Fiscal Risk Perception: Evidence from Analyst Forecasts*

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Abstract

Using a comprehensive U.S. procurement transaction-level database, earnings surprises, and stock returns, we show that analysts systematically under-forecast federal procurement obligations to firms, with significant variation both across and within firms. The effect is stronger for firms with lower bargaining power (micro uncertainty) and during periods of heightened fiscal budgetary risk (macro uncertainty). Excess stock returns increase with procurement exposure only around earnings announcements, consistent with a risk premium channel. These findings suggest that market participants view government contracts as "bad uncertainty," and reflect these beliefs in earnings forecasts and stock returns.

JEL Classification: G1, E6, H5, D8
Keywords: fiscal uncertainty, procurement, analyst, earnings forecasts

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"Current fiscal policy dysfunction," warning that the inability of Congress and the White House to work together on budget and spending bills "creates a level of fiscal uncertainty that is damaging to the U.S. economy." – International Monetary Fund (IMF) Managing Director Christine Lagarde, June 4, 2015, The News & Observer. https://www.newsobserver.com/news/nation-world/national/article23083419.html

1 Introduction

Fiscal risk has attracted increasing attention from both policymakers and the general public, yet our understanding of how market participants perceive this risk, and how it is reflected in financial variables, remains limited. Empirical challenges stem from the absence of high-frequency survey data and the broad scope of fiscal policy instruments (e.g., economic stimulus, tax reforms, tariffs, procurement contracts).

In this paper, we show that analysts systematically under-forecast promised federal government contract amounts, formally referred to as "procurement obligations," with significant variation evident both across firms and within firms over time. This result is more pronounced for firms with lower bargaining power with government (i.e., higher micro uncertainty) and during periods of heightened fiscal budgetary uncertainty (i.e., higher macro uncertainty). Moreover, excess stock returns increase significantly with procurement exposure only during earnings announcement periods. A one standard deviation increase in procurement corresponds to an increase in excess stock returns by 9.2% on an annualized basis. Both earnings surprises and returns evidence is consistent with a risk premium mechanism, indicating that market participants perceive government contracts as a source of "bad uncertainty." This uncertainty arises from the risk that the federal government may modify or terminate contracts after they have been signed. To discipline our empirical findings, we are able to interpret the main results through the lens of a stylized analyst's rational expectations model that incorporates investor loss aversion and fiscal uncertainty.

Our analysis begins with a complete archival download from USAspending.gov, a federal portal providing detailed records of government procurement contracts. Es-

tablished under the 2014 Digital Accountability and Transparency Act (DATA Act) to enhance transparency, the platform requires federal agencies to report every *obligation* transaction, which is when an agency agrees to purchase goods or services, in a timely manner. An obligation transaction can be interpreted as a "purchase signal" and does not, by law, correspond to a cash outlay. 83% of the contracts have only one documented obligation transaction. Observations indicate that these obligation transaction dates, rather than stated contract periods, likely more accurately reflect actual financial commitments and the timing of earnings accruals. It is noteworthy that Brogaard, Denes, and Duchin (2021) are among the first to systematically examine *contract-level* patterns and we are able to replicate their main summary statistics in an overlapped sample (2009-2012). By contrast, our study is among the first in the finance and economics literature to exploit the full *transaction-level* data from this database.

Detailed obligation data are available starting in 2008 with improved firm coverage after the Global Financial Crisis based on our replication of annual OMB reports. Our main sample spans June 2009 to December 2019 and focuses on firms with positive obligations in more than half of the sample quarters. We match earnings surprises with procurement obligations at the firm-fiscal quarter level, yielding 474 firms and 19,027 observations.

Our main dependent variable, Beat, equals one if the firm's actual earnings per share (EPS) are greater than the I/B/E/S consensus forecast median immediately prior to the announcement. We also consider a few standardized unexpected earnings (SUE) measures in the literature (e.g., Froot, Kang, Ozik, and Sadka (2017)). Our main independent variable captures firm procurement or fiscal exposure, measured as the total obligated amount for each firm-fiscal quarter, scaled by average revenue over the past 4 quarters; various robustness variables are also considered. Fiscal exposure for the average firm in our sample is approximately 2%, with values reaching up to 13.3% at the 95th percentile and 32% in the maximum case.

First, our closed-form rational expectations model predicts that analysts underforecast earnings more for firms with greater fiscal risk exposure. Consistent with this

prediction, our first empirical result shows that a one standard deviation (SD) increase in fiscal dependence is associated with a 1.9% increase (approximately 0.12 SD) in the likelihood of a Beat event. The results are extremely robust to alternative measures of fiscal exposure and earnings surprises, to restricting the sample to firms with more active obligations (intensive margin), and to excluding obligations issued by the Department of Defense, the largest procurement agency.

Next we introduce a panel framework that allows for time-varying fiscal exposure and examines both cross-firm and within-firm variation. This approach extends our analysis beyond the predictions of the conceptual model, enabling us to explore potential mechanism variables later. To start, pooling regressions that include time fixed effects yields results similar to the cross-firm analysis above, both economically and statistically, which is expected. More interestingly, we find that within a firm larger government obligations also predict a higher likelihood of positive earnings surprises. The economic magnitude of the within-firm variation is approximately half that of the full effect.

Over time, we find that such predictability is, and remains, both statistically significant and economically strong leading into late 2015, before peaking again in late 2017 and in late 2019. This temporal pattern is economically meaningful, as it aligns closely with several major episodes of fiscal uncertainty in recent history: the "Fiscal Cliff" of 2013–2014 and the sequence of debt limit suspensions enacted by Congress in late 2017 and again in late 2019. Taken together, this constitutes the first indication of a risk-based explanation.

We also investigate return implications beyond earnings announcement effects, as patterns in stock return responses should shed light on the underlying economic mechanism. If the excess returns of the high-fiscal-dependence portfolio increase only on earnings announcement days, this would suggest a belief correction that translates into a higher risk premium. By contrast, if the returns of the high- and low-fiscal-dependence portfolios remain statistically indistinguishable at all times, it would imply that investors incorporate fiscal dependence into firm fundamentals, an interpretation inconsistent with a risk premium channel. We find that there are significant risk premium gains explained

by government procurement exposures *only* during a firm's earnings announcement window. During earnings announcement days, a one standard deviation increase in procurement exposure corresponds to an increase in excess stock returns by 9.2% on an annualized basis. This constitutes the second evidence of a risk-based explanation.

We next formally test fiscal uncertainty as a potential mechanism underlying our results, as suggested by our closed-form solution. Under this channel, predictability should increase with budgetary uncertainty. Importantly, we need variation in such uncertainty: it should vary over time with changes in the aggregate fiscal environment and across firms depending on firm contractual positions and relations with the federal government. For instance, even when aggregate fiscal risk is elevated, firms with greater bargaining power may face lower effective uncertainty because they are better positioned to renegotiate or shield themselves from contract modifications or cancellations.

To capture both dimensions of this mechanism, we construct two empirical proxies at different levels of granularity. At the micro level, we exploit heterogeneity in firm-level bargaining power, constructing a renegotiation index that captures the ability of individual firms to manage or mitigate fiscal risk. At the macro level, we employ time-varying measures of government budgetary uncertainty that allow us to test whether return predictability is amplified during episodes of heightened fiscal stress. Together, these complementary measures enable us to disentangle the aggregate and firm-specific components of fiscal uncertainty and evaluate their respective contributions to the predictability we document.

First, we construct a firm-level "renegotiation index" following Brogaard, Denes, and Duchin (2021), who use three variables to capture bargaining power from contract renegotiations: award increase, deadline extension, and weak monitoring. Consistent with our hypothesis, firms with greater bargaining power, and thus lower micro uncertainty, exhibit significantly lower predictability.

Second, we construct two proxies designed to capture government budgetary uncertainty: (a) debt limit event dates, which directly identify periods of fiscal stress in the U.S., and (b) the component of Fiscal Uncertainty from Baker, Bloom, and Davis (2016)

attributable to newspaper discussions of the debt limit, providing a more continuous measure. Because this mechanism variable is time-series in nature, our interaction design returns to the panel framework with flexible fixed effects. Consistent with our hypothesis, predictability strengthens significantly during periods of heightened fiscal uncertainty. A one standard deviation increase in fiscal uncertainty raises predictability in the cross-firm margin by about 26-30%. Moreover, fiscal uncertainty accounts for nearly all statistically significant within-firm variation — an intuitive result. As budgetary uncertainty increases, analysts' under-forecasts become more strongly associated with the magnitude of government obligations to the firm, expressing greater doubt about whether these government promises will eventually translate into earnings.

Our rational expectations model also suggests that predictability could increase with analyst inattention or information delays, suggesting these as alternative mechanisms. A textual analysis of earnings call transcripts shows that variation in analyst attention to procurement does not significantly explain predictability in either the cross-firm or within-firm dimensions. Testing the information delay mechanism is empirically more challenging due to the absence of real-time posting data. To address this, we conducted two scraping exercises (October 1, 2023-January 18, 2024, and August 8, 2024-November 5, 2024), both of which indicate that most agencies release transactions promptly, typically within 30 to 40 calendar days of the transaction date.

Our work contributes to several strands of research. First, while there is an extensive literature on the asset pricing effects of fiscal policy,[1] there is little direct evidence on how market participants form their expectations of future fiscal policy and its associated risk in the literature. Among recent related work, Bianchi, Gómez-Cram, and Kung (2024) and Xu and You (2025) adopt distinct identification strategies – leveraging the timing of congressional tweets and the arrival of exogenous macroeconomic announcements, respectively – to examine how investors perceive fiscal risk. Both studies find that

[1]For instance, Goulder and Summers (1989), Akitoby and Stratmann (2008), Sialm (2009), Afonso and Sousa (2011), Croce, Kung, Nguyen, and Schmid (2012a), Croce, Nguyen, and Schmid (2012b), Baker and Yannelis (2017), D'Acunto, Hoang, and Weber (2018), Croce, Nguyen, and Raymond (2021), Jiang, Lustig, Van Nieuwerburgh, and Xiaolan (2023), Gomez Cram, Kung, and Lustig (2024), and Cassidy and Mirani (2025), and many others.

investors actively form expectations and perceive fiscal risk, with meaningful effects on high-frequency asset prices. Our paper contributes to this growing literature by being among the first to examine how a key group of market participants, financial analysts, perceive fiscal risk. By leveraging detailed procurement obligation data with broad firm-quarter coverage, we provide a comprehensive perspective on how fiscal exposure enters earnings forecasts. Our findings highlight a novel channel: fiscal uncertainty transmits to the private sector, i.e., the capital market, through firm links with federal government procurement contracts.

Second, our paper contributes to the public finance and procurement literature that currently focuses on the economics of procurement contracts. A traditional literature examines the structure and strategies of procurement contracts,[2] while a growing body of research uses direct (often non-U.S.) data to study their impact on firm outcomes. For instance, Ferraz, Finan, and Szerman (2016), Fadic (2020), and di Giovanni, García-Santana, Jeenas, Moral-Benito, and Pijoan-Mas (2022) use government data from Brazil, Ecuador, and Spain to show that government procurement contracts have a positive effect on firm output and employment growth. A recent publication by Brogaard, Denes, and Duchin (2021) is among the first to formally apply the extensive `USAspending.gov` database in the finance literature. The authors study plausible links between U.S. government contracts and firms and establish the important role of political connections. Our paper investigates a new question also using the granular data in `USAspending.gov`: how do investors perceive procurement contracts? Using earnings surprises, stock returns, and interaction-based frameworks, we provide evidence that procurement contracts serve as a channel through which fiscal uncertainty gets priced in financial markets. This suggests that the federal government can act as a source of market risk, rather than solely as a stabilizing force as implied by traditional models.

The remainder of the paper is organized as follows. Section 2 outlines the concep-

[2]In a book by Klemperer (2004), he discusses how auction design influences bidder behavior and procurement efficiency; Bajari, McMillan, and Tadelis (2009) compare competitive bidding with negotiation in procurement; Søreide (2002) reviews strategies to mitigate corruption in procurement; and Gereffi, Humphrey, and Sturgeon (2005) examine the working of procurement contracts in global value chains.

tual framework and model predictions. Section 3 describes the data. Sections 4 and 5 present the main results on predictability, return dynamics, and underlying mechanisms. Section 6 considers alternative explanations. Section 7 concludes.

2 Conceptual Framework

We consider a stylized model of analyst expectations formation, featuring investor loss aversion, fiscal uncertainty, and imperfect information.[3] Full detailed proofs are available in Appendix A.

Firm i's actual earnings from time $t-2$ to $t-1$ (i.e., $(t-2, t-1]$) are announced at time t, and we denote firm earnings as X_t^i. For simplicity, we omit the firm indicator i from here on. Analysts form earnings forecasts at time $t-1$ (without loss of generality) to be compared with actual earnings X_t announced at time t; we denote earnings forecasts as X_t^F. We next assume that buy-side investors are loss averse and they follow sell-side analysts' recommendations. As a result, analysts will be penalized more if their forecasts turn out to be higher than the actual value. To summarize, analysts choose forecast value X_t^F by solving the following minimization problem:

$$\min_{X_t^F} \mathbb{E}_{t-1} \left[(X_t - X_t^F)^2 + \lambda \cdot \mathbf{1}_{X_t^F > X_t} \cdot \frac{48(X_t^F - X_t)^2}{(X_t^F - min(X_t))^2} \right], \tag{1}$$

where $X_t - X_t^F$ denotes the earnings surprise, $\lambda > 1$ captures the loss aversion of investors, and term $\frac{48}{(X_t^F - min(X_t))^2}$ is included to obtain a closed-form solution under uniformly-distributed shock assumptions.

2.1 X_t's data generating process

X_t, the actual earnings of period $(t-2, t-1]$ announced at time t, is a flow variable that consists of two components: earnings made from retail sales, R_t, and earnings made

[3] Imperfect information can arise from either imprecise signals upon arrival or delays in information dissemination. While these mechanisms are typically modeled differently in the theoretical models, they yield equivalent implications within our framework. As a result, our conceptual model remains agnostic to the specific source of information imperfections.

from existing procurement contract transactions, κG_t:

$$X_t = R_t + \kappa G_t, \tag{2}$$

where X_t, R_t, and κ would have a superscript i. The parameter $\kappa > 0$ denotes fiscal dependence, as the ratio $\frac{\kappa G_t}{X_t}$ captures the proportion of a firm's earnings attributable to government spending. Direct evidence on the empirical properties of κ is limited beyond the present study. While in practice κ may vary over time, we assume it to be constant (or effectively stable) in order to maintain the tractability of the model. The empirical analyses in subsequent sections relax this assumption. We denote the long-run means of G and R by the constants $\bar{G} > 0$ and $\bar{R} > 0$, respectively.

We assume that analysts can collect sufficient information about retail sales and form rational expectations about R_t with uncertainty following a uniform distribution,

$$R_t = \bar{R} + \eta_t, \text{ where } \eta_t \sim U(-1, 1). \tag{3}$$

For government spending, we assume that G_t consists of three components: G_{t-1}, which denotes government spending during period $(t-3, t-2]$ and is known at time $t-1$ when analysts form forecasts; D_{t-1}, which presents information on deviations from G_{t-1} during period $(t-2, t-1]$ that analysts *observe* within period $(t-2, t-1]$; and ϵ_t, a residual term capturing fiscal and government budgetary uncertainty that constitutes the core source of risk in our theory:

$$G_t = G_{t-1} + D_{t-1} + \epsilon_t, \text{ where } \epsilon_t \sim U\left(-\frac{\phi}{K}, \frac{\phi}{K}\right). \tag{4}$$

The parameter $\phi > 0$ can be interpreted as procurement revenue risk relative to retail revenue risk (with retail uncertainty normalized above). Intuitively, greater fiscal uncertainty increases the conditional variance of G. For example, the federal government may revise contracts by reducing promised payment amounts over time. In Appendix Table A2, we show that promised payment amounts decrease statistically and significantly

during periods of heightened budgetary uncertainty.[4] Next we introduce the parameter $K > 0$ to capture parsimoniously the precision and timeliness with which analysts learn about spending deviation D_{t-1}. As $K \to \infty$, analysts obtain complete and precise information. Empirically, K may be proxied by measures such as information disclosure lags or analyst inattention. Finally, we assume $\bar{D} = 0$ and that the shocks η_t and ϵ_t are i.i.d. and mutually independent.

2.2 Model solution and testable predictions

After substituting the process for X_t into the objective function (1) and applying the standard rules of integration, the minimization problem simplifies to the following closed-form expression:

$$\min_{X_t^F} \left[(\bar{R} + \kappa G_{t-1} + \kappa D_{t-1} - X_t^F)^2 + \frac{1}{3}\left(1 + \frac{\kappa^2\phi^2}{K^2}\right) + \lambda \cdot \frac{\left(X_t^F - \bar{R} - \kappa G_{t-1} - \kappa D_{t-1} + \frac{\kappa\phi}{K} + 1\right)^2}{\frac{\kappa\phi}{K}} \right]. \tag{5}$$

The first-order condition is obtained by differentiating this with respect to X_t^F:

$$X_t^F = \frac{(\kappa G_{t-1} + \kappa D_{t-1} + \bar{R})(2 + \lambda/(\kappa\phi/K)) - \frac{\lambda(\kappa\phi/K+1)}{\kappa\phi/K}}{2 + \lambda/(\kappa\phi/K)}. \tag{6}$$

The expected earnings surprise, $\text{Surprise}_t(\kappa, \lambda, \phi, K)$, can be derived in closed form:

$$\text{Surprise}_t(\kappa, \lambda, \phi, K) = \bar{R} + \kappa G_{t-1} + \kappa D_{t-1} - X_t^F, \tag{7}$$

$$= \frac{\lambda(1 + \kappa\phi/K)}{\lambda + \kappa\phi/K} > 0. \tag{8}$$

[4] In our empirical sample, which we discuss later in Section 4, almost 30% of all government contracts show some pattern of revisions after a government contract is signed (i.e., through changing amounts, time needed, or monitoring force), channeling procurement uncertainty. In a more concrete example, the DoD canceled several large-scale programs such as the Army's Ground Combat Vehicle (GCV) program in 2014 due to funding constraints and shifting priorities within a reduced defense budget. Lockheed Martin (NYSE: LMT), a major U.S. defense contractor, was immediately impacted, among many others.

***Prediction 1**: Under reasonable parameter assumptions (i.e., $\kappa, \phi, K > 0$, $\lambda > 1$), it is always optimal for analysts to underestimate earnings.*

Next, we derive three parsimonious predictions that guide our baseline empirical and mechanism analyses. In a stylized setting with loss aversion $\lambda > 1$, non-zero fiscal uncertainty $\phi > 0$, and imperfect information arrivals $K \neq \infty$, a clear implication emerges regarding the relationship between the fiscal dependence parameter κ and earnings surprises. The derivative of $\text{Surprise}_t(\kappa, \lambda, \phi, K)$ with respect to κ, $\frac{\partial \text{Surprise}}{\partial \kappa}$ has a closed-form solution that is strictly positive:

$$\frac{\partial \text{Surprise}(\kappa, \lambda, \phi, K)}{\partial \kappa} = \frac{\lambda(\lambda - 1)\phi/K}{(\lambda + \kappa\phi/K)^2} > 0. \tag{9}$$

***Prediction 2:** Firms with greater fiscal exposure κ should exhibit larger earnings surprises.*

Intuitively, analysts choose to more heavily under-forecast the earnings of a firm with greater exposure to government budgetary risk, leading to a more positive earnings surprise on average. This is consistent with several influential papers in the accounting literature that discuss the relationship between analyst forecast accuracy and uncertainty (see, e.g., Moffat (1988), Gong, Li, and Wang (2011), You and Zhang (2009), and Bonsall IV, Green, and Muller III (2020), among others). Our model differs by introducing fiscal uncertainty directly.

In addition, we show that such a relationship in Equation (9) should increase with the effective government budgetary uncertainty ϕ in a general case when loss aversion is sufficiently large relative to scaled fiscal uncertainty:[5]

$$\frac{\partial \text{Surprise}(\kappa, \lambda, \phi, K)}{\partial \kappa \partial \phi} = \frac{\frac{\lambda(\lambda-1)}{K}(\lambda + \kappa\phi/K)\left[\lambda - \frac{\phi\kappa}{K}\right]}{(\lambda + \kappa\phi/K)^4} > 0. \tag{10}$$

[5]This assumption, $\lambda > \frac{\phi\kappa}{K}$, is likely quite realistic, as we later find that empirically κ is typically < 0.1 (i.e., a small κ) and we observe timely but not perfect transaction data postings (i.e., a large K); we provide empirical evidence for these two variables later.

An additional implication of the optimization concerns the parameter K, which governs the timeliness and precision of information: $\frac{\partial \text{Surprise}(\kappa,\lambda,\phi,K)}{\partial \kappa \partial K} = \frac{-\frac{\lambda(\lambda-1)\phi}{K^2}(\lambda+\kappa\phi/K)(\lambda-\frac{\kappa\phi}{K})}{(\lambda+\kappa\phi/K)^4} < 0$, if $\lambda > \frac{\phi\kappa}{K}$. Intuitively, the predictability of fiscal exposure for earnings surprises declines as the precision and timeliness of information, K, increases.

> ***Predictions 3 & 4:*** *The predictability of firm fiscal exposure to earnings surprises should* ***increase*** *with fiscal uncertainty (Prediction 3) and* ***decrease*** *with information precision and timeliness (Prediction 4).*

These model predictions form the core economic foundation of our subsequent empirical analysis. Predictions 1–3 are tested and examined in Sections 3–5, which constitute our main results, while Prediction 4 is discussed in Section 6 as an alternative mechanism.

3 Data and Summary Statistics

3.1 A transaction-level procurement contract database

Before 2020, government spending directed to firms primarily took the form of procurement contracts rather than economic stimulus programs. For instance, in fiscal year 2019 total discretionary spending amounted to approximately $1.3 trillion, of which $586 billion was allocated to procurement-related expenditures to individual firms and organizations. The remainder of the discretionary spending largely reflected operational costs, grants, and subsidies outside the scope of procurement contracts.[6] In this section, we explain how we obtain and use a transaction-level procurement contract database in our research.

Our analysis begins with downloading the complete archive of `USAspending.gov`, a federal government portal that offers comprehensive records of the federal government's

[6] According to Figure 7 in Xu and You (2025), which is also based on data from `USAspending.gov`, economic stimulus was the primary form of government spending during 2020 and 2021, accounting for approximately 68% of the total annual government spending. From 2010 to 2019, economic stimulus spending accounted for a nearly negligible fraction of annual government spending.

budget and is supervised under the Digital Accountability and Transparency Act of 2014 (DATA Act) to promote government transparency and accountability.[7] The archival data is mainly organized at the "obligation" transaction level:

> "*...obligation in layman's terms means a binding agreement that will result in outlays, immediately or in the future. For example, an agency incurs an obligation when it enters into an agreement to purchase goods or services. The agency pays the provider upon receipt of the goods or services; ... that payment is an outlay... When you place an order, sign a contract, award a grant, purchase a service, or take other actions that require the Government to make payments to the public or from one Government account to another, you incur an obligation.*"[8]

Two aspects of this institutional detail are important for our empirical design. First, an obligation transaction can only be interpreted as a "purchase signal" and provides limited clarity on when this firm realizes the associated revenue, which may occur in the current or subsequent quarters. Second, an obligation transaction does not, by law, correspond to a cash outlay. This limitation is nonetheless consistent with the DATA Act's primary focus on budgetary transparency rather than payment timing. Based on our observations of the original archival dataset, 83% of the contracts contain only a single documented obligation transaction. Obligation action dates in multi-transaction contracts display irregular yet frequent patterns, which can often be linked to specific contractual actions. This suggests that obligation action dates, rather than stated contract periods, likely more accurately reflect actual financial commitments and the timing of earnings accruals. We include additional discussions and examples in Appendix Section B. These direct observations also offer empirical insights for extending our baseline cross-firm test (Section 4.1) – as implied by our model – into a panel context (Section 4.2) later (e.g., within firm or stagger events).

Each obligation transaction entry includes details such as firm information, the

[7] Here is the link for accessing the archive: https://www.usaspending.gov/download_center/award_data_archive. The archival dataset is maintained and updated on a monthly basis. The full database exceeds 1.5 terabytes and continues to grow, posing an implicit barrier to researchers. In this study, we download and process data beyond our immediate scope, with the objective of further cleaning the database and making firm–time variables available to the research community.

[8] https://fedspendingtransparency.github.io/whitepapers/obligation/; https://obamawhitehouse.archives.gov/sites/default/files/omb/assets/a11_current_year/app_f.pdf

date, the obligated amount, and description codes. For our research, we aggregate those obligated amounts to the firm-fiscal quarter level. We also assess and validate the data coverage on USAspending.gov by comparing replicated total procurement spending with headline figures reported by the federal government.

Transaction-level data becomes available starting in 2008 and improves in reliability following the Global Financial Crisis (GFC). Accordingly, our main sample spans June 2009 to December 2019. Details on our firm sample are provided after the introduction of financial variables.

This website also provides contract-level – or what it refers to as "award-level" – information, including details such as awarding agency, start date, potential end date, contract type, revision history, and so on. We obtain and merge this information into our analysis in the mechanism section. It is noteworthy that we are not the first to use contract-level data from this website; in fact, Brogaard et al. (2021) are among the first to systematically examine contract-level patterns, and we are able to replicate their main summary statistics in an overlapping sample from 2009 to 2012. Building on this, our study comprehensively analyzes the full transaction-level data available from USAspending.gov. We relegate more details to Internet Appendix IA.1.

3.2 100-day scraping exercises: Measuring information delays

By law, federal agencies are required to report obligations in a timely manner, typically within days or weeks, except for the Department of Defense (DoD), which is allowed a 90-day delay due to national security considerations. The publication dates of obligation transactions on USAspending.gov could have profound implications for predictability according to our model. However, the archive does not provide such information. To evaluate whether information delays align with statutory requirements, we conducted two extensive web-scraping exercises to quantify the typical lag between the *actual* obligation transaction date and its *posting* date.

Our strategy is to capture real-time transaction posts on the website that have

not entered the archival data.[9] Transactions obtained through the API interface but absent from the most recent archival update represent incremental transactions since the last update. For each such transaction, we construct an "entry delay (days)" variable, defined as the number of days between the date the transaction is retrieved from the API endpoint and its recorded action date.

We conducted two daily scraping periods of just over 100 days each: October 1, 2023–January 18, 2024, and August 8–November 5, 2024. The two exercises reveal consistent patterns (see Appendix Figure A1). All federal agencies except the DoD publish transactions within 30 to 40 calendar days of the transaction date. Thus, even if a transaction occurs on the last day of a firm's fiscal quarter, it is typically made public before the earnings announcement, which usually takes place about 40 days after quarter-end (per Form 10-Q requirements). However, the exercises also underscore the importance of robustness tests both with and without DoD-sponsored obligations in our main analysis, which we show later.

3.3 Financial datasets

Building on the full procurement transaction database constructed above, we first restrict attention to firms with positive obligated amounts in more than half of the quarters in our sample period (2009/06–2019/12). This focuses the analysis on firms for which procurement contracts are economically relevant. We further exclude firms under NAICS code 54 (Professional, Scientific, and Technical Services) due to their persistently high and stable procurement levels, which are often tied to ultra high-specialty, high-security, scientific, or non-profit activities.[10]

Finally, we apply standard procedures when merging firm-time earnings forecasts

[9] On the technical front, we find that `USAspending.gov` provides multiple API endpoints for accessing more timely data. We mainly utilize two of them to download real-time updated award information (`https://api.usaspending.gov/api/v2/search/spending_by_award/`) and real-time updated historical transaction data related to specific parent awards (`https://api.usaspending.gov/api/v2/transactions/`).

[10] For example, *Leidos*, which provides IT and cybersecurity solutions to federal agencies; *Booz Allen Hamilton*, a firm known for its work with the U.S. government, especially in defense and cybersecurity consulting; *AECOM*, which works on major public works projects; and *RAND Corporation*, a nonprofit that undertakes research for policy and decision-making often funded by government grants and contracts.

and stock variables. We restrict the sample to firm-quarters with common shares traded on NYSE, AMEX, or NASDAQ, with at least one analyst forecast in I/B/E/S, and with positive quarterly revenue. These restrictions ensure that our variable constructions are economically meaningful. Additional financial data (e.g., market capitalization, book-to-market, and daily returns) are obtained from CRSP. The final sample consists of 474 firms and 19,027 firm-fiscal quarters.

3.4 Main variables and summary statistics

At each firm–quarter $\{i, t\}$, our primary dependent variable is a simple earnings surprise dummy, Beat, which equals one if the firm's actual earnings per share (EPS) exceed the I/B/E/S consensus forecast median immediately prior to the announcement. This measure is not affected by standardization methods or scaling choices, ongoing subjects of debate in the literature. In addition, we employ two standardized unexpected earnings (SUE) measures for robustness and mechanism tests. The first, SUE_1, is defined as the earnings surprise (actual EPS minus forecast median) scaled by analyst disagreement.[11] The second, SUE_2, follows Froot, Kang, Ozik, and Sadka (2017) and is defined as the earnings surprise (actual EPS minus forecast mean) scaled by the quarter-end stock price. We use all three measures in our main analyses and in the robustness tests reported in the Appendix.

Appendix Table A3, Panels A and B, reports summary statistics for the main variables at both the firm and panel levels. Actual EPS exceeds the analyst forecast in 66% of cases, a likelihood that is statistically greater than 50% (p-value = 0.00). This pattern indicates that analysts systematically under-forecast on average, consistent with both the prior literature and our theoretical Prediction 1 in Section 2. In economic terms, actual EPS is on average 1.2 standard deviations above the forecast median.

Another key variable is Procurement, defined as the total obligated amount in the current firm–quarter scaled by average quarterly revenues over the past four quarters

[11] Analyst disagreement is proxied by the standard deviation of analyst forecasts from the current and previous quarter. We use two quarters because the number of forecasts in a single quarter can be too small for a reliable standard deviation. Results are not sensitive to this empirical choice.

(including the current quarter). This size adjustment accounts for the well-documented positive relationship between firm size and earnings surprises (see, e.g., Loughran and McDonald (2011), among others). The measure can be interpreted as the share of a firm–quarter's revenue attributable to procurement, conceptually aligning with κ in our theoretical framework (Section 2). Firm-level averages are also constructed. According to Table A3, Panel A, fiscal exposure for the average firm in our sample is approximately 2.1%, rising to 13.3% at the 95th percentile and 32% at the maximum. By construction, lower-percentile observations are not exact zeros, given our sample selection criteria. Overall, there is sizable variation in fiscal exposure across firms.

Finally, we illustrate the composition of the final firm sample. Figure 1 presents three statistics at the NAICS 2-digit (industry) level: the total number of firms (shown by the numbers above the bars), average procurement exposure (bars), and average market capitalization (line). Of the 474 firms in our sample, 171 are in manufacturing (NAICS 33), which primarily includes heavy and complex manufacturing such as metals, machinery, electronics, and transportation equipment. The information and utility sectors are also well represented, whereas industries such as retail trade, hospitality, and arts and entertainment are scarcely represented. The construction industry exhibits the highest average procurement exposure with procurement earnings accounting for roughly 6.3% of quarterly revenues and more than 10% when restricted to the top quartile of firms (see Figure IB.1 in the Internet Appendix). Importantly, there is near-zero correlation between our industry-level procurement measure and stock market size. Finally, while not easily visualized in figures, substantial within-industry variation in procurement exposure also exists.

[Insert Figure 1 here]

4 Main Results

Assuming loss aversion and government budgetary uncertainty, the closed-form model solution in Section 2 predicts that analysts are more likely to under-forecast earn-

ings for firms with greater fiscal risk exposure, as stated in Prediction 2. Section 4.1 presents the main cross-firm results as directly implied by the conceptual model, while Section 4.2 conjectures a panel framework that allows for time variation in firm-level fiscal risk exposure. This framework extends the model's predictions by enabling an introduction of potential time-varying mechanism variables, which we explore in Section 5. Finally, we examine the stock market implications in Section 4.3.

4.1 Main cross-firm results

In this section, we follow the model prediction and establish a cross-firm relationship. We obtain firm-level earnings surprise proxies ($\overline{\text{Beat}}_i$) and fiscal exposures ($\overline{\text{Procurement}}_i$) and estimate the following specification:

$$\overline{\text{Beat}}_i = \alpha_{d(i)} + \beta\ \overline{\text{Procurement}}_i + \boldsymbol{\delta}\overline{\boldsymbol{X}}_{\boldsymbol{i}} + \varepsilon_i. \tag{11}$$

We include a set of control variables (denoted by $\boldsymbol{X}$) that are standard in the literature (e.g., Loughran and McDonald (2011), Akbas (2016), and Akbas, Jiang, and Koch (2020)). To tailor them to our setting, we compute firm-level averages. Specifically, the controls comprise market capitalization, book-to-market ratio, past stock returns over the [-61, -12] and [-6, -2] windows preceding the earnings announcement, proportion of shares held by institution, idiosyncratic volatilities estimated over the [-11, -2] window preceding the earnings announcement, turnover ratio over the [-61, -12] window preceding the earnings announcement, and the most recent earnings surprise. Detailed definitions of these variables are provided in Internet Appendix Table IB.1. We also include NAICS two-digit industry fixed effects, denoted $\alpha_{d(i)}$. Our coefficient of interest is β.

Table 1 presents the regression results. Our discussion centers on Columns (3) and (4), which incorporate the full set of controls. Both columns yield positive and statistically significant coefficient estimates of interest significant at the 1% and 5% levels, respectively. In terms of economic magnitude,the coefficient estimates of 0.3522^{***} and 0.3181^{***} imply that a one standard deviation increase in fiscal dependence corresponds

to a 1.7-1.9% higher likelihood of a Beat event, or roughly 0.11–0.12 standard deviations.

[Insert Table 1 here]

Appendix Table A4 reports robustness tests for the cross-firm specification. Panel A considers four alternative measures of fiscal exposure: (i) the logarithm of total obligated amounts, (ii) obligated amounts scaled by average quarterly revenues over the past two quarters, (iii) obligated amounts scaled by current-quarter revenue, and (iv) obligated amounts scaled by end-of-quarter market capitalization. The first alternative measure (reported in the second row) does not adjust for firm size. Nevertheless, all alternative measures produce highly consistent results with statistically significant positive coefficients throughout.

In Panel B, we examine predictability along the intensive margin by restricting the sample to firms with active obligated transactions in most quarters of the sample period (i.e., more than 80% of the quarters between 2009/Q2 and 2019/Q4). We refer to this subsample as the Intensive Margin sample. The t-statistic reported in Column (4) is borderline significant at 1.64, and the economic magnitude of the coefficients is slightly smaller than in Table 2. Taken together, these findings suggest that the extensive margin, comparing firms with inactive transactions to those with consistently active transactions, plays an important role in driving our main results.

Panel C excludes all transactions awarded by the Department of Defense (DoD) when constructing the procurement exposure variable. During our sample period, the DoD accounts for 2.23 million of the 10.78 million contracts and $416.06 billion of the total $1.84 trillion obligated amounts. This robustness test is particularly important given our scraping exercise in Section 3.2, which shows that the DoD systematically delays publication by roughly 90 days.

Finally, although the Beat measure is not sensitive to firm size or scaling choices, we also examine two continuous measures of standardized unexpected earnings (SUE), SUE1 and SUE2, introduced in Section 3.4. The first normalizes earnings surprises by analyst disagreement, while the second normalizes them by stock prices. In Panel D, the

β estimate in this cross-firm specification remains positive and statistically significant at 3.9405**. In terms of economic magnitude, a one standard deviation increase in procurement earnings is associated with a 0.21 unit increase in earnings surprises, equivalent to 0.16 standard deviations. This effect is economically meaningful given that the average value of SUE_1 in our sample is 1.24 units. The second measure, SUE_2, follows the methodology of Froot, Kang, Ozik, and Sadka (2017), which scales earnings surprises by the quarter-end stock price. Results using this measure remain robust and statistically significant.

4.2 Panel framework

Next, we extend Equation (11) to a firm–quarter panel and, as discussed earlier, match current quarter earnings surprises with contemporaneous obligation amounts. This framework allows us to first replicate the cross-firm evidence in a pooled regression (Table 2) and then exploit within-firm variation (Table 3). In both cases, we find strong evidence of predictability, holding in both the cross-sectional and within-firm aspects. The specification is given by:

$$\text{Beat}_{i,t} = \gamma_t \times \alpha_{d(i)} + \alpha_i + \beta \text{ Procurement}_{i,t} + \boldsymbol{\delta X_{i,t}} + \varepsilon_{i,t}. \tag{12}$$

The main variables are defined in Section 3.4. $\gamma_t \times \alpha_{d(i)}$ denotes industry–quarter fixed effects, where $d(i)$ represents firm i's NAICS two-digit industry classification. These fixed effects account for time-varying industry shocks and thereby help validate the cross-firm relationship in the panel setting. α_i denotes firm fixed effects, which absorb all time-invariant firm characteristics and allow us to exploit within-firm variation. Finally, β is the coefficient of interest.

Table 2 reports the pooling regression results. Columns (1)-(5) are at the firm-quarter level and Column (6) collapses the data to the industry-quarter level. At the firm-quarter level, the coefficient estimates of procurement exposure are all statistically significant and positive at the 1% level, after controlling for industry, quarter, or industry-

quarter fixed effects. These fixed effects make this specification a pooling regression, essentially averaging out time-varying cross-firm estimates. Economic magnitudes are similar to those in Table 1. In terms of economic magnitude, the coefficient estimate of 0.3074*** indicates that a one standard deviation (SD) increase in fiscal dependence is associated with a 1.97% increase in the likelihood of a Beat event. At the industry-quarter level (Column (6)), the coefficient has the expected sign but is statistically weaker, suggesting that cross-firm variation is not driven by cross-industry differences. Appendix Table A5 shows that this result is robust to alternative fiscal dependence measures, the aforementioned Intensive Margin firm subsample, exclusion of DoD transactions, and alterative earnings surprise measures.

[Insert Table 2 here]

Next, we examine within-firm predictability by including firm fixed effects. Table 3 presents the results. In Panel A, we find a positive but borderline significant coefficient (t-statistic = 1.66) when including only firm fixed effects in Column (6). This suggests that, within a given firm, larger government obligations predict a higher likelihood of positive earnings surprises. The economic magnitude of the within-firm effect is approximately half that of the full-sample effect. Panel B restricts the sample to firms with more intensive procurement activity—those with non-zero government obligations in nearly every quarter of the sample period—thereby providing richer within-firm variation to exploit. In this subsample, we find a more statistically significant and economically larger coefficient in Columns (6)-(7). Specifically, the estimate of 0.2174** implies that a one standard deviation increase in government obligations predicts a 1.4% increase in the likelihood of a Beat event in the current quarter.

[Insert Table 3 here]

Overall, we find that procurement transactions are strong predictors of earnings surprises, both across and within firms. Analysts under-forecast procurement-related

earnings, and we find no evidence of learning; specifically, there is no significant negative coefficient when we use last quarter's procurement exposure to predict earnings surprises.

Figure 2 plots the predictive coefficient β from the specification in Column (5) of Table 2, estimated using a rolling eight-quarter window. This specification captures both cross-firm and within-firm effects. Predictability is particularly strong and statistically significant leading into late 2015, with additional peaks in late 2017 and late 2019. The pattern reveals meaningful time variation coinciding with major episodes of fiscal uncertainty: the "Fiscal Cliff" of 2013-2014 and the recurring debt limit suspensions in late 2017 and late 2019. Taken together, these dynamics provide the first indication of a risk-based explanation.

[Insert Figure 2 here]

4.3 Return implications

Thus far, our results indicate that analysts systematically underestimate earnings related to procurement contracts both across and within firms. We now turn to stock return implications beyond earnings announcement days, as return patterns provide additional insight into the underlying economic mechanism. If the excess returns of the high-fiscal-dependence portfolio increase only on earnings announcement days, this would suggest a belief correction that translates into a higher risk premium. By contrast, if the returns of high- and low-fiscal-dependence portfolios remain statistically indistinguishable at all times, it would imply that investors incorporate fiscal dependence into firm fundamentals — an interpretation inconsistent with a risk premium channel. Our evidence strongly supports the former case: abnormal returns are concentrated around announcement days, consistent with belief updating and an associated risk premium adjustment.

To investigate this, we construct the following specification that expands our analysis to the firm i trading day τ level (i.e., including trading days when there are no

earnings announcements) as follows:

$$eRet_{i,\tau} = \gamma_{t(\tau)} \times \alpha_i + \beta_1 \text{Procurement}_{i,t(\tau)-1} + \beta_2 \text{I}_{i,ann.} + \beta_3 \text{I}_{i,ann.} \text{Procurement}_{i,t(\tau)-1} + \varepsilon_{i,\tau}. \tag{13}$$

Here, $eRet_{i,\tau}$ denotes the logarithm of abnormal returns for stock i on trading day τ, calculated as the difference between the daily log return of stock i and the CRSP daily log value-weighted market return including distributions. This construction effectively controls for market-wide return fluctuations. $\text{Procurement}_{i,t(\tau)-1}$ measures firm i's procurement exposure in the previous fiscal quarter, where $t(\cdot)$ maps each trading day τ to its corresponding fiscal quarter. Earnings announcements occur at some point within quarter $t(\tau)$. $\text{I}_{i,ann.}$ is a daily indicator variable equal to one during the earnings announcement window, defined as the three-day interval spanning [-1, +1] trading days around the announcement of earnings for the prior fiscal quarter that falls within the current quarter. $\gamma_{t(\tau)} \times \alpha_i$ denotes the set of fixed effects (consistent with the previous analysis). Our coefficient of interest is β_3, and we report double-clustered standard errors by firm and trading day. β_3 is coefficient of interest.

Table 4 reports the results. We find that government procurement exposure explains significant risk premium gains *only* during firms' earnings announcement windows. The coefficient estimates are consistently positive and statistically significant at the 1% level across specifications, including Columns (5) and (6) with firm fixed effects, and remain similar in magnitude. In terms of economic magnitude, the coefficient estimation of 0.5446^{***} indicates that during earnings announcement days, a one standard deviation increase in procurement exposure is associated with a 9.2% increase in unit of annual percents. This risk premium effect arises both across firms with higher procurement exposure and within firms during periods of increasing exposure over time.

[Insert Table 4 here]

Figure 3 illustrates the return results by plotting average daily abnormal returns for

firms in the high- and low-fiscal-exposure groups, defined using the mean cutoff reported in Table A3. Solid bars represent averages on earnings announcement days, while shaded bars represent averages on non-announcement days. We highlight two key observations. In the left panel, the solid bar is substantially taller than the shaded bars, reflecting a pronounced risk premium effect on earnings announcement days. Moreover, consistent with the regression results, the differences between the high- and low–fiscal exposure groups on non-announcement days are statistically indistinguishable.

[Insert Figure 3 here]

5 The Fiscal Uncertainty Mechanism

Under the fiscal risk-based mechanism, loss-averse analysts are expected to underestimate earnings more strongly for firms that are more exposed to government budgetary risk. As outlined in Prediction 3, a key testable implication is that return predictability should intensify with heightened budgetary uncertainty. In this section, we present evidence consistent with this prediction.

Budgetary uncertainty can vary over time across all firms. In addition, its effective impact may also differ across firms: even when facing the same level of aggregate fiscal risk, firms with greater bargaining power vis-à-vis the federal government are likely to experience lower effective budgetary uncertainty. To capture these two distinct dimensions, we construct and analyze two empirical proxies for budgetary uncertainty, each at a different level of granularity.

In Section 5.1, we build on Brogaard, Denes, and Duchin (2021) and construct a firm-level (micro) fiscal uncertainty proxy based on actual contract renegotiation histories. This measure captures firms' bargaining power with the federal government, where a higher renegotiation index indicates stronger bargaining power and, accordingly, lower procurement-based cash flow uncertainty. In Section 5.2, we construct a time-series (macro) fiscal uncertainty proxy that captures fluctuations in federal budgetary uncertainty. We identify periods surrounding debt limit events as episodes of heightened fiscal

uncertainty.

5.1 Micro uncertainty

Bajari and Tadelis (2001) argue that firms continue to face uncertainty regarding *ex post* adaptations even after a procurement contract is signed. Such uncertainties can arise from the firm's side (e.g., design failures, unexpected site or environmental conditions) as well as from the federal government's side (e.g., regulatory changes, budgetary risks). More directly related to our setting, Brogaard, Denes, and Duchin (2021) examine historical procurement patterns,[12] showing that successful contract renegotiations signal stronger bargaining power and political connectedness with the federal government. Consistent with the implications of our model, firms with a robust renegotiation history and thus greater bargaining power should exhibit lower predictability, as analysts perceive them to face less cash flow uncertainty (e.g., in the event of contract modifications or terminations). We test this implication in the next section.

We construct a firm-level "renegotiation index" to enable meaningful comparisons across firms. A key challenge is that renegotiation channels vary depending on the nature of the firm or contract.[13] Following Brogaard, Denes, and Duchin (2021), we rely on three contract-level variables that capture firm bargaining power. First, we calculate the cumulative changes in promised award amounts and create an award increase indicator that equals one if the cumulative amount increases are positive. Second, we compute the cumulative changes in contract end dates and create an award extension indicator that equals one if the cumulative day changes are positive. Third, we construct a weak monitoring indicator that is equal to one if the contract does not require incentive or performance provisions. To aggregate these measures, we apply a (0.4, 0.4, 0.2) weighting scheme to the three indicators, consistent with the greater importance of award increase and award extension, documented in Table 4 of Brogaard, Denes, and Duchin (2021). Our results remain robust under equal weighting. The index is first constructed at the

[12] The authors also rely on `USAspending.gov` as their primary data source.

[13] For instance, military weapons contracts often face strict deadlines and monitoring due to time sensitivities, making renegotiation more likely to occur through changes in the total award amount.

contract level and then averaged across all contracts associated with a given firm to produce the firm-level renegotiation index.

Panel C of Table A3 shows that for the average firm in our sample, 23.8% of contracts have been successfully renegotiated, and all firms exhibit some degree of renegotiation activity (i.e., the minimum value is nonzero). There is substantial cross-firm heterogeneity in the success of attempted renegotiation with the federal government, with rates ranging from 1% to 47%. Figure 4 further illustrates the distribution of renegotiation index values across industries. Median renegotiation success rates display little variation across industries.

[Insert Figure 4 here]

Table 5 reports the heterogeneity analysis by bargaining power. We find that firms with stronger bargaining power vis-à-vis the federal government exhibit significantly lower predictability, as reflected in the negative interaction coefficients. For example, consider two firms with equal procurement obligations: analysts are more likely to under-forecast earnings for the firm with weaker bargaining power because of higher perceived budgetary uncertainty. In terms of economic magnitude, the 5.7224^{***} in Column (1) indicates that a one standard deviation increase in the renegotiation index above the average reduces the procurement coefficient β by approximately -1.09, which is economically sizable as the main coefficient is 1.8460^{***}. This result remains robust when controlling for industry fixed effects (Column (2)) and when using alternative SUE measures.

[Insert Table 5 here]

5.2 Macro uncertainty

At the macro level, we employ empirical proxies that capture time-varying government budgetary uncertainty. We begin with an intuitive event-based measure: the months leading up to a new debt limit. This proxy has the advantage of providing a consistent interpretation across time. The public finance literature emphasizes that

debt limit episodes have historically generated substantial uncertainty in the U.S. economy (see, e.g., Missale (1997), Austin and Levit (2013), Escolano and Escolano (2010), Baker and Yannelis (2017), Xu and You (2025), and Cassidy and Mirani (2025), among many others). As a result, our first proxy is a dummy variable that is equal to one during debt limit event months and the month immediately preceding them (source: https://www.whitehouse.gov/omb/budget/historical-tables/)[14] and zero otherwise.

We validate the uncertainty interpretation of our debt limit event dummy by regressing several risk variables on it. Table 6 reports the results. Column (1) shows that a general measure of fiscal policy uncertainty (FPU), constructed by Baker, Bloom, and Davis (2016), is significantly higher when the debt limit event indicator equals one (t-statistic = 2.45). Figure 5 plots the time series of FPU (green dashed line) with debt limit events highlighted by gray shaded areas. The narratives underlying major FPU spikes reflect both *budgetary* uncertainty tied to debt limit debates (e.g., the 2011 Budget Control Act, the 2013 No Budget, No Pay Act, the 2013 Fiscal Cliff, the late-2013 Obamacare funding debate and government shutdown, and the 2017 hurricane relief debates) and *non-budgetary* uncertainty driven by other economic and political events (e.g., the 2010 midterm elections, the 2015 European debt crisis, the 2016 U.S. presidential election, and the 2019 trade war).

These observations motivate a more relevant uncertainty measure: EPU specifically attributed to debt limit events (source: Baker, Bloom, and Davis (2016), https://www.policyuncertainty.com/categorical_epu.html).[15] According to Figure 5 and Table 6, EPU attributed to debt limits is substantially elevated — 59.8% higher when the debt limit event indicator equals one relative to zero (t-statistic = 2.15). Comparing Column

[14] This website was delinked in early 2025. Although it may be restored in the future, the data used here are archived on the Wayback Machine at: https://web.archive.org/web/20240724222454/https://www.whitehouse.gov/omb/budget/historical-tables/. See Table 7.3 for the relevant information.

[15] According to the EPU website, one category is labeled "fiscal policy," which corresponds to what we refer to as FPU above. The website also provides a series titled "Ratio: EPU w/DebtCeiling to wo/DebtCeiling." The baseline EPU series corresponds to EPU *without* debt ceiling mentions. Hence, given EPU and this ratio, we compute the portion of EPU attributable to debt ceiling mentions in news articles as (ratio − 1) × EPU.

(1) to Column (2) of Table 6, we also observe a notable increase in explanatory power, with the R^2 rising from 6.4% to 14%. The results remain robust to the inclusion of year fixed effects (capturing within-year variation) and quarter fixed effects (controlling for seasonality) as reported in Panels B and C. We note, however, that the R^2 values become less interpretable in the presence of these fixed effects. By contrast, Columns (3)-(6) show that fear and anxiety related to government shutdowns (source: EPU website), market risk aversion (source: www.nancyxu.net), the VIX (source: CBOE), and the 22-day realized variance of stock market returns (source: DataStream and authors' calculations) do not exhibit significant changes during debt limit events. It is nonetheless reassuring that the coefficients are positive, consistent with the expected comovement among risk variables predicted by models such as Martin (2017) and Xu (2019). Taken together, these findings reinforce the interpretation that debt limit events are uniquely associated with heightened fiscal uncertainty.

[Insert Table 6 here]

[Insert Figure 5 here]

Table 7 presents the interaction results based on the firm-quarter panel framework. The specifications here use the same sets of fixed effects as in the earlier analysis to investigate the underlying mechanism. We consider three time-series measures of budgetary uncertainty: (1) an indicator variable for debt ceiling event periods, (2) the percentage change in the statutory debt ceiling, and (3) EPU attributed to debt limits. The first captures the extensive margin, while the latter two measures can be directly interpreted as economic variables and should reflect the intensive margin.

Panel A of Table 7 shows that across all earnings surprise measures (Beat, SUE_1, SUE_2), predictability strengthens significantly during periods of heightened fiscal uncertainty, as proxied by the debt limit event dummy. Comparing the coefficient magnitudes of the main and interaction terms reveals that the interaction effect accounts for roughly half of the total predictability effect. In Panel B, we replace the debt limit dummy variable

with the actual changes in debt limits, thereby capturing the intensive margin of fiscal risk. The interaction coefficients are more statistically significant across all specifications, including those with firm fixed effects.

Panel C of Table 7 employs debt limit uncertainty as a more direct proxy for fiscal uncertainty. This test is crucial for two reasons. First, the measure by Baker, Bloom, and Davis (2016) is constructed independently of our research design, ensuring exogeneity. Second, budgetary uncertainty may rise in public discourse even outside debt limit cycles, thereby providing continuous variation beyond the two months preceding debt limit events.

The results remain strong and largely robust except in the final three columns, which show similar coefficient magnitudes but lose statistical significance. Economically, Columns (1), (4) and (7) indicate that a one standard deviation increase in fiscal budgetary uncertainty significantly enhances predictability in the *cross-firm* margin by approximately 26-30%. In addition, budgetary uncertainty is able to explain almost all statistically significant within-firm variation, which is an intuitive result. As budgetary uncertainty increases, analysts' under-forecasts become more strongly linked to the scale of government obligations to the firm (i.e., how much the government promises to pay via procurement contracts). Taken together, these findings align closely with the predictions of our model.

[Insert Table 7 here]

6 Alternative Explanations

In this section, we examine alternative mechanisms that could also account for the observed predictability: analyst inattention and imperfect information. Under these explanations, predictability should increase when analysts either overlook government contracts (i.e., no attention) or face delays in obligation disclosures (i.e., no information). While both channels are conceptually consistent with *Prediction 4* of our model, we find little supporting evidence.

6.1 Limited analyst attention to government contracts

We expect that the limited-attention channel is unlikely to be the primary driver of our results based on two pieces of evidence. First, if analyst attention to procurement and fiscal risk increases during periods of heightened budgetary uncertainty, as seems likely,then predictability should weaken. This prediction contrasts with the empirical evidence in Section 5.2, which shows stronger predictability during such periods. Second, recent research, including Hassan, Hollander, Van Lent, and Tahoun (2019), employs advanced computational linguistic models to analyze earnings call transcripts and finds that financial analysts are generally attentive to political risks.

We also formally test this alternative channel, and the results do not suggest that variation in analyst attention meaningfully contributes to the predictability patterns documented above. In this analysis, we construct analyst attention measures through a simple textual analysis of earnings call transcripts and incorporate these measures into our empirical framework. Specifically, we obtain full firm-quarter earnings call transcripts (source: Capital IQ) and construct a firm-quarter variable capturing analyst mentions of procurement-related keywords. For each transcript, we identify the total number of words in analyst-spoken paragraphs that reference "government contracts," "procurement contracts," or close variations. We then normalize this count in two ways: (i) by the total number of words in the transcript (excluding operator speech) to create the variable "Analyst_mention1," and (ii) by the total number of words spoken by analysts to create "Analyst_mention2." These two measures capture different aspects of attention: the first reflects procurement-related discussion relative to the entire call, while the second reflects it relative to analyst speech specifically.

Figure 6 shows a significant positive relationship between executive mentions of government contracts and analyst mentions at the firm level. The left (right) panel corresponds to Analyst_mention1 (Analyst_mention2). This finding suggests that discussions of government contracts are actively initiated and sustained by both executives and analysts, and that analyst attention to these topics responds systematically to the

information conveyed by firm management.

Table 8 tests whether variation in analyst attention to government contracts mediates the observed predictability. We find no supporting evidence: firms with more analyst mentions of government contracts do not exhibit lower predictability. These results are robust across both analyst attention measures. Additional panel-level evidence with various fixed effects, as implemented before, is presented in Internet Appendix Table IB.10, where the findings remain similarly insignificant.

[Insert Figure 6 here]

[Insert Table 8 here]

6.2 Information delay

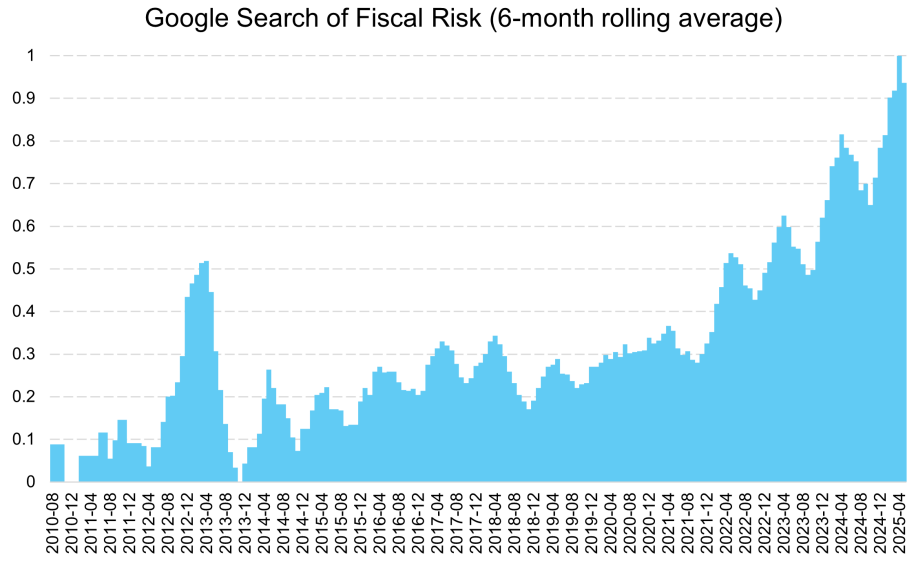
It is empirically challenging to test whether obligation information is substantially delayed, as `USAspending.gov` does not report the posting dates of individual obligation transactions. However, the two scraping exercises (10/1/2023–1/18/2024 and 8/8/2024–11/5/2024) discussed earlier in Section 3.2 yield consistent delay statistics. We find that most agencies release obligation transactions to the public within a reasonable time frame (typically within 30 days).

7 Conclusion

We construct a detailed obligation-transaction-level dataset of federal government procurement contracts from 2009 to 2019 and link them to 474 public firms. Using this comprehensive database together with earnings surprises and stock returns, we show that procurement contracts provide a channel through which fiscal uncertainty transmits to financial markets. Analysts systematically under-forecast obligations promised by the federal government, with substantial variation both across firms and within firms over time. The effect is more pronounced for firms with weaker bargaining power (micro uncertainty) and during periods of heightened fiscal risk (macro uncertainty). Moreover,

excess stock returns increase with procurement exposure only around earnings announcements, consistent with a risk premium channel. A one standard deviation increase in procurement exposure corresponds to a 9.2% annualized increase in excess returns.

While government spending is often regarded as a stabilizing force for economic growth, recurring debt limit deadlines and broader budgetary uncertainty rooted in U.S. political dynamics generate significant instability spilling over from the political realm into financial markets. Our results document this transmission through capital markets, showing that revenues from procurement contracts are perceived not as a safe growth anchor but as a source of "bad" uncertainty. As fiscal risk draws increasing public attention,[16] our findings highlight the importance of further research into how fiscal uncertainty propagates into financial markets and shapes corporate outcomes.



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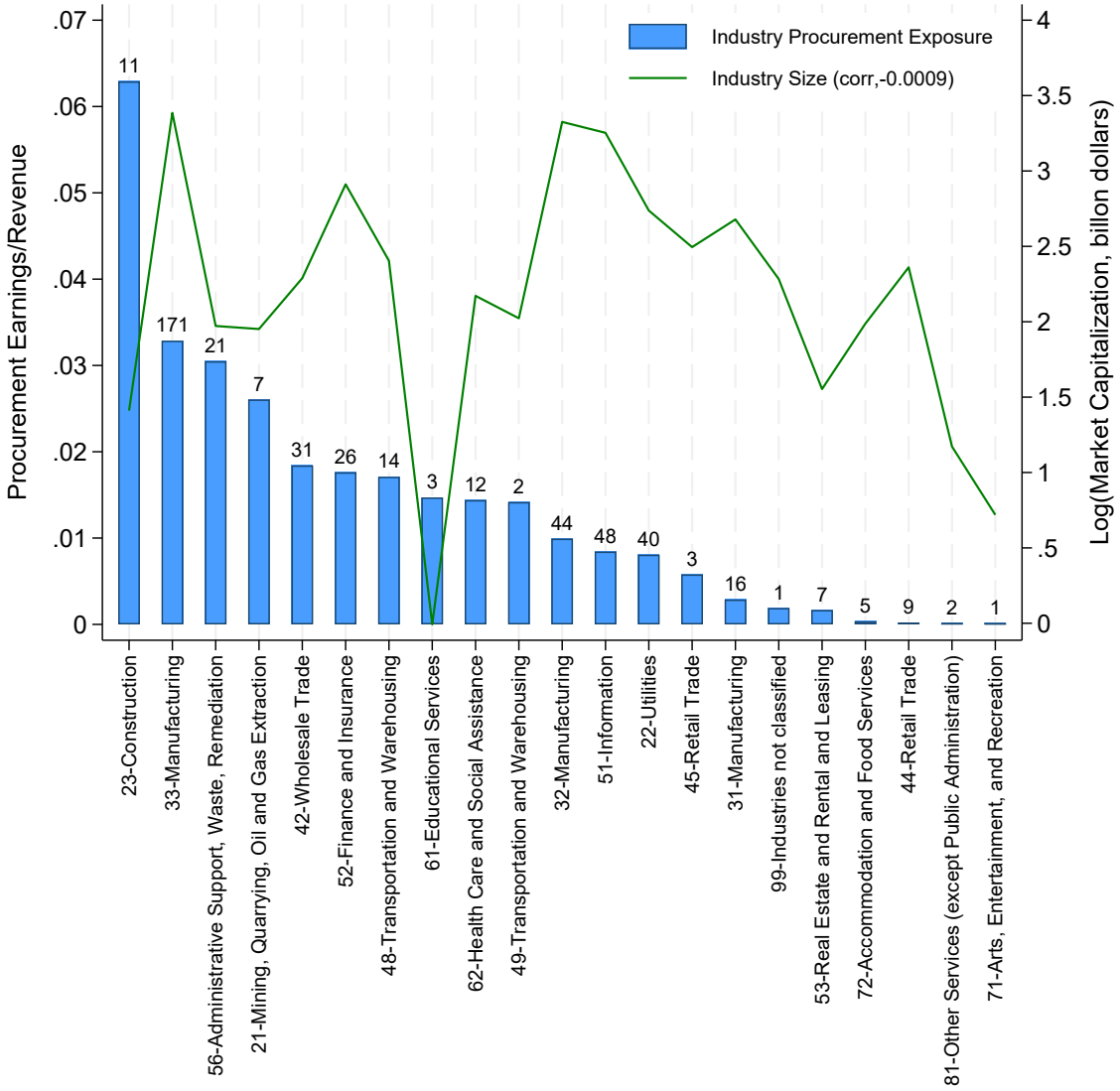


Figure 1: **Firm Sample Description.** This figure describes our firm sample: (1) the number on top of each bar represents the number of firms in each NAICS 2-digit industry classification (N=474); (2) the bar denotes the average firm-quarter Procurement$_{i,t}$ for each industry, which is calculated as total transaction obligated amount scaled by average quarterly revenues in the past 4 quarters; (3) the solid line denotes the logarithm of total market capitalization (in billions of dollars) of each industry represented in our firm sample. The x-axis denotes the industry classification; the left y-axis corresponds to (2), and the right y-axis corresponds to (3). Figure IB.1 in the Internet Appendix also shows where the largest 25% firm-quarter transactions sit.

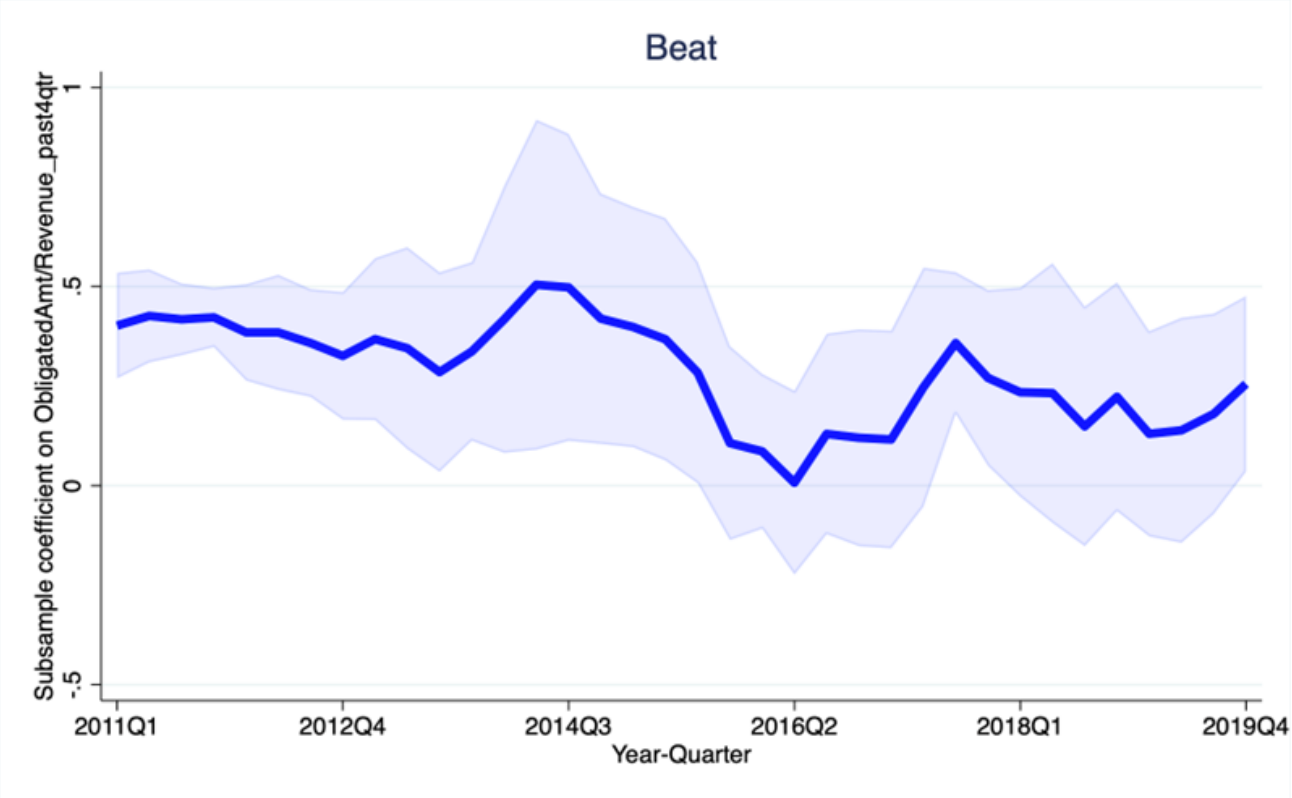


Figure 2: **Rolling Coefficient of the Main Predictive Result.**
This figure depicts the time series of the rolling coefficients of Procurement in regressions with control variables as shown in Table 2. Each regression uses a rolling window of 8 quarters. Robust standard errors are clustered at the firm and calendar year-quarter level. Shaded areas indicate 90% confidence intervals.

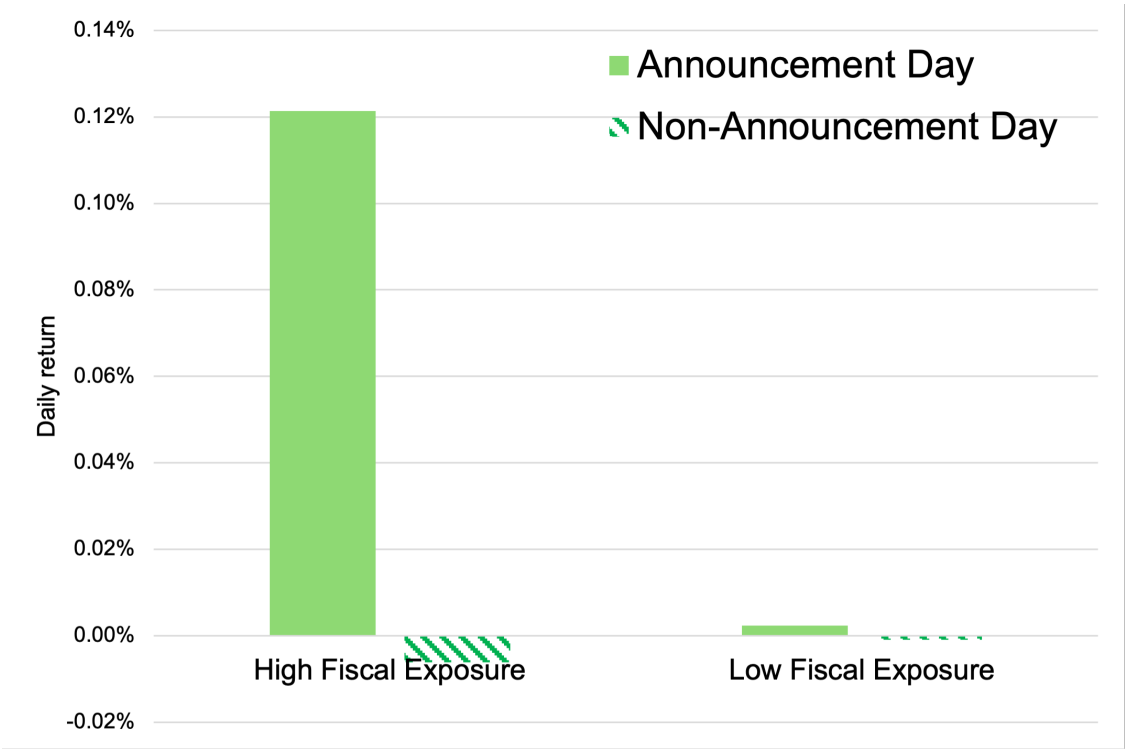


Figure 3: **Announcement vs. Non-Announcement Day.**
This figure demonstrates average logarithm of abnormal daily returns in four bins: (high fiscal exposure, low fiscal exposure) x (during announcement periods [-1,1], outside announcement periods). Logarithm of abnormal daily returns is computed as the difference between the daily logarithmic stock return and the CRSP daily logarithmic value-weighted market return (including distributions). Fiscal exposure is the Procurement variable used as our predictor throughout the paper, and we use its mean (0.02) as the cutoff to separate firm-quarters in high versus low fiscal exposure. This figure demonstrates Table 4 (which uses continuous Procurement measures) in a simple way.

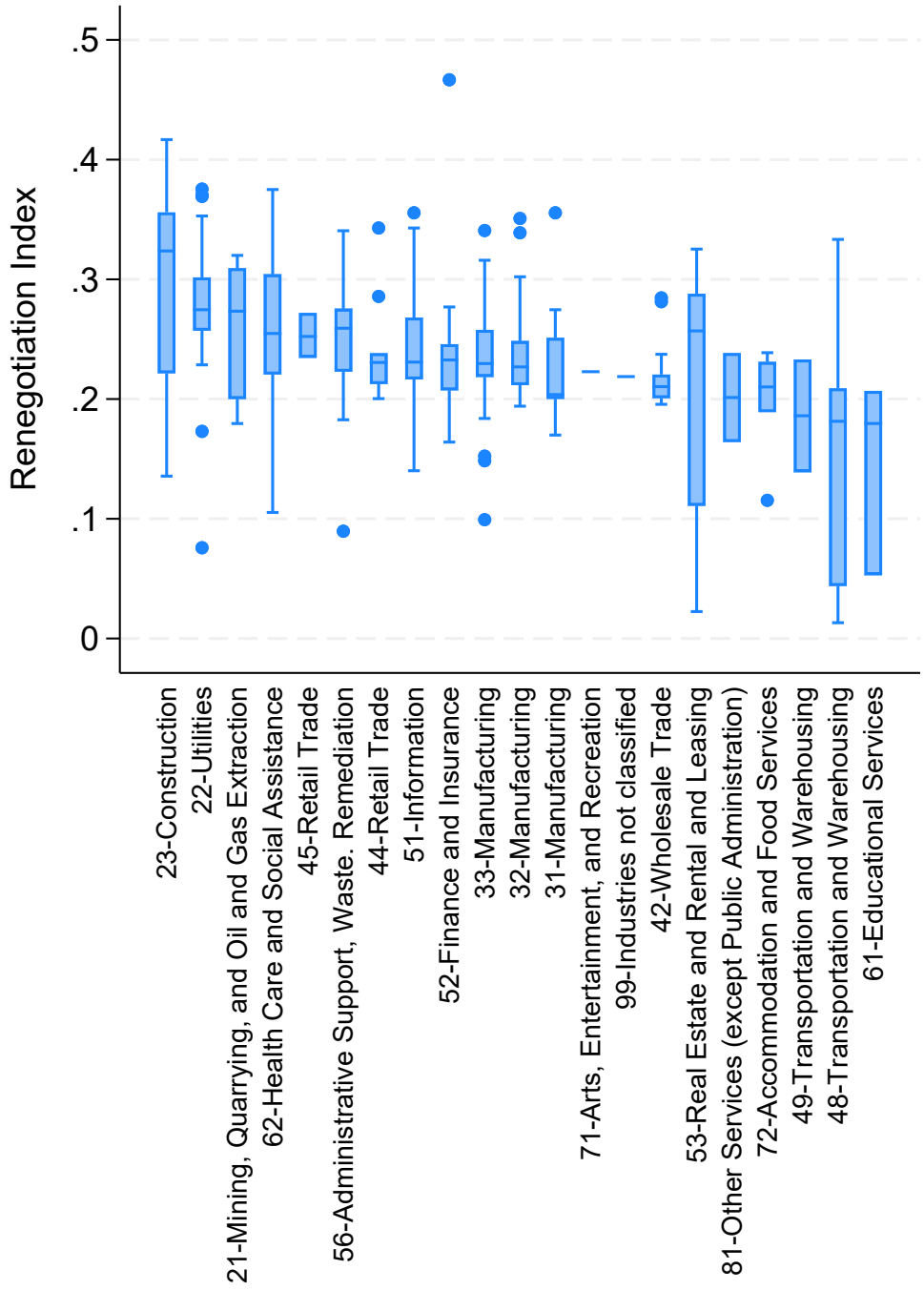


Figure 4: **Renegotiation Index, Illustrated by Industry.** This is the box plot of the firm renegotiation index within each NAICS 2-digit industry.

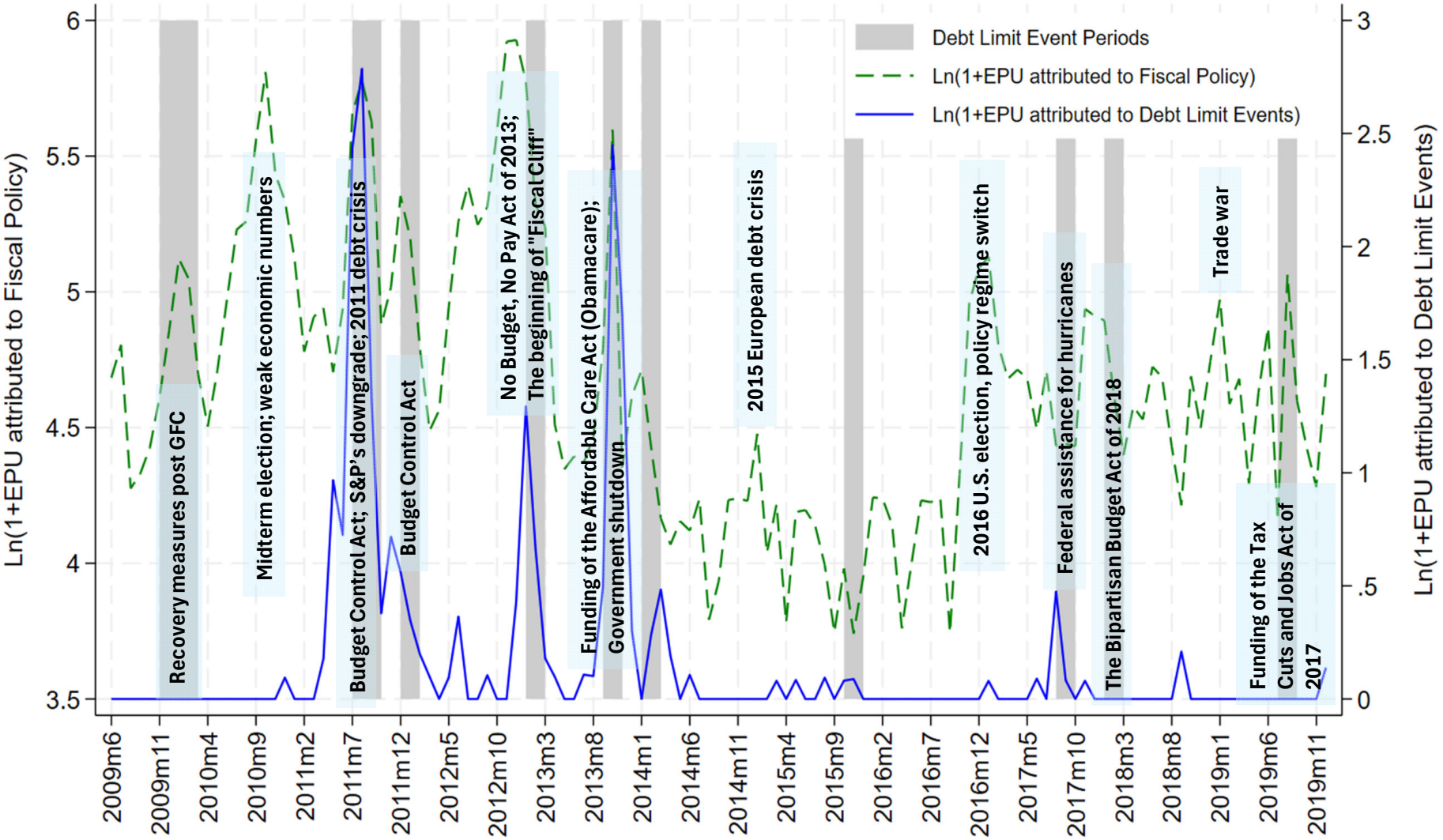
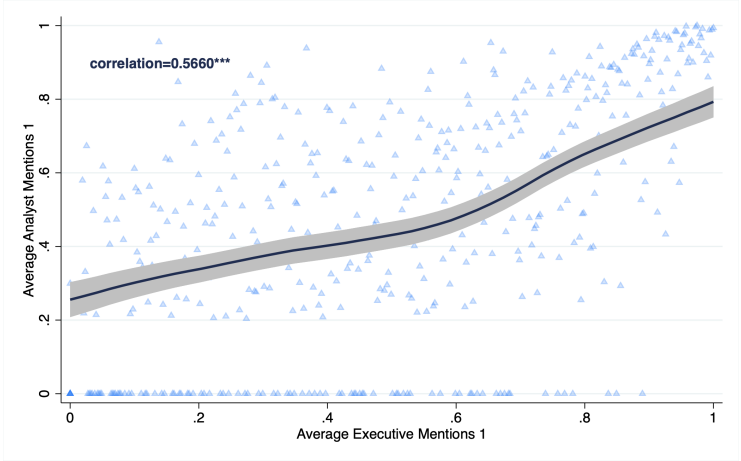
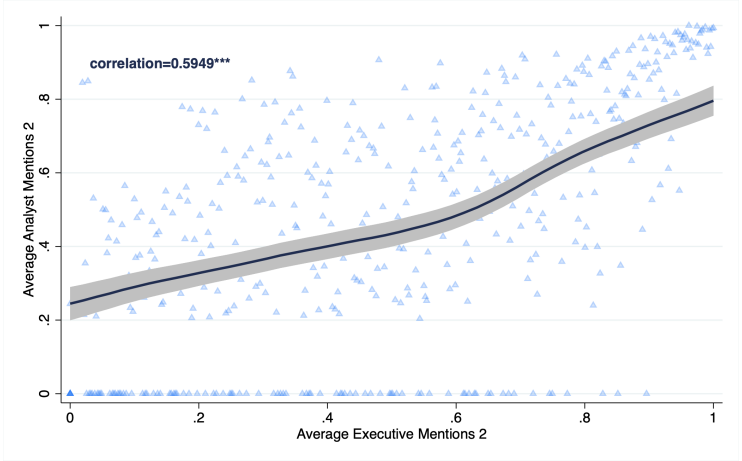


Figure 5: **The Fiscal Uncertainty Interpretation of Debt Ceiling Events.**
This figure illustrates Table 6 in a more direct way. The shaded area indicates the month of, and the month prior to, U.S. debt ceiling events; the events were obtained from https://www.whitehouse.gov/omb/budget/historical-tables/.



A. Analyst and executive mentions scaled by total number of words in the transcript excluding operator words.



B. Analyst and executive mentions scaled by total number of words in the transcript by analysts and executives, respectively.

Figure 6: **Earnings Call Transcripts: How Often Do Analysts and Executives Mention Government-Contract-Related Words?**

This figure demonstrates that analysts' and executives' mentions of government contracts in earnings calls are strongly and positively correlated. Specifically, for each earnings call transcript (firm-time level), we first construct two measures of analyst (executive) mentions of government: (A) the number of words in paragraphs spoken by analysts (executives) that mention "government contracts" or "procurement contracts" divided by the total number of words in the transcript excluding operator words, (B) and that number divided by the total number of words in the transcript spoken by analysts (executives). For demonstration purposes, this figure depicts the percentile ranks of firm-level averages. The solid line and shaded band indicate a local prediction and the 95% confidence interval. The correlations using raw analyst and executive averages (without percentile ranks) are 0.67 and 0.74 for (A) and (B), respectively.

Table 1: **Procurement Transactions and Earnings Beat: Cross-Firm Evidence.**

$$\overline{\text{Beat}}_i = \alpha_{d(i)} + \beta\ \overline{\text{Procurement}}_i + \boldsymbol{\delta}\overline{\boldsymbol{X}}_{\boldsymbol{i}} + \varepsilon_i,$$

where i denotes a firm and the bar above variable z, $\overline{z}$, denotes the average. Details on control variables are introduced in Section 4.1 and summarized in the Internet Appendix Table IB.1. This table complements Table 2 by collapsing variables into the firm level using the full sample, 2009-2019. Detailed regression results with controls using the full sample and (mostly) equally-spaced subsamples, 2009-2012, 2013-2016, and 2017-2019, are relegated to Appendix Table IB.2. Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

NAICS2 FE		Yes		Yes
	(1)	(2)	(3)	(4)
Dependent variable:	Beat	Beat	Beat	Beat
Procurement	0.2428	0.2459*	0.3522***	0.3181**
	(0.1513)	(0.1431)	(0.1312)	(0.1249)
Log(1+MarketCap)			0.0415***	0.0416***
			(0.0052)	(0.0054)
Log(1+Book-to-Market)			-0.0851***	-0.0181
			(0.0307)	(0.0361)
Log(1+Ret_m61tom12)			0.9963***	0.9380**
			(0.3838)	(0.3998)
Log(1+Ret_m6tom2)			1.9769**	1.0859
			(0.8128)	(0.8417)
Log(1+InstitutionOwnPct)			0.4389***	0.3691***
			(0.0699)	(0.0778)
Log(1+IVOL_m11tom2)			7.4059***	4.8425**
			(1.9031)	(2.0971)
Log(1+TOV_m61tom12)			-5.3240**	-4.4748*
			(2.5714)	(2.6681)
L.Beat	0.6552***	0.6558***	-0.5962***	-0.5448***
	(0.0075)	(0.0073)	(0.1440)	(0.1520)
Constant	0.6552***	0.6558***	-0.5962***	-0.5448***
	(0.0075)	(0.0073)	(0.1440)	(0.1520)
N	474	472	474	472
R^2	0.0072	0.11	0.25	0.30

Table 2: **Procurement Transactions and Earnings Beat: Pooling Evidence.**
This table shows the main earnings surprise regression results using the panel. The unit of observation is at the firm-quarter level. The specification is also discussed in Equation (12) or here:

$$\text{Beat}_{i,t} = \gamma_t \times \alpha_{d(i)} + \beta \text{ Procurement}_{i,t} + \boldsymbol{\delta X_{i,t}} + \varepsilon_{i,t},$$

where i denotes a firm and t denotes a quarter. $\text{Beat}_{i,t}$ compares firm i's actual earnings during quarter t and the I/B/E/S consensus forecast immediately prior to the earnings announcement (which happens typically some time in quarter $t+1$). $\text{Beat}_{i,t}$ equals 1 if actual beats forecast median, and 0 otherwise. $\text{Procurement}_{i,t}$ is the (obligated) transaction amount from procurement contracts a firm i receives from the government during quarter t, scaled by the firm's past 4 quarter revenue. $\boldsymbol{X_{i,t}}$ denote a series of control variables that are commonly used in the literature. γ_t ($\alpha_{d(i)}$) indicates quarter (industry) fixed effects. The pooling regression coefficient is essentially a different way to average out the time-varying cross-section estimates. Details on control variables are introduced in Section 4.1 or summarized in the Internet Appendix Table IB.1. Standard errors for Columns (1)-(5) are double-clustered at the firm and quarter levels and are reported in parentheses. Column (6) is double-clustered at the NAICS and quarter levels. Regression results without controls are relegated to Appendix Table IB.3. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE:		Yes		Yes		Yes
NAICS2 FE:			Yes	Yes		Yes
NAICS2 x Quarter FE:					Yes	
Unit of observation:	Firm-Quarter	Firm-Quarter	Firm-Quarter	Firm-Quarter	Firm-Quarter	NAICS2-Quarter
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:				Beat		
Procurement	0.3074***	0.2983***	0.2860***	0.2752***	0.2693***	0.4221
	(0.0669)	(0.0648)	(0.0665)	(0.0638)	(0.0657)	(0.7636)
Log(1+MarketCap)	0.0232***	0.0236***	0.0269***	0.0272***	0.0272***	0.0056
	(0.0036)	(0.0036)	(0.0037)	(0.0038)	(0.0038)	(0.0317)
Log(1+Book-to-Market)	-0.0793***	-0.0752***	-0.0200	-0.0131	-0.0159	0.0191
	(0.0249)	(0.0265)	(0.0248)	(0.0267)	(0.0281)	(0.1794)
Log(1+Ret_m61tom12)	0.1773***	0.2113***	0.1682***	0.2059***	0.1878***	0.2464*
	(0.0405)	(0.0365)	(0.0410)	(0.0371)	(0.0365)	(0.1409)
Log(1+Ret_m6tom2)	0.6327***	0.6079***	0.6027***	0.5771***	0.5858***	0.7893*
	(0.1040)	(0.1053)	(0.1023)	(0.1047)	(0.1170)	(0.4281)
Log(1+InstitutionOwnPct)	0.2249***	0.2584***	0.1671**	0.1978***	0.1924***	0.5059*
	(0.0586)	(0.0573)	(0.0619)	(0.0603)	(0.0628)	(0.2527)
Log(1+IVOL_m11tom2)	0.2834	0.1834	-0.1873	-0.3836	-0.3237	-3.4837
	(0.6055)	(0.6003)	(0.5693)	(0.5337)	(0.5666)	(2.8030)
Log(1+TOV_m61tom12)	0.5006	-0.3025	0.4217	-0.3888	-0.4311	1.2067
	(1.1470)	(1.1509)	(1.2284)	(1.2030)	(1.2324)	(6.6003)
L.Beat	0.1581***	0.1533***	0.1504***	0.1454***	0.1498***	-0.0278
	(0.0105)	(0.0106)	(0.0107)	(0.0107)	(0.0114)	(0.0568)
Constant	-0.0827	-0.1021	-0.1407	-0.1555	-0.1524	0.2497
	(0.0932)	(0.0941)	(0.0941)	(0.0960)	(0.0964)	(0.8846)
N	16696	16696	16696	16696	16622	824
R^2	0.048	0.056	0.055	0.063	0.11	0.19

Table 3: **Procurement Transactions and Earnings Beat: Within-Firm Evidence.** This table shows the main earnings surprise regression results using the panel. The unit of observation is at the firm-quarter level. The specification is also discussed in Equation (12) or here:

$$\text{Beat}_{i,t} = \gamma_t + \alpha_i + \beta \text{ Procurement}_{i,t} + \boldsymbol{\delta X_{i,t}} + \varepsilon_{i,t},$$

where α_i indicates firm fixed effects. Panel A shows the main sample; Columns (1)-(5) repeat those in Table 2 and Columns (6)-(7) include firm fixed effects. Panel B repeats Panel A on the specifications and focuses on a firm subsample that has more than 80% of quarters with non-zero obligation amounts; for convenience, we call this firm sample the Intensive Margin sample. Internet Appendix Table IB.4 also shows coefficient estimates of the control variables. Standard errors for all columns are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE		Yes		Yes			Yes
NAICS2 FE			Yes	Yes			
NAICS2 x Quarter FE					Yes		
Firm FE						Yes	Yes
With Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main sample							
Dependent variable:				Beat			
Procurement	0.3074***	0.2983***	0.2860***	0.2752***	0.2693***	0.1732	0.1489
	(0.0669)	(0.0648)	(0.0665)	(0.0638)	(0.0657)	(0.1041)	(0.1052)
Constant	-0.0827	-0.1021	-0.1407	-0.1555	-0.1524	0.6109*	0.9583**
	(0.0932)	(0.0941)	(0.0941)	(0.0960)	(0.0964)	(0.3199)	(0.3607)
N	16696	16696	16696	16696	16622	16696	16696
R^2	0.048	0.056	0.055	0.063	0.11	0.12	0.13
Panel B: Sample using firms that have transactions during almost all quarters							
Dependent variable:				Beat			
Procurement	0.2534***	0.2375***	0.2417***	0.2237***	0.2177***	0.2174**	0.1719**
	(0.0623)	(0.0619)	(0.0602)	(0.0589)	(0.0598)	(0.0827)	(0.0832)
Constant	-0.0272	-0.0728	-0.0308	-0.0688	-0.0711	0.8523**	1.2816***
	(0.1279)	(0.1295)	(0.1320)	(0.1383)	(0.1389)	(0.3283)	(0.4268)
N	12046	12046	12046	12046	11941	12046	12046
R^2	0.046	0.055	0.054	0.064	0.12	0.12	0.13

Table 4: **Announcement vs. Non-Announcement Day Excess Stock Returns.**
This table examines the impact of fiscal exposure on excess stock returns around earnings announcement days.

$$eRet_{i,\tau} = \gamma_{t(\tau)} \times \alpha_i + \beta_1 \text{Procurement}_{i,t(\tau)-1} + \beta_2 \text{I}_{i,ann.} + \beta_3 \text{I}_{i,ann.} \text{Procurement}_{i,t(\tau)-1} + \varepsilon_{i,\tau},$$

where $eRet_{i,\tau}$ represents the logarithm of abnormal returns for stock i on trading day τ, computed as the difference between the daily logarithmic stock return and the CRSP daily logarithmic value-weighted market return (including distributions). This dependent variable effectively controls for market-wide movements. $\text{Procurement}_{i,t(\tau)-1}$ reflects procurement exposure from the prior fiscal quarter, where $t(\cdot)$ maps the trading day to its corresponding fiscal quarter. $\text{I}_{i,ann.}$ is an indicator variable for the earnings announcement window, defined as the [-1, +1] trading days around the earnings announcement date (Day 0) for the previous quarter. $\gamma_{t(\tau)} \times \alpha_i$ represents various sets of fixed effects with particular emphasis on firm fixed effects α_i as in previous analysis. The coefficient of interest is β_3. Returns are in units of percentage. Standard errors are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Firm FE					Yes	Yes
NAICS2 FE		Yes	Yes			
Year-Calendar Quarter FE	Yes		Yes			Yes
NAICS2 x Quarter FE				Yes		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:			Log Excess Return (%)			
Procurement	-0.0351	-0.0058	-0.0219	-0.0237	0.0125	-0.0471
	(0.0402)	(0.0398)	(0.0367)	(0.0367)	(0.0898)	(0.0739)
Dummy(In Announcement Day -1 to 1 Window)	0.0106	0.0108	0.0106	0.0106	0.0108	0.0106
	(0.0290)	(0.0290)	(0.0290)	(0.0290)	(0.0290)	(0.0291)
Procurement × Dummy(In Announcement Day -1 to 1 Window)	0.5446***	0.5438***	0.5446***	0.5452***	0.5437***	0.5445***
	(0.1515)	(0.1513)	(0.1515)	(0.1512)	(0.1513)	(0.1523)
Constant	-0.0010	-0.0016	-0.0012	-0.0012	-0.0020	-0.0007
	(0.0015)	(0.0052)	(0.0015)	(0.0015)	(0.0047)	(0.0025)
N	1030270	1030270	1030270	1030270	1030270	1030270
R^2	0.00043	0.000071	0.00047	0.0020	0.00048	0.00088

Table 5: **Mechanism Test: Renegotiation and Bargaining Power with Government, Micro.**
This table examines whether firms' bargaining power can help explain variation in predictability across firms. For each contract, we first construct three measures of renegotiation level following Brogaard, Denes, and Duchin (2021): (A) an award increase indicator that equals one if the cumulative change in a potential award amount is greater than zero, (B) an award extension indicator that equals one if the cumulative change in the contract end date is greater than zero days, and (C) a weak monitoring indicator that equals one if the contract lacks incentive or performance features. We average these three indicator variables within each firm, and further construct the firm-level renegotiation index by summing the three variables with weights of (0.4, 0.4, 0.2). Detailed regression results for Columns (1)-(2) as well as alternative renegotiation index construction are relegated to Appendix Table IB.5. Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

NAICS2 FE		Yes		Yes		Yes
With controls:	Yes	Yes	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Beat	Beat	SUE_1	SUE_1	SUE_2	SUE_2
Procurement	1.8460***	1.8182***	13.4117	12.9891	3.2343**	2.8713***
	(0.6992)	(0.6304)	(10.0373)	(8.9920)	(1.4733)	(1.0401)
Renegotiation Index	-0.1135	-0.1372	-0.1878	-0.3941	-0.0757	-0.0807
	(0.1398)	(0.1697)	(1.0544)	(1.1776)	(0.1729)	(0.1765)
Procurement × Renegotiation Index	-5.7224**	-5.7493**	-36.3102	-34.8091	-11.5017*	-9.8632**
	(2.8137)	(2.5154)	(37.0289)	(32.1748)	(5.9320)	(4.0963)
N	473	471	473	471	473	471
R^2	0.26	0.31	0.19	0.25	0.070	0.16

Table 6: **Economic Interpretations of Debt Limit Events.**
This table provides economic interpretations of debt limit events using time-series regressions and various monthly asset pricing variables. The right-hand-side variable equals one for the debt ceiling event month and the previous month, and equals zero otherwise. Figure 5 shows that debt ceiling events are frequent, typically occurring once a year since 2009. The dependent variables in Columns (1)-(3) are directly downloaded or constructed from Baker, Bloom, and Davis (2016)'s Economic Policy Uncertainty variables and should capture perceived fundamental uncertainty related to fiscal policy, the debt ceiling, and government shutdown, where EPU Attributed to Debt Ceiling $= (\frac{\text{EPU with debt ceiling}}{\text{EPU without debt ceiling}} - 1) \times \text{EPU}$, *and* EPU Attributed to Government Shutdown $= (\frac{\text{EPU with government shutdown}}{\text{EPU without government shutdown}} - 1) \times \text{EPU}$. Columns (4)-(6) capture stock market risk and uncertainty according to the literature, such as Bekaert, Engstrom, and Xu (2022)'s risk aversion index (source: www.nancyxu.net), VIX (source: FRED/CBOE), and 22-day realized volatility, the square root of the sum of the daily return-squared within the same month as commonly constructed in the literature (source: authors' calculation; daily S&P500 returns obtained from the DataStream) (the unit is the same as for VIX, i.e., the annual volatility percent for comparison purpose). Panels B and C use the same specifications with year and quarter fixed effects, respectively. Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	EPU Fiscal Policy	EPU Attributed to Debt Ceiling	EPU Attributed to Government Shutdown	Risk Aversion	VIX	RV
Panel A. Without any fixed effects.						
is_debtlimit	46.1706**	1.9634**	1.1277	0.1230	2.1909	2.8876
	(18.8417)	(0.9147)	(1.3039)	(0.1364)	(1.5880)	(1.9847)
Constant	112.2147***	0.1302**	0.3631**	2.8884***	17.1709***	13.1406***
	(6.3201)	(0.0506)	(0.1550)	(0.0325)	(0.5154)	(0.5949)
N	127	127	127	127	127	127
R^2	0.064	0.14	0.021	0.014	0.022	0.027
Panel B. With year fixed effects.						
is_debtlimit	40.7374***	1.6178**	0.7741	0.0642	1.3420	2.1365
	(13.7480)	(0.7354)	(1.1127)	(0.1029)	(1.0706)	(1.6032)
Constant	113.1987***	0.1928**	0.4272**	2.8990***	17.3246***	13.2767***
	(4.5527)	(0.0907)	(0.1809)	(0.0223)	(0.3765)	(0.5477)
N	127	127	127	127	127	127
R^2	0.52	0.30	0.16	0.50	0.55	0.32
Panel C. With quarter fixed effects.						
is_debtlimit	41.3638**	2.0295**	1.1937	0.1353	2.2050	3.0365
	(20.1895)	(0.9267)	(1.3954)	(0.1352)	(1.6070)	(2.0362)
Constant	113.0852***	0.1183*	0.3512**	2.8862***	17.1683***	13.1137***
	(6.3100)	(0.0615)	(0.1660)	(0.0316)	(0.5098)	(0.6020)
N	127	127	127	127	127	127
R^2	0.10	0.16	0.061	0.052	0.043	0.033

Table 7: **Mechanism Test: Budgetary Uncertainty Triggered by Debt Ceiling Events.**
This table shows interaction results using the three dependent variables. Panel A adds an interaction term with an indicator variable that equals one if a firm-quarter ends in a debt limit event month or the month prior (source: whitehouse.gov) and is zero otherwise. Panel B adds an interaction term with the percentage change in the debt ceiling levels if a firm-quarter ends in a debt limit event month or the month prior and is zero otherwise. Panel C adds an interaction term with the monthly average EPU attributed to debt ceiling mentions in the news article. EPU Attributed to Debt Ceiling $= (\frac{\text{EPU with debt ceiling}}{\text{EPU without debt ceiling}} - 1) \times \text{EPU}$, where the right-hand-side EPU variables are Baker, Bloom, and Davis (2016)'s Economic Policy Uncertainty variables, which are directly downloadable from https://www.policyuncertainty.com/categorical_epu.html. Detailed regression results of the three panels are relegated to Appendix Tables IB.7, IB.8, and IB.9, respectively. Standard errors are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE			Yes			Yes			Yes
NAICS2 FE									
NAICS2 times Quarter FE	Yes			Yes			Yes		
Firm FE		Yes	Yes		Yes	Yes		Yes	Yes
With Controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Measure with extensive margin: An indicator variable for the debt ceiling event period.									
Dependent variable:	Beat	Beat	Beat	SUE_1	SUE_1	SUE_1	SUE_2	SUE_2	SUE_2
Procurement	0.2591***	0.1380	0.1139	2.3031**	0.7296	0.4370	0.1773	-0.0960	-0.1213
	(0.0734)	(0.1002)	(0.1020)	(0.9392)	(1.0804)	(1.0911)	(0.1131)	(0.2334)	(0.2278)
is_debtlimit	-0.0072	-0.0125	-0.0112	0.0100	-0.1598*	-0.0549	-0.0079	-0.0231*	-0.0107
	(0.0267)	(0.0120)	(0.0246)	(0.3079)	(0.0918)	(0.2403)	(0.0209)	(0.0133)	(0.0190)
Procurement × is_debtlimit	0.0621	0.1654***	0.1488***	1.8357**	2.4602***	2.4471***	0.2015**	0.2692**	0.2453**
	(0.0773)	(0.0371)	(0.0438)	(0.7773)	(0.8204)	(0.8500)	(0.0906)	(0.1111)	(0.1002)
N	16622	16696	16696	16218	16297	16297	16316	16390	16390
R^2	0.11	0.12	0.13	0.12	0.15	0.16	0.077	0.084	0.091

Year-Calendar Quarter FE			Yes			Yes			Yes
NAICS2 FE									
NAICS2 times Quarter FE	Yes			Yes			Yes		
Firm FE		Yes	Yes		Yes	Yes		Yes	Yes
With Controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel B. Measure with intensive margin: Percent changes in the actual debt ceiling levels.									
Dependent variable:	Beat	Beat	Beat	SUE_1	SUE_1	SUE_1	SUE_2	SUE_2	SUE_2
Procurement	0.2562***	0.1450	0.1218	2.4243**	0.9555	0.7110	0.1850*	-0.0746	-0.0977
	(0.0705)	(0.1021)	(0.1035)	(0.9268)	(1.1093)	(1.1337)	(0.1074)	(0.2263)	(0.2217)
% Changes in debt ceiling levels	0.0009	-0.0003	0.0010	0.0291	0.0030	0.0225	0.0012	-0.0019	0.0021
	(0.0020)	(0.0023)	(0.0020)	(0.0252)	(0.0246)	(0.0223)	(0.0021)	(0.0028)	(0.0020)
Procurement × % Changes in debt ceiling levels	0.0167**	0.0266***	0.0261***	0.2321***	0.2897***	0.2935***	0.0315**	0.0329*	0.0316*
	(0.0075)	(0.0073)	(0.0077)	(0.0732)	(0.0894)	(0.1001)	(0.0152)	(0.0180)	(0.0175)
N	16622	16696	16696	16218	16297	16297	16316	16390	16390
R^2	0.11	0.12	0.13	0.12	0.15	0.16	0.077	0.084	0.091
Panel C. Measure with intensive margin: Monthly Average EPU attributed to debt limit.									
Dependent variable:	Beat	Beat	Beat	SUE_1	SUE_1	SUE_1	SUE_2	SUE_2	SUE_2
Procurement	0.2471***	0.1356	0.1154	2.3558**	0.7866	0.5349	0.1940*	-0.0767	-0.0956
	(0.0708)	(0.1042)	(0.1068)	(0.9678)	(1.1033)	(1.1297)	(0.1037)	(0.2279)	(0.2240)
Monthly Average EPU Attributed to Debt Ceiling	-0.0051	-0.0022	-0.0010	0.0926	-0.0354	0.0993	-0.0153	-0.0012	-0.0144
	(0.0059)	(0.0027)	(0.0096)	(0.0678)	(0.0279)	(0.0641)	(0.0148)	(0.0034)	(0.0174)
Procurement × Monthly Average EPU Attributed to Debt Ceiling	0.0274***	0.0306***	0.0256***	0.2995***	0.4000***	0.3543***	0.0202	0.0293	0.0254
	(0.0079)	(0.0074)	(0.0061)	(0.0961)	(0.0947)	(0.0966)	(0.0167)	(0.0217)	(0.0222)
N	16622	16696	16696	16218	16297	16297	16316	16390	16390
R^2	0.11	0.12	0.13	0.12	0.15	0.16	0.077	0.084	0.091

Table 8: **Mechanism Test: Lack of Analyst Attention to Government Contracts.**
This table shows whether the cross-firm variation in predictability (from the previous tables) can be explained by analyst attention to firms' government contract exposure. Specifically, we construct 2 firm-quarter measures using detailed earnings call transcripts. For each transcript (firm-time level), we first construct two measures of analyst mentions of government: (A) the number of words spoken by analysts that mention "government contracts" or "procurement contracts" divided by the total number of words in the transcript excluding operator words and (B) analysts' procurement-related words divided by the total number of words spoken by analysts. Then, for each firm, Analyst_measure1 is the average of (A) and Analyst_measure2 is the average of (B). Results at the firm-quarter level with controls are relegated to Appendix Table IB.10. Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

NAICS2 FE:		Yes		Yes
With Controls:	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)
Dependent variable:		Beat		
Procurement	0.3356**	0.3420**	0.3473**	0.3687**
	(0.1673)	(0.1651)	(0.1762)	(0.1729)
Analyst_mention1	1.3365	1.5575		
	(3.2404)	(3.0449)		
Procurement × Analyst_mention1	-2.6421	-10.7303		
	(31.6769)	(31.0956)		
Analyst_mention2			0.3599	0.4666
			(0.5492)	(0.5331)
Procurement × Analyst_mention2			-1.3802	-3.3823
			(5.5642)	(5.5365)
N	472	471	472	471
R^2	0.25	0.30	0.25	0.30

Paper Appendices

A Detailed Proof of the Model Discussed in Section 2

(a). Notation.

We first clarify time stamps in the model. Time stamp t always denotes when events arrive. In our context, the actual earnings of the last period $t-1$ are announced at time t, so a firm's actual earnings in period $t-1$ are denoted as $X_{t(t-1)}$ or X_t for simplicity in the rest of the model. The analyst earnings forecast has information set $t-1$ but median forecasts are elicited at time t, so the analyst forecast of a firm's cash flow in period $t-1$ is denoted as $X^F_{t(t-1)}$, X^F_t for simplicity in the rest of the model. Without the loss of generality, we ignore firm indicator i for brevity.

(b). The analyst problem.

Analysts solve the following minimization problem:

$$\min_{X^F_t} \mathbb{E}_{t-1}\left[(X_t - X^F_t)^2 + 48\lambda \mathbf{1}_{X^F_t > X_t} \frac{(X^F_t - X_t)^2}{(X^F_t - min(X_t))^2}\right], \tag{A1}$$

where λ (> 1) captures the loss aversion of investors/clients. $X_t - X^F_t$ denotes realized earnings surprise. $\frac{48}{(X^F_t - min(X_t))^2}$ are scaling parameters in order to obtain a closed-form solution under uniform distributed shock assumptions.

(c). The data generating process for a closed-form solution.

The actual earnings of period $t-1$ announced at time t, $X_{t(t-1)}$ or X_t, which is a flow variable, consist of two components: earnings made by retail sales R_t and earnings paid by the government from existing procurement contracts G_t,

$$X_t = R_t + \kappa G_t, \tag{A2}$$

where κ (which would have a superscript i) measures the fiscal dependence of the firm. In the longer term, $\frac{\kappa \bar{G}}{R + \kappa \bar{G}}$ corresponds to fiscal dependence, which is the measure we use in our empirical section.

For simplicity, we assume that analysts can collect sufficient information about retail sales and can form rational expectations about $R_{t(t-1)}$ or R_t with uncertainty

following a uniform distribution,

$$R_t = \bar{R} + \eta_t, \text{ where } \eta_t \sim U(-1, 1). \tag{A3}$$

The conditional mean and variance values are then $\mathbb{E}_{t-1}[R_t] = \bar{R}, \mathbb{E}_{t-1}[\eta_t^2] = \frac{1}{3}$.

For government spending during period $t-1$ known by time t, without loss of generality, we assume that $G_{t(t-1)}$ or G_t has (1) a known smoothing component G_{t-1} (which is government spending during period $t-2$ and known by time $t-1$), (2) a *true* spending deviation from previous period D_{t-1} (which under perfectly timely disclosure and precision of information about these transactions is known during period $t-1$), and (3) an error term ϵ_t:

$$G_t = G_{t-1} + D_{t-1} + \epsilon_t, \tag{A4}$$

$$\epsilon_t \sim U\left(-\frac{\phi}{K}, \frac{\phi}{K}\right). \tag{A5}$$

The error term ϵ_t is core to our model. Parameter ϕ measures the relative risk associated with fiscal spending; in our context, this means that government could change or terminate contracts. Intuitively, higher ϕ indicates higher fiscal uncertainty. Parameter K controls for how precise the *true* spending deviation D_{t-1} is known to analysts. Intuitively, as K goes to inf, analysts know precise information. Lastly, we assume $E(D_{t-1}) = 0$ and denote $E(G_t) = \bar{G}$. The conditional mean and variance values are then $\mathbb{E}_{t-1}[G_t] = G_{t-1} + D_{t-1}, \mathbb{E}_{t-1}[(\epsilon_t)^2] = \frac{\phi^2}{3K^2}$. Both shocks η_t and ϵ_t i.i.d. from each other.

(d). Solving the minimizing problem.

Process X_t can be rewritten as

$$X_t = \bar{R} + \kappa G_{t-1} + \kappa D_{t-1} + \eta_t + \kappa\epsilon_t. \tag{A6}$$

After substituting the X_t process in Equation (A1), our minimization problem can be expanded as:

$$\min_{X_t^F} \underbrace{\mathbb{E}_{t-1}\left[(\bar{R} + \kappa G_{t-1} + \kappa D_{t-1} + \eta_t + \kappa\epsilon_t - X_t^F)^2\right]}_{\text{Part 1}}$$
$$+ \underbrace{\mathbb{E}_{t-1}\left[48\lambda \mathbf{1}_{X_t^F > \bar{R}+\kappa G_{t-1}+\kappa D_{t-1}+\eta_t+\kappa\epsilon_t} \frac{(X_t^F - \bar{R} - \kappa G_{t-1} - \kappa D_{t-1} - \eta_t - \kappa\epsilon_t)^2}{(X_t^F - \bar{R} - \kappa G_{t-1} - \kappa D_{t-1} - 1 - \kappa\phi/K)^2}\right]}_{\text{Part 2}}.$$

- Part 1: The first quadratic loss term can be easily derived as $(\bar{R} + \kappa G_{t-1} + \kappa D_{t-1} - X_t^F)^2 + \frac{1}{3}(1 + \kappa^2\phi^2/K^2)$.

- Part 2: The second penalty term has the following closed-form solution: $\lambda \cdot \frac{(X_t^F - \bar{R} - \kappa G_{t-1} - \kappa D_{t-1} + \kappa\phi/K + 1)^2}{\kappa\phi/K}$. We provide the proof next:
  - The relevant range is $X_t^F > \bar{R} + \kappa G_{t-1} + \kappa D_{t-1} + \eta_t + \kappa\epsilon_t$. One should integrate only over the range where this condition holds. η_t and ϵ_t are independent, with $f_\eta(\eta_t) = \frac{1}{2} \forall \eta_t \in [-1, 1]$ and $f_\epsilon(\epsilon_t) = \frac{K}{2\phi} \forall \epsilon_t \in \left[-\frac{\phi}{K}, \frac{\phi}{K}\right]$. The joint PDF is $f_{\eta,\epsilon}(\eta_t, \epsilon_t) = f_\eta(\eta_t) \cdot f_\epsilon(\epsilon_t) = \frac{K}{4\phi}, \quad (\eta_t, \epsilon_t) \in [-1, 1] \times \left[-\frac{\phi}{K}, \frac{\phi}{K}\right]$.
  - Define $C = \bar{R} + \kappa G_{t-1} + \kappa D_{t-1}$. The double integral question becomes:

$$\text{Part 2} = \frac{48\lambda K}{4\phi(X_t^F - C + 1 + \kappa\phi/K)^2} \cdot \int_{-\frac{\phi}{K}}^{\frac{X_t^F - C + 1}{\kappa}} \underbrace{\int_{-1}^{X_t^F - C - \kappa\epsilon_t} \left(X_t^F - C - \eta_t - \kappa\epsilon_t\right)^2 d\eta_t}_{\text{Part 2.1}} d\epsilon_t, \tag{A7}$$

And Part 2.1 can be solved as follows:

$$\begin{aligned}
\text{Part 2.1} &= \int_{-1}^{X_t^F - C - \kappa\epsilon_t} \left(X_t^F - C - \eta_t - \kappa\epsilon_t\right)^2 d\eta_t \\
&= \int_{-1}^{X_t^F - C - \kappa\epsilon_t} \left(X_t^F - C - \kappa\epsilon_t\right)^2 d\eta_t + \int_{-1}^{X_t^F - C - \kappa\epsilon_t} -2\left(X_t^F - C - \kappa\epsilon_t\right)\eta_t \, d\eta_t \\
&+ \int_{-1}^{X_t^F - C - \kappa\epsilon_t} \eta_t^2 \, d\eta_t \\
&= \left(X_t^F - C - \kappa\epsilon_t\right)^2 \left(\left(X_t^F - C - \kappa\epsilon_t + 1\right)\right) \\
&- \left(X_t^F - C - \kappa\epsilon_t\right)\left[\left(X_t^F - C - \kappa\epsilon_t\right)^2 - 1\right] \\
&+ \frac{1}{3}\left[\left(X_t^F - C - \kappa\epsilon_t\right)^3 - (-1)^3\right] \\
&= \frac{1}{3}\left(X_t^F - C - \kappa\epsilon_t + 1\right)^3
\end{aligned} \tag{A8}$$

We then substitute Equation (A8) back into Equation (A7) and define $A = X_t^F - C + 1$. The second integral can be solved:

$$\text{Part 2} = \frac{48\lambda K}{4\phi(A + \kappa\phi/K)^2} \cdot \frac{1}{3} \cdot \underbrace{\int_{-\frac{\phi}{K}}^{A/\kappa} (A - \kappa\epsilon_t)^3 \, d\epsilon_t}_{\text{Part 2.2}}. \tag{A9}$$

And Part 2.2 can be solved as follows:

$$\begin{aligned}
\text{Part 2.2} &= \int_{-\frac{\phi}{K}}^{A/\kappa} (A-\kappa\epsilon_t)^3 \, d\epsilon_t \\
&= \int_{-\frac{\phi}{K}}^{A/\kappa} A^3 - 3A^2\kappa\epsilon_t + 3A\kappa^2\epsilon_t^2 - \kappa^3\epsilon_t^3 \, d\epsilon_t \\
&= A^3\left(\frac{A}{\kappa}+\frac{\phi}{K}\right) - \frac{3}{2}A^2\kappa\left(\frac{A^2}{\kappa^2}-\frac{\phi^2}{K^2}\right) + A\kappa^2\left(\frac{A^3}{\kappa^3}+\frac{\phi^3}{K^3}\right) - \frac{\kappa^3}{4}\left(\frac{A^4}{\kappa^4}-\frac{\phi^4}{K^4}\right) \\
&= \frac{1}{4\kappa}A^4 + \frac{\phi}{K}A^3 + \frac{3}{2}\kappa(\frac{\phi}{K})^2A^2 + \kappa^2(\frac{\phi}{K})^3A + \frac{\kappa^3}{4}(\frac{\phi}{K})^4 \\
&= \frac{1}{4\kappa}\left(A+\frac{\kappa\phi}{K}\right)^4. \qquad \text{(A10)}
\end{aligned}$$

– Finally, we substitute Equation (A10) back into Equation (A9) and obtain:

$$\text{Part 2} = \lambda \cdot \frac{(X_t^F - \bar{R} - \kappa G_{t-1} - \kappa D_{t-1} + \kappa\phi/K + 1)^2}{\kappa\phi/K}. \qquad \text{(A11)}$$

As a result, the objective function can be further simplified into:

$$\min_{X_t^F}\left[(\bar{R}+\kappa G_{t-1}+\kappa D_{t-1}-X_t^F)^2 + \frac{1}{3}\left(1+\frac{\kappa^2\phi^2}{K^2}\right) + \lambda\cdot\frac{\left(X_t^F-\bar{R}-\kappa G_{t-1}-\kappa D_{t-1}+\frac{\kappa\phi}{K}+1\right)^2}{\frac{\kappa\phi}{K}}\right].$$

The first-order condition is obtained by differentiating this with respect to X_t^F:

$$-2(\bar{R}+\kappa G_{t-1}+\kappa D_{t-1}-X_t^F) + \frac{2\lambda}{\kappa\phi/K}(X_t^F-\bar{R}-\kappa G_{t-1}-\kappa D_{t-1}+\kappa\phi/K+1) = 0. \qquad \text{(A12)}$$

$$2(\bar{R}+\kappa G_{t-1}+\kappa D_{t-1}-X_t^F) = \frac{2\lambda}{\kappa\phi/K}(X_t^F-\bar{R}-\kappa G_{t-1}-\kappa D_{t-1}+\kappa\phi/K+1). \qquad \text{(A13)}$$

$$X_t^F = \frac{(\kappa G_{t-1}+\kappa D_{t-1}+\bar{R})(2+\lambda/(\kappa\phi/K)) - \frac{\lambda(\kappa\phi/K+1)}{\kappa\phi/K}}{2+\lambda/(\kappa\phi/K)}. \qquad \text{(A14)}$$

(e). The forecast bias variable.

The expected bias can be derived as a closed-form function, $\text{Surprise}_t(\kappa, \lambda, \phi, K)$:

$$\text{Surprise}_t(\kappa, \lambda, \phi, K) = \bar{R} + \kappa G_{t-1} + \kappa D_{t-1} - X_t^F, \tag{A15}$$

$$= \frac{\lambda(1 + \kappa\phi/K)}{\lambda + \kappa\phi/K} > 0. \tag{A16}$$

Prediction 1*: It is always optimal to underestimate the earnings, as* $\kappa, \phi, K > 0$ *and* $\lambda > 1$.

(f). Testable predictions.

First, we study the relationship between fiscal dependence κ and Bias. The derivative of Bias with respect to κ, $\frac{\partial \text{Surprise}}{\partial \kappa}$, becomes:

$$\frac{\partial \text{Surprise}}{\partial \kappa} = \frac{\lambda(\lambda - 1)\phi/K}{(\lambda + \kappa\phi/K)^2} > 0. \tag{A17}$$

Prediction 2: *Earnings surprises or biases monotonically increase with fiscal exposure* κ*, as long as* $\lambda > 1$.

Next, we study how $\frac{\partial \text{Surprise}}{\partial \kappa}$ changes with uncertainty ϕ and information precision K, one at a time, more explicitly. We use $g(\phi)$ to denote $\frac{\lambda(\lambda-1)\phi/K}{(\lambda+\kappa\phi/K)^2}$ and differentiate $g(\phi)$ with respect to ϕ using the quotient rule. The numerator is:

$$f(\phi) = \lambda(\lambda - 1)\phi/K,$$

and the denominator is:

$$h(\phi) = (\lambda + \kappa\phi/K)^2.$$

The quotient rule gives:

$$\begin{aligned}
\frac{dg}{d\phi} &= \frac{f'(\phi)h(\phi) - f(\phi)h'(\phi)}{h(\phi)^2}, \\
&= \frac{\frac{\lambda(\lambda-1)}{K}(\lambda + \kappa\phi/K)^2 - \frac{\lambda(\lambda-1)\phi}{K} \cdot 2(\lambda + \kappa\phi/K) \cdot \frac{\kappa}{K}}{(\lambda + \kappa\phi/K)^4} \\
&= \frac{\frac{\lambda(\lambda-1)}{K}(\lambda + \kappa\phi/K)\left[\lambda + \kappa\phi/K - \frac{2\phi\kappa}{K}\right]}{(\lambda + \kappa\phi/K)^4}, \\
&= \frac{\frac{\lambda(\lambda-1)}{K}(\lambda + \kappa\phi/K)\left[\lambda - \frac{\phi\kappa}{K}\right]}{(\lambda + \kappa\phi/K)^4}.
\end{aligned}$$

The denominator of $\frac{dg}{d\phi}$, $\frac{\lambda(\lambda-1)}{K}$, and $(\lambda + \kappa\phi/K)$ are always positive. The key

term in the numerator is $\lambda - \frac{\phi\kappa}{K}$. Thus, $\frac{dg}{d\phi}$ is positive if:

$$\lambda > \frac{\phi\kappa}{K}.$$

When λ (loss aversion) is sufficiently large relative to $\phi\kappa/K$ (which can be interpreted as scaled fiscal uncertainty), the predictability of fiscal exposure to earnings surprises or biases (the derivative of the Bias with respect to κ) increases with ϕ. This is likely the case as empirically κ typically is < 0.1 and we observe timely transaction data being posted (i.e., a large K).

Prediction 3: *Under reasonable parameter assumptions, the predictability of fiscal exposure to earnings surprises or biases should* ***increase*** *with fiscal uncertainty.*

We then use $g(K)$ to denote $\frac{\lambda(\lambda-1)\phi/K}{(\lambda+\kappa\phi/K)^2}$ and differentiate $g(K)$ with respect to K using the quotient rule. Using the quotient rule, let $f(K) = \lambda(\lambda-1)\phi/K$, $h(K) = (\lambda + \kappa\phi/K)^2$. The derivative is:

$$\begin{aligned}
\frac{dg}{dK} &= \frac{f'(K)h(K) - f(K)h'(K)}{h(K)^2}, \\
&= \frac{\left(-\frac{\lambda(\lambda-1)\phi}{K^2}\right)(\lambda+\kappa\phi/K)^2 - \left(\frac{\lambda(\lambda-1)\phi}{K}\right)\left(2(\lambda+\kappa\phi/K)\cdot\frac{-\kappa\phi}{K^2}\right)}{(\lambda+\kappa\phi/K)^4}, \\
&= \frac{-\frac{\lambda(\lambda-1)\phi}{K^2}(\lambda+\kappa\phi/K)(\lambda-\frac{\kappa\phi}{K})}{(\lambda+\kappa\phi/K)^4}.
\end{aligned}$$

The denominator and $(\lambda + \kappa\phi/K)$ are always positive. $(\lambda - \frac{\kappa\phi}{K})$ is positive if $\lambda > \frac{\kappa\phi}{K}$, which is typically satisfied under reasonable parameter values, as also assumed to derive Prediction 3 (see above). Finally, $-\frac{\lambda(\lambda-1)\phi}{K^2}$ is negative if $\lambda > 1$, which is also the general assumption. As a result, $\frac{dg}{dK}$ is negative.

Prediction 4: *Under reasonable parameter assumptions, the predictability of fiscal exposure to earnings surprises or biases should* ***decrease*** *with information precision.*

B Supplemental Discussion of Government Obligations

This section complements Section 3.1 by providing additional direct data examples of both multi- and single-transaction procurement obligations. These examples illustrate how our cross-firm test can be extended into a panel framework as developed in Section 4.2.

Table A1, Panel A, displays the full obligation history of a procurement contract issued by the federal government to the Hess Corporation with an end date of September 30, 2019. Two observations are particularly relevant. First, only fifteen days after the initial installment ($2,323,733) on March 20, 2018, a second obligation was issued. The obligation actions were irregular but frequent, consistent with the continuation of funding needed to sustain production. This suggests that obligations are intended as purchase commitments and plausibly reflect timely procurement activity. Second, during Q3 2018, Q3 2019, and Q1 2021, there are three sizable de-obligations due to budget changes or canceled work. The de-obligations in Q3 2018 and Q3 2019 coincide with the end of the federal fiscal year. Typically, September is a surge month for procurement as agencies rush to obligate remaining funds ("use-it-or-lose-it" behavior). However, when budgets are uncertain or debt ceilings loom, agencies may instead pull back or delay final obligations. Taken together, this contract-level timeline suggests that obligations are released in a timely manner and plausibly track contemporaneous production activity.

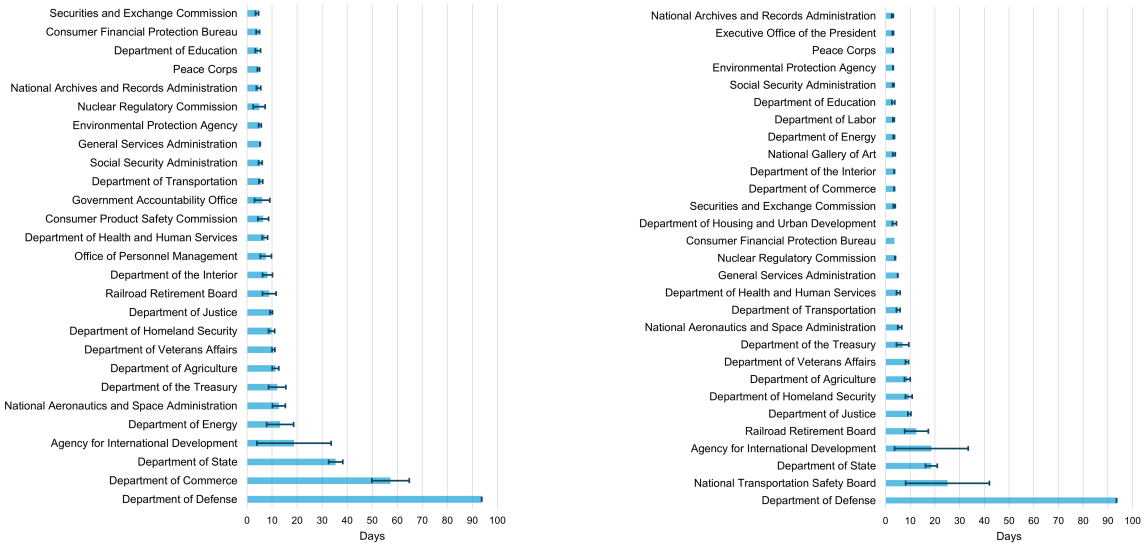
Panel B provides a second example from the Hess Corporation, but in this case the contract, which was expected to start and end on the same day (2/14/2013), is associated with only a single obligation transaction. The Department of Health and Human Services (HHS) issued a $1.2 million base contract in early 2013 to Hess for a supply of fuel, energy, or another commodity product on its campus. As this was a commodity acquisition rather than an ongoing service contract, no subsequent modifications were recorded.

In the remainder of Panel B, we present two additional examples of one-time obligations, drawn from different industries with high fiscal dependence and sponsored by distinct government agencies. First, the Starlims Corporation (a subsidiary of Abbott Laboratories) received a contract obligated on June 14, 2017 and funded by the Department of Justice for $875,830 to develop a system that automatically collects data from laboratory instruments. Notably, the obligation action date differs from the official contract start date. Second, EMCOR Government Services was awarded a contract that was obligated on May 31, 2017 and from the Government Accountability Office for approximately $2.59 million to provide operations and maintenance services at federal buildings. These additional cases highlight two recurring features of the data: the prevalence of single-day contracts and the frequent divergence between contract start dates and obligation dates.

These examples highlight that obligation action dates, rather than stated con-

tract periods, provide a more accurate reflection of actual financial commitments and the timing of earnings accruals. In practice, the obligation date, not the contract start date, more reliably indicates when the government's financial commitment is made and when firms can begin to recognize related earnings.

C Supplemental Results



A. Scraping exercise #1: 10/1/2023–1/18/2024

B. Scraping exercise #2: 8/8/2024–11/5/2024

Figure A1: **Two Scraping Exercises: The Average Delay (In Days) of the Publication of Transaction Data on `USAspending.gov` Sorted by Agency.** We discuss the technical details in Section 3.2. In short, each day we scrape the entire domain of `USAspending.gov`; we capture incremental transactions added and calculate the delay differences in real time. To produce this figure, we sort the transactions by awarding agencies. The bar chart shows the average and its 95% confidence interval.

Table A1: **Procurement Timeline of a Multi-Transaction Contract.**
Links: https://www.usaspending.gov/award/CONT_AWD_HHSN292201800020U_7529_SP060009D8016_9700;
https://www.usaspending.gov/award/CONT_AWD_HHSN292201300089U_7529_SP060009D8016_9700;
https://www.usaspending.gov/award/CONT_AWD_DJD17HQP0514_1524_-NONE-_-NONE-;
https://www.usaspending.gov/award/CONT_AWD_GA013SB00110117_0559_GA013SB0011_0559.

Panel A: Contract history with multiple obligations documented (Contract period expected: 3/20/2018-9/30/2019).

Company name:	***Hess Corporation***
Cusip:	***42809H10***
NAICS:	***21, Mining, Quarrying, and Oil and Gas Extraction***
Funding agency:	***DEPARTMENT OF HEALTH AND HUMAN SERVICES (HHS)***

Mod #	Obligation Action Date	QY		Obligation Amount	Action Type	Interpretation
0	3/20/2018	Q1,2018		$2,323,733	–	Initial base award of the contract.
P00001	4/5/2018	Q2,2018		$5,104,175	C: Funding Only Action	Major funding increase; likely activates or expands core work.
P00002	4/25/2018	Q2,2018		$38,307	C: Funding Only Action	Small additional funding, possibly for support costs or minor adjustments.
P00003	5/16/2018	Q2,2018		$3,080,550	C: Funding Only Action	Major funding action; could support new phase or milestone.
P00004	5/30/2018	Q2,2018		$3,185,500	C: Funding Only Action	Another large funding addition—suggests rapid project ramp-up.
P00005	7/20/2018	Q3,2018		$121,117	C: Funding Only Action	Minor adjustment or top-up for specific deliverables.
P00006	8/31/2018	Q3,2018		$3,106,035	C: Funding Only Action	Substantial continued funding; possibly for expanded scope or deliverables.
P00007	9/27/2018	Q3,2018	End-of-FY2018	-$2,097,032	C: Funding Only Action	De-obligation—funds removed, maybe due to overestimation or canceled work.
P00008	11/23/2018	Q4,2018		$70,965	C: Funding Only Action	Small funding update, likely patch or balance reallocation.
P00009	12/30/2018	Q4,2018		$43,008	C: Funding Only Action	Similar to above—minor adjustment.
P00010	3/13/2019	Q1,2019		$28,085	C: Funding Only Action	Another small top-up.
P00011	9/11/2019	Q3,2019	End-of-FY2019	-$1,616,781	C: Funding Only Action	Significant de-obligation—possibly after project phase ended or funds unused.
P00012	3/8/2021	Q1,2021		-$112,320	C: Funding Only Action	Final cleanup de-obligation, likely part of close-out process.

Panel B: Contract history with one obligation documented.

Firm (Cusip; NAICS)	Funding Agency	Contract Start Date	Potential End Date	Obligation Action Date	Obligation Amount	Interpretation
Hess Corporation (42809H10; 21)	DEPARTMENT OF HEALTH AND HUMAN SERVICES (HHS)	2/14/2013	2/14/2013	2/14/2013	$1,200,445.82	The contract purchased commodity.
STARLIMS CORPORATION (00282410; 33)	DEPARTMENT OF JUSTICE (DOJ)	6/7/2017	6/13/2018	6/14/2017	$875,830.49	The contract funded a system that automatically collect data from lab instruments.
EMCOR GOVERNMENT SERVICES, INC (29084Q10; 23)	GOVERNMENT ACCOUNTABILITY OFFICE (GAO)	5/31/2017	12/31/2018	5/31/2017	$2,585,296.84	The contract funded operations and maintenance services for federal buildings.

Table A2: **Changes in Contract Terms and Budgetary Uncertainty Periods.**
This table shows the relationship between changes in contract terms and budgetary uncertainty periods. For each contract-month, we calculate the potential award amount change from month-start to month-end and use the *amount increase* to indicate the direction of the change. Specifically, the *amount increase* equals 1, 0, or -1 to indicate an increase, no change, or a decrease in the contract amount, respectively. Amount increase ratio 1 is calculated by averaging the *amount increase* within each firm-month, and then taking the average across firms for each month. Amount increase ratio 2 is the average of *amount increase* for each month. Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Quarter FE		Yes		Yes
	(1)	(2)	(3)	(4)
Dependent variable:	Amount Increase Ratio 1	Amount Increase Ratio 1	Amount Increase Ratio 2	Amount Increase Ratio 2
Panel A. EPU related to fiscal policy				
EPU Fiscal Policy / 10000	-0.0318***	-0.0319**	-0.0074***	-0.0077***
	(0.0111)	(0.0144)	(0.0025)	(0.0027)
Constant	0.0010***	0.0010***	0.0002***	0.0002***
	(0.0003)	(0.0003)	(0.0000)	(0.0000)
N	127	127	127	127
R^2	0.013	0.018	0.046	0.049
Panel B. EPU related to government spending				
EPU Fiscal Policy: Spending / 10000	-0.0330***	-0.0339***	-0.0065***	-0.0067***
	(0.0094)	(0.0109)	(0.0017)	(0.0017)
Constant	0.0010***	0.0010***	0.0002***	0.0002***
	(0.0003)	(0.0003)	(0.0000)	(0.0000)
N	127	127	127	127
R^2	0.040	0.046	0.099	0.10
Panel C. EPU attributed to debt ceilings				
EPU Attributed to Debt Ceiling / 10000	-0.7056**	-0.7916**	-0.1377*	-0.1460*
	(0.3086)	(0.3809)	(0.0720)	(0.0745)
Constant	0.0006***	0.0006***	0.0001***	0.0001***
	(0.0002)	(0.0002)	(0.0000)	(0.0000)
N	127	127	127	127
R^2	0.0056	0.012	0.013	0.017

Table A3: **Summary Statistics.**
This table provides the summary statistics of the main variables used in Sections 4, 5, and 6.

	Count	Mean	SD	Min	p5	p25	p50	p75	p95	Max
Panel A. Variables used in the main cross-sectional specification										
Beat	474	0.660	0.154	0.235	0.395	0.558	0.674	0.767	0.907	1.000
SUE_1	474	1.240	1.328	-4.397	-0.695	0.484	1.114	1.829	3.566	9.169
SUE_2	474	0.068	0.147	-0.700	-0.133	0.020	0.059	0.116	0.288	0.786
Procurement	474	0.021	0.054	0.000	0.000	0.000	0.002	0.013	0.133	0.321
Non-DoD Procurement	474	0.004	0.011	0.000	0.000	0.000	0.001	0.003	0.025	0.065
Log(1+MarketCap)	474	22.182	1.725	17.992	19.350	20.875	22.155	23.399	25.222	26.901
Log(1+Book-to-Market)	474	0.403	0.196	0.026	0.120	0.259	0.378	0.523	0.761	1.438
Log(1+Ret_m61tom12)	474	0.024	0.019	-0.099	-0.004	0.014	0.026	0.036	0.051	0.084
Log(1+Ret_m6tom2)	474	0.003	0.008	-0.038	-0.010	-0.001	0.004	0.007	0.015	0.028
Log(1+InstitutionOwnPct)	474	0.584	0.090	0.185	0.428	0.531	0.602	0.650	0.695	0.752
Log(1+IVOL_m11tom2)	474	0.016	0.005	0.007	0.009	0.012	0.015	0.019	0.025	0.035
Log(1+TOV_m61tom12)	474	0.008	0.003	0.002	0.004	0.005	0.007	0.009	0.014	0.029
Panel B. Variables used in the main panel specification (2009/06-2019/12)										
Beat	19027	0.663	0.473	0.000	0.000	0.000	1.000	1.000	1.000	1.000
SUE_1	18602	1.255	3.756	-60.000	-3.250	-0.200	1.000	2.667	6.667	76.000
SUE_2	18710	0.067	0.543	-14.152	-0.390	-0.011	0.044	0.161	0.608	13.757
Procurement	16737	0.021	0.064	0.000	0.000	0.000	0.001	0.008	0.133	0.504
Non-DoD Procurement	16702	0.004	0.013	0.000	0.000	0.000	0.000	0.002	0.027	0.102
Log(1+MarketCap)	19027	22.237	1.766	16.782	19.420	20.979	22.156	23.437	25.356	27.702
Log(1+Book-to-Market)	19027	0.402	0.231	0.001	0.097	0.236	0.365	0.537	0.809	3.450
Log(1+Ret_m61tom12)	19021	0.025	0.124	-1.216	-0.181	-0.038	0.031	0.093	0.206	1.093
Log(1+Ret_m6tom2)	19024	0.003	0.040	-0.685	-0.060	-0.016	0.005	0.024	0.062	0.264
Log(1+InstitutionOwnPct)	19027	0.586	0.099	0.000	0.412	0.534	0.604	0.654	0.707	1.786
Log(1+IVOL_m11tom2)	19023	0.016	0.009	0.002	0.006	0.010	0.014	0.019	0.032	0.197
Log(1+TOV_m61tom12)	19021	0.008	0.005	0.000	0.003	0.005	0.007	0.009	0.016	0.150
Panel C. Interaction variables										
Renegotiation Index	473	0.238	0.052	0.013	0.172	0.211	0.231	0.264	0.324	0.467
is_debtlimit	19027	0.124	0.330	0.000	0.000	0.000	0.000	0.000	1.000	1.000
% Changes in debt ceiling levels	19027	0.617	2.247	0.000	0.000	0.000	0.000	0.000	3.400	29.170
Monthly Average EPU Attributed to Debt Ceiling (within each firm-quarter)	19027	0.472	1.663	0.000	0.000	0.000	0.028	0.180	1.431	10.087
Monthly Average Risk Aversion (within each firm-quarter)	19027	2.920	0.376	2.505	2.546	2.670	2.818	3.000	3.630	4.305
Monthly Average VIX (within each firm-quarter)	19027	17.592	5.229	10.093	10.833	14.683	15.817	19.430	29.553	34.847
Monthly Average RV (within each firm-quarter)	19027	13.754	5.664	5.977	7.151	10.226	12.302	15.896	26.101	36.102

Table A4: **Robustness Tests for Table 1.**
This table only reports the coefficients and SE of our main variable of interest, and each column should *not* be read as one regression. For Panels A, C, and D, we discuss the exact constructions of our alternative measures in Appendix Table IB.1. For Panel B, we include only firms with active transaction obligated amounts during most of our sample period (e.g., more than 80% of the quarters from 2009/Q2 to 2019/Q4). Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

NAICS2 FE		Yes		Yes
With Controls			Yes	Yes
	(1)	(2)	(3)	(4)
Panel A: Alternative fiscal dependence measures				
Dependent variable:			*Beat*	
Procurement (main measure; ObligatedAmt/Revenue_past4qtr)	0.2428	0.2459*	0.3522***	0.3181**
	(0.1513)	(0.1431)	(0.1312)	(0.1249)
Log(1+ObligatedAmt)	0.0088***	0.0115***	0.0061***	0.0072***
	(0.0017)	(0.0018)	(0.0017)	(0.0018)
ObligatedAmt/Revenue	0.2483	0.2530*	0.3626***	0.3286***
	(0.1520)	(0.1435)	(0.1308)	(0.1243)
ObligatedAmt/Revenue_past2qtr	0.2436	0.2508*	0.3534***	0.3221***
	(0.1502)	(0.1414)	(0.1301)	(0.1235)
ObligatedAmt/MarketCap	711.7513	1075.3532**	1207.1073***	1283.9438***
	(526.0133)	(470.0817)	(443.3280)	(410.7520)
Panel B: Intensive margin				
Dependent variable:			*Beat*	
Procurement	0.1494	0.1404	0.2148	0.2032
	(0.1565)	(0.1463)	(0.1306)	(0.1239)
Panel C: Drop Department of Defense-sponsored transactions				
Dependent variable:			*Beat*	
Non-DoD Procurement	1.9102***	2.0711***	2.0396***	2.0349***
	(0.5922)	(0.6007)	(0.4758)	(0.4966)
Panel D: Alternative scaled earnings surprise measures				
Dependent variable:		*SUE (surprise, scaled by analyst forecast standard deviation); SUE_1*		
Procurement	3.5166**	3.5771**	3.9806**	3.9405**
	(1.6385)	(1.5576)	(1.6264)	(1.5587)
Dependent variable:		*SUE (Froot, Kang, Ozik, and Sadka (2017)); SUE_2*		
Procurement	0.2072	0.2821*	0.2451	0.3103**
	(0.1889)	(0.1536)	(0.1911)	(0.1571)

Table A5: **Robustness Tests for Table 2: Pooling.**
This table only reports the coefficients and SE of our main variable of interest, and each column should *not* be read as one regression. For Panels A, C, and D, we discuss the exact constructions of our alternative measures in Appendix Table IB.1. For Panel B, we include only firms with active transaction obligated amounts during most of our sample period (e.g., more than 80% of the quarters from 2009/Q2 to 2019/Q4). Standard errors are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE:		Yes			Yes	
NAICS2 FE:	Yes	Yes		Yes	Yes	
NAICS2 x Quarter FE:			Yes			Yes
With Controls:				Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Alternative fiscal dependence measures						
Dependent variable:				*Beat*		
Procurement (main measure; ObligatedAmt/Revenue_past4qtr)	0.2716***	0.2656***	0.2624***	0.2860***	0.2752***	0.2693***
	(0.0946)	(0.0934)	(0.0959)	(0.0665)	(0.0638)	(0.0657)
Log(1+ObligatedAmt)	0.0071***	0.0071***	0.0070***	0.0038***	0.0035***	0.0033***
	(0.0012)	(0.0012)	(0.0012)	(0.0011)	(0.0010)	(0.0010)
ObligatedAmt/Revenue	0.2012*	0.1839*	0.1787*	0.2389***	0.2176***	0.2143***
	(0.1008)	(0.1023)	(0.1040)	(0.0689)	(0.0694)	(0.0702)
ObligatedAmt/Revenue_past2qtr	0.2397**	0.2242**	0.2208**	0.2583***	0.2387***	0.2340***
	(0.0977)	(0.0986)	(0.1003)	(0.0690)	(0.0692)	(0.0698)
ObligatedAmt/MarketCap	835.0857***	795.0571**	802.7230**	957.7549***	896.3433***	898.5603***
	(307.7815)	(311.3754)	(314.6267)	(225.4826)	(224.6480)	(224.8623)
Panel B: Intensive margin						
Dependent variable:				*Beat*		
Procurement	0.2211**	0.2085**	0.2023**	0.2417***	0.2237***	0.2177***
	(0.0861)	(0.0861)	(0.0883)	(0.0602)	(0.0589)	(0.0598)
Panel C: Drop Department of Defense-sponsored transactions						
Dependent variable:				*Beat*		
Non-DoD Procurement	1.5338***	1.5205***	1.4992***	1.3415***	1.3041***	1.2687***
	(0.4764)	(0.4845)	(0.5001)	(0.3376)	(0.3423)	(0.3485)
Panel D: Alternative scaled earnings surprise measures						
Dependent variable:	*SUE (surprise, scaled by analyst forecast standard deviation); SUE_1*					
Procurement	3.1667**	3.1109**	3.1061**	2.7052***	2.6122***	2.6074***
	(1.2952)	(1.3056)	(1.3055)	(0.9197)	(0.9207)	(0.9151)
Dependent variable:	*SUE (Froot, Kang, Ozik, and Sadka (2017)); SUE_2*					
Procurement	0.2369*	0.2228*	0.2265**	0.2173*	0.2051*	0.2093**
	(0.1189)	(0.1164)	(0.1091)	(0.1113)	(0.1094)	(0.1009)

Internet Appendices for "Fiscal Risk Perception"

IA Data Appendix

This appendix section complements and provides more details on the material covered in Section 3.

IA.1 USAspending.gov

We construct our database and conduct our scraping experiments from this public domain: https://www.usaspending.gov/. Section 3 provides some detailed explanations to help the reading of the main draft. In this internet appendix, we present raw interfaces and discuss other downloading details for future replications and extension work.

Key variables for each award:

- Award unique identifier
- Awarding agency
- Funding agency
- Award start date: the start of the entire contract period of performance
- Award end date
  - For a Contract type award, the field name is Period of Performance Potential End Date. The official definition: The date that the award ends, as agreed upon by the parties involved after exercising any pre-determined extension options. Note that the latest transaction for the award (known as the Latest Transaction Action Date) may be different than this date. Administrative actions related to this award may continue to occur after the Period of Performance Potential End Date. The Period of Performance Potential End Date does not apply to Contract Indefinite Delivery Vehicles under which Definitive Contracts may be awarded.
  - For IDVs type awards, the field name is Ordering Period End Date. The official definition: For procurement, the date on which, for the award referred to by the action being reported, no additional orders referring to it may be placed. This date applies only to procurement indefinite delivery vehicles (such as indefinite delivery contracts or blanket purchase agreements). Administrative actions related to this award may continue to occur after this date. The period of performance end dates for procurement orders issued under the indefinite delivery vehicle may extend beyond this date.
- Potential award amount: the total amount that could be obligated on a contract if the base and all options are exercised.

Key variables for each transaction:

- Transaction unique identifier
- Action date
- Amount: federal_action_obligation

In general, for each firm, we observe three types of information at the transaction level: firm specifics, transaction obligated amounts, awarding agency, and timing. https://www.usaspending.gov/recipient/53927ae0-321e-4c80-2dc9-430ca5135e33-P/latest In Figures IA.1, IA.2, and IA.3 below, we show the overview webpages of three companies. Boeing receives annual transactions of around 16 billion dollars from procurement contracts and 92% of them come from one single agency, the Department of Defense. AT&T receives annual transactions of around 168 million dollars and the awarding agencies are quite evenly distributed. ARCHER-DANIELS-MIDLAND COMPANY receives a similar amount but the awards are enitrely from the Department of Agriculture.

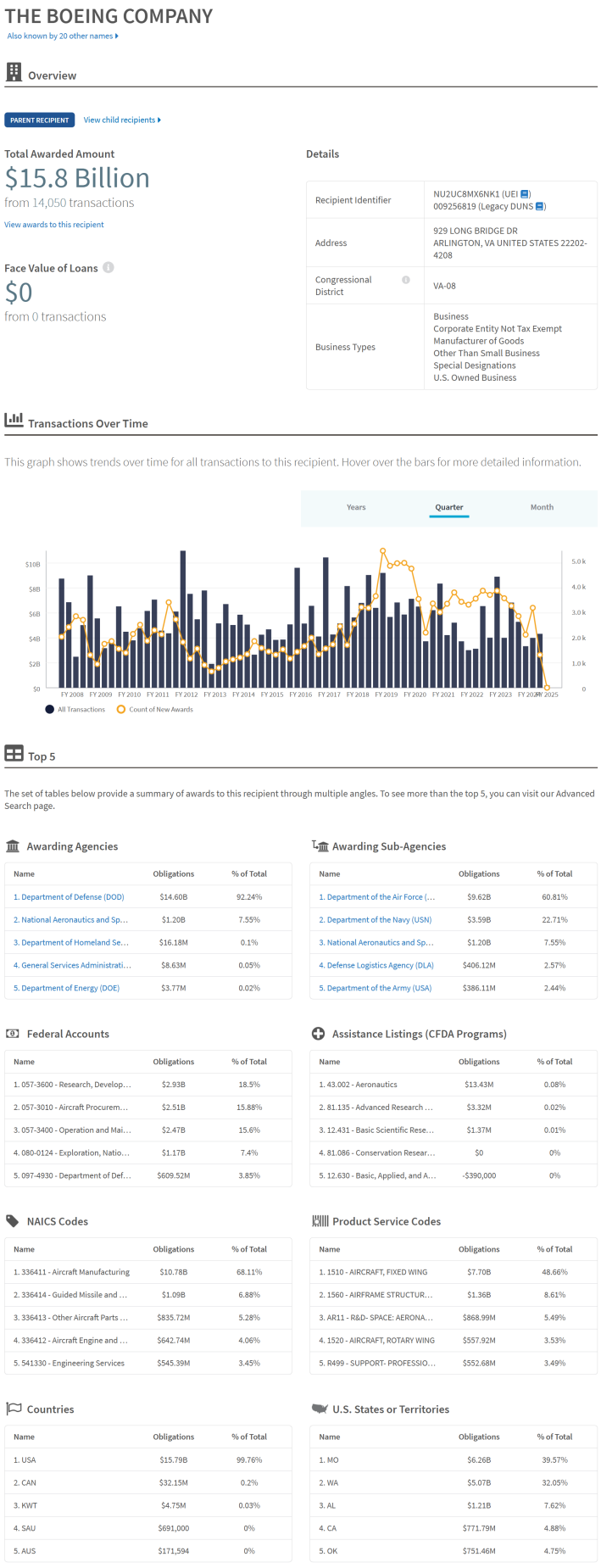


Figure IA.1: Boeing Webpage. https://www.usaspending.gov/recipient/419ccd27-d6f4-d363-aeaf-b9e2c3ae6f5d-P/latest

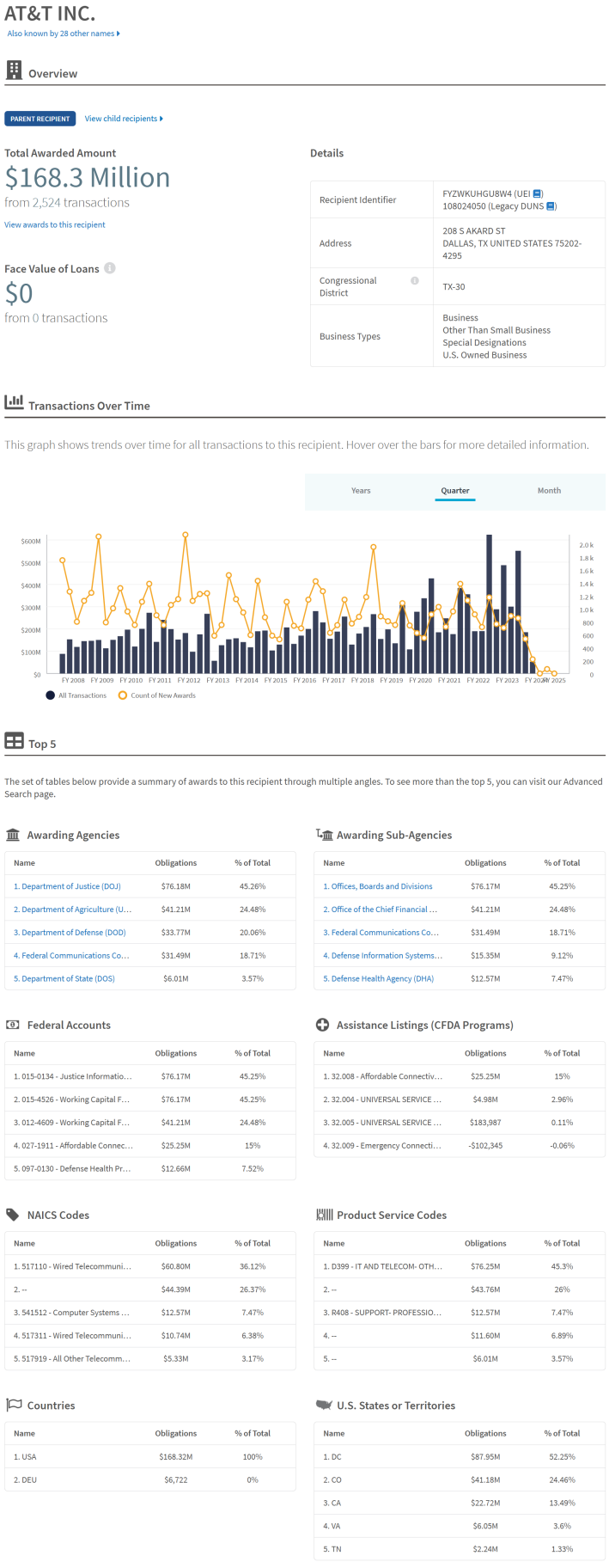


Figure IA.2: AT&T Webpage. https://www.usaspending.gov/recipient/53927ae0-321e-4c80-2dc9-430ca5135e33-P/latest

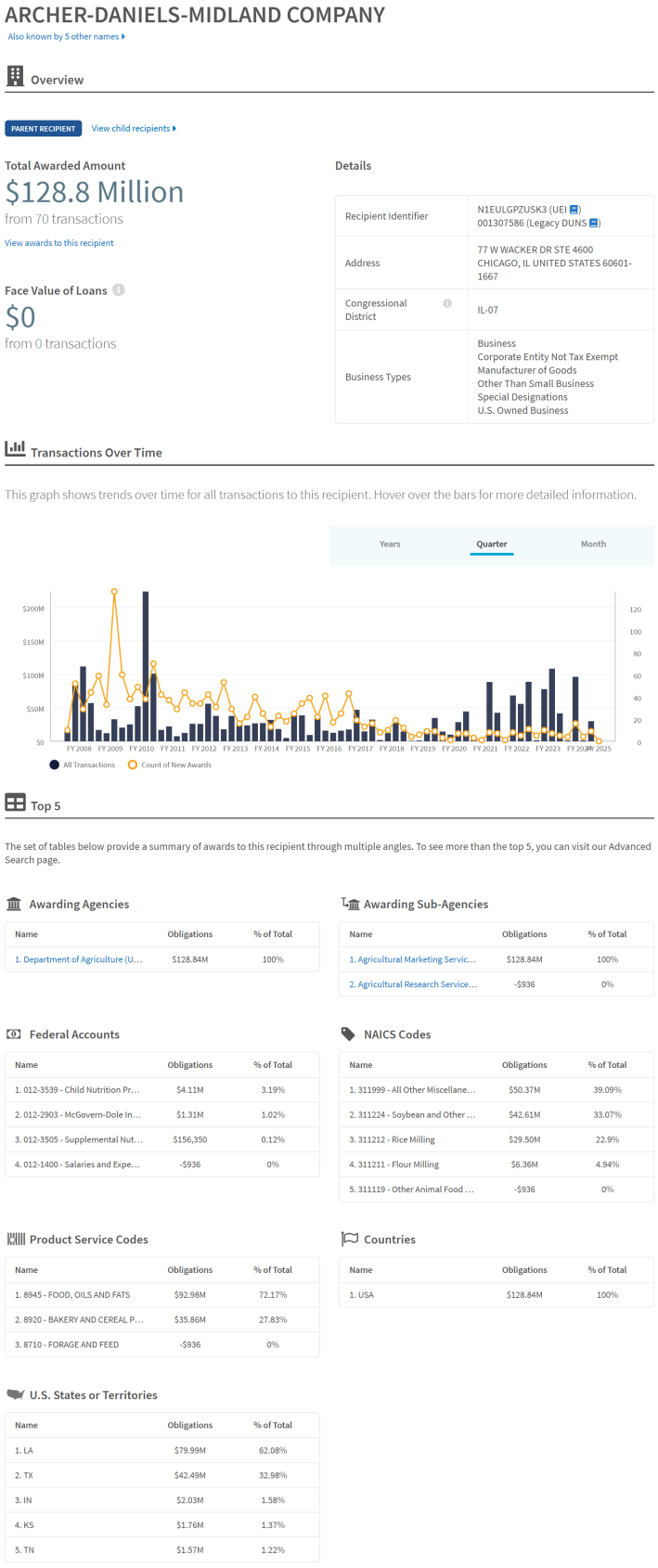


Figure IA.3: ARCHER-DANIELS-MIDLAND COMPANY Webpage. https://www.usaspending.gov/recipient/ef6337ce-be34-980c-d110-5c0e70f2a666-P/latest

IB Additional Tables and Figures

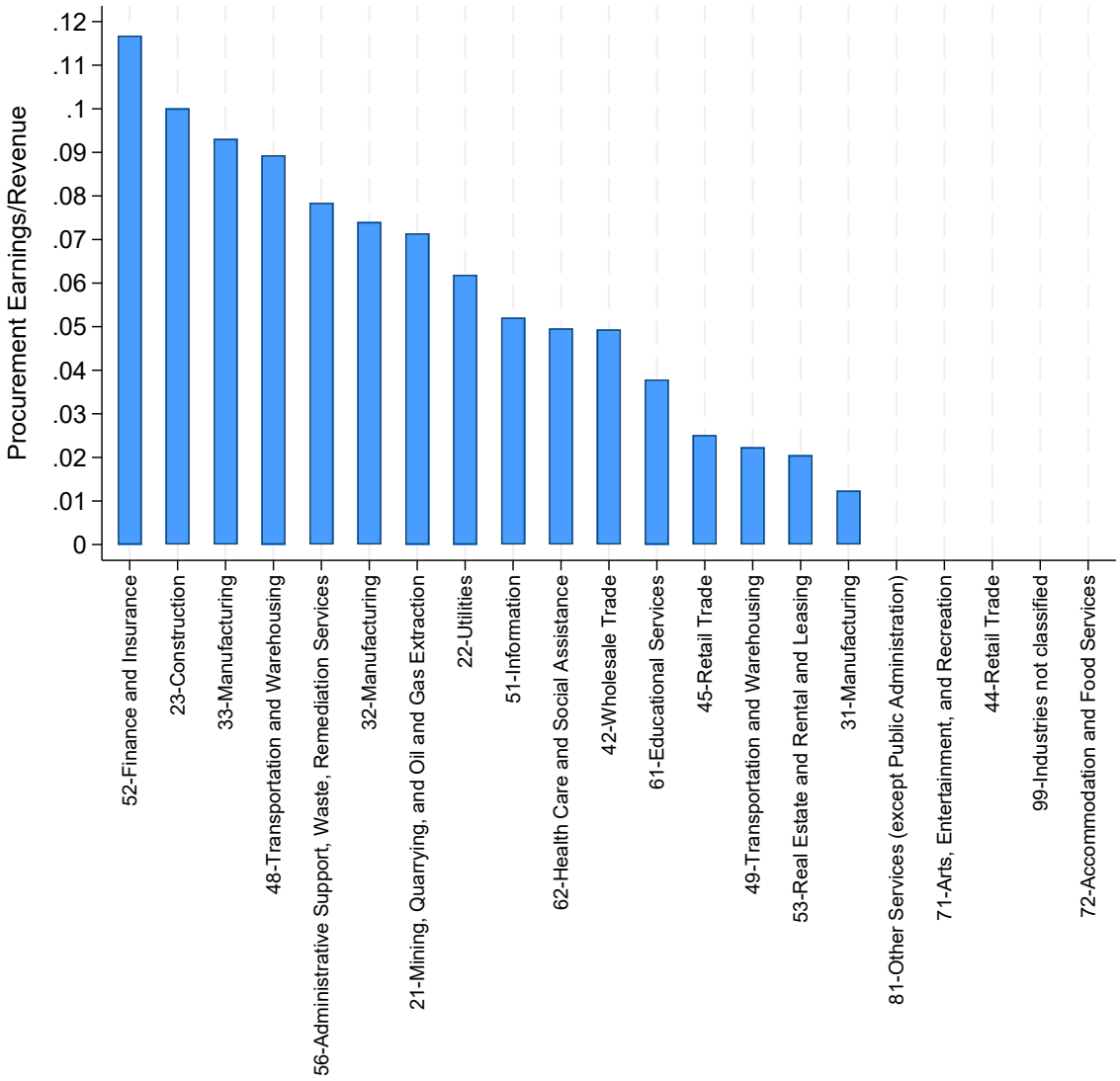


Figure IB.1: **Where Are Large Transactions Going?** The figure uses the largest 25% of firm-quarter transactions, and each bar calculates the average firm-quarter Procurement$_{i,t}$ for each industry using data from this subsample. The x-axis denotes the industry classification; the left y-axis corresponds to the fraction of procurement earnings scaled by average revenue in the past 4 quarters (as in the paper). Figure 1 in the main draft shows the full sample result.

Table IB.1: **Descriptions of Variables.**
This table provides explanations and constructions of the main variables used in this paper.

Variables	**Construction details**
Beat (main dependent variable)	An indicator variable equalling 1 when actual EPS is above the final analysts' consensus estimate and 0 otherwise.
SUE_1	Actual EPS minus the median estimated EPS (by analyst), scaled by the mean of the standard deviations of the analyst estimates for the current quarter and the previous quarter.
SUE_{11}	Actual EPS minus the mean of estimated EPS (by analyst), scaled by the mean of the standard deviations of the analyst estimates for the current quarter and the previous quarter.
SUE_{12}	Actual EPS minus the median estimated EPS (by analyst), scaled by the mean of the standard deviations of the analyst estimates for the current quarter.
SUE_2	Actual EPS minus the mean of estimated EPS (by analyst), scaled by the quarter-end stock price.
SUE_{21}	Actual EPS minus the median estimated EPS (by analyst), scaled by the quarter-end stock price.
ObligatedAmt	Total obligated amount received within each firm-fiscal quarter.
ObligatedAmt/Revenue	Total obligated amount scaled by quarterly revenue.
ObligatedAmt/Revenue_past2qtr	Total obligated amount scaled by the average revenue in the past two quarters.
Procurement (main predictor)	Total obligated amount scaled by the average revenue in the past four quarters.
Non-DoD Procurement	Total obligated amount sponsored by agencies other than the Department of Defense, scaled by the average revenue in the past four quarters.
MarketCap	Previous quarter-end market capitalization.
Book-to-Market	Previous quarter-end book-to-market ratio.

(Continuation of Table IB.1)

Variables	Construction details
Ret_m61tom12	Buy-and-hold return from day -61 to -12 before earnings announcement day.
Ret_m6tom2	Buy-and-hold return from day -6 to -2 before earnings announcement day.
InstitutionOwnPct	Percentage of shares held by institutions at the previous quarter-end.
IVOL_m11tom2	Standard deviation of daily stock returns between day -11 and -2 before earnings announcement.
TOV_m61tom12	Stock turnover ratio between day -61 to -12 before earnings announcement.
Analyst_measure1	Number of words in *paragraphs* spoken by analysts that mention "government contracts" or "procurement contracts" (or their variations), divided by total number of words in the transcript (excluding operator words).
Analyst_measure2	Number of words in *paragraphs* spoken by analysts that mention "government contracts" or "procurement contracts" (or their variations), divided by total number of words spoken by analysts.
Analyst_measure3	Same as "Analyst_measure1" except using *speaker blocks* rather than *paragraphs*.
Analyst_measure4	Same as "Analyst_measure2" except using *speaker blocks* rather than *paragraphs*.
Renegotiation Index	For each contract, first construct three measures of renegotiation level following Brogaard, Denes, and Duchin (2021): (A) an "award increase" indicator that equals one if the cumulative change in potential award amount is greater than zero, (B) an "award extension" indicator that equals one if the cumulative days change in the contract end date is greater than zero, (C) and a "weak monitoring" indicator that equals one if the contract lacks incentive or performance features. Then, average these three indicator variables within each firm, and further construct the firm-level renegotiation index by summing the three variables with weights of (0.4, 0.4, 0.2).

(Continuation of Table IB.1)

Variables	Construction details
is_debtlimit	An indicator variable that equals one if a firm-quarter ends in debt limit event month and the month pior and zero otherwise.
% Changes in debt ceiling levels	Percentage change in the debt ceiling levels if a firm-quarter ends in debt limit event month and the month prior and zero otherwise.
EPU Fiscal Policy	Economic Policy Uncertainty related to fiscal policy, directly downloadable from https://www.policyuncertainty.com/categorical_epu.html.
EPU Attributed to Debt Ceiling	Economic Policy Uncertainty related to debt ceiling: EPU Attributed to Debt Ceiling $= (\frac{\text{EPU with debt ceiling}}{\text{EPU without debt ceiling}} - 1) \times \text{EPU}$, where the EPU and the ratio related to debt ceiling are directly downloadable from https://www.policyuncertainty.com/categorical_epu.html.
EPU Attributed to Government Shutdown	Economic Policy Uncertainty related to government shutdown: EPU Attributed to Government Shutdown $= (\frac{\text{EPU with government shutdown}}{\text{EPU without government shutdown}} - 1) \times \text{EPU}$, where the EPU and the ratio related to government shutdown are directly downloadable from https://www.policyuncertainty.com/categorical_epu.html
VIX	Volatility Index (source: FRED/CBOE).
Risk Aversion	Bekaert, Engstrom, and Xu (2022)'s risk aversion index (source: www.nancyxu.net).
RV	22-day realized volatility, the square root of the sum of the daily return-squared within the same month as commonly constructed in the literature (source: authors' calculation; daily S&P500 returns obtained from the DataStream; unit is the same as VIX, i.e., annual volatility percent for comparison purpose).

(End of Table IB.1)

Table IB.2: **Detailed and Subsample Regression Results for Procurement Transactions and Earnings Beat: Cross-Firm Evidence.**
This table complements Columns (3)-(4), Table 1 and shows detailed regression results with controls using the full sample and (mostly) equally-spaced subsamples: 2009-2012, 2013-2016, and 2017-2019. Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Samples:	2009-2019		2009-2012		2013-2016		2017-2019	
NAICS2 FE:		Yes		Yes		Yes		Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:				Beat				
Procurement	0.3522***	0.3181**	0.4333***	0.4005***	0.3449*	0.2911*	0.1837	0.1580
	(0.1312)	(0.1249)	(0.1283)	(0.1255)	(0.1815)	(0.1684)	(0.1923)	(0.2019)
Log(1+MarketCap)	0.0415***	0.0416***	0.0220***	0.0216***	0.0350***	0.0371***	0.0381***	0.0379***
	(0.0052)	(0.0054)	(0.0067)	(0.0071)	(0.0059)	(0.0062)	(0.0069)	(0.0073)
Log(1+Book-to-Market)	-0.0851***	-0.0181	-0.0819**	-0.0130	-0.0807**	-0.0196	-0.0883**	-0.0310
	(0.0307)	(0.0361)	(0.0402)	(0.0474)	(0.0353)	(0.0415)	(0.0431)	(0.0510)
Log(1+Ret_m61tom12)	0.9963***	0.9380**	-0.1210	-0.0541	1.4941***	1.3284***	1.2883***	1.2511***
	(0.3838)	(0.3998)	(0.3974)	(0.4047)	(0.3731)	(0.3617)	(0.3084)	(0.3274)
Log(1+Ret_m6tom2)	1.9769**	1.0859	3.0678***	2.6683***	0.9137	0.4791	0.2497	0.0017
	(0.8128)	(0.8417)	(0.9174)	(0.9535)	(0.8066)	(0.8477)	(0.8641)	(0.8722)
Log(1+InstitutionOwnPct)	0.4389***	0.3691***	0.2762***	0.2120**	0.2573***	0.1900**	0.4698***	0.3892***
	(0.0699)	(0.0778)	(0.0886)	(0.0909)	(0.0893)	(0.0945)	(0.1296)	(0.1428)
Log(1+IVOL_m11tom2)	7.4059***	4.8425**	2.1818	-0.9584	4.2074*	3.3398	6.5033**	2.7932
	(1.9031)	(2.0971)	(2.0676)	(2.4947)	(2.3361)	(2.4976)	(2.5573)	(2.8990)
Log(1+TOV_m61tom12)	-5.3240**	-4.4748*	-1.9251	-0.9724	-1.8851	-2.3719	-6.7118*	-4.8343
	(2.5714)	(2.6681)	(2.5457)	(2.7258)	(3.1223)	(3.3487)	(3.9411)	(4.1160)
Constant	-0.5962***	-0.5448***	0.0131	0.0742	-0.3500**	-0.3571**	-0.4933**	-0.4177*
	(0.1440)	(0.1520)	(0.1889)	(0.1977)	(0.1651)	(0.1731)	(0.2085)	(0.2254)
N	474	472	454	452	474	472	465	463
R^2	0.25	0.30	0.13	0.18	0.18	0.25	0.18	0.22

Table IB.3: **Procurement Transactions and Earnings Beat: Pooling Evidence.**
This table provides the regression results from Table 2 without control variables. Standard errors for Columns (1)-(5) are double-clustered at the firm and quarter levels and are reported in parentheses. Column (6) is double-clustered at the NAICS and quarter levels. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE:		Yes		Yes		Yes
NAICS2 FE:			Yes	Yes		Yes
NAICS2 x Quarter FE:					Yes	
Unit of observation:	Firm-Quarter	Firm-Quarter	Firm-Quarter	Firm-Quarter	Firm-Quarter	NAICS2-Quarter
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Beat					
Procurement	0.2722**	0.2676**	0.2716***	0.2656***	0.2624***	0.4781
	(0.1016)	(0.1005)	(0.0946)	(0.0934)	(0.0959)	(0.7051)
Constant	0.6568***	0.6569***	0.6568***	0.6569***	0.6577***	0.6151***
	(0.0100)	(0.0067)	(0.0097)	(0.0062)	(0.0063)	(0.0070)
N	16737	16737	16737	16737	16663	824
R^2	0.0014	0.011	0.014	0.023	0.070	0.18

Table IB.4: **Procurement Transactions and Earnings Beat: Within-Firm Evidence.**
This table provides detailed regression results with the full set of controls for Table 3. Standard errors for all columns are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE		Yes		Yes			Yes
NAICS2 FE			Yes	Yes			
NAICS2 x Quarter FE					Yes		
Firm FE						Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main sample							
Dependent variable:				Beat			
Procurement	0.3074***	0.2983***	0.2860***	0.2752***	0.2693***	0.1732	0.1489
	(0.0669)	(0.0648)	(0.0665)	(0.0638)	(0.0657)	(0.1041)	(0.1052)
Log(1+MarketCap)	0.0232***	0.0236***	0.0269***	0.0272***	0.0272***	0.0063	-0.0101
	(0.0036)	(0.0036)	(0.0037)	(0.0038)	(0.0038)	(0.0139)	(0.0152)
Log(1+Book-to-Market)	-0.0793***	-0.0752***	-0.0200	-0.0131	-0.0159	-0.0671*	-0.0525
	(0.0249)	(0.0265)	(0.0248)	(0.0267)	(0.0281)	(0.0373)	(0.0414)
Log(1+Ret_m61tom12)	0.1773***	0.2113***	0.1682***	0.2059***	0.1878***	0.1615***	0.1997***
	(0.0405)	(0.0365)	(0.0410)	(0.0371)	(0.0365)	(0.0384)	(0.0369)
Log(1+Ret_m6tom2)	0.6327***	0.6079***	0.6027***	0.5771***	0.5858***	0.5863***	0.5796***
	(0.1040)	(0.1053)	(0.1023)	(0.1047)	(0.1170)	(0.1017)	(0.1099)
Log(1+InstitutionOwnPct)	0.2249***	0.2584***	0.1671**	0.1978***	0.1924***	-0.1993*	-0.1453
	(0.0586)	(0.0573)	(0.0619)	(0.0603)	(0.0628)	(0.1053)	(0.1043)
Log(1+IVOL_m11tom2)	0.2834	0.1834	-0.1873	-0.3836	-0.3237	-0.7230	-1.0953*
	(0.6055)	(0.6003)	(0.5693)	(0.5337)	(0.5666)	(0.6071)	(0.5551)
Log(1+TOV_m61tom12)	0.5006	-0.3025	0.4217	-0.3888	-0.4311	1.4622	0.2451
	(1.1470)	(1.1509)	(1.2284)	(1.2030)	(1.2324)	(1.0575)	(0.9990)
L.Beat	0.1581***	0.1533***	0.1504***	0.1454***	0.1498***	0.0705***	0.0647***
	(0.0105)	(0.0106)	(0.0107)	(0.0107)	(0.0114)	(0.0104)	(0.0104)
Constant	-0.0827	-0.1021	-0.1407	-0.1555	-0.1524	0.6109*	0.9583**
	(0.0932)	(0.0941)	(0.0941)	(0.0960)	(0.0964)	(0.3199)	(0.3607)
N	16696	16696	16696	16696	16622	16696	16696
R^2	0.048	0.056	0.055	0.063	0.11	0.12	0.13
Panel B: Sample using firms that have transactions almost all quarters							
Dependent variable:				Beat			
Procurement	0.2534***	0.2375***	0.2417***	0.2237***	0.2177***	0.2174**	0.1719**
	(0.0623)	(0.0619)	(0.0602)	(0.0589)	(0.0598)	(0.0827)	(0.0832)
Log(1+MarketCap)	0.0211***	0.0221***	0.0234***	0.0243***	0.0242***	-0.0022	-0.0226
	(0.0044)	(0.0046)	(0.0047)	(0.0050)	(0.0050)	(0.0132)	(0.0178)
Log(1+Book-to-Market)	-0.1157***	-0.1087***	-0.0623**	-0.0518	-0.0566	-0.1059**	-0.0816*
	(0.0305)	(0.0320)	(0.0300)	(0.0315)	(0.0337)	(0.0413)	(0.0451)
Log(1+Ret_m61tom12)	0.1800***	0.2331***	0.1704***	0.2278***	0.2038***	0.1666***	0.2254***
	(0.0504)	(0.0411)	(0.0510)	(0.0417)	(0.0413)	(0.0480)	(0.0434)
Log(1+Ret_m6tom2)	0.7156***	0.6456***	0.6905***	0.6224***	0.6384***	0.6540***	0.6140***
	(0.1019)	(0.0958)	(0.0984)	(0.0929)	(0.1094)	(0.1025)	(0.0993)
Log(1+InstitutionOwnPct)	0.2602***	0.3124***	0.1691*	0.2155**	0.2182**	-0.2128	-0.1467
	(0.0829)	(0.0801)	(0.0886)	(0.0873)	(0.0900)	(0.1456)	(0.1283)
Log(1+IVOL_m11tom2)	0.7217	0.5880	0.2189	-0.0083	-0.0453	-0.5792	-0.9863
	(0.7166)	(0.6696)	(0.7004)	(0.6279)	(0.6860)	(0.7146)	(0.6049)
Log(1+TOV_m61tom12)	-0.0433	-0.9642	-0.1626	-1.0845	-1.0027	0.6822	-0.5810
	(1.5534)	(1.5486)	(1.6406)	(1.6106)	(1.6829)	(1.1680)	(1.0682)
L.Beat	0.1513***	0.1466***	0.1419***	0.1370***	0.1422***	0.0605***	0.0546***
	(0.0127)	(0.0127)	(0.0126)	(0.0125)	(0.0130)	(0.0122)	(0.0117)
Constant	-0.0272	-0.0728	-0.0308	-0.0688	-0.0711	0.8523**	1.2816***
	(0.1279)	(0.1295)	(0.1320)	(0.1383)	(0.1389)	(0.3283)	(0.4268)
N	12046	12046	12046	12046	11941	12046	12046
R^2	0.046	0.055	0.054	0.064	0.12	0.12	0.13

Table IB.5: **Detailed Mechanism Test and Robustness Check: Renegotiation and Bargaining Power with the Government.**
This table provides detailed regression results and a robustness check with the full set of controls for Columns (1)-(2), Table 5. Columns (1)-(2) apply equal weights to aggregate the "award increase," "award extension," and "weak monitoring" indicators to construct the firm-level renegotiation index. Columns (3)-(4) use the same (0.4, 0.4, 0.2) weighting scheme as in Table 5. Robust standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

NAICS2 FE		Yes		Yes
With controls:	Yes	Yes	Yes	Yes
Contract Renegotiation – Award Increase:	Yes	Yes	Yes	Yes
Contract Renegotiation – Contract Extension:	Yes	Yes	Yes	Yes
Contract Renegotiation – Weaker Monitoring:	Yes	Yes	Yes	Yes
Index weighted scheme:	Equal	Equal	0.4,0.4,0.2	0.4,0.4,0.2
	(1)	(2)	(3)	(4)
Dependent variable:		Beat		
Procurement	1.7487*	2.0109**	1.8460***	1.8182***
	(0.9085)	(0.8219)	(0.6992)	(0.6304)
Renegotiation Index	0.0171	-0.0484	-0.1135	-0.1372
	(0.1129)	(0.1412)	(0.1398)	(0.1697)
Procurement × RenegotiationIndex	-4.0655	-4.9453**	-5.7224**	-5.7493**
	(2.7619)	(2.4444)	(2.8137)	(2.5154)
Log(1+MarketCap)	0.0404***	0.0401***	0.0395***	0.0395***
	(0.0053)	(0.0054)	(0.0052)	(0.0054)
Log(1+Book-to-Market)	-0.0872***	-0.0189	-0.0786***	-0.0151
	(0.0302)	(0.0361)	(0.0301)	(0.0356)
Log(1+Ret_m61tom12)	1.0016***	0.9344**	0.9525**	0.8756**
	(0.3804)	(0.3969)	(0.3817)	(0.3963)
Log(1+Ret_m6tom2)	2.1276***	1.2488	1.9340**	1.0573
	(0.8211)	(0.8578)	(0.8141)	(0.8463)
Log(1+InstitutionOwnPct)	0.4320***	0.3558***	0.4114***	0.3448***
	(0.0698)	(0.0775)	(0.0700)	(0.0774)
Log(1+IVOL_m11tom2)	7.0614***	4.3810**	6.9732***	4.5743**
	(1.9056)	(2.0888)	(1.8950)	(2.0985)
Log(1+TOV_m61tom12)	-5.2535**	-4.3573	-4.9687*	-4.1012
	(2.5823)	(2.6921)	(2.6062)	(2.7038)
Constant	-0.5683***	-0.4808***	-0.5077***	-0.4502***
	(0.1539)	(0.1611)	(0.1518)	(0.1580)
N	473	471	473	471
R^2	0.25	0.31	0.26	0.31

Table IB.6: **Debt Ceiling Events.**
This table summarizes all debt ceiling events (source: https://www.whitehouse.gov/omb/budget/historical-tables/, Table 7.3). Gray rows indicate debt ceiling logs that are mentioned in the White House records but result in zero change in the debt limit; we do not focus on these days in our analysis.

Debt Ceiling Date	Description	% Increase
2009/2/17	Increased the debt limit to $12104 billions.	6.97%
2009/12/28	Increased the debt limit to $12394 billions.	2.40%
2010/2/12	Increased the debt limit to $14294 billions.	15.33%
2011/8/2	Increased the debt limit to $14694 billions.	2.80%
2011/9/21	Effective after September 21, 2011, increased the debt limit to $15194 billions.	3.40%
2012/1/27	Effective after January 27, 2012, increased the debt limit to $16394 billions.	7.90%
2013/2/4	Suspended the existing debt limit from February 4, 2013, through May 18, 2013, and prospectively increased the limit to $16999.4 billions to accommodate the increase in such debt outstanding as of May 19, 2013.	3.69%
2013/5/19	Effective May 19, 2013, reestablished the debt limit at $16699.4 billions.	-1.76%
2013/10/17	Suspended the existing debt limit from October 17, 2013, through February 7, 2014, and prospectively increased the limit to $17211.6 billions to accommodate the increase in such debt outstanding as of February 8, 2014.	3.07%
2014/2/8	Effective February 8, 2014, reestablished the debt limit at $17211.6 billions.	0.00%
2014/2/15	Suspended the existing debt limit from February 15, 2014, through March 15, 2015, and prospectively increased the limit to $18113 billions accommodate the increase in such debt outstanding as of March 16, 2015.	5.24%
2015/3/16	Effective March 16, 2015, reestablished the debt limit at $18113 billions.	0.00%
2015/11/2	Suspended the existing debt limit from November 2, 2015, through March 15, 2017, and prospectively increased the limit to $19808.8 billions accommodate the increase in such debt outstanding as of March 16, 2017.	9.36%
2017/3/16	Effective March 16, 2017, reestablished the debt limit at $19808.8 billions.	0.00%
2017/9/8	Suspended the existing debt limit from September 8, 2017, through December 8, 2017, and prospectively increased the limit to $20456 billions to accommodate the increase in such debt outstanding as of December 9, 2017.	3.27%
2017/12/9	Effective December 9, 2017, reestablished the debt limit at $20456 billions.	0.00%
2018/2/9	Suspended the existing debt limit from February 9, 2018, through March 1, 2019, and prospectively increased the limit to $21987.7 billions to accommodate the increase in such debt outstanding as of March 1, 2019.	7.49%
2019/3/1	Effective March 1, 2019, reestablished the debt limit at $21987.7 billions.	0.00%
2019/8/2	Suspended the existing debt limit from August 2, 2019, through July 31, 2021, and prospectively increased the limit to $28401.5 billions to accommodate the increase in such debt outstanding as of July 31, 2021.	29.17%

Table IB.7: **Mechanism Test: Budgetary Uncertainty Triggered by Debt Ceiling Events.**
This table provides detailed regression results including the control variables for Table 7, Panel A. "is_debtlimit" is an indicator variable that equals one if a firm-quarter ends in a debt limit event month or the month prior (source: whitehouse.gov) and zero otherwise. Standard errors are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE			Yes			Yes			Yes
NAICS2 FE									
NAICS2 times Quarter FE	Yes			Yes			Yes		
Firm FE		Yes	Yes		Yes	Yes		Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Beat	Beat	Beat	SUE_1	SUE_1	SUE_1	SUE_2	SUE_2	SUE_2
Procurement	0.2591***	0.1380	0.1139	2.3031**	0.7296	0.4370	0.1773	-0.0960	-0.1213
	(0.0734)	(0.1002)	(0.1020)	(0.9392)	(1.0804)	(1.0911)	(0.1131)	(0.2334)	(0.2278)
is_debtlimit	-0.0072	-0.0125	-0.0112	0.0100	-0.1598*	-0.0549	-0.0079	-0.0231*	-0.0107
	(0.0267)	(0.0120)	(0.0246)	(0.3079)	(0.0918)	(0.2403)	(0.0209)	(0.0133)	(0.0190)
Procurement × is_debtlimit	0.0621	0.1654***	0.1488***	1.8357**	2.4602***	2.4471***	0.2015**	0.2692**	0.2453**
	(0.0773)	(0.0371)	(0.0438)	(0.7773)	(0.8204)	(0.8500)	(0.0906)	(0.1111)	(0.1002)
Log(1+MarketCap)	0.0272***	0.0055	-0.0102	0.0860***	0.0618	-0.0525	-0.0053	-0.0530***	-0.0403*
	(0.0038)	(0.0140)	(0.0152)	(0.0290)	(0.1359)	(0.1330)	(0.0040)	(0.0173)	(0.0215)
Log(1+Book-to-Market)	-0.0160	-0.0692*	-0.0526	-0.3477	-0.8012*	-0.6841*	0.0170	-0.1286	-0.0937
	(0.0282)	(0.0377)	(0.0412)	(0.2364)	(0.4001)	(0.3749)	(0.0503)	(0.1057)	(0.1121)
Log(1+Ret_m61tom12)	0.1881***	0.1612***	0.1996***	1.3394***	1.3076***	1.6137***	0.1579**	0.1089*	0.1761**
	(0.0363)	(0.0382)	(0.0367)	(0.3585)	(0.3561)	(0.3139)	(0.0697)	(0.0642)	(0.0698)
Log(1+Ret_m6tom2)	0.5854***	0.5889***	0.5788***	4.5977***	4.5955***	4.6360***	0.7764***	0.7433**	0.7945**
	(0.1168)	(0.1014)	(0.1098)	(1.0323)	(0.9370)	(0.9626)	(0.2824)	(0.3080)	(0.2974)
Log(1+InstitutionOwnPct)	0.1921***	-0.2008*	-0.1468	1.4924***	-2.3836***	-2.0828***	-0.0595	-0.3018*	-0.1707
	(0.0628)	(0.1053)	(0.1045)	(0.5073)	(0.8109)	(0.7108)	(0.0521)	(0.1739)	(0.1764)
Log(1+IVOL_m11tom2)	-0.3282	-0.7102	-1.1026*	-11.5862**	-10.9789**	-15.1597**	-2.1602	-2.2480*	-2.9607**
	(0.5616)	(0.6217)	(0.5479)	(5.5015)	(4.9325)	(5.7342)	(1.4032)	(1.2003)	(1.3597)
Log(1+TOV_m61tom12)	-0.4314	1.4700	0.2474	0.5309	28.1840***	21.1830**	3.8561**	3.8759	2.2784
	(1.2330)	(1.0551)	(1.0016)	(10.0757)	(9.6173)	(9.5072)	(1.8442)	(2.5399)	(2.4437)
L.Beat	0.1497***	0.0704***	0.0645***						
	(0.0114)	(0.0104)	(0.0104)						
L.SUE2				0.2081***	0.1198***	0.1151***			
				(0.0152)	(0.0150)	(0.0152)			
L.SUE5							0.0712	0.0036	0.0008
							(0.0540)	(0.0516)	(0.0509)
Constant	-0.1512	0.6322*	0.9614**	-1.5983*	1.3453	3.7851	0.2010	1.4795***	1.1258**
	(0.0962)	(0.3248)	(0.3614)	(0.8142)	(3.1171)	(2.9940)	(0.1217)	(0.4332)	(0.5327)
N	16622	16696	16696	16218	16297	16297	16316	16390	16390
R^2	0.11	0.12	0.13	0.12	0.15	0.16	0.077	0.084	0.091

Table IB.8: **Mechanism Test: Budgetary Uncertainty Triggered by Debt Ceiling Events.**
This table provides detailed regression results including the control variables for Table 7, Panel B. The interaction term is the percentage change in the debt ceiling levels if a firm-quarter ends in a debt limit event month or the month prior and is zero otherwise. Standard errors are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE			Yes			Yes			Yes
NAICS2 FE									
NAICS2 times Quarter FE	Yes			Yes			Yes		
Firm FE		Yes	Yes		Yes	Yes		Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Beat	Beat	Beat	SUE_1	SUE_1	SUE_1	SUE_2	SUE_2	SUE_2
Procurement	0.2562***	0.1450	0.1218	2.4243**	0.9555	0.7110	0.1850*	-0.0746	-0.0977
	(0.0705)	(0.1021)	(0.1035)	(0.9268)	(1.1093)	(1.1337)	(0.1074)	(0.2263)	(0.2217)
% Changes in debt ceiling levels	0.0009	-0.0003	0.0010	0.0291	0.0030	0.0225	0.0012	-0.0019	0.0021
	(0.0020)	(0.0023)	(0.0020)	(0.0252)	(0.0246)	(0.0223)	(0.0021)	(0.0028)	(0.0020)
Procurement × % Changes in debt ceiling levels	0.0167**	0.0266***	0.0261***	0.2321***	0.2897***	0.2935***	0.0315**	0.0329*	0.0316*
	(0.0075)	(0.0073)	(0.0077)	(0.0732)	(0.0894)	(0.1001)	(0.0152)	(0.0180)	(0.0175)
Log(1+MarketCap)	0.0272***	0.0064	-0.0100	0.0863***	0.0764	-0.0508	-0.0053	-0.0518***	-0.0400*
	(0.0038)	(0.0140)	(0.0152)	(0.0290)	(0.1382)	(0.1334)	(0.0040)	(0.0174)	(0.0215)
Log(1+Book-to-Market)	-0.0159	-0.0662*	-0.0514	-0.3438	-0.7567*	-0.6689*	0.0171	-0.1250	-0.0920
	(0.0281)	(0.0373)	(0.0411)	(0.2351)	(0.4003)	(0.3746)	(0.0502)	(0.1053)	(0.1120)
Log(1+Ret_m61tom12)	0.1869***	0.1612***	0.1986***	1.3267***	1.3054***	1.6058***	0.1567**	0.1094*	0.1745**
	(0.0364)	(0.0383)	(0.0368)	(0.3593)	(0.3566)	(0.3173)	(0.0697)	(0.0639)	(0.0700)
Log(1+Ret_m6tom2)	0.5871***	0.5855***	0.5811***	4.6403***	4.5472***	4.6737***	0.7788***	0.7384**	0.7978**
	(0.1166)	(0.1021)	(0.1094)	(1.0229)	(0.9437)	(0.9573)	(0.2819)	(0.3079)	(0.2970)
Log(1+InstitutionOwnPct)	0.1926***	-0.2008*	-0.1450	1.5041***	-2.3788***	-2.0521***	-0.0585	-0.3014*	-0.1673
	(0.0630)	(0.1054)	(0.1048)	(0.5068)	(0.8141)	(0.7112)	(0.0521)	(0.1741)	(0.1763)
Log(1+IVOL_m11tom2)	-0.3145	-0.7262	-1.0889*	-11.4874**	-11.2195**	-15.0790**	-2.1477	-2.2640*	-2.9476**
	(0.5659)	(0.6122)	(0.5568)	(5.5040)	(4.9230)	(5.7208)	(1.3985)	(1.1928)	(1.3585)
Log(1+TOV_m61tom12)	-0.4379	1.4634	0.2534	0.4502	28.0842***	21.1810**	3.8442**	3.8602	2.2810
	(1.2323)	(1.0552)	(0.9973)	(10.0841)	(9.6870)	(9.4791)	(1.8457)	(2.5336)	(2.4379)
L.Beat	0.1497***	0.0705***	0.0646***						
	(0.0114)	(0.0105)	(0.0104)						
L.SUE2				0.2078***	0.1199***	0.1150***			
				(0.0150)	(0.0150)	(0.0151)			
L.SUE5							0.0712	0.0036	0.0008
							(0.0540)	(0.0515)	(0.0509)
Constant	-0.1537	0.6088*	0.9539**	-1.6288*	0.9812	3.7005	0.1976	1.4507***	1.1141**
	(0.0965)	(0.3244)	(0.3618)	(0.8165)	(3.1734)	(3.0020)	(0.1210)	(0.4324)	(0.5334)
N	16622	16696	16696	16218	16297	16297	16316	16390	16390
R^2	0.11	0.12	0.13	0.12	0.15	0.16	0.077	0.084	0.091

Table IB.9: **Mechanism Test: Budgetary Uncertainty, Triggered by Debt Ceiling Events.**
This table provides detailed regression results including the control variables for Panel C, Table 7. The interaction term uses the monthly average EPU attributed to debt ceiling mentions in the news article, which is constructed from Baker, Bloom, and Davis (2016)'s Economic Policy Uncertainty variables. EPU variables are directly downloadable from https://www.policyuncertainty.com/categorical_epu.html. Standard errors are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE			Yes			Yes			Yes
NAICS2 FE									
NAICS2 times Quarter FE	Yes			Yes			Yes		
Firm FE		Yes	Yes		Yes	Yes		Yes	Yes
With Controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Beat	Beat	Beat	SUE_1	SUE_1	SUE_1	SUE_2	SUE_2	SUE_2
Procurement	0.2471***	0.1356	0.1154	2.3558**	0.7866	0.5349	0.1940*	-0.0767	-0.0956
	(0.0708)	(0.1042)	(0.1068)	(0.9678)	(1.1033)	(1.1297)	(0.1037)	(0.2279)	(0.2240)
Monthly Average EPU Attributed to Debt Ceiling	-0.0051	-0.0022	-0.0010	0.0926	-0.0354	0.0993	-0.0153	-0.0012	-0.0144
	(0.0059)	(0.0027)	(0.0096)	(0.0678)	(0.0279)	(0.0641)	(0.0148)	(0.0034)	(0.0174)
Procurement × Monthly Average EPU Attributed to Debt Ceiling	0.0274***	0.0306***	0.0256***	0.2995***	0.4000***	0.3543***	0.0202	0.0293	0.0254
	(0.0079)	(0.0074)	(0.0061)	(0.0961)	(0.0947)	(0.0966)	(0.0167)	(0.0217)	(0.0222)
Log(1+MarketCap)	0.0272***	0.0056	-0.0101	0.0860***	0.0599	-0.0536	-0.0053	-0.0515***	-0.0400*
	(0.0038)	(0.0141)	(0.0152)	(0.0289)	(0.1363)	(0.1321)	(0.0040)	(0.0177)	(0.0215)
Log(1+Book-to-Market)	-0.0158	-0.0680*	-0.0523	-0.3434	-0.7944*	-0.6800*	0.0171	-0.1241	-0.0937
	(0.0281)	(0.0376)	(0.0413)	(0.2362)	(0.4000)	(0.3735)	(0.0502)	(0.1054)	(0.1121)
Log(1+Ret_m61tom12)	0.1876***	0.1605***	0.1996***	1.3408***	1.2927***	1.6144***	0.1587**	0.1087	0.1775**
	(0.0367)	(0.0387)	(0.0371)	(0.3611)	(0.3610)	(0.3189)	(0.0699)	(0.0649)	(0.0699)
Log(1+Ret_m6tom2)	0.5869***	0.5887***	0.5802***	4.5892***	4.6125***	4.6342***	0.7789***	0.7374**	0.7972**
	(0.1173)	(0.1009)	(0.1103)	(1.0370)	(0.9330)	(0.9669)	(0.2827)	(0.3056)	(0.2975)
Log(1+InstitutionOwnPct)	0.1924***	-0.2004*	-0.1456	1.4970***	-2.3784***	-2.0649***	-0.0594	-0.3004*	-0.1699
	(0.0628)	(0.1052)	(0.1041)	(0.5093)	(0.8089)	(0.7129)	(0.0520)	(0.1743)	(0.1757)
Log(1+IVOL_m11tom2)	-0.3116	-0.6721	-1.0913*	-11.6383**	-10.1463*	-15.2048**	-2.1514	-2.2653*	-2.9493**
	(0.5676)	(0.6301)	(0.5549)	(5.5361)	(5.1939)	(5.7394)	(1.3971)	(1.2725)	(1.3556)
Log(1+TOV_m61tom12)	-0.4314	1.4952	0.2471	0.3677	28.7369***	20.6927**	3.8794**	3.8569	2.3293
	(1.2330)	(1.0555)	(1.0057)	(10.0296)	(9.8234)	(9.3860)	(1.8461)	(2.5646)	(2.4373)
L.Beat	0.1497***	0.0705***	0.0646***						
	(0.0114)	(0.0104)	(0.0104)						
L.SUE2				0.2082***	0.1198***	0.1152***			
				(0.0152)	(0.0150)	(0.0152)			
L.SUE5							0.0710	0.0036	0.0005
							(0.0540)	(0.0515)	(0.0509)
Constant	-0.1509	0.6267*	0.9578**	-1.6443*	1.3652	3.7475	0.2067*	1.4434***	1.1246**
	(0.0959)	(0.3266)	(0.3601)	(0.8184)	(3.1227)	(2.9576)	(0.1207)	(0.4390)	(0.5300)
N	16622	16696	16696	16218	16297	16297	16316	16390	16390
R^2	0.11	0.12	0.13	0.12	0.15	0.16	0.077	0.084	0.091

Table IB.10: **Mechanism Test and Alternative Analyst Mention Measures: Lack of Analyst Attention to Government Contracts.**

This table complements Table 8 and provides the regression results with controls at the firm-quarter level. For each earnings call transcript (firm-time level), we first construct two measures of analyst mentions of government: (A) the number of words in paragraphs spoken by analysts that mention "government contracts" or "procurement contracts" divided by the total number of words in the transcript excluding operator words and (B) the number of procurement-related analyst words divided by total number of analyst words. Then for each firm-quarter, Analyst_measure1 is the average of (A) and Analyst_measure2 is the average of (B). Analyst_measure3 and Analyst_measure4 are constructed similarly, except that they use speaker blocks rather than paragraphs. Standard errors are double-clustered at the firm and quarter levels and are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

Year-Calendar Quarter FE			Yes			Yes			Yes			Yes
NAICS2 FE												
NAICS2 times Quarter FE	Yes			Yes			Yes			Yes		
Firm FE		Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes
With Controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable:						Beat						
Procurement	0.2667***	0.1665**	0.1492*	0.2652***	0.1650**	0.1468*	0.2668***	0.1665**	0.1493*	0.2653***	0.1650**	0.1469*
	(0.0759)	(0.0822)	(0.0840)	(0.0759)	(0.0802)	(0.0825)	(0.0759)	(0.0821)	(0.0840)	(0.0759)	(0.0802)	(0.0825)
Analyst_mention1	0.8451	0.8698	1.1311									
	(0.9874)	(0.9438)	(0.9501)									
Procurement × Analyst_mention1	0.7055	7.9101	7.2993									
	(6.6845)	(5.4550)	(5.8628)									
Analyst_mention2				0.1746	0.1537	0.1808						
				(0.1556)	(0.1465)	(0.1472)						
Procurement × Analyst_mention2				0.0179	1.3298	1.2643						
				(1.0347)	(0.9096)	(0.9782)						
Analyst_mention3							0.8453	0.8689	1.1291			
							(0.9860)	(0.9420)	(0.9482)			
Procurement × Analyst_mention3							0.6712	7.8719	7.2595			
							(6.6691)	(5.4525)	(5.8578)			
Analyst_mention4										0.1746	0.1536	0.1806
										(0.1554)	(0.1463)	(0.1470)
Procurement × Analyst_mention4										0.0121	1.3240	1.2582
										(1.0330)	(0.9090)	(0.9773)
N	16298	16347	16347	16261	16317	16317	16298	16347	16347	16261	16317	16317
R^2	0.11	0.13	0.13	0.11	0.13	0.13	0.11	0.13	0.13	0.11	0.13	0.13