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MEASURING THE COST OF REGULATION: A TEXT-BASED APPROACH

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Abstract

We derive a measure of firm-level regulatory exposure from corporate earnings calls text and study its effects on growth, leverage, profitability, and equity returns. Higher regulatory exposure results in slower sales and asset growth, lower leverage, reduced profitability, but higher post-call equity returns. Most of these effects are mitigated for larger firms. We validate our measure by showing its explanatory power for cross-sectional stock returns during the 2016 Presidential election.

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I. INTRODUCTION

Regulation is often justified by the gains to the public that come from outcomes such as cleaner water and air, safer travel, less dangerous products, and more honest advertising. The costs of regulation are borne in large part by the firms that must comply with them. Costs can be roughly categorized into two sets: *physical operational costs* and *compliance risks*. The former are costs related to regulation's mandated changes (relative to what firms would otherwise do) in production, distribution, or sales practices. The latter are uncertainties about prospective future mandates. Some approaches to regulation can be inherently riskier. For example, since the 1970s, regulators have relied increasingly on "guidance" rather than formal rulemaking in setting regulatory standards (DeMuth 2016, Epstein 2016, Calomiris 2018), which observers believe has increased regulatory compliance risk. Guidance gives regulators greater flexibility in implementing regulation, but of course, that same flexibility implies greater uncertainties for firms about how regulation will evolve. Such uncertainty may in turn prevent firms from undertaking attractive investments due to the fear of an unforeseen regulatory response. Although many observers often express the belief that regulation is costly to firms both through its operational burdens and its compliance risks, research has not made much progress in measuring these costs in a comprehensive manner (one that accounts for both risks and physical compliance costs), or even demonstrating that, in general, regulations *are* costly to firms. Recently, the Trump Administration claimed that deregulation was an important contributor to the acceleration of growth in the years after Trump's 2016 election, but there is no hard evidence to quantify whether that is true, or if so, how much of that growth should be attributed to deregulation.

Ours is not the first paper to measure the cost of regulation or to use textual data for the measurement of regulation. With respect to the latter, several recent studies make use of the data produced by the Mercatus Center at George Mason University (GMU), which tracks the word flow of the federal government's formal rule making, and has devised a means of attributing the relevance of that word flow at the sectoral level in the economy. The top panel of Figure 1 shows the GMU measure as an annual average across all economic sectors. By this measure, regulation is increasing over time. However, measures that assess regulation via the importance of the requirements it imposes–for example, the number of regulations passed with high estimated compliance costs, compiled by George Washington University (GWU), and reported as an aggregate time series–show a precipitous decline in regulation in the first year of the Trump Administration. This is shown as the solid line in the top panel of Figure 1. The contrast between these two aggregate regulatory measures suggests a better approach is needed.

Our goal in this paper is twofold. First, we address the regulation measurement issue by proposing a novel text-based measure derived from corporate earnings calls. As we point out in our discussion of prior research, most other studies have employed data that are likely to identify physical operational costs, but not costs related to compliance risks. Because earnings calls capture news that is discussed between firm management and investors they are well-suited to identifying regulatory risks faced by firms. At the same time, discussions in earnings calls may not capture longstanding operational costs. From that perspective, our measure is geared toward identifying new costs, including new compliance risks, but may understate longstanding physical costs. For that reason, we see our paper as complementing other contributions in the literature by being able to measure costs related to compliance risk that other studies have not identified.

Second, if news about regulatory risk is an important cost highlighted by earnings calls, this can be validated by analyzing the reaction of a combination of dependent variables to news about regulation. Although we are not able to formally test the extent to which risk vs. new physical cost are driving our results, we develop two hypotheses that serve to organize the empirical implications of regulatory exposure depending on whether it reflects increased compliance risk or increased physical operational costs. We expect both kinds of regulatory exposure discourage growth, but they have different implications for other variables. Hypothesis 1 states that costs resulting from increased compliance risk should be reflected not only in lower growth but also in higher future expected stock returns and lower leverage (defined as the ratio of debt to the market value of assets). [1] Ceteris paribus, higher risk should cause firms: to reduce investment, resulting in lower growth; to reduce leverage as the likelihood of distress makes the tax advantage of debt less attractive; and to compensate equity investors more for bearing more risk. Hypothesis 2 states that increased current physical costs (as opposed to risks) should be reflected in both reduced growth and in immediate reductions in profit margins. We return to these hypotheses, summarized in Table 1, in our discussion of empirical findings. We think the findings across all the *NetReg* variables suggest that our measure captures both channels, and there are some results (the impact of our measure of new regulation on equity risk premia and leverage) that seem clearly linked to new risks related to regulation.

We apply natural language processing (NLP) methods to a corpus that filters the word flow related to regulation on the basis of its importance. Specifically, we undertake an NLP

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[1] This implication for leverage is a standard result of the tradeoff theory of leverage in which higher debt has the advantage of tax savings but the disadvantage of prospective costs of financial distress, which are an increasing function of asset or revenue risk. Kalmenovitz (2023) finds that increased paperwork costs are associated with higher debt and higher leverage. One can imagine that, if debt is the marginal source of funding (as in the pecking order theory of debt), a shock to costs mechanically could lead to an immediate increase in leverage. Because our observed leverage response occurs with a protracted lag, it must be the result of capital structure planning, not just a mechanical response to a shock to cash flow.

analysis of the transcripts of the earnings calls of publicly traded corporations. Earnings calls are quarterly opportunities for stockholders to hear from and question management about all the important influences on the values of companies. They are a primary opportunity to recognize news (including regulatory news) and discuss its strategic implications. For that reason, earnings calls are likely to highlight all elements of regulatory news, including both new compliance costs and new regulatory risks. Given the limited duration of the earnings calls – they last about one hour – if management and investors use the scarce resource of time to discuss regulation, that is a reliable indicator of its importance. Furthermore, since time is scarce for all earnings calls, the methodology should work equally well for identifying important regulatory issues across all industries. Finally, earnings calls are an opportunity for management and investors to share thoughts about strategy and risk on a forward-looking basis. If regulation entails compliance risks, this should be a key place where those concerns are reflected.

Because we analyze each company's earnings calls separately, if the same regulatory news favors one company and harms another, our measure will capture such differences in regulatory exposure. For example, utility regulation that reduces energy costs may harm utilities but help firms that use the energy produced by utilities. Indeed, we find some evidence that this is true.

Earnings calls also permit investors to question management, which means that important aspects of news about regulatory costs that may be neglected or exaggerated in management's presentation can be raised by investors in their questions. Of course, knowing that they will be questioned may make managers raise regulatory issues in anticipation of investors' concerns. Interestingly, for most regulatory consequences, the analysis of the Presentation section produces the strongest and most robust estimates of regulatory consequences. In contrast, however, we find that only the Q&A portion of calls is statistically significant for forecasting one-month ahead stock returns. This suggests that focusing on regulatory discussion in both the Presentation and Q&A portions of calls yields useful information.

For each earnings call, we construct separate measures for the management Presentation and Q&A parts of the transcript. Our measure of regulation is *NetReg*, which captures both mentions of regulation (via the presence of the word root *regulat*) and its direction: positive (negative) *NetReg* indicates greater (lesser) regulatory burden. As we discuss further below, and illustrate in the middle panel of Figure 1, this directional measure of regulation provides a dramatically different picture of regulation than one would get from just measuring the frequency of the use of regulation-related words in earnings calls, or measuring the quantity of word flow in government publications related to regulation. To measure the direction of regulation, we introduce two new word lists that are associated with increasing and decreasing regulatory burden, respectively.[2] As a benchmark, we also construct a measure that captures the sentiment score of the transcript as a whole (*AllSent*). Sentiment is measured using the Loughran and McDonald (2011) sentiment dictionary.

Our measure of regulation captures not only new rules and guidance created by all the relevant domestic and foreign government sources, but it also captures how regulations are enforced for individual firms, and how particular firms experience changes in both rules and their enforcement over time. A simple analysis of variance, reported below, suggests that the majority of variation that our measure captures is "within-firm" variation, rather than changes that are common across firms within a particular sector, or differences across firms that are constant, or variation across time that is common across all firms. Almost all of the variation

[2] Table 3 shows a list of the increasing and decreasing words and Table 4 shows sample regulatory sentences.

reflects idiosyncratic nuances in the changing ways that regulations are imposed on a particular firm, though our measure also reflects occasional economy-wide regulatory shocks.

Our results indicate that more regulation has major negative implications for the future growth and profitability of firms, that more regulation forecasts lower future firm leverage, that both compliance risk and physical operational costs are channels through which regulation affects firms, and that compliance risk is likely the more important of the two channels. We also find that regulation has fewer negative consequences for large firms than for small ones (see also Davis 2017). This result is consistent with a large literature on the political economy of regulation that sees regulation as less harmful to large firms because of their superior ability to lobby or the economies of scale in managing the operating costs and compliance risks associated with regulation, which in turn implies consequent competitive advantages of large firms over small firms that arise from greater regulation.[3]

We find that the predictive power of the Presentation section for firm outcomes appears to come largely from its time-series variation, whereas the predictive power of the Q&A *NetReg* measure depends more on its cross-sectional variation. We hypothesize that when a firm faces an important and novel regulatory development it explains it in the Presentation section of the call which makes the associated regulatory discussion in the Q&A section less informative (since the important new information has already been revealed).

To further validate our measure, we show that *NetReg* is useful for capturing exogenous variation in regulatory costs and for interpreting the cross-sectional variation in firm exposure to the surprise 2016 election of Donald Trump. Trump had campaigned partly on a platform of pro-

[3] Important theoretical contributions include Olson (1965), Stigler (1971, 1988), Krueger (1974), and Peltzman (1976). For discussions of the advantaged role of large firms in the regulatory process in finance, see Calomiris and Haber (2014), Chapters 7-8, Kirilenko, Mankad, and Michailidis (2014), Gordon and Rosenthal (2016), and Libgoer (2020).

growth deregulation with a focus on specific types of businesses. We find that while firms, on average, benefited from deregulation after Trump's election (in the sense that the impact of pre-election measure of *NetReg* on sales growth declined post-election), this effect was almost entirely concentrated in firms with high stock returns around the surprise election results. We also find those firms saw a relatively large post-election reduction in *NetReg*. This suggests that the stock market quickly understood which firms would particularly benefit from the election of Donald Trump, and that an important part of this benefit was a decrease in the impact of firms' regulatory burdens. We further show that the largest impact on firm discussion of the regulatory impact of Trump's election happens in the Presentation section, which supports our hypothesis that news about major regulatory events is largely contained in the Presentation section of earnings calls. Our text measures are available at http://www.measuringregulation.com.

In the Appendix, we provide a review of the literature on measuring the costs of regulation. Here we focus only on the fact that our *NetReg* measure captures information that is not closely related to prior measures of regulatory cost, which we believe reflects its comprehensive coverage of both new immediate costs and new risks that firms and their investors find worthy of discussion (as opposed to word counts of new regulations unweighted by importance, or measures of existing physical compliance costs, or measures that focus on other types of influences, such as political risks). Table 2 shows the lack of correlation between *NetReg* and the measures derived in other studies (Hassan, Hollander, van Lent and Tahoun 2019; Armstrong, Glaeser, and Hoopes 2023; Chang, Kalmenovitz, and Lopez-Lira 2023; Kalmenovitz 2023; Kalmenovitz, Lowry, and Volkova 2022).[4] Furthermore, in our regression analysis, we consider whether the effects captured by *NetReg* are also captured by other

[4] We do not include Donelson, Garfinkel, and Roudini (2023) in the comparison because it is a time-series, not a firm-level, measure.

measures of regulation in the literature (whose authors were willing to share their data with us – including Donelson, Garfinkel, and Roudini 2023, Armstrong, Glaeser, and Hoopes 2023, Chang, Kalmenovitz, and Lopez-Lira 2023, Kalmenovitz, Lowry, and Volkova 2023, Kalmenovitz 2023, and Hassan, Hollander, Van Lent, and Tahoun 2022). As we discuss later, we find that *NetReg*'s predictive importance is not affected by including measures from other regulatory studies in the regression analysis.

II. DATA

Our measures of regulation are derived from textual analyses of all quarterly earnings calls of publicly traded firms from S&P Global's Transcripts Data from 2009-2019. We merge these conference call data with pricing and accounting information for U.S. firms from CRSP and Compustat starting in 2008.[5] From CRSP, we collect daily stock returns, number of shares outstanding, and trading volume for firms traded on the NYSE, Nasdaq, and Amex. From Compustat, we obtain quarterly information on firm fundamentals. We exclude financial services firms (SIC codes beginning with 6) because performance measures, such as sales growth, for financial services firms are non-comparable to other firms.

Our primary measures of firm fundamentals are annual sales growth, annual asset growth, leverage (current liabilities and long-term debt over total assets), and operating (operating income over sales) and gross margins (sales minus cost of goods sold over sales), and annual changes in margins. We allow the consequences of regulation to depend on firm size. To

[5] We use a mapping provided by SP Global which associates an earning call's company identifier, *ciqCompanyID*, to Compustat's company identifier, *gvkey*. While there are instances where a *gvkey* is associated with multiple *ciqCompanyId*'s (this happens for 4% of all *gvkey*'s), the *gvkey-date* to *ciqCompanyId-date* mapping is unique (except for 4 firm-quarter observations which do not impact our results). We require that observations in S&P Global have valid CRSP *PERMNO* and Compustat *gvkey* identifiers.

measure the size of the firm, we use log sales over the quarter associated with the earnings call, with sales measured in millions of dollars.

All variables are measured relative to the quarter associated with the earnings call. As an example, for the quarter ending on June 30, 2012, Apple had its earnings release and conference call on July 24, 2012. All growth numbers are then relative to June 30, 2012.

Not all earnings calls discuss regulation, we introduce a *NoRegulat* dummy variable that equals one for firms that have mentioned regulations in some earnings call in our sample, but not in the present one, and is zero otherwise. Some firms in our sample never mention regulations in any of their earnings calls; for such firms we introduce a dummy variable *NeverRegulat,* which is set to one for all of their firm-quarters.

To study the implications of regulation for stock returns, we examine returns, both in excess of the risk-free rate (1-month T-bill) and risk-adjusted, using the Fama-French (2015) 5-factor plus momentum, over 1-, 5- , and 22-trading days following the earnings call. Factor loadings used to calculate abnormal returns and alphas are estimated over a window from 252 to 31 trading days prior to the earnings call; for abnormal returns, the training window alpha is assumed to be zero. Returns are measured from the closing price of day t (the date of the conference call) for calls occurring prior to 4 PM New York time and from the closing price of day $t+1$ (the following trading day) for calls occurring at 4 PM New York time or afterwards. This timing ensures that our future returns are not contemporaneous with the information revealed in the earnings call. Contemporaneous returns are either from day $t-1$ to day t, or from day t to day $t+1$, depending on whether the call is pre- or post-4 PM. For our Apple example, the July 24, 2012 conference call took place after 4 PM New York time. The 22-day return is measured from the close of July 25, 2012 to the close of August 24, 2012. The estimation window for the calculation of alphas and risk factor loadings runs from July 25, 2011 through June 8, 2012.

To mitigate the influence of outliers in our regression analysis, we winsorize standardized unexpected earnings and log turnover (see Section IV.B), as well as sales growth, asset growth, leverage, operating margin, gross margin, operating margin growth, gross margin growth, and SG&A expense, at the two percent level (impacting 4 percent of the observations). We also tried winsorization at the one percent level, which resulted in nearly identical results. Excluded from all firm performance regressions are firm quarters with missing values for total assets, or sales that are missing or are below $5 million. These quarters represent 21% of the sample. Table 5 summarizes the variable definitions, and Table 6 provides summary statistics for the data.

III. MEASURING REGULATION

Our text analysis is performed on the earnings call data set obtained from S&P Global. Before analyzing the calls, we perform the following cleaning steps: convert all words to lowercase; take out whitespace; remove stop words; tokenize and stem all words (for further details on these standard procedures see Gentzkow, Kelly, and Taddy 2019). For the sentiment analysis described below, we perform word negation, following the algorithm in Das and Chen (2007), which appends the string "_NEG" to all words in a sentence which follow an English language negation word, such as "don't" or "not". Word negation was performed prior to all other cleaning steps. In our sentiment analysis, we ignore negated sentiment words.

Our measure of regulation, labeled *NetReg*, can be positive or negative. Negative values indicate reduced regulation (or deregulation) and positive values indicate more regulation. To construct this measure, we begin by separately searching the Presentation and the Q&A parts of each quarterly transcript for the word root "*regulat*," which identifies the words that indicate the presence of a discussion of regulation (*regulate*, *regulated*, *regulation*, *regulator*, *deregulate*, etc.). We only focus on sentences mentioning *regulat* as well as one of a set of regulatory Concept words, to avoid instances of the use of *regulat* in other contexts (like engineering applications); we refer to such sentences as *regulatory sentences*. Focusing on regulatory sentences, as opposed to all sentences with *regulat*, has a minor effect on our *NetReg* measure. A further explanation of this filter and the list of Concept words are in Section 2 of the Online Appendix, and in Tables A1 and A2.

To explore the novelty of the influences captured by *regulat* when measuring discussions of regulation, we also considered a broader approach that included other words that might be relevant for regulation, such as *law* and *rule*. Our prior, based on reading a sample of sentences, was that these words would not be very useful because, as we discuss further below, regulation discussions are largely about enforcement, not new laws or rules. To validate our reliance solely on *regulat*, we enlisted the help of ChatGPT. We randomly selected sentences from the earnings call corpus that either contained *law*, *rule*, or the *regulat*. For these 3,000 sentences, we then asked ChatGPT to say whether those sentences contained salient information about the regulatory environment faced by the firm. For rule and law, ChatGPT found that the answer was yes less than half of the time (48% of the sentences including *rule* and 44% of the sentences including *law*). In contrast, ChatGPT answered yes for 90% of the sentences that included *regulat.* These results are summarized in Table A3 of the Online Appendix.

To gauge whether the discussion is one of increasing or decreasing regulation, we identify “Increasing” or “Decreasing” words that co-occur in the same sentence as *regulat* and convey a sense of increasing or decreasing regulatory exposure, respectively. These words are listed in Table 3 in order of their frequency of occurrence. Examples of sentences in which *regulat* is accompanied by Increasing or Decreasing words are provided in Table 4. It was from reading the context of these, and many other, sentences that we were able to judge whether words convey a sense of increasing or decreasing regulatory exposure. For example, it is not clear on an a priori basis whether the word "*adapt*" should be considered an Increasing word, a Decreasing word, or neither. By reading transcripts one discovers, however, that "*adapt*" is often used to indicate the need for a firm to adapt to an increased regulation; if regulations did not increase, there would be no need to adapt. Two examples illustrate the point: "We are well prepared to *adapt* to the changing legislative, regulatory and economic environment."; "Of course we're *adapting* our business model to the reality of regulation as it exists through the FDA [Food and Drug Administration]". More examples are in the Online Appendix. We emphasize that the choice of words indicating increasing or decreasing regulation was made by us prior to conducting the regression analysis in Section 4.

Our *NetReg* measure takes all regulatory sentences in the Presentation and Q&A sections, respectively, calculates the difference between the number of Increasing and Decreasing regulatory words occurring in those sentences, and divides by the total number of words in these sentences after stop words have been removed, i.e.,

$$NetRegS = \frac{N_S(Increasing\ regulatory\ words) - N_S(Decreasing\ regulatory\ words)}{N_S(All\ words)}, \tag{1}$$

where $S \in \{P, QA\}$ refers to the section of the call, and $N_S(\cdot)$ counts the number of occurrences of a particular word group in regulatory sentences of section *S*, excluding stop words. A higher (lower) value of *NetReg* implies a higher (lower) regulatory burden. Sections of calls with no regulatory sentences have *NetReg* of zero, indicating neutral regulatory tone. Our simple filter does a good job of (a) identifying meaningful regulatory references in earnings calls, as well as

(b) identifying the directionality of the reference. Furthermore, our regulatory measurement procedure is straightforward to implement, once the list of words in Table 3 is available.

We regard the use of subjective judgment in constructing the lists of Concept, Increasing, and Decreasing words as unavoidable for a simple reason: in the context of measuring regulation's impact, it is very challenging to use supervised learning techniques to identify these words. A natural supervised technique would be to infer Concept, Increasing and Decreasing words by identifying combinations of words that tend to result in positive or negative stock returns at the time of the earnings call.[6] The problem with this approach, however, is that there are many important high-frequency influences, other than regulation, on stock prices that are revealed in the earnings call, and thus the effect of regulatory mentions on the contemporaneous stock return may get swamped by these other factors; furthermore, it is not a priori obvious whether more regulation is good or bad for stock prices.

To validate our approach to measuring positive or negative regulation, we asked ChatGPT to read all the sentences in our database that contain *regulat*, and to score the sentences as indicating increasing, neutral or decreasing regulation. We then use that ChatGPT score to construct alternative measures of *NetRegP* and *NetRegQA*, which we label $NetRegP^{LLM}$ and $NetRegQA^{LLM}$, respectively (where LLM stands for large language model). The LLM measures consists of *NetReg* from (1), but applied to sentences where ChatGPT does not disagree with the sign of *NetReg*, evaluated using (1) at the sentence level. If there are no regulatory sentences which pass the no-disagreement filter, then the respective $NetRetP^{LLM}$ and $NetRegQA^{LLM}$ measures are left blank, i.e., unobserved. More details are in Section 2 of the Online Appendix.

[6] Examples of this approach include Ke, Kelly, and Xu (2018) and Glasserman, Krstovski, Laliberte, and Mamaysky (2020).

We, of course, do not know how ChatGPT reached its conclusions, and we would be uncomfortable relying on them entirely, but it is interesting to note that there is general agreement between the direction of regulation indicated by our *NetReg* measures and what is identified by ChatGPT. In our forecasting regressions, the coefficients on *NetRegP* and *NetRegQA* are nearly identical to those on $NetRegP^{LLM}$ and $NetRegQA^{LLM}$, which we discuss in Section III. As referenced in our literature review, we also show in Section III that including other studies' measures of regulation (which are not correlated with our measures, as shown in Table 2) does not affect our regression results for the effects of *NetReg*.

We also use the Loughran and McDonald (2011) (LM) sentiment dictionary to identify positive and negative sentiment words in the earnings calls. We then define two sentiment-related measures for each part (Presentation and Q&A) of each call. The first of these, *RegSent*, which we constructed for purposes of comparison with other studies' measures, computes the sentiment score for each sentence in which *regulat* appears together with a Concept word after dropping stop words, i.e.,

$$RegSentS = \frac{N_S(LM\ positive\ words) - N_S(LM\ negative\ words)}{N_S(All\ words)},$$

for the $N_S(\cdot)$ used in equation (1). The second sentiment-related measure, *AllSent*, calculates the sentiment score for the entire Presentation or Q&A discussion of the earnings call, after removing stop words, i.e.,

$$AllSentS = \frac{A_S(LM\ positve\ words) - A_s(LM\ negative\ words)}{A_S(All\ words)},$$

where $A_S(\cdot)$ measures the number of occurrences of a particular word group in all the sentences of section *S*. *AllSent* does not focus on regulation, but rather is useful as a benchmark for the effects of sentiment in general, against which to compare the effects of regulation captured in *NetReg*.

We recognize the challenges in measuring regulation. *NetRegP* and *NetRegQA* are not flawless measures: they can and will be improved by future researchers. The extent to which they are imperfect, but pertinent, measures introduces an errors-in-variables problem that works *against* us finding economically or statistically important results. Yet, as we show below, we do find that these measures are associated with large and statistically significant consequences for firms' performance. The ChatGPT-augmented $NetReg^{LLM}$ versions of our variables are even stronger forecasters of future outcomes. Our results likely provide a lower bound estimate for the impact of regulation on firms' outcomes because our measures of regulation are not perfect.

A. Regulatory trends

The middle panel of Figure 1 plots the time-series paths of $NetRegP_t$ and $NetRegQA_t$, which measure the quarterly equal-weighted average of *NetReg* for the Presentation and Q&A segments of calls respectively. It is interesting that these two aggregate measures, plotted in Figure 1, are not highly correlated (with a correlation of only 0.03). Nor do they exhibit similar low frequency variation. This highlights the advantage of considering the contents of the Presentation and Q&A sections separately, as we do. Management may not have an incentive to highlight all problems or risks, including those related to regulation, while investors' questions may be directed precisely at topics about problems or risks that management seeks to avoid.

The bottom panel of Figure 1 shows the proportion of earnings calls mentioning *regulat* in regulatory sentences. We see a clear, but small, upward trend. From 2010 to 2019 the proportion of earnings calls in which regulation is discussed rises from about 34% of the Calls to about 38% of them. The series has a large spike in 1Q2017, the quarter following the Trump election.

Figure 2 plots the four sentiment-related measures, which differ according to (a) whether sentiment is measured only within the sentence in which regulation is discussed or in the entire earnings call, and (b) whether they are constructed from the Presentation or the Q&A portions of the earnings calls. Some highly interesting patterns emerge, which we believe are intuitively appealing, and which help to validate these measures. First, sentiment scores for the Presentation portions are higher than the comparable sentiment scores for the Q&A portions (that is, $AllSentP_t > AllSentQA_t$ and $RegSentP_t > RegSentQA_t$). Unsurprisingly, management tends to be more sanguine than investors during earnings calls. Second, the sentiment scores of sentences in which regulation is the topic tend to be lower than the earnings calls as a whole (that is, $AllSentP_t > RegSentP_t$ and $AllSentQA_t > RegSentQA_t$). In other words, compared with other topics discussed in earnings calls, discussions of regulation, perhaps not surprisingly, tend to have more negative sentiment, whether it is discussed by management or investors. Third, sentiment scores are rising over time (sensible if improvements in economic activity are reflected in more positive sentiment), and similarly, there is some evidence that sentiment scores rose at the end of 2016 (the beginning of an acceleration in economic growth) for all four measures. Our two approaches to measuring regulation, *NetReg* and *RegSent*, are negatively correlated, as expected ($NetRegP_t$ and $RegSentP_t$ are correlated -0.53, and $NetRegQA_t$ and $RegSentQA_t$ are correlated -0.33 as shown in Online Appendix Figure A1). The definitions of the text variables are in Table 5, and summary statistics are in Table 6.

B. Case studies

To build intuition for how our measures capture the regulatory environment faced by firms, we examine *NetReg* and *AllSent* for Duke Energy, a large utility operating in many regulated markets. Duke devotes a relatively large fraction of its earnings calls, both in the Presentation and the Q&A sections, to discussing its regulatory landscape. Duke mentions regulations in most of its earnings calls, as can be seen from its nearly complete *NetReg* time series (Figure A2 in the Online Appendix). We restrict attention to sections of calls that mention Increasing or Decreasing regulatory words from Table 3 five or more times in regulatory sentences. Sections with more regulatory modifier words are more informative about firms' regulatory exposures.

The August 7, 2013 Duke Energy earnings call contains the Presentation section with the lowest *NetRegP* score among all Presentation sections with five or more decreasing regulatory modifier words. We expect this call, therefore, to indicate an improving regulatory environment. Indeed, we first hear that "we [i.e., management] expect the second half of the year to be relatively stronger than the first half, primarily as a result of 3 items: First, constructive rate case outcomes." As a regulated utility, Duke periodically asks its state regulators to approve rate increases, and here expresses satisfaction with the allowed rate increases in this cycle. The company then discusses the regulatory approval of a decision to retire a nuclear power plant called Crystal River 3 on the west coast of Florida. It goes on to say that "2013 is an important year, with a number of regulatory proceedings to position the company for the future. We operate in constructive regulatory jurisdictions and have 5 approved or pending settlements with annual revenue increases of around $600 million. This will result in less regulatory risk to the company, as well as more rate certainty for our customers." Note the emphasis on the *risk* aspect of regulation. The company does not mention the physical cost of its regulatory compliance, but rather the lower level of future *regulatory risk*. It goes on to say that given its "focus on resolution of near-term priorities and constructive regulatory outcomes, we have positioned Duke for low *risk*, primarily regulated growth through 2015." Duke is pleased about the low risk of its regulated growth. Finally, the company points out that "[l]ow load growth, new technologies, new regulations and ongoing cost pressures are just some of the forces that require new thinking and action. This includes innovation and technology deployment, continuous improvement [of] regulatory mechanisms." Again, the management of Duke is focused on, among other matters, evolving regulations and improvement of the regulatory mechanism.

There are two main takeaways from this. First, our scoring methodology does indeed identify this quarter as a very positive one for Duke Energy from a regulatory perspective. Second, Duke is concerned primarily about the risk associated with its regulations, as well as with the smooth functioning of the regulatory process. Nowhere is there mention of the explicit cost of regulation to the firm or of the resources the firm expends to manage its regulatory environment. Risk and a rational regulatory process are the primary concerns.

We also analyze regulatory mentions by American Axle, an automotive parts manufacturer, and by AutoNation, a national car dealer. American Axle and AutoNation face lower regulatory scrutiny than does Duke Energy. These two firms discuss regulations on their earnings calls very infrequently. When they do, it is often in response to unusual regulatory developments. In 2018, American Axle is concerned about an unanticipated regulator-mandated electricity rate increase. In 2015, AutoNation discusses the increased "regulatory burden" of additional consumer protection regulations. These examples illustrate the strength of our method: we can identify infrequent, but important, regulatory mentions; obtaining similar information from other data sources is nearly impossible. These two case studies are analyzed in Section 3 of the Online Appendix.

C. Analysis of variance

In Table 7, we perform a variance decomposition of our *NetRegP* and *NetRegQA* measures, which allows us to identify how much of the variation in those measures reflects average sectoral effects, firm fixed effects, time effects, or the residual (within-firm variation, after controlling for all of those). We perform a Cholesky-type decomposition which assigns to each category of influence (in the order reported in Table 7) a percentage of variance. We also compare these variance decompositions to similar analyses on other, sentiment-based, measures.

The results are striking. Close to 90 percent of the variation in *NetRegP*, and close to 95 percent of the variation in *NetRegQA* reflects within-firm variation in earnings call discussions of regulation, which is unrelated to the variation captured by time, industry, and firm fixed effects. Changes in rules for all firms that are enforced similarly should produce common effects across firms (time effects). Similarly, such changes in rules for a given sector should produce common effects for firms in that sector (sectoral fixed effects). We interpret the lack of importance of industry and time effects as implying the importance of idiosyncratic enforcement of regulations for particular firms. Furthermore, because firm fixed effects also explain little of the variation in our measures, we infer that regulatory enforcement varies importantly over time for firms in ways that are idiosyncratic. Despite this finding, there are occasional large systematic regulatory shocks that our measures capture and that we discuss further in an event study of the impact of *NetReg* around the Trump inauguration.

There are obvious examples of such variation, such as approval of new drugs, or anti-trust policies related to mergers. Our variance decomposition results suggest that such idiosyncratic events related to the changing enforcement of regulation are the main source of firm-level discussions of regulation. It is interesting to note the contrast between the variance decompositions for our *NetRegP* and *NetRegQA* measures and the sentiment-based measures we construct for earnings calls. As Table 7 shows, within-firm variation contributes less to the variance of sentiment-based measures, suggesting that sentiment-based measures have a greater tendency to capture general news that is common across firms or across firms within a particular sector, or that is specific to a firm, but not time-varying.

D. Topics of regulation

We also investigated whether the importance of NLP measures of regulation varies according to the specific regulatory topic being discussed, where topics are identified as "clusters" of associated words. For example, it may be that when regulation is discussed in the context of some topics (e.g., mergers and acquisitions or M&A) it has more or less importance than in the context of other topics (e.g., FDA approval of the company's experimental drug). After all, management references to regulation can mean different things: passing or repealing a new regulation, beginning or ending an investigation or an enforcement action, approving or denying a merger, approving or denying a drug's use, to name only a few. It is conceivable that some of these topical contexts are more important than others. Previous work has shown that sentiment can have very different meaning depending on topical context (Calomiris and Mamaysky 2019), suggesting topic modeling is an effective unsupervised-learning tool for economics applications.

To conserve space, we report those results in a separate Topic Analysis Appendix (Calomiris, Mamaysky and Yang 2024). For our topic model, we use Latent Dirichlet Allocation (LDA), first proposed by Blei, Ng, and Jordan (2003); Steyvers and Griffiths (2007) is a good primer. We estimate a ten-topic (more on this shortly) LDA model separately for the Presentation and Q&A portions of our corpus. An LDA topic model is represented as two sets of distributions: each of the ten topics is a probability distribution over the words present in earnings calls (the topic-word distribution); and each document has a probability distribution over the ten topics (the document-topic distribution).

With respect to the Presentation Section, we find that six topics (*FDA*, *Fins*, *Legalese*, *M&A*, *Margins*, and *Util*) are stable – in the sense that they systematically appear across different runs of the topic model estimation – and distinct enough that they are used on a standalone basis in our empirical analysis. The *ProdMkt-Client* group combines two stable, but similar, topics. Finally, the *Euro-Legalese2* group combines two topics which are unstable across LDA runs, and represent effectively a residual topic category. With respect to the Q&A Section, we find that there are four distinct and stable topics (*FDA*, *Fins*, *M&A*, *Neg*) which are used in the empirical analysis on a standalone basis. One topic group, *ProdMkt-Client-EuroComp*, is composed of relatively stable topics that have high similarity to one another, and the other topic group *CorpFin-Util-Margins* is unstable across evaluation runs and represents the residual topic category for the Q&A earnings call sections.

IV. EMPIRICAL FINDINGS

In this section, we first present our results examining the effects of *NetReg* on future values of sales growth, and other dependent variables, including asset growth, leverage, and margins, using a variety of control variables. We explore differences in those results for large and small firms and perform other robustness tests on our specifications. We find that regulation negatively affects firms' sales growth, asset growth, leverage, and margins. The effects are larger for small firms. We show (summarized in Table 9, Panels A and D) that these findings are similar whether or not firm fixed effects are included in the regressions, although we note that empirical studies using natural language measures like *NetReg* tend to favor estimation without the inclusion of fixed effects.[7]

We then subject the above results to a series of robustness checks, including the introducing non-linearities in our *NetReg* measure to the regression specification, checking the forecasting power of our ChatGPT-augmented $NetReg^{LLM}$ measures, and performing a comparison of the forecasting power of our *NetReg* measures versus alternative measures of regulatory burden proposed in the literature. Our *NetReg* measure is robust in all cases.

The third part of our analysis looks at how the outcome of the election of 2016 (where deregulation featured prominently in the winning candidate's platform) changed the impact and magnitude of *NetReg*. We find that firms with high pre-election *NetReg* values experienced less of a decline in sales growth after the election than those values of *NetReg* normally would produce. This reduction in the cost of pre-election exposure to *NetReg* is greater for firms whose stock returns responded more positively to the news of Trump's election. We also find that firms with high positive pre-election values of *NetReg* tend to see *NetReg* shrink after Trump's election. This validation of our measure as a measure of the effect of exogenous regulatory changes (such as those that result from an election) shows that our measure captures at least some exogenous variation in regulation, which argues for, but of course does not definitively prove, a causal interpretation of *NetReg*.

Then, we show regulation discussions in the Q&A section of Earnings Calls have positive effects on future excess returns. This implies that Q&A discussions about regulation imply an

[7] Those studies include Gentzkow and Shapiro (2010), Larcker and Zakolyukina (2012), Loughran and McDonald (2014), Dyer, Lang, and Stice-Lawrence (2017), Soo (2018), Brown, Crowley, and Elliott (2020), Hassan, Schreger, Schwedeler, and Tahoun (2021), and Hassan, Hollander, Van Lent, and Tahoun (2022).

elevated level of risk, which is consistent with Hypothesis 1. Fifth, we expand the time-horizon over which the effects on firm performance and leverage may occur, and we find that most effects of regulation news on future performance manifest over a two-year response horizon; the exception is leverage, where negative effects from regulation continue to grow beyond that time horizon. Here, we consider the implications of our findings from the perspectives of Hypotheses 1 and 2, and therefore, about the likely relative importance of compliance risk and physical operational burdens for explaining our findings. Sixth, we show that *NetReg* is not forecasted by lagged measures of firm performance or by lagged stock returns. We interpret *NetReg,* therefore, as measuring unforecastable regulatory news. This implies that the usefulness of *NetReg* for forecasting future sales growth, leverage, and other firm performance measures likely reflects exogenous regulation news (unrelated to lagged characteristics or contemporaneous control variables), rather than predictable responses of regulatory events to changes in firm performance. In contrast, our general sentiment measure for earning calls, *AllSent*, is predictable by lagged firm characteristics.

Finally, we extend our analysis to measure differences in the importance of topical context for our NLP measures. Our LDA topic model allows us to identify which aspects of regulatory news have the largest effects on firm performance. We find that regulatory discussions related to mergers and acquisitions (*M&A*), actions by the Food and Drug Administration (*FDA*), the regulation of financial institutions (*Fin*), and utilities regulation (*Util*) stand out as areas of particularly large influence, and we explore possible interpretations of these topic areas, with details provided in the Topic Analysis Appendix (Calomiris et al. 2024).

We provide no formal test to identify whether the effect of regulations on future outcomes we measure with *NetReg* is causal or simply proxies for some unobserved firm heterogeneity. Of course, even if the effect is not causal, the forecasting relationship we study in the paper – of present *NetReg* on future outcomes – is still of interest to market participants, corporate managers, and regulators.

We do, however, control for many other possible influences on future firm outcomes, and find that none of them drive out the forecasting power of present *NetReg*. And the results reported in Table 2 and Tables A6 and A7 suggest that measures from related literature discussed above do not account for our findings. Furthermore, as part of our robustness analysis in Section IV.F, we find that *NetReg* is not a predictable consequence of changes in past firm performance, but that *NetReg* does forecast future firm performance. For all these reasons, we believe it is unlikely that *NetReg* merely proxies for unobserved heterogeneity; if that were so, it would have to be important unobserved heterogeneity that is unrelated to multiple dimensions of past firm performance, which seems unlikely.

Finally, the 2016 election validation check on our measure also provides direct evidence that, at least in part, *NetReg* captures the effects of exogenous regulatory change on firm outcomes. Not only is this regime change visible in the time series of an aggregation of our firm-level measures, but we also are able to test whether the impacts of regulation for firms most subject to high pre-election regulation diminish the most in the immediate aftermath of Trump's taking office, and we find that they do. We believe the totality of the evidence suggests (but, of course, does not prove) a causal interpretation of regulatory influences on firms' performance.

A. Effects of regulatory tone on firm performance measures

Our core analysis is a panel regression with firm-quarter observations, where we study how future firm outcomes depend on *NetReg* and *AllSent*. In our specifications, we control for other potential influences on future outcomes, including lagged firm size (log sales), lagged annual firm growth, leverage, or profitability, industry-level measures of firm regulation calculated as equal-weighted averages in the firm's 2-digit SIC industry over the prior 90 days, dummies indicating absence of regulatory mention, and industry fixed effects. We also consider a version of the basic specification which drops the industry fixed effect but includes firm and time fixed effects. The basic specification for firm *i* in quarter *t* is

$$G_{i,t\to t+4} = a_j + b \times CT_{i,t} + [b_1 \times CT_{i,t} \times I_{i,t}] + c^{\top} X_{i,t} + \epsilon_{i,t\to t+4}, \tag{2}$$

where $G_{i,t\to t+4}$ is the firm variable of interest (e.g. future sales or asset growth, year-ahead leverage, etc.), a_j is the 2-digit SIC industry fixed effect, $CT_{i,t}$ is the text-based "call tone" measure (*NetReg* or *AllSent*), and $X_{i,t}$ is the vector of the quarter *t* control variables described above. In many specifications, $X_{i,t}$ will also contain the lagged dependent variable. In some specifications, we include the $CT_{i,t} \times \log{(Size)}_{i,t}$ (i.e., $I_{i,t} = \log(Size)_{i,t}$) interaction to study how the effect of our text measure depends on firm log sales. In all specifications that interact firm size with our regulatory sentiment measures, we demean log sales using the full-sample mean. All specifications include two dummy variables: *NoRegulat* (which takes the value of one if the firm does not mention regulation in a given call) and *NeverRegulat* (capturing that a firm never mentions regulation in the entire sample, see Table 5).[8] These control for any selection bias associated with the presence of any mention of regulation in the earnings call. Standard errors are clustered by 2-digit SIC codes and by quarter.[9] Note that clustering by 2-digit SIC codes is more conservative than clustering by firm because the former allows for interfirm correlations within an industry, whereas the latter does not.

[8] For *NetReg*, the b and b_1 coefficients in (2) are effectively preceded by an indicator $1[regulat]$ set to one if *regulat* appears in the call in question and otherwise set to zero.
[9] These quarters are obtained from the Compustat variable *datadate*. For example, November 30 and December 31 will both be classified as being 4th quarter observations.

Table 8 provides detailed results on all the specifications used to analyze the forecasting relationship between lagged *NetRegP* or *NetRegQA* and year-ahead sales growth. These results, and our findings for other dependent variables, are summarized in Table 9, which normalizes effects by the standard deviations of the *NetReg* and *AllSent* measures, and where outcomes are expressed in percentage changes for each dependent variable. Table 8 (and similar tables in the Online Appendix) report results in raw (not normalized) form, where all dependent variables are also expressed in percent.[10]

Table 8 shows that, in both the Presentation and Q&A sections, the two *NetReg* variables are associated with large and highly statistically significant effects on one year-ahead sales growth.[11] The effect is robust to the inclusion of various controls, and either the inclusion of industry fixed effects, or, as an additional robustness check, firm and time fixed effects. We begin with a discussion of the specifications that do not allow the effects of *NetReg* to vary by firm size, and that do not normalize for cross-industry differences. As reported in Table 9, a one standard deviation increase in *NetRegP* forecasts a 1.56%, i.e., -30.181 (coefficient estimate from 1st column of Table 8) × 0.052 (standard deviation of *NetRegP* from Table 6), reduction in next year's sales. The comparable reduction in sales using the coefficient value for *NetRegQA* in column (2) is 0.97%, i.e., -18.019 × 0.054 (standard deviation of *NetReqQA*). The negative coefficients on *No Regulat* (current call does not mention *regulat*) and *Never Regulat* (*regulat* is never mentioned in any call for that company) indicate that companies whose earnings calls do not mention regulation tend to have lower sales growth in the subsequent year. Columns (3) and

[10] Section 1 of the Online Appendix provides a summary of all the exhibits contained therein.
[11] Lagged sales growth is mildly autoregressive. In our sales growth regressions reported in Table 8, coefficients on lagged sales growth range from 0.07 to 0.09. In a pooled regression with only lagged sales growth as the independent variable, we observe an even lower lagged sales growth coefficient. This is consistent with Chan, Karceski and Lakonishok (2003) who document low persistence in sales growth and other measures of firm growth over similar horizons.

(4) of Table 8 show the version of this regression with firm and quarter fixed effects. The results remain statistically and economically strong.

Columns (5) and (6) of Table 8 explore differences in the sales growth consequences of *NetReg* that are associated with firm size, by interacting *NetReg* with the full-sample demeaned firm size. In both the Presentation and Q&A sections of the earnings calls, there are positive coefficients on the interaction of firm size and *NetReg*; the effect is significant at the 10% level for the Presentation section, and significant at the 10.5% level for the Q&A section. Using both the simple coefficient values for *NetReg* and their interactions with size, for an average-sized firm, a one standard deviation increase in *NetRegP* is associated with a 2.2%, i.e., -42.667 (coefficient in column 5) × 0.052, decline in sales growth, but at the 75th percentile of size, the effect is a 1.7% decline in sales growth, i.e., -42.667 × 0.052 + 9.019 (interaction coefficient in column 5) × 0.052 × (6.813 - 5.644) (difference between 75th percentile and mean of size). For the largest firm in our sample (with log sales of 11.8, which is 6.2 above the mean), there is actually a small positive effect of 0.7% of *NetRegP* on sales growth. At the 25th percentile of size the effect is a 2.8% decline in sales growth, i.e., -42.667 × 0.052 + 9.019 × 0.052 × (4.419 - 5.644) (difference of 25th percentile and mean size). The comparable computation for *NetRegQA* results in a 1.7% decrease in sales growth, i.e., -31.278 (coefficient from column 6) × 0.054. At the 75th percentile of size, the effect is a decline of only 1.1%, i.e., -31.278 × 0.054 + 9.254 (interaction term in column 6) × 0.054 × (6.813 - 5.644). At the 25th percentile of size, the effect is a decline of 2.3%, i.e., -31.278 × 0.054 + 9.254 × 0.054 × (4.419 - 5.644). For the largest firm in the sample, the effect of *NetRegQA* on sales growth is roughly a positive 1.4%, in line with the *NetRegP* finding. This confirms the common view in the regulation literature that large firms enjoy an economy of scale in dealing with regulation.

Columns (7) and (8) of Table 8 measure *NetReg* in a way that adjusts for any cross-industry differences at the two-digit SIC level, while also allowing its effect to vary by firm size. We adjust for cross-industry differences in *NetReg* by constructing two new variables, *Ind. Adj. NetRegP* and *Ind. Adj. NetRegQA*, which are the firm-level *NetReg* measures minus the 2-digit SIC industry average *NetReg* of the respective portions of earnings calls on that reporting date and over the prior 90 days (*Ind. NetRegP* and *Ind. NetRegQA*, see Table 5). The coefficients on *Ind Adj. NetReg* remain negative and highly statistically significant, and their magnitudes are similar to the unadjusted *NetReg* measures. For an average size firm, after taking out the industry-specific mean of regulation, the implied reductions in sales from standard deviation increases in *Ind. Adj. NetRegP* and *Ind. Adj. NetRegQA* are 1.4%, i.e., -30.163 (coefficient from column 7 of Table 8) × 0.048 (standard deviation of *Ind. Adj. NetRegP*), and 1.1%, i.e., 22.368 × 0.050, respectively. The industry average effects (*Ind. NetRegP* and *Ind. NetRegQA*) are also very large and negative. A standard deviation increase in *Ind. NetRegP*, reduces sales growth for the firms in the industry, on average, by 1.2%, i.e., -62.073 (coefficient in column 7) × 0.019 (standard deviation of *Ind. NetRegP*); a one standard deviation increase in *Ind. NetRegQA* reduces sales growth by 0.7%, i.e., 33.897 (coefficient in column 8) × 0.021 (standard deviation of *Ind. NetRegQA*). This industry effect is in addition to any effects of firm-specific deviations from the industry mean, which are captured by *Ind. Adj. NetRegP* and *Ind. Adj. NetRegQA*.

In columns (9) and (10) of Table 8 we also include the lagged value of the *NoRegulat* variable, which controls for the absence of *regulat* in the earnings call from a year before, as well as the one-year lagged value of *NetReg*. We control for *NetReg* from a year ago to see the extent to which the effect of *NetReg* in the present quarter on future sales growth is a manifestation of regulation already found in the past. We find that values of *NetReg*, beyond the most recent ones, have little effect on next year's sales growth.

In Table 9, we summarize (in Panels A through D) normalized results of the effects of a one-standard deviation change in *NetRegP* and *NetRegQA* on year-ahead changes in each of our seven firm performance measures (sales growth, asset growth, leverage, operation margin change, and gross margin change). As a benchmark for comparison, in Table 9 we also include a general sentiment measure (*AllSent*) for the Presentation and Q&A sections of the Earnings Calls, and show the impact of a one standard deviation change in this measure on the dependent variables. For our seven performance measures, Panel A reports results for the whole sample with industry fixed effects. Panel B checks for the robustness of the results of Panel A using quarter and firm fixed effects. without differentiating by firm size. Panels C and D consider our results from Panel A from the standpoint of small and large firms.[12]

Table 9 shows that *NetRegP* discussions have economically and statistically significant negative effects on three (with firm fixed effects) or four (without firm fixed effects) of the five dependent variables (and the impact on leverage becomes larger as more time passes, as we explain below). *NetRegQA* (without firm fixed effects) displays similar effects on sales growth, asset growth and leverage, but not on the margin change measures, but these results are absorbed by firm fixed effects. The magnitude of the effects of *NetRegP* on sales growth and asset growth are relatively larger than the effects on margins in the absence of firm fixed effects, and when

[12] If the coefficient on *NetReg* is *b* and on the size interaction $NetReg \times Size$ is *c*, Panels B and C report $b\sigma_{NR} + c\sigma_{NR}\left(Size_p - \overline{Size}\right)$, where $Size_p$ is the p[th] percentile of the size distribution in the full sample and $\overline{Size}$ is the mean size in the full sample, and where σ_{NR} is the standard deviation of *NetReg*. The standard error of this size-adjusted coefficient is $\sigma_{NR}\left(se\left(\hat{b}\right)^2 + \left(Size_p - \overline{Size}\right)^2 se(\hat{c})^2 + 2\left(Size_p - \overline{Size}\right)cov\left(\hat{b},\hat{c}\right)\right)^{1/2}$ where $se(\cdot)$is the standard error of coefficient estimates and $cov\left(\hat{b},\hat{c}\right)$ is the covariance of the *b* and *c* estimates.

firm fixed effects are included, the effects on margins also lose statistical significance. All effects are larger for small firms than for large firms.

B. Robustness checks

In results reported in Online Appendix Tables A4 and A5, we experimented with alternative functional forms to the simple linear treatment of *NetReg* in the results reported thus far. The Table A4 alternative specification adds the square of *NetReg* to all the specifications. In some cases, the squared term was significant and positive (indicating a diminution of the linear effect), and in others it was significant and negative (indicating an increase in the linear effect). But the coefficients on *NetReg* are similar to the linear specification and the overall results are qualitatively the same to those reported above.

The Table A5 alternative specification divides *NetReg* into its positive ($IncReg = \max(0, NetReg)$) and negative ($DecReg = -\min(0, NetReg)$) component parts. The null hypothesis that the effects of *IncReg* and *DecReg* are the same but of opposite sign is not rejected in most cases. The coefficient values for *IncReg* are typically negative and those for *DecReg* are typically positive. The coefficients for the margin variables tended to be larger for *IncReg*, while those for *DecReg* were greater for sales and asset growth.

It is conceivable that firms might discuss regulation a year ahead of anticipated bad performance to shift blame for reduced growth or profitability away from themselves. That possibility would work against a causal interpretation of a positive coefficient on *NetReg*. However, this “cheap talk” interpretation of our evidence implies that when one divides *NetReg* into its positive (more regulation) and negative (deregulation) components, the positive components (i.e., attempts to shift blame) would drive the results as managers have no incentive to falsely give deregulation credit for their own successes. However, our finding that *DecReg* has a larger impact for sales and asset growth argues against the "cheap talk" interpretation.

The two rightmost columns in Table 9 repeat the analysis after replacing our *NetReg* measures with their $NetReg^{LLM}$ counterparts which use our measures only if the directionality of the effect is confirmed by ChatGPT. In the vast majority of cases, the forecasting power of the ChatGPT-augmented versions of our *NetReg* measures is higher than the forecasting power of our base measures. This suggests that our results represent a lower bound of the impact of regulations on firm outcomes. More nuanced measures will likely find even higher impacts. We conjecture that the main reason for the relative weakness of *NetRegQA* effects in Table 9 is that firms choose to discuss important and novel regulatory developments in the Presentation section of their earnings calls, which may reduce the frequency and importance of similar discussions in the QA section. We report results without firm fixed effects because one might be concerned that firm fixed effects can overlook cross-sectional differences, especially for firms with infrequent but important regulatory events. In any case, results with and without firm fixed effects are similar, suggesting within-firm *NetReg* variation is an important driver of our results.[13]

In Online Appendix Tables A6 and A7 we examine the impact of other studies' measures of regulation on our outcome variables. We come to three conclusions. First, in standalone regressions which contain only the external measures, but not our own *NetReg* measures, Table

[13] Firm fixed effects may distort the impact of firms with rare regulatory events like American Axle and AutoNation. These firms discuss regulations very infrequently, but when they do, it is because the impacts from the regulatory events are highly material. We also note that the variance decomposition of NetReg in Table 7 shows that firm and time fixed effects explain very little of NetReg variation. Furthermore, as we discuss below, NetReg is not predictable using a large set of firm operating characteristics; to the extent that there are time effects in NetReg they likely reflect exogenous regulatory influences rather than endogenous regulatory responses related to firms' fundamentals, suggesting that time fixed effects could over-control for important exogenous changes (such as elections).

A6 shows the external regulatory measures typically have less forecasting power for our outcome variables relative to our own measures. Second, our *NetReg* measures do far better in the firm and quarter fixed effect regressions than the external regulatory variables, suggesting that firm fixed effects are a very high hurdle for *any* firm-level measure of regulatory burden, and our measures capture more within-firm variation than do external measures.

Third, when including these various external regulatory measures in our forecasting regressions with *NetReg*, the forecasting coefficients on our *NetReg* measures remain largely unchanged. Consistent with our findings in Table 2, which shows the external regulatory measures are not correlated with our measures, Table A7 shows that our regulatory measures contain information that is not spanned by other measures proposed in the literature.

Finally, in Table A6, we also investigate whether the questions part of the Q&A section exhibits different behavior from the entirety of the Q&A section. We find that *NetReqQ* – a measure constructed using equation (1) applied to only the questions part of the Q&A section – behaves similarly to *NetRegQA* but with less forecasting power. We conclude that the answers part of the Q&A section contains important regulatory information.

C. Validation of NetReg by Trump's Election

We investigate how the surprise election of Donald Trump as President in 2016 affected regulatory outcomes for firms. We begin by recognizing that many outcomes affecting sales growth, including our measure, *NetReg*, itself may have changed as a result of the election. Our approach to investigating how Trump's election mattered for regulatory costs begins by measuring all firms' *NetRegP* and *NetRegQA* in the quarter immediately prior to the election (when Trump's election was not forecastable, and was viewed by most as less likely than not). We investigate whether Trump's inauguration as President was associated with a diminution of the effect on sales growth that would have happened in the year after Trump took office (a period captured by the indicator variable, *Inauguration*) if he had not been elected. We investigate this, both for the average of sales growth across all firms, and differentially across firms.

The time period of our analysis runs from January 1, 2015 to January 31, 2018. Quarters with sales growth starting from April 20, 2016 to October 20, 2016 are omitted. Quarters prior to this are classified as pre-election quarters, and quarters after this period are classified as post-election. Since Inauguration Day was January 21, 2017, this means pre-election quarters had at least nine months of their twelve-month ahead sales growth happen before Trump was inaugurated, whereas post-election quarters had at least nine months of the twelve-month ahead sales growth taking place after Trump's inauguration. In the pre-election period, we use the usual *NetRegP* and *NetRegQA* measures as in all other specifications. In the post-election period, we use the *NetReg* measures from the quarter immediately prior to the election.

To capture firms whose prospects were particularly sensitive to regulatory concerns related to Trump's election we construct the variable *Trump*, which measures the stock return of the firm in the three-day window around the November 8, 2016 election. In Table A9 of the Online Appendix, we observe that the firms that benefited most from Trump's election were firms involved in natural resources extraction, for-profit education, and various heavy industries. By comparison, the industries that reacted the least positively and, in a few cases, negatively reflected a more eclectic group of businesses that were not the focus of Trump's policies.

Specifically, we distinguish five effects related to Trump's inauguration in the regression analysis in Table 10. The first two capture aspects unrelated to regulation per se. In the first row of the table, the coefficient on *Trump* by itself measures how firms with different values of *Trump* fared differently in their sales growth in the pre-election period. Second, the coefficient on *Inauguration* by itself measures whether all firms (irrespective of their regulatory treatment or *Trump* values) experienced an average increase in sales growth after Trump's election. For example, if Trump's tax or foreign policies were relatively favorable for business growth, those would be reflected in this coefficient.

The next three effects capture cross-sectional characteristics related to regulation and their effects in the post-inauguration period. The first of these, the interaction of *Trump* and the two pre-election *NetReg* variables, measures whether the coefficient on *NetReg* for firms with high *Trump* values were different from other firms in the pre-election period. A negative coefficient indicates that firms whose stocks benefited more from Trump's election were also those with greater negative effects in the pre-election period for any given *NetReg* score. Second, the interaction of *Inauguration* with either *NetReg* variable would be positive if Trump's post-inauguration period saw an increase in sales growth conditional on the pre-election *NetReg* values, indicating that the negative effect of *NetReg* on sales growth from the pre-election period was diminished by Trump's presidency. Finally, the triple interaction of the two *NetReg* variables with *Inauguration* and *Trump* should be positive if firms that were the greatest stock market beneficiaries from Trump's election experienced a bigger post-election reversal in the negative effects associated with their *NetReg* values in the pre-election period.

The first two columns in Table 10 report results for a simplified version of the model that does not take into account the interactions with the *Trump* variable; the last two columns include those interactions. The results reported in the first two columns show that the coefficient on *Inauguration* is positive and significant, indicating that Trump's first year in office was associated with higher sales growth for firms on average. The coefficients on the interactions of *Inauguration* with both of the *NetReg* variables are positive and significant, indicating that firms with high pre-election *NetReg* values experienced higher sales growth than other firms, which we interpret as evidence that Trump's election mitigated the costs of exposure to high regulatory costs prior to the election.

We take cross-sectional variation in *Trump* into account in the last two columns of Table 10. The interaction of *Trump* and both *NetReg* variables is negative and significant, indicating that firms with higher *Trump* values faced greater regulatory costs on sales growth in the pre-election period for a given value of the *NetReg* variables. The coefficient on the triple interaction is positive and significant for *NetRegP* but is statistically insignificant for *NetRegQA,* which mirrors the weaker statistical significance of results related to *NetRegQA* compared to *NetRegP* in Table 9.

One interpretation of the weaker response of the triple interaction related to the Q&A section is that discussions in the Q&A section may be a noisier measure of regulation, especially in quarters will large regulatory events – like the Trump election – which management teams discuss in the Presentation section of earnings calls. For example, if regulatory issues are discussed adequately in the Presentation section, investors may not raise them in the Q&A section. To test this, we analyze the relationships between pre-election and post-inauguration values of *NetRegP* and *NetRegQA*, which we report in Online Appendix Table A10. Consistent with the view that the Q&A section measure is noisier, we find that serial correlation across the two periods of *NetRegP* is greater than serial correlation of *NetRegQA*. We also find that for firms with high *Trump* values and positive *NetRegP* values prior to the election, there is a tendency for *NetRegP* to fall after the inauguration. But consistent with the view that Q&A measures of *NetReg* are noisier, there is no similar evidence of a reversal for firms with high *Trump* values and positive pre-election *NetRegQA*.

D. Return regressions

In Table 11, we report results of *NetReg* measures for stock returns. Here, as is standard practice, we control for the log of market equity (in millions), the log of the book-to-market (BM) ratio (the log of book equity over market equity), and standardized unexpected earnings (SUE) defined similarly to Bernard and Thomas (1989). These variables are measured on the end date of the quarter corresponding to the earnings call. As in Tetlock, Saar-Tsechansky and Macskassy (2008), we control for abnormal excess returns on the day of the call and over the 21 trading days before the call. We also control for alpha, and log of share turnover defined as daily shares traded divided by shares outstanding on the day of the earnings call.[14]

We find that *NetRegQA* (but not *NetRegP*) has positive, large, and statistically significant effects on future excess and risk-adjusted abnormal returns in the 22-day period following earnings calls (the methodology to calculate returns is detailed in Section II). We find negative, albeit statistically insignificant, impacts of *NetReg* on contemporaneous call-day excess and abnormal returns.[15] Though we do not have power to reject the null for contemporaneous returns, the same-day negative returns and future positive returns due to *NetReg* are consistent with the risk explanation.[16] Higher regulatory exposure makes investors demand higher risk compensation which is accomplished via lower stock prices on the day of the call, and higher future returns. We interpret the difference between *NetRegQA* and *NetRegP* effects on returns as

[14] The specification for the returns of firm i on earnings call day t is $RX_{i,t+\delta\rightarrow t+\delta+h} = a + b \times CT_{i,t} + c^{\top}X_{i,t} + \epsilon_{i,t+\delta\rightarrow t+\delta+h}$, where h is either 1, 5, or 22 trading days, δ is either 0 or 1 depending on whether the call was pre- or post-4 PM, $RX_{i,t+\delta\rightarrow t+\delta+h}$ is either the excess or risk-adjusted return for firm i over the ensuing h trading days after the call, $CT_{i,t}$ is the conference call tone variable of interest, and $X_{i,t}$ is a vector of controls. In the contemporaneous version of the regression, we drop log share turnover as a control variable, because of endogeneity concerns, as well as the day-of-call return itself (obviously). We report standard errors clustering by conference call dates and by 2-digit SIC codes.

[15] We also tried running one-day and five-day ahead regressions. The results are consistent with the 22-day results but are weaker. These results are available from the authors. These results are available upon request.

[16] The evidence from returns that risk exposure increases when there is more discussion of regulation is confirmed by the leverage results.

indicating that questions raised by analysts about regulation (while perhaps noisier in general as a measure of regulation, as discussed at the end of the prior section) may be more relevant for measuring the importance of the discussion as news to the market about risk.[17]

E. Persistence of effects beyond one year ahead

Our empirical findings thus far have focused on one-year ahead forecasts, but the magnitudes of the effects we measure could be misleading if these variables adjust with protracted lags to changes in regulation news. We use the local projections method of Jorda (2005) to calculate cumulative impulse responses to a one standard deviation shock in *NetRegP* and *NetRegQA*.[18]

In Figure 3, we report impulse responses for sales growth and leverage. After two years the effect of a *NetRegP* shock on an average-sized firm's sales growth flattens out. The cumulative four-year effect on sales growth is slightly larger than the one-year effect. For *NetReqQA*, almost the entire effect for an average-sized firm happens in year one, as the four-year cumulative effect is very similar to the one-year effect. We also calculate the impulse responses for a larger firm (75th percentile by log sales) and find in both the Presentation and Q&A cases it is smaller than the effect for an average-sized firm, as was to be expected given the results in Table 8. We conclude that *NetRegQA* has a one-time, persistent effect on the level of sales, while *NetRegP* has a continuing effect over the next year or two. There is no evidence of reversals in either case.

[17] It is also interesting to compare the results for *NetReg* against the benchmark of *AllSent* measures in Table 11. Not surprisingly, sentiment of earnings calls often has positive predictive relevance for stock returns and operating performance measures, and magnitudes are similar to those from regulation measures. But sentiment is a significant predictor of firm performance less frequently than is *NetReg*.

[18] This method – which involves running our specification in (2) for two-, three-, and four-year ahead outcomes – is robust to data generating process misspecification and accommodates potential nonlinearities, as opposed to a traditional vector autoregression approach. The impulse response is the sum of the *NetReg* coefficients across the time horizons.

Figure 3 shows a very different picture for the impulse response for leverage. As in the case of sales growth, there is no reversal in the impulse response. However, in the case of leverage, by the fourth year, the cumulative response is an order of magnitude larger than the one-year response, especially for *NetRegP*, and after four years the decline in leverage does not appear to be flattening. The effect is most pronounced for average-sized firms, but is also present for large (75th percentile) firms. This suggests that leverage adjusts much more slowly than sales growth to regulatory shocks and the long-run effect on leverage is substantially understated by the coefficient reported in Table 9. Impulse responses for other variables are reported in the Online Appendix.

The combination of our empirical findings (that regulatory news predicts significant reductions in growth and leverage, an increase in expected future returns, and smaller negative change in operating margins) contains elements that confirm both Hypothesis 1 and Hypothesis 2 about the channels through which regulation affects firms (reflecting a combination of compliance risk and physical operational costs). Nevertheless, we believe the findings suggest the relative importance of Hypothesis 1 (consequences of increased regulatory compliance risk), given that the magnitudes of the long-run effects on growth, leverage and returns are large compared to the coefficients on the margin-related variables, as shown in Table 9 and by discussion of long-term impacts on leverage in Figure 3.

F. Is regulatory discussion forecastable by other variables?

Next, we examine the question of whether *NetReg* itself is forecasted by firms' past operating performance, as measured by sales, asset growth, margins, and stock returns. A potential concern about our interpretation of our regulation measures as indicators of news is that discussions of regulation may reflect "cheap talk" by firms attempting to blame previous poor performance on regulation when in fact, poor performance reflects other influences. If that were true, we would expect problems in sales growth or profitability to predict mentions of regulation. If the *NetReg* measures capture news they should not be forecastable using prior firm performance.

In the specification that includes all lagged firm performance measures (sales, asset, and operating margin growth), none of them are statistically significant forecasters of *NetReg*, either for the Presentation or the Q&A sections of Earnings Calls. The adjusted R-squareds are around nine percent for *NetRegP* and three percent for *NetRegQA* in all specifications.[19] The regulatory discussion from the Presentation section is more forecastable than the unscripted regulatory discussion from the Q&A section. *NetRegP* and *NetRegQA* are predicted positively by their own lagged values (with *NetRegQA* much less so), and *NetRegP* is predicted negatively by firm size. The lagged one-month risk-adjusted return has no forecasting power for *NetReg*. This is an important finding because it addresses the concern that discussions about regulation may reflect managerial cheap talk (e.g., poor stock market performance prompting managers or shareholders to talk more about regulation).

In summary, *NetRegQA* and *NetRegP* are mainly forecastable by their own past and *NetRegP* is related to firm size. Adjusted R-squareds are small. Other variables related to firm income measures or past stock returns have little forecasting power for *NetRegP* or *NetRegQA*. In contrast, *AllSent* is highly predictable by lagged firm performance.[20] The adjusted R-squareds are much higher and lagged operating performance and stock returns have significant forecasting power for future *AllSent*. Therefore, *AllSent*'s limited ability for forecasting future firm outcomes, as documented in Table 9, cannot necessarily be interpreted as the impact of new

[19] Results are available upon request.
[20] Results are available upon request.

information about the firm, because a lot of this information was knowable based on lagged firm operating characteristics. This highlights the specialness of the regulatory information conveyed by *NetReg*, which is effectively unpredictable by past firm performance.

G. Breaking down NetReg or capturing the importance of different topical contexts

Does the impact of *NetReg* depend on identifiable differences in the kinds of news captured by the measure? Might one be able to distinguish within *NetReg* observations between the effects of concerns about new regulations vs. concerns about changes in enforcement of existing regulation? Are there identifiable differences in the responses to risks of future regulation as opposed to current operational costs? Is it possible to differentiate between discussions of preexisting regulation and new regulation (that is, regulatory stock vs. flow)?

We explored those possibilities by enlisting the help of ChatGPT. We developed prompts for ChatGPT that asked it to characterize *NetReg* into categories based on these two sets of distinctions (risk vs. current cost, and stock vs. flow). The ChatGPT queries associated with these analyses are shown in the Online Appendix. In each case we modified our core regression in (2) by interacting NetReg (i.e., *CT* in the regression) with an indicator that signals whether any regulatory sentences in the call contained discussion of risk or cost in one specification, and stock or flow in another. We did not find consistent results implying that our *NetReg* results are driven by one category rather than the other, and that was true for risks versus current operational costs, and existing stock versus flow of regulations.

We report one additional experiment for illustrative purposes. Given the lack of salient results using the risk-cost and stock-flow prompts, we constructed a considerably more sophisticated prompt to classify regulatory sentences into those about enforcement, rulemaking, or neither. This prompt, shown in the Online Appendix, allows us to classify whether a call discusses specific rule-related news, specific enforcement news, or neither. In Table A8, we show the results of these interaction regressions for all our dependent variables. For sales growth, the positive significant coefficient on the interaction of the enforcement dummy with NetRegP seems to suggest that rule making is more important than enforcement in driving our NetRegP results, but the coefficient on the interaction of rulemaking and *NetReg* is zero, not positive significant, so that interpretation is not correct.

That seemingly inconsistent result in Table A8 mainly reflects the fact that ChatGPT was unable to categorize most of our observations into one or the other category. In other words, our reported effects for *NetReg* above reflect observations that are not coded either as new rules, or new enforcement issues, or both; most of the observations (120,000 out of our nearly 200,000 observations of *NetReg*) are simply not coded by AI as falling either of the two categories. Furthermore, when we tried to validate those AI rankings by reading a random sample of the observations, the rankings did not seem to plausibly capture any identifiable differences that we could see. We tried many alternative prompts without any change in the outcome. We conclude that *NetReg* captures news about regulation in vague and broad ways, and attempts to parse the data in order to distinguish responses to regulatory stock from flow, regulatory risk or current costs, or regulatory rule making or enforcement, is presently difficult with our data but can be interesting to investigate in future research.

We also considered sorting *NetReg* observations according to topical differences. Using the LDA method for identifying topics related to regulation, we explored in Calomiris et al. (2024) whether *NetReg*'s effects on dependent variables are different across topical categories, which we explain in the Topic Model Appendix. For both the Presentation and Q&A sections, we find that the most significant topic areas with negative regulatory influence are M&A and

FDA. This makes sense given the potential importance of regulatory approval for mergers or new products. Financial regulation (Fins) shows a negative effect on leverage in the Q&A section. Since we restrict our sample to non-financial firms this finding is not mechanical. We interpret the effect of Fins on leverage of non-financial firms as implying a negative effect on borrowing firms from the regulation of financial firms (perhaps reflecting reduced credit supply effects from financial regulation). Interestingly, we also observe a topic with a positive coefficient: utility regulation for asset growth and leverage. This positive effect is driven by observations for non-utilities, perhaps suggesting a positive effect of more utility regulation on utility customers.

Overall, these results confirm the view of regulation we presented in the introduction. Regulatory news can mean very different things (e.g., new prudential regulations on financial intermediaries, as opposed to drug or merger approvals, or utility price limits), and each of these can have different implications for different firms (e.g., consumers or producers of energy). The analysis of topical context reinforces our view that a text-based measure of regulation, like *NetReg*, can capture a wide variety of influences.[21]

V. CONCLUSION

We study a new way to measure regulation and its effects on firm growth, profitability, leverage, and stock returns. Our measure of regulation, *NetReg*, identifies mentions of the string *regulat* accompanied by words indicating increasing or decreasing regulation in corporate earnings calls. We believe corporate earnings calls are an ideal setting in which to measure the impact of regulations on firms, and that this setting is equally relevant for all industries

[21] A similar point about the importance of topical context in earnings calls is made by Meursault et al. (2021).

regardless of their specific regulatory bodies. The majority of variation in our regulatory measure is due to idiosyncratic within-firm changes in regulatory exposure, suggesting that our measure captures information that industry- and economy-wide measures cannot reflect.

Higher *NetReg* has substantial negative effects on future sales growth, asset growth, and leverage from increased regulatory burden expressed in the Presentation section of earnings calls. The discussion of regulatory exposure in the Presentation section of earnings calls also has a negative (but lesser) effect on profit margins, and only in the absence of firm and time fixed effects. Excess stock returns are substantially higher after earnings calls exhibiting higher *NetRegQA*. This suggests that regulatory exposures identified in earnings calls reflect, at least in part, regulatory risks..

Effects of regulation are smaller for large firms, indicating substantial economies of scale in managing exposure to regulation. The decreased impact of our regulatory measure on future corporate outcomes after the Trump inauguration for firms with high pre-election *NetReg* and strong positive stock reactions to Trump's election demonstrate that our measure captures, at least in part, exogenous variation in firm-level regulatory exposure.

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Regulatory measures differ dramatically

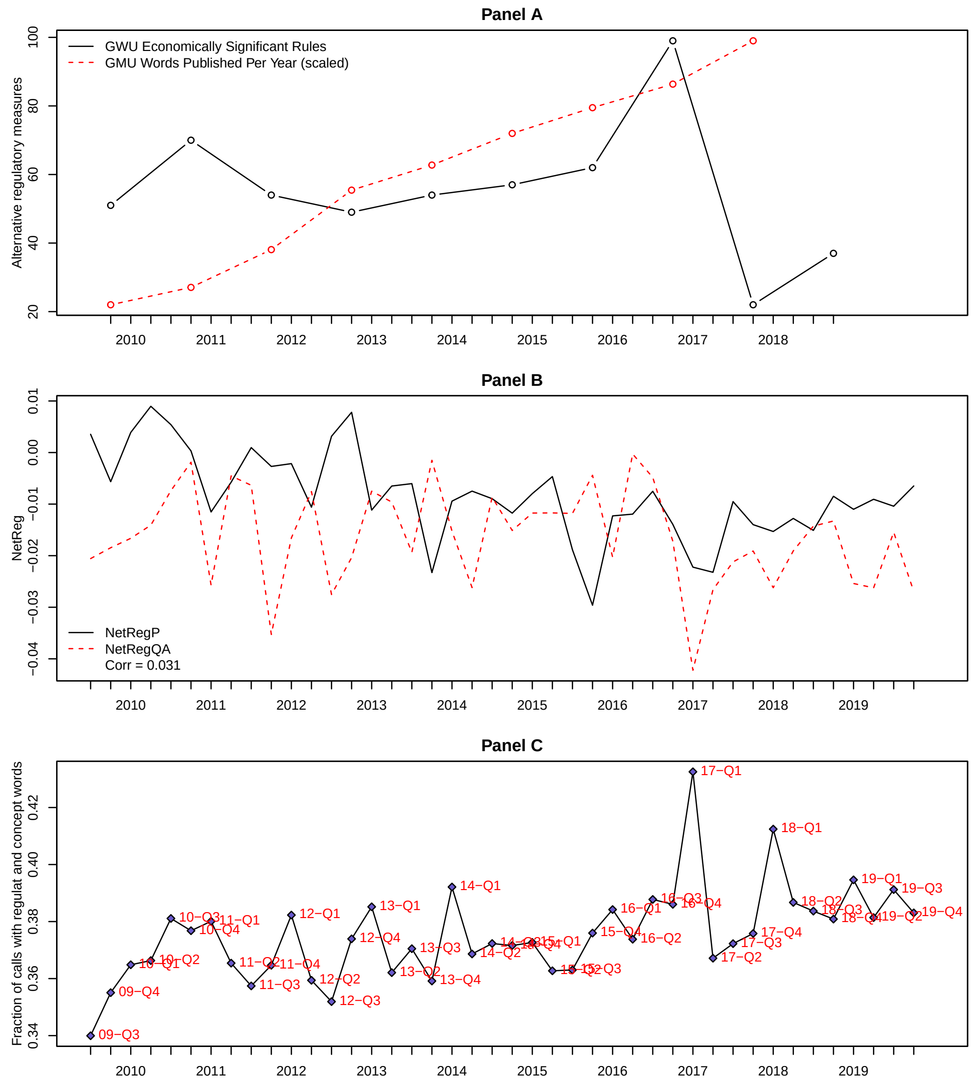

Figure 1: Panel A contrasts the George Mason University's (GMU) approach with the George Washington University's (GWU) approach. The GMU presented by plotting the average, for each year, of the word counts of regulations published across 3-digit NAICS industries. The annual 3-digit NAICS industry-level regulation data comes from Al-Ubaydli and McLaughlin (2017) and are based on the Code of Federal Regulations. GWU refers to the number of economically significant regulatory rules tracked by George Washington University following Executive Order 12866, identifies important regulations with an annual effect on the economy of $100 million or more. Panel B plots our measure of regulation NetReg separately for Presentation and Q&A sections of the quarterly earnings call. Panel C presents the percentage of all earnings call in the S&P Global data set that contain at least one sentence in either the Presentation or Q&A portion of the call that satisfies our regulatory filter.

Alternative measures of conference call sentiment

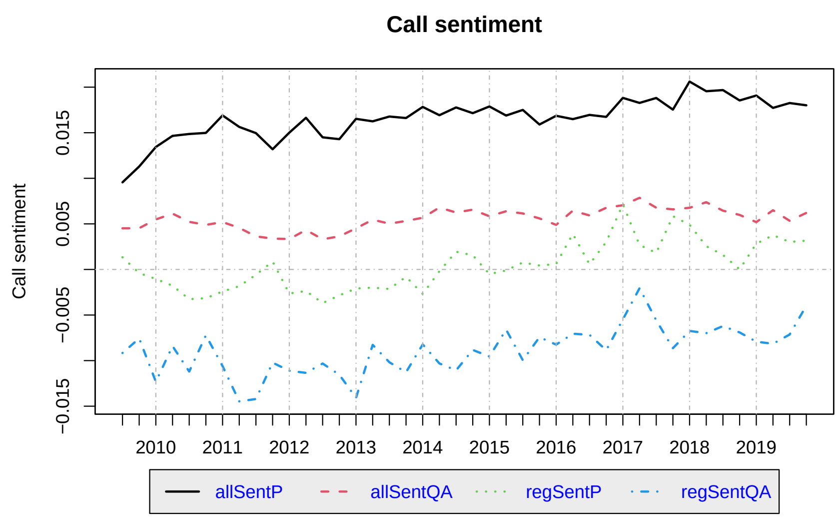

Figure 2: Sentiment series using Loughran-McDonald (LM) dictionary in sentences matching our regulatory filter in the Presentation ($RegSentP_t$) and Q&A ($RegSentQA_t$) portions of earnings calls. Also shown are LM sentiment of the Presentation ($AllSentP_t$) and Q&A ($AllSentQA_t$) portions of the earnings call. The underscore t indicates each series is an equally-weighted average of individual call measures within each quarter. Data are quarterly.

Impulse response of sales growth and leverage to a $NetReg$ shock

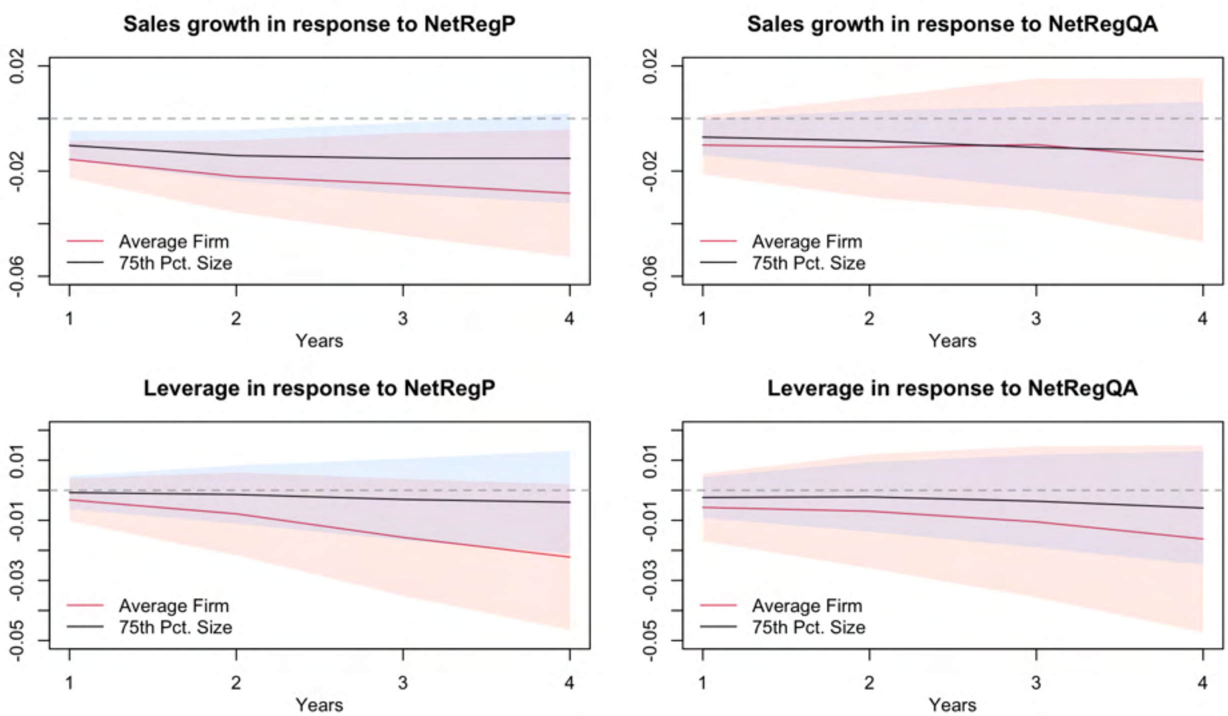

Figure 3: The response of sales growth and leverage to a one-standard deviation shock to *NetRegP* (presentation) and *NetRegQA* (Q&A). We use the local projection method of Jorda (2005) to calculate the cumulative impulse response, as the sum of all prior prior and current single period responses to a one standard deviation shock of the respective $NetReg$ measure. Shown are the cumulative response for an average-sized firm, as well as for a firm in the 75^{th} percentile. The impulse response assumes that the *NetReg* shock is orthogonal to all other influences. Standard errors assume independence of successive shocks. The bands in the figure show 95% confidence intervals.

Table 1: This table outlines the hypotheses motivating our empirical analysis.

Channels of Regulatory Impact

Hypothesis	**Explanation**
H1: If regulatory discussions imply increased regulatory risk, future expected stock returns should be higher, future leverage should be lower, and firm growth should be abated.	Increased returns compensate for higher risk. If the firm was already at an optimal level of leverage (e.g., based on a tradeoff between expected costs of distress and tax gains from debt service), then higher risk should cause reduced leverage. Higher risk makes incremental growth more costly, thereby producing a reduction in growth.
H2: If regulatory discussions imply increased physical compliance costs, future profit margins and growth both should be reduced.	Higher physical costs reduce the ratio of profits to sales. Higher operating costs make growth less profitable and thereby produce a reduction in growth.

Table 2: We calculate firm-quarter correlations between the external measures and ours. In all cases, we stack all overlapping observations and calculate a pooled correlation.

Low Correlations with Other Measures

Paper	**Our Measure**	**Correlation**
Hassan, Hollander, van Lent, and Tahoun (2019) *PRisk*	*NetRegP*	0.004
	NetRegQA	-0.008
Hassan, Hollander, van Lent, and Tahoun (2019) *PSentiment*	*RegSentP*	0.055
	RegSentQA	0.032
Armstrong, Glaeser, and Hoopes (2023) *Total Government Agency Exposure*	*NetRegP*	-0.081
	NetRegQA	-0.092
Chang, Kalmenovitz, and Lopez-Lira (2023) *RegPipline*	*NetRegP*	0.011
	NetRegQA	0.003
Kalmenovitz (2023) *Intensity (Regulations)*	*NetRegP*	0.034
	NetRegQA	0.022
Kalmenovitz (2023) *Intensity (Response)*	*NetRegP*	0.001
	NetRegQA	-0.002
Kalmenovitz (2023) *Intensity (Time)*	*NetRegP*	0.014
	NetRegQA	0.003
Kalmenovitz (2023) *Intensity (Dollars)*	*NetRegP*	0.031
	NetRegQA	0.025
Kalmenovitz, Lowry, and Volkova (2022) *Fragmentation*	*NetRegP*	0.030
	NetRegQA	0.011

Table 3: The table shows stemmed modifying (increasing or decreasing) words, along with the number of times each word occurs in the Presentation and Q&A sections.

Regulatory directionality word lists

Category
Increasing
increas 16442, growth 11183, addit 8982, uncertainti 5419, higher 5273, high 4666, grow 3422, pressur 2944, concern 2803, negat 1908, difficult 1454, add 1319, ad 1277, restrict 1117, hard 1028, strengthen 880, hurdl 830, strength 793, adapt 784, stringent 705, burden 701, stress 669, rise 599, incur 588, uncertain 519, aggress 517, strict 475, heavili 398, complic 332, heavi 321, bad 292, caution 219, adher 218, penalti 217, poor 112, violat 93, penal 78, fear 77, wors 76, prolifer 69, disproportion 39, litigi 9
Decreasing
approv 22832, posit 8503, improv 5130, clear 4725, good 4370, progress 4317, benefit 4307, lower 4155, reduc 3197, construct 2612, better 2376, reduct 2155, declin 2120, low 2030, less 1965, decreas 1940, unregul 1700, favor 1511, deregul 1511, nonregul 1337, stabl 1271, clariti 1197, permit 1193, attract 900, stabil 835, flexibl 800, optim 779, fall 585, relief 489, optimist 431, happi 388, friend 194, overcom 185, permiss 148, fewer 147, fell 110, shrink 81, diminish 67, congratul 42, deregulatori 35, happili 6, congrat 6

Table 4: Sample sentences that satisfy our regulatory filter from the Presentation and Q&A portions of earnings calls. Each sentence is shown along with its Increasing, Decreasing and Concept words.

Sample sentences

	Sentences
1	Market's been deregulated. [dec: deregul 1] [inc:] [concept: deregul 1, market 1]
2	And we have less regulatory measures there and also more attractive margins, which is good. [dec: good 1, less 1, attract 1] [inc:] [concept: measur 1, regulatori 1]
3	The regulatory approval process is progressing very well. [dec: approv 1, progress 1] [inc:] [concept: regulatori 1, approv 1, progress 1]
4	We continue to work on regulatory approvals and permitting. [dec: approv 1, permit 1] [inc:] [concept: regulatori 1, approv 1]
5	As a result of deregulation of petrol and diesel, this is very attractive. [dec: attract 1, deregul 1] [inc:] [concept: deregul 1]
6	There are regulatory pressures as you grow and as an industry matures, that's absolutely normal and we have to adapt to it. [dec:] [inc: pressur 1, adapt 1, grow 1] [concept: regulatori 1, pressur 1]
7	Competition, pricing and regulatory pressure have increased and are increasingly having an impact on our revenue. [dec:] [inc: pressur 1, increas 2] [concept: impact 1, regulatori 1, pressur 1]
8	There could well be an increased regulatory burden. [dec:] [inc: increas 1, burden 1] [concept: regulatori 1, burden 1]
9	We did this to serve a highly stressed industry pressured by increased regulatory burdens, growing transactional volumes and emerging payment technologies. [dec:] [inc: stress 1, high 1, pressur 1, burden 1, increas 1, grow 1] [concept: regulatori 1, pressur 1, burden 1]
10	This continues to be of particular importance as the regulatory burden grows disproportionately. [dec:] [inc: disproportion 1, burden 1, grow 1] [concept: regulatori 1, burden 1]
11	_A_ We have all regulatory approvals for construction. [dec: approv 1, construct 1] [inc:] [concept: regulatori 1, approv 1]
12	_Q_ Congrats on the regulatory progress. [dec: progress 1, congrat 1] [inc:] [concept: regulatori 1, progress 1]
13	_A_ And those are very friendly, deregulated markets. [dec: deregul 1, friend 1] [inc:] [concept: market 1, deregul 1]
14	_A_ And again, it's just regulatory approvals. [dec: approv 1] [inc:] [concept: regulatori 1, approv 1]
15	_A_ And only about 1/3 of those were for regulatory approvals. [dec: approv 1] [inc:] [concept: regulatori 1, approv 1]
16	_A_ It's highly regulated, so the barriers to entry are high. [dec:] [inc: high 2] [concept: barrier 1]
17	_A_ And what are the regulatory hurdles? [dec:] [inc: hurdl 1] [concept: regulatori 1, hurdl 1]
18	_Q_ Is this because of regulatory pressure? [dec:] [inc: pressur 1] [concept: regulatori 1, pressur 1]
19	_A_ Now we're being faced with some of the additional regulatory pressures. [dec:] [inc: addit 1, pressur 1] [concept: regulatori 1, pressur 1]
20	_Q_ Is it regulatory hurdles? [dec:] [inc: hurdl 1] [concept: regulatori 1, hurdl 1]

Table 5: This table describes the data series involving firm fundamental characteristics, market returns, and the S&P Global earnings call data.

Variable Name	**Description**
Sales growth	Percentage growth in sales from quarter $t-4$ to quarter t, where t is the quarter of the earnings call; expressed in % points.
Asset growth	Percentage growth in total assets from quarter $t-4$ to quarter t, where t is the quarter of the earnings call; expressed in % points.
Operating margin	Operating income after depreciation divided by sales; all numbers are from the quarter associated with the earnings call; expressed in % points.
Operating margin Δ	Change in operating margin from quarter $t-4$ to quarter t, where t is the quarter of the earnings call; expressed in % points.).
Gross margin	Revenues minus cost of goods sold divided by sales; all numbers are from the quarter associated with the earnings call; expressed in % points.
Gross margin Δ	Change in gross margin from quarter $t-4$ to quarter t, where t is the quarter of the earnings call; expressed in % points.).
Leverage	Sum of current liabilities and long-term debt divided by total assets; all numbers are from quarter associated with the earnings call; expressed in % points.
Cost of goods sold	Cost of goods sold divided by sales; all numbers are from the quarter associated with the earnings call; expressed in % points.
SG&A	SG&A divided by sales; all numbers are from the quarter associated with the earnings call; expressed in % points.
Excess Ret	Stock return in excess of the risk-free rate; expressed in % points. Note: Returns are measured from the close of day t (i.e. the earnings reporting date) for calls occurring prior to 4PM New York time, and from the close of day t + 1 (the next business day) for calls occurring after 4PM New York time.
FF6 Ret	Excess stock return with respect to the Fama-French (2015) 5-factor model augmented with the momentum factor; expressed in % points. Note: Returns are measured from the close of day t (i.e. the earnings reporting date) for calls occurring prior to 4PM New York time, and from the close of day t + 1 (the next business day) for calls occurring after 4PM New York time.
FF6 Alpha	The alpha estimated from the FF6 model over the trading-day window [-252,-31]; expressed in % points.
Size	Log sales from the quarter associated with the earnings call
log(ME)	ME is the closing price times shares outstanding, measured as of the end date of the quarter associated with the earnings call
log(BM)	BM is book value of common equity divided by market equity, both measured as of the end date of the quarter associated with the earnings call
SUE	Standardized unexpected earnings (SUE) follow the construction in Bernard and Thomas (1989) and Tetlock, Saar-Tsechansky, Macskassy (2008). SUE is equal to unexpected earnings (UE) minus mean of UE across the previous 20 quarters divided the std. dev. of UE across the previous 20 quarters. UE is defined as earnings (i.e. income before extraordinary items) in quarter t, the quarter of the earnings call, minus earnings in quarter $t-4$. We set the mean of UE to zero if firms have fewer than 16 quarters of earnings data. For the std. dev., firms must have at least 5 quarters of earnings data; otherwise we treat the std. dev. as missing.
log(share turnover)	Share turnover is defined as shares traded divided by shares outstanding, on the day of the earnings call if the call is released prior to 4PM, and otherwise on the next trading day.
[Inc/Dec/Tot][P/QA]	Average number of [increasing/decreasing/total] words in regulatory sentences of the Pres or Q&A section
NetReg[P/QA]	Net difference of increasing words and decreasing words in regulatory sentences scaled by total words within that window for the Pres or Q&A section
RegSent[P/QA]	Net difference of positive tone words and negative words, based on Loughran and McDonald (2011), within regulatory sentences scaled by total words within that window for the Pres or Q&A section
AllSent[P/QA]	Net difference of positive tone words and negative words, based on Loughran and McDonald (2011), scaled by total words for the entire Pres or Q&A section
Ind. NetReg[P/QA]	2-digit SIC industry average of NetReg[P/QA] over the [t-90,t] window, where t is the date for which the reporting quarter ends)
Ind. RegSent[P/QA]	2-digit SIC yearly industry average of RegSent[P/QA] over the [t-90,t] window, where t is the date for which the reporting quarter ends
Ind. AllSent[P/QA]	2-digit SIC yearly industry average of AllSent[P/QA] over the [t-90,t] window, where t is the date for which the reporting quarter ends
Ind. Adj. NetReg[P/QA]	Firm-level NetReg[P/QA] minus Ind. NetReg[P/QA]
Ind. Adj. RegSent[P/QA]	Firm-level RegSent[P/QA] minus Ind. RegSent[P/QA]
Ind. Adj. AllSent[P/QA]	Firm-level AllSent[P/QA] minus Ind. AllSent[P/QA]
NoRegulat	Dummy variable (e.g., for quarter t-4) equal to 1 if the conference call (from 4 quarters ago) had no mention of "regulat" but if some other earning call for this firm has mentioned "regulat", and equals to 0 otherwise
NeverRegulat	Dummy variable set to one for firms that have never mentioned "regulat" in any of their conference calls

Table 6: Summary statistics for firm-level operating characteristics, returns, and text measures. The data start in January 2009 and go through December 2019.

Summary statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Sales Growth	69,500	10.859	29.338	−47.204	−2.506	17.747	142.495
Asset Growth	69,644	11.834	30.488	−32.244	−2.298	15.058	152.556
Operating Margin	69,525	6.595	20.321	−88.303	2.557	15.931	42.632
Operating Margin Δ	69,195	1.202	13.099	−36.079	−2.122	2.759	65.529
Size (Log Sales)	70,177	5.644	1.776	1.610	4.419	6.813	11.822
Leverage	67,113	25.217	21.008	0.000	6.358	38.065	83.246
Gross Margin	69,575	41.028	22.631	−11.769	24.265	57.206	88.473
Gross Margin Δ	69,359	0.764	7.620	−20.472	−1.567	2.030	39.106
Excess Ret (22-day)	67,917	1.528	12.975	−80.591	−4.899	6.990	395.474
FF6 Ret (22-day)	65,577	0.214	11.650	−75.771	−5.250	4.805	438.124
Excess Ret (Call Day)	65,541	0.159	7.796	−72.312	−3.295	3.575	342.934
FF6 Ret (Call Day)	65,541	0.109	7.671	−72.288	−3.147	3.389	343.045
Log Share Turnover	70,014	−4.168	1.093	−7.001	−4.828	−3.433	−1.928
SUE	33,077	−2.412	1.837	−7.161	−3.467	−1.164	1.622
Log Book-to-Market	68,278	−0.958	0.809	−3.138	−1.424	−0.397	0.748
Log Market Equity	71,996	7.265	1.839	0.317	6.014	8.445	13.886
IncP	18,912	1.001	1.796	0.000	0.000	1.000	50.000
DecP	18,912	1.089	1.936	0.000	0.000	1.000	37.000
TotP	18,912	44.148	48.723	2.000	16.000	52.000	891.000
NetRegP	18,912	−0.004	0.052	−0.333	−0.020	0.013	0.429
IncQA	13,565	0.446	0.890	0.000	0.000	1.000	14.000
DecQA	13,565	0.637	1.199	0.000	0.000	1.000	42.000
TotQA	13,565	31.335	32.559	1.000	12.000	39.000	979.000
NetRegQA	13,565	−0.007	0.054	−0.667	−0.018	0.000	0.500
RegSentP	18,912	0.002	0.063	−0.429	−0.026	0.034	0.375
RegSentQA	13,565	−0.006	0.062	−0.500	−0.027	0.000	0.500
AllSentP	24,922	0.018	0.013	−0.048	0.009	0.027	0.075
AllSentQA	24,794	0.010	0.012	−0.091	0.003	0.017	0.143
Ind. NetRegP	18,912	−0.004	0.019	−0.250	−0.012	0.006	0.214
Ind. RegSentP	18,912	0.002	0.023	−0.261	−0.005	0.014	0.188
Ind. AllSentP	24,922	0.018	0.004	−0.028	0.016	0.021	0.057
Ind. Adj. NetRegP	18,912	−0.0001	0.048	−0.336	−0.017	0.020	0.418
Ind. Adj. RegSentP	18,912	−0.0001	0.059	−0.372	−0.025	0.029	0.355
Ind. Adj. AllSentP	24,922	−0.0001	0.013	−0.065	−0.008	0.008	0.054
Ind. NetRegQA	13,565	−0.007	0.021	−0.200	−0.015	0.0003	0.231
Ind. RegSentQA	13,565	−0.006	0.024	−0.368	−0.013	0.005	0.222
Ind. AllSentQA	24,794	0.011	0.004	−0.014	0.008	0.014	0.049
Ind. Adj. NetRegQA	13,565	0.0003	0.050	−0.635	−0.015	0.017	0.470
Ind. Adj. RegSentQA	13,565	−0.0001	0.057	−0.408	−0.020	0.022	0.456
Ind. Adj. AllSentQA	24,794	−0.0005	0.011	−0.099	−0.007	0.006	0.131

Table 7: This table displays the (incremental) adjusted R^2 from a projection of our text-based measures on different fixed effects: the R^2 from a regression with only a time FE (quarter of call); the incremental contribution to R^2 of adding an industry FE (2-digit SIC) to the time FE regression; and the incremental contribution to R^2 of adding a firm FE (gvkey) to the time and industry FE regression.

Variance Decomposition of Text-based Measures

	NetRegP	NetRegQA	NetRegP^{LLM}	NetRegQA^{LLM}	RegSentP	RegSentQA	AllSentP	AllSentQA
Time FE	0.0006	0.0003	0.0004	0.0002	0.0005	0.0006	0.0058	0.0085
Industry FE	0.0146	0.0102	0.0102	0.0081	0.0120	0.0045	0.0558	0.0304
Firm FE	0.1062	0.0446	0.1171	0.0387	0.1544	0.0270	0.3279	0.2390

Table 8: This table shows the results of regressing four-quarter-ahead sales growth on our net regulatory trends, as well as other control variables. Control variables include company size (log sales), a dummy variable to indicate whether the respective section of a given call had a regulatory mention, a decomposition of net regulatory trends into a company-specific and industry-specific (2-digit SIC code) component, as well as lags and interactions of the above variables. Columns (3) and (4) replicate columns (1) and (2) but include firm and quarter fixed effects. Standard errors, clustered by 2-digit SIC and quarter, are reported in parentheses. Significance is indicated via: *p<0.1; **p<0.05; ***p<0.01.

Effects of $NetReg$ on sales growth

	Sales Growth$^i_{t+4}$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
NetRegP^i_t	-30.181^{***}		-19.647^{***}		-42.667^{***}				-28.964^{***}	
	(5.539)		(4.436)		(11.038)				(7.090)	
NetRegQA^i_t		-18.019^{***}		-7.499^{*}		-31.278^{***}				-17.320^{***}
		(3.754)		(4.268)		(8.665)				(5.235)
Ind. Adj. NetRegP^i_t							-30.163^{***}			
							(8.780)			
Ind. Adj. NetRegQA^i_t								-22.368^{***}		
								(4.037)		
NetRegP$^i_{t-4}$									-16.745	
									(12.592)	
NetRegQA$^i_{t-4}$										1.978
										(4.875)
Size^i_t	-3.470^{***}	-3.415^{***}	-31.147^{***}	-31.203^{***}	-3.451^{***}	-3.392^{***}	-3.469^{***}	-3.414^{***}	-2.944^{***}	-2.804^{***}
	(0.916)	(0.862)	(1.606)	(1.780)	(0.897)	(0.840)	(0.913)	(0.860)	(0.876)	(0.763)
Ind. NetRegP^i_t							-62.073^{**}			
							(24.196)			
Ind. NetRegQA^i_t								-33.897^{**}		
								(14.749)		
Sales Growth^i_t	0.067^{**}	0.070^{**}	-0.019	-0.021	0.067^{**}	0.070^{**}	0.067^{**}	0.070^{**}	0.079^{**}	0.088^{***}
	(0.031)	(0.031)	(0.015)	(0.016)	(0.031)	(0.031)	(0.031)	(0.031)	(0.034)	(0.030)
No Regulat Dummy^i_t	-1.274	-2.623^{*}	-1.193^{***}	-1.615^{***}	-1.318	-2.618^{*}	-1.225	-2.534^{*}	-1.804^{**}	-2.825^{***}
	(1.218)	(1.447)	(0.392)	(0.380)	(1.218)	(1.436)	(1.158)	(1.403)	(0.773)	(1.002)
No Regulat Dummy$^i_{t-4}$									0.708	-0.033
									(1.085)	(1.201)
Never Regulat Dummy^i_t	-4.875^{**}	-6.223^{***}			-4.898^{**}	-6.194^{***}	-4.843^{**}	-6.139^{***}	-4.390^{*}	-6.066^{**}
	(2.089)	(2.271)			(2.073)	(2.239)	(2.034)	(2.226)	(2.343)	(2.623)
NetRegP^i_t*Size^i_t					9.019^{*}					
					(5.069)					
NetRegQA^i_t*Size^i_t						9.254				
						(5.704)				
Ind. Adj. NetRegP^i_t*Size^i_t							3.363			
							(3.282)			
Ind. Adj. NetRegQA^i_t*Size^i_t								5.085^{*}		
								(2.578)		
2-digit SIC Ind. FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm & Quarter FE?	No	No	Yes	Yes	No	No	No	No	No	No
Observations	54,042	49,718	54,042	49,718	54,042	49,718	54,042	49,718	38,488	34,562
R^2	0.079	0.076	0.435	0.435	0.079	0.076	0.079	0.076	0.073	0.066
Adjusted R^2	0.078	0.075	0.401	0.399	0.078	0.075	0.078	0.075	0.071	0.064

Note: *p<0.1; **p<0.05; ***p<0.01

Table 9: This table summarizes the impact of NetReg and AllSent on overall (Panel A), quarter and firm fixed effects (Panel B), small firm (i.e., 10th size percentile, Panel C), and large firm (i.e., 90th size percentile, Panel D) year-ahead sales growth, asset growth, leverage, change in operating margins, and change in gross margins. Standard errors in Panels A, C, and D are clustered by 2-digit SIC and quarter. Panel B restricts the sample to firms with at least 24 firm-quarter observations and clusters standard errors by firm and quarter. All outcomes are annualized and measured in percent. Coefficients denote the effect from a one standard deviation increase in the independent variable. Non-missing entries reflect effects that are significant at the 10% level or better. Significance is indicated via: *p<0.1; **p<0.05; ***p<0.01.

Impacts of NetReg and AllSent on Growth, Leverage, and Margins

Panel A: Overall	NetRegP	NetRegQA	AllSentP	AllSentQA	NetRegP^{LLM}	NetRegQA^{LLM}
Sales Growth	-1.561***	-0.971***			-1.602***	-0.985***
Asset Growth	-1.292***	-0.641**	1.449***	1.050***	-1.399***	-0.671**
Leverage		-0.132**			-0.179*	
Operating Margin Δ	-0.302*				-0.383**	
Gross Margin Δ	-0.268***				-0.305***	
Panel B: Quarter FE + Firm FE	NetRegP	NetRegQA	AllSentP	AllSentQA	NetRegP^{LLM}	NetRegQA^{LLM}
Sales Growth	-1.098***		0.536**	0.663***	-1.041***	
Asset Growth	-1.336***		1.733***	1.149***	-1.474***	-0.453*
Leverage	-0.138*				-0.165**	
Operating Margin Δ				0.137*		
Gross Margin Δ						
Panel C: Small Firm	NetRegP	NetRegQA	AllSentP	AllSentQA	NetRegP^{LLM}	NetRegQA^{LLM}
Sales Growth	-3.069***	-2.606**	1.032*		-3.192***	-2.556**
Asset Growth	-1.494**	-1.18**	2.053***	0.998**	-1.75***	-1.040***
Leverage	-0.576***	-0.530***	0.409***		-0.477**	
Operating Margin Δ					-1.150*	
Gross Margin Δ	-0.684***				-0.585***	
Panel D: Large Firm	NetRegP	NetRegQA	AllSentP	AllSentQA	NetRegP^{LLM}	NetRegQA^{LLM}
Sales Growth	-0.777**				-0.784**	
Asset Growth	-1.185***		1.179***	1.087**	-1.215***	
Leverage						
Operating Margin Δ						0.331*
Gross Margin Δ		0.305*			-0.161*	0.245*

Table 10: This table shows the results of regressions in which we examine the effect of *NetReg* on sales growth in response to Trump taking over the White House. The time period is restricted to sales growth (which are one-year ahead outcomes) with starting dates from January 1, 2015 to January 31, 2018. Observations with sales growth starting dates from April 20, 2016 to October 20, 2016 are omitted. This is a six-month period centered on the cutoff date of July 20, 2016, which is the date six months prior to the inauguration held on January 20, 2017. The post-inauguration dummy Inauguration is set to one for dependent variables with starting dates after October 20, 2016. Trump is a variable that measures the three-day stock return around the November 8, 2016 election. The sample is restricted to firms that exist both before and after the July 20, 2016 cutoff. Values of *NetReg* after July 20, 2016 takes on the value of *NetReg* from the quarter immediately before July 20, 2016. Standard errors, clustered by 2-digit SIC and quarter, are in the parentheses. Significance is indicated via: *p<0.1; **p<0.05; ***p<0.01.

Effects of *NetReg* Around Trump Inauguration

	Sales Growth$^i_{t+4}$			
	(1)	(2)	(3)	(4)
Trump^i			0.035	0.025
			(0.172)	(0.153)
NetRegP^i_t	−46.663***		−25.754***	
	(9.624)		(5.344)	
NetRegQA^i_t		−13.859		23.605
		(10.995)		(17.432)
Inauguration^i_t	4.182**	4.490**	2.417*	2.679**
	(1.814)	(1.783)	(1.208)	(1.087)
Size^i_t	−2.658**	−2.448**	−2.606**	−2.408**
	(0.972)	(0.938)	(0.960)	(0.936)
Sales Growth^i_t	0.110*	0.111***	0.104	0.105*
	(0.051)	(0.037)	(0.100)	(0.051)
No Regulat Dummy^i_t	−1.407	−3.644*	−1.694	−4.198**
	(1.348)	(1.846)	(1.215)	(1.726)
Never Regulat Dummy^i_t	−4.393*	−6.441**	−5.017**	−7.380**
	(2.216)	(2.699)	(2.165)	(2.509)
Trump^i*NetRegP^i_t			−3.772***	
			(0.954)	
Trump^i*NetRegQA^i_t				−8.082***
				(2.255)
Trump^i*Inauguration^i_t			0.270	0.292*
			(0.240)	(0.153)
NetRegP^i_t*Inauguration^i_t	42.464**		14.418	
	(18.427)		(13.116)	
NetRegQA^i_t*Inauguration^i_t		31.551***		7.626
		(9.231)		(22.700)
Trump^i*NetRegP^i_t*Inauguration^i_t			4.134***	
			(0.966)	
Trump^i*NetRegQA^i_t*Inauguration^i_t				3.655
				(4.094)
2-digit SIC Ind. FE?	Yes	Yes	Yes	Yes
Observations	16,597	15,285	15,918	14,684
R^2	0.084	0.080	0.090	0.087
Adjusted R^2	0.081	0.076	0.086	0.083

Note: *p<0.1; **p<0.05; ***p<0.01

Table 11: This table summarizes the main results for the effects on returns (in percentages). The 22-day rows are pure forecasting regressions and the Call Day rows show the contemporaneous association. The standard errors are clustered by 2-digit SIC and earnings call date. For FF6 returns, the factor loadings used to calculate these risk-adjusted, or abnormal, returns and alphas are estimated over a training window from 252 to 31 trading days prior to the earnings call. In the 22-trading day period following each earnings call, we use [t,t+22] returns for pre-4pm day t calls, and [t+1,t+23] for post-4pm day t calls. Coefficients denote the effect from a one standard deviation increase in the independent variable. Non-missing entries reflect effects that are significant at the 10% level. Significance is indicated via: *p<0.1; **p<0.05; ***p<0.01.

Impacts of NetReg and AllSent on Returns

	NetRegP	NetRegQA	AllSentP	AllSentQA	NetRegP^{LLM}	NetRegQA^{LLM}
Excess Ret (22-day)		0.229**	0.188**	0.400***		0.318***
FF6 Ret (22-day)		0.152*	0.256***	0.350***		0.237***
Excess Ret (Call Day)			0.672***	0.785***		
FF6 Ret (Call Day)			0.683***	0.784***		