



Federal Reserve  
Bank of Dallas

# Uncertainty, Stock Prices, and Debt Structure: Evidence from the U.S.-China Trade War

---

Ali Ozdagli and Jianlin Wang

**Working Paper 2212**

**August 2022**

Research Department

<https://doi.org/10.24149/wp2212>

Working papers from the Federal Reserve Bank of Dallas are preliminary drafts circulated for professional comment. The views in this paper are those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of Dallas or the Federal Reserve System. Any errors or omissions are the responsibility of the authors.

# Uncertainty, Stock Prices, and Debt Structure: Evidence from the U.S.-China Trade War\*

Ali Ozdagli<sup>†</sup> and Jianlin Wang<sup>‡</sup>

July 2022

## Abstract

Using the recent U.S.-China trade war as a laboratory, we show that policy uncertainty shocks have a significant impact on stock prices. This impact is less negative for firms that heavily rely on bank debt whereas non-bank debt does not have a mitigating effect. Moreover, the mitigating effect of bank debt is concentrated among zombie firms. A zombie firm that derives half of its capital from bank debt has no negative stock price reaction to increased uncertainty. These results are consistent with bank debt providing insurance for zombie firms in bad economic times.

**Keywords:** Policy uncertainty; asset prices; debt structure; zombie firms; trade war

**JEL Codes:** E44, F13, G12, G20, G30

---

\* We thank José Fillat, Scott Frame, Pierre-Olivier Gourinchas, Victoria Ivashina, Haoyang Liu, Sebnem Kalemli-Ozcan, Chen Lian, Ulrike Malmendier, Maurice Obstfeld, Giovanni Olivei, Joe Peek, Walker Ray, Geoff Tootell, and seminar participants for feedback at various stages. Monica Barbosa provided excellent research assistance. The views expressed herein are those of the authors and do not indicate concurrence by the Federal Reserve Bank of Dallas, the principals of the Board of Governors, or the Federal Reserve System.

<sup>†</sup>Ali Ozdagli, Research Department, Federal Reserve Bank of Dallas, [ali.ozdagli@dal.frb.org](mailto:ali.ozdagli@dal.frb.org).

<sup>‡</sup>Jianlin Wang, Department of Economics, University of California at Berkeley, [jianlinw@berkeley.edu](mailto:jianlinw@berkeley.edu).

# 1 Introduction

Uncertainty has been at the forefront of discussions among policymakers, academics, and investors for at least the last two decades. The Federal Open Market Committee (FOMC) minutes repeatedly emphasize uncertainty as a key factor in every recession since 2000. Recent macroeconomics and finance research studies the role of uncertainty in business cycles and its effect on financial markets (Pastor and Veronesi 2012; Basu and Bundick 2017; Bloom et al. 2018). At the same time, a parallel strand of literature examines debt structure (Rauh and Sufi 2010; Colla et al. 2013) and shows its effect on the transmission of macroeconomic shocks (Ippolito et al. 2018, Gurkaynak et al. 2021). Given the importance of macroeconomic uncertainty for the financial markets and the influence of debt structure on the transmission of macroeconomic shocks, a natural question is whether and how debt structure matters in the transmission of uncertainty shocks.

We fill this gap by studying the effect of uncertainty shocks on stock prices. To identify the impact of exogenous changes in uncertainty, we investigate the U.S. and China trade war in 2018 and 2019, which has created a large fluctuation in financial markets. The effect of trade war on financial markets is widely attributed to increased uncertainty (Carney 2019). Despite the plethora of channels through which the trade war can be transmitted to the macroeconomy, the estimated total effects coming from these channels have been relatively small in comparison to the large reaction of financial markets.<sup>1</sup> Accordingly, former U.S. Treasury Secretary Summers commented that the large gyrations in financial markets that are attributed to the trade war are puzzling (Summers 2019). Nevertheless, policy uncertainty associated with trade war can raise risk premia (Pastor and Veronesi 2012; Bianconi, Esposito, and Sammon 2021), thereby tightening financial conditions as argued by

---

<sup>1</sup>For example, a report by Goldman Sachs estimates that the total peak effect from all macroeconomic channels will amount to 0.2 percent of GDP (Hatzius et al. 2019), which is in line with the estimates reported by Amiti et al. (2019) and Fajgelbaum et al. (2020, 2021).

Bank of England Governor Mark Carney.<sup>2</sup>

We find that the increased uncertainty due to the trade war has had a significant negative impact on stock prices. Specifically, a trade war news that raises the Chicago Board Options Exchange’s Volatility Index (VIX) by two percentage points per annum (one standard deviation change) reduces stock prices by about 0.9 percent.<sup>3</sup> However, the impact is less negative for firms that heavily rely on bank debt whereas non-bank debt does not have a mitigating effect. The mitigating effect of bank debt is concentrated among the so-called zombie firms, defined as mature firms that are persistently unable to generate enough profits to cover their interest expenses. These firms have recently attracted growing attention from academic and policy circles (Favara, Minoiu, and Perez-Orive 2021) as well as public media, due to their potentially influential role in depressing economic growth, especially when bank credit is directed to keep the firms afloat.<sup>4</sup> We find that a zombie firm that derives half of its capital from bank debt has no negative stock price reaction to increased uncertainty, suggesting that bank debt plays an important role in insulating zombie firms from uncertainty shocks.

Though many factors can affect a firm’s risk premium, the timing and magnitude of exogenous shocks to these factors are often difficult to measure. Our paper uses the U.S.-China trade war as a unique laboratory to address this concern by systematically collecting a series of trade-war-related news, the precise timing of which is unexpected. Since news

---

<sup>2</sup>While Fajgelbaum et al. (2020) and Amiti et al. (2019) do not consider the impacts of trade policy uncertainty, the latter acknowledges that “higher uncertainty may be reflected in the substantial falls in equity markets around the time of some of the most important trade policy announcements.” Hatzius et al. (2019) estimates an additional 0.4 percent drop in GDP due to financial conditions.

<sup>3</sup>Our choice of VIX as the uncertainty measure is consistent with previous literature, such as Bloom (2009) and Basu and Bundick (2017). The measured effect is quantitatively important as it is equivalent to the Federal Reserve increasing the federal funds target rate by about 25 basis points (Bernanke and Kuttner 2005; Gurkaynak et al. 2005a).

<sup>4</sup>In public media, Bloomberg reports that 739 firms of the Russel 3000 index with around \$1.98 trillion debt could not cover their interest expenses by the end of 2020 based on the trailing 12-month operating income. See <https://www.bloomberg.com/news/articles/2020-11-17/america-s-zombie-companies-have-racked-up-1-4-trillion-of-debt>.

about the trade war are reflected in market prices quickly, we are able to identify the effect of the exogenous uncertainty shocks due to trade war news by using the heteroskedasticity-based estimator of Rigobon and Sack (2003, 2004, 2005) with high-frequency financial market data. This estimator addresses the two big identification challenges that (i) trade-war-related uncertainty is not directly observable, in that the trade war news on any given day cannot be precisely quantified, and (ii) other factors are continuously influencing asset prices in addition to trade war uncertainty.

Based on our identification strategy, we first check if the exogenous uncertainty shocks created by the trade war news between January 2018 and August 2019 have left a hefty footprint on the US financial markets. The event dates associated with the trade war news explain three quarters of the increase in high-yield spreads and about half of the decline in yields on long-term Treasury debt during our sample period. We find that a trade war news that raises VIX by two percentage points per annum (one standard deviation) increases high-yield spreads by 7 basis points, decreases the 10-year Treasury rate by 2.6 basis points, and lowers stock prices by 0.9 percent. The effect on the stock market is quantitatively important as it is equivalent to the Federal Reserve increasing the federal funds target rate by 25 basis points (Bernanke and Kuttner 2005; Gurkaynak et al. 2005a). As an additional verification that we indeed capture trade-war-related shocks, we find that firms are more responsive to trade war news if they derive more of their revenue from China but not if they derive more of their revenue from the U.S. or from other foreign countries. Although our paper is not the first to document an adverse impact of the trade war on U.S. asset prices, our paper is, to the best of our knowledge, the first to establish and quantify the causal relationship between the exogenous uncertainty shocks associated the trade war news and the gyrations in the U.S. financial markets.

Given the negative effect of the uncertainty shocks on the stock prices, we next ask if debt structure, specifically bank vs. non-bank debt, can play a special role in modifying

this impact. Bank debt can have two opposing effects under macroeconomic shocks. On the one hand, worsened economic conditions can substantially reduce loan supply by banks and thereby amplify the negative effect on the borrowers relying more on bank debt (Jimenez et al. 2012). This is the so-called bank balance-sheet channel. On the other hand, the informational advantage of banks and the relationship between banks and their borrowers can insulate bank-dependent borrowers from negative shocks by providing flexibility during bad economic times due to ease of renegotiation (Berlin and Mester 1992; Hadlock and James 2002). We call this the financial flexibility channel.

We decompose a firm's book leverage in 2017 into the leverage made of bank debt and non-bank debt, taking advantage of the U.S. disclosure requirements for public firms. In line with the argument that bank debt can offer financial flexibility during periods of distress, we find that a firm's stock price is less responsive to trade war news if the firm relies more heavily on bank debt as a source of capital. Specifically, a one standard deviation increase in bank debt usage, defined as the ratio of bank debt to total assets, can mitigate around 6% of the impact on a firm's stock value.

Next, we shed more light on the mechanism through which the financial flexibility channel operates. We start by noting that borrowers that value the option to renegotiate their debt are more likely to benefit from relying on bank debt. A natural candidate for such firms is the group of zombie firms, which are those firms persistently unable to generate enough profits to cover their interest expenses. These firms have attracted growing attention from academic and policy circles.<sup>5</sup> The share of zombie firms has been trending up across advanced economies, from below 2% of non-financial firms in the 1980s to around 12% nowadays, through upswings in the wake of economic downturns that are not fully reversed in subsequent recoveries.<sup>6</sup> For the United States, a recent note from the Federal Reserve

---

<sup>5</sup>See, for example, Caballero et al. (2008), Gouveia and Osterhold (2018), Blattner et al. (2019), Acharya et al. (2019, 2020).

<sup>6</sup>See Adalet McGowan et al. (2018) and Banerjee and Hofmann (2018).

Board (Favara et al. 2021) suggests that around 10% of the public non-financial firms in 2020 are zombie firms, a number that can increase in the post-pandemic environment due to unprecedented fiscal and monetary policy support.<sup>7</sup>

Following previous literature, we define a zombie firm as a firm that is mature and has an interest coverage ratio (ICR) less than one for three years leading to 2017.<sup>8</sup> For these firms, a one standard deviation increase in bank debt usage can mitigate around 45% of the negative impact on the stock value. Moreover, the mitigating effect of bank debt is economically small and statistically insignificant for the non-zombie firms.

These results not only support the financial flexibility channel, but they also rule out other potential explanations for the mitigating effect of bank debt. For example, one could argue that banks may be more risk-averse than other investors due to regulations or banks may be better at picking winners due to their informational advantage, and therefore lend to firms that are less sensitive to macroeconomic news in general. If the mitigating effect of bank debt were driven by banks' greater risk aversion or ability to pick winners, such differences would also show up for non-zombie firms and bank debt usage would also have a similar effect on the responsiveness of non-zombie firms, which is not the case in the data. These results become more striking when contrasted with non-bank debt usage. In contrast to bank debt usage, higher non-bank debt usage is associated with greater responsiveness to trade war news and zombie status does not have any effect on this association. To the best of our knowledge, the literature on zombie firms typically does not have data on the non-bank debt, which makes our data and result unique in this literature, as the lack of result for the

---

<sup>7</sup>The earlier literature focused on zombie lending as a mechanism for avoiding the recognition of a loan loss by a financially weak bank, thereby worsening inefficient allocation of funds. Recent research puts a more positive spin on this issue, suggesting that it may be optimal for even a financially strong bank to evergreen the loan when the bank has market power and internalizes the effect of its choices on the borrowing firm's policies (Faria-e-Castro, Paul, and Sanchez 2021).

<sup>8</sup>See Adalet McGowan et al. (2018), Banerjee and Hofmann (2018, 2020), Acharya et al (2020). The ICR is defined as the ratio of earnings before interest, taxes, depreciation and amortization (EBITDA) over interest expenses. Our results are robust when the ICR is defined as earnings before interest and taxes (EBIT) over interest expenses instead.

non-bank debt provides a falsification test.

Our results are robust to controlling various firm-level characteristics that have been shown to affect the responsiveness of stock prices to macroeconomic shocks, including Tobin's Q, firm size, balance sheet liquidity, along with firm and industry-date fixed effects. We further show that our results are not driven by changes in firms' debt structure in anticipation of a trade war, using the firm-level debt structure before Donald Trump's presidency as an instrument. Additionally, our results remain consistent when we restrict our sample to firms with a zero revenue share from China, suggesting that the revenue exposure to China does not explain our results. Our findings also cannot be explained by the differential usage of bank debt and non-bank debt between the zombie and non-zombie firms because the two groups' utilization of both types of debt turn out to be similar. Finally, we show that our results are robust after creating matched samples of zombie and non-zombie firms based on their firm-level characteristics.

Our paper is related to the growing literature that uses high-frequency data to identify the effects of macroeconomic news on asset prices. Bernanke and Kuttner (2005) use daily stock prices and fed funds futures on FOMC announcement days to study the effect of unanticipated monetary policy changes on stock prices. Gurkaynak et al. (2005b) study the effects of unexpected changes of macroeconomic variables on the days of macroeconomic data releases, such as output, employment, inflation, on various asset prices. More recent papers refine this approach to study the cost of nominal frictions and associated risk premia, e.g. Gorodnichenko and Weber (2016) and Weber (2015), or transmission of monetary policy shocks through financial frictions and bank lending, e.g. Ippolito, et al. (2018) and Gurkaynak et al. (2021). Our paper contributes to the literature by studying, for the first time to the best of our knowledge, the effect of uncertainty shocks and trade tensions.

Our paper is also related to the recent literature studying the effects of uncertainty shocks. This literature finds that increased uncertainty can generate countercyclical markups and



depress output, consumption, and investment (e.g. Bloom 2009; Julio and Yook 2012; Basu and Bundick 2017). We contribute to the literature by studying the reaction of financial markets using well-identified uncertainty shocks with high-frequency data and by documenting evidence consistent with the theoretical predictions of finance literature (Pastor and Veronesi 2012). Finally, we contribute to the recent event studies that investigate the effect of the U.S.-China trade war through macroeconomic spillovers and trade networks (Egger and Zhu 2020; Huang et al. 2021; Amiti et al. 2021). We differ from these papers not only in terms of our focus on uncertainty and debt structure, but also in terms of our empirical approach that addresses potential identification concerns in event studies.

## **2 Measuring the Effect of Uncertainty on Financial Markets using the U.S.-China Trade War**

Anecdotal evidence suggests that news related to the trade war can have a significant effect on financial markets. For example, Figure 1 shows how the S&P 500 index was flat until Friday, August 23, 2019, and reacted immediately when President Trump tweeted that he “hereby ordered” American companies to “immediately start looking for an alternative to China.” That day, the stock market closed down about 2.5 percent.

Obviously, a single event does not provide enough evidence to argue that the trade war has had a significant effect on financial markets. A more comprehensive analysis is needed to make this determination. We use public sources to create a timeline of events related to the trade war and identify 28 event days during the period from January of 2018 through August of 2019. Since our daily event study is intended to improve the measurement of the estimates, it is important to check if the events occurred after trading hours. For this purpose, Factiva is used to check the earliest timestamp of each news event. The details of these events are described in Appendix A. For those events that occurred after trading hours, we shift the event window to ensure that the window for the change in asset prices

includes the event.

During the period from January 2018 through August 2019, these news events related to the trade war account for an increase of 50 basis points (of the total 70 basis points rise) in the high-yield spread, and about 55 basis points of the 90 basis points decline in the 10-year Treasury yield (Table 1, Panel A). Overall, trade news seems to impact the financial markets in an economically significant way.

However, to make a more precise assessment of the effect that the uncertainty induced by the trade war has on U.S. financial markets, we also need to make sure that the markets are moving in the direction we expect them to move on the event days. In particular, we expect that news about the escalating trade war that raises market uncertainty would push the long-term rates and stock prices down and high-yield spreads up. Quantifying this effect is a challenging problem because multiple factors can affect asset prices on any given day. To address this challenge, we follow the daily event study approach of Rigobon and Sack (2003, 2004, 2005).<sup>9</sup>

Rigobon and Sack assume that the daily changes in financial variables can be characterized by a system of linear equations. If we let  $z_1$  be trade war shocks,  $z_2$  be (the vector of) all other shocks, and  $\Delta y_1$  and  $\Delta y_2$  be changes in the values of two financial market variables, we can write the equations that determine these day-by-day changes as:

$$\Delta y_1 = \alpha_1 z_1 + \alpha_2 z_2 + e_1 \tag{1}$$

$$\Delta y_2 = \beta_1 z_1 + \beta_2 z_2 + e_2, \tag{2}$$

where  $e_1$  and  $e_2$  are idiosyncratic shocks.

---

<sup>9</sup>Alternatively, one could use an intraday event study approach to minimize the effect of confounding factors. However, it is difficult to figure out the precise minute that a trade news event occurred, with the exception of the timing associated with the President's tweets. Moreover, the President's tweets do not always reveal new information about trade policy so, at best, any identification is weak.

There are two identification challenges. First, the variable  $z_1$  is unobservable, in that on any given day, the news related to the trade war cannot be precisely quantified. However, we can let  $y_1$  be an asset that we believe to be significantly affected by trade uncertainty, such as the Chicago Board Options Exchange’s Volatility Index (VIX), and  $y_2$  to be an asset whose reaction to the trade news we want to study, such as long-term rates.<sup>10</sup> Then we can answer the question: “What is the effect of a trade war shock that moves VIX by one percentage point per annum on 10-year Treasury yields?” In other words, although one cannot directly measure  $z_1$ , and hence its effect  $\beta_1$ , even if news about the trade war were the only factor driving movements in the financial variables, it is possible to identify  $\beta_1/\alpha_1$  to gauge the significance of the effect that trade war shocks have on financial markets.

The system of linear equations also illustrates the second identification challenge. Asset prices are affected by factors other than just news related to the trade war. Therefore, if we were to simply regress the daily change in long-term rates on the daily change in VIX, the resulting coefficient would not be informative about what significance should be attributed to news events related to the trade war.<sup>11</sup> This problem is addressed by employing the heteroskedasticity-based estimator of Rigobon and Sack. Their approach uses a set of event and non-event dates and two identification assumptions: (i) the variance of the trade-related news ( $z_1$ ) is higher on event dates, (ii) the variance of other news and the variance of the idiosyncratic shocks on event dates are equal to their counterparts on non-event dates. Intuitively, this identification scheme allows for all types of news to be present on any given day but assumes that any difference in the volatility of the movements in the financial variables occurring on event dates, relative to non-event days, is attributable to news about the trade war.<sup>12</sup>

---

<sup>10</sup>This choice conforms with the previous literature that has extensively used VIX as an uncertainty measure, such as Basu and Bundick (2017).

<sup>11</sup>In the context of equations (1) and (2), this would be a regression of  $\Delta y_2$  on  $\Delta y_1$ . The associated OLS estimate would satisfy  $E(\hat{\beta}_{OLS}) = cov(\Delta y_1, \Delta y_2)/var(\Delta y_1) = [\beta_1\alpha_1var(z_1) + \beta_2\alpha_2var(z_2)]/[\alpha_1^2var(z_1) + \alpha_2^2var(z_2) + var(e_1)] \neq \beta_1/\alpha_1$  due to omitted variables,  $var(z_2) \neq 0$ , and measurement error,  $var(e_1) \neq 0$ .

<sup>12</sup>This assumption can fail, for example, if news about the trade war is actually systematically driven

Under these identification assumptions, when we run the following instrumental variable regression using the set of event and non-event dates,

$$\Delta y = \beta \Delta VIX + \varepsilon \quad (3)$$

and instrument  $\Delta VIX = (\Delta VIX_{event}, \Delta VIX_{nonevent})$  with  $IV = (\Delta VIX_{event}, -\Delta VIX_{nonevent})$ , we get a consistent estimator for  $\beta_1/\alpha_1$ , where  $\Delta y$  is the change in the financial variable of interest. Intuitively, using non-event dates cleans out the effect of the movements in VIX stemming from shocks unrelated to the trade war on event days.

To see this mathematically, note that the instrumental variables estimate of  $\beta$  satisfies

$$\text{plim } \hat{\beta}_{IV} = \frac{\text{cov}(\Delta y, IV)}{\text{cov}(\Delta VIX, IV)} = \frac{\text{cov}(\Delta y, \Delta VIX)_{event} - \text{cov}(\Delta y, \Delta VIX)_{non-event}}{\text{var}(\Delta VIX)_{event} - \text{var}(\Delta VIX)_{non-event}}. \quad (4)$$

Once we replace  $\Delta VIX$  in equation (4) with  $\Delta y_1$  in equation (1) and  $\Delta y$  in equation (4) with  $\Delta y_2$  equation (2) and apply Rigobon and Sack's identification assumptions, it is straightforward to show that  $\text{plim } \hat{\beta}_{IV} = \beta_1/\alpha_1$ .<sup>13</sup> Since only the variance of trade war shocks is different on event dates when compared to non-event dates, all the other sources of variance in  $\Delta y$  and  $\Delta VIX$  on event dates get cleaned out, leading to a consistent estimate of  $\beta_1/\alpha_1$ .

Figure 2 presents the VIX time series since January of 2018, along with the 28 event dates listed in the Appendix. We choose each of the non-event days as the same weekday, but two weeks before the given event date, in order to have a sufficient time interval between the event

---

by other shocks that can move financial variables. One such case can be that the news is revealed to systematically divert public attention from other events happening around the same time. Nevertheless, it is unlikely that the majority of the news related to the trade war fits this category, especially since this paper is studying high-frequency (daily) event windows. Accordingly, controlling for lagged changes in asset prices in the regressions does not affect the results.

<sup>13</sup>To see this quickly, note that under the identification assumption that  $\text{var}(z_2)_{event} = \text{var}(z_2)_{non-event}$ , the numerator in equation (4) becomes  $\beta_1\alpha_1[\text{var}(z_1)_{event} - \text{var}(z_1)_{nonevent}]$  and the denominator becomes  $\alpha_1^2[\text{var}(z_1)_{event} - \text{var}(z_1)_{nonevent}]$ , leading to  $\text{plim } \hat{\beta}_{IV} = \beta_1/\alpha_1$ .

and non-event dates.<sup>14</sup> The main results are not affected by alternative choices for these non-event dates. The identification assumptions underlying the Rigobon-Sack estimator imply that financial market variables should be more volatile on event dates compared to non-event dates. This implication is confirmed by the comparison of summary statistics (standard deviations and range) in the two panels in Table 1. While the heteroskedasticity-based estimator has been popular in studying the effect of monetary policy on asset prices (Rigobon and Sack 2003, Nakamura and Steinsson 2018), our paper, to the best of our knowledge, is the first to implement such approach to study the effect of policy uncertainty shocks on financial markets.

We expect that trade war news that raises market uncertainty and risk premia would push the Treasury yields and stock prices down and high-yield spreads up. Table 2 summarizes the regression results for news events related to the trade war that move VIX by one unit, meaning a one percentage point increase in the annualized implied volatility, roughly half a standard deviation for the sample period. Indeed, we see that such news reduce two-year and ten-year Treasury yields by about 15 basis points, while increasing high-yield corporate bond spreads by 3.5 basis points and reducing the S&P 500 index by about about 0.46 percent.

One might be concerned that the event of a single day could heavily influence our results. In order to alleviate this concern, we exclude one event at a time and re-estimate the coefficients in Table 2. Figure 3 shows the histogram resulting from this exercise. We see that all coefficient estimates remain very similar, being less than one standard deviation away from the estimates in Table 2, confirming that our results are not driven by outlier events.

Finally, we use cross-sectional stock return data to confirm that our heteroskedasticity-based identification approach indeed captures the shocks specific to the U.S.-China trade war. If this is the case, we should observe that (i) firms that derive a larger share of their

---

<sup>14</sup>In the case of the event date for August 23, 2019, the non-event day two weeks prior overlaps with another event related to the trade war that occurred on August 9, 2019. Therefore, August 2, 2019, was chosen as the non-event date corresponding to August 23, 2019.

revenue from China are more negatively affected, while (ii) the share of revenue from foreign countries other than China does not play a significant role in the shock transmission.

Our stock return data comes from the Center for Research in Security Prices (CRSP). Specifically, our analysis includes the daily stock returns in 2018 and 2019 for firms headquartered in the United States. We calculate each firm’s shares of revenue from China and other regions using the geographic segment data from Compustat Historical Segments.<sup>15</sup> To avoid look-ahead bias and reduce endogeneity concerns, we use 2017 Compustat data to calculate the revenue shares. To improve the precision of our estimates, we exclude a firm from our sample if the firm reports sales to Asia-Pacific regions but do not delineate its sales to China.

We use the set of event and non-event days to estimate the following fixed-effects instrumental variable regression:

$$r_{i,t} = \alpha_i + \beta \times \Delta VIX_t + \gamma \times \Delta VIX_t \times ChinaShare_i + \delta \times \Delta VIX_t \times X_i + \varepsilon_{i,t} \quad (5)$$

by instrumenting  $\Delta VIX = (\Delta VIX_{event}, \Delta VIX_{nonevent})$  with  $IV = (\Delta VIX_{event}, -\Delta VIX_{nonevent})$  as we did earlier. The dependent variable,  $r_{i,t}$ , is the return on company  $i$ ’s stock on date  $t$ .  $ChinaShare_i$  is company  $i$ ’s share of revenue from China.  $X_i$  includes two additional control variables: the revenue share of company  $i$  from non-U.S. sources other than China and a dummy variable capturing whether company  $i$  derives all of its revenue from the U.S.

We start by showing that firms that derive a larger share of their revenue from China react more negatively to uncertainty driven by U.S.-China trade war news. Table 3 presents how daily stock returns react to uncertainty driven by trade war news that move VIX by one

---

<sup>15</sup>Another dataset used in the literature is the FactSet data. We have spot checked the Compustat with FactSet and confirmed that the information matches in both datasets using publicly reported FactSet data: <https://www.marketwatch.com/story/trade-war-watch-these-are-the-us-companies-with-the-most-at-stake-in-china-2018-03-29>

percentage point (half a standard deviation increase in the annualized implied volatility). Column (1) shows that the stock returns experience a 0.4 percent decline on average, whereas column (2) indicates the negative impact is stronger for firms with a larger share of revenue from China. In particular, a 10 percentage point increase in the revenue share from China (*ChinaShare*) would lead to an additional 0.04 percent decline in stock returns. These results remain very similar when we restrict our sample to firms that have revenue from any foreign region, thereby excluding domestic firms, in column (3). We also obtain similar results when we further restrict the sample to those firms that have only China as a foreign revenue source in column (4). Therefore, columns (1) to (4) demonstrate that firms with a larger revenue share from China are more negatively affected by the uncertainty shocks from the U.S.-China trade war, as expected.

Next, we show that the share of revenue from sources other than China does not play a significant role in the transmission of shocks due to U.S.-China trade war. Therefore, column (5) extends the regression in column (2) by including two additional variables: the firm-level revenue share from foreign regions other than China (*ForeignNonChinaShare*) and a dummy variable for those firms that only have U.S. domestic sales (*USonly*). The coefficients of the variables included both in column (2) and column (5), in particular the effect of *ChinaShare*, remain very similar. Furthermore, the stock price reaction is not significantly affected by the revenue share from foreign sources other than China or by the domestic-sales-only status of the firms. Finally, we account for the concern that some industries, such as those that depend more heavily on Chinese inputs in their production, can react differently to different trade war news by including industry-date fixed effects. Column (6) confirms that our results remain similar to those in column (5). Therefore, we conclude that firms with a greater revenue share from countries other than China do not experience an additional impact from the uncertainty shocks identified from the U.S.-China trade war, as expected.

Overall, we find that the trade war uncertainty has depressed firm-level stock values, regardless of the firm’s sale exposure to China. While the sale exposure to China amplified the effect of the trade war uncertainty, the sale exposure to other countries did not. These results provide evidence that our identification strategy indeed captures the exogenous uncertainty shocks from the U.S.-China trade war.

### **3 Uncertainty, Debt Structure, and Zombie Firms**

In this section, we investigate the effect of debt structure and zombie status on the transmission of uncertainty shocks to stock prices. We start by describing our data sources, the construction of the study sample, and summary statistics, before discussing the regression specifications and estimation results.

#### **3.1 Data**

Our data on firm-level characteristics comes from Compustat Fundamentals Annual and the Capital Structure Summary database of Capital IQ. We use Compustat Fundamentals Annual to obtain firm-level variables, including interest expenses (XINT), net earnings (EBITDA), short-term debt (DLC), long-term debt (DLTT), current liabilities (LCT), cash and cash equivalents (CHE), and total assets (AT). We then merge Compustat and Capital IQ to acquire detailed information on firm-level capital and debt structure from the latter source.

Thanks to the disclosure requirements by the Regulations S-X and S-K of the Securities Act of 1933, public firms in the U.S. report detailed information on their capital and debt structure. Capital IQ has been systematically compiling the relevant information by going through firms’ regulatory filings, including financial footnotes. From 2002 onward, the coverage by Capital IQ is comprehensive (Rauh and Sufi 2010; Colla, Ippolito, and Li 2013; Gurkaynak, Karasoy-Can, and Lee 2021). As a result, we are able to decompose total debt



into bank debt and non-bank debt. This allows us to calculate *BankLeverage* as the ratio of total bank debt to total assets and *NonBankLeverage* as the ratio of total non-bank debt to total assets, where the total non-bank debt is defined as total debt (DLC+DLTT) minus the total bank debt from Capital IQ.

Following our practice in Section 2, our analysis uses the firm-level characteristics in 2017, the year before the U.S.-China trade war, to alleviate concerns regarding the endogenous responses to the trade war. Like before, we use CRSP for the daily stock returns on the event and non-event dates in 2018 and 2019.

In addition, we shed light on the role of zombie firms in the transmission of uncertainty shocks by identifying zombie firms in our sample following the guidelines of existing literature. Though there is no single formal definition of a zombie firm, it is generally agreed that these firms are relatively mature firms that could not independently serve their debt with only their net earnings (Caballero, Hoshi, and Kashyap 2008). We therefore adopt the definition from the recent studies that directly captures this idea (Adalet McGowan et al. 2018; Banerjee and Hofmann 2018, 2020).

Specifically, we define a zombie firm as a firm that is mature and has an interest coverage ratio (ICR) less than one in the three years leading to 2017.<sup>16</sup> Since the age of a firm is not directly observable for U.S. public companies, we consider a firm as mature if it has been at least 5 years since the firm's initial public offering (IPO) by the end of 2017.<sup>17</sup> Accordingly, we only keep firms that fit this age profile in our sample, regardless of their zombie status. The resulting share of zombie firms is thus around 7% in our sample, lower than the 10%

---

<sup>16</sup>The ICR is defined as the ratio of earnings before interest, taxes, depreciation and amortization (EBITDA) over interest expenses. Our results are robust when the ICR is defined as earnings before interest and taxes (EBIT) over interest expenses instead.

<sup>17</sup>Adalet McGowan et al. 2018 and Banerjee and Hofmann 2018 use a 10-year cutoff to call a firm mature. According to Ritter (2021), the median IPO age in the US is 8 years old based on the sample of 1980-2020 and 11 years old for the sample of 2001-2020. Therefore, our mature firm definition may be more restrictive. Our results are robust when dropping the IPO age restriction or changing the IPO age restriction to a range of values around 5 years.

reported in Favara et al. (2021) for U.S. public firms.

Though, in our analysis, a firm’s debt structure and zombie status has been decided before the trade war, which relieves endogeneity concerns, we include additional control variables that have been shown to affect the responsiveness of stock prices to macroeconomic shocks (e.g. Ippolito et al. 2018, Favara et al. 2021). These variables include Tobin’s Q (*TobinQ*), firm size (*LogAssets*), and the quick ratio (*QuickRatio*), which captures a firm’s balance sheet liquidity. *TobinQ* is computed as  $(MVE+DLC+DLTT)/AT$ , where MVE is the market value of equity calculated as the common shares outstanding (CSHO) multiplied by the annual close price (PRCC\_F) from Compustat. *LogAssets* is the natural logarithm of total assets. *QuickRatio* is defined as  $CHE/LCT$ .

Following Colla, Ippolito, and Li (2013), we remove utilities (SIC codes 4900-4949) and financials (SIC codes 6000-6999), firms with missing or zero values for total assets, firm-years with missing or zero total debt, and firms for which the difference between total debt as reported in Compustat and the sum of debt types as reported in Capital IQ exceeds 10% of total debt.<sup>18</sup>

Table 4, Panel A presents the summary statistics for the full sample, the sample of zombie firms, and the sample of non-zombie firms. Since we are interested in the role of debt structure in transmitting uncertainty shocks to changes in stock prices, it is reassuring to observe that the *BankLeverage* and the *NonBankLeverage* turn out to be similar for zombie and non-zombie firms. However, consistent with Favara et al. (2021), we do detect significant differences in Tobin’s Q, firm size, and the quick ratio across the two groups, as reflected by the last column of Table 4, Panel A. As a consequence, we will directly control these variables in our regression analysis. To account for any remaining endogeneity concern associated with confounding factors correlated with our firm-level characteristics in 2017,

---

<sup>18</sup>In addition to bank debt, the debt types include commercial paper, senior bonds and notes, subordinated bonds and notes, capital leases, and other debt.

we engage in two distinct matching exercises in our robustness checks. First, we construct a matched sample based on Mahalanobis matching using the method of equal percent bias reducing. Second, we construct a matched sample based on coarsened exact matching using the method of monotonic imbalance bounding, which is a new state-of-the-art method in this literature. As will be shown in Section 3.3, our results remain robust after both exercises.

### **3.2 The Role of Debt Structure and Zombie Firms**

Given the negative effect of the uncertainty shocks on stock prices from Section 2, we now ask if debt structure, specifically bank vs. non-bank debt, can play a special role in modifying this impact. Bank debt can have two opposing effects under macroeconomic shocks. On the one hand, worsened economic conditions can substantially reduce loan supply by banks and thereby amplify the negative effect on the borrowers relying more on bank debt (Jimenez et al. 2012). This is the so-called bank balance-sheet channel. On the other hand, banks specialize in acquiring private information about firms and have informational advantage in minimizing inefficient liquidation. Because of the relationship between banks and their borrowers, bank debt is easier to renegotiate than public debt, which is typically held by a larger group of investors. Thus, bank debt can offer financial or contractual flexibility during temporary distress (Boot et al 1993, Chemmanur and Fulghieri 1994, Berlin and Mester 1992, Hadlock and James 2002, Denis and Mihov 2003, De Fiore and Uhlig 2011). With heightened uncertainty, the option value of a debt renegotiation rises. This can help insulate the stock prices of bank-dependent borrowers from uncertainty shocks, a mechanism we dub the financial flexibility channel.

A natural candidate that can particularly benefit from the financial flexibility channel is the group of zombie firms, which, in our study, are defined as mature firms that persistently could not generate enough profits to cover their interest expenses. There are several reasons why zombie firms could be shielded by the financial flexibility channel. To begin with, when a zombie firm depends on bank debt to survive, any reduction in credit supply likely implies

the recognition of a loan loss, which can have undesirable consequences for the lending bank (Blattner et al. 2019, Bonfim et al. 2020). For instance, calling back a non-performing loan often requires a bank to write off existing capital, which in turn pushes the bank up against regulatory capital constraints (Caballero et al. 2008). Even without such regulatory constraints, recent research suggests that it may be optimal for even a financially strong bank to evergreen the loan when the bank has market power and internalizes the effect of its choice on the borrowing firm’s default decision (Faria-e-Castro, Paul, and Sanchez 2021). Moreover, bank managers can have incentives to avoid recognizing previous mistakes, especially when it is difficult to identify better lending opportunities with heightened uncertainty. As a result, if the financial flexibility channel dominates the bank balance-sheet channel, the mitigating effect of bank debt can be particularly strong for zombie firms.

To test the role of debt structure and zombie firms in transmitting the uncertainty shocks, we estimate the following fixed-effects instrumental variable regression:

$$\begin{aligned}
r_{i,t} = & \alpha_i + \beta_0 \times \Delta VIX_t + \gamma_0 \times \Delta VIX_t \times BankLeverage_i + \delta_0 \times \Delta VIX_t \times NonBankLeverage_i \\
& + \beta_1 \times \Delta VIX_t \times Zombie_i + \gamma_1 \times \Delta VIX_t \times BankLeverage_i \times Zombie_i \\
& + \delta_1 \times \Delta VIX_t \times NonBankLeverage_i \times Zombie_i \\
& + \text{Controls and their interactions with } Zombie_i \text{ and } \Delta VIX_t + \varepsilon_{i,t},
\end{aligned} \tag{6}$$

using the set of event and non-event days and instrumenting  $\Delta VIX = (\Delta VIX_{event}, \Delta VIX_{nonevent})$  with  $IV = (\Delta VIX_{event}, -\Delta VIX_{nonevent})$  as we did in Section 2. The dependent variable,  $r_{i,t}$ , is the return of company  $i$ ’s stock on date  $t$ .  $BankLeverage_i$  is the ratio of bank debt to total assets.  $NonBankLeverage_i$  is the ratio of non-bank debt to total assets. Finally,  $Zombie_i$  equals one if a firm is classified as a zombie firm in our sample and zero otherwise. These variables are measured at the end of 2017, before the trade war started, in order to avoid endogeneity concerns.

Table 5 presents our main results. We start our analysis by including only  $\Delta VIX_t \times$

$BankLeverage_i$  in Equation (6) in addition to  $\Delta VIX_t$  and the security fixed effects,  $\alpha_i$ . Column (1) shows that a higher usage of bank debt in the debt structure in fact stabilizes the negative impact of the uncertainty shock. Specifically, an uncertainty shock that moves VIX by one percentage point (half a standard deviation increase in the annualized implied volatility) causes a 0.425 percent decline in the daily stock returns for a firm with no bank debt and a one standard deviation (0.17) increase in the 2017  $BankLeverage_i$  can mitigate around 6% ( $= 0.17 * 0.15 / 0.425$ ) of the impact of the uncertainty shock. A natural concern is that firms with more bank debt have more debt in general and we inadvertently capture the effect of all debt, in which case there is nothing special about bank debt and debt structure. We address this concern by controlling for the usage of non-bank debt in column (2), which reveals that the coefficient of  $BankLeverage_i$  does not change significantly. Moreover, a higher usage of non-bank debt seems to amplify, rather than attenuate, the effect of the uncertainty shock. These findings suggest that bank debt is special in terms of its ability to mitigate the impact of a sudden increase in uncertainty. Our results so far thus lend support to the financial flexibility channel of bank debt over the bank balance-sheet channel.

If the financial flexibility channel is at work, one would expect that the positive effect of the channel is especially strong for the natural beneficiaries - the zombie firms. Column (3) tells us this is the case. In fact, column (3) indicates the mitigating effect of bank debt can be completely attributed to the zombie firms. On average, a one standard deviation (0.20) increase in the bank leverage for a zombie firm can mitigate around 45% ( $= 0.20 * 0.89 / 0.39$ ) of the negative impact of the uncertainty shock on the daily stock return. For the non-zombie firms, the mitigating effect of bank debt is economically small (0.038) and statistically insignificant. Furthermore, a firm's zombie status does not play a meaningful role through the usage of non-bank debt, as evidenced by the economically small (0.044) and statistically insignificant coefficient of  $NonBankLeverage_i \times Zombie_i$ . It should also be noted that the coefficient of the zombie status is not statistically significant ( $-0.082$ ), suggesting it is the

interaction between zombie firms and the bank leverage that matters, not the zombie status itself.

Our findings are robust after controlling for the firm-level characteristics that have been shown to affect the responsiveness of stock prices to macroeconomic shocks, as shown in column (4). Column (5) further confirms the robustness of our results after including the industry-date fixed effects.

Our benchmark regressions use the firm-level debt structure in 2017, which is before the start of the trade war. This addresses the concern that our results are driven by the effect of the trade war on debt structure through stock prices. Nevertheless, one might still be concerned that firms may have already adjusted their debt structure during 2017 in anticipation of the future events after Donald Trump became the president at the beginning of 2017. In order to address this concern, we instrument bank leverage and non-bank leverage in 2017 with their counterparts in 2016. Using lagged debt structure as an instrument is not only common (Ippolito, Ozdagli, and Perez 2018; Gurkaynak, Karasoy-Can, and Lee 2021), but it is also a particularly suitable approach in our case because Trump's presidency came mostly as a surprise. Column (6) shows that the results remain very similar to those in column (5).

Finally, to check if the bank debt usage in our sample might be correlated with a firm's revenue exposure to China, we repeat our regressions after restricting our sample to firms with an observed zero revenue share from China. As column (7) demonstrates, our results remain robust.

To the best of our knowledge, our research is the first to provide empirical evidence on the role of corporate debt structure in the response of stock prices to uncertainty shocks. We show that the usage of bank debt helps mitigate the negative impact of a sudden increase in uncertainty. Moreover, we present evidence that the mechanism at work is the financial flexibility channel of bank debt and that the mitigating effect of bank debt is driven by the

zombie firms.

The latter finding about zombie firms also rules out alternative explanations. For instance, one may be concerned that the positive coefficient of bank leverage can be caused by endogenous selection by the banks. In particular, banks may face additional scrutiny from the regulators and therefore be more risk-averse than other investors. Banks can also be better at picking “winners” due to their informational advantage. As a result, banks can lend to firms that are less sensitive to macro uncertainty shocks. In this case, our result would be interpreted not as “bank debt is special” but as “firms that rely more on bank debt are special.” Though a reasonable concern, it is unlikely that our findings can be explained by such arguments. If our results are driven by the endogenous lending decisions because banks are more risk averse or are good at picking “winners,” the bank leverage should display a similar, if not stronger, effect for the non-zombie firms, which is not the case in the data.<sup>19</sup>

### 3.3 Matching Analyses

Our results so far are unlikely to be driven by the differential usage of bank debt and non-bank debt between the zombie and non-zombie firms because the debt structure of these firms turn out to be similar (Table 4, Panel A). Moreover, our regressions so far control for confounding factors by adding the relevant firm characteristics as control variables. Nevertheless, one can argue that this approach is not enough since the zombie firms and non-zombie firms in our sample are fundamentally different in terms of their characteristics. An alternative approach to address this concern is pruning observations from the data so that the remaining zombie and non-zombie firms are similar (matched) to each other. We therefore conduct further robustness checks by performing two such matching exercises.

---

<sup>19</sup>It should also be noted that other large investors in the non-bank debt market, such as insurance companies, are also regulated. This further decreases the likelihood that our results can be explained by the endogenous lending decisions of banks because they are more risk averse due to regulatory reasons.

### 3.3.1 Mahalanobis Matching

We start our matching analyses with Mahalanobis matching, a widely used matching method belonging to a class of matching methods known as equal percent bias reducing (EPBR). Despite its popularity, we acknowledge that an EPBR matching method has shortcomings in achieving balance between the treatment and control groups, which, in our case, corresponds to the zombie and non-zombie firms (e.g. Mielke and Berry 2007). In fact, an EPBR matching method does not guarantee any level of imbalance reduction in any given data set (Iacus, King, and Porro 2012). We therefore use the Mahalanobis matching as a robustness check and address the associated concerns by implementing an alternative matching method known as coarsened exact matching (CEM) in Section 3.3.2.

To construct our matched sample with Mahalanobis matching, we first calculate the Mahalanobis distance between each zombie and non-zombie firms using all firm-level characteristics in Equation (6). The matched sample is then created by pairing each zombie firm with non-zombie firms based on their Mahalanobis distance. To improve the matching quality, we implement the pairing with replacement. That is, the selections of non-zombie firms for different zombie firms are independent of each other. Because this requirement reduces the size of the matched non-zombie group, we pair each zombie firm with the top three non-zombie firms based on the shortest Mahalanobis distance to ensure a sufficiently large sample size.

Table 4, Panel B reports the summary statistics for the matched sample. Comparing with the summary statistics for the full sample (Table 4, Panel A), the balance between zombie and non-zombie firms has been notably improved. Yet, significant differences in Tobin's Q, firm size, and the quick ratio between the two groups can still be detected within an already small matched sample, as can be seen from the last column of Table 4, Panel B. This problem can be overcome by the CEM discussed in the next section.



Table 5, column (8) presents the regression outcomes remain robust based on the Mahalanobis-matched sample.

### 3.3.2 Coarsened Exact Matching

To address the concerns associated with Mahalanobis matching, we construct a matched sample using CEM as a further robustness check. The basic idea of CEM is to coarsen each variable of interest into bins, before exactly matching the coarsened data according to the bins to prune unmatched units. After the matching, the coarsened data are discarded and the original (uncoarsened) values of the matched data are retained. As shown by Iacus, King, and Porro (2009, 2011), CEM dominates commonly used existing (EPBR and other) matching methods in its ability to reduce imbalance, model dependence, estimation error, bias, and other criteria. Particularly, as a member of the class of matching methods known as monotonic imbalance bounding (MIB), CEM guarantees to eliminate all imbalances beyond the chosen levels of coarsening, which bounds the maximum level of imbalance in the data *ex ante*. In comparison, an EPBR matching method, such as Mahalanobis matching, does not guarantee any level of imbalance reduction. Moreover, the coarsening choice for any given variable has no effect on the imbalance bound for any other variables under CEM. In comparison, an EPBR matching method can reduce the imbalance for some variables *ex post*, while worsen the imbalance for some other variables. It is also worthy noting that CEM satisfies the congruence principle, which requires congruence between the data space and analysis space. An EPBR matching method violates the congruence principle, leading to less robust inferences with sub-optimal properties (Mielke and Berry 2007).<sup>20</sup> Finally, CEM has the practical advantage that it tends to produce a well-matched sample with a reasonably large sample size.

We perform the CEM by coarsening each of the firm-level characteristics that display

---

<sup>20</sup>In specific, Mahalanobis matching violates the congruence principle by projecting covariates from the natural  $n$ -dimensional data space to a different space defined by the Mahalanobis distance.

a significant difference between the zombie and non-zombie firms. Concretely, we coarsen Tobin's Q and the quick ratio into 4 bins defined by their 25th, 50th, and 75th percentiles in the main sample (the sample of Table 4, Panel A). We coarsen firm size, as measured by the natural logarithm of the total assets, into 6 bins defined by its 10th, 25th, 50th, 75th, and 90th percentiles to address the concern that firm size is the dimension in which we observe the largest difference between the zombie and non-zombie firms.<sup>21</sup> To adjust for the different stratum sizes caused by different numbers of observations of the non-zombie firms that are matched to the observations of the zombie firms, we weigh our matched sample following Iacus, King, and Porro (2012).

The summary statistics for our matched sample based on the 96 strata are reported in Table 4, Panel C. Comparing with the matched sample from the Mahalanobis matching (Table 4, Panel B), we can now achieve a better balanced sample without a statistically significant difference across all of the firm-level characteristics, as shown by the last column of Table 4, Panel C. The improvement of the matched sample also comes with a larger sample size.

Table 5, column (9) presents the regression outcomes of the CEM matching. As the column demonstrates, our results remain robust. Combining with the Mahalanobis matching, we have conducted matching analyses based on different classes of matching methods. The fact that we are able to obtain estimates consistent with the main findings relieves the concern that our results are driven by confounding factors associated with the firm-level characteristics included in the analysis.

---

<sup>21</sup>Our results are robust to alternative ways of coarsening, including coarsening using all of the firm-level characteristics in Equation (6) with the bank and non-bank leverages.

## 4 Conclusion

This paper provides new evidence on the effect of uncertainty on financial markets, using the U.S.-China trade war as a laboratory. The trade war between the U.S. and China affects the U.S. financial markets in an economically meaningful way. A rise in the trade war uncertainty increases high-yield spreads and depresses long-term Treasury rates as well as stock prices. In addition, there are significant cross-sectional differences in the sensitivity of stock prices to the uncertainty shocks created by the trade war. We find that the usage of bank debt helps stabilize the negative impact of uncertainty shocks, but this effect is concentrated among zombie firms. The usage of non-bank debt, on the other hand, does not have a similar mitigating effect.

Although the focus of our paper is the U.S., our findings can be applicable to other regions, especially Europe, where bank debt tends to weight heavier in the corporate debt structure and zombie firms are increasingly prevalent in the post-pandemic economic landscape. Furthermore, while our paper utilizes the U.S.-China trade war as a natural experiment, its approach and results can help us better understand the more recent events, such as the effect of uncertainty associated with COVID-19, global supply chain disruptions, and the Russia-Ukraine conflict. We hope our paper can stimulate future research in today's uncertain economic environment.

## References

- Acharya, Viral V., Tim Eisert, Christian Eufinger, and Christian Hirsch. 2019. "Whatever It Takes: The Real Effects of Unconventional Monetary Policy." *Review of Financial Studies* 32(9): 3366-3411. <https://www.reuters.com/article/us-usa-trade-china-timeline-idUSKCN1UZ24U> <http://dx.doi.org/10.1093/rfs/hhz005>
- Acharya, Viral V., Matteo Crosignani, Tim Eisert, and Christian Eufinger. 2020. "Zombie

Credit and (Dis-)Inflation: Evidence from Europe.” *NBER Working Paper No. 27158*.  
<http://dx.doi.org/10.2139/ssrn.3603788>

Adalet McGowan, Müge, Dan Andrews, and Valentine Millot. 2018. “The Walking Dead? Zombie Firms and Productivity Performance in OECD Countries.” *Economic Policy* 33(96): 685-736. <http://dx.doi.org/10.1093/epolic/eiy012>

Amiti, Mary, Sang Hoon Kong, and David Weinstein. 2021. “Trade Protection, Stock-Market Returns, and Welfare.” *NBER Working Paper No. 28758*. <http://dx.doi.org/10.2139/ssrn.3842733>

Amiti, Mary, Stephen Redding, and David Weinstein. 2019. “The Impact of the 2018 Tariffs on Prices and Welfare.” *Journal of Economic Perspectives*, 33 (4): 187-210. <http://dx.doi.org/10.1257/jep.33.4.187>

Banerjee, Ryan, and Boris Hofmann. 2018. “The Rise of Zombie Firms: Causes and Consequences.” *BIS Quarterly Review September*.

Banerjee, Ryan, and Boris Hofmann. 2020. “Corporate Zombies: Anatomy and Life Cycle” *BIS Working Paper No. 882*. <http://dx.doi.org/10.1093/epolic/eiac027>

Basu, Susanto, and Brent Bundick. 2017. “Uncertainty Shocks in a Model of Effective Demand.” *Econometrica*, 85(3), 937–958. <http://dx.doi.org/10.3982/ECTA16262>

Berlin, Mitchell, and Loretta J. Mester. 1992. “Debt Covenants and Renegotiation.” *Journal of Financial Intermediation* 2(2): 95-133. [http://dx.doi.org/10.1016/1042-9573\(92\)90005-X](http://dx.doi.org/10.1016/1042-9573(92)90005-X)

Bernanke, Ben, and Ken Kuttner. 2005. “What Explains the Stock Market’s Reaction to Federal Reserve Policy?” *Journal of Finance* 60: 1221–1257. <http://dx.doi.org/10.1111/j.1540-6261.2005.00760.x>

Bianconi, M., Esposito, F. and Sammon, M., 2021. “Trade policy uncertainty and stock returns.” *Journal of International Money and Finance* 119: 1024-92. <http://dx.doi.org/10.1016/j.jimonfin.2021.102492>

- Blattner, Laura, Luisa Farinha, and Francisca Rebelo. 2019. "When Losses Turn into Loans: The Cost of Undercapitalized Banks." *ECB Working Paper No. 2228*. <http://dx.doi.org/10.2139/ssrn.3366998>
- Bloom, Nicholas. 2009. "The Impact of Uncertainty Shocks," *Econometrica*, 77(3): 623–685. <http://dx.doi.org/10.3982/ECTA6248>
- Bloom, Nicholas, Max Floetotto, Nir Jaimovich, Itay Saporta-Eksten, and Stephen J. Terry. 2018. "Really Uncertain Business Cycles." *Econometrica* 86(3): 1031-1065. <http://dx.doi.org/10.3982/ECTA10927>
- Bonfim, Diana, Geraldo Cerqueiro, Hans Degryse, and Steven Ongena. 2020. "On-Site Inspecting Zombie Lending." *Swiss Finance Institute Research Paper No. 20-16*. <http://dx.doi.org/10.1287/mnsc.2022.4452>
- Boot, Arnoud WA, Stuart I. Greenbaum, and Anjan V. Thakor. 1993. "Reputation and Discretion in Financial Contracting." *American Economic Review* 83(5): 1165-1183.
- Caballero, Ricardo J., Takeo Hoshi, and Anil K. Kashyap. 2008. "Zombie Lending and Depressed Restructuring in Japan." *American Economic Review* 98(5): 1943-1977. <http://dx.doi.org/10.1257/aer.98.5.1943>
- Carney, Mark. 2019. "The Growing Challenges for Monetary Policy in the Current International Monetary and Financial System." Speech given at the Jackson Hole Symposium, August 23.
- Chemmanur, Thomas J., and Paolo Fulghieri. 1994. "Reputation, Renegotiation, and the Choice between Bank Loans and Publicly Traded Debt." *Review of financial Studies* 7(3): 475-506. <http://dx.doi.org/10.1093/rfs/7.3.475>
- Colla, Paolo, Filippo Ippolito, and Kai Li. 2013. "Debt Specialization." *Journal of Finance* 68(5): 2117-2141. <http://dx.doi.org/10.1111/jofi.12052>
- De Fiore, Fiorella, and Harald Uhlig. 2011 "Bank Finance versus Bond Finance." *Journal of Money, Credit and Banking* 43(7): 1399-1421. <http://dx.doi.org/10.1111/j.1538-4616.2011.00429.x>

- Denis, David J., and Vassil T. Mihov. 2003. "The Choice among Bank Debt, Non-Bank Private Debt, and Public Debt: Evidence from New Corporate Borrowings." *Journal of Financial Economics* 70(1): 3-28. [http://dx.doi.org/10.1016/S0304-405X\(03\)00140-5](http://dx.doi.org/10.1016/S0304-405X(03)00140-5)
- Egger, Peter H., and Jiaqing Zhu. 2020. "The US–Chinese Trade War: An Event Study of Stock-Market Responses." *Economic Policy* 35(103): 519-559. <http://dx.doi.org/10.1093/epolic/eiaa016>
- Fajgelbaum, P.D., Goldberg, P.K., Kennedy, P.J. and Khandelwal, A.K. 2020. "The return to protectionism." *The Quarterly Journal of Economics*, 135(1): 1-55. <http://dx.doi.org/10.1093/qje/qjz036>
- Fajgelbaum, P.D., Goldberg, P.K., Kennedy, P.J. and Khandelwal, A.K. 2021. "Updates to Fajgelbaum et al. (2020) with 2019 tariffs." Working Paper
- Faria-e-Castro, Miguel, Pascal Paul, and Juan M. Sánchez. 2021. "Evergreening." *Federal Reserve Bank of St. Louis Working Paper No. 2021-012A*. <http://dx.doi.org/10.24148/wp2022-14>
- Favara, Giovanni, Camelia Minoiu, and Ander Perez-Orive. 2021. "U.S. Zombie Firms: How Many and How Consequential?" *FEDS Notes No. 2021-07-30-2*. <http://dx.doi.org/10.17016/2380-7172.2954>
- Gorodnichenko, Yuriy, and Michael Weber. 2016. "Are Sticky Prices Costly? Evidence from the Stock Market." *American Economic Review* 106(1): 165–199. <http://dx.doi.org/10.1257/aer.20131513>
- Gouveia, Ana Fontoura, and Christian Osterhold. 2018. "Fear the Walking Dead: Zombie Firms, Spillovers and Exit Barriers." *OECD Productivity Working Papers No. 13*. <https://doi.org/10.1787/e6c6e51d-en>
- Gürkaynak, Refet S., Gokce Karasoy-Can, and Sang Seok Lee. 2021. "Stock Market's Assessment of Monetary Policy Transmission: The Cash Flow Effect." *Journal of Finance*, forthcoming. <http://dx.doi.org/10.1111/jofi.13163>

Gürkaynak, Refet S., Brian P. Sack, and Eric T. Swanson. 2005a. “Do Actions Speak Louder Than Words? The Response of Asset Prices to Monetary Policy Actions and Statements.” *International Journal of Central Banking* 1(1): 55-93. <http://dx.doi.org/10.2139/ssrn.633281>

Gürkaynak, Refet S., Brian Sack, and Eric Swanson. 2005b. “The Sensitivity of Long-Term Interest Rates to Economic News: Evidence and Implications for Macroeconomic Models.” *American Economic Review* 95(1): 425–436. <http://dx.doi.org/10.1257/0002828053828446>

Hadlock, C.J. and James, C.M., 2002. “Do Banks Provide Financial Slack?.” *Journal of Finance* 57(3): 1383-1419. <http://dx.doi.org/10.1111/1540-6261.00464>

Hatzius, Jan, Alec Phillips, David Mericle, Spencer Hill, Daan Struyven, David Choi, Blake Taylor, and Ronnie Walker. 2019. *A Bigger Growth Hit from the Trade War*. Global Investment Research Division, US Economics Analyst Report, August 11. New York: Goldman Sachs & Co.

Huang, Yi, Chen Lin, Sibio Liu, and Heiwai Tang. 2021. “Trade Networks and Firm Value: Evidence from the US-China Trade War.” *CEPR Discussion Paper No. DP14173*. <http://dx.doi.org/10.2139/ssrn.3227972>

Iacus, Stefano, Gary King, and Giuseppe Porro. 2009. “CEM: Software for Coarsened Exact Matching.” *Journal of statistical Software* 30: 1-27. <http://dx.doi.org/10.18637/jss.v030.i09>

Iacus, Stefano M., Gary King, and Giuseppe Porro. 2011. “Multivariate Matching Methods that are Monotonic Imbalance Bounding.” *Journal of the American Statistical Association* 106(493): 345-361. <http://dx.doi.org/10.1198/jasa.2011.tm09599>

Iacus, Stefano M., Gary King, and Giuseppe Porro. 2012. “Causal Inference without Balance Checking: Coarsened Exact Matching.” *Political Analysis* 20(1): 1-24. <http://dx.doi.org/10.1093/pan/mpr013>

- Ippolito, Filippo, Ali K. Ozdagli, and Ander Perez-Orive. 2018. "The Transmission of Monetary Policy through Bank Lending: The Floating Rate Channel." *Journal of Monetary Economics* 95: 49-71. <http://dx.doi.org/10.1016/j.jmoneco.2018.02.001>
- Jiménez, G., Ongena, S., Peydró, J.L. and Saurina, J. 2012. "Credit Supply and Monetary Policy: Identifying the Bank Balance-Sheet Channel with Loan Applications." *American Economic Review*, 102(5): 2301-26. <http://dx.doi.org/10.1257/aer.102.5.2301>
- Julio, Brandon, and Youngsuk Yook. 2012. "Political Uncertainty and Corporate Investment Cycles." *Journal of Finance* 67(1): 45-83. <http://dx.doi.org/10.1111/j.1540-6261.2011.01707.x>
- Mielke, Paul W., and Kenneth J. Berry. 2007. "Permutation Methods: A Distance Function Approach." *New York: Springer*. <http://dx.doi.org/10.1007/978-0-387-69813-7>
- Nakamura, Emi and Jon Steinsson. 2018. "High Frequency Identification of Monetary Non-neutrality." *Quarterly Journal of Economics* 133: 1283–1330 <https://doi.org/10.1093/qje/qjy004>
- Pastor, Lubos, and Pietro Veronesi. 2012. "Uncertainty about Government Policy and Stock Prices." *The Journal of Finance* 67 (4): 1219-1264. <http://dx.doi.org/10.1111/j.1540-6261.2012.01746.x>
- Rauh, Joshua D., and Amir Sufi, 2010. "Capital structure and debt structure." *Review of Financial Studies* 23, 4242–4280. <http://dx.doi.org/10.1093/rfs/hhq095>
- Rigobon, Roberto, and Brian Sack. 2003. "Measuring the Reaction of Monetary Policy to the Stock Market." *Quarterly Journal of Economics* 118(2): 639–669. <http://dx.doi.org/10.1162/003355303321675473>
- Rigobon, Roberto, and Brian Sack. 2004. "The Impact of Monetary Policy on Asset Prices." *Journal of Monetary Economics* 51(8):1553–1575. <http://dx.doi.org/10.1016/j.jmoneco.2004.02.004>



Rigobon, Roberto, and Brian Sack. 2005. "The Effects of War Risk on US Financial Markets." *Journal of Banking and Finance* 29(7): 1759–1789. <http://dx.doi.org/10.1016/j.jbankfin.2004.06.040>

Ritter, Jay R. 2021. "Initial Public Offerings: Median Age of IPOs Through 2020." *IPO Data*. University of Florida.

Summers, Lawrence H. 2019. "There's a Revealing Puzzle in the China Tariffs." *The Washington Post*.

Weber, Michael. 2015. "Nominal Rigidities and Asset Pricing." *University of Chicago Working Paper*. <http://dx.doi.org/10.2139/ssrn.2478500>

## Appendix A: U.S.-China Trade War Timeline

Unless indicated otherwise, all events are taken from Reuters/CNBC timeline, which reports important milestone dates and (sometimes multiple) events happening on that date.<sup>1</sup> Using only those events from Reuters/CNBC does not materially change the results. All events are checked for the first timestamp in Factiva to see if these occurred before or after trading hours.

March 22, 2018: Trump asked the United States trade representative (USTR) to investigate applying tariffs on US\$50–60 billion worth of Chinese goods. (Source: Wikipedia/CNN, <https://www.cnn.com/2018/03/22/politics/donald-trump-china-tariffs-trade-war/>.)

April 2, 2018: China imposes tariffs of up to 25 percent on 128 U.S. products.<sup>2</sup>

April 4, 2018: On April 3 (after trading hours), Trump unveils plans for 25 percent tariffs on about \$50 billion of Chinese imports. On April 4, China responds with plans for retaliatory tariffs on about \$50 billion of U.S. imports.

May 19/20, 2018 (weekend): Chinese officials agreed to “substantially reduce” America’s trade deficit with China by committing to “significantly increase” its purchases of American

---

<sup>1</sup>See <https://www.reuters.com/article/us-usa-trade-china-timeline-idUSKCN1UZ24U> or <https://www.cnbc.com/2019/08/23/reuters-america-timeline-key-dates-in-the-u-s-china-trade-war.html>. We omit the events that are not exclusively related to U.S.-China trade, such as accusations of IP theft or tariffs on all imported goods regardless of origin.

<sup>2</sup>On March 8, 2018, Trump orders 25% tariffs on steel imports and 10% on aluminum from all suppliers - not just China. Since this was not explicitly related to China, it has been eliminated.

goods. As a result, Treasury Secretary Steven Mnuchin announced that "We are putting the trade war on hold". (Source: Wikipedia/AP News, <https://www.apnews.com/41443aaca704426b9f35b16607271a60>).

May 29, 2018: The White House announced that it would impose a 25 percent tariff on \$50 billion of Chinese goods with "industrially significant technology;" the full list of products affected to be announced by June 15, 2018. (Wikipedia/CNN, <https://money.cnn.com/2018/05/29/news/economy/china-tariffs/index.html>).

June 15, 2018: The United States sets an effective date of July 6 for 25 percent levies on \$34 billion of Chinese imports. It says 25 percent tariffs will also kick in on an additional \$16 billion of goods after a public comment period. China responds in kind with tariffs on \$34 billion of U.S. goods.

June 18, 2018 (after trading hours): The White House declared that the United States would impose additional 10 percent tariffs on another \$200 billion worth of Chinese imports if China retaliated against these U.S. tariffs. China retaliates, threatening its own tariffs on \$50 billion of U.S. goods, and stating that the United States had launched a trade war. (Wikipedia/CNN, <https://money.cnn.com/2018/06/18/news/economy/trump-china-tariffs-retaliation/>)

July 6, 2018: American tariffs on \$34 billion of Chinese goods came into effect. China imposed retaliatory tariffs on US goods of a similar value. (Source: Wikipedia/BBC, <https://www.bbc.com/news/economy-45678901>)

[//www.bbc.com/news/business-44707253](http://www.bbc.com/news/business-44707253)).

July 10, 2018 (after trading hours): The United States unveils plans for 10 percent tariffs on \$200 billion of Chinese imports.

August 1, 2018 (after trading hours): Trump orders the USTR to increase the tariffs on \$200 billion of Chinese imports to 25 percent from the originally proposed 10 percent.

August 7, 2018 (after trading hours): The United States releases the list of \$16 billion of Chinese goods to be subject to 25 percent tariffs. China retaliates with 25 percent duties on \$16 billion of U.S. goods.

August 14, 2018: China filed a complaint with the World Trade Organization (WTO), stating that U.S. tariffs on foreign solar panels clash with WTO ruling and have destabilized the international market for solar PV products. (Source: Wikipedia/Reuters, <https://www.reuters.com/article/us-usa-trade-china-solar/china-says-u-s-solar-tariffs-violate-trade-rules-lodges-wto-complaint-idUSKBN1L001K>)

September 6, 2018: Trump threatens tariffs on \$200 billion more of Chinese imports. (Originally reported as September 7 by Reuters, corrected using Factiva.)

September 18, 2018: On September 17 (after trading hours): the United States announced its 10 percent tariff on \$200 billion worth of Chinese goods would begin on September 24, 2018, increasing to 25 percent by the end of the year. China promptly responded with 10 percent tariffs on \$60 billion of U.S. imports. (Source: Wikipedia/CNBC, <https://www.cn>)

[bc.com/2018/09/18/china-says-new-tariffs-on-us-goods-worth-60-billion-effective-sept-24.html](https://www.bbc.com/news/technology-46181818))

September 24, 2018: The United States implements 10 percent tariffs on \$200 billion of Chinese imports. The administration says the rate will increase to 25 percent on Jan. 1, 2019. China answers with duties of its own on \$60 billion of U.S. goods.

December 1, 2018 (Saturday): The United States and China agree on a 90-day halt to new tariffs. Trump agrees to put off the Jan. 1 scheduled increase of tariffs on \$200 billion of Chinese goods until early March while talks between the two countries take place. China agrees to buy a “very substantial” amount of U.S. products.

February 24, 2019 (Sunday): Trump extends the March 1 deadline, leaving the tariffs on \$200 billion of Chinese goods at 10 percent on an open-ended basis.

May 5, 2019 (Sunday): Trump tweets that he intends to raise the tariff rate on \$200 billion of Chinese goods to 25 percent on May 10.

May 8, 2019: The Trump administration gives formal notice of its intent to raise tariffs on \$200 billion of Chinese imports to 25 percent from 10 percent, effective May 10. Earlier, Reuters reported that China had backtracked on almost all aspects of a draft trade pact with the United States.

May 15, 2019 (after trading hours): President Trump signed an executive order Wednesday that allows the U.S. to ban telecommunications network gear and services from foreign

adversaries, in a measure widely believed to be aimed at China and telecom companies including Huawei Technologies Co. and ZTE Corp. (Source: Wikipedia/CNBC, <https://www.cnbc.com/video/2019/05/15/trump-signs-executive-order-targeting-huawei.html>).

June 18, 2019: Trump and Xi speak by phone, and the two sides agree to rekindle trade talks ahead of a planned meeting between the two leaders scheduled for the Group of 20 (G20) summit in Japan at the end of June.

June 29, 2019 (Saturday): At the G20 meeting in Osaka, the United States and China formally agree to restart trade talks after concessions from both sides. Trump agrees to no new tariffs and an easing of restrictions on Chinese telecom powerhouse Huawei Technologies Co Ltd. China agrees to unspecified new purchases of U.S. farm products.

August 1, 2019: After two days of trade talks with little progress and complaints by Trump that China has not followed through on a promise to buy more U.S. farm products, he announces 10 percent tariffs on \$300 billion worth of Chinese imports, in addition to the 25 percent tariffs already levied on \$250 billion worth of Chinese goods. Trump says the talks between Washington and Beijing would continue despite the new tariffs, and that the rate could be increased above 25 percent in stages.

August 5, 2019: China's Commerce Ministry responds to the latest U.S. tariffs by halting purchases of U.S. agricultural products, and the Chinese currency, the yuan, weakens past the seven yuan per one dollar level, sending equity markets sharply lower. After U.S. markets

close, the U.S. Treasury says it has determined for the first time since 1994 that China is manipulating its currency, knocking the U.S. dollar sharply lower and sending gold prices to a six-year high.

August 6, 2019: China's central bank, the People's Bank of China, says Beijing has not and will not use the yuan to respond to trade frictions. A senior Trump aide says U.S.-China trade talks are still planned in Washington in September, and the latest tariffs could still be changed if talks go well, a message that helps calm markets.

August 9, 2019: Trump says he is not ready to make a deal with Beijing and suggests he may cancel in-person trade talks with China scheduled for Washington in September.

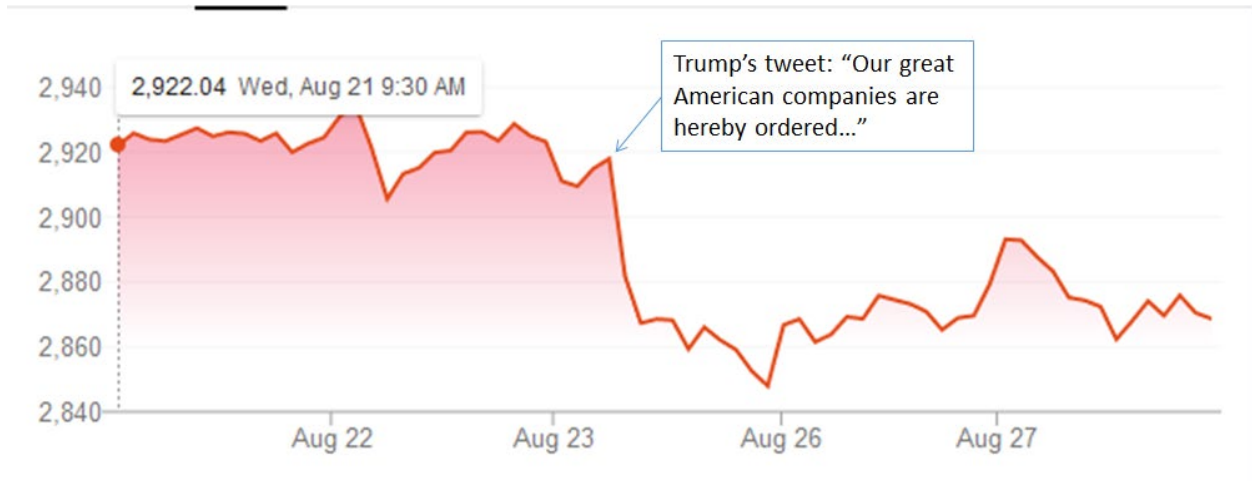
August 13, 2019: Trump delayed some of the tariff increases that he had announced earlier. Trump and his advisors, Peter Navarro, Wilbur Ross and Larry Kudlow, conceded that the higher tariffs were postponed to avoid harming American consumers during the Christmas shopping season. (Source: Wikipedia/CNBC, <https://www.cnn.com/2019/08/13/trump-says-he-delayed-tariffs-because-of-concerns-over-christmas-shopping-season.html>)

August 23, 2019: China announced that it will impose additional retaliatory tariffs against about \$75 billion worth of U.S. goods. Trump tweeted that he "hereby ordered" American companies to "immediately start looking for an alternative to China." Furthermore, tariffs are to be raised from 25 percent to 30 percent on the existing \$250 billion worth

of Chinese goods beginning on October 1, 2019, and from 10 percent to 15 percent on the remaining \$300 billion worth of goods beginning on December 15, 2019.



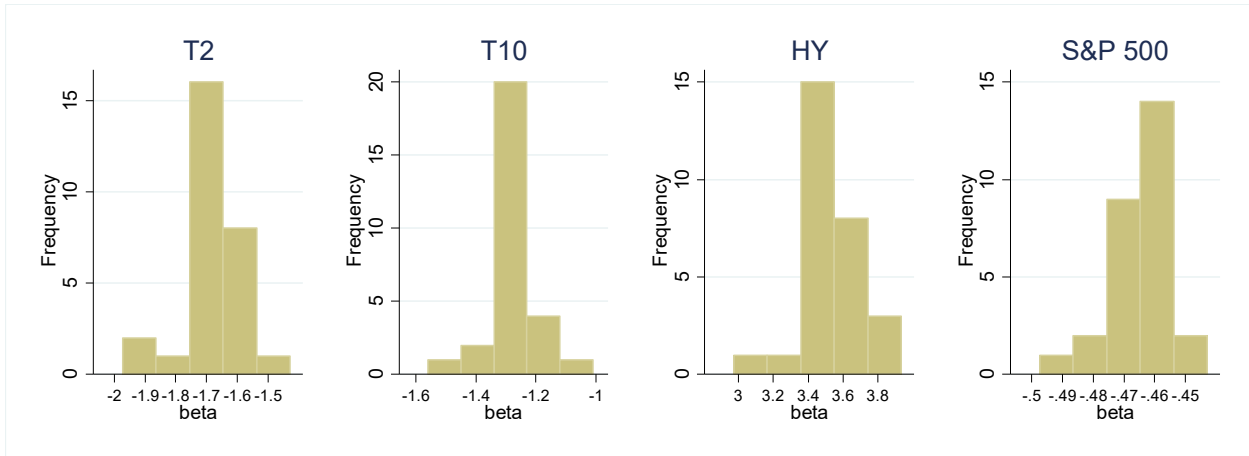
**Figure 1: S&P 500 Index around Trump’s Tweet on August 23, 2019**



**Figure 2: VIX and China Trade War Events**



**Figure 3: Robustness to Outlier Events**



For each financial variable, we plot the histogram of the coefficient estimates in Table 1 after dropping event-nonevent pairs one at a time. HY refers to high-yield spreads, measured as the Bank of America Merrill Lynch U.S. High Yield Master II Option-Adjusted Spread. T10 (T2) refers to the 10-year (two-year) Treasury yield, T3m refers to the three-month Treasury yield, and SP500 refers to the S&P 500 index.

**Table 1: Summary Statistics of Financial Variables on Event and Non-Event Dates****Panel A: Event Dates**

Variable	Obs.	Mean	Std. Dev.	Min	Max
$\Delta$ VIX	28	0.524	2.505	-4.420	6.980
$\Delta$ T2	28	-0.014	0.058	-0.160	0.080
$\Delta$ T10	28	-0.020	0.051	-0.160	0.060
$\Delta$ HY	28	0.017	0.104	-0.150	0.340
$\Delta$ SP500	28	-0.162	1.232	-2.978	1.513

**Panel B: Non-Event Dates**

Variable	Obs.	Mean	Std. Dev.	Min	Max
$\Delta$ VIX	28	-0.420	0.920	-1.970	1.850
$\Delta$ T2	28	0.004	0.031	-0.060	0.070
$\Delta$ T10	28	0.000	0.029	-0.040	0.060
$\Delta$ HY	28	-0.001	0.054	-0.170	0.100
$\Delta$ SP500	28	0.222	0.561	-0.728	2.143

The data is from Federal Reserve Economic Data (FRED) by Federal Reserve Bank of St. Louis. VIX refers to the Chicago Board Options Exchange's Volatility Index; T10 (T2) refers to 10-year (two-year) Treasury yield; HY refers to high-yield spreads, measured as the Bank of America Merrill Lynch U.S. High Yield Master II Option-Adjusted Spread, and SP500 refers to S&P 500 index. For T1, T10, and HY, the unit is percentage points; for VIX the unit is percent per annum; and for SP500 the change refers to percentage change in the S&P500 index.

**Table 2: The Effect of Uncertainty Shocks on Financial Markets**

	(1)	(2)	(3)	(4)
	$\Delta T2$	$\Delta T10$	$\Delta HY$	$\Delta SP500$
$\Delta VIX$	-0.017***	-0.013***	0.035***	-0.465***
	(0.005)	(0.004)	(0.007)	(0.045)
<i>Observations</i>	56	56	56	56

Heteroskedasticity-robust standard errors are in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . HY refers to high-yield spreads, measured as the Bank of America Merrill Lynch U.S. High Yield Master II Option-Adjusted Spread. T10 (T2) refers to the 10-year (two-year) Treasury yield, and SP500 refers to the S&P 500 index.

**Table 3: The Cross-Sectional Effect of Uncertainty Shocks on Daily Stocks Returns**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	ALL	ALL	Non-U.S.	China-Only	ALL	ALL
$\Delta VIX$	-0.392***	-0.383***	-0.378***	-0.411***	-0.386***	
	(0.0715)	(0.0720)	(0.0689)	(0.0759)	(0.0721)	
$\Delta VIX * \text{ChinaShare}$		-0.400***	-0.415***	-0.327**	-0.419***	-0.522***
		(0.150)	(0.151)	(0.164)	(0.151)	(0.156)
$\Delta VIX * \text{ForeignNonChinaShare}$					0.0227	0.00312
					(0.0380)	(0.0358)
$\Delta VIX * \text{USOnly}$					-0.00614	-0.00450
					(0.0233)	(0.0233)
Observations	84,784	84,784	52,951	10,363	84,784	84,504
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-date FE	No	No	No	No	No	Yes

This table presents the results of the regression model displayed in equation (5) in the text. All regressions include stock-level fixed effects. Only firms head-quartered in the U.S. are included. ChinaShare is the ratio of the sales to China to total sales. ForeignNonChinaShare is the ratio of the sales to non-China foreign countries to total sales. USOnly is a dummy variable that is equal to 1 if all sales are in the U.S. and zero otherwise. The regressions with column title "ALL" includes all (1587) stocks, for which there is sufficient information to calculate sales to China. The column with title "Non-U.S." includes the subset of stocks (990 in total) that have a sale to a foreign (non-U.S.) country. The column with title "China-Only" includes only the 191 stocks with sales to China. The last column includes two-digit SIC industry-time fixed effects. The sample excludes utility firms and financial firms. Standard errors in parentheses are double-clustered by stock and by date. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 4: Summary Statistics****Panel A: The Full Sample**

	Full Sample (N = 72,037)		Non-Zombies (N = 67,154)		Zombies (N = 4,883)		T-Test Zombie=NonZombie
	Mean	SD	Mean	SD	Mean	SD	P-value
Daily Returns	.01	3.34	-.003	3.06	.21	5.95	0.000
BankLeverage	.14	.17	.14	.17	.14	.20	0.588
NonBankLeverage	.17	.22	.17	.22	.16	.26	0.658
TobinQ	1.83	1.63	1.73	1.34	3.23	3.48	0.000
LnTA	7.34	2.00	7.55	1.87	4.55	1.58	0.000
QuickRatio	.88	2.14	.76	1.96	2.59	3.38	0.000

Number of zombie firms: 95. Number of firms in total: 1,316.

**Panel B: The Matched Sample based on Mahalanobis Matching**

	Full Sample (N = 13,388)		Non-Zombies (N = 8,505)		Zombies (N = 4,883)		T-Test Zombie=NonZombie
	Mean	SD	Mean	SD	Mean	SD	P-value
Daily Returns	.11	4.63	.04	3.67	.21	5.95	0.043
BankLeverage	.16	.20	.17	.21	.14	.20	0.567
NonBankLeverage	.15	.25	.15	.24	.16	.26	0.723
TobinQ	2.65	2.85	2.32	2.35	3.23	3.48	0.022
LnTA	5.14	1.73	5.48	1.72	4.55	1.58	0.000
QuickRatio	1.83	2.72	1.40	2.14	2.59	3.38	0.002

Number of zombie firms: 95. Number of firms in total: 254.

**Panel C: The Matched Sample based on Coarsen Exact Matching**

	Full Sample (N = 22,997)		Non-Zombies (N = 18,114)		Zombies (N = 4,883)		T-Test Zombie=NonZombie
	Mean	SD	Mean	SD	Mean	SD	P-value
Daily Returns	.10	4.41	.08	3.90	.21	5.95	0.054
BankLeverage	.14	.20	.14	.19	.14	.20	0.554
NonBankLeverage	.17	.39	.18	.42	.16	.26	0.830
TobinQ	2.89	2.37	2.80	1.96	3.23	3.48	0.176
LnTA	4.75	1.49	4.80	1.46	4.55	1.58	0.148
QuickRatio	2.22	3.36	2.13	3.34	2.59	3.38	0.350

A zombie firm is defined as a firm with an IPO at least five years before 2017 and an interest coverage ratio (Compustat EBITDA/XINT) less than one in the three years leading to 2017. BankLeverage is the total bank debt from Capital IQ divided by the total assets (AT) from Compustat, NonBankLeverage is book leverage ((DLC+DLTT)/AT from Compustat) minus BankLeverage, TobinQ is (MVE+DLC+DLTT)/AT, where MVE is the market value calculate as price times common shares outstanding (CSHO) from Compustat, LogAssets is the natural logarithm of total assets, and QuickRatio is the ratio of cash and cash equivalents (CHE) to current liabilities (LCT). Following Colla, Ippolito, and Li (2013), we remove utilities (SIC codes 4900-4949) and financials (SIC codes 6000-6999), firms with missing or zero values for total assets, firm-years with missing or zero total debt, and firms for which the difference between the total debt from Compustat and the sum of debt types in Capital IQ exceeds 10% of the total debt from Compustat. Number of zombie firms: 95. Number of firms in total: 427.

**Table 5: Debt Structure, Zombie Firms, and the Effect of Uncertainty Shocks on Daily Stock Returns**

VARIABLES	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Full Sample	(5) Full Sample	(6) IV	(7) ChinaShare=0	(8) Mahalanobis	(9) CEM
$\Delta VIX$	-0.425*** (0.0692)	-0.400*** (0.0682)	-0.393*** (0.0706)	-0.106 (0.105)					
$\Delta VIX * \text{Zombie}$			-0.0817 (0.121)	0.115 (0.257)	0.0544 (0.243)	0.256 (0.274)	-0.162 (0.308)	-0.0541 (0.259)	-0.164 (0.311)
$\Delta VIX * \text{BankLeverage}$	0.150*** (0.0554)	0.117** (0.0536)	0.0376 (0.0376)	-0.0296 (0.0387)	-0.0708 (0.0441)	-0.0733 (0.0610)	-0.122* (0.0680)	0.0954 (0.137)	-0.128 (0.202)
$\Delta VIX * \text{BankLeverage} * \text{Zombie}$			0.888*** (0.258)	1.002*** (0.261)	1.127*** (0.308)	1.056** (0.522)	1.641*** (0.456)	0.943*** (0.311)	1.148*** (0.374)
$\Delta VIX * \text{NonBankLeverage}$		-0.113* (0.0625)	-0.102 (0.0619)	0.0341 (0.0582)	0.0113 (0.0485)	-0.0513 (0.0664)	-0.0241 (0.0700)	-0.159 (0.128)	0.0471 (0.151)
$\Delta VIX * \text{NonBankLeverage} * \text{Zombie}$			-0.0441 (0.183)	0.0115 (0.189)	0.0852 (0.200)	0.270 (0.330)	0.375 (0.244)	0.237 (0.236)	0.0199 (0.294)
$\Delta VIX * \text{TobinQ}$				-0.0129 (0.00975)	-0.0190** (0.00825)	-0.0174* (0.00955)	-0.0197** (0.00951)	-0.0122* (0.00709)	-0.0348 (0.0286)
$\Delta VIX * \text{TobinQ} * \text{Zombie}$				-0.00725 (0.00511)	-0.000392 (0.00641)	-0.0325 (0.0253)	-0.00152 (0.00412)	-0.00461 (0.00862)	0.0157 (0.0344)
$\Delta VIX * \text{LogAssets}$				-0.0373*** (0.00954)	-0.0357*** (0.0105)	-0.0339*** (0.0109)	-0.0369*** (0.0126)	-0.0568** (0.0276)	-0.0551** (0.0240)
$\Delta VIX * \text{LogAssets} * \text{Zombie}$				-0.0626* (0.0364)	-0.0580 (0.0352)	-0.0883* (0.0525)	-0.0373 (0.0547)	-0.0418 (0.0398)	-0.0429 (0.0436)
$\Delta VIX * \text{QuickRatio}$				0.00428 (0.00275)	0.00427* (0.00231)	0.00403** (0.00197)	0.0109 (0.0131)	0.00539 (0.0116)	-0.00450 (0.00729)
$\Delta VIX * \text{QuickRatio} * \text{Zombie}$				-0.00400 (0.0138)	-0.00750 (0.0141)	-0.00561 (0.0163)	-0.0339 (0.0554)	-0.00342 (0.0131)	0.00685 (0.0174)
Observations	72,037	72,037	72,037	72,037	72,037	65,447	37,183	13,388	22,997
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-date FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes

This table presents the results of the regression model displayed in equation (6) in the text. Column 6 includes only firms that do not have any revenue from China and column 7 includes non-zombie firms that are matched with zombie firms based on the characteristics used in the first column. A zombie firm is defined as a firm with an IPO at least five years before 2017 and an interest coverage ratio (Compustat EBITDA/XINT) less than one in the three years leading to 2017. BankLeverage is the total bank debt from Capital IQ divided by the total assets (AT) from Compustat, NonBankLeverage is book leverage ((DLC+DLTT)/AT from Compustat) minus BankLeverage, TobinQ is (MVE+DLC+DLTT)/AT, where MVE is the market value calculate as price times common shares outstanding (CSHO) from Compustat, LogAssets is the natural logarithm of total assets, and QuickRatio is the ratio of cash and cash equivalents (CHE) to current liabilities (LCT). Following Colla, Ippolito, and Li (2013), we remove utilities (SIC 4900-4949) and financials (SIC 6000-6999), firms with missing or zero values for total assets, firm-years with missing or zero total debt, and firms for which the difference between the total debt from Compustat and the sum of debt types in Capital IQ exceeds 10% of the total debt from Compustat. Standard errors in parentheses are double-clustered by stock and by date. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

