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Impact of Regulations on Firm Value: Evidence from the 2016 U.S. Presidential Election

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Abstract

Using the 2016 U.S. presidential election result as a shock to the expectations about the future regulatory environment, I find that most regulated firms earned approximately 4% higher cumulative abnormal stock returns than least regulated firms during the first 10 trading days after the election. Exploring economic mechanisms, I find evidence consistent with the explanation that more regulations disproportionately harm high-growth firms and allow incumbent firms to extract rents through lower competition and political favoritism. Stock returns are also followed by a shift in firm fundamentals over 3 years after 2016, consistent with the economic mechanisms.

I. Introduction

It is widely believed that there has been a significant increase in government regulations over the past decades, both in the United States (U.S.) and around the world (Shleifer (2005), Davis (2017)). The increasing pervasiveness of the regulatory state in the U.S. is also reflected in the page count of the Code of Federal Regulations (CFR), which has increased from 20,000 in 1950 to almost 180,000 in 2016 (Davis (2017)). Presumably, the regulatory environment also changes when different political parties assume power at the White House, with Republican presidents likely to be less in favor of government regulations than Democratic presidents. Unsurprisingly, companies spend billions of dollars developing political connections (such as by lobbying policymakers, making campaign contributions, etc.) to relax regulatory burdens on them and thereby maximize shareholder value. However, it is not clear whether changes in regulations themselves impact firm value. For example, it is not clear whether positive returns to political connections and lobbying are due to changes in regulations or less enforcement of regulations (Yu and Yu (2011)), or due to government support in other forms (Duchin and Sosyura (2012)). In this study, I attempt to fill this gap by providing empirical evidence of the effect of expected regulatory changes on firm value.

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A priori, it is not clear how regulatory changes impact firm value. On the one hand, an expansive regulatory environment can benefit incumbent firms by increasing their innovativeness over the long term (Porter (1991)), shielding incumbent firms from competition (Djankov, LaPorta, Lopez-De-Silanes, and Shleifer (2002), Gutiérrez and Philippon (2019)), and by enabling incumbent firms to capture the regulators (Stigler (1971), Posner (1974)). In this case, deregulation in the future could harm the value of incumbent firms today. On the other hand, an increase in regulations can impose an additional burden on firms by increasing their compliance costs, limiting their growth (Djankov, McLiesh, and Ramalho (2006), Dawson and Seater (2013)), and reducing their innovativeness compared with other less regulated firms (Jaffe, Peterson, Portney, and Stavins (1995)). Such a line of argument suggests that future deregulation benefits the shareholders of incumbent firms.

Various empirical challenges have inhibited scholars from exploring the above possibilities. First, regulation is an abstract concept that is difficult to quantify. Additionally, the incidence of regulatory burdens is largely unobserved for the vast majority of firms in the economy. Many studies consider that certain industries, such as the utility and finance industries, are regulated. However, firms in other industries may be equally or more strictly regulated, such as firms in the pharmaceutical industry or manufacturing-related industries. Jaffe et al. (1995) and Berman and Bui (2001) highlight some of these issues. Second, and perhaps most importantly, regulatory changes take effect over long periods involving public debates and votes. Thus, it becomes difficult to identify when economic agents incorporate expected changes in regulations into their actions (Binder (1985)).

In this study, I use an empirical setting that addresses these empirical challenges. First, in line with studies such as Snowberg, Wolfers, and Zitzewitz (2007), assuming stock markets are efficient in processing forward-looking information, I use the short-term abnormal price reaction of a firm's listed common equity to measure the expected economic benefits of future lower regulations that investors expect to accrue to the firm. However, this approach only allows for valid inference if the date of changes in expectations of investors about the future cash flows is known reasonably accurately. Hence, I use the 2016 U.S. presidential election result as an exogenous shock to the expectation about the future federal regulatory policy in the U.S. As noted in Wagner, Zeckhauser, and Ziegler (2018a), (2018b) and Child, Massoud, Schabus, and Zhou (2020), Trump's election was largely unexpected. Such an unexpected nature of the event helps me to capture a more precise time for the change in the market's expectations. Additionally, deregulation was one of the top priorities of President Trump's election campaign; during his campaign, he published his "Contract with the American Voter," which specifically mentions rolling back two regulations for every new federal regulation introduced and reducing the role of government in the economy by imposing hiring freezes and budget cuts (Belton, Krutilla, and Graham (2017)). Finally, I use a novel measure of industry-specific regulation from the RegData database, as developed by Al-Ubaydli and McLaughlin (2017), to assign firms to different groups depending on the level of regulatory restrictions of their 3-digit North American Industry Classification System (NAICS) based industries.

Firms in the most regulated industries earned approximately 4% higher cumulative abnormal stock returns (adjusted by the Fama–French 5 factors and the momentum factor) during the first 10 trading days after the election results were declared than firms in the least regulated industries. This was equivalent to $25 million for an average firm (roughly $27 billion on aggregate) in the most regulated industries. These results are obtained after controlling for various firm characteristics (i.e., market capitalization, illiquidity, capital structure, expected tax rates, import dependency of an industry, political connections, and size), which account for potential omitted variables that could also shape investors' expectations around Trump's election. For example, Wagner et al. (2018a) document that investors expected lower tax burdens for firms after Trump's election. Following similar logic, controls such as import dependence and exposure to government spending in an industry account for a possible shift in investors' expectations related to Trump's foreign trade policy and infrastructure spending, respectively. Additionally, the specifications take into account state-fixed effects and broader industry-fixed effects. Hence, I account for any state-level or broader industry-wide unobserved factors that may potentially bias my estimates. To understand how the fixed effects might be useful in accounting for potential omitted variable bias, let us consider the case of the construction industry (with the 2-digit NAICS code of 23). It is plausible that Trump's ambitious plans for infrastructure spending could have been good news for investors in the construction sector. But, I employ a NAICS 2-digit industry fixed effect that compares firms within the construction sector that are allocated to differently regulated granular industries.

Although I control for several firm characteristics, it is still possible that Trump's election changed investors' expectations about unobserved factors other than regulations. For example, the current specifications cannot account for *within*-industry heterogeneity that is also correlated with the measure of regulation and a shift in investors' expectation after Trump's election. Let us consider the utilities sector (having the 3-digit NAICS code of 221). Within this sector, Trump's election may have been a positive shock to more polluting (fossil fuel-based) power producers and a negative shock to less polluting (renewable) power producers. I show below that the results remain unchanged after accounting for environmental regulations, firm-specific pollution, or climate responsibility (Ramelli, Wagner, Zeckhauser, and Ziegler (2021)). Furthermore, I also document that the results are robust to alternative specifications accounting for event-time clustering. While I acknowledge that it is difficult to rule out all possible alternative explanations, the robustness of the results alleviates such concerns to a large extent. Additionally, the results are an intention-to-treatment effect, that is, the abnormal reaction of the stock price is in expectation of deregulation in the future and not of deregulation itself.

I provide three potential economic channels through which investors might have expected most regulated firms to benefit. First, existing empirical evidence suggests that regulation affects economic growth by imposing various costs on doing business. For example, Alesina, Ardagna, Nicoletti, and Schiantarelli (2005), Djankov et al. (2006), Dawson and Seater (2013), Pizzola (2018), and Gutiérrez and Philippon (2019) and many others show that increased government regulation impedes economic growth by imposing costs on firms capitalizing on their growth opportunities. Hence, deregulation can be beneficial for high-growth firms, *especially* firms that are strictly regulated. I find evidence confirming the existence of this channel. That is, the positive returns of firms in the most regulated industries are driven by firms with ex ante higher growth opportunities, as measured by Tobin's Q. For example, a 1-standard-deviation higher Tobin's Q was associated with an approximately 1.5% increase in abnormal stock returns by the end of the 10-day event window for firms in the most regulated industries, compared with those in the least regulated industries.

Second, it is well-established that deregulation leads to an increase in competition. Early theories of regulation (Stigler (1971), Posner (1974)) suggest that one of the purposes of regulation is to thwart competition within each industry by "capturing" the regulators. As such, regulation serves the purpose of incumbents in an economy. Hence, significant deregulation can increase competition and thus affect the economic rents gathered by regulated industry incumbents. Thus, if deregulation is expected, firms facing low competition ex ante would likely lose the most in future. By using the text-based product market concentration measure developed by Hoberg and Phillips (2016), I find that the valuation of firms in the most regulated industries *and* with ex ante high product market concentration is negatively affected during the event window. That is, during the 10-day event window, for the most regulated firms, a 1-standard-deviation higher industry concentration was associated with an approximately 2.3% decrease in firm valuation relative to that of the least regulated firms.

Third, I find evidence consistent with political favoritism existing in regulatory policies. Political scientists have long noted that economic regulation is often moderated by the political environment of the regulated entities (Short (2019)). For example, a Republican federal government might be more inclined to enact less stringent regulations and/or impose laxer regulatory standards for firms located in Republican states than for firms located in Democratic states (Asher and Novosad (2017)). A decrease in overall regulations would make such favoritism less valuable for firms in Republican states. Consistent with this literature, I find that firms incorporated in states with more Republican members of Congress gained value after the election. In contrast, in line with the intuition that political favoritism is less valuable when deregulation is expected, firms that were in the most regulated industries and incorporated in Republican states experienced a negative abnormal stock return of approximately 4.1% during the 10-day event window compared with firms in the least regulated industries. These results show that political favoritism of regulators is one of the major mechanisms enabling incumbent firms to derive benefit for their shareholders.

In the last set of analyses, I study whether the documented initial stock-price reactions were followed by changes in firm fundamentals over the next few years. I perform a difference-in-differences type analysis over the sample period of 2014 to 2019. I investigate whether firms in the most regulated industries, and firms with ex ante higher growth opportunities, lower competitive threat, and in Republican states in the most regulated industries experienced any changes in their profitability, cash flow, sales, and sales growth during 2017–2019 compared with 2014–2016 *and* compared with other firms. I do find some evidence consistent with the initial stock price reaction. Although not invariably statistically significant, the evidence suggests there was a shift in the firm fundamentals of the most regulated firms during the Trump presidency, which lends additional support to the economic mechanisms documented above.

The primary contribution of this article is its documenting of evidence suggesting that changes in the federal regulatory environment have a first-order impact on firm value for firms in the most regulated industries. There is a literature on political connections and how they impact firm valuation (e.g., Faccio (2006), Ferguson and Voth (2008), De Figueiredo and Richter (2014), Akey (2015), Borisov, Goldman, and Gupta (2016), Child et al. (2020), and many others). However, most of these studies do not explicitly examine how regulated firms react to expected regulatory changes conditional on being politically connected. For example, Akey (2015) discusses anecdotal evidence suggesting there are regulatory benefits to firms but does not examine the impact of changes in regulations themselves. In a similar vein, Child et al. (2020) provide evidence on how S&P500 firms connected to Trump benefited from lower regulatory actions and penalties against them but do not examine how changes in overall regulations affected firm valuations. I complement this strand of literature by documenting direct evidence that the expectation of a more lenient federal regulatory environment had a positive impact on firm valuation particularly that of the most regulated firms.

My second contribution is to complement earlier studies that document the impact of regulation on broader macro-economic outcomes. For example, Dawson and Seater (2013) examine the trend of U.S. regulations and GDP growth, Gutiérrez and Philippon (2019) examine the sensitivity of firm entry based on the growth rate of its industries and how regulations moderate such sensitivities, and Simkovic and Zhang (2020) investigate innovativeness of new entrants based on regulations. I complement these studies and the earlier studies on regulatory changes by Schwert (1981) and Binder (1985) by providing direct firm-level evidence on how the expectation of a more lenient regulatory federal environment impacts incumbent firms.

The remainder of this article is organized as follows: Section II describes the institutional setting and data used in this study; Section III provides the main empirical results; Section IV provides evidence on various economic channels; Section V provides evidence on real effects based on firm fundamentals; and Section VI concludes.

II. Institutional Setting, Data, and Methodology

A. Institutional Setting

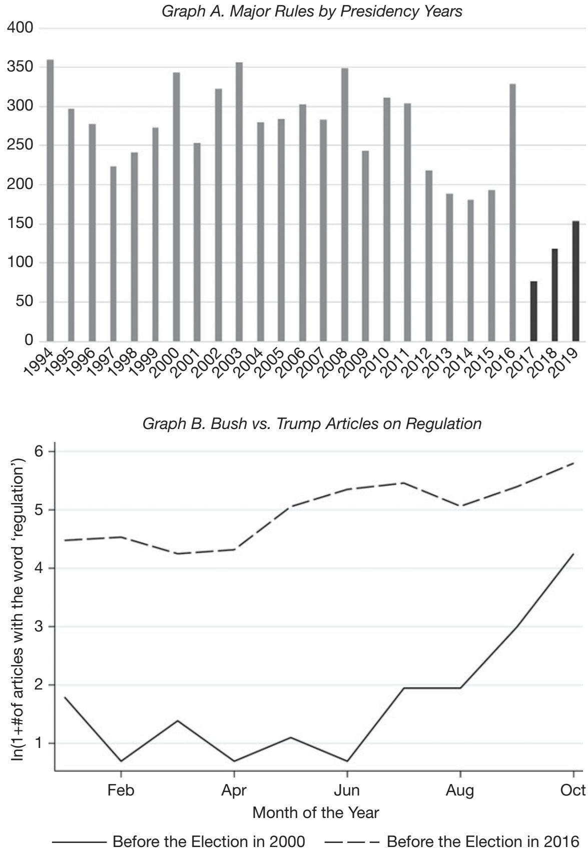
One of the reasons why this study focuses on Trump's election is that lowering federal regulations was a key platform of his election campaign. Consistent with his election campaign promise, immediately after assuming office, President Trump signed Executive Order 13771, entitled "Reducing Regulation and Controlling Regulatory Costs" which directed agencies to implement his campaign promise, as published in his "contract." According to the Office of Information and Regulatory Affairs and data collected by the Regulatory Studies Center of George Washington University, the number of "major" rules[1] published during the first 3

[1]1.0 A major rule is defined as "one that has resulted in or is likely to result in i) an annual effect on the economy of $100 million or more; ii) a major increase in costs or prices for consumers,

FIGURE 1

Comparison of the Emphasis on Regulations by Donald Trump

Graph A of Figure 1 plots the number of major rules published by each presidential year (Feb. 1 to Jan. 31) from 1994 to 2019 as published by the Office of Information and Regulatory Affairs. The vertical axis represents the number of major rules, and the presidential year is plotted along the horizontal axis. The darker bars on the right belong to the years under the Trump administration. Graph B plots the logarithm of the number of news articles pertaining to national "Presidential Elections" in the Factiva database that mentions the word "regulation" during the first 10 months (January to October) leading up to the elections in 2000 and 2016. The dashed (solid) line is for the election in 2016 (2000) leading to the victory of Donald Trump (George W. Bush).



years of Trump's presidency was one of the lowest in the past 25 years. Graph A of Figure 1 demonstrates this point. It plots the number of major rules per year from 1994 to 2019. The black bars indicate the years of the Trump administration. Additionally, in Table A2 in the Supplementary Material, I formally document that the number of such rules passed during the Trump presidency between 2017 and 2019 was 60% ($= \exp(0.48) - 1$) – 80% ($= \exp(0.60) - 1$) lower than in other years from 1981 to 2019. This finding is consistent with the report from Belton

individual industries, federal, state, or local government agencies, or geographic regions; or iii) significant adverse effects on competition, employment, investment, productivity, or innovation, or on the ability of the United States-based enterprises to compete with foreign-based enterprises in domestic and export markets."

and Graham (2019), which shows that there was slow but somewhat effective progress on deregulation under the Trump administration by the end of 2018 with 514 deregulatory rulemakings outstanding across various agencies. Moreover, the Trump administration kept a vast majority of the administrative positions empty as an indirect way to reduce the burden of regulations (Heidari-Robinson (2017)).

Was Trump's focus on lower regulations unique? It is believed that Republicans are much more against regulation than Democrats. For example, in a Dec. 2016 Pew Survey, 71% of Republican-oriented respondents agreed that government regulation of business is harmful, compared with 31% of Democrats (see https://www.pewresearch.org/politics/2016/12/08/3-political-values-government-regulation-environment-immigration-race-views-of-islam/). Hence, regulation might have been an equally important topic of discussion during other Republican presidential campaigns. However, I document below that this was not the case.

First, although columns 2 and 4 of Table A2 in the Supplementary Material suggest that compared with Democratic administrations, Republican administrations are associated with approximately $20\%(=\exp(0.192)-1)$ fewer major rules being passed, this is not statistically significantly different from the number of major rules passed by the Democratic administrations from 1981 to 2019. However, the number of major rules that were passed during the Trump administration was 60% lower than during other administrations, even after controlling for Republican presidencies in general. Second, to further establish this point, I collect English-language news articles from Factiva mentioning the word "regulation" related to the subject of national "Presidential Elections" in the United States during the Bush (2000) and the Trump (2016) presidential campaigns. I focus on the 2000 election as this election was also closely contested and resulted in a Republican president taking office. On average, there were 150 more articles in a given month mentioning the word "regulation" during the months leading up to the general election of 2016 than during the months leading up to the general election of 2000. Graph B of Figure 1 shows the difference in the number of articles on a logarithmically transformed scale in the months leading up to the election in each election year (i.e., a comparison of Jan. to Oct. 2000 with this period in 2016). As can be seen, on average, during this period, in 2016 there were three times as many news articles mentioning the word "regulation" than in 2000. In unreported results, I find that these estimates are statistically significant at the 1% level even after taking into account a month-of-the-year fixed effect.

Hence, the Trump administration indeed followed a more lenient policy on enactment and enforcement of regulations than other Republican and Democratic administrations. These results indicate that decreasing regulatory restrictions was one of the main platforms of the Trump election campaign and was a greater focus of discussion than during another closely contested presidential election campaign. Hence, the Trump election is analyzed as the cleanest setting in which a credible signal about possible deregulation in the future was sent to investors.

B. Regulation Data and Measurement

Measurement of regulation is in itself a complicated exercise. Some research in this field measures regulatory penetration using page counts of the CFR, as in

Dawson and Seater (2013).[2] Similarly, Mulligan and Shleifer (2005) use file size data of legislation in the U.S. as a proxy for regulatory intensity. Becker and Mulligan (2021) use different measures of regulation such as Congressional committee size and regulatory costs of various federal agencies as a percentage of GNP. However, for my cross-sectional test, I need a measure of regulation that differentiates between groups of firms based on their regulatory intensity. Hence, I use a recently devised NAICS 3-digit industry-specific measure of regulation that is based on textual analysis of the CFR (Al-Ubaydli and McLaughlin (2017)). The data are provided by the RegData project of Mercatus Center of George Mason University.[3] For an elaborate discussion of the measure, I refer the reader to Al-Ubaydli and McLaughlin (2017). I group industries in quartiles based on their regulatory restrictions. Table 1 provides examples of industries in each quartile.

Table 1 shows that the Al-Ubaydli and McLaughlin (2017) method largely aligns the industries in a way that one would expect ex ante. For example, most of the industries in the top quartile are likely to have higher regulations than the industries in other quartiles. For example, utilities, petroleum and coal products manufacturing, and chemical manufacturing are classified as some of the most regulated industries. This distribution of industries gives sufficient confidence that the method can be applied for further analysis.

C. Other Financial Data

Daily stock returns data are obtained from CRSP, and the accounting variables are taken from Compustat. The Fama and French (2015) factors and the momentum factors are obtained from Kenneth French's website. The industry definitions of the NAICS are taken from the United States Census Bureau website (https://www.census.gov/naics/). As control variables, I use variables that have been previously found to predict stock returns. Amihud (2002) shows that the illiquidity measure is related to stock returns through an illiquidity premium. The daily Amihud (2002) illiquidity measure is calculated using the absolute return scaled by the total dollar volume of shares traded on that day. I also control for firm size by taking the logarithm of total assets for the fiscal year ending 2015 and the market capitalization of a firm as of at the beginning of the event window. I obtain the data for the fiscal year ending 2015 from Compustat. Furthermore, as Bhandari (1988) and Welch (2004) find that a firm's stock returns are related to capital structure reflected by its debt-equity ratio, I also control for each firm's debt-equity ratio in the empirical setup. In addition, I control for expected cash tax rates as these are shown to have been an important driver of stock returns around the 2016 U.S. presidential election (Wagner et al. (2018a)). I calculate 1-year expected (effective) cash tax rates as given by equation (1):

[2] The CFR contains the most recent relevant regulations and is updated each year in four waves.

[3] I thank the Mercatus Center at the George Mason University and Omar Al-Ubaydli and Patrick A. McLaughlin for making the data available. Several recent studies use these data. For example, Gutiérrez and Philippon (2019) and Coffey, McLaughlin, and Peretto (2020).

TABLE 1

Example of Industries in Each Regulatory Quartile

Table 1 presents a list of 3-digit NAICS industries that form the top 10 in each regulated industry quartile. The industry definitions are taken from United States Census Bureau website. Column 4 lists the 10-day value-weighted cumulative abnormal returns (CARs) associated with each industry for the study. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively, based on a *t*-test comparing the value-weighted CARs to 0.

Quartile	NAICS	Name of the Industries	10-day CAR (%)
4	325	Chemical Manufacturing	4.83***
	324	Petroleum and Coal Products Manufacturing	6.72***
	515	Broadcasting (except Internet)	3.18***
	522	Credit Intermediation and Related Activities	3.25***
	611	Educational Services	6.14***
	562	Waste Management and Remediation Services	4.24*
	481	Air Transportation	2.22
	221	Utilities	−2.58**
	541	Professional, Scientific, and Technical Services	2.15**
	523	Securities, Commodity Contracts, and Other Activities	1.99**
3	336	Transportation Equipment Manufacturing	−1.12
	112	Animal Production and Aquaculture	3.95
	624	Social Assistance	−3.14
	327	Nonmetallic Mineral Product Manufacturing	2.35
	524	Insurance Carriers and Related Activities	1.74
	621	Ambulatory Health Care Services	2.38
	111	Crop Production	−6.77**
	312	Beverage and Tobacco Product Manufacturing	−3.54***
	512	Motion Picture and Sound Recording Industries	−2.89
	212	Mining (except Oil and Gas)	2.84
2	331	Primary Metal Manufacturing	5.83***
	445	Food and Beverage Stores	−1.61
	517	Telecommunications	−2.17
	483	Water Transportation	4.11
	486	Pipeline Transportation	1.12*
	211	Oil and Gas Extraction	−4.44***
	311	Food Manufacturing	−4.90***
	424	Merchant Wholesalers, Nondurable Goods	−2.16
	454	Nonstore Retailers	−1.52
	423	Merchant Wholesalers, Durable Goods	−1.85
1	335	Electrical Equipment, Appliance, & Comp. Mfg	0.35
	213	Support Activities for Mining	−4.19**
	425	Wholesale Electronic Markets, Agents & Brokers	3.79**
	236	Construction of Buildings	−2.93*
	519	Other Information Services	−3.38**
	333	Machinery Manufacturing	−0.98
	334	Computer and Electronic Product Manufacturing	−1.27**
	115	Support Activities for Agriculture and Forestry	0.89
	326	Plastics and Rubber Products Manufacturing	−2.89
	551	Management of Companies & Enterprises	2.96***

$$(1)\quad \text{EXPECTED\_CASH\_TAX\_RATE}_i = \text{CASH\_TAX\_PAID}_i / (\text{PRETAX\_INCOME}_i - \text{SPECIAL\_ITEMS}_i).$$

I use the 1-year effective cash tax rate as a proxy for future tax rates as it is shown to predict future tax rates better than a 10-year tax rate (Wagner et al. (2018a)). However, in unreported results, I find that the main conclusions of the study remain unaffected if I use the long-term (10-year) average cash tax rates as a proxy for expected effective tax rates.

Another source of omitted variable bias could be import dependence if firms with high import dependence also have higher regulatory restrictions. It is difficult to find firm-level data on imports. However, I collect import data for 3-digit NAICS industries from the United States International Trade Commission (USITC). Additionally, I control for whether an industry has government exposure, based on Belo, Gala, and Li (2013). This allows me to account for other possible Trump policies such as infrastructure and defense, which might confound with the regulatory burden of firms.

RegData provides regulatory restriction scores for 54 NAICS 3-digit industries. Hence, I discard firms that are not in these 54 industries. I retain the daily returns data from CRSP for the common shares of the firms (i.e., stock returns belonging to share codes 10 and 11). I also drop firms that are incorporated outside the U.S. Additionally, I exclude firms allocated NAICS code 525 (funds, trusts, and other financial vehicles) and 35 firms that changed their NAICS code during the study. This procedure leads to a final sample of 2,413 firms, which are used in this study.

D. Other Nonfinancial Data

Together with the presidential election, 435 congressional districts for the House of Representatives and 34 out of 100 senate seat elections were held on the same day in 2016. Hence, I collect the members of the U.S. Congress from each state as of the election date and their respective party affiliation by using election results data from MIT Election Lab (see https://electionlab.mit.edu/). Additionally, I control for political connections in all of my regression specifications. I measure political connection based on donations to the Trump campaign from firms via their Political Action Committees (PACs), or through individual contributions, or based on the lobbying expenditure of firms during the 3 years before 2016. I collect these data from the Center for Responsive Politics. Political connections are particularly important as compared with nonhighly regulated industries, highly regulated industries could be expected to have more political connections to lower their regulatory burden. To explicitly control for such omitted variables that may affect the results, I control for political connections for each firm *within* each regulatory quartile. Finally, I obtain product market competition data from Hoberg and Phillips (2016).[4]

Table 2 presents a brief overview of how the main variables used in this study are distributed across different regulation quartiles. As can be seen, the regulated firms in the fourth quartile tend to have lower liquidity than those in other quartiles. However, there is no apparent difference in the size, debt-equity ratio, and cash tax rates of firms across quartiles. In addition, the average gross return of firms during the 10-day event window is higher for firms in the most regulated industries than for firms in other industries. This gives an initial indication that the most regulated firms had a more positive price reaction to their firm value than the other firms.

For some of my analyses, I use the climate responsibility of firms by using data from the MSCI KLD database. I use the scores related to the environmental strengths and concerns as covered in this database following Ramelli et al. (2021). Climate responsibility for a firm is calculated by subtracting environmental concerns from environmental strengths, as reported by the MSCI KLD database.[5]

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[4]I thank Gerard Hoberg and Gordon Phillips for making these data available.

[5]I thank Stefano Ramelli and Alexander Wagner for sharing their data.

TABLE 2
Summary Statistics

Table 2 reports descriptive statistics for the variables used in the main analyses of the article. The main sample includes 2,413 firms around Nov. 8, 2016. QUTILE_1-QUTILE_4 are the four groups of firms based on the quartiles of regulatory restrictions of the NAICS 3-digit industry that the firm belongs to. QUTILE_1 is for least regulated and QUTILE_4 is for most regulated firms. The variables are: ILLIQUIDITY is a firm-specific illiquidity measure based on Amihud (2002), log(MARKET_CAP) is the natural logarithm of market capitalization for each firm, log(TOTAL_ASSETS) is the natural logarithm of total assets for each firm, DEBT_TO_EQUITY is the debt-equity ratio based on the book value of debt and equity of a firm, EXPECTED_CASH_TAX_RATE is expected tax rate calculated according to Wagner et al. (2018a), POLITICAL_CONNECTION is a dummy variable if a firm has donated to Republican PAC and/or any individuals from the firm has donated to Donald Trump's campaign during the 2016 election cycle, GOVT_EXPOSURE is a dummy variable identifying industries that have higher than median level of exposure to government spending calculated as per Belo et al. (2013), IMPORT_DEPENDENT is a dummy variable indicating industries that are import dependent based on data from United States International Trade Commission (USITC). TOBINS_Q is the firm-level measure of growth opportunities measured as in Akey (2015), TEXT_BASED_HHI is the firm-level measure of competition from Hoberg and Phillips (2016), REPUBLICAN is a dummy variable indicating states with more Republican members of Congress and 10-DAY CAR is the cumulative abnormal returns for each stock over 10 days after the election. All variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables have been winsorized at 1 and 99 percentiles.

	QUTILE_1			QUTILE_2			QUTILE_3			QUTILE_4		
Variables	*N*	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*	Mean	Std. Dev.
ILLIQUIDITY	596	0.00	0.01	354	0.00	0.00	312	0.00	0.01	1,158	0.00	0.01
log(MARKET_CAP)	603	13.15	2.28	356	13.75	2.24	316	13.84	2.33	1,166	13.36	2.15
log(TOTAL_ASSETS)	603	6.43	2.20	356	7.13	2.09	316	7.33	2.41	1,166	6.91	2.30
DEBT_TO_EQUITY	603	0.36	2.08	356	0.65	2.97	316	0.67	2.41	1,166	0.52	2.41
EXPECTED_CASH_TAX_RATE	603	0.13	0.16	356	0.12	0.16	316	0.17	0.16	1,166	0.13	0.16
POLITICAL_CONNECTION	603	0.41	0.49	356	0.48	0.50	316	0.51	0.50	1,166	0.40	0.49
GOVT_EXPOSURE	603	0.60	0.49	356	0.61	0.49	316	0.68	0.47	1,166	0.89	0.31
IMPORT_DEPENDENCE	603	0.66	0.47	356	0.52	0.50	316	0.52	0.50	1,166	0.28	0.45
TOBINS_Q	549	1.82	1.31	339	1.59	0.91	298	1.45	1.39	1,112	1.64	1.87
TEXT_BASED_HHI	575	0.32	0.29	336	0.30	0.27	294	0.32	0.31	1,125	0.21	0.24
REPUBLICAN	602	0.11	0.31	356	0.12	0.32	316	0.12	0.33	1,165	0.13	0.33
10-DAY CAR	603	−0.01	0.10	356	−0.02	0.11	316	0.00	0.11	1,166	0.03	0.10

E. Empirical Strategy

I employ an event study methodology for this analysis. I take Nov. 9, 2016, as the first day after the event (the election results were declared on Nov. 8) when the market starts as the beginning of the event window. For each firm in my sample, I use a 252-day estimation window that ends 30 days before the event. I calculate the Fama–French 5 factors and momentum-adjusted cumulative abnormal returns (CARs) for each firm over the event window. I use multiple definitions of the event window: 1, 5, and 10 trading days after the event. I examine a short term (maximum of 10 trading days) period for two reasons. First, as mentioned in Fama (1998), the "bad-model problem" is limited over shorter horizons. Second, as noted by Wagner et al. (2018a), the reaction of stock prices to policy changes becomes increasingly difficult to measure over longer periods due to the inherent difficulties in distinguishing changes in economic agents' expectation from a policy change (Schwert (1981)). Some industries may be affected differently from others due to having had different expectations about policies, besides deregulation, that would be enacted by the Trump administration. For example, investors could have expected that firms in the construction industry or the defense industry would benefit from the Trump administration. Hence, I employ broader 2-digit NAICS-based industry fixed effects to absorb such unobserved industry-specific heterogeneity that might have driven the stock returns during this short period. I also employ state-fixed effects, as states might vary with respect to their local economic conditions, industry characteristics, political economy features, and other economic policies. Moreover, as I only consider expected changes in federal regulations, employing state-fixed effects helps to account for unobserved heterogeneity due to variation in expectations of the state-level regulatory environment. In all of the specifications, the standard errors are clustered at the 2-digit NAICS industry level. For each event window specification, I calculate each firm's CAR and regress it on the dummy variable identifying the quartile of regulation that the firm is in (controlling for other firm characteristics). The empirical specification is formulated as below:

$$\text{CAR}_{\text{id}} = \alpha + \sum_{i=2}^{4} \beta \times \text{QUTILE\_i} + \gamma \times \mathbf{X}_i + \varepsilon_i + \vartheta_i + v_i. \tag{2}$$

In the above equation, α is the intercept and QUTILE_i is a dummy variable that takes a value of 1 if the firm is in the ith least regulated industry quartile, and 0 otherwise. In the basic empirical setup, QUTILE_1 is taken as the reference category, as it indicates firms that are in the least regulated industries. I use quartiles to group firms in terms of their level of regulatory restrictions to account for any potential nonlinearity of the stock-price reaction to expected deregulation. $\mathbf{X}$ is the vector of control variables (as discussed previously) for each firm. ε_i is the industry fixed effect, ϑ_i is the state-fixed effect, and v_i is the error term. CARid denotes the cumulative abnormal return for firm i until day d after the event. In this case, d takes the values 1, 5, and 10, as mentioned above.

III. Empirical Results

A. Regulation and Stock Returns

In this section, I first present graphical evidence of the effect of Trump's win in the U.S. presidential election of 2016 on the stock-price reaction of firms in the most regulated industries.

Figure 2 presents the difference in value-weighted CARs of the firms in quartile four (most regulated) and quartile one (least regulated) around the event date (i.e., Nov. 8, 2016). As is apparent from the figure, there is a distinct increase in abnormal returns on Nov. 9, 2016, for this "long-short" portfolio: firms in the most regulated industries on average gained approximately 3% in market value on the first day after the election outcome was announced. This effect persisted over the following days. Hence, Figure 2 provides some initial evidence of the effect of expected deregulation on stock prices.

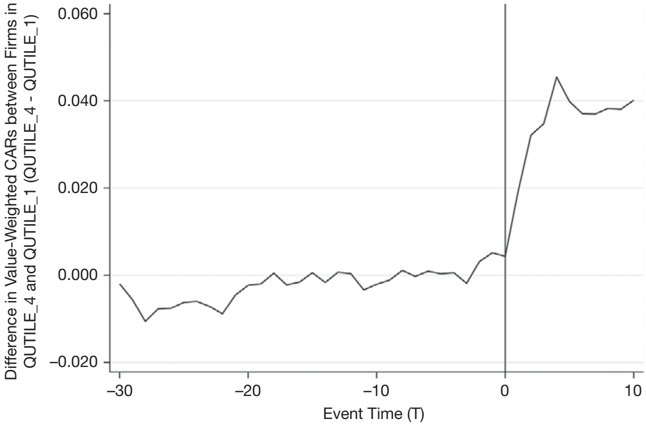
Next, I analyze this relationship more formally. Table 3 presents the results of estimating equation (2), where I regress individual firm's CARs on QUTILE (representing which quartile of regulated industries the firm belongs to, with 2 being the second lowest and 4 being the highest) and other control variables.

Columns 1–3 show the result of regressing 1-day, 5-day, and 10-day CARs of the firms on a dummy variable QUTILE_N (where N = 2,3, and 4) representing the respective regulatory quartile in which the firm is placed, based on its industry and other control variables. The asymmetric price reaction is evident from the results. Compared with firms in lower quartiles, higher quartiles have higher CARs during

FIGURE 2

Cumulative Abnormal Returns Around the Election Day

Figure 2 plots the difference between the value-weighted cumulative abnormal returns of firms in the highest quartile of regulated industries (QUTILE_4) to those for firms in the lowest (QUTILE_1) quartile around Nov. 8, 2016, the event date (i.e., QUTILE_4–QUTILE_1) in the vertical axis in basis points. The horizontal axis plots the trading day with respect to the event date marked by the vertical line in the graph at time $T = 0$.



the 10-day event window after the election. For example, the point estimate on QUTILE_3 in column 3 suggests that firms in the third quartile had 2.2% higher CARs during the event window than the firms in the lowest quartile. CARs are highest for stocks in the fourth quartile. As suggested by the point estimate on QUTILE_4 in column 3, firms in this most regulated quartile had 3.9% higher CAR, on average, than firms in the least regulated quartile. In line with the findings of Wagner et al. (2018a), I also find that the coefficient on EXPECTED_CASH_TAX_RATE is positive and statistically significant. In columns 4–6, I control for political connections for firms within each regulatory quartile. This specification aids interpretation of the point estimates on the regulatory quartile dummies after accounting for political connections within each quartile. As is evident from the results, the point estimates remain almost unchanged after applying these controls. Hence, the baseline results cannot be explained by political connections of regulated firms that could potentially be jointly correlated with regulations and stock returns around the event date. I also find that industries that were import-dependent experienced lower returns than industries that were not import-dependent, as denoted by the coefficient on IMPORT_DEPENDENT. This suggests that the "America First" policy of the Trump campaign led to an increased trade-policy uncertainty.

The specification can explain approximately 9% to 12% of the variation in CARs. A 3.9% increase in abnormal returns is equivalent to approximately $25 million (=$3.9\% \times \exp(13.35)$) for an average firm in the most regulated industries. Overall, these results show that investors expected a significant economic benefit to accrue to the firms in the most regulated industries due to expected deregulation during the Trump presidency. This result is unlikely to be driven by broader industry-level shocks as they are obtained after including 2-digit NAICS fixed

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TABLE 3

Baseline Results: Regulation and Stock Returns

In Table 3, the dependent variable is CAR_K, where $K = \{1, 5, 10\}$, is the K-day cumulative abnormal returns after Nov. 8, 2016. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions, QUTILE_1 is the reference category. ILLIQUIDITY is a firm-specific illiquidity measure based on Amihud (2002), log(MARKET_CAP) is the natural logarithm of market capitalization for each firm, log(TOTAL_ASSETS) is the natural logarithm of total assets for each firm, DEBT_TO_EQUITY is the debt-equity ratio based on the book value of debt and equity of a firm, EXPECTED_CASH_TAX_RATE is expected tax rate calculated following Wagner et al. (2018a), MISS_TAX_RATE is a dummy variable indicating firms whose tax rates could not be computed as of 2015, POLITICAL_CONNECTION is a dummy variable if a firm has donated to Republican PAC and/or any individuals from the firm has donated to Donald Trump's campaign during the 2016 election cycle, GOVT_EXPOSURE is a dummy variable identifying industries that have higher than median level of exposure to government spending calculated as per Belo et al. (2013), IMPORT_DEPENDENT is a dummy variable indicating industries that are import dependent based on data from the United States International Trade Commission (USITC). Columns 4–6 additionally control for firm-level political connection within each regulatory quartile. Variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state-fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CAR_01	CAR_05	CAR_10	CAR_01	CAR_05	CAR_10
QUTILE_2	0.012*	0.014	0.014	0.014**	0.016	0.015
	(1.88)	(1.02)	(1.07)	(2.61)	(1.21)	(1.25)
QUTILE_3	0.013***	0.027***	0.022***	0.018***	0.031***	0.025**
	(2.89)	(4.05)	(3.61)	(3.10)	(3.04)	(2.29)
QUTILE_4	0.021***	0.042***	0.039***	0.022***	0.045***	0.040**
	(4.89)	(3.41)	(2.86)	(4.60)	(3.18)	(2.83)
ILLIQUIDITY	−0.223**	−0.232	−0.233	−0.224**	−0.232	−0.234
	(−2.37)	(−1.30)	(−0.92)	(−2.38)	(−1.28)	(−0.93)
log(MARKET_CAP)	0.004	0.003	−0.002	0.004	0.003	−0.002
	(1.37)	(0.79)	(−0.72)	(1.36)	(0.78)	(−0.73)
log(TOTAL_ASSETS)	−0.003	−0.006	−0.003	−0.003	−0.006	−0.003
	(−1.20)	(−1.18)	(−0.65)	(−1.19)	(−1.17)	(−0.65)
DEBT_TO_EQUITY	−0.000	−0.000	−0.001	−0.000	−0.000	−0.001
	(−0.22)	(−0.41)	(−0.75)	(−0.20)	(−0.39)	(−0.75)
EXPECTED_CASH_TAX_RATE	0.015*	0.021**	0.014	0.015*	0.022**	0.014
	(1.85)	(2.27)	(1.37)	(1.84)	(2.33)	(1.35)
MISS_TAX_RATE	0.002	0.004	0.014	0.002	0.004	0.014
	(0.45)	(0.49)	(1.39)	(0.48)	(0.51)	(1.40)
POLITICAL_CONNECTION	−0.000	−0.001	−0.003	0.003	0.005	−0.000
	(−0.02)	(−0.18)	(−0.45)	(0.94)	(0.81)	(−0.07)
GOVT_EXPOSURE	−0.000	−0.005	−0.007	−0.000	−0.005	−0.007
	(−0.04)	(−0.52)	(−1.03)	(−0.03)	(−0.52)	(−1.01)
IMPORT_DEPENDENT	−0.015***	−0.021**	−0.015	−0.016***	−0.021**	−0.016*
	(−4.36)	(−2.32)	(−1.65)	(−4.65)	(−2.40)	(−1.76)
QUTILE_2 × POLITICAL_CONNECTION				−0.004	−0.006	−0.002
				(−0.92)	(−0.79)	(−0.23)
QUTILE_3 × POLITICAL_CONNECTION				−0.010	−0.007	−0.006
				(−1.48)	(−0.81)	(−0.52)
QUTILE_4 × POLITICAL_CONNECTION				−0.002	−0.009	−0.002
				(−0.64)	(−1.03)	(−0.34)
R^2	0.11	0.12	0.09	0.11	0.12	0.09
No. of obs.	2,413	2,413	2,413	2,413	2,413	2,413
2-digit NAICS FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

effects. Additionally, unobserved state-level factors that might change simultaneously on election day and are correlated with lower expected federal regulations are unlikely to drive the results, as state-fixed effects are likely to account for this.

Based on a back-of-the-envelope calculation, one can back-out the true effect of deregulation based on Trump's election and his pre-election winning odds. For example, if the probability of Trump's win was p before the election and that of

Hillary Clinton was $(1-p)$, then the observed firm value immediately before the election (V_1) can be written as: $V_1 = p \times \mathrm{DR} + (1-p) \times R$, where, DR is the firm value associated with the new regulatory regime if Trump wins, and R is the firm value associated with continued regulation if Hillary Clinton wins. After the election, the market became certain about DR (i.e., p became equal to 1). Hence, after the election, the observed firm value, V_2, can be written as $V_2 = \mathrm{DR}$. Therefore, the change in firm value, $\Delta V = V_2 - V_1$, is $(\mathrm{DR} - R)/(1-p)$. Hence, it follows that $\mathrm{DR} - R$, ΔR, is the true change in firm value due to expected changes in regulation after Trump's win and that the actual firm value change associated with deregulation due to Trump's win, ΔR, is $\Delta V/(1-p)$. As per FiveThirtyEight (https://projects.fivethirtyeight.com/2016-election-forecast/), the probability of Donald Trump's win immediately before the election day was approximately 28% (i.e., p was equal to 0.28). ΔV can be proxied by stock returns. Hence, for the most regulated firms, the true effect on firm value (using the point estimates from Table 3) due to Trump's deregulation plan would have been approximately 5.55% $(= 4\%/0.72)$.

To interpret the result causally, one would ideally require the election win of Trump to be only associated with changes in future regulations. However, this was not the case. In addition to deregulation, Trump's presidency was also associated with lower expected tax rates (Wagner et al. (2018a)), possibly more political uncertainty given his relatively unconventional nature, more lenient views on environmental and climate change policies, disproportionately larger benefits to businesses tied to him (and not to the Republican party (Child et al. (2020))), larger fiscal spendings on infrastructure and military, and more trade uncertainty because of his "America First" stance, among other things. Even though, in all of my specifications, I control for some of these confounding factors (e.g., expected tax rates, dependence on government spending of industry to account for the effect of larger fiscal spend, import dependence to account for more trade policy uncertainty, political connection to the Republican Party as well as to Donald Trump through individual and PAC donations within each regulatory quartile, a broader industry fixed effects to take into account within industry heterogeneity), I cannot rule out that an unobservable omitted variable is still biasing my estimates. Hence, in the following subsections, I perform a series of robustness tests to demonstrate the stability of the main findings and alleviate such concerns as much as possible.

B. Robustness Tests

1. Accounting for Event Date Clustering and Alternate Specifications

In the main specification, the standard errors are clustered at the 2-digit NAICS industry level. This implicitly assumes that if stock returns are contemporaneously correlated in any other way, the test statistics might be biased upward. To account for such a bias, I follow Karpoff and Malatesta (1995) and estimate a seemingly unrelated regression (SUR) model. To implement this model, I calculate the value-weighted abnormal returns for a portfolio of firms belonging to each regulatory restriction quartile over event windows and analyze four time series of returns. The event windows are to [−22,1], [−22,5] and [−22,10] corresponding to the 1-, 5- and 10-day comparisons of abnormal returns. For each regression specification,

TABLE 4

Alternate Specifications: Regulation and Stock Returns

Panel A of Table 4 implements a seemingly unrelated regression (SUR) to account for event-time clustering. For each regulatory quartile, I calculate a value-weighted portfolio returns over the period of [−22,10] days around the election. Columns 1–4 of Panel A denote the coefficient of the SUR model on the dummy variable indicating whether a given day is after the election day of not for the most regulated (QUTILE_4) to the least regulated (QUTILE_1) industries, respectively. Columns 5–7 compare the coefficients of QUTILE_1 with those of other quartiles using a Wald test. In Panel B, columns 1–4 implements a 1-factor model where the factor is either the index return of the iShares All Country World Index which excludes the U.S. (in columns 1 and 2) or it is the value-weighted returns of the universe of Compustat firms excluding the U.S. (in columns 3 and 4). In columns 5–6, I calculate *t*-statistics of the regression output in Table 3 following Cohn et al. (2016). The dependent variable is CARK, where $K = \{1, 5\}$, is the K-day cumulative abnormal returns after Nov. 8, 2016, based on the Fama–French 5 factors and the momentum factor model. As before, QUTILE_1 indicates the least and QUTILE_4 indicates the most regulated industries with QUTILE_1 being the reference category. For brevity, point estimates on other control variables are not reported. Variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. N presents number of firms in the regression. The regressions in Panel B are with 2-digit NAICS industry fixed effects and state-fixed effects. Standard errors are clustered at 2-digit NAICS level in columns 1–4. *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Adjusting for Event-Day Clustering Using Seemingly Unrelated Regressions

	Coefficients on:				Difference with QUTILE_1		
	QUTILE_4	QUTILE_3	QUTILE_2	QUTILE_1	Q4 − Q1	Q3 − Q1	Q2 − Q1
1-Day	0.005***	−0.006***	−0.013***	−0.010***	0.015***	0.004*	−0.003
5-Day	0.003***	−0.003**	−0.004	−0.004	0.007***	0.001	0.000
10-Day	0.002***	−0.002	0.000	−0.001	0.003**	−0.001	0.000

Panel B. Alternative Methods to Account for Event-Day Clustering

	1-Factor Model Using:				*t*-stats. Computed Using:	
	Compustat		ACWX		Cohn et al. (2016)	
	CAR_01	CAR_05	CAR_01	CAR_05	CAR_01	CAR_05
QUTILE_2	0.016*	0.028	0.029	0.012**	0.014	0.016
	(2.02)	(1.58)	(2.09)	(1.66)	(0.37)	(1.51)
QUTILE_3	0.012**	0.018	0.012	0.012**	0.018***	0.031***
	(2.76)	(1.38)	(2.68)	(1.18)	(4.34)	(3.50)
QUTILE_4	0.024***	0.043***	0.030*	0.024***	0.022***	0.045***
	(3.56)	(2.93)	(3.53)	(2.81)	(4.98)	(4.68)
R^2	0.15	0.15	0.12	0.16	0.15	0.15
No. of obs.	2,413	2,413	2,413	2,413	2,413	2,413
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

I incorporate a dummy variable that takes the value of 1 for days after the election, and 0 otherwise. I compare the coefficient on this dummy variable across the four regressions to obtain statistical inference for the SUR model.

The results of the analysis are shown in Panel A of Table 4. The coefficient estimate of 0.005 on QUTILE_4 in row 1 and column 1 of the table shows that firms in the fourth quartile had 0.5% higher (value-weighted) abnormal returns on the first day after the election result than on the 22 days before the election. This result is statistically significant at the 1% level. Next, the coefficient in row 1 and column 5 shows that firms in the most regulated industries earned on average 1.5% higher abnormal returns than firms in the least regulated industries. This is essentially the difference between the coefficients in column 1 and column 4 of row 1, where the test statistic is the Wald test of equality between the two coefficients based on chi-square distribution. Other coefficients in columns 1–4 and columns 5–7 can be interpreted in a similar way. In terms of economic magnitude, 1.5% is close to the 2% difference in CAR obtained in the baseline specifications for firms in the most regulated industries. Similarly, the coefficients over the 10-day event window suggests that the firms in the most regulated industries experienced 0.3% more abnormal returns *per day* than those firms in the least regulated industries.

Although the SUR model takes into account contemporaneous correlation of outcomes across different observation units, it cannot incorporate cross-sectional time-invariant variables. Thus, using this model, it is not possible to control for important confounding factors, such as expected tax rates, and political connections.[6]

Another important concern is the lack of a proper control group; the baseline specification invariably compares the abnormal returns of firms in highly regulated quartiles to those of firms in the least regulated quartile. Hence, if some confounding factors affect firms in different regulation quartiles asymmetrically, the point estimates in the main specification could be biased. One way of dealing with this is to compute (abnormal) returns using other world indices (excluding the U.S.), as done by Zhang (2007). Hence, I calculate abnormal returns using a 1-factor model that represents the return of global markets, aside from the U.S. For this purpose, I use two types of implementation. First, I calculate value-weighted total returns in dollars from the Compustat Global universe of common stocks.[7] I use these value-weighted returns as a single factor in a 1-factor model to predict stock returns. I use the abnormal returns calculated from this 1-factor model as the dependent variable to estimate the specification of equation (2). The first two columns in Panel B of Table 4 present the results obtained from using this methodology. For brevity, I do not report the coefficients on the control variables. Essentially, I replicate the specifications in Table 3. The inference remains largely the same; firms in the most regulated quartile gained 4.3% more market value, on average, than those in the least regulated quartile during the 5 days following Trump's election.[8] In the second implementation, I use the return of the iShares All Country World Index Exchange Trade Fund (ACWX) as the factor predicting stock returns in a 1-factor model. The ACWX comprises large and mid-cap stocks across the world, excluding the U.S. As of Mar. 2022, the ACWX had 4.4 trillion U.S. dollars under management. Once I calculate the abnormal returns from this model, I proceed in the same way as before, by estimating the baseline specification of equation (2). Columns 3 and 4 in Panel B of Table 4 present the results obtained from using this specification. As can be seen, the same inference is obtained.

Yet another way of dealing with the problem of event-time clustering is to calculate the standard errors of the estimate in a different way. To this end, I follow

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[6]Additionally, the SUR model is not completely equivalent to the specification in equation (2), as the SUR model compares a time-series change in CARs for a given portfolio, whereas the specification in equation (2) is a cross-sectional comparison of CARs.

[7]I download all the exchange rates from Refinitiv to convert local currency to U.S. dollars. I then apply the formula Total Return = $((\mathrm{PRCCD}_t/\mathrm{AJEXDI}_t) \times \mathrm{TRFD}_t))/(((\mathrm{PRCCD}_{t-1}/\mathrm{AJEXDI}_{t-1}) \times \mathrm{TRFD}_{t-1})) - 1$.

[8]However, one important caveat is worth mentioning in this approach. This approach will lead to valid inference if global stock markets are unaffected by U.S. macroeconomic news. Given the extant literature documenting how information, especially macroeconomic news, in the U.S. spills over to other markets (e.g., Eun and Shim (1989), Becker, Finnerty, and Gupta (1990), and Bongaerts, Roll, Rösch, van Dijk, and Yuferova (2022)), the implicit assumption that other markets are unaffected by the outcome of the U.S. election is likely to be violated.

Cohn, Gillan, and Hartzell (2016). I compute 1-, 5-, and, 10-day CARs over a "nonevent" period (from Oct. 1, 2015, to Sept. 30, 2016). To compute the CARs, I use the same factor model with an estimation period dating back to Oct. 1, 2014.[9] After calculating the CARs, I run the same specification as in equation (2) and obtain the coefficients on the three regulatory quartile dummies. I then calculate the *t*-statistics by subtracting the mean point estimate over the nonevent period from the event-day point estimate and dividing the resulting difference by the standard deviation of the point estimates over the nonevent period. In the last two columns in Panel B of Table 4, I report the same specification as in Table 3 but *t*-statistics are calculated based on the methodology of Cohn et al. (2016). As can be seen, the results remain unaltered. Additionally, in unreported results, I double cluster standard errors across 2-digit NAICS and the state of incorporation of firms. The results remain unchanged.

Finally, I use a continuous measure of regulatory restrictions instead of using quartiles. As can be seen in Table A3 in the Supplementary Material, the inference remains unaltered. In this regard, I note that the use of quartiles allows me to demonstrate the possible non-linear impact on firms of an expected decrease in regulations (i.e., only the most regulated firms are affected when regulations are expected to decrease). For example, in Table 3, point estimates are significant only for QUTILE_3 and QUTILE_4. Additionally, the economic significance of the point estimate for QUTILE_4 is considerably larger than that for the others. Hence, I use quartiles as my preferred specification.

In a nutshell, the results in this section suggest that unaccounted for cross-correlation of stock returns across different firms due to event-date clustering is unlikely to drive the main result. The results remain robust to using alternate (continuous) measure of regulations at the industry level.

2. Other Robustness Tests

First, I account for the foreign operation of firms. The focus of the Trump campaign was "Make America Great Again" which emphasized giving advantages to firms that are more domestically oriented. This might have affected the valuation of more domestically oriented firms differently to those of less domestically oriented firms. To perform this analysis, I collect data on firms' foreign operations from Compustat (namely, foreign sales from the segment files) and calculate the average percentage of foreign sales for each firm over the 3 years before 2016. When I control for foreign sales of a firm, the results remain unchanged, as shown in Table A4 in the Supplementary Material. To some extent, this accounts for the firm-level impact of trade policy uncertainty under the Trump administration.

Second, in Table A5 in the Supplementary Material, I further investigate if the results are robust after accounting for firm-specific policies aiming to become climate responsible in the future. Ramelli et al. (2021) show that firms with more responsible climate policies gained in market value after the 2016 election compared with firms having less responsible climate policies. Thus, my results might wrongly attribute the positive valuation effect of firms to expected deregulation if

[9]Hence, for each of the 252 days (from Oct. 1, 2015, to Sept. 30, 2016). I calculate CARs using a rolling event window of 1 year.

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investors' expectations regarding climate change-related policies account for stock-price reactions. To examine this possibility, I use the climate responsibility data from Ramelli et al. (2021). As can be seen, the main results remain unaffected after accounting for such climate responsibility in my sample. Hence, the results do not appear to be biased by the possibly laxer climate change-related policies of the Trump administration.

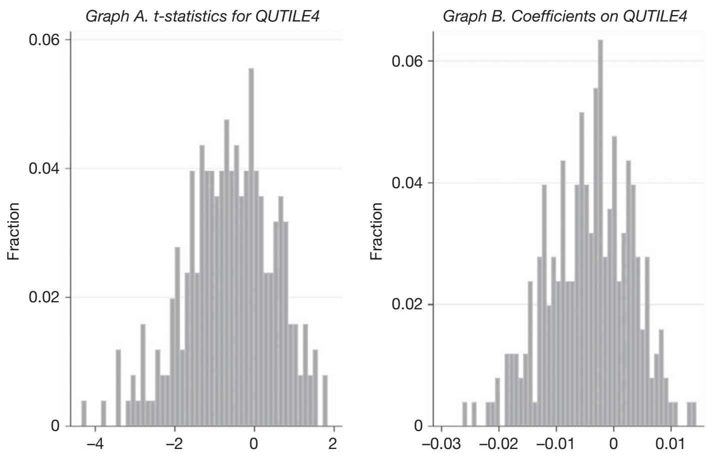
Additionally, I do a falsification test. As this study is based on one event, it might be possible that the firms in the top quartile might have been most likely to generate such abnormal positive returns on any random date due to some unobserved characteristics that are not absorbed by industry fixed effects. In order to address these concerns, I employ a falsification test. First, during the 3-year period before the election result was declared, I randomly select 252 trading days and run a similar event study around each day. If there is any bias in the regression analysis, I should find a large fraction of these randomly selected 252 days also display a statistically significant and economically similar positive reaction.

Graph A of Figure 3 shows the distribution of regression *t*-statistics, and Graph B shows the distribution of coefficients obtained by executing the specification of Table 3 pertaining to the point estimate on QUTILE_4 for 252 randomly generated dates for 1-day CARs. As can be seen, the likelihood of observing positive and statistically significant returns for an average firm in the top quartile of regulation is 0. None of the regression coefficients are significant at the 5% level and the economic magnitudes are also very small compared with those reported in Table 3. While the actual estimate from Table 3 is approximately 2.0%, the maximum estimate obtained in the falsification tests is 0.4%. That is, on any given year (consisting of approximately 252 trading days), the likelihood

FIGURE 3

Falsification Tests

Graph A of Figure 3 plots the frequency distribution of *t*-statistics on the coefficient estimate of QUTILE_4 while running the main regression specification presented in equation (2) across 252 randomly generated dates between Jan. 1, 2013, and Nov. 8, 2016. Graph B plots the frequency distribution corresponding to the point estimates of QUTILE_4 across the same regressions.



of any random day generating an abnormal return for the firms in the topmost quartile of regulation is 0.

Overall, this section documents that explanations other than the expectation of deregulation post-election day do not seem to alter the inference of the main result. Thus, while the election is not a perfect instrument for expected deregulation, the extensive robustness tests described above alleviate concerns that other omitted variables drive both the election result and the abnormal returns for the most regulated firms and provide suggestive evidence of a causal impact of expected deregulation after Trump's 2016 election win.

C. Event Study Around Shift in Trump's Odds of Winning

In this section, I investigate whether the stock price of firms in the most regulated industries behaved in the same way when Trump's probabilities of winning changed during the months prior to the election. I conduct an event study in the same way as before around July 30, 2016, when, according to the analytics website FiveThirtyEight, Donald Trump had 50.1% probability of winning compared with 49.9% for Hillary Clinton (https://projects.fivethirtyeight.com/2016-election-forecast/). According to FiveThirtyEight, this was the only date on which Trump had a higher probability of winning than Hillary Clinton leading up to the presidential election. While the gap between Clinton and Trump closed gradually leading up to July 30, there was a sudden widening of their winning probabilities immediately after July 30. Thus, the sudden *decrease* in Trump's probability of winning allows me to investigate whether and how stock prices of firms in the most regulated industries react to investors' expectations about future *increase* in regulations.

To analyze abnormal returns around July 30, I implement the same event study framework as before. However, in this case, the event date is considered to be July 30. The first day of trading after the event day is Aug. 1, 2016, as July 30, 2016, was a Saturday. In this case, one would expect that firms in the most regulated industries would react negatively compared with firms in the least regulated industries as Trump's probability of winning suddenly decreased.

The results of the analysis are shown in Table 5. As can be seen from the point estimate on QUTILE_4, firms in the most regulated industries reacted negatively during the 1–3 day event window after July 30. The point estimate on QUTILE_4 in column 1 of the table implies that firms in the most regulated industries earned 0.7% lower abnormal returns than firms in the least regulated industries on Aug. 1, 2016. However, in unreported results, I find that such negative reactions did not persist over the medium-term (e.g., 10-day).

Aside from documenting the robustness of the main results, this analysis sheds light on two issues. First, whether one should expect the most regulated firms to also react when there is an expected *increase* in regulations (rather than a decrease due to the election event itself). Second, whether such a reaction would be negative. One line of argument is that the most regulated firms benefit from any change in regulations, irrespective of whether the change is an increase or decrease in regulations. This can be due to many reasons, including but not limited to regulators adjusting pre-existing regulations to reflect more contemporary developments in an

TABLE 5

Other Trump Event: Regulation and Stock Returns

Table 5 presents the results of implementing the baseline specification of equation (2) after July 30, 2016. Donald Trump's probability of winning was 50.1% on July 30, 2016. According to the data from FiveThirtyEight, this was the only day when Donald Trump had a higher probability of winning than Hillary Clinton leading up to the election day. The dependent variable is CARK, where $K = \{1, 2, 3\}$, is the K-day cumulative abnormal returns after July 30, 2016. As before, the abnormal returns are calculated from a 5-factor model including momentum over the last year 1 month from the event date. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions, QUTILE_1 is the reference category. ILLIQUIDITY is a firm-specific illiquidity measure based on Amihud (2002), log(MARKET_CAP) is the natural logarithm of market capitalization for each firm, log(TOTAL_ASSETS) is the natural logarithm of total assets for each firm, DEBT_TO_EQUITY is the debt-equity ratio based on the book value of debt and equity of a firm, EXPECTED_CASH_TAX_RATE is expected tax rate calculated following Wagner et al. (2018a), MISS_TAX_RATE is a dummy variable indicating firms whose tax rates could not be computed as of 2015, POLITICAL_CONNECTION is a dummy variable if a firm has donated to Republican PAC and/or any individuals from the firm has donated to Trump's campaign during the 2016 election cycle, GOVT_EXPOSURE is a dummy variable identifying industries that have higher than median level of exposure to government spending calculated as per Belo et al. (2013), IMPORT_DEPENDENT is a dummy variable indicating industries that are import dependent based on data from the United States International Trade Commission (USITC). Variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state-fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CAR_01	CAR_02	CAR_03
QUTILE_2	−0.002	−0.001	−0.005
	(−0.69)	(−0.27)	(−1.42)
QUTILE_3	0.001	−0.004	−0.008
	(0.47)	(−1.24)	(−1.36)
QUTILE_4	−0.007**	−0.008**	−0.010**
	(−2.64)	(−2.79)	(−2.37)
ILLIQUIDITY	−0.028*	−0.024	−0.011
	(−1.89)	(−1.29)	(−0.99)
log(MARKET_CAP)	−0.000	−0.001	−0.002***
	(−0.57)	(−0.45)	(−3.24)
DEBT_TO_EQUITY	−0.000	−0.000	−0.000
	(−1.01)	(−1.20)	(−0.96)
EXPECTED_CASH_TAX_RATE	0.011**	0.013*	0.016*
	(2.28)	(1.87)	(1.81)
MISS_TAX_RATE	0.002	0.001	0.008**
	(1.07)	(0.54)	(2.71)
GOVT_EXPOSURE	−0.005*	−0.007**	−0.011**
	(−1.84)	(−2.46)	(−2.54)
IMPORT_DEPENDENT	0.008***	0.006***	0.015***
	(5.06)	(3.10)	(6.46)
POLITICAL_CONNECTION	0.004*	0.002	0.001
	(1.79)	(0.94)	(0.49)
QUTILE_2 × POLITICAL_CONNECTION	0.002	0.002	0.002
	(0.77)	(0.40)	(0.30)
QUTILE_3 × POLITICAL_CONNECTION	−0.006**	0.004	0.004
	(−2.74)	(1.23)	(1.33)
QUTILE_4 × POLITICAL_CONNECTION	−0.000	0.001	0.005
	(−0.04)	(0.56)	(1.54)
R^2	0.05	0.04	0.07
No. of obs.	2,409	2,409	2,408
2-digit NAICS FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

industry, the ability of regulated firms to comply with changes in regulation, and implementation of regulations through lobbying that could only benefit already regulated firms. If so, one might expect regulated firms to always benefit from a change in regulations. However, the results of this analysis indicate that a possible increase in expected regulations is considered negative by investors in most regulated firms in this study.

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IV. How Deregulation Benefits Shareholders

In this section, I explore the specific economic channels through which investors expected the firms to benefit from deregulation.

A. Regulation and Growth Opportunities

One of the ways through which regulation impacts economic growth is by imposing additional constraints on firms. Thus, firms maximize their profits subject to an additional constraint, which leads to inefficient allocation of resources and decreases firm profitability (Jaffe and Palmer (1997)). Such constraints can be particularly harmful to high-growth firms. For example, Arnold, Nicoletti, and Scarpetta (2011) show that European product market regulations impact firms with above-average productivity growth more than other firms. Furthermore, regulations impose costs that firms must meet by using otherwise productive resource (such costs can be both administrative costs and policy costs (Crafts (2006))), which harms the profitability of firms. Thus, one would expect that a reduction in regulations benefit high-growth firms in regulated industries more than low-growth firms, such that shareholders adjust their valuations of firms accordingly. Two challenges in testing this hypothesis are that regulations or firms' compliance costs and/or innovation decisions do not change randomly and it is difficult to observe regulatory burdens at the individual firm level. Thus, in the following analysis, I focus on an ex ante measure of expected firm growth.

Empirically, I test the hypothesis that high-growth firms are affected more than low-growth firms by employing the baseline specification of equation (2) and introducing interaction terms between each regulatory quartile and Tobin's Q (as a measure of expected future growth of firms). I measure Tobin's Q (TOBINS_Q) as the ratio of the sum of market capitalization and total assets minus book equity minus deferred tax liability to total assets. However, as noted in Laeven and Levine (2007), this conventional way of calculating Tobin's Q may be unsuitable for banks. Hence, I follow Laeven and Levine (2007) for the financial firms in the sample, by measuring Tobin's Q as the ratio of operating income (earnings before interest and taxes) to total assets. As with other accounting variables, I take the data for calculating Tobin's Q for the 2015 fiscal year.

The results of the analysis are presented in Table 6. Not all firms have all of the data for computing Tobin's Q. Hence, the sample size in these regressions differs from those in the earlier tables. Columns 1–3 implement an interaction term between each quartile of regulated industries and Tobin's Q. It is evident from the results that firms having higher growth opportunities *and* present in the most regulated industry quartile reacted more positively after the event than those firms in the least regulated industry quartile. In terms of the economic magnitude of the abnormal returns, a 1-standard-deviation increase in Tobin's Q for firms in the most regulated industries resulted in approximately 1.5% ($=0.8\% \times 1.87$) more positive abnormal returns during the 10-day event window, as compared to other firms in the least regulated industries. This is equivalent to a \$9.40 million relative gain in market value for an average firm in the most regulated industries. These results

TABLE 6

Mechanism: Regulation and Growth Opportunities

Table 6 investigates whether growth opportunities are related to stock returns for firms in the most regulated industries. The dependent variable is CARK, where K = {1,5, 10}, is the K-day cumulative abnormal returns after Nov. 8, 2016. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions, QUTILE_1 is the reference category. Growth Opportunities is measured by TOBINS_Q as in Akey (2015) (i.e., the ratio of the total assets plus market value of equity minus common equity minus deferred tax liability to total assets). Variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. Point estimates on the control variables are not reported for brevity. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state-fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CAR_01	CAR_05	CAR_10
QUTILE_2	0.008	−0.005	0.005
	(0.99)	(−0.40)	(0.60)
QUTILE_3	0.011	0.026**	0.025
	(1.05)	(2.15)	(1.54)
QUTILE_4	0.009	0.024**	0.026**
	(1.71)	(2.64)	(2.60)
TOBINS_Q	−0.002	0.004**	0.006**
	(−1.71)	(2.28)	(2.52)
QUTILE_2 × TOBINS_Q	0.002	0.013**	0.008
	(0.91)	(2.22)	(1.67)
QUTILE_3 × TOBINS_Q	0.003	0.000	−0.001
	(0.77)	(0.07)	(−0.17)
QUTILE_4 × TOBINS_Q	0.008***	0.011**	0.007**
	(3.36)	(2.77)	(2.18)
R^2	0.13	0.15	0.11
No. of obs.	2,272	2,272	2,272
Other controls	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

imply that higher regulations impact firms with higher growth opportunities more than firms with lower growth opportunities and, thus can stymie economic growth.

B. Regulation and Competition

In the economic theory of regulation of Stigler (1971), the demand for regulation comes from industry and "is designed and operated primarily for its benefit." This theory is often referred to as the "capture" theory of regulation. Posner (1974) suggests that industry incumbents seek regulation to protect themselves from future competition from new entrants and extract rents from consumers. After these seminal works of Stigler (1971) and Posner (1974), many other studies show that deregulation benefits consumers by introducing competition into various sectors of the economy. For example, Djankov et al. (2002) show that countries with higher entry regulations have lower per-capita GDP than countries with lower entry regulations. Bertrand and Kramarz (2002) analyze the French retail industry and find that regulation decreased competition. Besley and Burgess (2004) reach similar conclusions investigating labor regulations in India. Similarly, the removal of interstate branch banking restrictions introduced credit competition in the U.S. (Rice and Strahan (2010)).

Accordingly, I explore whether Trump's election impacted firms facing more competition differently from firms facing less competition for a given level of

TABLE 7

Mechanism: Regulation and Competition

Table 7 investigates if competition is related to stock returns for firms in more regulated industries. The dependent variable is CARK, where K = {1, 5, 10}, is the K-day cumulative abnormal returns after Nov. 8, 2016. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions, QUTILE_1 is the reference category. Competition is the text-based measure of Hoberg and Phillips (2016) as of 2015. Variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. Point estimates on the control variables are not reported for brevity. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state-fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CAR_01	CAR_05	CAR_10
QUTILE_2	0.013	0.018	0.025**
	(1.69)	(1.69)	(2.27)
QUTILE_3	0.033***	0.051***	0.039*
	(3.88)	(3.37)	(1.94)
QUTILE_4	0.036***	0.071***	0.062***
	(4.54)	(3.66)	(3.15)
TEXT_BASED_HHI	0.011*	0.022	0.002
	(1.77)	(1.65)	(0.16)
QUTILE_2 × TEXT_BASED_HHI	−0.003	0.002	−0.009
	(−0.30)	(0.04)	(−0.22)
QUTILE_3 × TEXT_BASED_HHI	−0.039***	−0.045*	−0.023
	(−3.64)	(−2.03)	(−0.87)
QUTILE_4 × TEXT_BASED_HHI	−0.047***	−0.099***	−0.088**
	(−3.61)	(−3.62)	(−2.84)
R^2	0.13	0.15	0.11
No. of obs.	2,303	2,303	2,303
Other controls	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

regulatory intensity. I use the product market concentration measure developed by Hoberg and Phillips (2016) for measuring the industry concentration of the firms' product market. As with all other variables, I use product market concentration (TEXT_BASED_HHI) data as of 2015. If investors expect that firms having higher product market concentration due to deregulation will experience a threat to their future profitability, one would expect to see firms in more regulated industries *and* having ex ante lower competitive threat, as indicated by the higher HHI, to have lower cumulative abnormal returns than firms operating in less regulated industries. Table 7 presents the results of this analysis. Columns 1–3 of the table show that firms in more regulated industries facing lower competition lost significant market value during the 10-day event window. At the end of the 10-day event window (column 3), a 1-standard-deviation (= 0.27) increase in TEXT_BASED_HHI of a firm was associated with an approximately 2.35% (= 8.8% × 0.27) decrease in abnormal returns for a firm in the most regulated industries compared with those for firms in the least regulated industries. This translates to an average relative decline of $14.75 million in firm value for an average firm in the most regulated industry compared with a firm facing similar competition in the least regulated industry. These results show that investors lowered their expectation of future profitability for firms having ex ante higher market power in more regulated industries. This implies that higher regulations help incumbent firms derive monopoly rents by shielding them from competition.

C. Regulation and Political Favoritism

In this section, I explore to what extent links to powerful politicians can explain the abnormal returns observed. The extensive literature on political connections shows that politically connected firms tend to gain market value after their connections win elections. For example, using data from 47 countries, Faccio (2006) documents positive abnormal returns of 1.5% when a political connection of a firm becomes active. Ferguson and Voth (2008) study the value of political connections in Nazi Germany and find that connected firms gained significant value immediately after the Nazis came to power in 1933. Akey (2015) documents the positive impact of political networks by examining the political contribution of firms in close elections in the U.S. More recently, Child et al. (2020) document that firms connected to Trump had positive CARs following the 2016 election.

One of the possible reasons for these patterns mentioned in this strand of the literature is that politically connected firms benefit from favorable regulatory treatment because of their connections. However, there is no direct evidence supporting this claim. For example, although Child et al. (2020) show that firms politically connected to Trump gained market value in the 20 days after the election and some of these firms received favorable regulatory treatment, it is not clear how much of the positive reaction was due to the expectation of such favorable regulatory treatment. That is, compared with when deregulation is not expected, when deregulation is expected political connections might become less valuable, as an imminent decrease in the overall cost of regulations means that more favorable regulatory treatment due to political connections is of less value to firms. Conversely, favorable regulatory treatment might be so firm-specific that an overall decrease in regulations does not change the value of political connections, or firms benefit from political connections other than by getting regulatory benefits.

Short (2019) reviews the large literature in political science that documents the role of politicians in enforcing regulations depending on their political affiliations and that of their constituents. There is some (albeit limited) evidence that firms may benefit through such political favoritism. For example, Ansolabehere and Snyder (2006), Albouy (2013), and Asher and Novosad (2017) present evidence of political favoritism exhibited by ruling parties in the U.S. and other developing countries for regions represented by elected members belonging to the ruling party. Furthermore, Khwaja and Mian (2005) present evidence that politicians' strength in their local constituencies affects firms' ability to be favored by lenders. More closely related to the current study is that of Asher and Novosad (2017), which documents that private firms in India receive favorable regulatory treatment in regions represented by the members of the ruling party.

If firms do benefit from political favoritism through the leniency of regulators, then such benefits might decrease when overall regulations are reduced. To test whether this is the case, I follow the literature on corporate law and define firms as politically connected to the Republican Party if they are incorporated in a state with a greater than median proportion of Republican members of Congress. Such a definition is motivated by two reasons. First, corporate law scholars note that the U.S. Congress, based on its authority over interstate commerce via the Commerce Clause, has discretion over various aspects of corporate law (Kahan and Rock

(2005), Winship (2013)). Thus, defining political connection in such a way helps to capture possible political favoritism in the enactment of federal regulations that can impose additional costs on shareholders via corporate laws. This reflects the fact that while corporate laws might appear to impact only the relationship between the shareholders and the managers of a firm, they can also impact the overall regulatory compliance of a firm, as the costs and the benefits of which are borne by the shareholders of a firm (Wallace (2009)). For example, if compliance with environmental regulations reduces the litigation cost of a firm, then, under corporate law, such compliance is part of the fiduciary duty of the managers of a firm toward their shareholders. Second, one could use the headquarter state of a firm as a proxy for the probability of firms being favored by regulators. However, such a definition would likely mismeasure the economically relevant regions for a firm and thus introduce noise.

The results of the analysis are shown in Table 8. As with the previous analyses, I interact a dummy variable (REPUBLICAN) indicating states with a greater than median proportion of Republican members of Congress with each regulatory quartile to tease out the economic channel. As one would expect, columns 1–3 show that the firms in states having more Republican than Democratic members of Congress gained significant market value during the event window; this is demonstrated by the positive and statistically significant point estimate on the REPUBLICAN variable. This finding is consistent with the literature on political

TABLE 8

Mechanism: Regulation and Political Favoritism

Table 8 investigates if political favoritism to states with more Republican members of Congress is related to stock returns for firms in more regulated industries. The dependent variable is CARK, where $K = \{1, 5, 10\}$, is the K-day cumulative abnormal returns after Nov. 8, 2016. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions, QUTILE_1 is the reference category. REPUBLICAN is a dummy variable indicating the U.S. states with greater than median number of Republican members of Congress. All variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. Point estimates on the control variables are not reported for brevity. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state-fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CAR_01	CAR_05	CAR_10
QUTILE_2	0.030**	0.029	0.035
	(2.27)	(1.23)	(1.45)
QUTILE_3	0.024***	0.055***	0.059**
	(4.02)	(2.89)	(2.69)
QUTILE_4	0.042***	0.083***	0.085**
	(3.92)	(2.73)	(2.49)
REPUBLICAN	0.019***	0.033***	0.040***
	(3.07)	(4.03)	(5.35)
QUTILE_2 × REPUBLICAN	−0.016*	−0.014	−0.020
	(−1.85)	(−1.01)	(−1.51)
QUTILE_3 × REPUBLICAN	−0.006	−0.024*	−0.033*
	(−0.14)	(−1.74)	(−1.82)
QUTILE_4 × REPUBLICAN	−0.018**	−0.036**	−0.040*
	(−2.67)	(−2.19)	(−2.09)
R^2	0.10	0.12	0.08
No. of obs.	2,418	2,418	2,418
Other controls	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes
State FE	No	No	No

connections, which documents positive abnormal returns for politically connected firms after an election win by the politician to whom they are connected. The results also complement previous studies in the corporate law literature documenting federal intervention in state corporate laws in the U.S. (Kahan and Rock (2005), Bebchuk and Hamdani (2006), and Winship (2013)). Moreover, the coefficients of interaction with the regulation quartile dummies have a negative sign. The point estimates imply that a firm in the most regulated industry and from a state with a greater than the median number of Republican members of Congress are associated with a 4% decrease in firm value by the end of the 10-day event window after the election, compared with the least regulated firms. Thus, investors considered political connections less valuable for firms in the most regulated industries than for those in the least regulated industries, consistent with the hypothesis that deregulation decreases opportunities for firms to seek political favors and capture regulators.

D. Robustness of Mechanisms

I also validate the robustness of the results by calculating the abnormal returns, as in Section III.B.1, using a 1-factor model that represents the return of global markets (except the U.S.), as measured by the returns of the ACWX. I rerun the specifications stated in Sections IV.A–IV.C by replacing the dependent variable with the abnormal returns calculated by adjusting the returns by ACWX. For brevity, I show the results for 5-day CAR in Table A6 in the Supplementary Material. As can be seen, the results remain qualitatively similar to those in Tables 6–8. For example, column 1 shows that firms in the most regulated industries *and* having higher ex ante growth opportunities experienced higher abnormal returns than other firms. Column 2 shows that firms in the most regulated industries *and* with lower ex ante competitive threat, experienced lower abnormal returns than other firms. Finally, column 3 documents that firms that could possibly gain from connection to Republican members of Congress *and* were in the most regulated industries also experienced lower abnormal stock returns than other firms.

Overall, the results presented in Table A6 in the Supplementary Material demonstrate that the economic mechanisms documented above are not sensitive to the selection of the model for calculating abnormal returns.

V. Real Effects

Thus far, the results document how investors expected possible deregulation to impact firms in the most regulated industries. However, it remains unclear whether firms' fundamentals over the few years following the 2016 election were consistent with investors' reactions around the election date, especially those of firms in the most regulated industries. Therefore, I explore whether firms in the most regulated industries experienced changes in their profitability, cash flow, or sales after 2016. I measure profitability by earnings before interest and taxes (EBIT) and cash flows (CASH_FLOW) by adding back depreciation and amortization to EBIT. As measures of sales, I use the natural logarithm of total sales (log(SALES)), and sales growth, measured as the yearly percentage change in sales (SALES_GROWTH). I estimate the following empirical specification:

$$(3)\qquad Y_{i,j,t} = \alpha + \sum_{N=1}^{4} \beta_{1\mathrm{N}} \times \mathrm{QUTILE\_N}_j \times \mathrm{POST}_{i,t} + \beta_3 \times \mathbf{C}_{i,j,t} + \gamma_{j,t} + \nu_i + \varepsilon_{i,j,t}.$$

In the above equation, $Y_{i,j,t}$ is the profitability, sales, or sales growth for firm i in a NAICS 2-digit industry j in year t. QUTILE_N is a dummy variable indicating the four regulatory quartiles, as before. This equation is estimated for the sample period of 2014 to 2019. The variable POST takes the value of 1 for the years 2017–2019, and 0 otherwise. $\mathbf{C}_{i,j,t}$ is a vector of firm-level time-varying control variables, which are the logarithm of assets, debt-to-equity ratio, and tax rates. I also employ firm-fixed effects (ν_i) and NAICS 2-digit industry times year fixed effects ($\gamma_{j,t}$). Unlike the regressions in the previous sections, I cannot take into account unobserved time-invariant state-specific effects by employing state-fixed effects along with a firm-fixed effect (as none of the firms changed its location between 2014 and 2019). Hence, to account for the possible cross-correlation of firm fundamentals for firms within a state, I cluster the standard errors at the firm and the state level.

The results of the estimation are shown in Table 9. Most regulated firms had higher sales and sales growth during the years under the Trump administration than in the previous 3 years. The point estimates imply that firms in the most regulated industries (QUTILE_4) had 9.5% higher sales and 5.7% higher sales growth than firms in the least regulated industries during 2017–2019 than during 2014–2016. This is consistent with the abnormal stock returns for firms in the most regulated industries around the election day. However, as documented in the first two columns of the table, there were no significant differences in firm profitability or cash flows.

TABLE 9

Real Effects: Firm Fundamentals

Table 9 investigates if firm fundamentals changed over the 3 years after 2016 in accordance with the stock price reaction around the election day compared with 3 years before the election. The dependent variables in columns 1–4 are EBIT, CASH_FLOW, natural logarithm of sales (log(SALES)) and SALES_GROWTH, respectively. EBIT is earnings before interest and taxes, CASH_FLOW is EBIT with depreciation and amortization added back and SALES is the revenue of a firm. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions, QUTILE_1 is the reference category. The regressions also control for logarithm of firm assets, expected tax rates and debt-equity ratio as time-varying firm-level controls. The regressions are estimated on a sample period between 2014 and 2019. POST is a dummy variable taking the value of 1 for the years 2017–2019, and 0 otherwise. All variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. *N* presents number of firms in the regression. The regressions are with 2-digit NAICS industry × year-fixed effects and firm-fixed effects. Standard errors are clustered at the firm and state levels. *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	EBIT	CASH_FLOW	log(SALES)	SALES_GROWTH
QUTILE_2×POST	0.022	−0.109	0.037	0.008
	(1.12)	(−0.39)	(0.95)	(0.36)
QUTILE_3×POST	−0.020*	−0.678	−0.014	0.002
	(−1.96)	(−1.43)	(−0.48)	(0.11)
QUTILE_4 × POST	−0.001	0.178	0.092***	0.057***
	(−0.04)	(0.46)	(3.22)	(3.32)
No. of obs.	10,576	10,392	11,122	11,590
Other controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
2-digit NAICS × year FE	Yes	Yes	Yes	Yes

Furthermore, in Table A7 in the Supplementary Material, I document that firm fundamentals also changed consistent with the economic mechanisms as implied by the stock price reactions. For example, the point estimates in Panel A imply that a 1-standard-deviation increase in Tobin's Q was associated with a 1.9% (= 1.56 × 0.012) increase in profitability or a 10% increase in sales or 2.6% increase in sales growth for firms in the most regulated industries compared with firms in the least regulated industries during 2017–2019 than during 2014–2016. I find similar results for competition and political favoritism in Panels B and C, respectively.

While the point estimates are not always statistically significant, these results are suggestive evidence consistent with the initial stock price reaction. Ex ante, there are at least three reasons that might bias against finding any significant effect on firm fundamentals. First, it might take many years for deregulation to impact firm fundamentals. Given that the Trump presidency lasted for 4 years, the time horizon may be insufficient to observe such effects. Second, the previous results on stock returns capture an intention-to-treat effect rather than a treatment effect. Hence, it is still possible that there was insufficient deregulation to affect firm fundamentals. Finally, it is possible that the Trump administration was not able to carry out effective deregulation due to political and bureaucratic hurdles, as noted by Belton and Graham (2019) and Coglianese et al. (2021). Hence, the evidence, although weak, provides additional credibility to the economic mechanisms driving the main set of results.

As noted by Hassan, Hollander, van Lent, and Tahoun (2019), a significant portion of firm-level political risk, as measured from the quarterly earnings conference calls, is driven by regulation and legislation. If the Trump administration was lenient on the enactment and enforcement of regulations then market participants might have expected lower political risk for firms in the most regulated industries during the Trump administration years than before. I further examine this possibility in Table 10. For this analysis, I collect firm-level quarterly political risk scores from Hassan et al. (2019) and analyze them over the period of 2014 to 2019, as before. The dependent variable in the regression is the natural logarithm of one plus the firm-level political risk score (PRISK). The coefficients of interest are the coefficients on the interaction term of each regulatory quartile dummy and POST. As before, POST is a dummy variable that takes a value of 1 for the years 2017–2019, and 0 otherwise. I additionally employ firm-fixed effects and year-fixed effects. As can be seen in columns 1–3 of Table 10, firm-level political risk decreased by 9% to 18% for the most regulated firms during the Trump administration years of 2017–2019 than in 2014–2016. In column 3, I further employ a NAICS 2-digit industry and year-fixed effects. The results remain statistically significant and economic magnitude also increases.

Overall, the results in this section document evidence that firm fundamentals (particularly those of firms in the most regulated industries) during the Trump administration years reacted in line with what investors had expected around election day in 2016.

VI. Conclusion

Using the election of Donald Trump as the President of the United States as an unexpected exogenous shock to the expectation of future deregulation in

TABLE 10

Real Effects: Political Risk

Table 10 investigates if firms in the most regulated industries experienced lower political risk during the Trump presidency. The dependent variable is natural logarithm of the text-based firm-level political risk (PRISK) as measured by Hassan et al. (2019) from the quarterly earnings conference calls. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions, QUTILE_1 is the reference category. The regressions are estimated on a sample period between 2014 and 2019. POST is a dummy variable taking the value of 1 for the years 2017–2019, and 0 otherwise. The regressions control for logarithm of assets, expected tax rates, and debt-equity ratio measured at a firm-year level. All variables are defined in Table A1 in the Supplementary Material. All nonlogarithmic continuous variables are winsorized at 1 and 99 percentiles. *N* presents number of firms in the regression. Standard errors are clustered at the firm and state levels. *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	PRISK		
QUTILE_2 × POST	0.014	0.042	0.076
	(0.15)	(0.45)	(0.95)
QUTILE_3 × POST	−0.039	−0.041	−0.030
	(−0.49)	(−0.49)	(−0.24)
QUTILE_4 × POST	−0.088*	−0.120**	−0.182***
	(−1.86)	(−2.05)	(−2.88)
log(TOTAL_ASSETS)		0.146***	0.108***
		(4.20)	(3.50)
EXPECTED_CASH_TAX_RATE		−0.022	−0.023
		(−0.32)	(−0.33)
DEBT_TO_EQUITY		−0.003	−0.003
		(−0.11)	(−0.11)
	(214.65)	(4.98)	(5.84)
R^2	0.31	0.31	0.31
No. of obs.	39,010	31,116	31,116
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	No
2-digit NAICS FE × year FE	No	No	Yes

the U.S., I find that firms in the most regulated industries gained approximately \$25 million more than firms in the least regulated industries during the 10 trading days following the election. This indicates that the markets expected that due to deregulation, approximately, \$27 billion more benefits would accrue to firms in the most regulated industries than for firms in the least regulated industries. This price reaction is significant after controlling for a host of possible confounding factors and alternative specifications. The positive effect on the valuation of the most regulated firms is driven by high-growth firms. Firms with less competitive threats lost market value in the most regulated industries, implying that increased regulations benefit incumbent firms by shielding them from competition. Additionally, I provide novel evidence that political favoritism becomes less valuable for more regulated firms under the expectation of deregulation.

However, there are limitations to the study. While the results provide suggestive evidence of a causal impact of the Trump election on expected deregulation, they do not confirm a causal relationship, as it is challenging to do so based on a single event. Furthermore, this study does not speak to the overall welfare effects of changes in the regulatory environment. Additionally, the study does not explore the effect on private firms. Furthermore, changes in the scale and complexity of regulations and their effects on firms are not explored. These topics are left for future research.

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Supplementary Material

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