
Less: Hospital-year observations with insufficient data	(22)
Total hospital-year observations	1,282

[a] Non-comparable reports result in data that are not comparable to that filed by other hospitals. Each fiscal year, the OSHPD identifies a list of non-comparable reports which generally includes, for example, Kaiser hospitals, long-term care emphasis hospitals, Shriner's hospitals, and state hospitals.

[b] The number of licensed beds was identified using OSHPD Annual Financial data files (1, 1, 5).

Table 13. Descriptive statistics, Chapter 2. (N = 1,282)

Variable	Mean	Std Dev	Lower Quartile	Median	Upper Quartile
$CORE_EXP_t$	0.183	0.053	0.147	0.169	0.206
$NONCORE_EXP_t$	0.111	0.045	0.084	0.101	0.123
$CORE_EXP_{t-1}$	0.190	0.055	0.151	0.175	0.217
$NONCORE_EXP_{t-1}$	0.117	0.047	0.088	0.106	0.134
ATO_t	1.599	0.564	1.200	1.543	1.939
ΔREV_t	0.152	0.206	0.081	0.124	0.184
$NEG_\Delta REV_t$	−0.006	0.046	0.000	0.000	0.000
CURRENT	1.990	1.526	1.030	1.621	2.470
DAYS_CASH	29.372	44.755	1.324	12.716	38.512
TEACHING	0.095	0.294	0.000	0.000	0.000
LnAGE	3.779	0.451	3.659	3.900	4.078
LnASSETS	18.783	1.110	18.004	18.786	19.577
CMI	1.161	0.230	1.000	1.130	1.290
HHI	0.233	0.474	0.023	0.075	0.251
U_CORE_EXP	0.000	0.017	−0.007	0.000	0.006
U_NONCORE_EXP	0.000	0.017	−0.005	−0.001	0.005

See Appendix C for variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 14. Estimation of expected core expenses and expected non-core expenses.

	$CORE_EXP_t$		$NONCORE_EXP_t$	
Variable	Coef.	p-value	Coef.	p-value
Intercept	0.014	0.244	0.021*	0.065
$CORE_EXP_{t-1}$	0.899**	<.0001		
$NONCORE_EXP_{t-1}$			0.861**	<.0001
ATO_t	0.003**	0.006	0.000	0.923
ΔREV_t	−0.018**	<.0001	−0.034**	<.0001
$NEG_\Delta REV_t$	−0.069**	<.0001	−0.121**	<.0001
CURRENT	0.001*	0.032	0.000	0.356
DAYS_CASH	0.000	0.105	0.000**	0.001
TEACHING	0.001	0.469	0.001	0.645
LnAGE	−0.001	0.372	−0.001	0.217
LnASSETS	0.000	0.722	0.000	0.643
CMI	0.001	0.712	0.001	0.633
HHI	0.002*	0.061	−0.001	0.296
Adj. R^2	0.907		0.874	
N	1,282		1,282	

The results represent a pooled regression of all 1,282 hospital-year observations in the sample. However, when estimating expected expenses of hospital *i* in year *t*, we estimate a model that includes only hospitals other than hospital *i* in the same fiscal year. Coefficient estimates from these models times actual amounts for hospital *i* in year *t* are used to calculate expected expenses for hospital *i* in year *t*. See Appendix C for variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. *, ** indicates significance at the 0.10, 0.01 level for a two-tailed t-test.

Table 15. Correlations between hospital characteristic partitioning variables.

		2	3	4	5	6	7	8	9	10	11
1.	*U_CORE_EXP*	0.38	0.00	−0.06	−0.04	0.00	0.03	0.01	−0.04	−0.05	−0.01
2.	*U_NONCORE_EXP*		0.00	0.02	−0.02	0.03	0.01	−0.04	−0.02	−0.05	−0.03
3.	*RURAL*			0.04	0.02	0.21	−0.03	−0.06	0.15	0.05	0.02
4.	*CHURCH*				0.28	0.09	−0.01	−0.12	0.00	0.13	0.13
5.	*SYSTEM*					0.16	−0.10	0.01	−0.05	0.18	−0.03
6.	*HIGH_CHARITY*						0.04	−0.19	−0.02	0.04	0.01
7.	*LOW_MEDICARE*							−0.48	0.00	−0.07	0.04
8.	*LOW_MEDICAID*								0.03	0.04	−0.07
9.	*NO_AUDIT*									−0.01	−0.07
10.	*LOW_ FISC_FEES*										0.00
11.	*HIGH_DONATIONS*										

U_CORE_EXP (*U_NONCORE_EXP*) is calculated as actual core (non-core) expenses less expected core (non-core) expenses, where expectations are based on estimation in Table 14. See Appendix C for other variable definitions.

Table 16. The relation between unexpected core expenses and unexpected non-core expenses for suspect versus non-suspect hospitals based on an aggregate measure of cost shifting tendency.

Panel A: Hospitals reporting net income within or well above the zero-profit benchmark.

Variable	Coef.	p-value
Intercept	−0.001	0.179
$SUSPECT^{A}$	0.005*	0.097
U_NONCORE_EXP	0.465**	<.0001
U_NONCORE_EXP × $SUSPECT^{A}$	−1.308**	<.0001
Adj. R^2	0.075	
N	601	

Panel B: Hospitals reporting net income within or below the zero-profit benchmark.

Variable	Coef.	p-value
Intercept	−0.001	0.194
$SUSPECT^{B}$	0.004	0.230
U_NONCORE_EXP	0.500**	<.0001
U_NONCORE_EXP × $SUSPECT^{B}$	−0.172	0.236
Adj. R^2	0.230	
N	648	

The results represent a regression of *U_CORE_EXP* on *U_NONCORE_EXP*, after partitioning on an aggregate measure of cost shifting tendency, $SUSPECT^{A(B)}$. $SUSPECT^{A(B)}$ is the sum of the nine partitioning indicator variables (scaled by 9) when earnings are well above (below) the zero-profit benchmark. *U_CORE_EXP* (*U_NONCORE_EXP*) is calculated as actual core (non-core) expenses less expected core (non-core) expenses, where expectations are based on estimation in Table 14. Hospitals well above (below) [within] the zero-profit benchmark include those reporting bottom-line net income scaled by lagged total assets greater than 0.10 (less than 0.00) [greater than or equal to 0.00 and less than 0.04]. See Figure 2 for benchmark classifications and Appendix C for other variable definitions. *, ** indicates significance at the 0.10, 0.01 level for a two-tailed t-test.

Table 17. The relation between unexpected core expenses and *positive* or *negative* unexpected non-core expenses for suspect versus non-suspect hospitals based on an aggregate measure of cost shifting tendency.

Panel A: Hospitals reporting net income within or well above the zero-profit benchmark.

	Negative *U_NONCORE_EXP*		Positive *U_NONCORE_EXP*		F-Test: $\beta_3^{neg} = \beta_3^{pos}$
Variable	Coef.	p-value	Coef.	p-value	p-value
Intercept	0.002	0.260	−0.001	0.433	
SUSPECT^A	−0.019**	0.001	0.002	0.560	
U_NONCORE_EXP	0.799**	<.0001	0.355**	0.000	
U_NONCORE_EXP × *SUSPECT*^{A}	−3.541**	<.0001	−0.045	0.910	<.0001
Adj. R^2	0.146		0.091		
N	335		266		

Panel B: Hospitals reporting net income within or below the zero-profit benchmark.

	Negative *U_NONCORE_EXP*		Positive *U_NONCORE_EXP*		F-Test: $\beta_3^{neg} = \beta_3^{pos}$
Variable	Coef.	p-value	Coef.	p-value	p-value
Intercept	0.002	0.293	−0.002	0.245	
SUSPECT^B	0.002	0.737	0.004	0.367	
U_NONCORE_EXP	0.768**	<.0001	0.463**	<.0001	
U_NONCORE_EXP × *SUSPECT*^{B}	−0.488	0.149	−0.119	0.547	0.341
Adj. R^2	0.186		0.284		
N	339		309		

The results represent a regression of *U_CORE_EXP* on positive and negative values of *U_NONCORE_EXP*, after partitioning on an aggregate measure of cost shifting tendency, *SUSPECT*$^{A(B)}$. *SUSPECT*$^{A(B)}$ is the sum of the nine partitioning indicator variables (scaled by 9) when earnings are well above (below) the zero-profit benchmark. *U_CORE_EXP* (*U_NONCORE_EXP*) is calculated as actual core (non-core) expenses less expected core (non-core) expenses, where expectations are based on estimation in Table 14. Hospitals well above (below) [within] the zero-profit benchmark include those reporting bottom-line net income scaled by lagged total assets greater than 0.10 (less than 0.00) [greater than or equal to 0.00 and less than 0.04]. See Figure 2 for benchmark classifications and Appendix C for other variable definitions. The final column provides p-values of an F-test of whether the coefficients for the interactions of *U_NONCORE_EXP* and *SUSPECT*$^{A(B)}$ differ for negative versus positive values of *U_NONCORE_EXP*. *, ** indicates significance at the 0.10, 0.01 level for a two-tailed t-test.

Table 18. The relation between unexpected core expenses and *positive* or *negative* unexpected non-core expenses for suspect versus non-suspect hospitals based on individual measures of cost shifting tendency.

Panel A: Hospitals reporting net income within or well above the zero-profit benchmark.

	Negative *U_NONCORE_EXP*		Positive *U_NONCORE_EXP*		F-Test: $\beta_3^{neg} = \beta_3^{pos}$
Variable	Coef.	p-value	Coef.	p-value	p-value
Intercept	0.002	0.128	−0.001	0.391	
RURAL^A	−0.004	0.591	0.008	0.197	
CHURCH^A	0.001	0.797	−0.002	0.603	
SYSTEM^A	0.004	0.307	−0.001	0.713	
HIGH_CHARITY^A	−0.008*	0.022	0.001	0.645	
LOW_MEDICARE^A	0.001	0.732	0.002	0.450	
LOW_MEDICAID^A	−0.004	0.159	0.003	0.396	
NO_AUDIT^A	0.000	0.972	0.002	0.595	
LOW_FISC_FEES^A	0.003	0.390	−0.002	0.462	
HIGH_DONATIONS^A	−0.008*	0.026	−0.001	0.795	
U_NONCORE_EXP	0.876**	<.0001	0.352**	0.000	
U_NONCORE_EXP × *RURAL*^{A}	−1.947**	0.001	−0.761	0.136	0.140
U_NONCORE_EXP × *CHURCH*^{A}	0.803	0.180	−0.121	0.822	0.273
U_NONCORE_EXP × *SYSTEM*^{A}	0.071	0.868	0.374	0.274	0.587
U_NONCORE_EXP × *HIGH_CHARITY*^{A}	−0.774*	0.031	−0.014	0.966	0.132
U_NONCORE_EXP × *LOW_MEDICARE*^{A}	−0.355	0.202	0.049	0.873	0.366
U_NONCORE_EXP × *LOW_MEDICAID*^{A}	−0.674*	0.035	−0.653*	0.041	0.965
U_NONCORE_EXP × *NO_AUDIT*^{A}	0.132	0.657	0.036	0.923	0.856
U_NONCORE_EXP × *LOW_FISC_FEES*^{A}	0.270	0.509	0.167	0.631	0.853
U_NONCORE_EXP × *HIGH_DONATIONS*^{A}	−1.068**	0.007	0.033	0.910	0.028
Adj. R^2	0.335		0.086		
N	335		266		

Panel B: Hospitals reporting net income within or below the zero-profit benchmark.

	Negative *U_NONCORE_EXP*		Positive *U_NONCORE_EXP*		F-Test: $\beta_3^{neg} = \beta_3^{pos}$
Variable	Coef.	p-value	Coef.	p-value	p-value
Intercept	0.003*	0.077	−0.001	0.649	
RURAL^B	0.006	0.496	0.002	0.821	
CHURCH^B	−0.002	0.740	−0.001	0.737	
SYSTEM^B	0.003	0.384	−0.006*	0.100	
HIGH_CHARITY^B	−0.001	0.725	0.012**	0.001	
LOW_MEDICARE^B	−0.005	0.140	−0.005	0.110	
LOW_MEDICAID^B	−0.003	0.500	0.001	0.703	
NO_AUDIT^B	0.007	0.139	−0.005	0.185	
LOW_FISC_FEES^B	−0.001	0.847	0.003	0.285	
HIGH_DONATIONS^B	−0.003	0.356	−0.001	0.813	
U_NONCORE_EXP	0.993**	<.0001	0.360**	0.002	
U_NONCORE_EXP × *RURAL*^{B}	0.779	0.130	0.553	0.227	0.743
U_NONCORE_EXP × *CHURCH*^{B}	0.248	0.483	−0.084	0.646	0.398
U_NONCORE_EXP × *SYSTEM*^{B}	0.377	0.248	0.360*	0.054	0.963
U_NONCORE_EXP × *HIGH_CHARITY*^{B}	−0.378	0.169	−0.467**	0.008	0.783
U_NONCORE_EXP × *LOW_MEDICARE*^{B}	−0.711*	0.018	0.166	0.204	0.007
U_NONCORE_EXP × *LOW_MEDICAID*^{B}	−1.198**	0.002	−0.394	0.092	0.072
U_NONCORE_EXP × *NO_AUDIT*^{B}	0.583*	0.034	0.274*	0.068	0.317
U_NONCORE_EXP × *LOW_FISC_FEES*^{B}	0.048	0.900	−0.344*	0.069	0.351
U_NONCORE_EXP × *HIGH_DONATIONS*^{B}	−0.627*	0.013	0.340*	0.075	0.002
Adj. R^2	0.259		0.342		
N	339		309		

Table 18 (Continued)

The results represent a regression of *U_CORE_EXP* on positive and negative values of *U_NONCORE_EXP*, after partitioning on nine hospital characteristics. For Panel A (B), each indicator is coded as 1 when the hospital has that characteristic and reports earnings well above (below) the zero-profit benchmark, 0 otherwise. *U_CORE_EXP* (*U_NONCORE_EXP*) is calculated as actual core (non-core) expenses less expected core (non-core) expenses, where expectations are based on estimation in Table 14. Hospitals well above (below) [within] the zero-profit benchmark include those reporting bottom-line net income scaled by lagged total assets greater than 0.10 (less than 0.00) [greater than or equal to 0.00 and less than 0.04]. See Figure 2 for benchmark classifications and Appendix C for other variable definitions. The final column provides p-values of an F-test of whether the coefficients for the interactions of *U_NONCORE_EXP* and the partitioning variables differ for negative versus positive values of *U_NONCORE_EXP*. *, ** indicates significance at the 0.10, 0.01 level for a two-tailed t-test.

Table 19. Aggregate and firm-level statistics of political contributions.

Panel A: Aggregate statistics.

	Total	Executives	Non-Executives	PACs
No. of contributions	373,393	45,315	130,587	197,491
Percent of total sample	100%	12.14%	34.97%	52.89%
Aggregate dollar amount	$441,090,114	$41,411,371	$102,535,972	$297,142,771
Percent of total sample	100%	9.39%	23.25%	67.36%

Panel B: Firm-level statistics.

	Mean	Std Dev	Lower Quartile	Median	Upper Quartile
Political Contributions (unscaled)	142,448	215,406	11,950	57,225	182,214

Aggregate statistics are computed on the full data set of political contributions made by the employees and PACs of parent and subsidiary companies belonging to the S&P 500 for which sufficient data are available during fiscal years 2005-2011. A firm that entered and/or departed the S&P 500 during the sample period is included in the analysis during the years in which they belonged to the index. We restrict the upper bound of the sample to fiscal year 2011. Consistent with recommendations offered in the Center for Responsive Politics' OpenSecrets user's guide, we impose several exclusions. In the employee subsample, the following contribution-level observations are excluded: noncontributions (RealCode beginning with "Z9"), contributions to committees (RecipID beginning with "N"), and contributions to PACs (RecipID beginning with "P"). In the PAC subsample, the following contribution-level observations are excluded: noncontributions (RealCode beginning with "Z9") and transfers between committees as well as contributions to joint fundraising committees (RealCode beginning with "Z4").

Table 20. The market reaction to *Citizens United*: Descriptive statistics and univariate analyses.

Panel A: Descriptive statistics (N = 133).	Mean	Std Dev	Lower Quartile	Median	Upper Quartile
CAR(0, 0), in %	–0.082	1.591	–1.051	–0.116	0.846
CAR(0,+2), in %	–0.123	2.039	–1.279	–0.334	1.142
CAR(0,+5), in %	–0.482	3.648	–2.142	–0.612	0.995
Net Total Assets, in $ millions	18,215	25,816	4,571	9,844	23,056
Market-to-Book	2.254	1.281	1.440	1.868	2.666
Debt/NTA	0.266	0.153	0.170	0.261	0.340

Panel B: Univariate results for subsamples of political connectedness.

	POLITICAL (N = 60)				NEUTRAL (N = 73)				POLITICAL – NEUTRAL			
	Mean $\hat{\delta}$ (%)	t-statistic	% Positive	t-statistic	Mean $\hat{\delta}$ (%)	t-statistic	% Positive	t-statistic	Diff. in $\hat{\delta}$	t-statistic	Diff. in % Pos.	χ^2 stat
CAR(0, 0)	–0.475**	–2.256	28.33%***	–3.690	0.240	1.377	56.16%	1.050	–0.715**	–2.616	–27.83%***	10.373
CAR(0,+2)	–0.633***	–2.902	23.33%***	–4.840	0.300	1.146	50.69%	0.120	–0.929***	–2.746	–27.36%***	10.421
CAR(0,+5)	–1.219**	–2.248	31.67%***	–3.030	0.123	0.349	43.84%	–1.050	–1.343**	–2.074	–12.17%	2.063

Panel A presents descriptive statistics for a combined sample of POLITICAL and NEUTRAL firms. "POLITICAL" refers to our sample of historically politically connected firms, defined as firms in the fourth quartile of median firm-level pre-*Citizens United* contributions (scaled by NTA). "NEUTRAL" refers to our sample of historically non-politically connected firms, defined as firms in the first quartile of median firm-level pre-*Citizens United* contributions (scaled by NTA), as well as firms that did not contribute to political campaigns prior to *Citizens United*. CARs are estimated surrounding the date of the *Citizens United* decision, January 21, 2010, and represent average $\hat{\delta}$ estimates, where the parameter $\hat{\delta}$ is derived from the seemingly unrelated regression (SUR), $R_i = \alpha_i + \beta_i R_m + \delta_i Event + \varepsilon_i$. In this specification, R_i is the return series for individual firm i, R_m is the return series for CRSP value-weighted index (including dividends), and *Event* is a dummy variable that equals 1 on days included in the event window (0 otherwise). Daily returns are measured between April 1, 2008, and March 31, 2010, and are retrieved from the CRSP daily returns file. The event parameter estimate $\hat{\delta}$ corresponds to the average abnormal return for firm i in a given event window, and is multiplied by increments of 100% for each day in the event window to obtain the cumulative abnormal return (CAR). Control variables are measured as of fiscal year-end 2009 and are winsorized at the 1st and 99th percentiles. // Panel B presents summary statistics and univariate tests of average $\hat{\delta}$ estimates for the subsamples of POLITICAL and NEUTRAL firms, in addition to a nonparametric test based on the proportion of positive CARs. t-statistics reflecting the statistical significance of differences in mean $\hat{\delta}$ values assume unequal variances between the two groups. Statistical tests of the difference in proportions of positive CARs between the two sub-groups utilize the Chi-squared distribution.

Table 21. The market reaction to *Citizens United*: Cross-sectional analysis.

	CAR(0, 0)	CAR(0,+2)	CAR(0,+5)
Variable	I	II	III
Political Dummy	–0.750***	–0.915**	–1.327**
	(0.007)	(0.012)	(0.042)
Log(Net Total Assets)	–0.042	–0.046	–0.022
	(0.747)	(0.788)	(0.944)
Market-to-Book	0.268**	0.047	0.085
	(0.020)	(0.756)	(0.753)
Debt/NTA	0.112	0.378	1.095
	(0.898)	(0.742)	(0.598)
R^2	0.103	0.055	0.037
N	133	133	133

The results represent estimates from a multivariate ordinary least squares regression where average $\hat{\delta}$ estimates are regressed on a measure of historical political connectedness (Political Dummy) and control variables. The parameter $\hat{\delta}$ is derived from the seemingly unrelated regression (SUR), $R_i = \alpha_i + \beta_i R_m + \delta_i Event + \varepsilon_i$ where R_i is the return series for individual firm i, R_m is the return series for CRSP value-weighted index (including dividends), and *Event* is a dummy variable that equals 1 on days included in the event window (0 otherwise). Daily returns are measured between April 1, 2008, and March 31, 2010, and are retrieved from the CRSP daily returns file. The event parameter estimate $\hat{\delta}$ corresponds to the average abnormal return for firm i in a given event window, and is multiplied by increments of 100% for each day in the event window to obtain the cumulative abnormal return (CAR). Control variables are measured as of fiscal year-end 2009 and are winsorized at the 1st and 99th percentiles. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. Standard errors are corrected for contemporaneous cross-correlation in the residuals through use of the SUR methodology. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 22. Univariate analysis.

		All firms		Political Contribution Quartiles 1		2		3		4		Difference	
		N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	(1) – (4)	t-statistic
Panel A: Cash holdings and its traditional determinants.													
Log(Cash/NTA)	Pre-*Citizens United*	871	–2.652	217	–2.818	224	–2.718	220	–2.761	210	–2.295	–0.523***	–4.127
	Post-*Citizens United*	454	–2.423	111	–2.619	115	–2.414	112	–2.650	116	–2.025	–0.594***	–3.462
	Pre – Post		–0.229***		–0.199		–0.304**		–0.111		–0.269*		
	t-statistic		–2.980		–1.389		–2.068		–0.710		–1.691		
Log(Total Assets)	Pre-*Citizens United*	871	9.371	217	9.146	224	9.338	220	9.732	210	9.260	–0.114	–1.063
	Post-*Citizens United*	454	9.566	111	9.392	115	9.414	112	9.949	116	9.512	–0.120	–0.812
	Pre – Post		–0.195***		–0.246*		–0.075		–0.217**		–0.252*		
	t-statistic		–2.980		–1.892		–0.519		–2.061		–1.961		
Log(Sales)	Pre-*Citizens United*	871	9.319	217	9.140	224	9.271	220	9.568	210	9.295	–0.154	–1.473
	Post-*Citizens United*	454	9.436	111	9.296	115	9.308	112	9.671	116	9.469	–0.173	–1.183
	Pre – Post		–0.116*		–0.155		–0.037		–0.102		–0.174		
	t-statistic		–1.840		–1.264		–0.249		–1.129		–1.335		
Debt/NTA	Pre-*Citizens United*	870	0.255	217	0.242	224	0.272	220	0.273	209	0.230	0.012	0.858
	Post-*Citizens United*	454	0.284	111	0.250	115	0.312	112	0.306	116	0.268	–0.017	–0.923
	Pre – Post		–0.030***		–0.008		–0.041**		–0.033*		–0.038**		
	t-statistic		–3.330		–0.481		–2.104		–1.941		–2.187		
Market-to-Book	Pre-*Citizens United*	871	2.380	217	2.128	224	2.220	220	2.374	210	2.815	–0.687***	–4.680
	Post-*Citizens United*	454	2.163	111	1.963	115	2.146	112	2.114	116	2.418	–0.454***	–2.889
	Pre – Post		0.217**		0.165		0.075		0.259		0.398**		
	t-statistic		2.480		1.303		0.496		1.239		2.051		
Cash Flow/NTA	Pre-*Citizens United*	866	0.113	216	0.109	223	0.111	220	0.103	207	0.130	–0.021***	–3.200
	Post-*Citizens United*	450	0.112	111	0.109	113	0.115	112	0.106	114	0.119	–0.010	–1.056
	Pre – Post		0.001		0.000		–0.004		–0.003		0.011		
	t-statistic		0.160		0.018		–0.624		–0.345		1.265		
Std(Cash Flow/NTA)	Pre-*Citizens United*	871	0.036	217	0.032	224	0.034	220	0.034	210	0.043	–0.011***	–2.761
	Post-*Citizens United*	454	0.035	111	0.027	115	0.036	112	0.035	116	0.041	–0.014***	–3.521
	Pre – Post		0.001		0.005		–0.002		–0.001		0.002		
	t-statistic		0.480		1.439		–0.586		–0.113		0.481		
Net Working Capital/NTA	Pre-*Citizens United*	871	0.001	217	0.039	224	–0.002	220	–0.001	210	–0.032	0.071***	5.368
	Post-*Citizens United*	454	0.008	111	0.040	115	0.006	112	–0.001	116	–0.010	0.050***	3.042
	Pre – Post		–0.007		–0.001		–0.009		0.000		–0.022		
	t-statistic		–0.970		–0.054		–0.513		–0.023		–1.291		
R&D/Sales	Pre-*Citizens United*	871	0.038	217	0.026	224	0.036	220	0.034	210	0.056	–0.030***	–4.563
	Post-*Citizens United*	454	0.035	111	0.024	115	0.029	112	0.034	116	0.051	–0.027***	–3.187
	Pre – Post		0.003		0.001		0.006		0.000		0.005		
	t-statistic		0.780		0.216		0.892		0.039		0.553		

Table 22 (Continued)

R&D Missing Dummy	Pre-*Citizens United*	871	0.369	217	0.327	224	0.362	220	0.432	210	0.352	–0.025	–0.548
	Post-*Citizens United*	454	0.368	111	0.306	115	0.365	112	0.438	116	0.362	–0.056	–0.888
	Pre – Post		0.001		0.021		–0.004		–0.006		–0.010		
	t-statistic		0.030		0.383		–0.065		–0.098		–0.174		
Capital Expenditures/NTA	Pre-*Citizens United*	871	0.060	217	0.066	224	0.060	220	0.057	210	0.054	0.012***	2.764
	Post-*Citizens United*	454	0.051	111	0.059	115	0.053	112	0.054	116	0.040	0.018***	3.507
	Pre – Post		0.008***		0.007		0.007		0.003		0.014***		
	t-statistic		3.390		1.224		1.316		0.956		3.881		
Dividend Dummy	Pre-*Citizens United*	871	0.836	217	0.848	224	0.813	220	0.841	210	0.843	0.005	0.145
	Post-*Citizens United*	454	0.855	111	0.856	115	0.817	112	0.857	116	0.888	–0.032	–0.721
	Pre – Post		–0.019		–0.008		–0.005		–0.016		–0.045		
	t-statistic		–0.890		–0.190		–0.109		–0.387		–1.118		
Panel B: Governance variables.													
E-Index	Pre-*Citizens United*	213	4.132	51	3.863	61	4.393	43	3.930	58	4.241	–0.379*	–1.799
	Post-*Citizens United*	357	1.742	87	1.690	93	1.882	85	1.506	92	1.870	–0.018	–1.164
	Pre – Post		2.389***		2.173***		2.512***		2.424***		2.372***		
	t-statistic		26.190		11.555		13.555		14.396		13.477		
Proportion of Busy Directors	Pre-*Citizens United*	327	0.176	74	0.173	86	0.167	79	0.162	88	0.201	–0.028	–1.224
	Post-*Citizens United*	380	0.132	88	0.114	97	0.137	95	0.142	100	0.134	–0.020	–1.259
	Pre – Post		0.044***		0.059***		0.031		0.019		0.067***		
	t-statistic		4.330		3.005		1.526		0.846		3.591		
Excess CEO Compensation	Pre-*Citizens United*	868	–0.047	216	–0.175	222	–0.085	220	0.035	210	0.039	–0.214**	–2.066
	Post-*Citizens United*	453	0.099	111	0.067	115	0.053	112	0.248	115	0.030	0.037	0.315
	Pre – Post		–0.146**		–0.242**		–0.139		–0.213*		0.009		
	t-statistic		–2.510		–2.078		–1.396		–1.655		0.077		
Shareholder Inactivism Dummy	Pre-*Citizens United*	251	0.570	51	0.667	63	0.746	74	0.378	63	0.540	0.127	1.373
	Post-*Citizens United*	237	0.578	45	0.600	56	0.750	71	0.451	65	0.554	0.046	0.477
	Pre – Post		–0.008		0.067		–0.004		–0.072		–0.014		
	t-statistic		–0.190		0.672		–0.049		–0.880		–0.160		

Univariate analyses are computed on the full sample of S&P 500 firms for which sufficient data are available during fiscal years 2005-2011. Quartiles of political connectedness are based on the Political Dummy, defined as median firm-level pre-*Citizens United* contributions (scaled by NTA). To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 23. The differential relation in political contributions for historically politically connected firms relative to historically non-politically connected firms in the post-*Citizens United* period.

Variable	Coef. (p-value)
Political Dummy	1.556* (0.075)
Contributions/NTA	0.808*** (0.000)
Log(Net Total Assets)	–0.234 (0.536)
Debt/NTA	–2.148 (0.392)
Market-to-Book	0.035 (0.930)
Cash Flow/NTA	–2.603 (0.719)
Std(Cash Flow/NTA)	3.465 (0.789)
Net Working Capital/NTA	–0.870 (0.770)
R&D/Sales	–3.649 (0.622)
R&D Missing Dummy	0.399 (0.659)
Capital Expenditures/NTA	–12.201 (0.299)
Dividend Dummy	–0.756 (0.487)
Adj. R^2	0.776
N	259

The results represent pooled regressions of Political Contributions/NTA on traditional determinants of cash holdings and a measure of historical political connectedness (Political Dummy), and are estimated for the period 2010-2011 for which sufficient data are available. All traditional determinants of cash holdings are lagged in an effort to alleviate endogeneity. To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. Political Dummy is a measure of historical political connectedness and is based on median firm-level pre-*Citizens United* contributions (scaled by NTA), set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 24. The change in cash holdings modeled as a function of the change in political contributions as computed in year 2010 (post-*Citizens United*) relative to year 2008 (pre-*Citizens United*).

Variable	Coef. (p-value)
ΔContributions/NTA	0.017** (0.043)
ΔLog(Net Total Assets)	–0.820*** 0.000
ΔDebt/NTA	1.745*** (0.004)
ΔMarket-to-Book	0.172** (0.020)
ΔCash Flow/NTA	1.401 (0.102)
ΔStd(Cash Flow/NTA)	1.914 (0.454)
ΔNet Working Capital/NTA	–0.339 (0.583)
ΔR&D/Sales	1.759 (0.593)
ΔR&D Missing Dummy	–0.502 (0.162)
ΔCapital Expenditures/NTA	–4.885** (0.015)
ΔDividend Dummy	0.191 (0.640)
Adj. R^2	0.186
N	236

The results represent pooled regressions of 236 firm-level differences computed as observed values in 2010 (post-*Citizens United*) less observed values in 2008 (pre-*Citizens United*). The dependent variable is ΔLog(Cash/NTA). To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 25. The differential relation in cash holdings for historically politically connected firms relative to historically non-politically connected firms in the post-*Citizens United* relative to pre-*Citizens United* periods.

Variable	Coef. (p-value)
Political Dummy	–0.018
	(0.764)
Post-*Citizens United* Dummy	–0.072
	(0.416)
Political Dummy × Post-*Citizens United* Dummy	0.181**
	(0.026)
Log(Cash/NTA)	0.770***
	0.000
Log(Net Total Assets)	–0.011
	(0.676)
Debt/NTA	–0.314*
	(0.076)
Market-to-Book	0.020
	(0.304)
Cash Flow/NTA	0.783
	(0.102)
Std(Cash Flow/NTA)	1.354**
	(0.026)
Net Working Capital/NTA	–0.089
	(0.682)
R&D/Sales	0.709*
	(0.062)
R&D Missing Dummy	–0.086
	(0.160)
Capital Expenditures/NTA	–2.149***
	0.000
Dividend Dummy	–0.149**
	(0.027)
Adj. R^2	0.798
N	707

The results represent a regression of Log(Cash/NTA) on traditional determinants of cash holdings, a measure of historical political connectedness (Political Dummy), and year fixed effects, and are estimated for the period 2005-2011 for which sufficient data are available. All traditional determinants of cash holdings are lagged in an effort to alleviate endogeneity. Standard errors are clustered by firm. To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. Political Dummy is a measure of historical political connectedness and is based on median firm-level pre-*Citizens United* contributions (scaled by NTA), set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 26. The differential relation in corporate cash holdings with respect to political connectedness and governance quality in the post-*Citizens United* relative to pre-*Citizens United* periods.

Variable	High Compensation	Busy Boards	E-Index	Proposal Entertained during 2007-2008 or 2010-2011	Proposal Entertained during 2007-2008	Proposal Entertained during 2010-2011
	I	II	III	IV	V	VI
Political Dummy	0.030	0.157	0.523	0.119	–0.116	0.053
	(0.695)	(0.358)	(0.151)	(0.525)	(0.610)	(0.749)
Post-*Citizens United* Dummy	0.218*	0.230**	0.055	0.194	–0.086	0.214
	(0.080)	(0.044)	(0.827)	(0.414)	(0.784)	(0.362)
Political Dummy × Post-*Citizens United* Dummy	–0.063	–0.161	–0.556	–0.057	–0.135	–0.059
	(0.601)	(0.416)	(0.179)	(0.740)	(0.549)	(0.682)
Weak Governance	0.011	0.758**	–0.039	–0.311	0.101	–0.277
	(0.888)	(0.035)	(0.420)	(0.249)	(0.764)	(0.296)
Political Dummy × Weak Governance	–0.092	–0.878	–0.150	–0.243	0.116	–0.148
	(0.398)	(0.173)	(0.102)	(0.258)	(0.648)	(0.476)
Post-*Citizens United* Dummy × Weak Governance	–0.311**	–0.742	–0.085	–0.266	0.127	–0.266
	(0.034)	(0.176)	(0.470)	(0.325)	(0.711)	(0.314)
Political Dummy × Post-*Citizens United* Dummy × Weak Governance	0.403**	1.586*	0.244	0.670**	–0.000	0.583*
	(0.028)	(0.065)	(0.114)	(0.035)	(0.999)	(0.062)
Control variables?	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.800	0.837	0.818	0.866	0.861	0.865
N	705	380	316	238	238	238

The results represent a regression of Log(Cash/NTA) on traditional determinants of cash holdings, a measure of historical political connectedness (Political Dummy), a measure of weak corporate governance (the Compensation Dummy in column I, Busy Directors in column II, E-Index Dummy in column III, or Shareholder Inactivism Dummy in columns IV, V, and VI), and year fixed effects, and are estimated for the period 2005-2011 for which sufficient data are available. Data used to estimate the E-Index and Busy Directors measures are derived from RiskMetrics and available for the period 2007-2011. Data used to estimate the Compensation Dummy are derived from Execucomp and available for the period 2005-2011. All traditional determinants of cash holdings are lagged in an effort to alleviate endogeneity. Standard errors are clustered by firm. To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. Political Dummy is a measure of historical political connectedness and is based on median firm-level pre-*Citizens United* contributions (scaled by NTA), set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms. Control variables include the lagged values of Log(Cash/NTA), Log(Net Total Assets), Debt/NTA, the Market-to-Book ratio, Cash Flow/NTA, Std(Cash Flow/NTA), Net Working Capital/NTA, R&D/Sales, R&D Missing Dummy, Capital Expenditures/NTA, and the Dividend Dummy. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 27. The differential relation in cash holdings as a function of within-firm deviation of political beliefs in the post-*Citizens United* relative to pre-*Citizens United* periods.

Variable	Coef. (p-value)
Political Deviation	–0.274***
	(0.010)
Post-*Citizens United* Dummy	–0.323**
	(0.011)
Political Deviation × Post-*Citizens United* Dummy	0.537**
	(0.011)
Log(Cash/NTA)	0.845***
	0.000
Log(Net Total Assets)	0.008
	(0.813)
Debt/NTA	–0.437
	(0.136)
Market-to-Book	–0.016
	(0.398)
Cash Flow/NTA	0.548
	(0.400)
Std(Cash Flow/NTA)	0.529
	(0.374)
Net Working Capital/NTA	0.177
	(0.400)
R&D/Sales	0.465
	(0.272)
R&D Missing Dummy	–0.062
	(0.470)
Capital Expenditures/NTA	–2.999**
	(0.015)
Dividend Dummy	–0.198**
	(0.018)
Adj. R^2	0.848
N	281

The results represent a regression of Log(Cash/NTA) on traditional determinants of cash holdings, a measure of historical within-firm diversion in political beliefs (Political Deviation), and year fixed effects, and are estimated for the period 2005-2011 for which sufficient data are available. To be included in the test, the firm-year observation must report positive executive contributions and positive non-executive contributions, and be drawn from our sample of historically politically active firms (i.e., the upper quartile of pre-*Citizens United* political contributions). All traditional determinants of cash holdings are lagged in an effort to alleviate endogeneity. Standard errors are clustered by firm. To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. Political Deviation is a ranked variable to capture the degree of historical, within-firm divergence in political beliefs, ranging from 0.10 (bottom decile) to 1.00 (top decile). Deciles are formed based on the median firm-level absolute difference in the proportion of PAC contributions to Republicans and the proportion of executive contributions to Republicans as observed during the pre-*Citizens United* period. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 28. Validity of the natural experiment: Placebo regressions.

Event shifted backwards by:	I. One year	II. Two years	III. Three years
Variable	Coef. (p-value)	Coef. (p-value)	Coef. (p-value)
Political Dummy	0.001	–0.012	–0.009
	(0.993)	(0.864)	(0.916)
Post-*Citizens United* Dummy	–0.009	–0.076	0.052
	(0.914)	(0.361)	(0.557)
Political Dummy × Post-*Citizens United* Dummy	0.061	0.036	–0.002
	(0.378)	(0.578)	(0.984)
Log(Cash/NTA)	0.774***	0.773***	0.802***
	(0.000)	(0.000)	(0.000)
Log(Net Total Assets)	–0.015	–0.019	–0.027
	(0.541)	(0.422)	(0.214)
Debt/NTA	–0.385**	–0.399***	–0.371***
	(0.012)	(0.005)	(0.009)
Market-to-Book	0.024	0.027*	0.027
	(0.161)	(0.088)	(0.110)
Cash Flow/NTA	0.638	0.606	0.285
	(0.128)	(0.134)	(0.460)
Std(Cash Flow/NTA)	1.185*	1.602**	1.495
	(0.072)	(0.023)	(0.110)
Net Working Capital/NTA	0.030	0.033	0.004
	(0.879)	(0.845)	(0.977)
R&D/Sales	0.432	0.715**	0.481
	(0.207)	(0.039)	(0.125)
R&D Missing Dummy	–0.067	–0.063	–0.066
	(0.199)	(0.204)	(0.181)
Capital Expenditures/NTA	–1.689***	–1.599***	–1.187**
	(0.000)	(0.000)	(0.019)
Dividend Dummy	–0.183***	–0.201***	–0.145**
	(0.002)	(0.000)	(0.030)
Adj. R^2	841	878	890
N	0.793	0.805	0.808

The results represent a regression of Log(Cash/NTA) on traditional determinants of cash holdings, a measure of historical political connectedness (Political Dummy), and year fixed effects. In column I (II) [III], the post-*Citizens United* event window is shifted by one (two) [three] year(s) into the past; that is, Post-*Citizens United* Dummy is set to 1 for fiscal years 2009-2011 (2008-2011) [2007-2011], and 0 for fiscal years 2005 through the year before the post-event window. Standard errors are clustered by firm. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. Political Dummy is a measure of historical political connectedness and is based on median firm-level pre-event contributions (scaled by NTA), set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms. Political Dummy is redefined for each of the three possible pre-event windows presented in the table. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 29. Robustness checks on the market reaction to *Citizens United*: Univariate analyses.

I. After accounting for confounding events.

	POLITICAL			NEUTRAL			POLITICAL – NEUTRAL	
	N	Mean $\hat{\delta}$ (%)	t-statistic	N	Mean $\hat{\delta}$ (%)	t-statistic	Difference	t-statistic
CAR(0, 0)	55	–0.592***	–3.240	70	0.227	1.260	–0.820***	–3.190
CAR(0,+2)	50	–0.737***	–3.480	67	0.253	1.050	–0.989***	–3.090
CAR(0,+5)	34	–0.785*	–1.750	56	–0.010	–0.030	–0.775	–1.330

II. After excluding the defense, energy, and utilities industries.

	POLITICAL			NEUTRAL			POLITICAL – NEUTRAL	
	N	Mean $\hat{\delta}$ (%)	t-statistic	N	Mean $\hat{\delta}$ (%)	t-statistic	Difference	t-statistic
CAR(0, 0)	50	–0.548**	–2.474	64	0.211	1.092	–0.759**	–2.582
CAR(0,+2)	50	–0.631**	–2.581	64	0.336	1.203	–0.967**	–2.604
CAR(0,+5)	50	–1.413**	–2.231	64	0.237	0.609	–1.650**	–2.220

III. After redefining POLITICAL on the basis of within-industry pre-Citizens United contributions.

	POLITICAL			NEUTRAL			POLITICAL – NEUTRAL	
	N	Mean $\hat{\delta}$ (%)	t-statistic	N	Mean $\hat{\delta}$ (%)	t-statistic	Difference	t-statistic
CAR(0, 0)	58	–0.272	–1.391	67	0.229	1.294	–0.500*	–1.899
CAR(0,+2)	58	–0.459**	–2.113	67	0.275	1.015	–0.734**	–2.113
CAR(0,+5)	58	–0.766*	–1.784	67	0.080	0.210	–0.845	–1.476

IV. Placebo analysis.

	POLITICAL			NEUTRAL (placebo)			POLITICAL – NEUTRAL	
	N	Mean $\hat{\delta}$ (%)	t-statistic	N	Mean $\hat{\delta}$ (%)	t-statistic	Difference	t-statistic
CAR(0, 0)	60	–0.475**	–2.256	60	0.609***	2.970	0.011***	3.691
CAR(0,+2)	60	–0.633***	–2.902	60	0.504	1.565	0.004***	2.927
CAR(0,+5)	60	–1.219**	–2.248	60	0.085	0.189	0.002*	1.855

This table presents sensitivity analyses on the results provided in Table 20 Panel B. In analysis I, we exclude firms that are associated with a confounding event, defined as a firm filing a form 8-K with the SEC during the event window or the day preceding the *Citizens United* decision. In analysis II, we exclude politically exposed firms, i.e., those belonging to the defense, energy, or utilities industries, where Fama-French 48-industry classifications are used to identify firms belonging to the defense (industry code 26), energy (industry codes 29 and 30), and utilities (industry code 31) industries. In analysis III, Political Dummy is redefined in terms of median firm-level pre-*Citizens United* contributions (scaled by Net Total Assets) within each 1-digit SIC industry, set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms. In analysis IV, politically "neutral" firms are replaced with "placebo-neutral" firms, defined as companies that match to POLITICAL firms on industry (two-digit SIC) and size (Log of Total Assets) within the same fiscal year, and where Political Dummy is set to 0 for these "placebo-neutral" companies. Summary statistics and univariate tests are based on average $\hat{\delta}$ estimates, where the parameter $\hat{\delta}$ is derived from the seemingly unrelated regression (SUR), $R_i = \alpha_i + \beta_i R_m + \delta_i Event + \varepsilon_i$ where R_i is the return series for individual firm i, R_m is the return series for CRSP value-weighted index (including dividends), and *Event* is a dummy variable that equals 1 on days included in the event window (0 otherwise). Daily returns are measured between April 1, 2008, and March 31, 2010, and are retrieved from the CRSP daily returns file. The event parameter estimate $\hat{\delta}$ corresponds to the average abnormal return for firm i in a given event window, and is multiplied by increments of 100% for each day in the event window to obtain the cumulative abnormal return (CAR). "POLITICAL" refers to our sample of historically politically connected firms, defined as firms in the fourth quartile of median firm-level pre-*Citizens United* contributions (scaled by NTA). "NEUTRAL" refers to our sample of historically non-politically connected firms, defined as firms in the first quartile of median firm-level pre-*Citizens United* contributions (scaled by NTA), as well as firms that did not contribute to political campaigns prior to *Citizens United*. t-statistics reflecting the statistical significance of differences in means assume unequal variances between the two groups.

Table 30. Robustness checks on the market reaction to *Citizens United*: Multivariate analyses.

	I. Confounding events			*II. Politically exposed firms*			*III. Redefining POLITICAL*		
Variable	CAR(0, 0)	CAR(0,+2)	CAR(0,+5)	CAR(0, 0)	CAR(0,+2)	CAR(0,+5)	CAR(0, 0)	CAR(0,+2)	CAR(0,+5)
Political Dummy	–0.841***	–0.975***	–0.653	–0.804***	–0.985**	–1.735**	–0.549**	–0.752**	–0.807
	(0.001)	(0.006)	(0.298)	(0.008)	(0.016)	(0.022)	(0.041)	(0.043)	(0.176)
Log(Net Total Assets)	–0.092	–0.047	–0.259	–0.074	0.065	0.182	–0.034	–0.006	–0.004
	(0.481)	(0.788)	(0.475)	(0.603)	(0.739)	(0.614)	(0.789)	(0.971)	(0.989)
Market-to-Book	0.297***	0.096	–0.270	0.294**	0.056	0.165	0.258**	0.104	0.052
	(0.008)	(0.509)	(0.413)	(0.015)	(0.728)	(0.582)	(0.023)	(0.503)	(0.835)
Debt/NTA	–0.002	0.443	–0.339	0.216	0.759	1.091	0.085	0.118	1.227
	(0.998)	(0.676)	(0.849)	(0.814)	(0.542)	(0.636)	(0.922)	(0.922)	(0.526)
R^2	0.159	0.080	0.028	0.128	0.059	0.051	0.084	0.038	0.021
N	125	117	90	114	114	114	125	125	125

IV. The impact of corporate governance quality on the announcement date.

Variable	E-Index	Busy Directors	High Compensation
Political Dummy × Weak Governance	–0.449***	–1.222	–0.791**
	(0.005)	(0.454)	(0.027)
(1 – Political Dummy) × Weak Governance	–0.186	2.210	–0.141
	(0.297)	(0.210)	(0.651)
Log(Net Total Assets)	–0.139	–0.041	–0.072
	(0.351)	(0.790)	(0.587)
Market-to-Book	0.034	0.151	0.250**
	(0.838)	(0.357)	(0.031)
Debt/NTA	0.638	0.150	0.093
	(0.509)	(0.888)	(0.917)
R^2	0.115	0.042	0.087
N	85	110	133

This table presents sensitivity analyses on the results provided in Table 21. In analysis I, we exclude firms that are associated with a confounding event, defined as a firm filing a form 8-K with the SEC during the event window or the day preceding the *Citizens United* decision. In analysis II, we exclude politically exposed firms, i.e., those belonging to the defense, energy, or utilities industries, where Fama-French 48-industry classifications are used to identify firms belonging to the defense (industry code 26), energy (industry codes 29 and 30), and utilities (industry code 31) industries. In analysis III, Political Dummy is redefined in terms of median firm-level pre-*Citizens United* contributions (scaled by Net Total Assets) within each 1-digit SIC industry, set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms. In analysis IV, the market reaction to *Citizens United* is modeled as a function of corporate governance quality. The results represent estimates from a multivariate ordinary least squares regression where average $\hat{\delta}$ estimates are regressed on a measure of historical political connectedness (Political Dummy) and control variables. The parameter $\hat{\delta}$ is derived from the seemingly unrelated regression (SUR), $R_i = \alpha_i + \beta_i R_m + \delta_i Event + \varepsilon_i$ where R_i is the return series for individual firm i, R_m is the return series for CRSP value-weighted index (including dividends), and *Event* is a dummy variable that equals 1 on days included in the event window (0 otherwise). Daily returns are measured between April 1, 2008, and March 31, 2010, and are retrieved from the CRSP daily returns file. The event parameter estimate $\hat{\delta}$ corresponds to the average abnormal return for firm i in a given event window, and is multiplied by increments of 100% for each day in the event window to obtain the cumulative abnormal return (CAR). Control variables are measured as of fiscal year-end 2009 and are winsorized at the 1st and 99th percentiles. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. Standard errors are corrected for contemporaneous cross-correlation in the residuals through use of the SUR methodology. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.

Table 31. Robustness checks on the differential relation in cash holdings for historically politically connected firms relative to historically non-politically connected firms in the post-*Citizens United* relative to pre-*Citizens United* periods.

	Accounting for politically exposed industries		Industry-adjusted values
Variable	I	II	III
Political Dummy	–0.028	–0.023	–0.062
	(0.646)	(0.692)	(0.322)
Post-*Citizens United* Dummy	–0.124	–0.036	–0.252**
	(0.189)	(0.696)	(0.019)
Political Dummy × Post-*Citizens United* Dummy	0.175**	0.153*	0.250***
	(0.026)	(0.058)	(0.005)
Control variables?	Yes	Yes	Yes
Adj. R^2	0.793	0.803	0.672
N	596	669	559

This table represent a regression of Log(Cash/NTA) on traditional determinants of cash holdings, a measure of historical political connectedness (Political Dummy), and year fixed effects, and are estimated for the period 2005-2011 for which sufficient data are available. // The results in columns I and II parallel those provided in Table 25 after controlling for politically exposed firms. In column I, firms belonging to the defense, energy, and utilities industries are excluded, where Fama-French 48-industry classifications are used to identify firms belonging to the defense (industry code 26), energy (industry codes 29 and 30), and utilities (industry code 31) industries. In column II, Political Dummy is redefined in terms of median firm-level pre-*Citizens United* contributions (scaled by Net Total Assets) within each 1-digit SIC industry, set equal to 1 for firms in the fourth quartile and equal to 0 for firms in the first quartile and for historically non-politically active firms. // The results in column III parallel those provided in Table 25 after industry-adjusting both the dependent and independent variables. Industry-adjusted forms of all continuous variables are defined relative to the annual industry median. Fama-French 48-industry classifications are used for the industry adjustment. To be included in the test, the industry corresponding to each firm-year observation must include a minimum of five observations for that year. // All traditional determinants of cash holdings are lagged in an effort to alleviate endogeneity. Standard errors are clustered by firm. To provide a cleaner test, fiscal year 2009 observations (i.e., those immediately preceding passage of *Citizens United*) are excluded. Net Total Assets (NTA) is computed as Total Assets less Cash & Cash Equivalents. See Appendix D for other variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles. p-values are shown in parentheses. *, **, *** indicates significance at the 0.10, 0.05, 0.01 level for a two-tailed t-test.