PACs and Super PACs: Measuring the Changing Nature of Corruption Behavior

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Corruption behavior has been difficult to both observe and measure despite its illegal nature and importance to firms and society both economically and politically. In empirical research, proxies for corruption are frequently generated. A popular proxy, as in Smith (2016), is corruption convictions. This study extends Smith's analysis by examining data from 1998-2016, using Smith's shielding and liquidity hypotheses. Through an updated and enhanced dataset, this study finds that the shielding hypothesis is supported for the sample; however, in the post-Citizens United subsample corruption convictions are not significant in various regressions. This study discusses the implications of the latter result and underscores the need for further research with a more comprehensive model to examine corruption in greater detail.

Keywords: corporate finance, corruption, political action committees

INTRODUCTION

Corruption has increasingly become an important issue, both in the popular press and academic research. Corruption is a robust term with a multiplicity of definitions. Since Aristotle, scholars have defined corruption differently using many observable qualities (Jiang, 2017). In addressing corruption, scholars must isolate the various behaviors to be addressed. A working definition of political corruption has been synthesized from the extant literature and is defined in the current study, as the abuse of public office for personal gain (Smith, 2016). This study examines both bribe-seeking and extortion behavior, collectively known as rent-seeking activities.

Corruption-based research usually employs a standard tool, the Corruption Perceptions Index (CPI), which serves as a proxy for the level of corruption. The CPI is constructed using professional assessments and surveys from various agencies to determine investment risk (Lambsdoff, 1999). CPI is a non-ideal tool as professional assessments in surveys are largely subjective accounts (Smith, 2016), thereby creating a rater bias.

Paldam (2002) finds that within the CPI the differences in the amount and growth of real income per capita, the inflation rate, and the economic freedom index, all affect the prevalence of corruption both within and across specific jurisdictions. Because the CPI treats an entire nation as homogeneous in

measuring corruption, it misses critical differences that can occur when a country has different legal and economic environments across different regions, states, or provinces.

Much of the research has taken place at the national level, in keeping with the CPI. However, recently there has been an expansion to consider a within-country scope. An example of this is Smith's (2016) study which examines 94 judicial districts in the US. He examines COMPUSTAT data from 1980-2009, using Public Integrity Section (PIN) survey information to obtain corruption conviction rates within a given judicial district. There are two interesting findings in Smith's (2016) study.

First is the finding of significant heterogeneity between judicial districts in the US. Using the CPI the US would have been given one flat score. Smith's (2016) study provides a deeper level of nuance when considering corruption within the US. This finding should be applied in a broader context in corruption-based research as researchers attempt to better understand the concept and behavior of corruption.

Second, Smith finds that firm's financial policies are influenced by their corruption settings. Specifically, he finds that firms treat corruption as a de facto form of taxation, and seek out additional leverage while holding less cash, in order to minimize the propensity of their targeting by bribe-seeking behavior.

Smith's (2016) study examines the period from 1980-2009, however, in 2011 a significant judgement was rendered by the US Supreme Court. This ruling referred to as Citizens United created new, legal avenues for politicians to seek unregulated money from corporations through political action committees (PACs) and Super PACs. Super PACs are PACs that can raise money from individuals, corporations, or unions. Super PACs do not have an upper limit on their spending and, as a result, could theoretically spend unlimited amounts of money on behalf of a political candidate's campaign for various activities, such as ad-buys. Babenko, Fedaseyeu, and Zhang (2018) find that corporations which financially supported a winning candidate are subsequently granted favors.

As a result of the Citizens United ruling, corruption behavior has arguably been less regulated in its methods to seek funds from corporations. These corporations, in turn, create their own Super PACs with the intent of exchanging their support of politicians to reap the benefits through favorable legislation after their election. Owing to the decreased scrutiny arising from these measures, it is unlikely that politicians or firms would be convicted as frequently prior to 2011. Therefore, the PIN data is less likely to be a true reflection of rent-seeking behavior of political candidates. This could make corruption convictions a poor proxy in sample after 2011 and would make Smith's (2016) model difficult to expand to these data as it relies primarily on corruption convictions for its identification within judicial districts which exhibit corruption.

Smith (2016) discusses that the metrics of corruption can be potentially unreliable within a district which could potentially include a corrupt judiciary and/or under-funded enforcement. Given this statement and the finding by Babenko, Fedaseyeu, and Zhang (2018) which indicates a support for potential collusion between politicians and corporation, it is posited that the nature of corruption behavior has changed and that when examining the data beyond 2011, the relationships found by Smith (2016) would not be present.

COMPUSTAT and PIN data from 1998-2016 are used and then matched to its corresponding judicial district. The sample is split into two sub-periods 1998-2016 and 2011-2016 to illustrate the difference in the periods pre- and post- Citizens United. Univariate tests and OLS modeling are conducted on both samples. Remarkably similar relationships are found with Smith (2016) over the whole sample, however, these relationships are then absent for the 2011-2016 period. Firms in corrupt districts do not hold less cash and do not take on more debt post-Citizens United compared to non-corrupt districts according to the PIN conviction rates. The conclusion is that the changing nature of the legal environment provides firms the opportunity to engage in corruption behavior by seeking favors from politicians in return for rents and that this corruption behavior is not reflected by PIN data owing to the lack of convictions that result of a behavior that, since 2011, has been deemed as legal. Given these results, it is evident that corruption conviction is not a sufficient proxy to capture corruption given the decreased scrutiny that corruption behavior has been afforded since the Citizens United decision.

LITERATURE REVIEW

Empirical research on corruption is not new. The issue of influence of bribery and other illegal activities of public officials and the abuse of public office for personal gain are major concerns for government. These areas are also of interest to finance and economics research due to the potential influence on both fiscal policy and economic conditions.

Lambsdorff (1999) conducted a review of different influences on corruption variables. Lambsdorff's review endorses subjective assessments of the level of corruption in various jurisdictions, by contending that perceptions are commonly a reliable indicator of the actual level of corruption. Lambsdorff, however, advocates for the consideration of data-driven approaches using socio-economic data, such as corruption convictions to develop a more accurate economic model through the formation of better informed policies.

La Porta et al. (2002) argue that corrupt officials raise funds through loans from financial institutions much more easily if corruption is common. Additional researchers support the intuition that firms with higher debt levels discourage rent-seeking behavior due to the difficulty of acquiring the desired rents owing to the appearance of being unstable (Bronars and Deere, 1991; Perotti and Spier, 1993). From this perceived instability, rent-seeking politicians would avoid rent-seeking from these firms due to the increased risk of a firm relocating, declaring bankruptcy or observing large reduction in investor returns, which would increase the chances of being caught, thereby reducing the probability of re-election (Aidt, 2003; Stulz, 2005).

The findings of Smith (2016) align with the presumption that firms would manage their financial policies downward, meaning they hold less cash and take on greater leverage to reduce exposure to rent-seeking politicians. These behaviors imply that districts with higher rates of corruption would have firms that exhibit less liquidity.

> I cannot count only those convictions that plausibly affect the firm. That is, included in the variable are offenses not likely to directly impact the firm, e.g., election crimes or other crimes of a strictly political nature. (Smith, 2016, p.353)

This study further postulates that same trepidation would also be present in the analysis of the current dataset. The underlying assumption is that various types of corruption would exhibit positive correlation. Therefore, districts with high exhibited corruption convictions are assumed to have a culture conducive to corruption. These assumptions have been made in previous studies, [Fisman and Gatti (2002), Glaeser and Saks, (2006); Butler, Fauver, and Mortal, (2009)]. These assumptions provide a rationale for a change in the strength of the relationship presented by Smith (2016), which may be exhibited owing to the difference in the nature of corruption convictions not being an accurate proxy measure for corruption, given the legal and legislative changes over the past 20 years.

Since the 2008 financial crisis, various legislative enactments have altered the implications of corruption pertaining to firms. In 2008, Sarbanes Oxley legislated higher standards of disclosure for firms, financial reporting, and the relationship between auditing and consulting to name but a few of the more substantive changes. This legislation was meant to protect investors and taxpayers from undue risks caused by information purposefully withheld. Goel (2014) demonstrates that the growth in PACs is positively related to greater corruption.

In 2011, following the Supreme Court's decision on Citizens United, donations up to $10,000 were allowed per PAC (Kennedy, 2010). Moreover, the ruling also prevented federal regulation of money donated to and spent on behalf of political campaigns by both corporations and labor unions. There is also differential treatment of PACs and Super PACs during election cycles when donations may be limited but spending on behalf of a campaign is not. Following these rulings, it would be expected that the relationship of corruption should change as politicians now have a method to solicit donations from corporations using legal means (Babenko, Fedaseyeu, and Zhang, 2018). Consequently, the relationship between rent-seeking politicians and firms is likely to have changed as firms may be less inclined to

manage their financial policies, as explored in Smith (2016) over the period 1980-2003. If firms donate to campaigns using PACs, they must solicit funds from individual employees (Babenko, Fedaseyeu, and Zhang, 2018). The CEOs of these firms would choose to back and fund (through a PAC) a politician who is anticipated to be beneficial to the firm's performance. A change in the nature of the rent-seeking relationship could go from exploitation to negotiation, as corporations may now seek a politician that presents the best potentially profitable prospects.

When a firm backs a politician through PACs, it is not only a legal way of buying influence, it is also beneficial to the firm, albeit indirectly. Should the politician be elected to office, the firm that backed the respective campaign would experience an increase in their financial performance through securing of government contracts or other desirable favors (Babenko, Fedaseyeu, and Zhang, 2018). Consequently, there is no illegality, though the behavior is much the same as the consideration of corruption in many studies, including the current one.

In addition, prior to the Citizens United decision in 2011 and the creation of the American Anti-Corruption Act (ACAA), politicians were not subject to stringent regulation. Previously, they had been subject to the more restrictive 1979 amendments to the Federal Election Campaign Act (FECA) (Woolley and Peters, 2008). The FECA amendments prescribed that political parties may spend unregulated amounts of soft and hard money on activities, such as party building and non-federal administrative tasks. However, later that year, FECA was extended to include the practice of candidate-related issue ads. As PACs were limited in their functioning from 1974 until Citizens United in 2011, politicians would need to seek other methods to fund their campaigns outside of party contributions. Accordingly, rent-seeking could be an attractive option. Since 2011, and the implementation of the ACAA, rent-seeking politicians have experienced increased hurdles in illegal rent-seeking, including a specific framework how election-fundraising is legally conducted. A list of areas where the ACAA anti-corruption resolutions have been enacted and put into force is on the ACAA's website.

Owing to increased disclosure requirements, coupled with the reduction in total convictions across the US from 1998-2016, politicians have either engaged in rent-seeking behavior less frequently or, conversely, there has been an increase in the proficiency of concealing the activity. Due to the increased power of PACs and Super PACs, and their role on campaign spending, the politicians using rent-seeking to acquire campaign contributions have likely changed. However, negotiating with firms is costly, both in terms of time and costs of the deal. It would be tempting for a politician to engage in rent-seeking behavior if they deemed the costs of negotiation to be excessive. Consequently, the data should demonstrate the same pattern as Smith (2016). If the relationship were to change, Lambsdorff (1999) argues that convictions per 100,000 as the sole proxy for corruption behavior would be insufficient and additional proxies should be employed to gain an enhanced picture of corruption relationships, which would understandably include socio-economic indicators.

DATA AND METHOLOGY

The pre-Citizens United period 1998-2011 is analyzed. These years may not be fully representative of corruption behavior, given the changes arising in 2011. Additionally, within the 2011-2016 timeframe various businesses in certain judicial districts are underrepresented. Expanding the time period, however, reduces the impact of missing judicial districts. Standard Industry Classification (SIC) numbers are used as the industry control measurement following Smith (2016).

The COMPUSTAT data for 1998-2016 were scanned for non-reporting of income, dates, or zip codes. Additionally, these data were examined for firms that report negative assets or sales. With these observations omitted, the resulting sample size is 111,061 entries. Yearly PIN corruption conviction values are obtained from the DOJ website, which maintains conviction data for the 94 U.S. district courts. Those values are then recalculated to reflect convictions per 100,000 for each year to account for the different population sizes between districts. These yearly values are then manually matched to the respective judicial districts using zip codes to the corresponding Federal Information Processing Standard (FIPS) codes, creating a panel dataset as a proxy for corruption behavior.

For the DOJ conviction measures, the 94 federal justice districts are used to examine the data. Smith (2016) follows this procedure, as it allows for within state variation, which adds power to the analysis, which is necessary given that these districts face similar tax codes and economic conditions, yet potentially divergent enforcement. Accordingly, this method allows for different levels of corruption to be exhibited within a single state.

The SIC codes of 6000-6999 and 4900-4999 are omitted due to government mandated liquidity ratios that would affect the data. The cash, ln (cash/net assets), net working capital, sales turnover, cash dividends, market value, and long term debt were winsorized at 1% and 99% to prevent errors and outliers in the dataset that would adversely affect the results.

This study replicates the Smith (2016) measures by using OLS regressions, but has a dual timeframe (1998-2016 and 2011-2016) to determine if the relationships have changed significantly over time. This analysis measures differences between corrupt and non-corrupt districts using univariate tests to compare variables, such as cash, net working capital, EBIT, and ln (cash/net assets) to determine if there are significant differences between firms headquartered within these districts and if the conclusions still support the shielding hypothesis (Smith, 2016) in both time frames. Firms are coded as being in a corrupt district if they fall in the top quartile of the district median scores.

The study then uses regression models for cash holdings observations in a state-by-state analysis. In the regression model, the study considers convictions per 100,000 as the proxy for corruption and uses ln (cash/net assets), long term debt, market value, net working capital, cash dividends, and sales, as firm level control variables. The OLS regression models are employed to test for a statistically significant relationship between the firms' controls and corruption convictions, with several regressions performed using various control combinations to test for the significance of corruption convictions. Only those that maintain significance in the final regression, when all controls are applied, are reported.

RESULTS

In the following tables, the standard errors are clustered by state and time to ensure robustness to unspecific time and state correlations. Table 1 consists of the descriptive statistics for comparison with previous studies. It contains the control variables and the variable of interest in this study.

TABLE 1
DESCRIPTIVE STATISTICS

This table reports the summary statistics for the main variables of interest and controls. This sample contains COMPUSTAT firm years from 1998-2016 which includes headquarter location data, as well as book value of assets, sales data, and accounting data. Missing data further trimmed the data values which are winsorized at 1% and 99%. Financial firms (SIC 6000-6999) and utilities (SIC 4900-4999) are excluded from the sample which leaves an unbalanced sample of 86,063 firm-year observations. Panel A shows the descriptive statistics for the whole sample while Panel B shows the descriptive statistics for the sample post-Citizens United (2011-2016).

Panel A (1998-2016):

	N	Mean	Median	Standard Deviation	Minimum	Maximum
Convictions Per 100,000	85,964	0.133	0.079	0.159	0	2.67
Cash	85,964	123.90	11.084	389.25	0	2865
Ln(Cash/Net Assets)	83,609	0.307	0.231	0.272	-0.435	2.125
Leverage	85,964	1.80	0.418	5.573	0.0032	45.09
Market-to-Book	85,964	6.24	3.34	8.536	-6.94	43.68
Cash Flow	84,397	23.80	0	107.56	0	857
NWC	84,284	2.972	1.89	3.761	0	25.27
Capital Expenditure	85,421	83.56	4.115	287.48	0	2146.37
Negative NI Dummy	85,925	0.489	0	0.500	0	1
Dividend Dummy	85,964	0.315	0	0.464	0	1
Dividends	85,732	23.52	0	104.86	0	834
Net Income	85,964	66.37	0.135	339.34	-556	2533
R&D	73,868	-.208	0	1.503	-13.3	0
Acquisitions	82,926	30.19	0	121.53	-1.228	922.47
Sales	85,964	1462	110.05	4606.25	0	34209
PPE	84,885	880.12	39.69	3061.39	0	23147
EBITDA	85,716	209.04	6.47	726.73	-106.778	5460

Panel B (2011-2016):

	N	Mean	Median	Standard Deviation	Minimum	Maximum
Convictions Per 100,000	22,096	0.118	0.0735	0.147	0	1.125
Cash	22,096	200	23.49	510.29	0	2865
Ln(Cash/Net Assets)	21,479	0.352	0.282	0.281	-0.435	1
Leverage	22,096	1.41	0.386	4.55	0.0032	45.09
Market-to-Book	22,096	7.69	3.75	4.55	-6.94	43.68
Cash Flow	21,818	41.03	0	142.72	0	857
NWC	21,688	2.991	1.89	3.87	0	25.27
Capital Expenditure	22,015	122.10	6.87	359.48	0	2146.37
Negative NI Dummy	22,089	0.485	0	0.500	0	1
Dividend Dummy	22,096	0.343	0	0.475	0	1
Dividends	22,019	40.37	0	139.43	0	834
Net Income	22,096	110.82	0.328	427.95	-556	2533
R&D	22,078	-0.096	0	153.56	-13.3	0
Acquisitions	21,386	44.75	0	153.56	-1.228	922.47
Sales	22,096	2083.8	193.9	5597.13	0	34209
PPE	21,654	1328.1	75.40	3848.88	0	23147
EBITDA	22,053	316.07	12.71	910.78	-106.778	5460

Table 2 provides univariate test statistics by level of corruption for the financial policy and control variables. The statistics are generated by determining the firm's median value for each variable and then averaging by level of corruption. This analysis is conducted in order to avoid interpretation issues caused by a large sample size with the same firm repeated many times in yearly observations. For the corruption proxy, the median of the time series of convictions for each district is taken and the district median is compared to the other district medians. Using this process, corrupt districts are in the top quartile of convictions based on district medians for convictions, while non-corrupt districts are in the bottom quartile.

The results of Table 2 indicate that differences in the means of the financial policy variables are large and statistically significant. Across the whole sample, firms with headquarters in corrupt districts carry less cash, have lower market values, carry more debt, have a higher sales/turnover ratio, and have a higher EBIT. The results in Panel A are consistent with the shielding hypothesis of Smith (2016), implying that firms reduce liquidity to limit expropriation. This study further supports Smith (2016) as both studies demonstrate that liquidity is reduced through significantly higher debt in corrupt districts compared to non-corrupt districts. Also consistent with the Smith (2016) study, these results remain inconsistent with the liquidity hypothesis.

However, when examining the 2011-2016 period, the shielding hypothesis becomes inconsistent with Smith's (2016) study, as no significant difference for firms in their cash levels was found. In comparing corrupt to non-corrupt districts, the firms also do not carry more debt in this subsample, which they did over the entire sample (1998-2016). This study also finds that the lower EBITDA relationship disappears and that firms still have lower market values, higher sales/turnover ratio. However, these relationships are weaker in the subsample of post-Citizens United.

Both studies find that dividends were not significant. Smith (2016) acknowledges that the differences in the current data could either be driven by corruption or they could be the result of unobserved heterogeneity between firms.

TABLE 2
DESCRIPTIVE STATISTICS BY CORREUPT AND NON-CORRUPT DISTRICTS

This table reports the average values in corrupt and non-corrupt districts. The statistics are calculated using the firm's sample median value for each variable and then average those medians by level of corruption in the corresponding year. Firms are coded as having headquarters in corrupt districts if their district median is in the top quartile of district medians given their corruption score for the corresponding year. This sample in Panel A contains COMPUSTAT firm years from 1998-2016 which includes headquarter location data, as well as book value of assets, sales data, and accounting data. Panel B contains the same data as Panel A but for the adjusted timeframe of 2011-2016. Missing data further trimmed the data values and they are also winsorized at 1% and 99%. Financial firms (SIC 6000-6999) and utilities (SIC 4900-4999) are excluded from the sample which leaves an unbalanced sample of 86,063 firm-year observations for Panel A and 22,096 firm-year observations for Panel B.

Panel A: 1998-2016

	Non-Corrupt Districts	Corrupt Districts	T-Stat
Cash	126.72	114.86	3.77***
Ln(Cash/Net Assets)	0.312	0.289	10.52***
Leverage	1.73	2.032	-6.74***
Market-to-Book	6.12	6.66	-7.89***
NWC	3.04	2.74	9.74***
Dividends	23.46	23.76	-0.35
PPE	862.09	941.61	-2.30***
Capital Expenses	84.08	82.04	0.87
Cash Flow	23.78	23.90	-0.14
R&D	-0.235	-0.119	-8.88***
Sales	1423.55	1590.81	-4.50***
EBITDA	207.74	213.76	-1.02
Acquisitions	29.74	31.70	-1.96**

Panel B: 2011-2016

	Non-Corrupt Districts	Corrupt Districts	T-Stat
Cash	201.67	194.68	0.85
Ln(Cash/Net Assets)	0.356	0.336	4.36***
Leverage	1.43	1.38	0.68
Market-to-Book	7.50	8.38	-5.21***
NWC	3.05	2.79	4.07***
Dividends	40.24	40.96	-0.35
PPE	1307.97	1403.07	-1.51
Capital Expenses	123.25	118.25	0.86
Cash Flow	40.97	41.35	-0.16
R&D	-0.111	-0.0420	-4.12***
Sales	2020.32	2312.29	-3.22***
EBITDA	311.57	332.88	-1.44
Acquisitions	44.16	46.91	-1.09

The results in Table 3 show the regressions that used held cash as the dependent variable. The models contain time-dependent indicators. The first model contains no controls aside from convictions per 100,000. The coefficient is -1.658, which is not significant.

TABLE 3
CASH HOLDING AND CORRUPTION

This table shows the OLS results which estimate how cash holdings vary with corruption convictions per 100,000. The sample contains all COMPUSTAT firms years from 1998-2016 in Panel A which includes headquarter location data, as well as book value of assets, sales data, and accounting data. Panel B contains the same data as Panel A but measures the timeframe of 2011-2016. Missing data further trimmed the data values and they are also winsorized at 1% and 99%. Financial firms (SIC 6000-6999) and utilities (SIC 4900-4999) are excluded from the sample which leaves an unbalanced sample of 85,964 firm-year observations for Panel A and 22,096 firm-year observations for Panel B. Industry fixed effects are indictors for the SIC code. Standard errors are in parenthesis below each coefficient estimate. In these tests, the errors are heteroscedasticity robust which are clustered by industry. *, **, and *** denote significance at the 10%, 5%, and 1% level respectively.

Panel A: 1998-2016

Dependent variable is Cash	(1)	(2)	(3)
Convictions per 100,000	-2.16	8.80	5.97
	(8.36)	(6.88)	(18.66)
Market-to-Book		2.97***	3.13***
		(0.143)	(0.577)
Leverage		1.13***	1.13**
		(0.200)	(0.482)
Cash Flow		0.324***	0.321*
		(0.063)	(0.182)
NWC		2.34***	2.72***
		(0.285)	(0.769)
Capital Expense		0.435***	0.439***
		(0.01)	(0.111)
R&D		-23.55***	-23.786***
		(0.731)	(2.99)
Acquisitions		0.277***	0.274***
		(0.009)	(0.066)
Dividends		1.03***	1.04***
		(0.064)	(0.213)
Negative NI Dummy		-22.16***	-20.71***
		(2.38)	(5.85)
R&D missing Dummy		0	0
Intercept	218.84***	58.57***	23.42
	(6.62)	(5.34)	(18.04)
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	No	No	Yes
N	85,964	68,582	68,582
Adj. R-Squared	0.0228	0.4991	0.5010

Panel B: 2011-2016

Dependent variable is Cash	(1)	(2)	(3)
Convictions per 100,000	9.41	38.06	29.98
	(23.46)	(36.01)	(32.44)
Market-to-Book		3.88***	4.00***
		(0.723)	(0.728)
Leverage		0.951	.994**
		(0.754)	(0.774)
Cash Flow		0.133	0.121
		(0.465)	(0.461)
NWC		2.48***	3.12***
		(0.714)	(0.962)
Capital Expense		0.413***	0.421***
		(0.133)	(0.128)
R&D		-30.20***	-31.02***
		(6.26)	(6.22)
Acquisitions		0.335***	0.330***
		(0.0705)	(0.069)
Dividends		1.373***	1.388***
		(0.470)	(0.467)
Negative NI Dummy		-31.27***	-27.40***
		(8.99)	(9.39)
R&D missing Dummy		0	0
Intercept	217.58***	45.07***	-.873
	(9.06)	(14.35)	(26.10)
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	No	No	Yes
N	22,096	20,738	20,738
Adj. R-Squared	0.0001	0.5336	0.5354

In the second model, the firm policies of net working capital and sales/turnover ratio are introduced. The coefficient on the corruption measure is not significant. In the third model introduces the full set of controls including industry and fixed effects. The corruption measure's coefficient is 5.97, which is not significant. The adjusted R^2 increases considerably across models 1, 2, and 3, from 0.0228 to 0.4991, to 0.5010, which suggests the addition of a powerful set of controls. Net working capital, long-term debt and market value are positively related to firm cash holdings, while other controls are not significant.

The industry controls used in the current study suggest that a particular industry does not drive this association. None of the tests in the OLS regressions finds statistical significance for the convictions per 100,000 control, thus failing to reproduce the results of Smith (2016) over the period 1998-2016. The table following for 1998-2016 results shows the results from 2011-2016 to coincide with the Citizens United ruling. The corruptions per 100,000 values from the 1998-2016 and 2011-2016 models are both insignificant. Smith's (2016) results for cash holdings were weakly significant, as a result, we examine the stronger finding for leverage from his results.

Table 4 shows the results for the leverage of firms. For the corruption coefficient in the second model Market-to-Book, PPE, sales, and EBITDA are all controlled. The corruption coefficient is 0.476, which is significant. In the third model industry fixed effects are controlled resulting in a corruption coefficient of 0.467, which is also significant. In this model, samples from 2011-2016 are shown in Panel B. For all these models, using the same specifications as before, the corruption per 100,000 coefficients are

insignificant for all three models in this sample period. This result, specifically for the 2011-2016 period, fails to reproduce the results from Smith (2016). Again, as in our univariate tests, we find that the post-Citizens United period is not consistent with the shielding hypothesis. Further, convictions are inconsistent in all three models.

TABLE 4
LEVERAGE AND CORRUPTION

This table shows the OLS results which estimate how leverage varies with corruption convictions per 100,000. The sample contains all COMPUSTAT firms years from 1998-2016 in Panel A which includes headquarter location data, as well as book value of assets, sales data, and accounting data. Panel B contains the same measures but for the timeframe of 2011-2016. Missing data further trimmed the data values and they are also winsorized at 1% and 99%. Financial firms (SIC 6000-6999) and utilities (SIC 4900-4999) are excluded from the sample which leaves an unbalanced sample of 85,964 firm-year observations for Panel A and 22,096 firm-year observations for Panel B. Industry fixed effects are indictors for the SIC code. Standard errors are in parenthesis below each coefficient estimate. In these tests, the errors are heteroscedasticity robust which are clustered by industry. *, **, and *** denote significance at the 10%, 5%, and 1% level respectively.

Dependent variable is Leverage	(1)	(2)	(3)
Convictions per 100,000	0.428**	0.476**	0.467**
	(.206)	(.197)	(0.194)
Market-to-Book		-0.10***	-0.10***
		(0.01)	(0.01)
PPE		0.001***	0.001***
		(0.00)	(0.00)
Sales		0.001***	0.001***
		(0.001)	(0.182)
EBITDA		-0.001***	-0.001***
		(0.000)	(0.000)
Intercept	1.47***	2.07***	1.99***
	(0.180)	(0.227)	(0.399)
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	No	No	Yes
N	85,964	68,582	68,582
Adj. R-Squared	0.0118	0.0406	0.0407

Dependent variable is Leverage	(1)	(2)	(3)
Convictions per 100,000	-0.160	-.0928	-0.1009
	(.347)	(0.295)	(0.2776)
Market-to-Book		-0.055***	-0.100***
		(0.093)	(0.0096)
PPE		0.0002***	0.0002***
		(0.00004)	(0.00004)
Sales		0.0003**	0.00004**
		(0.0001)	(0.00002)
EBITDA		-0.0009***	-0.0009***
		(0.0002)	(0.0002)
Intercept	1.54***	1.82***	1.79***
	(0.196)	(0.232)	(0.419)

Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	No	No	Yes
N	22,096	21,637	21,637
Adj. R-Squared	0.0022	0.0285	0.0407

DISCUSSION

This study shows that Smith's (2016) shielding hypothesis does not hold post-Citizens United. The current study finds that for the whole sample, firms in corrupt districts reduce their liquidity and hold less cash, when compared to non-corrupt districts in pre-Citizens United samples. However, the regression models do not show that corruption convictions as statistically significant over the time period after Citizens United. Further, this study supports the statement from Smith (2016) that changing legislation and environments over time have led to a change in the strength of corruption convictions as a proxy for measuring corruption. Specifically, given the changes in legislation and treatment of PACs and super PACs in the US political system, this study finds that over time these circumstances have changed the relationship of rent-seeking between corrupt politicians and firms, thereby driving convictions downward. This study was conducted under the assumptions that the spending, with the lack of regulation and oversight of political spending given the 2011 change in legislation, could provide an alternative, legal pathway for corruption to lower the number of corruption convictions.

These findings indicate that a culture of corruption creates differences for financial policies of companies between corrupt and non-corrupt districts. However, the number of convictions, when controlled for firm financial policies, along with time and industry variables, do not exhibit statistical significance. Districts may actually remain corrupt; however, the rate of convictions may not fully capture this effect. As a result, posit that firms are likely still modifying their financial policies to take advantage or to mitigate against political corruption and rent-seeking behavior, but corruption convictions are no longer a useful measure in differentiating districts to illustrate this effect.

As demonstrated earlier, corruption convictions have declined over time, which could be the result of changing legislation, increasing effectiveness in corruption activities, or an increase in extorting funds through employees of companies by legal PACs or Super PACs. Since Citizens United, there are more methods by which firms can donate to political campaigns. Further, Super PACs may engage in unlimited spending for political campaigns, which can be a powerful tool for firms to purchase political goodwill (Babenko, Fedaseyeu, and Zhang, 2018).

Since 1980, corruption has been studied extensively, and as a result of a broader and stronger lens, engaging in corruption has carried a risk of arrest and loss of re-election. Consequently, corruption behavior would only be logically engaged in if the behavior were relatively secure. Over time, it can be posited that one reason for the decrease in corruption convictions is that politicians have become more proficient at these behaviors, either because of better technology or simply by using methods, such as circumventing legislation/legislative processes that would lead to their conviction. Additionally, while the legislation dictating the creation of PACs began in the late 1970's, these PACs were not adequately empowered until Citizens United. Further, the mechanism by which firms engage in this corruption behavior changes over time. The results from 1998-2016 in Table 4 indicate that leverage is significantly positive, thus providing support for the shielding hypothesis. This result is consistent with firms reducing their liquidity in order to discourage corruption behavior from targeting firms. The result in the sample period of 2011-2016 does not show this same relationship. This study concludes that the changing legislature surrounding political contribution has changed the relationship of political relationships that firms hold. As a result, firms likely have abandoned the behavior of taking on debt in order to reduce their liquidity.

A caveat that this study outlines in its findings concerns an underlying assumption in Smith (2016). Goel and Nelson (1998) report a significant, positive association between the variables of public spending

per capita and corruption convictions. Goel and Nelson (1998) argue that state intervention and public spending give rise to rent-seeking behavior. However, it should be considered that with more funding and more resources judicial districts may confirm more convictions, which may question the proxy used in Smith (2016). This study is reliant on the assumption that judicial court convictions are indicators of a culture of corruption. If the efficacy of judicial district court convictions, as a proxy for corruption convictions, is called into question, a fundamental issue exists with the assumptions of both studies. Further, what must be a consideration within the assumption of a culture of corruption is that corruption in a given district may also lead to corruption inside a district court or may cause politicians to pass legislation that loosens oversight, regulation, or prosecutorial power in district courts. Consequently, corruption convictions as a proxy for corruption and corruption behavior may become suspect as a proxy when used in isolation.

As corruption convictions remain an easily accessible and consistently used measure, the likelihood of convictions to be regarded as an effective proxy should not be discounted lightly. However, from this study's findings, a consideration of variables as proxies for corruption, in addition to convictions themselves, would likely provide a more robust model than one solely based on convictions. Lambsdorff (1999) examines socio-economic variables and their correlative nature with corruption behavior. Including variables, such as poverty, inequality, public spending on policing and judicial processes, union membership, among others, would likely augment the model and make it more robust capturing the effect of previously enacted legislative changes.

The findings in the present study indicate that a broad array of variables could be used to provide a more robust explanatory model. In addition to maintaining corruption convictions as a proxy for corruption, poverty may also be an appropriate addition to the model. Poverty has been demonstrated to be positively correlated with corruption behavior (Lambsdorff, 1999). When considering the corruption rates in high poverty districts compared to lower poverty districts, a more complete picture of a district containing a culture of corruption can be formulated. This dichotomy would provide a view of the court system in conjunction with the socio-economic conditions that have been previously proven to be correlated with corruption. Additionally, corruption activity that leads to a culture of corruption is likely to manifest itself through a variety of socio-economic conditions. As the benefits of corruption are likely to flow to the wealthy at the expense of the impoverished, areas with high corruption are likely to experience higher levels of inequality. Gupta, Davoodi, & Alonso-Terme (2002) perform a cross sectional analysis of 37 countries and find that corruption is positively correlated with income inequality. Husted (1999) and Swamy et al. (2001) find that this relationship is self-reinforcing.

Another aspect that could enhance this study is the consideration of the rigor of competition and its effect on corruption behavior. Smith (2016), World Bank (1997) and Henderson (1997) have commented on the extent that corruption is explained by a low level of business competition. Smith (2016) found that businesses lowering their cash flow and taking on more debt and leverage were methods to shield themselves from rent-seeking behavior. However, in areas with greater competition, these policies may not be implemented without the consideration of other businesses vying for market share (World Bank, 1997). Additionally, healthy economies that promote competition have been shown to lower the rents of economic activities and, therefore, would reduce the motive of politicians to engage in corruption behavior (Henderson, 1999).

In addition, Smith (2016) assumes that the culture of corruption would also be explained by differing government styles. Given legislative changes and the empowerment of PACs over time, this study acknowledges that these events alter the manner of government interactions with the society it governs. For example, Treisman (1999) found evidence that governmental styles that have less centralized oversight systems are more corrupt than centralized ones. Smith (2016) referenced Fisman & Gatti (2002) who argued that the decentralization of government and corruption behavior have a strong negative relationship. With the passage of particular legislation, interpretation by the courts, differential capacities of judicial districts to monitor and convict corruption cases, as well as different degrees of competition, and styles of government, it is highly unlikely that corruption convictions alone would act as an effective proxy to adequately measure corruption. Furthermore, Fisman and Gatti (2002) contend that the federal

legal oversight for corruption may not necessarily be interpreted and handled inside state level jurisdictions, as these jurisdictions may be more susceptible to influence from corruption culture or perhaps even a differential in terms of how a state manages corruption behavior.

CONCLUSION

This study investigates how US political corruption relates to firm financial policies by conducting a similar study to Smith (2016) by expanding the sample to include the post-Citizens United period. The shielding hypothesis of Smith (2016) posits that firms shield themselves by decreasing cash and increasing debt obligations, which increases profit variability, while also limiting free cash flow (Matsa, 2010). Using DOJ data for the entire sample over 1998-2016, this study finds that firms headquartered in corrupt locales hold significantly less cash, hold more debt, have lower market values, have higher sales/turnover ratios, and higher EBITDA; consistent with Smith (2016). However, this study's findings differ from Smith (2016) as expected, in that firms do not appear to engage in shielding behaviour during our post-Citizens United period of 2011-2016. These findings indicate that firms, after the Citizens United ruling, do not carry less cash and do not take on more debt to reduce their liquidity to avoid being the target of rent-seeking behaviour.

The current study challenges the efficacy of using corruption convictions as a proxy for corruption after the passage of Citizens United. Additionally, it is postulated that through the models in this firms may no longer subscribe to the idea that they need to shield themselves from rent-seeking by holding less cash or taking on more leverage, although admittedly that this hypothesis requires more direct study.

This study underlines the non-significance of corruption convictions in our OLS model, when a larger set of significant controls is introduced. This relationship becomes particularly relevant when considering the concept of a culture of corruption and also when considering how the strength of convictions as a proxy for corruption that can vary both over time and by legal environment. Smith (2016) proposes that future research could focus on US corruption and present evidence on how and why firms pay bribes and by studying the channels through which bribes are paid. Indeed, neither this study nor Smith (2016) examines the measures on how politicians would punish deviating firms and if there would be any variability in method or severity given local economic conditions.

This study's main findings posit that corruption convictions may need to be combined with additional proxies to accurately gauge corruption. Future research should examine socio-economic variables and how they may affect a culture of corruption. Additionally, this study references recent work on how corporations pay bribes and some of the benefits of paying bribes manifest in greater firm value (Babenko, Fedaseyeu, & Zhang, 2018). This study is intended as the beginning of a subsequent stream of research that directly examines the motivations of firms to engage in corruption behavior.

These studies all underline the importance of further research when considering firm behavior toward rent-seeking by politicians. Based on more recent results, including these, it could be viable that certain firms with sufficient resources view paying bribes to corrupt politicians to be desirable based on potential rewards through the granting of contracts. Further, the empowerment of PACs could, at the same time, eliminate corruption behavior in the legal sense, but yet still have the same effects. Specifically, in reference to the disadvantage or punishment from not financing a politician's campaign. While this would normally manifest as a direct punishment, such as fines or delays in licenses, this relationship may have changed after Citizens United by encouraging firms to donate or spend on behalf of a politician's campaign through PACs or Super PACs through the awarding of government contracts to donating businesses. Firms that elect not to donate would be put at a disadvantage in the local market, due to their competitors receiving benefits that they do not. Meanwhile, this behavior of firms and politicians is legal and would not be considered corruption, although the effects are identical to corruption and the results the same absent the potential for conviction. Therefore, while corruption convictions continue to drop over time, the conditions that corruption creates are likely to persist. Research surrounding how firms perform financially in corrupt districts compared to non-corrupt districts is also merited, when considering the

awarding of government contracts given a firm's donation to a political campaign (Babenko, Fedaseyeu, and Zhang, 2018).

Further research could also focus on the differences in corruption behavior created by centralized governance compared to decentralized governance within a nation. Research could also be conducted on how to define corruption culture and which socio-economic variables are associated with higher corruption convictions and compare these to the previous relationships explored in prior research. Finally, in agreement with Smith (2016), this study acknowledges that more work on firm-level effects of corruption is warranted.

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