Corporate Political Connectedness and Accounting Quality: A Quasi-Natural Experiment

Albert Kwame Mensah
College of Business
City University of Hong Kong
83 Tat Chee Avenue
Kowloon Tong
Hong Kong SAR
akmensah2-c@my.cityu.edu.hk
(+852) 3442-2255 /53162520

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ABSTRACT:
A number of townships, cities, counties, and states across the U.S. have recently passed measures, resolutions, ordinances, and laws modeled on the American Anti-Corruption Act, which aims to: (1) "make it illegal to purchase political influence," and (2) "end secret money." I exploit these staggered events—occurring between 2014 (the first year of adoption) and 2017—as exogenous negative shocks to both legal and illegal affiliations with politicians, and provide new causal evidence on the effect of political connection on financial reporting, as a key corporate decision area. Using a difference-in-differences estimation for the 2010-2018 quarterly reporting period, I find that, relative to firms in non-adopting locations, firms headquartered in adopting locations have significantly higher accounting quality (as proxied by several non-directional measures of accruals). I then focus on target beating (as a specific type of managerial incentive), where I find that, relative to control firms, treated firms are also less likely to: (1) use income-increasing accruals to meet/beat analyst earnings forecasts, and (2) meet/beat analyst earnings targets by up to one cent. Finally, I show that the stock market responds favorably to this ex-post enhancement in the quality of earnings, which also culminates in stock prices better capturing information about future earnings and cash flows.

Keywords: American Anti-Corruption Act; Political Connection; Accounting Quality; Target Beating; Price Efficiency

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I. INTRODUCTION

Though U.S. campaign finance laws and lobbying rules generally allow firms' political donations and lobbying expenditures, several high-profile convictions have exposed public officials granting political favors in exchange for illegal donations. For example, in an extensive investigation code-named "Operation Tennessee Waltz," prominent state senator John Ford and 11 other state officials demanded payments from firms in return for legislative action or support through numerous shake-downs. The investigation led to a series of convictions from August 21, 2006 to August 28, 2007. In another widely-covered case, former Alabama governor Don Siegelman was, on June 29, 2006, convicted on several counts of extortion and for politically orchestrating, in exchange for $500,000, the appointment of the CEO of HealthSouth (NYSE: HLS) to the regulatory board of a state hospital. In a more recent case, federal corruption charges were brought, on January 21, 2014, against then-Governor of Virginia Bob McDonnell and his wife after a federal investigation into gifts, trips, loans, and other items valued at more than $175,000 "improperly" received from a political donor (Jonnie Williams Sr., former CEO of Star Scientific, Inc. [NASDAQ: STSI]). McDonnell and his wife were convicted of extortion under color of official right, among other charges.[1]

The avalanche of such above-highlighted violations and abuse of political power have heightened calls to tighten anti-corruption laws, consequently triggering anti-corruption efforts, campaigns, and legislation across the U.S. One such campaign is currently pushing for the nationwide adoption of the American Anti-Corruption Act (hereafter, "AA-CA"), which has

[1] On January 26, 2015, McDonnell was sentenced to two years in prison followed by a two-year supervised release. The Supreme Court overturned this verdict on June 27, 2016, but left open the option to retry him if the prosecution brings sufficient evidence warranting conviction. However, there is suspicion that the Supreme Court's decision was compromised, as the justices themselves had been suspected of receiving expensive gifts or travel while serving on America's highest court (see, for example, articles in New York Times [February 26, 2016] and Washington Post [June 27, 2016]).

already been introduced locally by certain U.S. townships, cities, counties, and states. Its official website (https://anti-corruptionact.org/) declares that the AA-CA aims to: "make it illegal to purchase political influence" and "end secret money."[2] Aside from receiving considerable attention from lawmakers at city-, state- and federal- level,[3] the AA-CA-based measures, resolutions, ordinances, and laws (hereafter, "AA-CA-based promulgations") adopted across the U.S. have implications for research, in particular, because it allows for the use of "a shock-based research design that mitigates endogeneity concerns inherent in examining cross-sectional relations" (Fauver, Hung, Li, and Taboada 2017, p.120).

In this study, I exploit the passage of AA-CA-based promulgations across the U.S. (i.e., adoptions by townships, cities, counties, and states) as an exogenous shock to both *legal* and *illegal* corporate connections with politicians.[4] These AA-CA-based promulgations are significantly increasing the costs associated with political connection, to the extent that they can exceed the benefits from affiliating with public officials. Ultimately, this disincentivizes firms from initiating or maintaining connections with government representatives. Using this setting, I examine how an exogenous decline in political connection impacts firm behavior, focusing particularly on financial reporting quality.[5] I examine the link between political connection (hereafter, "PC") and financial

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[2] The AA-CA agenda is being pushed across the U.S. by RepresentUs. As stated in the anti-corruption resolution passed by the city of Southfield (Michigan) on September 26, 2016, "RepresentUs is building a movement to pass Anti-Corruption Acts in cities, states and federally. Each Act is uniquely tailored to meet the needs of locales across the country." This resolution also states that AA-CA "is model legislation that sets a standard for city, state and federal laws that prevent money from corrupting American government" (see https://represent.us/our-wins-old/).

[3] As of October 6, 2018, "more than 80 Anti-Corruption Acts and Resolutions [have been passed] in cities and states across the country" (https://represent.us/our-wins-old/).

[4] Consistent with the above-stated objectives of the AA-CA, PC in this paper thus refers to two connection types: (1) *observed/legal connections*, such as those established through campaign contributions and lobbying spending; and (2) *unobserved/illegal connections*, such as those pointed out in the anecdotes where firms and politicians establish connections in secret. The second connection type has been largely unexamined in the political connection literature because such connections are unobservable at inception. The use of a shock-based design, however, helps to overcome this concern: both legal and illegal connections affected by AA-CA-based laws are bundled as treatment group.

[5] Throughout this paper, albeit I use "financial reporting quality," "earnings quality," "accounting quality," and "earnings management" interchangeably as in Chaney et al. (2011), I focus on *accruals-based earnings management* and therefore make subsequent discussions along this dimension.

reporting quality because: (1) to date, no prior research has made definitive causal inferences about this link (for related studies, see, e.g., Chaney, Faccio, and Parsley 2011; Ramanna and Roychowdhury 2010);[6] (2) the focus of prior PC studies (including those cited above on the financial reporting consequences of PC) has been largely on legal/observed connections rather than illegal/secret connections; and (3) institutions and disciplining mechanisms that constrain managerial opportunistic behavior are known to improve the quality of financial information supplied by firms to the capital market, consequently improving price efficiency (see, e.g., Fang, Huang, and Karpoff 2016; Fauver et al. 2017).[7] I consider these AA-CA-based promulgations as potential disciplining mechanisms capable of generating analogous effects.

Because PC in my setting encompasses both legal connection (hereafter, "LC") and illegal/secret connection (hereafter, "SC") [i.e., PC = LC + SC], I formulate separate hypotheses delineating the manner in which LC and SC are uniquely related to accounting quality and then later show how AA-CA-based promulgations overall impact reporting behavior of PC firms.[8] I first put LC firms into perspective, where theories discussed in Chaney et al. (2011) and other related studies present opposing arguments on the effect of LC on financial reporting quality. On

[6] Of all related studies, only Ramanna and Roychowdhury (2010), to my knowledge, have made significant attempts at addressing endogeneity, albeit they do not expressly claim causality in their study. A likely reason for not claiming causality could be attributed to the fact that the assignment of firms to treatment and control groups was based on connections to political candidates formed long before the election event itself. Hence, although elections, as used in their paper and other papers (e.g., Baloria and Klassen 2018), are exogenous shocks to "election-related incentives," they were not the basis upon which firms were first assigned to groups. The concern of lack of complete randomness therefore remains. Aside from this, credible pollsters can predict election outcomes and some firms could use these credible polls to inform their decision to retain or drop connections. This concern is not entirely eliminated even if the setting looked at is close elections or elections with contradictory polls. This is because, based on pollster track records or firm experience with pollsters, some firms may trust certain polls more than others. For instance, in closely-contested elections, Republican-leaning corporations are more likely to believe Republican pollsters (e.g., Fox News) over pollsters that lean to the left. Ultimately, certain firms (if not all) self-select their connections; hence, the outcome of elections is not completely random. Houston, Jiang, Lin, and Ma (2014) seem to acknowledge this deficiency by stating that the validity of their causal inferences is contingent on the following identification premise: "outcome of the midterm election is unpredictable so that the election represents an exogenous event to the firms" (p.235).

[7] In a later section, I will empirically test whether this ex-post improvement in information quality does generate any capital market benefits.

[8] LC and SC are also subsequently used in this paper as firm descriptors to identify firm types. That is, "LC firm" corresponds to "legally connected firm," whereas "SC firm" corresponds to "illegally connected firm."

the one hand, LC firms tend to be subject to substantial monitoring and controls arising from public scrutiny (Fich and Shivdasani 2006) that serves to discipline managerial opportunistic tendencies. From this perspective, LC should be positively associated with earnings quality, all else equal: that is, compared to unconnected firms, LC firms are likely to have better accounting quality. On the other hand, there are two scenarios in which a negative association between LC and earnings quality would be expected. First, to mislead investors in order to extract private gains at their expense, LC firms could intentionally conceal, obscure, or delay the reporting of political benefits (i.e., gains obtained from positive interactions with connected politicians [see, e.g., Zimmerman 1983; Smith 2016; Baloria and Klassen 2018]) in financial statements, thus contributing to poor earnings quality. Second, anecdotal evidence and a large volume of academic literature suggest that connected politicians can shield LC firms from adverse consequences, whether self-made or extraneous (e.g., Dean, Vryza, and Fryxell 1998; Chen, Ding, and Kim 2010; Boubakri, Mansi, and Saffar 2013; Houston et al. 2014; Kim and Zhang 2016; Baloria and Klassen 2018). Such government-provided protection could embolden managers to engage in opportunistic reporting, or it could simply cause them to be inattentive to reporting quality. On these two bases, LC firms should have poorer earnings quality than unconnected firms.

I next put SC firms into perspective, where different theories generate a common directional prediction on the relationship between SC and accounting quality. Under the first reasoning, the need for secrecy makes SC "distortionary" and therefore detrimental to economic outcomes (Shleifer and Vishny 1993). To avoid potential disciplinary action from stakeholders (e.g., investors, regulators) arising from SC firms' concern that stakeholders can discern illegality from reported benefits derived from secret political activities (Zingales 1994; Shleifer and Vishny 1997; Leuz et al. 2003), managers of SC firms could conceal political benefits from stakeholders, consequently distorting the quality of earnings (Chaney et al. 2011). Under the second reasoning (similar to the one under LC), colluding government representatives' promise to provide protection to SC firms in times of difficulty could embolden managers of SC firms to engage in opportunistic reporting; it could also invoke a "decision inattention" problem in financial reporting, where managers of SC firms simply "care less" about the quality of accounting information (Chaney et al. 2011). Collectively, these scenarios suggest that, relative to unconnected firms, SC firms are likely to report poorer earnings.

As anti-corruption promulgations increasingly curtail the purchase of political influence (i.e., constrain both LC and SC), impacts on the hypothesized reporting behavior of connected firms should be considered.[9] From the first perspective, namely that LC firms report high-quality earnings because they are subject to substantial monitoring and scrutiny, anti-corruption promulgations can be expected to have no impact on accounting quality. This is because LC firms were already reporting high-quality accounting information prior to these promulgations' adoption. However, from the perspective that LC and SC firms aggressively engage in opportunistic

[9] Note here that, as mentioned before, the quasi-natural experiment adopted in this study helps me to put both LC and SC firms affected by these anti-corruption laws into one basket (i.e., treatment group), thus helping to examine the overall effect of the shock on PC. It is infeasible to tease out SC firms before applying the shock because SCs are unobservable, for SC firms establish connections in secret (please refer to the hypothesis development section for more discussion on this). Relatedly, selecting firms (e.g., LC firms) into the treatment group based on some observable firm characteristics (e.g., campaign contributions) before applying the shock will defeat the principle of randomness, which is extremely important in quasi-natural experiments. The shock has to be the *only* basis for sorting firms into treatment and control groups (see, e.g., shock-based studies such as Bertrand and Mullainathan 1999a, b, 2003; Bertrand, Duflo and Mullainathan 2004; Armstrong, Balakrishnan, and Cohen 2012; Jayaraman and Shivakumar 2013; Fauver et al. 2017, who *always* put in the treatment group, all firm types headquartered in locations affected by a shock). For example, Fauver et al. (2017) use country-level board reforms that require greater outside representation on the board of corporations. In the experiment, they put all firms from countries affected by the shock in the treatment group regardless of the fact that, in reality, there are different firm types (e.g., type 1 firms [i.e., those with more than one outside director], type II firms [i.e., those with one outside director], and type III firms [i.e., those with no outside directors]). It appears that the shock would be more suited for type III firms, but then these firms were not isolated in their study as those that the shock applied to: this was avoided to allow for the *random* assignment of firms into groups. In contrast, studies using the setting of close elections (e.g., Baloria and Klassen 2018; Ramanna and Roychowdhury 2010) appear to first condition firms' assignment to groups on firm fundamentals (e.g., campaign contributions) before applying the elections shock. Clearly a firm's decision to make campaign donations is endogenous, consequently challenging randomness in these election studies.

reporting or are simply inattentive to reporting quality, anti-corruption promulgations can be expected to constrain these problems, hence improving the quality of accounting information. Whether these anti-corruption promulgations affect accounting quality is ultimately an empirical question, which I attempt to explore in this study.

To provide causal evidence on the PC-earnings quality link, I begin by retrieving current headquarter (HQ) locations (city and state) and financial data for all Compustat U.S. firms from the first fiscal quarter of 2010 ("2010Q1") to the second fiscal quarter of 2018 ("2018Q2"). Using the GeoNames geographical database to identify firms' originating counties, I assign firms to treatment and control groups based on whether their originating township, city, county, or state adopts an AA-CA-based measure, resolution, ordinance, or law. This process yields a sample of 3,148 firms (from 50 states) reporting 60,451 firm-quarter financials. To allow other tests (e.g., target beating, price efficiency), this sample reduces after merging with data from the Institutional Brokers' Estimate System (I/B/E/S) and the Center for Research in Security Prices (CRSP).

I utilize a difference-in-differences (hereafter, "DID") design to examine the causal effect of corporate political connectedness on financial reporting quality. Overall, the results reveal that, relative to firms headquartered in non-adopting locations, firms headquartered in adopting locations tend to have significantly higher earnings quality, as proxied by several measures of discretionary accruals estimated from traditional Jones-type accruals models. I then focus on a specific manifestation of EM (i.e., the managerial incentive to meet/beat earnings targets), where for instance, managers of PC firms can, at period end, avoid reporting certain non-cash political costs (e.g., in-kind gifts to existing LC politicians, or in-kind bribe payments to SC politicians) in order to raise earnings to meet/beat targets. For this case, I find that, relative to control firms, treatment firms are less likely to: (1) use income-increasing discretionary accruals to meet/beat analyst earnings forecasts, and (2) meet/beat analyst earnings targets by up to one cent. I then estimate a dynamic DID model and uncover that all the effects documented above only occur after (and not before) the adoption of AA-CA-based promulgations.

These findings are robust to using: (1) measures based on total accruals instead of discretionary accruals (to overcome concerns that accruals models suffer from type I and II error problems); (2) alternative DID specifications; (3) first-stage accruals model regressors as additional controls in second-stage accounting quality regressions (to overcome the potential misspecification concern identified in Chen, Hribar, and Melessa 2018); (4) potential omitted factors as additional controls; and (5) a stricter sample that reconstructs both treatment and control groups by excluding city- and county-level resolutions deferring ultimate legislative action to the state and/or Congress. In additional analysis where I proxy PC with campaign contributuions, I test the implicit assumption that these local laws constrain PC. Results show that these shocks directly curtail PC. I then provide systematic evidence that the accounting quality effects documented are due to the constraints that AA-CA-based laws impose on both LC and SC.

I also empirically test whether the ex-post improvement in accounting quality is priced by the capital market. Relative to control firms, the evidence shows that treatment firms' ex-post high earnings quality results in significant increases in stock returns. This finding is consistent with the prediction that the capital market positively prices high-quality accounting information. However, effects not linked to the improvement in earnings quality seem to be negatively associated with stock returns. One plausible interpretation is that, after AA-CA-based promulgations, the market punished LC firms with poor accounting quality for losing their connections with politicians, which would have generated certain political benefits. Alternatively, the market could simply be punishing treated firms for poorer earnings quality.

Given the ex-post enhancement in earnings quality, I then empirically test if, ex-post, this culminates in stock prices better capturing information about future earnings and cash flows. This test is similar in spirit to Fang et al.'s (2016) test of whether, after the event of exemption of short-sale uptick rules (or price tests), stock prices impound high-quality information about future earnings. Using the future earnings response coefficient (FERC) design, I find evidence consistent with my conjecture. Collectively, the finding of capital market consequences provide useful insights: the capital market rewards (punishes) treatment firms for reporting high- (low-) quality earnings, and that stock prices of treatment firms better impound ex-post high-quality information about future earnings and cash flows.

This study contributes to the literature in the following ways. First, and most importantly, it generally contributes to the PC literature by explaining corporate behavior with a plausibly exogenous shock to PC that, to my knowledge, has not been previously explored. As Faccio (2016) rightly points out, studies exploiting exogeneity have mostly used connections arising from elections (e.g., Houston et al. 2014; Akey 2015; Baloria and Klassen 2018). Faccio also points out that "other (presumably exogenous) shocks to connections might arise because of the death of some politicians or connected directors, resignations from office, et cetera." (p.118). In this study, I use a new, plausibly exogenous shock (i.e., AA-CA-based promulgations) to provide causal evidence on the effect of PC on corporate decision-making, focusing particularly on financial reporting quality.

Second, this study contributes to literature on the determinants of target meeting/beating using income-increasing tools (see, e.g., Davis, Soo, and Trompeter 2009; Doyle, Jennings, and Soliman 2013). Prior studies have mostly focused on *firm-level characteristics* (e.g., profitability, firm size, leverage, cash flows, sales growth, growth prospects, auditor identity and tenure, and analysts' coverage and their earnings forecast properties). While these determinants only provide evidence of associations, as they are subject to several endogeneity concerns, my study makes an attempt to provide causal evidence based on effects caused by a plausibly exogenous public policy.

Third, this study also examines a novel PC consequence: accruals-based target meeting/beating. Prior research has documented numerous PC consequences, including effects on earnings quality (Chaney et al. 2011); tax aggressiveness/management (Kim and Zhang 2016; Baloria and Klassen 2018); firm value (Faccio 2006; Goldman, Rocholl, and So 2009; Cooper, Gulen, and Ovtchinnikov 2010; Hadani and Schuler 2013; Akey 2015; Hung, Wong, and Zhang 2015); cost of debt (Houston et al. 2014; Chaney et al. 2011); auditor choice (Guedhami, Pittman, and Saffar 2013); IPO audit fees, market share, and client rejection risk (Yang 2013); corporate risk-taking (Boubakri et al. 2013); corporate bailouts (Faccio, Masulis, and McConnell 2006); analyst forecast accuracy (Chen et al. 2010); analyst stock recommendations (Christensen, Mikhail, Walther, and Wellman 2016); regulatory enforcement actions (Correia 2014); negative information suppression (Piotroski, Wong, and Zhang 2015); and political uncertainty (Wellman 2017). Different from the above-mentioned consequences, I show that PC can also affect the corporate use of income-increasing accruals to meet/beat analyst earnings forecasts.

Fourth, related to the preceding paragraph, prior studies examining PC consequences have largely focused on connections that are *observable* and perfectly *legal*. For example, prior studies have identified connections from corporate campaign contributions (Cooper et al. 2010; Ramanna and Roychowdhury 2010; Kim and Zhang 2016; Baloria and Klassen 2018), lobbying efforts and spending (Kim and Zhang 2016), and appointment of politicians or connected directors (e.g., Faccio 2006; Faccio et al. 2006). In my study, I bring a perspective that additionally focuses on connections that are made in secret, which I call *illegal connections*. Using the setting of AA-CA-based promulgations, which aim to constrain both observable and secret connections, I provide systematic evidence showing that illegal connections are distortionary (Shleifer and Vishny 1993).

While this study is closely related to Chaney et al. (2011), it differs in certain respects. Specifically, whereas Chaney et al. use the *cross-country setting* to document an *association* between LC and accounting quality, this study uses the *U.S. setting* to provide *causal evidence* on the effect of both LC and SC on accounting quality. This work is also related in a certain sense to Ramanna and Roychowdhury (2010). The authors use the setting of closely watched U.S. congressional elections to document an association between LC firms' outsourcing activities and *downward earnings management*.[10] Whereas the authors examine *one type of incentive* (i.e., concerns about potential negative political scrutiny of firms' outsourcing-related job cuts) and focus on one connection type (i.e., LC), this study focuses on two connection types (i.e., LC and SC) and *composite managerial incentives* (as reflected in the variety of *non-directional and upward* EM proxies used).

The rest of the paper proceeds as follows: section II presents the institutional background and formulates hypotheses; section III details the methodology; section IV discusses the results; and section V concludes.

II. INSTITUTIONAL BACKGROUND AND HYPOTHESES DEVELOPMENT

The American Anti-Corruption Act

In 2011, the American Anti-Corruption Act (AA-CA)—a model legislation—was crafted by former Federal Election Commission chairman Trevor Potter after having consulted with several "strategists, democracy reform leaders and constitutional attorneys from across the

[10] Hereafter, "earnings management" is denoted as "EM."

political spectrum."[11] The official website of the AA-CA declares three main objectives: "(1) *stop political bribery* so special interests can't use job offers and donations to influence politicians; (2) *end secret money* so people know who's buying political power; (3) *fix our broken elections* so the people, not the political establishment, are the ones in control."[12]

RepresentUs, which has members across the U.S., is the campaign movement behind the AA-CA agenda. Several resolutions already passed across the U.S. have expressly referenced RepresentUs as pushing for the passing of Anti-Corruption Acts in cities/states and federally. A unique feature of the AA-CA is that each Act is: (1) "uniquely tailored to meet the needs of locales across the country,"[13] and (2) "crafted in consultation with local political and legal experts to ensure they meet the specific needs of each individual community" (see AA-CA website).

As of October 6, 2018, RepresentUs has worked closely with townships, cities, counties, and states to pass "more than 80 Anti-Corruption Acts and Resolutions in cities and states across the country."[14] For example, a number of states (i.e., South Dakota, Ohio, Maine, and Alaska) have passed state-wide anti-corruption laws encapsulating the AA-CA, while several other cities and counties (e.g., San Francisco, California; Miami-Dade County, Florida; Portland, Oregon; etc.) have also passed measures, resolutions, and ordinances in pursuit of the AA-CA agenda.[15] These already passed promulgations sought to, *inter alia*, ban gifts from lobbyists, require registration fees and more frequent reports from "expenditure lobbyists," lower campaign donation thresholds,

[11] See AA-CA website FAQ section (https://anticorruptionact.org/faq/)
[12] See AA-CA website (https://anti-corruptionact.org/).
[13] See the September 26, 2016 resolution passed by the city of Southfield, Michigan ("Joint Resolution in Support of The American Anti-Corruption Act"): available at RepresentUs website (https://represent.us/our-wins/ or https://represent.us/our-wins-old/).
[14] See RepresentUs website.
[15] According to RepresentUs, "Conservatives and progressives worked together to pass America's *first citywide* Anti-Corruption Act in Tallahassee, Florida in 2014, and in 2016, RepresentUs members passed the *first statewide* Act in South Dakota" (see https://represent.us/about/).

require disclosure of the true funders of political ads, require lobbyists and political consultants to disclose their political services, and constrain gerrymandering.

These legislative changes impose significant costs on PC firms that more than offset potential gains from affiliations with politicians. Ultimately, this discourages firms from establishing or maintaining connections with government representatives, thus suggesting that, in terms of impact, AA-CA-based promulgations compel an exogenous decline in PC. There is anecdotal support that AA-CA-based laws could have a real impact on PC. For instance, in a New York Times article dated December 19th 2012, it was stated that "Republicans should embrace legislation like the proposed American Anti-Corruption Act, which would rid both parties (i.e., Democrats and Republicans) of their dependence on big money from groups like the N.R.A.," who, via their contributions, expect politicians "to oppose any laws that regulate guns." I will explain in detail how I utilize the AA-CA setting to conduct my experiment in section III, while later providing empirical evidence on the effects of AA-CA-based laws in section IV.

Local Laws in the U.S.

This subsection will provide general definitions and explanations of the key legal terminologies used in this paper. In the U.S., local laws include city and county charters, ordinances, and codes (Egler 2001). Cities and counties are granted local administrative powers by their state's constitution. A charter is the city's or county's own "constitution," defining its terms of existence and providing the foundation for all other regulations. Due to the difference in systems of administration across the U.S., a charter can be either a general law charter or home rule charter. Egler (2001, p.1) clearly explains this distinction:

> "Cities and counties that operate under the procedures set forth in the laws and constitution of their state have a general law charter. General law charters are created by and are consistent with the state law. State constitutions also give cities and counties the ability to write and adopt their own

charter. These are home rule charters… Typically, a majority of the population in a city or county must vote in favor of a home rule charter in order for the charter to be adopted. Once the home rule charter is adopted, it becomes the governing law of the city or county."

Measures and resolutions passed and adopted on obtaining majority votes from constituents, such as those based on the AA-CA, are consequently akin to home rule charters. Measures or simply "ballot measures" are pieces of proposed legislation, often in the form of propositions or questions that eligible voters can vote to approve or reject (Matsusaka 2004). In a legal opinion to the city council, the Missoula City Attorney's Office (Montana) defines a resolution as follows:

> "a resolution is a formal expression of the opinion or will of an official municipal body adopted by a vote. Pursuant to Montana statute, a resolution may be a statement of policy by the municipal governing body (city council) or an order of the municipal governing body that a specific action be taken. Resolutions go into effect immediately upon adoption."[16]

Ordinances, as another type of local law, are created by cities and counties to regulate matters of local concern (e.g., local law enforcement, local parks, animal control, local roads, etc.; Egler 2001). Again, cities' and counties' power to legislate is strictly confined by what is granted under state constitutions or laws. Just as for states, city and county codes are, essentially, codified ordinances arranged in a certain subject order to make the jurisdiction's laws more easily accessible (Egler 2001).

Hypotheses Development

Legal Connections and Accounting Quality

Ex-ante, it is unclear how LC affects accounting quality. This is because existing theories take opposing stances on the link between them. Under the first stream of thought, by their very nature, firms with observable connections to government representatives (present or past) tend to

[16] For a copy of this legal opinion, see ftp://ftp.ci.missoula.mt.us/Documents/Attorney/Opinions/062002.pdf (last accessed October 7, 2018).

attract public scrutiny, including substantial press coverage, that fosters better corporate governance practices (Chaney et al. 2011). For example, Fich and Shivdasani (2006) extensively discuss the significant public scrutiny surrounding government official Elaine Chao's directorship at six firms (C.R. Bard, Clorox, Columbia/HCA Healthcare, Dole Foods, Northwest Airlines, and Protective Life).[17] When appointed to President-elect George W. Bush's cabinet, pressure from public scrutiny (encompassing the media, shareholder activists, institutional investors, and regulators)—advocating effective corporate governance among firms—forced Chao to vacate her directorships.[18] Scrutiny and monitoring by the public thus plays a disciplining role in corporate governance. Consequently, under this setup, I expect effective governance associated with LC firms to constrain managerial incentives to opportunistically report earnings. Consequently, compared to less-scrutinized unconnected firms, I expect highly scrutinized LC firms to report relatively high-quality accounting information (Hypothesis I).

On the other hand, two other streams of thought predict a negative association between LC and accounting quality (Hypothesis II). In the first scenario, as suggested in all anecdotes discussed so far, LC firms tend to benefit from their affiliations with government officials. Aside from the more obvious and publicly observable benefits accruing from connections with politicians (e.g., bailout protection in times of financial distress [Faccio et al. 2006; Boubakri et al. 2013]; trade protection [Zimmerman 1983; Baloria and Klassen 2018]), positive interactions with politicians could bring other benefits that are more subject to LC firms' reporting discretion. Examples include lobbying-induced government subsidies, gains from preferential access to government

[17] Establishing PC by appointing government officials to boards is well explored in prior studies (see, e.g., Goldman et al. 2009; Chaney et al. 2011; Kim and Zhang 2016).
[18] It should be noted that she remained connected to these firms via her "not too remote" previous appointments (Faccio 2006), meaning they could still benefit from this affiliation. As Fich and Shivdasani (2006, p.690) clearly explain, "investors might expect the six firms, whose boards Ms. Chao vacated, to benefit from her new political influence."

contracts and operating licenses/permits (Jarrell 1979; Zimmerman 1983; Jones 1991; Mills, Nutter, and Schwab 2013; Smith 2016; Baloria and Klassen 2018), or even savings from averting potential government intrusions (Watts and Zimmerman 1978). Managers with empire-building incentives—intending to gain at investors' expense (Schipper 1989; Leuz, Nanda, and Wysocki 2003)—may use their discretion over financial reporting to delay or, at worst, intentionally conceal the reporting of such political benefits (Chaney et al. 2011). Consequently, the earnings quality of LC firms will be poorer than that of unconnected firms, all else equal.

In the second scenario, there is a long-held view in the literature that connected public officials tend to shield PC firms from potentially adverse consequences (see, e.g., Dean et al. 1998; Faccio et al. 2006; Chen et al. 2010; Boubakri et al. 2013; Houston et al. 2014; Kim and Zhang 2016), and there appears to be some anecdotal support for this view. For instance, a 2012 *Boston Globe* article reported then-Senate candidate Elizabeth Warren (Democrat) describing her (Republican) opponent's ties to big businesses as follows: "Wall Street is investing in Scott Brown because they know he will protect them" (Warren 2012; Baloria and Klassen 2018). Given this potential government-provided protection, managers of LC firms could be emboldened to engage in opportunistic reporting in pursuit of their personal interests, or could simply be inattentive to the quality of accounting information (Chaney et al. 2011). Consequently, relative to unconnected firms, LC firms are more likely to report poorer quality accounting information.

Illegal Connections and Accounting Quality

A fundamental reason as to why SCs are unobservable is because such connections are shrouded in "secrecy" (Shleifer and Vishny 1993, 1994). This is in sharp contrast to legally disclosed (and publicly observed) connections established through a variety of means, such as through the appointment of politicians to boards, corporate campaign donations, or corporate lobbying spending. Hints about the existence of SCs therefore only come to the fore when firms and politicians are implicated in political corruption scandals (Smith 2016), which usually occur long after the connections were first established, thus making such connections unobservable at inception. Establishing connections with politicians in secret amounts to collusion because such connections would not happen in the first place if ulterior motives were non-existent. This assertion is supported by Shleifer and Vishny (1993), who theorize the incidence of firms "secretly" colluding with politicians, with both parties expecting certain gains. SC usually thrives on a veiled *quid pro quo*, with colluding managers often sacrificing liquidity (in the form of payment of bribes at the expense of shareholders) in exchange for certain "undeserved" favors from politicians (Smith 2016). Shleifer and Vishny (1993) are unequivocal about this tendency: "government officials often collect bribes for providing permits and licenses, for giving passage through customs, or for prohibiting the entry of competitors" (p.599). The several anecdotes discussed earlier on support this assertion and show how illegal exchanges take place in the political system.

The fact that managers hide such connections potentially creates additional incentives to hide any other thing (e.g., political benefits) resulting from these connections. Insiders generally tend to have incentives to hide their private control benefits from outsiders because of the potential disciplinary action that outsiders are likely to take against them if these benefits are detected (see, e.g., Zingales 1994; Shleifer and Vishny 1997; Leuz et al. 2003).[19] Existing works such as Chaney et al. (2011) posit and show a likely outcome of the managerial incentive to hide political benefits: connected firms distort the quality of reported earnings if they intentionally conceal political benefits from investors. This reasoning aligns with Shleifer and Vishny's (1993) view that the need

[19] In this case, I assume that investors and other stakeholders (e.g., regulators) can discern SCs from firms' reporting of political benefits derived from secret affiliations with government representatives. Managers of SC firms therefore decide to conceal such political benefits as a way of protecting themselves and their colluding counterparts (i.e., politicians) from potential disciplinary action, such as criminal conviction by a court of law, et cetera (see anecdotes).

for secrecy makes SCs "distortionary" and therefore detrimental to economic outcomes, which in my study would be reported earnings. In my setting, both upward and downward EM are plausible outcomes of hiding secret political affiliations. For example, concealing non-cash bribe payments (e.g., giving politicians firms' products for free) [a political cost] and not reporting this would result in upward EM. Similarly, if SC firms receive "undeserved" subsidies from politicians, or if through lobbying or warding off of potential government intrusion, they make savings from funds previously dedicated to payment of fines (a political benefit) and do not report this, there would be downward EM. All of the above arguments point to the suggestion that SC firms are more likely to report low-quality earnings relative to unconnected firms, ceteris paribus.

Further, the promise to return political favor in the form of government-provided protection, which is an unobservable political benefit, could also embolden managers of SC firms to be generally opportunistic in their financial reporting; it could also cause them to be inattentive to the quality of their reporting (Chaney et al. 2011). These latter arguments suggest, anew, that, relative to unconnected firms, SC firms are more likely to have poorer accounting quality. Because all of the predictions for the link between SC and accounting quality point to the same direction, I label both as one directional prediction: Hypotheses III.

Accounting Quality under AA-CA-Based Promulgations

With regard to how an exogenous decline in both LC and SC (induced by AA-CA-based promulgations) affects accounting quality under Hypotheses I, II and III, I argue as follows. Under Hypothesis I (hereafter, "H_I"), since the corporate governance effects from public scrutiny already constrain LC firms' opportunistic reporting tendencies, thereby inducing a positive relationship between LC and earnings quality, a further disciplining effect from AA-CA-based promulgations would be inconsequential; that is, it would not affect accounting quality. This is because, before the anti-corruption promulgations, these LC firms were already reporting high-quality earnings. Thus, under H_I, I do not expect an exogenous decline in LC to impact accounting quality.

Because Hypotheses II and III (hereafter, "H_{II}" and "H_{III}") are respectively about LC and SC and both hypotheses make the same directional prediction about how PC affects accounting quality, I will therefore consolidate as one, the separate predictions under LC and SC. Under both hypotheses, the managerial incentive for PC firms to opportunistically report earnings is pervasive. An exogenous decline in PC brought by the adoption of anti-corruption promulgations, and the consequential disciplining effect via criminalizing the purchase of political influence, would help constrain the opportunistic reporting problem associated with PC firms. Consequently, under H_{II} and H_{III}, I expect firms located in areas that adopt anti-corruption promulgations to subsequently enhance accounting quality relative to firms in non-adopting locations.

III. METHODOLOGY

Sample

To construct a sample to investigate the research question, I retrieved data on current HQ locations (city and state) for the entire universe of Compustat U.S. firms, as well as data on their corresponding financials for the period from 2010Q1 to 2018Q2. I then used the GeoNames geographical database to identify firms' originating counties.[20] With complete information on each

[20] GeoNames provides comprehensive data for places within cities, their postal codes, counties, and states in the U.S. (and similar data for other countries). To access this database, visit http://www.geonames.org/postal-codes/US/. Data for 9,896 places (based on postal codes) were sourced from this database to identify each Compustat firm's county location. Note that the "county" field in Compustat North America, although available for selection in the WRDS database, yields missing data for all U.S. firms: this is because only city and state names have available data. I also used WRDS to check the Compustat Legacy database (i.e., the version discontinued in 2006), but this database is limited to county locations identified by Federal Information Processing Standard (FIPS) codes. Besides, in the corresponding Compustat Legacy data manual, only a limited number of cities are coded by Compustat under counties. Consequently, a large number of firms' county locations cannot be identified using the Compustat Legacy database.

firm's HQ location (city, county, and state), I assigned firms to treatment and control groups based on whether their originating township, city, county, or state adopts an AA-CA-based promulgation.

Appendix A shows the full list of twenty (20) different locations that *actually* adopted AA-CA-based promulgations during the 2014-2017 period.[21,22] From the original list of 64 locations claimed by RepresentUs as anti-corruption breakthroughs (available on their website), I select locations that: (I) have *locally* passed their own anti-corruption measures, ordinances, or laws modeled on the AA-CA; or (II) have stated in an official community resolution that the AA-CA has been tailored for their locality and that they support or will enforce the AA-CA or any related anti-corruption measure as part of their own legislation, while also looking to the state and/or U.S. Congress to pass ultimate anti-corruption legislation.[23,24] Many of the original 64 anti-corruption promulgations did not satisfy these criteria, and so were excluded from my final list. Most of the excluded locations were cities or counties that did not themselves adopt any local anti-corruption measure, but rather passed resolutions urging their states and/or Congress to enact tough anti-corruption laws. Examples include Wyoming County (Pennsylvania), city of Ferndale (Michigan), and city of Princeton (New Jersey).[25]

The 20 selected locations comprise one township (Ewing Township, New Jersey), ten cities (e.g., Seattle, Washington; San Francisco, California), eight counties (e.g., Dekalb County, Illinois;

[21] San Francisco is listed twice as it first passed an "expenditure lobbyist" measure before later adopting a more severe stance against lobbying by banning gifts from lobbyists. My analysis focuses on the "bans" event because it constitutes much stricter anti-corruption legislation.
[22] Though 2018 events are also available, I only use 2014-2017 events because: (1) the first year of adoption is 2014; and (2) a DID design requires pre and post periods, which I achieve by focusing on quarterly reporting intervals.
[23] I manually read these anti-corruption documents before determining the locations listed in Appendix A. I attach excerpts of one of these documents as Appendix B. To access other documents, simply click on the respective hyperlink for each location listed on RepresentUs' website (https://represent.us/our-wins-old/).
[24] For criterion II, where the resolution or measure does not explicitly state that the AA-CA has been locally tailored, the fact that it is listed on the website of RepresentUs as one of their successes is enough evidence to prove that it is AA-CA-based. This is because RepresentUS works with local governments to adopt local versions of the AA-CA.
[25] Copies of these resolutions are also available on the mentioned website.

Miami-Dade County, Florida), and one state (South Dakota). After applying the above selection procedure and also deleting financial firms (i.e., 6000 ≤ SIC ≤ 6999) from the initial Compustat sample, my final sample comprised 3,148 firms (from 50 states) reporting 60,451 firm-quarter financial reports.[26,27] The distribution of the final sample across states (reported as Table 1) is similar to those reported in prior studies using location-level analysis (see, e.g., Butler, Fauver, and Mortal 2009; Smith 2016). For instance, similar to these studies, states such as Alaska, Delaware, Hawaii, Maine, Montana, North Dakota, South Dakota, Vermont, West Virginia and Wyoming have low corporate HQ presence, whereas states such as California, Florida, Illinois, Massachusetts, Minnesota, New Jersey, New York and Texas have high corporate HQ presence. To conduct target beating and capital market tests, I further merged the final sample with data from I/B/E/S and CRSP, which shrinks the size of the sample used for these tests.

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[26] Whereas most prior studies (including those on the determinants of earnings quality) use *annually* reported financials, I use *quarterly* financials. Some variables, such as those calculated from "volatile" stock prices (e.g., *MARKET_TO_BOOK*) and "potentially cyclical" operating activities (e.g., *CASHFLOW_VOL*), are likely to fluctuate substantially during the course of a year. Outliers are, therefore, more likely for such variables in the *quarterly* setting. To ensure this is not an issue, I reviewed the distribution of these variables. Having found certain quarterly "flare-ups," I elected to winsorize such variables at the 1% and 99% level to remove the potential impact of outliers on estimations. I then checked the distribution of all the remaining variables used in the study and they appear "normal" when compared to prior studies (e.g., Dechow and Dichev 2002; Roychowdhury 2006; Yun 2008; Bergstresser and Philippon 2006; Chaney et al. 2011; Smith 2016; Collins, Pungaliya, and Vijh 2017; Baloria and Klassen 2018).

I did not mechanically winsorize all variables because the goal is to remove the impact of outliers without generating distributions that differ substantially from those in prior studies. This treatment is similar to studies excluding outliers "selectively." For instance, Kothari (2001, p.154) points out how previous studies differ in the selective treatment of outliers. More recently, Cen, Maydew, Zhang, and Zuo (2017), as one of many studies, removed outliers for only certain (but not all) variables to make those distributions look "normal."

Notwithstanding the above, in unreported tests using the raw sample without removing these outliers, I found qualitatively similar results.

[27] This sample is the intersection of the main variables of interest required for the baseline earnings quality test. Financial firms are deleted because "working capital is less meaningful for these firms" (Dechow, Hutton, Kim, and Sloan 2012, p.291).

Research Design

To investigate the research question, I employ the following DID model for the staggered-events setting involving multiple treatment groups: [28]

$$AQ_{i,t} = \alpha_0 + \alpha_1\, TREATED \times POST_{l,t} + \sum_{j\geq 2} \alpha_j\, CONTROLS_{i/l,t} + LOC.FE + TIME\ FE + \varepsilon_{i,t} \quad (1)$$

The subscripts i, l, and t respectively refer to reporting firms, HQ locations, and fiscal periods (i.e., year-quarters). The dependent variable AQ is a vector of non-directional measures of accruals proxying for the quality of accounting information. Specifically, I use the following measures: (1) volatility of discretionary accruals, defined as the 5-quarter-rolling-window standard deviation of discretionary accruals, estimated from two separate Jones-type accruals models (i.e., the Jones model [Dechow, Sloan, and Sweeney 1995; Chaney et al. 2011] and the performance-adjusted modified Jones model [Chaney et al. 2011; Kothari, Leone, and Wasley 2005; Collins et al. 2017]); and (2) absolute values of discretionary accruals, estimated from the above-mentioned separate accruals models (for the use of the absolute accruals measure, see Dechow and Dichev 2002; Bergstresser and Philippon 2006).[29]

Regarding the DID terms, $TREATED$ is a dummy equal to 1 if the township, city, county, or state of a firm's HQ adopts promulgations modeled on the AA-CA, and 0 otherwise. $POST$ is an indicator variable (specific only to firms with $TREATED$ = 1) that takes the value of 1 for all

[28] Note that the staggered-events setting used in this study differs somewhat from those used in prior studies, which focus on events occurring in the same type of location (i.e., only at either *state*-level or *country*-level). By contrast, my setting considers a mix of *township*-, *city*-, *county*-, and *state*-level events.

[29] For the sake of brevity in the manuscript, detailed definitions and labels of these measures are presented in Appendix C. Note that these models omit the property, plant, and equipment (PPE) term because this study focuses on "current (working capital) accruals," not "long-term accruals" (see, e.g., Chaney et al. 2011; Collins et al. 2017). Consistent with prior studies using the U.S. or another single-country setting, the models also omit the macro-level variables (i.e., inflation and GDP) used in Chaney et al.'s (2011) cross-country design. To provide confidence that the results are not purely due to model choice, I later use total accruals instead of discretionary accruals in estimations, which I report as a robustness test.

periods starting from the one in which an event date falls, and 0 otherwise (for a similar measurement, see Bertrand and Mullainathan 1999a, p.544).[30,31] The coefficient of $TREATED \times POST$ (i.e., α_1) captures the DID effect; that is, the difference between the cross-sectional difference (i.e., accounting quality of treated firms minus that of control firms) and time series difference (i.e., accounting quality in post-periods minus that in pre-periods). α_1 is the variable of interest for testing the hypotheses on how accounting quality is impacted by the staggered adoption of anti-corruption promulgations. Following prior staggered-events studies employing either a DID or alternative designs (see, e.g., Bertrand et al. 2004; Kim, Liu, and Zheng 2012;[32] Jayaraman and Shivakumar 2013; Christensen, Hail, and Leuz 2013; Daske, Hail, Leuz, and Verdi 2013), I control for fixed differences between treatment and control groups using location fixed effects, which I label as $LOC.FE$.[33] Albeit different location types (i.e., mainly cities, counties, and a state) adopt the AA-CA-based promulgations, I choose as location fixed effects, county fixed effects rather than city or state fixed effects, because the AA-CA-based promulgations adopted by

[30] Note that the passage of these promulgations are staggered events with several event dates (i.e., not a single event occurring on one particular date), so all control firms are automatically assigned $POST$ = 0 (see Jayaraman and Shivakumar 2013, p.104).

[31] An important feature of this study's DID design is that $TREATED \times POST$ captures an effect that is the same as simply defining a single DID effect variable, as in prior staggered-event studies (see, e.g., Bertrand and Mullainathan 2003; Armstrong et al. 2012; Fauver et al. 2017). Therefore, while $TREATED$ strictly identifies control groups as firms in locations that never passed laws, $TREATED \times POST$ does not restrict control groups to firms in locations that never passed laws. Instead, the control group includes all firms in locations not passing laws at time t, even if their locations have already passed laws or will pass them at some time after time t (for a discussion on this, see Bertrand and Mullainathan 2003, p.1056; Armstrong et al. 2012, p.192). The way $POST$ is defined, when interacted with $TREATED$, makes certain of this. On the way $POST$ is defined in a staggered-event setting, Bertrand and Mullainathan (1999a) corroborate as follows: "many laws were passed and at different times. By defining an "After Law" dummy firm by firm, we can easily allow for the staggering of laws" (p.544).

[32] I include Kim et al. (2012) among studies using the staggered-events setting because they use IFRS adoption in both 2005 and 2006 to identify Worldscope IFRS adopters: "Adoption year t in this study is 2005 (2006) for firms with a December (non-December) fiscal year-end." (p.2063).

[33] In the staggered-events setting, studies propose alternative approaches to control for fixed differences between treatment and control groups. For approaches employing *location fixed effects*, see, for example, Bertrand et al. (2004), Kim et al. (2012), Jayaraman and Shivakumar (2013), Christensen et al. (2013), and Daske et al. (2013). For approaches employing *firm fixed effects*, see, for example, Bertrand and Mullainathan (1999a, b, 2003), Armstrong et al. (2012), Jayaraman and Shivakumar (2013), and Fauver et al. (2017). While I use location fixed effects in the baseline model, in additional analysis, I later use other alternative DID designs including one that employs firm fixed effects.

multiple cities within a county are mostly identical whereas those adopted in other counties differ in substance.[34] $TIME\ FE$ (i.e., year × quarter effects) as specified in Eq. (1) is intended to control for aggregate fluctuations (see Bertrand and Mullainathan 1999a, b, 2003; Bertrand et al. 2004). In estimating Eq. (1), I cluster at the state × year-quarter level to estimate heteroskedasticity-adjusted standard errors.

$CONTROLS$ (i.e., control variables) in Eq. (1) comprises an array of firm- and location-level determinants of discretionary accruals taken from prior studies (e.g., Chaney et al. 2011; Fang et al. 2016). Specifically, I include the following: (i) *operating risk*, proxied by volatility of operating cash flows (*CASHFLOW_VOL*), volatility of sales (*SALES_VOL*), and volatility of sales growth (*ΔSALES_VOL*); (ii) *closeness to debt covenant violation* (proxied by *LEVERAGE*); (iii) *firm size* (proxied by *SIZE_MV*); (iv) *growth prospects* (proxied by *MARKET_TO_BOOK*); (v) *financial distress* (proxied by *RFIN_HEALTH_PCA*); and (vi) *corruption* (proxied by *STATE_INTEGRITY*). For conciseness in the manuscript, full definitions for these variables are given in Appendix C.

I make the following predictions on the relationship between these control variables and earnings quality. i) *Operating risk*: firms with larger earnings volatility tend to have high capital costs (for both equity and debt; Minton and Schrand 1999). Such (risky) firms are likely to increase their use of abnormal accruals to mitigate their perceived risk (Warfield, Wild, and Wild 1995) or to engage in earnings smoothing to reduce their cost of equity capital (Bowen, Rajgopal, and Venkatachalam 2008). I thus expect a positive coefficient on all three operating risk measures

[34] In fact, within the same county, multiple city-level AA-CA-based resolutions and measures collated on the official website of RepresentUs reveal substantial similarity. As a result, with respect to county-level promulgations, only firms headquartered in adopting counties will be similarly affected. This notwithstanding, in untabulated results (available upon request), I later use a more granular form of location fixed effects (i.e., city fixed effects) and find qualitatively similar results.

(*CASHFLOW_VOL*, *SALES_VOL*, and Δ*SALES_VOL*). (ii) *Closeness to debt covenant violation*: firms with more debt tend to manage earnings to avoid the costly violation of debt covenants (Dichev and Skinner 2002), so I expect a positive coefficient on *LEVERAGE*. (iii) *Firm size*: large companies tend to face greater political costs (Watts and Zimmerman 1985; Jones 1991), so I expect large firms to have poorer earnings quality (i.e., a positive coefficient on size_mv). On the other hand, firm size is often used to proxy for a more transparent information environment (e.g., Lin, Ma, Malatesta, and Xuan 2013). In this case, one would expect a negative coefficient on *SIZE_MV*. (iv) *Growth prospects*: the market gravely punishes growth firms (*higher market-to-book*) in the presence of negative earnings surprises (Skinner and Sloan 2002), so growth firms are strongly incentivized to meet earnings targets by manipulating reported earnings. I thus expect a positive coefficient on *MARKET_TO_BOOK*. (v) *Financial distress*: earnings quality declines as default risk rises for equity stocks (Plummer and Tse 1999). As a firm's financial strength surges, earnings movements and resultant earnings informativeness (earnings quality) become very important to equity owners, since their rewards are directly tied to firm value. Consequently, I expect a positive coefficient on *RFIN_HEALTH_PCA*. (vi) *Corruption*: firms located in areas with weak institutions tend to have lower earnings quality (Leuz et al. 2003), so I expect a positive coefficient on *STATE_INTEGRITY*. On the contrary, it is also possible that places where corruption is high could demand high transparency, consequently inducing a negative association between *STATE_INTEGRITY* and proxies of accounting quality.

IV. RESULTS

Descriptive Statistics

Table 2 reports the summary statistics of key variables specified in my model. It shows that 4.5 percent of firm-quarters in the sample are in the treatment group. If we view firms in the treatment group as those potentially losing connections with politicians, then the percentage of firms affected by AA-CA-based promulgations is similar to the percentage of PC firms reported in prior studies (see, for example, Faccio 2006, p.374; Chaney et al. 2011, p.63).

Regarding the measures of earnings quality, summary statistics of all proxies are comparable to those reported in prior studies. For instance, the mean values of the two variables proxying volatility of discretionary accruals (i.e., 6.66 percent and 6.94 percent of assets, respectively) are comparable with the volatility measures reported in Chaney et al. (2011, p.63). Also, the mean values of the absolute accruals measures (i.e., both discretionary and raw accruals), ranging between 4.41 percent and 5.03 percent of assets, are also similar to the average absolute change in working capital values in Dechow and Dichev (2002, p.48) and the mean absolute accruals values in Bergstresser and Philippon (2006, p.517).[35]

Summary statistics for several control variables (e.g., cashflow_vol, sales_vol, size_mv, leverage, market_to_book) are also within a range of values consistent with prior studies (e.g., Dechow and Dichev 2002; Roychowdhury 2006; Bergstresser and Philippon 2006; Yun 2008; Lin et al. 2013; Smith 2016; Baloria and Klassen 2018). Table 3 shows the Pearson correlation between each pair of control variables used in this study. The results indicate that, overall, there are no severe correlations among variables included side-by-side in the three distinct multivariate regressions (i.e., the earnings quality, target beating, and price efficiency models).

Primary Findings

Table 4 reports the results of DID tests examining the causal effect of corporate political connectedness on financial reporting quality. Columns 1-2 respectively present results for the two

[35] Note that "change in working capital" (Dechow and Dichev 2002) is synonymous with "working capital accruals," and "current accruals" (Dechow et al. 2012; Collins et al. 2017). The accruals definition used in this study follows Collins et al.'s (2017) definition in the quarterly setting.

volatility measures of discretionary accruals estimated from the Jones model (*DCA_J_VOL*) and modified Jones model (*DCA_MJ_VOL*). Columns 3-4 present results for the two absolute value measures of discretionary accruals estimated from these models, respectively proxied with *ABS_DCA_J* and *ABS_DCA_MJ*.[36] Recall that the DID effect in Eq. (1) is the coefficient on the interaction term $TREATED \times POST$. Interestingly, the coefficient reported in each of the four columns is negative and significant at the 5% level. These results imply that, relative to firms with HQs in non-adopting locations, earnings quality increases significantly from the pre- to the post-adoption period for treated firms headquartered in adopting locations. In economic terms, this DID effect translates to a decline in discretionary accruals of between 4.84 and 6.87 percent of assets. This finding is thus consistent with H_{II} and $H_{III,}$ which posits that AA-CA-based promulgations constrain managerial opportunistic reporting tendencies.

All control variables have the predicted signs. Specifically, consistent with expectations, I find that firms with high operating risks, high leverage, small size, high growth prospects, and high financial distress tend to have low-quality earnings. The negative coefficient on the corruption measure is also consistent with the earlier argument that places where political corruption is high could demand more transparency.

Given the *ex-post* decrease in use of the accruals tool to manipulate earnings, one would also expect a decline in managers' use of income-increasing accruals to meet/beat certain earnings targets (e.g., analysts' earnings forecasts) after the passage of anti-corruption promulgations. Theory suggests that firms that miss (beat) earnings targets are gravely punished (rewarded) by the stock market (Skinner and Sloan 2002; Bhojraj, Hribar, Picconi, and McInnis 2009), leading some firms to use income-increasing tools (e.g., positive discretionary accruals [Davis et al. 2009;

[36] Please see Appendix C for detailed definition of these measures.

Bhojraj et al. 2009], opportunistically-defined non-GAAP earnings [Doyle et al. 2013]) to meet/beat these earnings targets.[37] Recall that, as I previously argued, managers of PC firms can, at period end, avoid reporting certain non-cash political costs (e.g., in-kind gifts to existing LC politicians, or in-kind bribe payments to SC politicians) in order to raise earnings to meet/beat targets. I proxy this target meeting/beating phenomenon with variables that capture firms' tendency to use income-increasing discretionary accruals to meet/beat analysts' earnings forecasts. Two measurements suffice here because income-increasing (i.e., positive) discretionary accruals are derived from residuals estimated from both the Jones and modified Jones models, respectively: *MBE_DCA_J* (column 1) and *MBE_DCA_MJ* (column 2).[38] Tabulated results reveal that, relative to firms in non-adopting locations, firms in adopting locations are less likely to use income-increasing discretionary accruals to meet/beat analysts' earnings forecasts.[39] Specifically, in economic terms, the odds of treated firms using income-increasing accruals to meet/beat analysts' earnings forecasts is about 22.59 to 25.92 percent lower than for control firms.[40]

Further, following Fang et al. (2016), I employ a pure target beating measure that is widely used as a proxy for EM. As Fang et al. explain, "many papers (e.g., Bhojraj et al. 2009) infer earnings management from the tendency of firms to meet or beat the analyst consensus by up to one cent" (p.1270). I proxy this phenomenon with *MBE_1_CENT*, which I define as an indicator

[37] To the extent that some PC studies show that the capital market responds negatively to PC (see, e.g., Hadani and Schuler 2013) and given further that the above-stated target beating argument posits the potential threat of adverse capital market response to target misses, it is therefore intriguing to examine the question of how an exogenous decline in PC affects target beating ex-post. This question is especially important because it could help identify the specific instances in which managers of PC firms care about investors' expectations about reported earnings.
[38] Please see Appendix C for the detailed definition of these measures.
[39] Introducing the perspective of meeting/beating analyst earnings into the analysis, I augmented Eq. (1) with additional controls (i.e., *FORSTD* and *N_ANALYSTS*). These variables are often used in studies on the determinants of target beating (see, e.g., Davis et al. 2009).
[40] Economic significance in a model that specifies both dependent and independent variables as dummies can be calculated as follows. Suppose that Y and X are the respective dependent and independent variables in the model $Y = a_1 + a_2 X + \varepsilon$. In this logistic model, the ratio of odds of the likelihood that Y=1 for X=1 to the odds of the likelihood that Y=1 for X=0 is e^{a2}.

equal to 1 if a firm meets/beats analysts' earnings per share (EPS) consensus forecasts by up to \$0.01, and 0 otherwise.[41] In column 3 of Table 5, I re-estimate the DID model to establish whether the preceding findings are also obtained using this alternative proxy. Consistent with expectations, I find that, compared to firms in non-adopting locations, the likelihood that firms in adopting locations meet/beat analysts' earnings forecasts by up to one cent decreases significantly from the pre to the post-adoption period.

Overall, the results from all estimations using multiple measures of discretionary accruals and several target beating proxies support H_{II} and $H_{III,}$ confirming that anti-corruption laws exert a disciplining effect on managerial opportunistic reporting behavior, thereby leading to an improvement in accounting information quality.

Dynamic Estimation

The analysis thus far shows that the passage of AA-CA-based promulgations causes a significant improvement in financial reporting quality. One may argue, however, that this finding could be possibly driven by a declining trend in accounting quality caused by some "non-remote" events occurring prior to the anti-corruption events (i.e., a violation of the parallel trend assumption in a DID setup). If this assertion is true, then one should observe a decline in accounting quality prior to the adoption of AA-CA-based laws. To test this, I follow prior studies using staggered events to conduct quasi-natural experiments (e.g., Bertrand and Mullainathan 2003; Armstrong et al. 2012; Fauver et al. 2017) by splitting the DID effect term (i.e.,$TREATED \times POST$) into the following four separate terms: $TREATED \times QTR\ T-1$; $TREATED \times QTR\ T$; $TREATED \times QTR\ T+1$; and $TREATED \times QTR\ T+2\ AND\ BEYOND$. These decompositions allow for examining dynamic effects, thus helping to provide insights about when the effects kick in.

[41] Please see Appendix C for the detailed definition of this measure.

Tables 6 and 7 report regression results of the dynamic model for measures of discretionary accruals and target beating, respectively. For the discretionary accruals tests in Table 6, I find that the coefficients on $TREATED \times QTR\ T+1$ are significant in all four columns, while those on both $TREATED \times QTR\ T$ and $TREATED \times QTR\ T+2\ AND\ BEYOND$ are insignificant in all four columns. This finding suggests that the effects are prominent only in the quarter following the passage of anti-corruption promulgations. However, for the target meeting/beating tests in Table 7, the effects seemed to manifest from the quarter in which the laws were passed and extend to subsequent quarters. Taken together, these results indicate there were no pre-trends in accounting quality, as the documented effects only occurred after the shock.

Robustness Checks

Model Issues

With respect to the measurement of discretionary accruals proxies, one could also argue that the study's Jones-type accruals models are potentially flawed: they are susceptible to concerns such as misspecification (excessive type I error rate) and low test power (excessive type II error rate) [Dechow et al. 2012]. To alleviate these concerns and to show that the results are neither ad hoc nor influenced by model choice, I use volatility and absolute value measurements based on "raw" accruals, instead of the earlier-used "modeled" accruals. Table 8 re-estimates Eq. (1) and tabulates results using the raw accruals measures. Column 1 (2) [3] documents results for the volatility (absolute value) [accruals-based target beating] measures of raw accruals. The results in these three columns suggest that my earlier inferences on how anti-corruption promulgations impact accounting quality do not change with model choice.

Relatedly, a recent work by Chen, Hribar and Melessa (2018) [hereafter, "CHM"] has raised some concerns that empirical approaches that transform residuals from accruals models (i.e., the first-stage) and use it as dependent variable in the second-stage without again controlling for all first-stage regressors could be misspecified, and thus bias the coefficients and standard errors of the second-stage regressors.[42] In my setting, this concern could potentially apply to estimations using the volatility and absolute value measures of discretionary accruals because these are direct transformations of the residuals estimated from accruals models—the first-stage. I therefore follow CHM's recommended solution to include the first-step regressors in the second-step regression (see p.783 of their paper).[43] Specifically, for each earnings quality measure, I re-estimate Eq. (1) by specifying $1/ASSETS_{t-1}$ and $(\Delta SALES_{t})/ASSETS_{t-1}$ as additional controls in models that base dependent variables on the Jones model, as well as specifying $1/ASSETS_{t-1}$, $(\Delta SALES_{t} - \Delta RECEIVABLES_{t})/ASSETS_{t-1}$, and ROA_{t} in models that base dependent variables on the performance-adjusted modified Jones model. In untabulated results (available upon request), I find that, after controlling for all first-stage regressors, results are qualitatively similar. These new results thus suggest that the biases identified by CHM are unlikely confounds to my findings.

Alternative DID Techniques

As explained earlier, prior studies exploiting staggered events via DID propose alternative approaches to control for fixed differences between treatment and control. To show that results are robust to several DID designs, I use two alternative approaches to control for fixed differences between experiment groups. While the primary DID model includes location fixed effects (via *county fixed effects*) to account for these fixed differences, in this subsection, I employ the following techniques: (1) a parsimonious treatment effect dummy ($TREATED$); and (2) firm fixed

[42] Note that the biases specifically analyzed in their paper do not explicitly apply to transformed residuals, such as absolute value or volatility of residuals (see Table 1 of their paper). Nonetheless, CHM caution that the issues and biases they analyze are likely to also apply to settings using transformed residuals, such as Eq. (1) in my study. I thus follow their recommendation to correct these potential biases.

[43] Note that because the dependent variables in Eq. (1) are transformed residuals (and not the raw accruals term) from the first-stage model estimated at the industry-time level, there is therefore no need to additionally control for industry-time dummies and their interactions with the first-stage regressors. Please refer to CHM's recommended solutions.

effects. To the extent that in quasi-natural experiments, firms classified in the treatment (control) group are, in a similar way, thought to be affected by (immune to) a shock, and given further that, in the staggered-events setting, fixed differences between treatment and control groups can be controlled for via either specifying firm or location fixed effects,[44] $TREATED$ (as used under approach [1]) can therefore be viewed as "a linear combination of firm (location) fixed effects" (Fauver et al. 2017, p.132) that also helps to estimate a *parsimonious* DID model.[45] Firm fixed effects, on the other hand, has other advantages albeit it makes a DID less parsimonious: they "help mitigate concern of correlated omitted variables" (Fauver et al. 2017, p. 126).

In Table 9, I re-estimate Eq. (1) by alternating county fixed effects with either the treatment effect dummy (i.e., $treated$) or firm fixed effects. The new estimations, reported for the measures of discretionary accruals and target beating, do not alter my economic inferences on the sign and magnitude of the effect of AA-CA-based promulgations on accounting quality.

Omitted Variables

Despite my efforts to control for a number of specific factors that relates to accounting quality (proxied by non-directional measures of accruals), there could be potential omitted factors that relate to accounting quality and/or PC. As a robustness check (unreported, but available upon request), I include some factors, including one that captures the effect of time-invariant unobservables. Specifying these new variables is additional to the potential correlated omitted firm-specific factors controlled for via firm fixed effects in the preceding subsection. In my new

[44] For instance, in their classic work recommending econometric solutions to DID estimation, Bertrand et al. (2004) use location (i.e., state) fixed effects to control for fixed differences between treatment and control groups (see p.267 of their paper). However, in other works involving the same authors, they use firm fixed effects (see Bertrand and Mullainathan 1999a, b, 2003). Subsequent studies following the authors' works have since used either of these alternative approaches (see, e.g., Armstrong et al. 2012; Jayaraman and Shivakumar 2013; Fauver et al. 2017).
[45] Note that because $TREATED$ captures similar effects as firm (location) fixed effects, specifying both $TREATED$ and firm (location) fixed effects will make the DID model "redundant" (Bertrand and Mullainathan 1999b, p.14; Low 2009).

estimations, I include: (i) industry fixed effects because some industries (e.g., coal, defense, and tobacco industries) are more likely to be politically connected (Kim and Zhang 2016) and their financial reporting behavior could be shaped by their political activities; and (ii) a fourth quarter indicator because prior studies have argued that the annual audit requirement usually occurs in the fourth quarter and tends to constrain managers' use of discretionary accruals (see, e.g., Brown and Pinello 2007; Das, Kim, and Patro 2011).[46] Results (untabulated for brevity, but available upon request) show that the inclusion of these additional controls does not drive away my earlier results.

Test of Implicit Assumption —Do AA-CA-Based Promulgations Constrain PC?

Throughout the paper, I relied on reasoning to argue that the constraints that AA-CA-based promulgations impose compel an exogenous decline in PC. Though intuitively sound, in this subsection, I make a formal attempt to validate this implicit assumption. To do this, I replace the accounting quality measures in Eq. (1) with a measure of LC, while also replacing location fixed effects with firm fixed effects.[47] To identify LCs, I use an indicator of firms' Political Action Committee (PAC) contributions to the campaign of political candidates. Following prior studies (e.g., Cooper et al. 2010; Ramanna and Roychowdhury 2010; Kim and Zhang 2016; Baloria and Klassen 2018), I manually retrieve PAC contributions data from the Federal Election Comission

[46] Including fourth quarter indicator can therefore be also seen as a way of controlling for auditor effects.

[47] I specify firm fixed effects in the PC binary model because prior studies on the determinants of PC have noted that some time-invariant firm-specific factors (e.g., distance from a firm's headquarter [HQ] to capital of a country {barring HQ relocations}) are important predictors of PC (see Chaney et al. 2011; Kim and Zhang 2016). Using a firm fixed effect model to predict the likelihood of PC also helps to ascertain the effect of AA-CA-based promulgations on PC after accounting for the potential impact of firm-specific *unobservable* factors. For example, in reality, firms tend to have unique "election-related incentives" (Ramanna and Roychowdhury 2010, p.473). Some unique election-related incentives identified in the literature include the incentive to avoid negative public scrutiny and its follow-on political costs following a firm's indulgence in outsourcing-related job cuts (Ramanna and Roychowdhury 2010), the incentive to avoid releasing politically damaging financial information during elections (Baloria and Klassen 2018), et cetera. While these aforementioned incentives can be discerned from companies' fundamentals, a common empirical challenge, however, is the difficulty in observing every single election-related incentive. Including firm fixed effects could to some extent help control for an aggregate of these firm-specific *unobservable* incentives.

(FEC) database,[48] where I specifically extract data from the PAC summary file. "This file gives overall receipts and disbursements for each PAC and party committee registered with the commission, along with a breakdown of overall receipts by source and totals for contributions to other committees, independent expenditures made and other information" (see FEC database). Following Cooper et al. (2010) and Kim and Zhang (2016), I define a firm as legally connected (i.e., *PC_IND* =1) if it registered a PAC in a year, 0 otherwise.[49]

Table 10 presents DID estimates of the effect of AA-CA-based promulgations on PC. Column (1), reporting results for the original sample, aims to ascertain the effect of all AA-CA-based promulgations on PC. Columnn (2), on the other hand, focuses analysis on only AA-CA-based laws that ban contributions from PACs as a way of more closely aligning the nature of laws with the dependent variable, which is proxied by an indicator of PAC contributions. Laws retained under Column (2) encompass local legislations that, for instance, (i) prohibit campaign contributions from regulated corporations/industries that hire lobbyists (see, e.g., the copy of local law passed in the city of Seattle WA), (ii) prohibit politicians from taking campaign money from the special interests and industries they regulate (see, e.g., the copy of resolutions adopted by McHenry County IL and the city of Souhtfield MI), and (iii) prohibit the bundling of campaign contributions for politicians from several corporations (see, e.g., the copy of local ordinance passed in the city and county of San Francisco CA).

[48] http://classic.fec.gov/finance/disclosure/ftpsum.shtml

[49] Given that I use the quarterly setting and given further that PACs make campaign contribution decisions throughout the election year (and periods leading to the election year) before finally electing to use either monthly, quarterly, semi-annual, or annual filing of records with the FEC, in assigning *PC_IND*=1, I therefore count all quarters in an election year (periods leading to the election year) as falling within the election year (pre-election year). This treatment is based on the premise that campaign contributions at different dates in an election year (pre-election year) ultimately belong to that election year (pre-election year). Note that the PAC summary file only contains year-end reporting dates (for election year filers), and a mix of monthly and semi-annual reporting dates (for pre-election year filers).

Results tabulated in Columns (1) and (2) show a DID coefficient of -0.485 and -0.530, respectively, with both statistically significant at the 10% level. Given that the model estimated specifies both dependent and independent variables as indicator variables, I use the odds ratio to interpret the economic magnitude of findings. In general, the DID coefficients represent the log of odds ratio between the treatment group and control group (from the pre- to the post-period). The odds ratio (i.e., the ratio of odds of being politically connected for treated firms to the odds of being politically connected for control firms) under Column (1) is 0.6157 (i.e., exp -0.485), whereas the odds ratio under Column (2) is 0.5886 (i.e., exp -0.530). These ratios can be interpreted as follows: all else equal, the odds of being politically connected for treated firms is about 38.43 to 41.14 percent lower than for control firms. On the basis of both statistical and economic significance, the results suggest that AA-CA-based promulgations indeed constrain PC.

Decomposing Accounting Quality Effects —Legal versus Illegal Connections

While the test above shows that AA-CA-based promulgations indeed impact LCs, nothing is established about the impact on SCs. In this subsection, I make an attempt at providing some evidence that could provide some answers. Specifically, I follow prior studies (e.g., Baloria and Klassen 2018; Ramanna and Roychowdhury 2010) and first condition firms' assignment to groups based on whether they were connected before the first adoption year (i.e., 2014). To identify these firms, I look at the 4-year period prior to the first event year (i.e., 2010-2013) [see Baloria and Klassen 2018] to isolate firms that made campaign contributions during this 4-year period. I label these firms as Type I firms (i.e., LC firms), and for the rest of firms that did not make any campaign contribution, I label as Type II firms (i.e., SC and unconnected firms). I then conduct separate experiments for these two sub-samples using the same DID design. For example, for all Type I firms, those headquartered at locations adopting (not adopting) AA-CA-based promulgations will be assigned to the treatment (control) group. I use the same assignment criterion to select Type II firms into treatment and control groups.

Table 11, Panels A and B present the DID estimates for Type I and Type II firms, respectively.[50] To the extent that the shock is not expected to affect unconnected firms, which were bundled with SC firms in the treatment group in Panel B, any DID effect documented in Panel B is thus entirely assigned to SC. The results tabulated in both panels show statistically significant DID effects in the subsamples of both LC (Panel A) and SC (Panel B). In terms of economic magnitude, it appears that the impact on accounting quality for SC firms is more than that for LC firms. The impact on target beating, however, reverses the order of which effect dominates; that is, the effect of the shock on target beating for LC firms is more than that for SC firms.

Owing to the concern I earlier raised that conditioning firms' selection into groups on some firm characteristic(s) before applying a shock challenges randomness, readers are therefore advised to exercise caution in interpreting the above results as definitive. To get around this limitation, I alternatively use a moderating effect design to analyze in a single estimation, the difference in DID effects between both connection types. This approach preserves randomness in the experiment. I use an indicator variable ($LC_{\text{2010-2013}}$) to earmark all LC firms previously analyzed in Panel A. Specifically, $LC_{\text{2010-2013}}$ is a dummy equal to 1 if a firm makes any campaign contribution during the 4-year period preceding the first adoption event, 0 otherwise. I then interact this indicator with the DID term in Eq. (1). In this new estimation (reported as Panel C), the coefficient on $TREATED \times POST$ ($TREATED \times POST \times LC_{\text{2010-2013}}$) represents the total (incremental) effect of anti-corruption laws for SCs (LCs). The coefficient on $TREATED \times POST$

[50] In column (1) of Panel A, the percentage of firm-quarters that are legally connected is 5.29% (i.e., 3190 / [3190+57165]). This percentage is similar to the ones reported in prior studies (see, e.g., Faccio 2006, p.374; Chaney et al. 2011, p.63).

$\times$ $LC_{\text{2010-2013}}$ is therefore intepreted as the DID effect for LC firms minus that for SC firms, whereas the sum of these coefficients is the total effect for LC firms. The results tabulated in Panel C overall confirm results earlier tabulated in Panels A and B.[51]

The findings in this subsection taken together with the one in the preceding subsection suggest that AA-CA-based promulgations constrain managerial opportunism via imposing constraints on both legal and secret connections.

Capital Market Response and Price Efficiency

The preceding analyses shows that firms in adopting locations are less likely to engage in opportunistic earnings reporting. In this subsection, I report two tests on the impact of AA-CA-based promulgations on price efficiency with respect to *ex-post* improvement in earnings quality. While the first test focuses on how the capital market perceives *ex-post* improvement in earnings quality following AA-CA-based promulgations, the second test focuses on the extent to which future earnings and cash flows are incorporated in current stock prices.

In the first capital market test, I specify the following model to empirically analyze whether the documented *ex-post* improvement in accounting quality is priced by the capital market:[52]

$$\begin{aligned} R_{i,t} &= \alpha_0 + \alpha_1\, TREATED \times POST_{l,t} + \alpha_2\, TREATED \cdot POST_{l,t} \times high\,AQ_{i,t} \\ &+ \alpha_3\, high\,AQ_{i,t} + \textstyle\sum_{j\geq 4} \alpha_j\, CONTROLS_{i/l,t} + LOC.FE + TIME\,FE + \varepsilon_{i,t} \qquad (2) \end{aligned}$$

[51] To address the concern that some firms included in the subsample in Panel B might be connected through other means such as lobbying, in untabulated results (available upon request), I use lobbying spending measure of PC (Kim and Zhang 2016) instead of PAC campaign contributions as the conditioning variable to separate LCs from the rest of the sample and find qualitatively similar results to the above findings. Alternatively, I also jointly use lobbying spending and PAC campaign contributions to isolate LC firms from other firms and find that results are generally similar.

[52] Prior studies on the capital market consequences of PC present opposing views on how investors perceive political connectedness (e.g., Faccio 2006; Goldman et al. 2009; Cooper et al. 2010; Hadani and Schuler 2013; Akey 2015; Hung et al. 2015). Therefore, I specify a model that isolates the earnings quality improvement channel. This focus on the stock return moderating effect design is analogous to the empirical model used in Skinner and Sloan (2002).

In Eq. (2), the coefficient of interest is α_2, and $r_{i,t}$ is the ex-dividend stock return for the quarter (Tucker and Zarowin 2006). Control variables used in all price efficiency tests are adopted from prior research (see, e.g., Doyle et al. 2013). If investors reward firms for reporting high-quality earnings, then I expect α_2 to be significantly positive. In the model, $high\,AQ$ is a vector of measures distinguishing firms with high-quality earnings from those with low-quality earnings. For this analysis, which is reported in Table 12, I focus on three volatility measures of accruals to construct dummies that distinguish between these two firm groups. The proxies for high earnings quality are: *LOWER_AQ_VOL_J* (column 1), *LOWER_AQ_VOL_MJ* (column 2), and *LOWER_AQ_VOL* (column 3), which are equal to 1 if *DCA_J_VOL*, *DCA_MJ_VOL*, and *ACCR_VOL*, respectively, are below their sample medians, and 0 otherwise.

The results in Table 12 show that, after the adoptions, treatment firms' *ex-post* improvement in earnings quality results in significant increases in stock returns relative to control firms. Specifically, all three-way interaction terms in Table 12 are positive and significant at the conventional levels. This finding is consistent with the earlier conjecture that the capital market positively prices high-quality accounting information. However, effects not linked to the *ex-post* improvement in earnings quality, as captured by α_1 in Eq. (2), seem to be negatively associated with stock returns. Given that investors only observe LCs but not SCs owing to the secret nature of the latter, $\alpha_1$could be interpreted to mean that, after AA-CA-based promulgations, the market punished LC firms with poor accounting quality for losing their connections with public officials, which would have generated certain political benefits. Alternatively, it could simply mean that the market punished treated firms for poorer earnings quality; that is, treated firms that were unable to adjust their reporting strategies in the post-adoption period.

For the second capital market test, which focuses on examining whether stock prices impound information about future earnings and cash flows, I adopt Tucker and Zarowin's (2006) FERC model:[53]

$$R_{i,t} = \alpha_0 + \alpha_1 X_{i,t-1} + \alpha_2 X_{i,t} + \alpha_3 X_{i,t3} + \alpha_4 R_{i,t3} + \alpha_5 TREATED \times POST_{l,t}$$
$$+ \alpha_6 TREATED \cdot POST_{l,t} \times X_{i,t-1} + \alpha_7 TREATED \cdot POST_{l,t} \times X_{i,t} + \alpha_8 TREATED \cdot POST_{l,t} \times X_{i,t3}$$
$$+ \alpha_9 TREATED \cdot POST_{l,t} \times R_{i,t3} + \sum_{j \geq 10} \alpha_j CONTROLS_{i,t} + LOC.FE + TIME\ FE + \varepsilon_{i,t} \quad (3)$$

Given the *ex-post* enhancement in quality of earnings reports, Eq. (3) aims to test if, ex-post, this culminates in stock prices better capturing information about future earnings and cash flows. This test is thus similar in spirit to that of Fang et al. (2016), who examine the impact of short selling on earnings quality: they find that, following an improvement in earnings quality induced by the disciplining role of short selling, stock prices incorporated more information about future earnings. The coefficient of interest in Eq. (3) is α_8. If my FERC conjecture is correct, I expect this coefficient to be significantly positive.

In column 1 of Table 13 (Panels A and B), the results are consistent with expectations. Specifically, based on analysis performed using earnings (Panel A) and operating cash flows as a component of earnings (Panel B), I find a significantly positive α_8. These results indicate that, in response to enhanced earnings quality after the passage of AA-CA-based promulgations, stock prices incorporated more information about future earnings and cash flows. In column 2 of Table 13 (Panels A and B), I split the aggregate future earnings and aggregate cash flows variables—

[53] Variables specified in the FERC model are defined as: R_t = the ex-dividend stock return for the quarter; $R_{t3=}$ the compounded stock return for quarters t+1 through t+3; X_{t+n} = the earnings per share for quarter t+n, scaled by stock price at the beginning of quarter t; and X_{t3} = the sum of earnings per share for quarters t+1 through t+3, scaled by stock price at the beginning of quarter t. Instead of just focusing on earnings (i.e., X), I also conduct a separate analysis on operating cash flows as a component of earnings (Tucker and Zarowin 2006, p.262). Additional variables in that analysis are defined as follows: OCF_{t+n} = operating cash flows (OCF) for quarter t+n, scaled by market capitalization at the beginning of quarter t; and OCF_{t3} = the sum of OCF for quarters t+1 through t+3, scaled by market capitalization at the beginning of quarter t.

aggregated over the three future periods, t+1, t+2, and t+3—into their corresponding future time components. As shown in column 2 of Panels A and B, it is the disaggregated information about future earnings in the next two quarters that drive the FERC price efficiency results.[54]

Overall, the results estimated from Eq. (2) and Eq. (3) indicate that anti-corruption promulgations—a constraint to corporate political connectedness—impact price efficiency through enhancing the quality of reported earnings.

Reconstructed (Stricter) Sample

To classify treatment firms under the second criterion stated in the Methodology section, I required that, cities or counties adopting resolutions deferring the passage of ultimate anti-corruption laws to the state and/or Congress should have at least formally expressed their support for or committed to enforcing a tailored AA-CA through local legislation. Consequently, firms headquartered in locations that merely passed resolutions deferring ultimate legislative action to their states and/or Congress were automatically classified in the control group.

However, with regard to the treatment group, localities adopting resolutions that defer ultimate legislative action to the state and/or Congress may be considered to carry less weight than localities specifically passing their own local measures, ordinances, or laws to stop political bribery. The idea here is that, at the locality level, deferring ultimate legislative action could be considered less impactful. To address this concern, I reconstruct a stricter sample that completely excludes all firms headquartered in cities and counties that passed resolutions deferring ultimate legislative

[54] For disaggregated information about future cash flows in the next two quarters, the signs are as expected, but the coefficients are only marginally significant (with reference to the 10% statistical significance level).

action to the state and/or Congress. Removal of these cases from both treatment and control groups shrinks the sample compared to that used in all previous analyses. [55]

In unreported tests (available upon request), I re-estimate all baseline DID, dynamic effect and price efficiency models. The results are found to be qualitatively similar.[56]

V. CONCLUSION

This study exploited the staggered adoption of anti-corruption measures, resolutions, ordinances, and laws across the U.S. as an exogenous shock to corporate political connectedness to provide causal evidence on the link between PC and accounting quality. These local anti-corruption laws are modeled on the AA-CA, which aims to criminalize the purchase of political influence. Adoption of these local laws has imposed significant costs on PC firms that potentially exceed the benefits obtained from affiliations with politicians. Consequently, firms are discouraged from establishing or maintaining connections with politicians.

I find that, relative to control firms, treatment firms exhibit significant improvements in earnings quality after the adoption of these anti-corruption promulgations. Treatment firms' use of income-increasing discretionary accruals to meet/beat analysts' earnings consensus forecasts, as well as their tendency to meet/beat this earnings target by up to one cent, also significantly diminish after these promulgations. This evidence indicates that AA-CA-based promulgations play

[55] Note that in the same way that an argument was made for the reconstructed treatment group to exclude locations deferring ultimate tough legislation to the state and/or Congress, for the control group, one may also argue that it may not be appropriate to include in the same control group, locations that adopt resolutions deferring ultimate legislation versus those that did not adopt any resolution, measure, ordinance, or law. One plausible reason why it may not be appropriate to include both location types in the same control group is provided below: locations adopting resolutions have at least expressed their *intention* —albeit not leading to the immediate passage of local laws —to curb political bribery, whereas non-adopting locations have expressed no such intention. To the extent that a formal communication of *intention* could be consequential in the psychology literature (see, e.g., Cohen, Morgan, and Pollack 1990), the deletion of all resolution-adopting-locations from the control group is also justified. In the psychology work cited above, Cohen et al. argue the following: "intention today must somehow influence later action; otherwise, why bother today to form an intention about tomorrow?.., once formed, intention today will persist until tomorrow and then guide what will then be present action" (p.16).

[56] For conciseness in the manuscript, I do not report these alternative tests.

a disciplining role that constrains managerial opportunism. Further, price efficiency tests show that the ex-post enhancement in earnings quality (brought by these local anti-corruption laws) is favorably priced by the stock market, with stock prices also better incorporating information about future earnings and cash flows.

This study has implications for future research. Researchers could consider exploiting this exogenous source of variation in corporate political connectedness to better understand corporate decisions. While this study examined financial reporting decisions, future studies could focus on other key decision areas, such as corporate financing and investment decisions.

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Appendix A: Full list of measures, resolutions, ordinances, and laws mimicking the American Anti-Corruption Act (AA-CA) [2014-2017]

	Location Name	Regulation Type	Adoption Date	Local law Passed/custom-tailored AA-CA adopted?	Requires Congress and/or State to pass ultimate law?
1	Genoa, Illinois	Anti-Corruption Resolution & Legislation	November 4 2014	Yes	No
2	Tallahassee, Florida	City-wide Anti-Corruption Act	November 4 2014	Yes	No
3	Ewing Township, New Jersey	Anti-Corruption Resolution	February 11 2015	Yes	Yes
4	Winnebago County, Illinois	Anti-Corruption Resolution	April 7 2015	Yes	No
5	DeKalb County, Illinois	Anti-Corruption resolution	April 7 2015	Yes	No
6	Seattle, Washington	Law modeled after the American Anti-Corruption Act	November 3 2015	Yes	No
7	San Francisco, California	Expenditure Lobbyist Measure; an anti-corruption ballot measure	November 3 2015	Yes	No
8	South Brunswick, New Jersey	Anti-Corruption Resolution	April 12 2016	Yes	Yes
9	Portland, Oregon	Local Ordinance/Legislation: new rule requires lobbyists and political consultants to disclose their political services.	April 20 2016	Yes	No
10	Miami-Dade County, Florida	Transparency Legislation/Ordinance: requires transparency regarding from all candidates for local office and lowers the campaign donation threshold	May 17 2016	Yes	No
11	Cocoa, Florida	Anti-Corruption Resolution	July 27 2016	Yes	No
12	Southfield, Michigan	Anti-Corruption Resolution	September 26 2016	Yes	Yes
13	San Francisco, California	Ordinance: Prop T, a ban on gifts from lobbyists	November 8 2016	Yes	No
14	Multnomah County, Oregon	Measure 26-184: Limit contributions from individuals and PACs to $500, limit independent spending, and require disclosure of the true funders of political ads.	November 8 2016 {effective September 1 2017}	Yes	No
15	Boone County, Illinois	Anti-Corruption Resolution	November 8 2016	Yes	No
16	McHenry County, Illinois	Anti-Corruption Resolution	November 8 2016	Yes	No
17	South Dakota	Anti-Corruption Act (state-wide)	November 8 2016	Yes	Yes
18	Portland, Oregon	Open & Accountable Elections: Curbed the power of wealthy donors in city elections	December 14 2016	Yes	No
19	Stephenson County, Illinois	Anti-Corruption Resolution: Called for reform after seeing ideas of the American Anti-corruption Act	April 4 2017	Yes	Yes
20	Burnsville, North Carolina	Comprehensive Anti-Corruption Resolution & Comprehensive Anti-Gerrymandering Resolution	June 26 2017	Yes	No
21	Yancey County, North Carolina	Anti-Corruption Resolution	November 13 2017	Yes	Yes

Source: https://represent.us/our-wins-old/ or https://represent.us/our-wins/ via American Anti-Corruption Act website (https://anti-corruptionact.org/) {last accessed Oct. 4 2018}

APPENDIX B: Excerpts of an AA-CA-based Ordinance (San Francisco, California)

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DEPARTMENT OF ELECTIONS

LEGISLATIVE DIGEST

[Initiative Ordinance - Campaign and Governmental Conduct Code - Restricting Gifts and Contributions from Lobbyists]

Ordinance amending the Campaign and Governmental Conduct Code to gifts and campaign contributions from lobbyists.

Existing Law

The City's Lobbyist Ordinance, Article II of the Campaign and Governmental Conduct Code, imposes registration and reporting requirements on two types of lobbyists: contact lobbyists and expenditure lobbyists. A contact lobbyist is any individual who either (1) makes five or more contacts in a month with City officers on behalf of the individual's employer, or (2) makes at least one contact in a month with an officer of the City and County on behalf of a client who pays that individual for lobbyist services. Campaign & Gov'tal Conduct Code ("C&GC Code") § 2.105. An expenditure lobbyist is any person who engages in indirect lobbying, by spending at least $2,500 in a month to solicit, request, or urge others to lobby City officers.

Amendments to Current Law

1. Registration and reporting requirements

The proposed measure would require lobbyists to identify which City agencies they intend to influence. The proposal would also explicitly impose a duty on local lobbyists to amend and update their registration information and monthly reports within five days of any changed circumstances that would affect the accuracy of information previously provided.

2. Gifts

The proposed measure would prohibit lobbyists from making any gift to a City officer, regardless of value, and would prohibit City officers from accepting or soliciting such gifts. But the proposal provides an exception to 501(c)(3) nonprofit organizations that would allow such nonprofit organizations to provide gifts of food or refreshment worth $25 or less to City officers, if the 501(c)(3) nonprofit organization offers such gifts to all attendees of a public event that the organization is hosting.

The amendments would also clarify that a lobbyist could not use a third-party to circumvent the Lobbyist Ordinance's restrictions on gifts.

3. Campaign contributions

The proposed measure would prohibit lobbyists from making contributions to or bundling contributions for City elected officials or candidates for City elective offices, if the lobbyists had been registered to lobby the officials' agencies within 90 days of the date any contribution is made.

The measure would extend this prohibition to any candidate-controlled committee, including candidate controlled ballot measure committees. But this prohibition would not extend to any candidate-controlled states committees, i.e., committees that would support or oppose any candidates for State elective office or State ballot measures.

Source: https://represent.us/our-wins-old/ (accessed October 4, 2018)

APPENDIX C
Variable Definition

Accrual-based variables (and dep. Variables)	
ACCRUALS	A "quarterly accruals" estimate, calculated as: "(CHGAR + CHGINV + CHGAP + CHGTAX + CHGOTH), where the bracketed quantities represent the change in accounts receivable (item RECCHY), inventories (item INVCHY), accounts payable (item APALCHY), taxes (item TXACHY), and other items (item AOLOCHY), all taken from the quarterly cash flow statement and appropriately differenced to calculate the individual quarterly amounts. We recode missing values of RECCHY, INVCHY, APALCHY, and TXACHY as 0 if there is a nonmissing value of AOLOCHY. Conversely, if AOLOCHY is missing, but the other items are not missing, then we recode AOLOCHY as 0." (Collins et al. 2017, p.84). The sum of these components are then scaled by lagged assets (ATQ).
DCA_J	The discretionary accruals estimate, calculated as residuals from the Jones (1991) model without the PPE component {see Dechow et al. 1995, p.198; Chaney et al. 2011, p.61}: accruals = a1×(1/ lagged assets [ATQ]) + a2×(Δsales [SALEQ]/lagged assets [ATQ]) + ε. As is the case in prior studies (Chaney et al. 2011; Kothari et al. 2005; Collins et al. 2017), the model is estimated for each industry in a given period (i.e., each industry-year-quarter) and residuals (ε) are calculated based on estimated coefficients.
DCA_MJ	The discretionary accruals estimate, calculated as residuals from the Modified-Jones model without the PPE component{see Dechow et al. 1995, p.199; Kothari et al. 2005, p.174; Chaney et al. 2011, p.61}: accruals = a1×(1/ lagged assets[ATQ]) + a2×{(Δsales [SALEQ] - Δreceivables [RCVTQ])/ lagged assets [ATQ]} + a3×ROA + ε, where ROA=net income [NIQ] / lagged assets [ATQ]. As is the case in prior studies (Chaney et al. 2011; Kothari et al. 2005; Collins et al. 2017), the model is estimated for each industry in a given period (i.e., each industry-year-quarter) and residuals (ε) are calculated based on estimated coefficients.
DCA_J_VOL	The 5 quarters-rolling-window standard deviation of *DCA_J* (see Chaney et al. 2011, p.64)
DCA_MJ_VOL	The 5 quarters-rolling-window standard deviation of *DCA_MJ* (see Chaney et al. 2011, p.64)
ACCR_VOL	The 5 quarters-rolling-window standard deviation of accruals (see Chaney et al. 2011, p.64)
ABS_DCA_J	The absolute value of *DCA_J* (see Dechow and Dichev 2002)
ABS_DCA_MJ	The absolute value of *DCA_MJ* (see Dechow and Dichev 2002)
ABS_ACCR	The absolute value of *ACCRUALS* (see Dechow and Dichev 2002)
MBE_DCA_J	An indicator variable that identifies the incidence that a firm that meets/beats analysts' earnings [EPS] forecasts uses income-increasing discretionary accruals; 0 otherwise. To identify firms that meet/beat analysts' earnings forecasts, I first compute forecast error (FE) as: [Actual EPS - Median IBES EPS forecast] and set it equal to 1 if either positive or zero; 0 otherwise {see Skinner and Sloan 2002; Davis et al. 2009; Doyle et al. 2013}. Income-increasing discretionary accruals here is taken as positive values of dca_j. This measurement is analogous to Doyle et al.'s (2013, p.51-52), who use a dummy to earmark the incidence that firms that meet/beat analysts' earnings forecasts strictly use income-increasing tools.
MBE_DCA_MJ	An indicator variable that identifies the incidence that a firm that meets/beats analysts' earnings [EPS] forecasts uses income-increasing discretionary accruals; 0 otherwise. To identify firms that meet/beat analysts' earnings forecasts, I first compute forecast error (FE) as: [Actual EPS - Median IBES EPS forecast] and set it equal to 1 if either positive or zero; 0 otherwise {see Skinner and Sloan 2002; Davis et al. 2009; Doyle et al. 2013}. Income-increasing discretionary accruals here is taken as positive values of dca_mj. This measurement is analogous to Doyle et al.'s (2013, p.51-52), who use a dummy to earmark the incidence that firms that meet/beat analysts' earnings forecasts strictly use income-increasing tools.
MBE_ACCR	An indicator variable that identifies the incidence that a firm that meets/beats analysts' earnings [EPS] forecasts uses income-increasing accruals; 0 otherwise. To identify firms that meet/beat analysts' earnings forecasts, I first compute forecast error (FE) as: [Actual EPS - Median IBES EPS forecast] and set it equal to 1 if either positive or zero; 0 otherwise {see Skinner and Sloan 2002; Davis et al. 2009; Doyle et al. 2013}. Income-increasing accruals here is taken as positive values of accruals. This measurement is analogous to Doyle

	et al.'s (2013, p.51-52), who use a dummy to earmark the incidence that firms that meet/beat analysts' earnings forecasts strictly use income-increasing tools.
MBE_1_CENT	An indicator variable equal to 1 if a firm meets/beats analysts' earnings [EPS] forecasts by up to $0.01; 0 otherwise. To identify firms that meet/beat analysts' earnings forecasts, I first compute forecast error (FE) as: [Actual EPS - Median IBES EPS forecast] and set it equal to 1 if either positive or zero; 0 otherwise {see Skinner and Sloan 2002; Davis et al. 2009; Doyle et al. 2013}.
Independent variables	
TREATED	A dummy equal to 1 if a township, city, county, or state that a firm's headquarter (HQ) is located in adopts measures, laws, resolutions, or ordinances modeled after the American Anti-Corruption Act; 0 otherwise.
POST	An indicator variable (specific to only firms with $TREATED$ =1) that takes the value of 1 for all periods starting from each event date, and 0 for all pre-event periods (for similar measurement, see Bertrand and Mullainathan 1999a, p.544).
Firm Characteristics	
CASHFLOW_VOL	The 5 quarters-rolling-window standard deviation of operating cash flows over assets (see Chaney et al. 2011, p.65), where operating cash flows over assets is calculated as: OANCFQ / ATQ. Note that Compustat NA Quarterly database only provides data on year-to-date operating cash flows (OANCFY) and so one can simply compute quarterly operating cash flows (OANCFQ) as ΔOANCFY t.
SALES_VOL	The 5 quarters-rolling-window standard deviation of sales over assets (see Chaney et al. 2011, p.65), where sales over assets is calculated as: SALEQ / ATQ.
Δ*SALES_VOL*	The 5 quarters-rolling-window standard deviation of quarterly sales growth (see Chaney et al. 2011, p.65), where quarterly sales growth is calculated as: ΔSALEQ t / ΔSALEQ t-1
LEVERAGE	The sum of long-term debt (DLTTQ) and debt in current liabilities (DLCQ) divided by total assets (ATQ)
SIZE_MV	The natural logarithm of market capitalization (PRCCQ*CSHOQ) in millions of US dollars.
MARKET_TO_BOOK	"The sum of book value of debt and market value of equity scaled by total assets (ATQ),where market value of equity equals price per share times the total number of shares outstanding (PRCCQ*CSHOQ), and book value of debt equals total assets minus book value of equity (ATQ - CEQQ)"
RFIN_HEALTH_PCA	The rank of the principal component factor extracted from three proxies of financial constraint, namely: (1) *Modified Altman (1968)'s Z-SCORE*, calculated as: (3.3×earnings before interest and tax [OIADPQ] + 1×sales [SALEQ] + 1.2×current assets [ACTQ] + 1.4×retained earnings [REQ]) /total assets [ATQ] {See Smith 2016}; (2) *Ohlson's (1980) O-SCORE*, calculated as: O-score = -1.32 - 0.407×(log of total assets[ATQ]) + 6.03×(total liabilities [LTQ]/total assets [ATQ]) - 1.43×(working capital [WCAPQ]/total assets [ATQ]) + 0.076×(current liabilities [LCTQ]/current assets [ACTQ]) - 1.72×(1 if total liabilities [LTQ] > total assets [ATQ], 0 otherwise) - 0.521×((net income [NIQ] t - net income [NIQ] t-1)/(│net income [NIQ] t│ + │net income [NIQ] t-1│)); and (3) *Kaplan & Zingales' (1997) KZ-INDEX*, calculated as: -1.002×(operating cash flow [OANCFQ] / total assets [ATQ]) - 39.368×(cash dividends [DVPSPQ×CSHOQ] /total assets [ATQ]) - 1.315×(total cash balance [CHQ] / total assets [ATQ]) + 3.139×leverage + 0.283×market_to_book. Factor loadings from the principal component analysis are then used to calculate the following index: *FIN_HEALTH_PCA* = -0.664×*Z-SCORE* + 0.663×*O-SCORE* + 0.345×*KZ-INDEX*. Unreported Pearson correlations show that *FIN_HEALTH_PCA* is negatively related to *Z-SCORE* but positively related to *O-SCORE* and *KZ-INDEX*. The higher this index, the poorer the financial health of the firm (for related measurement and interpretation, see Bharath, Sunder, and Sunder 2008, p.10-12). The decile rank of this index (i.e., *RFIN_HEALTH_PCA*) is then used as the main proxy (instead of the raw index) because it helps to reduce the impact of measurement errors.
STATE_INTEGRITY	A 2011 perception-based corruption risk index by the Center for Public Integrity (www.stateintegrity.org) {data source: Scheele, Losco, and Hall 2012}

SALES_GROWTH	A seasonally-differenced sales growth measure, calculated as: (SALEQ t - SALEQ t-4) / SALEQ t-4
FORSTD	The I/B/E/S analysts EPS forecast dispersion, measured as the standard deviation of analysts' EPS forecasts {as provided by I/B/E/S}
N_ANALYSTS	The number of I/B/E/S analysts covering a firm

TABLE 1

Sample Distribution by States

	State Name	#Firms	#Obs		State Name	#Firms	#Obs
1	Alabama	12	243	**26**	Montana	5	70
2	Alaska	2	62	**27**	Nebraska	20	499
3	Arizona	89	1,593	**28**	Nevada	94	1,332
4	Arkansas	15	381	**29**	New Hampshire	15	257
5	California	296	5,017	**30**	New Jersey	105	2,118
6	Colorado	153	2,878	**31**	New Mexico	5	136
7	Connecticut	72	1,675	**32**	New York	229	4,003
8	Delaware	8	212	**33**	North Carolina	54	1,198
9	Florida	193	3,553	**34**	North Dakota	2	66
10	Georgia	109	1,984	**35**	Ohio	49	1,189
11	Hawaii	6	69	**36**	Oklahoma	39	821
12	Idaho	12	237	**37**	Oregon	40	841
13	Illinois	138	3,044	**38**	Pennsylvania	70	1,334
14	Indiana	20	423	**39**	Rhode Island	12	329
15	Iowa	7	175	**40**	South Carolina	14	298
16	Kansas	20	454	**41**	South Dakota	7	172
17	Kentucky	28	584	**42**	Tennessee	63	1,256
18	Louisiana	26	502	**43**	Texas	432	8,362
19	Maine	4	79	**44**	Utah	63	1,113
20	Maryland	57	984	**45**	Vermont	4	47
21	Massachusetts	206	3,742	**46**	Virginia	67	1,315
22	Michigan	47	919	**47**	Washington	95	1,644
23	Minnesota	79	1,759	**48**	West Virginia	5	119
24	Mississippi	4	88	**49**	Wisconsin	45	1,067
25	Missouri	7	158	**50**	Wyoming	4	50
					TOTAL	**3,148**	**60,451**

TABLE 2

Descriptive Statistics

Variable	N	Mean	Median	Std Dev	10th Pctl	25th Pctl	75th Pctl	90th Pctl
TREATED	60,451	0.045	0.000	0.206	0.000	0.000	0.000	0.000
DCA_J × 100	60,451	-0.292	0.000	67.552	-3.304	-0.976	0.959	3.211
DCA_MJ × 100	60,451	0.332	-0.002	57.978	-3.608	-1.099	1.078	3.569
ACCRUALS × 100	60,451	-0.045	0.000	87.123	-3.090	-0.911	0.918	3.083
DCA_J_VOL × 100	60,355	6.659	1.692	77.251	0.396	0.795	3.837	8.758
DCA_MJ_VOL × 100	60,355	6.944	1.826	55.213	0.446	0.868	4.097	9.567
ACCR_VOL × 100	60,375	9.528	1.658	161.293	0.381	0.773	3.754	8.495
ABS_DCA_J × 100	60,451	4.411	0.966	67.408	0.108	0.335	2.558	6.341
ABS_DCA_MJ × 100	60,451	5.031	1.087	57.760	0.129	0.385	2.820	6.962
ABS_ACCR × 100	60,451	4.469	0.915	87.008	0.097	0.311	2.432	5.953
MBE_DCA_J	36,673	0.239	0.000	0.426	0.000	0.000	0.000	1.000
MBE_DCA_MJ	36,673	0.237	0.000	0.425	0.000	0.000	0.000	1.000
MBE_ACCR_T	36,673	0.238	0.000	0.426	0.000	0.000	0.000	1.000
MBE_1_CENT	36,673	0.062	0.000	0.242	0.000	0.000	0.000	0.000
CASHFLOW_VOL	60,451	0.293	0.066	1.619	0.025	0.040	0.115	0.305
SALES_VOL	60,451	0.113	0.024	4.080	0.005	0.011	0.056	0.125
Δ*SALES_VOL*	60,451	1.043	0.131	43.317	0.037	0.066	0.285	0.680
LEVERAGE	60,451	0.514	0.224	4.037	0.000	0.035	0.407	0.661
SIZE_MV	60,451	5.949	6.150	2.569	2.531	4.098	7.795	9.131
MARKET_TO_BOOK	60,451	4.650	1.660	23.342	0.981	1.209	2.758	5.429
RFIN_HEALTH_PCA	60,451	4.653	4.604	2.730	0.927	2.306	6.928	8.477
STATE_INTEGRITY	60,451	22.625	25.000	13.927	4.000	11.000	33.000	42.000
SALES_GROWTH	60,451	-0.743	0.018	91.201	-0.281	-0.068	0.102	0.250
FORSTD	35,464	1.346	0.038	53.712	0.012	0.021	0.073	0.142
N_ANALYSTS	60,451	1.123	0.000	1.917	0.000	0.000	2.000	4.000

TABLE 3

Pearson Correlation Matrix

		1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**
1	*CASHFLOW_VOL*	1.000										
2	*SALES_VOL*	0.156***	1.000									
3	Δ*SALES_VOL*	0.002	0.001	1.000								
4	*LEVERAGE*	0.207***	0.079***	0.001	1.000							
5	*SIZE_MV*	-0.174***	-0.024***	-0.009**	-0.124***	1.000						
6	*MARKET_TO_BOOK*	0.234***	0.055***	0.009**	0.479***	-0.123***	1.000					
7	*RFIN_HEALTH_PCA*	0.139***	0.025***	0.010**	0.148***	-0.516***	0.198***	1.000				
8	*STATE_INTEGRITY*	0.010**	0.010**	-0.006	0.014***	-0.001	0.001	0.027***	1.000			
9	*SALES_GROWTH*	-0.0001	-0.0001	-0.946***	0.0004	0.001	-0.001	-0.0001	0.004	1.000		
10	*FORSTD*	0.011**	0.003	-0.00004	-0.006	-0.039***	-0.004	0.036***	0.006	0.0002	1.000	
11	*N_ANALYSTS*	-0.072***	-0.011***	-0.006	-0.034***	0.500***	-0.059***	-0.236***	-0.006	0.0017	-0.008	1.000

TABLE 4
Exogenous Decline in Political Connection and its Impact on Accounting Quality

This table reports difference-in-differences (DID) regression estimates for the effect of exogenous decline in political connection on accounting quality [See Appendix C for detailed definitions of variables]. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for county and year×quarter fixed effects.

	(1) *DCA_J_VOL* ×100	(2) *DCA_MJ_VOL* ×100	(3) *ABS_DCA_J* ×100	(4) *ABS_DCA_MJ* ×100
TREATED × POST	**-6.202****	**-4.932*****	**-6.870****	**-4.839****
	(-2.36)	(-2.58)	(-2.30)	(-2.32)
CASHFLOW_VOL	0.749*	1.232***	2.186*	1.436**
	(1.79)	(3.76)	(1.79)	(2.10)
SALES_VOL	13.777***	5.930***	7.532	3.014
	(15.91)	(11.14)	(1.30)	(1.55)
Δ*SALES_VOL*	0.010	0.009	0.008***	0.008***
	(1.52)	(1.60)	(4.91)	(5.68)
LEVERAGE	0.003	0.689	0.412	0.885
	(0.01)	(1.64)	(0.70)	(1.32)
SIZE_MV	-0.942***	-0.960***	-0.870***	-0.789***
	(-12.43)	(-14.67)	(-9.69)	(-9.09)
MARKET_TO_BOOK	0.342***	0.228***	0.183***	0.171***
	(6.80)	(6.18)	(3.66)	(3.82)
RFIN_HEALTH_PCA	0.572***	0.775***	0.189***	0.435***
	(4.58)	(8.59)	(2.60)	(3.38)
STATE_INTEGRITY	-0.371	-0.358	-0.085	-0.090**
	(-1.27)	(-1.29)	(-1.53)	(-2.30)
County Fixed Effects?	Yes	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes	Yes
N	60355	60355	60451	60451
Adjusted R-square	0.562	0.251	0.235	0.086

TABLE 5
Exogenous Decline in Political Connection and its Impact on Target Beating

This table reports difference-in-differences (DID) regression estimates for the effect of exogenous decline in political connection on: (1) the incidence that a firm that meets/beats analysts' earnings forecasts uses income-increasing accruals {Columns 1-2}; and (2) the incidence that a firm meet/beats analysts' earnings forecasts by up to one cent {Column 3} [See Appendix C for detailed definitions of variables]. Because the dependent variables are dummies, a PROBIT regression is used in this estimation. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for county and year×quarter fixed effects.

	(1) *MBE_DCA_J*	(2) *MBE_DCA_MJ*	(3) *MBE_1_CENT*
TREATED × POST	**-0.256*****	**-0.300*****	**-0.367****
	(-2.78)	(-3.06)	(-2.57)
CASHFLOW_VOL	-0.019	-0.020	-0.010
	(-1.29)	(-1.39)	(-0.38)
SALES_VOL	-0.048	-0.012	-1.016***
	(-0.59)	(-0.15)	(-3.30)
ΔSALES_VOL	0.000**	0.000*	-0.003
	(2.46)	(1.82)	(-1.00)
LEVERAGE	-0.075	-0.067	-0.220***
	(-1.58)	(-1.47)	(-3.15)
SIZE_MV	0.053***	0.057***	0.017**
	(9.31)	(9.71)	(1.97)
MARKET_TO_BOOK	0.030***	0.030***	0.031***
	(6.62)	(6.61)	(5.67)
RFIN_HEALTH_PCA	-0.017***	-0.025***	0.012
	(-3.31)	(-4.75)	(1.51)
STATE_INTEGRITY	-0.002	-0.004	-0.007*
	(-0.82)	(-1.56)	(-1.82)
FORSTD	0.000*	0.000***	-0.142
	(1.71)	(4.58)	(-1.38)
N_ANALYSTS	-0.015***	-0.020***	-0.022***
	(-3.66)	(-4.71)	(-3.69)
County Fixed Effects?	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes
N	34089	34089	34089
Pseudo R-square	0.030	0.032	0.037

TABLE 6
Dynamic Effects - Accounting Quality

This table reports regression estimates of dynamic effects of exogenous decline in political connection on accounting quality [See Appendix C for detailed definitions of variables]. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for county and year×quarter fixed effects.

	(1) *DCA_J_VOL* ×100	(2) *DCA_MJ_VOL* ×100	(3) *ABS_DCA_J* ×100	(4) *ABS_DCA_MJ* ×100
TREATED **×** *QTR T-1*	1.213	1.624	4.138	5.878
	(0.64)	(0.70)	(1.02)	(1.05)
TREATED × QTR T	**-2.443**	**-1.678**	**-5.251**	**-2.619**
	(-0.63)	(-0.49)	(-1.32)	(-0.95)
TREATED × QTR T+1	**-5.633***	**-4.928****	**-9.414****	**-5.406***
	(-1.75)	(-2.13)	(-2.37)	(-1.94)
TREATED × QTR T+2 AND BEYOND	**-1.821**	**-2.794***	**-1.586**	**-3.679****
	(-1.13)	(-1.91)	(-0.99)	(-2.29)
Controls Included?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes	Yes
N	60355	60355	60451	60451
Adjusted R-square	0.562	0.251	0.235	0.086

TABLE 7
Dynamic Effects - Target Beating

This table reports regression estimates for the dynamic effect of exogenous decline in political connection on: (1) the incidence that a firm that meets/beats analysts' earnings forecasts uses income-increasing accruals {Columns 1-2}; and (2) the incidence that a firm meet/beats analysts' earnings forecasts by up to one cent {Column 3} [See Appendix C for detailed definitions of variables]. Because the dependent variables are dummies, a PROBIT regression is used in this estimation. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for county and year×quarter fixed effects.

	(1) *MBE_DCA_J*	(2) *MBE_DCA_MJ*	(3) *MBE_1_CENT*
TREATED × QTR T-1	-0.107	-0.126	-0.106
	(-0.62)	(-0.65)	(-1.05)
TREATED × QTR T	**-0.303****	**-0.276****	**-0.682***
	(-2.40)	(-2.24)	(-1.72)
TREATED × QTR T+1	**-0.788*****	**-0.728*****	**0.000**
	(-2.76)	(-2.71)	(.)
TREATED × QTR T+2 AND BEYOND	**-0.191****	**-0.269*****	**-0.052**
	(-2.35)	(-3.36)	(-0.50)
Controls Included?	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes
N	34089	34089	34089
Pseudo R-square	0.030	0.032	0.037

TABLE 8
Estimations Based on Total Accruals

This table reports difference-in-differences (DID) regression estimates for the effect of exogenous decline in political connection on: (1) accounting quality [Columns 1-2]; and (2) the incidence that a firm that meets/beats analysts' earnings forecasts uses income-increasing accruals {Column 3} [See Appendix C for detailed definitions of variables]. Because the dependent variable in Column 3 is a dummy, a PROBIT regression is used in this estimation. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for county and year×quarter fixed effects.

	(1) *ACCR_VOL* ×100	(2) *ABS_ACCR* ×100	(3) *MBE_ACCR*
TREATED × POST	**-11.404****	**-7.609***	**-0.255*****
	(-2.11)	(-1.73)	(-2.76)
Controls Included?	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes
N	60375	60451	34089
Adjusted/Pseudo R-square	0.172	0.183	0.030

TABLE 9
Alternative DID Specifications

Using county and firm fixed effects, this table reports alternative difference-in-differences (DID) regression estimates for the effect of exogenous decline in political connection on: (1) accounting quality [Columns 1-4]; (2) the incidence that a firm that meets/beats analysts' earnings forecasts uses income-increasing accruals {Columns 5-6}; and (3) the incidence that a firm meet/beats analysts' earnings forecasts by up to one cent {Columns 7-8} [See Appendix C for detailed definitions of variables]. Because the dependent variables in Columns 5-8 are dummies, PROBIT regressions are used in these estimations. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. For the sake of conciseness, only measures based on the Jones model are used here. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regressions control for year×quarter fixed effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DCA_J_VOL ×100		*ABS_DCA_J* ×100		*MBE_DCA_J*		*MBE_1_CENT*	
TREATED* × *POST	**-4.614****	**-4.783****	**-5.218****	**-6.082****	**-0.222****	**-0.297*****	**-0.257****	**-0.495*****
	(-2.35)	(-2.29)	(-2.21)	(-2.15)	(-1.99)	(-2.64)	(-2.00)	(-2.71)
TREATED	1.874**		2.266**		0.097**		0.127**	
	(2.08)		(2.29)		(2.28)		(2.49)	
Controls Included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects?	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**
Year × Quarter Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	60355	60355	60451	60451	34089	34089	34089	34089
Adjusted/Pseudo R-square	0.561	0.697	0.236	0.229	0.019	0.084	0.014	0.096

TABLE 10
Test of Assumption – the Impact of AA-CA-Based Promulgations on PC

This table reports DID regression estimates for the effect of AA-CA-based promulgations on political connection, proxied by an indicator of Political Action Committee (PAC) contributions to election campaigns (labeled as *PC_IND*) [See Appendix C for detailed definitions of variables]. Because the dependent variable is a dummy, a PROBIT regression is used in this estimation. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for firm and year×quarter fixed effects.

	Dependent Variable: ***PC_IND***	
	(1)	(2)
	All AA-CA-based laws	Only laws banning PAC contributions
TREATED × POST	**-0.485***	**-0.530***
	(-1.92)	(-1.88)
Controls Included?	Yes	Yes
Firm Fixed Effects?	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes
N	60451	59319
Pseudo R-square	0.808	0.805

TABLE 11
Isolating DID Effects for Legal vs. Illegal Connections

This table reports DID regression estimates for the effect of exogenous decline in political connection on: (1) accounting quality [Columns 1-2]; (2) the incidence that a firm that meets/beats analysts' earnings forecasts uses income-increasing accruals {Column 3}; and (3) the incidence that a firm meet/beats analysts' earnings forecasts by up to one cent {Column 4} [See Appendix C for detailed definitions of variables]. Panel A focuses on the sub-sample of firms legally connected in the 4-year period prior to the first adoption event, whereas Panel B focuses on the remainder of firms that did not make campaign contribution during the aforesaid period. Panel C uses an alternative design to compare DID effect of connection types in a single estimation. Because the dependent variables in Columns 3-4 are dummies, PROBIT regressions are used in these estimations. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. For the sake of conciseness, only measures based on the Jones model are used here. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regressions control for county and year×quarter fixed effects.

Panel A: DID Effect for Legal Connections

	(1) *DCA_J_VOL* ×100	(2) *ABS_DCA_J* ×100	(3) *MBE_DCA_J*	(4) *MBE_BY_1_CENT*
TREATED × POST	**-0.441****	**-0.809*****	**-4.234*****	**-3.595*****
	(-2.49)	(-3.86)	(-19.84)	(-8.44)
Controls Included?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes	Yes
N	3190	3193	2825	2825
Adjusted/Psuedo R-square	0.250	0.189	0.036	0.131

Panel B: DID Effect for Illegal Connections

	(1) *DCA_J_VOL* ×100	(2) *ABS_DCA_J* ×100	(3) *MBE_DCA_J*	(4) *MBE_BY_1_CENT*
TREATED × POST	**-6.434****	**-7.134****	**-0.228****	**-0.381*****
	(-2.38)	(-2.31)	(-2.36)	(-2.63)
Controls Included?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes	Yes
N	57165	57258	31264	31264
Adjusted/Psuedo R-square	0.562	0.234	0.031	0.038

Panel C: Analyzing the Difference in DID Effect of Connection Types in a Single Estimation

	(1) *DCA_J_VOL* ×100	(2) *ABS_DCA_J* ×100	(3) *MBE_DCA_J*	(4) *MBE_BY_1_CENT*
TREATED × POST	**-6.346****	**-6.980****	**-0.199***	**-0.308****
	(-2.37)	(-2.32)	(-1.93)	(-2.03)
TREATED × POST × LC $_{2010\text{-}2013}$	**4.770****	**3.402**	**-5.431*****	**-3.714*****
	(1.98)	(1.47)	(-29.65)	(-16.47)
LC $_{2010\text{-}2013}$	-1.178***	0.197	-0.056*	-0.130***
	(-3.04)	(0.47)	(-1.87)	(-2.81)
Controls Included?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes	Yes
N	60355	60451	34089	34089
Adjusted/Psuedo R-square	0.562	0.235	0.030	0.037

TABLE 12
Capital Market Consequence

This table reports regression estimates for the effect on stock returns of an improvement in accounting quality induced by the negative shock to political connection. The proxies for high accounting quality are: *LOWER_AQ_VOL_J* (col. 1), *LOWER_AQ_VOL_MJ* (col. 2), and *LOWER_AQ_VOL* (col. 3), which respectively are indicator variables set equal to 1 *IF DCA_J_VOL*, *DCA_MJ_VOL*, and *ACCR_VOL* are below their sample medians [See Appendix C for detailed definitions of other variables]. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for county and year×quarter fixed effects.

	(1)	(2)	(3)
	R_t	R_t	R_t
TREATED × POST	-0.040***	-0.036***	-0.039***
	(-3.99)	(-3.33)	(-4.03)
TREATED × POST × LOWER_AQ_VOL_J	**0.035*** **		
	(2.65)		
TREATED × POST × LOWER_AQ_VOL_MJ		**0.026***	
		(1.67)	
TREATED × POST × LOWER_AQ_VOL			**0.030*** **
			(2.35)
LOWER_AQ_VOL_JONES	0.002		
	(1.31)		
LOWER_AQ_VOL_MOD_JONES		0.001	
		(0.86)	
LOWER_AQ_VOL			0.002
			(1.33)
LEVERAGE	-0.001	-0.001	-0.002
	(-0.22)	(-0.21)	(-0.30)
SIZE_MV	0.001	0.001	0.001
	(1.31)	(1.34)	(1.25)
MARKET_TO_BOOK	0.006***	0.006***	0.006***
	(8.41)	(8.38)	(8.58)
SALES_GROWTH	-0.000***	-0.000***	-0.000***
	(-5.95)	(-5.95)	(-5.82)
FORSTD	-0.000**	-0.000**	-0.000*
	(-1.99)	(-1.99)	(-1.84)
N_ANALYSTS	-0.002***	-0.002***	-0.002***
	(-3.38)	(-3.37)	(-3.32)
County Fixed Effects?	Yes	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Yes
N	37,644	37,641	37,766
Adjusted R-square	0.161	0.161	0.160

TABLE 13
Price Efficiency

This table reports regression results of the Future Earnings Response Coefficient (FERC) model {see Tucker and Zarowin 2006}. Variables specified in the FERC model are defined as: R_t = the ex-dividend stock return for the quarter; $r_{t3=}$ the compounded stock return for quarters t+1 through t+3; X_{t+n}= the earnings per share for the quarter t+n, scaled by the stock price at the beginning of quarter t; X_{t3} = the sum of earnings per share for quarters t+1 through t+3, scaled by the stock price at the beginning of quarter t; OCF_{t+n}= the operating cash flows (OCF) for the quarter t+n, scaled by the market capitalization at the beginning of quarter t; OCF_{t3} = the sum of operating cash flows (OCF) for quarters t+1 through t+3, scaled by the market capitalization at the beginning of quarter t [See Appendix C for detailed definitions of other variables]. Asterisk demarcations *, ** and *** represent statistical significance at 10%, 5% and 1% levels, respectively. The table uses two-way clusters (state and year×quarter) to report heteroskedasticity-adjusted t-values in parentheses and the regression controls for county and year×quarter fixed effects.

Panel A: Analysis based on Earnings			**Panel B: Analysis based on Operating Cash Flows as a component of Earnings**		
	(1) R_t	(2) R_t		(1) R_t	(2) R_t
X_{t-1}	-0.033	-0.033	OCF_{t-1}	0.002	0.002
	(-1.35)	(-1.34)		(0.09)	(0.09)
X_t	-0.007	-0.007	OCF_t	-0.015	-0.015
	(-1.40)	(-1.39)		(-0.53)	(-0.53)
X_{t3}	0.006	0.006	OCF_{t3}	0.000	0.000
	(1.16)	(1.11)		(0.01)	(0.02)
R_{t3}	0.002	0.001	R_{t3}	0.003	0.003
	(0.77)	(0.52)		(1.28)	(1.28)
TREATED × POST	-0.054***	-0.029*	*TREATED × POST*	-0.045**	-0.042*
	(-2.89)	(-1.75)		(-1.99)	(-1.85)
TREATED × POST × X_{t-1}	-0.540**	-0.698**	*TREATED × POST ×* OCF_{t-1}	-0.076**	-0.059
	(-2.36)	(-2.37)		(-1.97)	(-0.43)
TREATED × POST × X_t	0.669***	0.049	*TREATED × POST ×* OCF_t	0.416***	0.263***
	(3.70)	(0.21)		(3.35)	(2.63)
TREATED × POST × $\boldsymbol{X_{t3}}$	**0.046****		***TREATED × POST ×*** $\boldsymbol{OCF_{t3}}$	**0.241*****	
	(1.99)			(2.63)	
TREATED × POST × $\boldsymbol{X_{t+1}}$		**0.177*****	***TREATED × POST ×*** $\boldsymbol{OCF_{t+1}}$		**0.146**
		(4.42)			(1.54)
TREATED × POST × $\boldsymbol{X_{t+2}}$		**1.237*****	***TREATED × POST ×*** $\boldsymbol{OCF_{t+2}}$		**0.156**
		(3.82)			(1.34)
TREATED × POST × $\boldsymbol{X_{t+3}}$		**-0.386**	***TREATED × POST ×*** $\boldsymbol{OCF_{t+3}}$		**0.167**
		(-1.49)			(0.87)
TREATED × POST × R_{t3}	-0.033**	-0.018	*TREATED × POST ×* R_{t3}	-0.029	-0.029
	(-2.16)	(-1.17)		(-1.64)	(-1.53)
Controls Included?	Yes	Yes	Controls Included?	Yes	Yes

County Fixed Effects?	Yes	Yes	County Fixed Effects?	Yes	Yes
Year × Quarter Fixed Effects?	Yes	Yes	Year × Quarter Fixed Effects?	Yes	Yes
N	34,996	33,385	N	32,145	32,145
Adjusted R-square	0.173	0.180	Adjusted R-square	0.172	0.172