Trade Policy Uncertainty and Stock Returns*

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Abstract

This paper documents new stylized facts on the effects of trade policy uncertainty on stock returns. We exploit quasi-experimental variation in exposure to policy uncertainty arising from annual votes by Congress to revoke China's NTR tariff rates between 1990 and 2001. Before China was permanently granted NTR rates, US manufacturing industries more exposed to trade policy uncertainty had stock returns 4.3% higher per year than less exposed sectors. Our results are not driven by stock prices' responses to policy-related news, nor by the effect of Chinese competition on expected or realized returns. We argue instead that this difference in average returns is a risk premium for exposure to trade policy uncertainty. Moreover, we document that more exposed sectors had more volatile stock prices, and that indirect exposure to uncertainty through Input-Output linkages also commanded a risk premium.

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1 Introduction

The recent escalation in threats of a trade war between US and China has brought trade policy uncertainty to the forefront of the economic and policy debate. Figure 1 shows that average trade policy uncertainty in the US was more than six times higher in 2018 than in 2015.[1]

A growing empirical literature analyzes the effect of trade policy uncertainty on economic outcomes (see e.g. Handley and Limão (2015), Pierce and Schott (2016) and Handley and Limão (2017)). Uncertainty about future tariffs, however, could affect not only the *current* economic performance, but also investors' expectations of *future* performance and risk, affecting firms' stock returns, which are an important determinant of household wealth, firms' value and investment decisions (see e.g. Black (1976), Christie (1982), Davis et al. (2006)).

In this paper, we estimate the effect of trade policy uncertainty (henceforth TPU) on US firms' stock returns. We focus on the uncertainty arising from annual votes by Congress to revoke China's ''Most Favored Nation'' (MFN) status between 1990 and 2001. Starting in 1980, US imports from China were subject to the relatively low Normal Trade Relations (NTR), or equivalently MFN tariff rates reserved for WTO members, even though China was not a member of the WTO. This required annual renewals by Congress, which were essentially automatic until the Tiananmen Square Crackdown in 1989. Starting in 1990, NTR renewal in Congress became more politically contentious, with the House passing resolutions against Chinese MFN renewal in 1991 and 1992.[2] Had NTR status been revoked, tariffs would have reverted to the higher non-NTR rates, established under the Smoot-Hawley Tariff Act of 1930.[3] The uncertainty about substantial U.S. import tariff increases on Chinese goods ended in 2001, when the US granted China ''Permanent Normal Trade Relations'' (PNTR), which eliminated the need for annual votes on MFN renewal.

We argue that the annual Congressional votes generated uncertainty because (i) investors were uncertain about whether China's NTR status would be revoked, and (ii) they were uncertain about the future performance of US industries after (and if) NTR was revoked. We follow Pierce and Schott (2016), and quantify this uncertainty via the "NTR gap," defined as the difference between the non-NTR rates, to which tariffs would have risen if annual renewal had failed, and the NTR rates.

Figure 2 shows that, during the contentious annual renewal period, industries more

[1] The figure is based on the Trade Policy Uncertainty index constructed by Baker et al. (2016).

[2] China's tariff status, however, did not change because the US Senate did not pass the House resolutions.

[3] Before PNTR was granted, the average NTR rate was 4 percent, while the average Smoot-Hawley tariff was 31 percent.

exposed to TPU, i.e. industries with larger NTR gaps, had significantly higher average returns than less exposed industries. In the decades not characterized by uncertainty, i.e. before the Tiananmen Square Crackdown, 1980-1989, and after PNTR was implemented, 2002-2007, there was no significant difference in returns between industries more and less exposed to TPU.

We formally document this difference in stock returns across industries using a difference-in-difference methodology, which exploits the large cross-sectional variation in the NTR gaps before China was granted PNTR. In particular, we regress monthly value-weighted industry portfolio stock returns between 1980 and 2007, on the industry-level NTR gap, interacted with a dummy for the TPU period. The identification rests on the fact that 79% of the variation in the NTR gaps comes from variation in non-NTR rates, set by the Smoot-Hawley act in 1930, which are likely exogenous to the US industries' stock returns 70 years after they were set.

Our baseline results suggest that US manufacturing industries more exposed to tariff uncertainty, i.e. industries that had a higher gap between non-NTR and NTR rates, experienced significantly higher stock returns than less exposed industries in the 1990-2001 period. Our specification controls for unobserved industry- and time-specific characteristics, for industry-time variation in firm fundamentals and valuation metrics, as well as other contemporaneous US-China policy changes, such as the expiration of the global Multi-Fiber Arrangement (MFA) and the reduction in Chinese import tariffs associated with China's accession to WTO. The difference-in-differences coefficient in the baseline specification is significant at 1% level, and implies that going from an industry less exposed from trade policy uncertainty (at the 25th percentile of the distribution of NTR gaps in 1990), to an industry more exposed to trade policy (at the 75th percentile of the distribution), increases stock returns by 4.3% per year during the uncertainty period.[4]

One concern with our identification strategy is that the US government could have set high tariff rates on WTO members, i.e. high NTR rates, to protect industries that they expected to perform poorly after PNTR. This concern is mitigated by the results of a two-stage least squares specification, in which we instrument the baseline DID term with an interaction of the pre-PNTR indicator and the Smoot-Hawley non-NTR tariff rates. Our results imply an IV coefficient very similar to the baseline.[5] Another concern is that high-gap firms may have higher expected returns than low-gap firms because of unobserved differences

[4]Our results are robust to the length of the control period: extending the sample to 2017 or dropping the years before 1990 does not affect significantly the findings.

[5]We also perform a placebo test, in which we randomly draw a NTR gap from the cross-sectional distribution of gaps each year and randomly assign it to an industry. Reassuringly, the coefficient on the placebo gap is insignificant.

in fundamentals or shocks between 1990 and 2001, besides the ones we are able to control for in the regressions. To mitigate this concern, we estimate the baseline specification using stock returns for a group of selected high-income countries.[6] Reassuringly, we document an insignificant relationship between the NTR gap and expected returns in these countries during the period of US trade policy uncertainty, suggesting that our results are not driven by industry-time varying unobserved shocks.

Once we have established that more exposure to TPU implies higher average returns, we argue that this difference in returns can be interpreted as a risk premium for exposure to tariff uncertainty. We outline a simple reduced form model, in which the expected return of a stock depends on the expected policy risk premium, i.e. the compensation for risk associated with policy uncertainty.[7]

We then provide additional evidence that corroborates the risk premium hypothesis. First, we perform a portfolio analysis, along the lines of Barrot et al. (2018). The advantage of undertaking a portfolio analysis is that idiosyncratic firm-level shocks within each portfolio cancel out if the sample is sufficiently large. We rank all the industries in our sample in 3 sub-groups, based on the NTR gap in 1990, construct capitalization-weighted portfolios, and calculate monthly returns between 1980 and 2007. We then construct a "Trade Policy Uncertainty" (TPU) portfolio, which is the difference in returns between the industries with the highest and lowest gaps. We show that, even after conditioning on the Fama and French (2015) 5-factor model, the TPU portfolio experienced significantly higher average risk-adjusted returns during the uncertainty period, suggesting that TPU was, between 1990 and 2001, a systematic factor that could *not* be diversified away.

Second, we rely on the predictions of the Pástor and Veronesi (2013) model, which implies that higher policy uncertainty should be associated with higher volatility of stock prices. During the uncertainty period, as the probability of a policy change varied through the 1990's with Congressional votes, news and Presidential actions, high-gap firms' stock prices should have varied more in response to such events, relative to low-gap firms' stock prices. In fact, we find that firms in high-gap industries had significantly higher realized volatility than firms in low-gap industries during the tariff uncertainty period.

To lend further support to our risk premium hypothesis, we discuss two other potential explanations for our results and show that they are not consistent with the empirical evidence. The first alternative explanation for our results is that the higher returns for high-gap industries in the uncertainty period were driven by *realized* returns, i.e. the response of stock

[6] The countries for which we have enough data coverage in Compustat Global are Australia, France, Japan, South Korea, and UK.

[7] Our specification can be micro-founded, for example, with the model in Pástor and Veronesi (2013), in which policy uncertainty directly enters the Stochastic Discount Factor of the representative investor.

prices to news and policy-related events, rather than expected returns (see e.g. MacKinlay (1997)). In order to assess this hypothesis, we look at the response of stock returns in a tight window around the dates of i) PNTR-related policy announcements, ii) Congressional votes to revoke China's MFN status, iii) firms' earnings announcements. We find that high-gap industries had significantly lower realized returns than low-gap industries when President Clinton signed, on 10/10/2000, the law to grant China PNTR, and around earnings announcement dates after 2001, suggesting that investors initially under-estimated the impact of the policy on these industries. To evaluate whether our results are driven by the slow reaction of investors to PNTR or by stock prices responses to policy-related news, we re-run the main specification but exclude a 5-7 day window around each of these event days when computing stock returns. Reassuringly, we find that removing these days has a small effect on the estimated risk premium.

A related concern is that our findings could be driven by negative realized returns in the post-2001 period due to increased Chinese competition. Specifically, high-gap firms may have experienced a series of negative shocks, as investors learned about the effects of Chinese competition on more exposed US industries, as documented by Pierce and Schott (2016), Acemoglu et al. (2016) and Pierce and Schott (2017). We show, however, that our results go through even after dropping the entire post-period.

A second explanation for our results could be that investors expected that, once China entered the WTO, more exposed US industries would be hurt by stronger Chinese competition, and therefore commanded higher returns as compensation for this risk, *during* the uncertainty period. In this scenario, our estimated risk premium would not be caused by tariff uncertainty per se, but would be instead compensation for future Chinese competition risk. However, we document that our results are robust to controlling for an industry-year measure of import exposure from China, constructed as in Acemoglu et al. (2016), that proxies for investors' expectations about the extent of future competition from China.

Finally, motivated by the recent empirical evidence on the importance of Input-Output linkages for the economy, we investigate whether *indirect* exposure to trade policy uncertainty through domestic and international IO linkages predicts average returns during the tariff uncertainty period. We construct three measures of linkages in production that may affect each industry's exposure to TPU. The first, "China Upstream Exposure", is a weighted average of the NTR gaps of the sectors from which an industry is sourcing inputs from China. Intuitively, uncertainty about future tariffs applied to intermediate products sourced from China may generate uncertainty on production costs. The second, "Downstream exposure", is a weighted average of the NTR gaps of the purchasers of an industry's output, while the third, "Upstream exposure", is a weighted average of the NTR gap of the suppliers of each industry. A more complete measure of indirect exposure further accounts not only for the uncertainty of an industry's immediate buyers or suppliers, but also for the full set of input-output relationships among all connected industries (e.g., uncertainty of an industry's buyers, its buyers' buyers, etc), as in Acemoglu et al. (2016). Applying such full input-output measure of exposure to TPU, we find that US industries which were more reliant on higher NTR-gap sectors in China earned an additional 1.6% per year, while those more exposed to downstream TPU earned an additional 2.5%, and those more exposed to upstream TPU earned an additional 4.6% per year.

Another piece of evidence in favor of the importance of IO linkages for the risk premium comes from the relationship between expected returns and industry concentration. We find that the increase in expected returns during the policy uncertainty period was smaller for more concentrated industries. Intuitively, such industries could more easily pass on higher input costs, arising from higher tariffs on Chinese inputs, to their buyers (see e.g. Ali et al. (2008)). Therefore, they were less subject to tariff uncertainty arising from upstream exposure from China, and thus commanded a lower risk premium.

There is an extensive literature that attempts to empirically assess how policy uncertainty is priced into stocks and options (see e.g. Pastor and Veronesi (2012), Pástor and Veronesi (2013), Brogaard and Detzel (2015), Kelly et al. (2016), Christou et al. (2017), Baker et al. (2018), and Greenland et al. (2019)), and economic uncertainty in general (see e.g. Fillat and Garetto (2015), Barrot et al. (2018)). The methodology proposed in this paper has several advantages relative to this literature. First, the identification strategy relies on non-NTR tariff rates that were set 70 years before the implementation of the policy, providing the quasi-experimental variation needed to estimate the risk premium. Aside from satisfying exogeneity, it has the advantage of being an *ex-ante* measure of uncertainty, and therefore is not subject to a look-ahead bias. In addition, our measure of uncertainty is directly observable, and thus its construction is not subject to measurement error, nor does it rely on assumptions about the underlying volatility process and the investors' objective function.[8]

Our paper is complementary to the empirical literature that investigates the effects of trade policy uncertainty, and economic uncertainty in general, on *current* economic outcomes, such as Pierce and Schott (2016), Handley and Limão (2017), Feng et al. (2017), Esposito (2018), Crowley et al. (2018) and Steinberg (2019). Our contribution is to assess how tariff uncertainty influences investors' expectations of *future* risk and cash flows, affecting firms' stock returns, which are an important determinant of household wealth, firms' value and

[8]For instance, the widely-used method in Carr and Wu (2008) uses ex-post realized variance as a proxy for ex-ante expected variance, introducing a look-ahead bias. Bollerslev et al. (2009) uses lagged volatility as a measure of expected future volatility, but it relies on the strong assumption that volatility follows a martingale.

investment decisions.

We also contribute to the recent literature on how China's accession to WTO, and specifically the granting of PNTR, has affected the US economy.[9] Pierce and Schott (2016), Acemoglu et al. (2016) and Pierce and Schott (2017) have documented that US manufacturing industries more exposed to import competition from China experienced a decline in employment and investment. Our paper offers a novel set of empirical results that shed light on the effects of the uncertainty associated with China's WTO accession on stock returns, while at the same time controlling for the effect of Chinese competition on realized and expected returns.

Finally, our work contributes to the literature that studies the impact of input-output linkages on economic outcomes, such as Caliendo and Parro (2014), Blaum et al. (2015), Antras et al. (2017), and Wang et al. (2018), and on stock returns, as Cohen and Frazzini (2008) and Huang et al. (2018). Our contribution is to show that sectoral production linkages significantly amplify the effect of trade policy uncertainty on stock returns.

The paper proceeds as follows. Section 2 documents the effect of tariff uncertainty on average stock returns across US manufacturing industries. Section 3 argues that such effect is a risk premium for exposure to trade policy uncertainty, and rules out alternative explanations. Section 4 extends the analysis to examine the role of input-output linkages on expected stock returns, while Section 5 concludes.

2 Tariff Uncertainty and US Stock Returns

In this section, we use quasi-exogenous variation in exposure to tariff uncertainty across US industries to identify the causal effect of trade policy uncertainty on stock returns.

2.1 Data and identification strategy

Starting in 1980, US imports from China were subject to the relatively low Normal Trade Relations (NTR) tariff rates reserved for members of the World Trade Organization (WTO).[10] From 1980 to 1989, renewal of these NTR rates for China was essentially automatic. After the Tiananmen Square incident in 1989, however, the US House of Representatives introduced and voted on legislation to revoke China's temporary NTR tariffs every year from 1990 to

[9]Recent papers studying the impact of Chinese competition on the US include Autor et al. (2013), Adao et al. (2018) and Caliendo et al. (2019), among others. Coelli (2018) studies the impact of PNTR on innovation by Chinese firms. Griffin (2018) studies the effect of PNTR on US industry concentration and stock market listing.

[10]US president Jimmy Carter began granting such waiver to China annually in 1980, under the premises of the US Trade Act of 1974.

2001. If Congress had failed to roll over the NTR rates, import tariffs on Chinese goods would have reset to the higher rates established in the Smoot-Hawley Tariff Act of 1930.

Anecdotal evidence from media reports suggests that US companies were concerned about threats to withdraw China's NTR rates. Testifying before the House on June 1997, Eugene Milosh, President of the American Association of Exporters and Importers, stated: "Any annual review process introduces uncertainty, weakening the ability of U.S. traders and investors to make long-run plans, and saddles US/China trade and investment with a risk factor cost not faced by our international competitors".[11]

In October 2000, the United States granted China Permanent Normal Trade Relations (PNTR) conditional on China joining the WTO. China joined the WTO at the end of 2001, and PNTR went into effect at the start of 2002. Granting China PNTR *permanently* removed tariff uncertainty by fixing US taxes on Chinese imports at NTR levels.

Note that the likelihood of a policy change was the same for all industries, since either all would revert to Smoot-Hawley rates, or all would keep lower NTR rates. However, the *exposure* to tariff uncertainty was different across industries, depending on the difference between Smoot-Hawley tariffs and the NTR rates. Therefore, we follow Pierce and Schott (2016) and quantify the exposure to tariff uncertainty via the "NTR gap", defined as the difference between the NTR and non-NTR rates to which tariffs would have risen if annual renewal had failed:

$$NTRGap_{it} = NonNTR_i - NTR_{it} \tag{1}$$

where i stands for industry and t for year.[12]

We investigate how tariff uncertainty, as measured by NTR gaps, affected the perceived riskiness of US industries throughout the years 1990-2001. In our view, there are two main ways in which trade policy uncertainty could have affected firms' cash flows and discount rates. The first, which we name "competition channel", refers to the fact that the threat of higher tariffs may have deterred Chinese firms from entering US markets, as documented by Pierce and Schott (2016). Thus, industries with higher NTR gaps had more uncertainty about the extent of future Chinese competition. The second, which we name the "input channel", refers to the fact that uncertainty about future tariffs applied to intermediate products sourced from China may have generated uncertainty on production costs. Our empirical findings suggest that the competition channel had a larger effect on expected returns than the input channel. Therefore, we focus on that in the baseline specification, while we

[11] See Online Appendix of Pierce and Schott (2016) for additional pieces of anecdotal evidence.

[12] Pierce and Schott (2016) compute NTR gaps using ad-valorem equivalent NTR and non-NTR tariff rates from 1989 to 2001 provided by Feenstra et al. (2002). Both types of tariffs are set at the eight-digit Harmonized System (HS) level. The gap for industry i is the average NTR gap across the eight-digit HS tariff lines belonging to that industry.

document the effect of the input channel in Section 4.1.[13]

Our identification relies on the fact that, as shown in Pierce and Schott (2016), 79% of the variation in the NTR gap across industries arises from variation in non-NTR rates, set 70 years prior to passage of PNTR. This feature of non-NTR rates mitigates concerns of reverse causality, that would arise if non-NTR rates could be set to protect struggling industries.[14]

Our difference-in-differences identification strategy exploits the large cross-sectional variation in the NTR gaps across US industries in the years 1990-2001, *before* China was granted PNTR. We compare the stock returns of US manufacturing firms in high NTR gap industries to low NTR gap industries (first difference), during the uncertainty period, 1990-2001, versus the years 1980-1989 and 2002-2007 (second difference). These potential tariff increases were substantial: in 1999, the last year before conditional PNTR was granted, the average NTR gap across industries was 29% with a standard deviation of 15.6%. The distribution of NTR gaps in 1999 is displayed in Figure 3.

In order to have time-consistent industry definitions for tracking stock returns and other controls over our sample period, we use the algorithm developed in Pierce and Schott (2012) to create "families" of four-digit SIC industries. Unless otherwise noted, all references to "industry" in this paper refer to these families.

To compute industry-level stock returns, we start with the universe of publicly listed US firms in CRSP that can be matched to Compustat, from where we download all the firm-level variables used as controls in the regressions. We then filter for ordinary common shares traded on major exchanges (NYSE, AMEX and NASDAQ). We match the SIC code in Compustat to the Pierce and Schott (2012) families of industries and only keep the matched firms.[15] Each month, we construct value-weighted portfolios at the industry level, where the weights are proportional to each firm's 1-month lagged market capitalization. We value-weight the portfolios to reduce the influence of small firms (see e.g. Hou et al. (2017)). Table 1 reports some summary statistics about our sample in 1989 and 1999. We can see that low gap industries were, at the beginning of the uncertainty period, larger than high-gap industries, while they had the same market capitalization in 1999, right before conditional PNTR was granted.

[13]In Section 4.1, we also explore how exposure through upstream and downstream linkages within US industries affect expected returns.

[14]Nevertheless, in Section 2.3.2 we perform several tests of the exogeneity of our proxy for tariff uncertainty.

[15]Our procedure of deferring to the SIC code in Compustat, rather than the SIC code in CRSP, has also been adopted in Barrot et al. (2018). As a robustness check, we run a version of our baseline specification using CRSP industries in Table 4, and find similar results.

2.2 Main specification

In order to assess the statistical significance of the differences in average returns across high and low gap industries, and to control for confounding factors, we run the following regression at the US industry/month level:

$$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 PrePNTR_t + \beta_3 NTRGap_{i,y-1} + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it} \quad (2)$$

where the dependent variable is the return of each value-weighted industry portfolio i in month t and year y, for the years 1980 to 2007. The first term on the right-hand side is the Difference-in-Differences (DID) term of interest, an interaction of the one year-lagged NTR gap and an indicator for the pre-PNTR period, i.e. the years characterized by tariff uncertainty, 1990-2001.[16] We also control for the un-interacted indicator for the pre-PNTR period and for the industry-level NTR gap in 1990, but in other specifications we add industry and time fixed effects. $\boldsymbol{X}_{it-1}$ is a vector of lagged industry-time controls, to be specified below.

Using time-varying NTR gaps prevents a look-ahead bias, and it allows for time variation in the measure of uncertainty, therefore better identifying the response of stock returns over time.[17] We use the lagged NTR gaps to avoid endogeneity issues, which may arise if NTR tariff rates responded to contemporaneous changes in stock returns. In addition, the implicit assumption is that every year, investors' used previous-year NTR gaps to assess the level of each industry's tariff uncertainty.[18]

Regression estimates are weighted by industry stock market capitalization in January, 1989, the year before the uncertainty period started.[19] Weighting by market capitalization in 1989 minimizes the influence of small industries, and avoids biasing our results toward firms that eventually became large, specifically firms that benefited from threatening to revoke China's NTR rates. Finally, standard errors are clustered at the industry level to allow for arbitrary error correlations within industries over time. The final sample consists of 130 different industries, over 27 years, for a total of 41,241 observations.

[16] Although President Clinton signed the law granting PNTR in October 2000, China actually entered the WTO in December 2001. Protracted accession negotiations meant that in the summer of 2001, Congress again voted on whether to revoke China's NTR rates. Therefore we include the year 2001 in the uncertainty period. Results are similar when removing the year 2001 entirely.

[17] As shown in Section 2.3.2, our results go through even if we fix the NTR gap at the first year of uncertainty, 1990, or the last year before conditional PNTR was granted, 1999.

[18] The absence of monthly-level data on tariffs prevents the construction of NTR gaps at the monthly level.

[19] In the robustness section 2.3 we experiment with weighing by previous year-end market capitalization and market capitalization in January, 1979. We find that the results are similar.

The baseline results are shown in Table 2. Column 1 shows the result of the regression in equation (2) without controls. One concern is that contemporaneous policy changes related to China's accession to the WTO could have influenced the performance of US industries over our sample period. We control for these policy changes by including, in Column 2, NTR tariff rates, Chinese import tariffs from Brandt et al. (2012), and data on US textile and clothing quotas from Khandelwal et al. (2013).[20] Column 3 estimates the same regression as Column 2 but only for industries for which we have complete data on control variables in Compustat, to account for a possible selection effect before adding all the controls. Column 4 adds several industry-level financial characteristics known to be correlated with expected returns, as well as several valuation and leverage metrics, such as the industry average price/earnings ratio, price/book ratio, return on investment, return on equity, the EV/EBITDA ratio, debt/equity ratio and current ratio.[21] To eliminate the possibility that a size effect (see e.g. Banz (1981) or Fama and French (1993)) is driving our results, we also control for one-year lagged industry market capitalization.[22] In Column 5 we follow Petersen (2009), including dummy variables for each time period (month) and clustering the standard errors by industry.[23] This represents the "baseline" specification to which we refer throughout the remainder of the paper. The reason we do not include industry fixed effects in the baseline is because we want to measure the average effect of the NTR gap on stock returns across the whole sample. In Table 3, we add industry fixed effects and show that the results are similar.

We can see that the coefficient on the DID term of interest is positive and statistically significant throughout all the specifications: this implies that industries with higher uncertainty on the level of tariffs imposed on Chinese imports had relatively higher stock returns during the uncertainty period. Notice that the point estimate of the DID coefficient increases as we add controls. One possible reason is that, as documented in Table 1, firms in high-gap industries had high market-to-book ratios relative to low-gap industries, i.e. they were on average growth firms, which typically have lower expected returns. Once we control for this and other previously omitted characteristics correlated with average returns, we expect the coefficient on the interaction term to increase. Interestingly, the coefficient on the un-interacted NTR gap term is negative across all specifications, and statistically significant once we add controls for contemporaneous policy changes. We argue this is the result of low realized returns in the post period, as documented in Section 2.3.

[20] Between 2002 and 2005, the Multi Fiber Arrangement implied the removal of import quotas on textile and apparel imports from less-developed countries, including China.

[21] In Appendix 7, we describe the methodology used to compute the variables and their data sources.

[22] Even without controlling for firm size, it is unlikely that a size effect is driving our result, as size effects have been shown to be weak after 1990, see e.g. Asness et al. (2018). Results are also robust to controlling for one-month lagged market capitalization.

[23] For a more thorough analysis of the standard errors, see Section 2.3.

The difference-in-differences coefficient of 0.021 in the baseline specification is significant at 1% level, and it implies that going from an industry less exposed from trade policy uncertainty (at the 25th percentile of the distribution of NTR gaps in 1990), such as ''Aluminum Sheet, Plate, and Foil Manufacturing'', to an industry more exposed to trade policy (at the 75th percentile of the distribution), such as ''Heating Equipment Manufacturing'', increases stock returns by 4.3% per year during the uncertainty period.

2.3 Robustness

In this section we perform several exercises to gauge the robustness of our baseline results.

2.3.1 Different specifications

In our baseline specification, if time effects are not correlated, clustering by industry and including time fixed effects should yield unbiased standard errors (see Petersen (2009)). If there is time-series autocorrelation in the regression residuals, however, time fixed effects and industry clustering would, instead, yield biased standard errors. We address this issue in Table 3. Column 1 reports the results of the baseline specification, but with standard errors double clustered at the industry and month level, which would account for the potential autocorrelation of the industry-level residuals. With the double clustering, the coefficient of interest is still significant at the 5% level.[24] Column 2 adds industry fixed effects, while still double clustering at the industry and month level. These first two columns suggest that autocorrelation in the residuals is not driving our baseline results. With industry fixed effects, the implied effect of moving from the 25th to 75th percentile of trade policy uncertainty is 4.8% higher returns per year. The fact that including industry fixed effects yields an estimate so similar to the specification which just controls for each industry's NTR gap suggests that the main reason for differences in average returns across industries in our sample is the NTR gap itself.

We then investigate the robustness of our results to the weighting scheme. Column 3 weighs observations by the industry's previous year-end market capitalization and Column 4 uses the industry's market capitalization in January, 1979, before the start of our sample. We can see that the point estimates are similar across these two alternative weighting schemes.

In Table 4, we report the results of additional robustness exercises, which confirm that our baseline findings are: i) robust to the classification used to define manufacturing industries,

[24] A parametric way to account for the autocorrelation in the residuals is panel Newey-West. In unreported results, we re-run the baseline using this technique with 12 lags, and find our coefficient of interest is still significant at the 5% level.

ii) not driven by the dot-com crash, iii) robust to controlling for exposure to market risk and 5 Fama-French factors, iv) robust to restricting to the constant manufacturing sample in Pierce and Schott (2016).

2.3.2 Threats to Identification

One concern with our identification strategy is that the US government could have set high tariff rates on WTO members, i.e. high NTR rates, to protect industries that they expected to perform poorly after PNTR. In this case, while the non-NTR rates are exogenous because they were set by the Smoot-Hawley act, NTR rates are not. A second related concern is that the decision to vote on legislation to revoke China's temporary NTR status could have motivated by economic reasons, rather than geo-political reasons, i.e. the Tiananmen Square incident. This could generate an omitted variable problem in our regressions, leading to biased estimates.

To mitigate these concerns, we examine four alternative specifications. First, we estimate the DID regression using the NTR gap in 1990, at the beginning of the uncertainty period, rather than the time-varying lagged NTR gap. Similarly, in a second specification we employ the NTR gap in 1999, the year before conditional PNTR was granted, near the end of the uncertainty period. As indicated in Columns 1 to 4 in Table 5, in both cases, both with and without controls, the DID coefficient remains statistically significant and close to the baseline. Third, we follow Pierce and Schott (2016) and estimate a two-stage least squares specification in which we instrument the baseline DID term, $PrePNTR_t \times NTRGap_{it-1}$, with an interaction of the pre-PNTR indicator and the Smoot-Hawley non-NTR tariff rates, $PrePNTR_t \times NNTR_{it}$. As reported in Columns 5 and 6 of Table 5, the DID coefficient is positive, statistically significant and close to the baseline. Lastly, we perform a placebo test, in which we draw a random NTR gap from the cross-sectional distribution of gaps each year and randomly assign it to an industry. If the gap, rather than some unobserved industry-by-time component, is responsible for the observed differences in returns, we would expect the coefficient on the placebo gap to be insignificant. Reassuringly, this is indeed what Columns 7 and 8 in Table 5 report.

A third concern is that high-gap firms may have higher expected returns than low-gap firms because of unobserved differences in fundamentals or shocks between 1990 and 2001, besides the ones we are able to control for in the regressions. To mitigate this concern, we estimate the baseline specification using stock returns for a group of high-income countries, including Australia, France, Japan, South Korea, and UK.[25] Table 6 reports that there is no

[25] We only include countries for which we have a good coverage of the time series and cross-sectional dimensions of the data in Compustat Global.

significant relationship between the NTR gap and expected returns in these countries during the period of US trade policy uncertainty, suggesting that our results are not driven by some unobserved shocks to high-gap and low-gap industries.

3 A Risk Premium for Tariff Uncertainty

Our empirical results show that high gap industries had higher average returns, relative to low gap industries, before China was granted PNTR. Further, this difference in expected returns seems to be only explained by the NTR gap itself. In this section, we argue that this difference in returns can be interpreted as a risk premium for exposure to tariff uncertainty. We start by showing why our baseline regression specification is well suited to capture such a risk premium.

3.1 Reduced-form model

Suppose we have the following return-generating process:

$$r_{i,t} = a_i + b_i r_{m,t} + c_i r_{p,t} + e_{i,t} \tag{3}$$

where $r_{i,t}$ is the return for industry i in month t, $r_{m,t}$ is the return on the market portfolio and $r_{p,t}$ is the return on a "Trade Policy" portfolio. This trade policy portfolio is designed to reflect changes in the probability that China's tariff status will change. In other words, that tariffs on Chinese goods will either i) be raised to Smoot-Hawley levels, ii) be permanently set to NTR rates, or iii) continue at NTR rates with annual renewals. We assume that such a Trade Policy portfolio should either be i) long high-NTR-gap firms and short low-NTR-gap firms, or ii) long low-NTR-gap firms and short high-NTR-gap firms.[26]

Assuming that the risk associated with policy changes is priced, the return-generating process implies the following for expected returns:

$$E[r_{it}] = b_i MRP_t + c_i PRP_t \tag{4}$$

where MRP_t is the expected market risk premium at time t, but could be generalized to any set of factor risk premia, such as size, value, etc., as we do in the portfolio analysis that follows. PRP_t is the expected policy risk premium, i.e. the compensation for risk associated with policy uncertainty. This reduced-form specification can be micro-founded, for instance,

[26] We cannot distinguish between options (i) and (ii) without making an assumption about the sign of the price of risk.

with the model in Pástor and Veronesi (2013), in which investors must be compensated for uncertainty about the future costs of government policy actions. We assume for simplicity that b_i and c_i, the loadings of industry i on the risks, are constant. This is a reasonable assumption for c_i, since non-NTR tariff rates (and thus exposure to the tariffs) were set by the Smoot-Hawley act in 1930.

It is useful at this stage to recall the timeline of the events. Although China was granted temporary WTO-member tariff rates in 1980, annual renewals were almost automatic until the Tiananmen Square incident in 1989. Then, from 1990 until China joined the WTO in 2001, NTR status required annual renewal by Congress. After 2001, this uncertainty was removed permanently. Following the identification strategy used in the previous section, we assume that there are two types of firms: (1) High-gap, which are exposed to trade policy uncertainty and thus have $|c_H| > 0$; (2) Low-gap, which are not exposed to trade policy uncertainty, and thus have $c_L = 0$. Note that in our empirical specification we use the NTR gaps to proxy for the *magnitude* of the exposure to trade policy uncertainty, but do not ex-ante take a stance on the *sign* of such exposure, i.e. the sign of the price of risk. For this reason, we impose here that the *absolute* value of c_H is different from zero.

We define $t = 1$ for the 1980-1989 period, $t = 2$ for the uncertainty period 1990-2001, and $t = 3$ for the 2002-2007 period. This implies that $PRP_1 = PRP_3 = 0$, because there was little or no tariff uncertainty during the first and third period. On the other hand, $|PRP_2| > 0$.

Through the lens of this reduced form model, when we run the difference-in-difference regressions shown in the previous section, we are essentially testing the following hypothesis:

$$(E[r_{2H}] - E[r_{2L}]) - (w_1 (E[r_{1H}] - E[r_{1L}]) + w_3 (E[r_{3H}] - E[r_{3L}])) = 0$$

where we assume for now that $w_1 + w_3 = 1$.[27] Because $c_L = 0$ and $PRP_1 = PRP_3 = 0$, as discussed before, this simplifies to:

$$(b_H MRP_2 + c_H PRP_2 - b_L MRP_2) - \left(\left(w_1 b_H MRP_1 - w_1 \frac{1}{2} b_L MRP_1 \right) + (w_3 b_H MRP_3 - w_3 b_L MRP_3) \right) = 0$$

Under the assumption that the market risk premium and the loadings on the risks do not change over time, and that the weights add up to one, this further simplifies to:

$$c_H PRP_2 = 0 \tag{5}$$

Therefore, our difference in differences regression is designed to test the following null and

[27] In the DID regression we do not exactly equally weigh the control and treatment periods. However, if we properly rescale the weights such that the observations in the control and treatment periods are equally weighted, results do not change much.

alternative hypotheses:

- Null: $c_H PRP_2 = 0$. Given that we assumed $|c_H| > 0$, this would only be true if $PRP_2 = 0$. Empirically, this could occur if trade policy uncertainty was not priced because it was fully diversifiable.

- Alternative 1: $c_H PRP_2 < 0$. This will be true if either i) $c_H > 0$ and $PRP_2 < 0$ or ii) $c_H < 0$ and $PRP_2 > 0$. This could occur if the market perceived trade policy uncertainty as good news for high gap firms, because it deterred Chinese firms from competing with US firms, thus making high gap firms *less* risky.

- Alternative 2: $c_H PRP_2 > 0$. This will be true if either: i) $c_H > 0$ and $PRP_2 > 0$ or ii) $c_H < 0$ and $PRP_2 < 0$. As discussed earlier, this alternative does not take a stance on the price of risk, but implies that high gap firms were perceived as *more* risky and thus earned higher returns during the period of policy uncertainty.

Empirically, our baseline regression is consistent with alternative hypothesis 2: high gap firms earned higher returns during the period of policy uncertainty, because of their exposure to trade policy uncertainty.[28] In the following subsections, we first provide additional evidence that corroborates this hypothesis. We then discuss alternative explanations for our results and show that they are not consistent with the empirical evidence.

3.2 Additional empirical evidence

3.2.1 Portfolio analysis

We first turn to a portfolio analysis. The advantage of undertaking a portfolio analysis is that idiosyncratic firm-level shocks within each portfolio cancel out if the sample is sufficiently large. Further, this helps overcome the econometric concern that the residuals are autocorrelated. It also eliminates the issue of a possibly not positive-definite covariance matrix when estimating the baseline regression with double-clustered standard errors.[29]

[28] Note that, even if trade policy uncertainty does not directly enter the Stochastic Discount Factor, as in equation (4), tariff uncertainty may still explain our results. Suppose investors are ambiguity averse, as in Gilboa and Schmeidler (1989). In such a model, investors assume the worst case scenario, and thus increases in uncertainty about China's NTR status are bad news for investors, who then need to be compensated with higher returns.

[29] Numerical simulations, available upon request, show that if systematic risks are omitted or incorrectly measured, then double clustering can lead to a negative definite covariance matrix. Also note that we do not perform Fama MacBeth regressions because, in our setting, we only observe an ordinal ranking of exposure to the risk factor, i.e. the NTR gaps, but not the risk factor or factor beta itself.

We first rank all the industries in our sample in 3 sub-groups, based on the NTR gap in 1990, such that there is an equal amount of market capitalization in each group.[30] This approach reduces the influence of very small firms that appear in industries with the highest NTR gaps. We construct value-weighted portfolios, with weights proportional to each firm's market capitalization in the previous month, and calculate monthly returns between 1980 and 2007. We then construct a "Trade Policy Uncertainty" (TPU) portfolio, which is the difference in returns between the industries with the highest and lowest gaps. We then run the following regression, separately for each portfolio p:

$$R_t^p = \theta PrePNTR_t + \boldsymbol{F}_t' \lambda + \alpha + \epsilon_t \tag{6}$$

where R_t^p is the excess return on portfolio p in month t, $PrePNTR_t$ is a dummy equal to one if the year is between 1990 and 2001, and $\boldsymbol{F}_t$ is a vector containing the 5 Fama and French (2015) factors: Market, Size, Value, Profitability and Investment.[31]

Our results in Table 7 show that, even after conditioning on the Fama and French (2015) 5-factor model, the TPU portfolio experienced higher average risk-adjusted returns during the uncertainty period, significant at a 5% level. Furthermore, the average returns before PNTR are monotonically increasing from the low gap to high gap portfolios. These results imply that trade policy uncertainty was, between 1990 and 2001, a systematic factor that could *not* be diversified away across stocks with similar NTR gaps.

3.2.2 Realized Volatility

As discussed earlier, Pastor and Veronesi (2012) and Pástor and Veronesi (2013) both predict that political uncertainty commands a risk premium. Another theoretical prediction is that exposure to policy uncertainty is associated with higher realized volatility. In this section we investigate whether uncertainty about US-China trade relationships was also associated with more volatile stock prices. Intuitively, as the probability of a policy change varied through the 1990's with Congressional votes, news and Presidential actions, high-gap firms' stock prices should have varied more in response to such events, relative to low-gap firms' stock prices. Once the policy uncertainty was resolved, this additional component of volatility would disappear, and both high and low gap firms should have similar realized volatility. We test this hypothesis with the following regression:

[30] Given the lack of yearly NTR rate data early and late in our sample, any classification is always going to involve some look-ahead bias, i.e. using NTR gaps from future dates to form portfolios. We find that our results are robust to forming 3 groups based on the NTR gap in 1999. Note also that we keep the groups fixed after tariff uncertainty was resolved, even though the NTR gap was no longer related to policy uncertainty.

[31] We obtain the monthly returns on these factors from Ken French's `https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html`.

$$RV_{jt} = \theta \cdot PrePNTR_t \times NTRGap_{jy-1} + \boldsymbol{X}'_{jt}\lambda + \delta_j + \delta_t + \alpha + \epsilon_{jt} \tag{7}$$

where j is a US industry, t is a month. Realized volatility, RV_{jt}, is computed as the sum of squared daily returns for each firm in month t, and is aggregated at the industry level using last-month market capitalization as weights. We use the sum of squared daily returns, rather than the standard deviation, to avoid estimation issues in small samples i.e. within a month. Table 8 documents that firms in high-gap industries had significantly higher realized volatility than firms in low-gap industries during the tariff uncertainty period. This lends further support to our reduced form model of returns, and using the Pastor and Veronesi model to micro-found our baseline results.

3.3 Alternative Explanations

In this section we discuss a number of alternative explanations that could potentially rationalize our results, and show that they are not consistent with the empirical evidence.

3.3.1 Alternative Explanation I: Realized Returns

A potential explanation for our results could be that the higher returns for high-gap industries in the uncertainty period were driven by *realized* returns, i.e. the response of stock prices to news and policy-related events, rather than expected returns. If capital markets are efficient, stock prices adjust quickly after a news announcement, incorporating any changes in expected future cash-flows and discount rates (see e.g. MacKinlay (1997)). In order to assess this hypothesis, we look at the response of stock returns around policy-related events. We then show that our baseline results are robust to removing the days around policy-related events, and therefore are not driven by realized returns.

Policy announcements. We first perform an event study on days with PNTR-related news announcements to estimate the market's perceived effect of the policy change on firm performance. We focus on three news announcements: i) 10/10/2000, when China was granted NTR, conditional on joining the WTO; ii) 12/11/2001, when China joined the WTO; iii) 1/2/2002, the day the PNTR actually went into effect.

We regress the industry-level daily stock returns in a 5-day window around these dates on the NTR gap in 1999, the year right before the policy events:

$$R_i^{(t-n,t+m)} = \theta NTRGap_{i1999} + \alpha + \epsilon_i \tag{8}$$

where $R_i^{(t-n,t+m)}$ is the cumulative return for industry i from $t-n$ to $t+m$, where t is the event-date of interest. We report results for $n=1$ and $m=3$, but results are robust to the choice of the time window. In this specification, we do not control for differences in industry/firm fundamentals as, given that we are only including a tight window around the announcement, we expect the announcement to be the main factor driving differences in returns.[32]

Columns 1-3 of Table 9 report the results. We can see that, around 10/10/2000, when China was granted PNTR conditional on joining the WTO, industries with higher gaps experienced lower stock returns than industries with lower gaps. We interpret this finding as financial markets perceiving high-gap industries as riskier, or having lower future dividends/cash flows than low-gap industries, as a result of PNTR. In contrast, for the other two dates, we do not find significant difference between high and low gap industries. This is consistent with the effect of PNTR being already priced into stocks by the time China joined the WTO.[33]

Earnings announcements. A concern may be that the differences in stock returns we pick up across high and low gap industries are driven by returns around quarterly earnings announcements. This could be a concern for our research design if investors did not fully anticipate the differential effects of PNTR on firm performance. We identify earnings days using the Institutional Brokers Estimates System (I/B/E/S) database.[34] To avoid issues with missing time of earnings releases early in the sample, and possible discrepancies in earnings days between I/B/E/S and Compustat, we look at the cumulative return from t-5 to t+1, where t is an earnings announcement date, as in Barrot et al. (2018). We run the following regression, using data from 1990-2007:[35]

$$R_{jt} = \theta \cdot PrePNTR_t \times NTRGap_{i1999} + \delta_i + \delta_y + \alpha + \epsilon_{jt} \tag{9}$$

where R_{jt} is the 5-days cumulative returns of firm j in quarter t, δ_i is an industry fixed effect, δ_y is a year fixed effect. We exclude the pre-period, as we want to compare earnings announcement returns during the period of policy uncertainty to the post period. Column 4

[32] In unreported results, we re-run the regression using CAPM residuals, both computed with in-sample betas and out-of-sample betas on the market factor. We find that results are similar, but less statistically significant. One explanation for this is that estimating the betas introduces an additional degree of noise, which over such short horizons could drown out the effects of the news event.

[33] Since the effect of PNTR was already priced in 2000, we repeat the baseline analysis excluding 2001 from the uncertainty period, and results are very similar.

[34] If earnings are announced after the market is closed, or on a trading holiday, we set the effective earnings day to the first trading-day after earnings are announced.

[35] We do not include the pre-period, 1980-1989, in this analysis due to a lack of coverage and a significant number of missing announcement times in IBES.

of Table 9 reports a positive and 5% statistically significant coefficient for the interaction between the NTR gap in 1999 and the 1990-2001 period dummy.[36] Therefore, high-gap industries, around the days of earnings announcements, experienced significantly lower stock returns than low-gap industries, after PNTR was granted. The implied average (relative) effect, however, is small, being only 50 basis points per earnings day. This is consistent with investors initially under-estimating the effect of PNTR on high-gap firms' performance.

Voting days. We also investigate the market's expectations on the effects of revoking MFN status. In particular, we look at the returns of high and low gap stocks around Congressional voting dates:

$$R_{it}^{(t-1,t+3)} = \theta \cdot NTRGap_{iy-1} + \alpha + \epsilon_{it} \tag{10}$$

where $R_{iy}^{(t-1,t+3)}$ is the cumulative return of industry i from t-1 to t+3, where t is the day of the Congressional vote, and $NTRGap_{iy-1}$ is the NTR gap in industry i in the year before the event t. If the outcomes from Congressional votes regarding China's MFN status updated investors' beliefs about the likelihood of PNTR and its effects on US firms, we should observe instantaneous responses in stock returns. As highlighted in Table 10, results are mixed: high gap firms responded more (in absolute terms) to the voting announcements than low gap firms, but the direction is not always the same.[37] This is consistent with the policy announcements driving increased volatility, but not higher expected returns.

Expected returns revisited. In light of this evidence about the effect of policy-related events on realized returns, we repeat the main specification in equation (2), but exclude 7-day windows around PNTR-related announcement dates, earnings announcements, and Congressional voting dates on China's NTR tariff rates. We exclude these days and sum daily log-returns to compute monthly returns. We do this, rather than compounding the daily returns up to monthly returns, to reduce the influence of convexity. Column 1 in Table 11 reports the results from the baseline specification, but using log returns. Not surprisingly, the implied effect is 2.58%, smaller than the main specification in Table 2, where we use continuously compounded returns. Columns 2-5 document that removing the policy-related

[36] As for policy announcement dates, we use the NTR gap in 1999 because that was the last year before the uncertainty was removed, and thus we can use such variable to sort industries after PNTR.

[37] Although the results on signed returns are mixed, our prior is that high-gap firms should respond more (in absolute value) to news about possible policy changes, consistent with the model in Pástor and Veronesi (2013). In unreported results, when we pool together all voting days, we find that the absolute returns are larger on average for high gap firms than low gap firms. We also conducted a test where we choose days between the voting days to be placebo voting days. We find no difference in average absolute returns between high and low gap firms on these placebo voting days. This confirms that high-gap firms were more sensitive to voting news than low-gap firms.

dates does not significantly alter the estimated risk premium, which is down to 2.41% in our most conservative estimate.

It is possible that the days we excluded above are not all of the days in which relevant news about NTR-related policy changes were released. For example, in the pre-period, high gap firms may have been negatively shocked by China being initially granted temporary NTR rates in 1980, and in 1989 when it became obvious that Congress would try to revoke China's MFN status after the Tiananmen Square crackdown. In the post-period, high gap firms may have experienced a series of negative shocks, as investors learned about the negative effects of Chinese competition on more exposed US industries (see e.g. Pierce and Schott (2016), Acemoglu et al. (2016) and Pierce and Schott (2017)).

To address this concern, in Table 12 we re-run the baseline regression with different sample periods. Column 1 shows the baseline results, while Column 2 excludes the pre-period 1980-1989, and Column 3 excludes the post-period, 2002-2007. By dropping the pre- and post-periods, we remove all the realized returns that occurred there. Finally, Column 4 extends the sample to 2017. This is another way to account for realized returns in the 2002-2007 post-period, as by 2007 we would expect the effects of PNTR to be fully incorporated into prices. In all 3 alternative sample periods, our baseline effect remains significant at the 1% level.

3.3.2 Alternative Explanation II: Compensation for Future China Shock

One possible explanation for our results is that industries with higher import penetration after China joined the WTO had low realized returns in the post period, and investors demanded compensation for this expected poor future performance, even before 2001. In this scenario, our estimated risk premium would not be caused by tariff uncertainty per se, but would instead be compensation for Chinese competition risk.

We test this hypothesis in Table 13 with the following triple difference regression, for the years 1991-2007:[38]

$$R_{it} = \theta \cdot PrePNTR_t \times China_i \times NTRGap_{iy-1} + \boldsymbol{X}'_{it}\lambda + \delta_i + \delta_t + \alpha + \epsilon_{it} \qquad (11)$$

in which we interact the baseline DID term with a dummy variable, $China_i$, equal 1 if an industry had above median import exposure from China between 2002 and 2007. Intuitively, we are assuming that during the sample period, investors had perfect foresight on the effects of future competition from China, as proxied by the actual import exposure in the post-PNTR

[38] We exclude the years before 1991, as we cannot match our sample to the import exposure of Acemoglu et al. (2016) in those years.

period. Note that the possibility of forecast errors by the analysts would work against our results, as we have shown that investors under-estimated the actual impact of PNTR on US manufacturing firms. We measure import exposure, ΔIP_{it}, with the change in the imports from China in industry i between 1991 and each year t, divided by the initial industry absorption in 1991, from Acemoglu et al. (2016).[39]

Column 1 replicates the baseline results in the matched sample with import exposure data. Column 2 shows that, despite the baseline DID term remaining significant, the triple interaction term is not significant. Column 3 adds the interaction term between realized ΔIP_{it} and the post period, to further account for unexpected negative shocks from Chinese competition. In this specification, the baseline DID term also remains significant. These results suggest that differences in average returns were not concentrated among industries that were more exposed to future Chinese competition, lending further support to our risk premium hypothesis.

4 Extensions

4.1 IO linkages and Stock Returns

A recent literature has emphasized the importance of input-output linkages in the propagation of a shock to the economy, e.g. Caliendo and Parro (2014), Boehm et al. (2015) and Acemoglu et al. (2016). We analyze the role of such linkages in the effect of tariffs uncertainty on expected returns.

Anecdotal evidence from media reports and congressional testimony suggests that uncertainty on the tariff status of suppliers and buyers was indeed a concern for US companies. For instance, testifying before the Senate in June 1996, Harry Pearce, Chief Financial Officer of Tyco Toys Inc., declared: "We cannot plan and run our business if we are wondering whether our most important source of supply is about to disappear. Without continuity and certainty of supply, American toy companies also cannot plan to take advantage of the growing Chinese market."

We identify three sources of linkages in production that may affect an industry's exposure to trade policy uncertainty. The first is related to the fact that US firms, besides competing with Chinese firms on goods markets, may use Chinese products as intermediate inputs in production. Uncertainty about US tariffs applied to Chinese inputs, then, translates into uncertainty about the cost of production. To capture the exposure of a US industry to tariff uncertainty through inputs sourced from China, we construct the "China Upstream

[39] Absorption is measured as industry shipments, plus industry imports, minus industry exports.

Exposure'' as follows:

$$\eta_j^c = \sum_s \omega_s^j Gap_s \tag{12}$$

where ω_s^j is the share of intermediate inputs expenditures of US industry j on Chinese industry s, and Gap_s is the NTR gap of industry s.[40] Intuitively, industries that, before 2000, were sourcing inputs from Chinese industries that were threatened by potentially high tariffs, could have been perceived as riskier by financial markets, affecting their expected returns.

The second and third sources of IO linkages refer to production linkages within the entire US economy. In particular, we follow Acemoglu et al. (2016) and construct the ''Downstream Exposure'' measure as:

$$\eta_j^d = \sum_s \alpha_j^s Gap_s \tag{13}$$

where α_s^j is the share of industry j's total sales that are used as inputs by industry s. Thus, (13) is a weighted average of the tariff uncertainty, proxied by the NTR gap, faced by the purchasers' of j's output. Selling a large fraction of the sales to industries highly exposed to tariff uncertainty, could potentially increase the uncertainty on industry's sales and thus profits, affecting an industry's expected returns.

Similarly, we construct the ''Upstream Exposure'' measure as:

$$\eta_j^u = \sum_s \pi_s^j Gap_s \tag{14}$$

where π_{st}^j is the share of intermediate inputs expenditures of industry j on industry s. Thus, (14) is a weighted average of the tariff uncertainty, proxied by the NTR gap, faced by the suppliers of industry j. If an industry sources a large fraction of its inputs from industries highly exposed to tariff uncertainty, that can increase the uncertainty on industry j' s production costs and thus profits, affecting its expected return.

The upstream and downstream measures just described only capture the exposure of an industry to its ''direct'' purchasers or suppliers. However, each supplier (buyer) of an industry is itself exposed to the uncertainty faced by its suppliers (buyers), and they are in turn exposed to uncertainty of their suppliers (buyers), and so on. To account for the full chain of linkages in production, we follow Acemoglu et al. (2016) and compute for each industry the inverse of the Leontief matrix of US downstream linkages:[41]

[40] For brevity we omit the time subscript, but, as in the baseline, for every year we use the NTR gaps of the previous year.

[41] These different measures of IO linkages can be easily derived by means of a first order approximation of a trade model with a production function with constant input expenditure shares, i.e. Cobb-Douglas, and CES demand, in which gross profits are a constant share of revenues. See Acemoglu et al. (2016) for details.

$$\eta_j^{Ld} = \sum_s l_j^s Gap_s \tag{15}$$

where l_j^s is the j-th element of the s-th column of the matrix $(I - A)^{-1}$, which is the is the Leontief inverse of the IO matrix A. Similarly for US upstream linkages:

$$\eta_j^{Lu} = \sum_s l_j^s Gap_s \tag{16}$$

where l_j^s is the j-th element of the s-th row of the Leontief inverse. To avoid identification issues, related to the fact that IO linkages may have endogenously changed over time, we construct the trade shares, needed to construct the exposure measures, using data for the first available year. Specifically, to compute the shares ω_{st}^j needed to construct η_j^c, we use data on manufacturing trade flows in final goods and intermediate inputs from the World Input Output Database for 1995. Instead, we compute the shares α_j^s, π_s^j and l_j^s using the US Input-Output table from the BEA in 1992 and we follow the cleaning procedure used in Acemoglu et al. (2016). For all the measures, we multiply the constructed trade shares with the lagged NTR gaps, and run the following specification using the entire sample 1980-2007:

$$R_{it} = \theta \cdot PrePNTR_t \times \eta_{jt-1}^k + \boldsymbol{X}'_{it}\lambda + \delta_i + \delta_t + \alpha + \epsilon_{it} \tag{17}$$

for $k \in (c, d, u, Ld, Lu)$, where as before $PrePNTR_t$ is equal 1 for the years 1990-2001 and 0 otherwise.

Results in Table 14 provide evidence that PNTR's effect on stock returns can be transmitted through supply chains. Column 1 shows that a higher upstream exposure from China, i.e. a higher weighted average of the NTR gaps of the Chinese industries used as inputs, implied higher expected returns before the uncertainty was removed. This effect is significant at 1% level. In other words, industries that were sourcing more from riskier sectors in China were also commanding a risk premium before PNTR. Columns 2 and 3 in Table 14, instead, show no significant effect on the risk premium of having more uncertain customers or suppliers. Instead, columns 4 and 5 show that if we incorporate higher-order IO linkages, both downstream and upstream exposures were significantly priced into stock returns during the uncertainty period.

Columns 6-10 also include the industry's own NTR gap, to control for the direct effect of TPU on stock returns. When including the NTR gap itself in the IO linkages regressions, the gap remains significant in all specifications, but we lose significance on the IO linkages measures. The reason is that the upstream/downstream measures are highly collinear, by construction, with the industry's own NTR gap, and this significantly inflates the standard errors.[42] If we control for the industry NTR gap, the implied effects of moving from the 25th to 75th percentile of Chinese upstream exposure is a 1.6% yearly risk premium, in addition to the effect of the NTR gap itself. Similarly, moving from the 25th to 75th percentile of US downstream exposure implies a 2.5% per year increase in expected returns, while for the US upstream exposure the additional effect is 4.6% per year. Therefore, indirect exposure to trade policy uncertainty through Input-Output linkages was significantly priced into stock returns.

4.2 Industry Concentration and Stock Returns

In this section we assess whether the level of sales concentration in an industry affects the risk premium earned for exposure to tariff uncertainty. Intuitively, more concentrated (or, less competitive) industries could more easily pass on higher input costs, arising from higher tariffs on Chinese inputs, to their customers (see e.g. Ali et al. (2008)). Therefore, they should be less subject to tariff uncertainty arising from upstream exposure from China.

We construct industry concentration from Census data using the method in Barkai (2016).[43] The industry data are constructed at the 4-digit SIC level, so in Column 1 of Table 16, we re-run the baseline regression, using value-weighted SIC-4 portfolios.[44] In Columns 2 and 3, we run a triple difference specification, in which we interact the NTR gap in the uncertainty period with two measures of concentration: i) the share of sales going to the top 8 firms, and ii) the Herfindhal Index of sales for the top 50 firms. We find that in both cases, the triple interaction term is significant and negative, suggesting that high-gap and highly concentrated industries had relatively lower returns than high gap and less concentrated industries. This provides additional evidence that upstream exposure is an important channel that affects the risk premium associated with trade policy uncertainty.

5 Conclusions

We use quasi-experimental variation arising from China's temporary NTR status to show that US manufacturing industries more exposed to trade policy uncertainty experienced significantly higher stock returns than less exposed industries. Our measure of uncertainty, which relies on the difference between current NTR and non-NTR tariff rates, has the

[42]For instance, the China downstream exposure has a correlation of 0.9 with the NTR gap.

[43]We are grateful to Simcha Barkai for helping us in the construction of the concentration data.

[44]Aggregating to the Pierce and Schott family of industry level does not make sense, as the market power measure should only be relevant at the most granular-industry level.

advantage of being directly observable, exogenous, and fully ex-ante. As such, it is not subject to the concerns associated with ex-post measures of uncertainty used by the literature.

Our estimated risk premium is substantial, even after accounting for the fact that investors may have under-estimated the effects of granting China PNTR, for the response of stock prices to PNTR-related policy announcements, and for the effect of Chinese import competition on realized and expected returns. Industries highly exposed to policy uncertainty earned a risk premium of 4.3% per year relative to less exposed sectors, suggesting a large impact of trade policy uncertainty on the perceived riskiness of exposed stocks.

Our approach, which exploits cross-sectional variation in industry exposure to uncertainty, allows us to decompose the differences in returns between high and low gap industries into a realized and expected part and we argue that this decomposition is important for the identification of risk premia. In addition, we show that even indirect exposure to trade policy uncertainty, through IO linkages, is priced in the cross-section of stock returns.

While several papers examined the real implications of China's temporary NTR status on employment and investment of firms, we focus on the implications on the financial performance of firms and uncover the potential asset allocation impact of trade policy uncertainty.

Important avenues for future research emerge from our study. Further research into the implications of the mechanisms of trade policy uncertainty through IO linkages on the cross-section of stock returns is worth pursuing as it is an innovative contribution of this paper. Our focus here was on the removal of uncertainty after China entered the WTO, but currently the U.S.-China trade relationships are also subject to political uncertainty. Our results of a large risk premium for this type of uncertainty may serve as a word of caution to policymakers.

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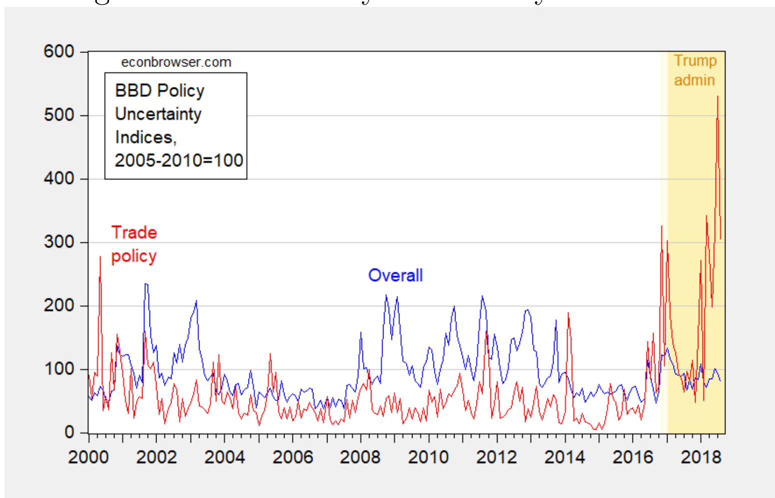
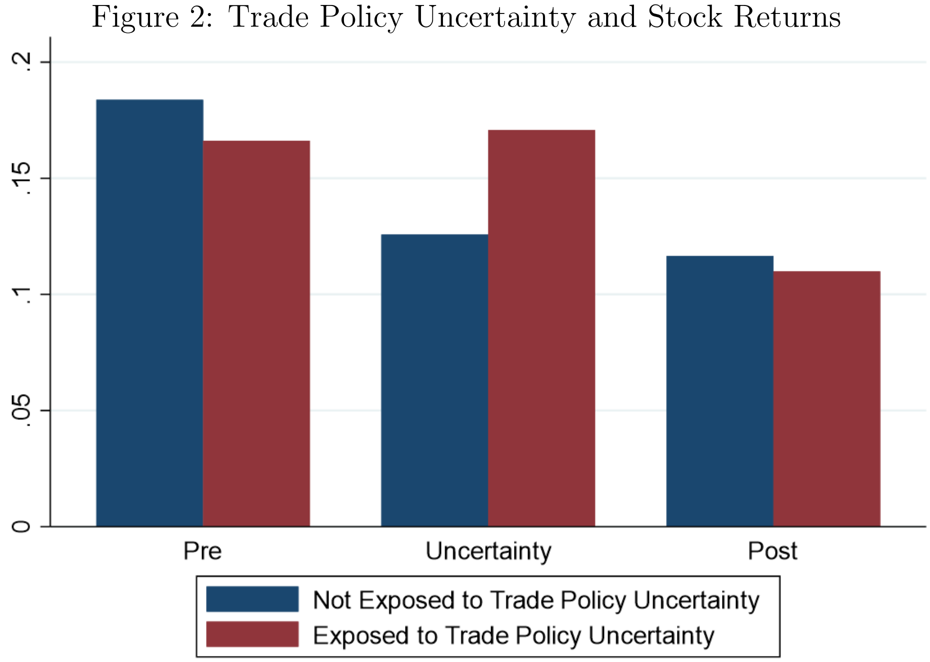
6 Tables and Figures

Figure 1: Trade Policy Uncertainty in the News



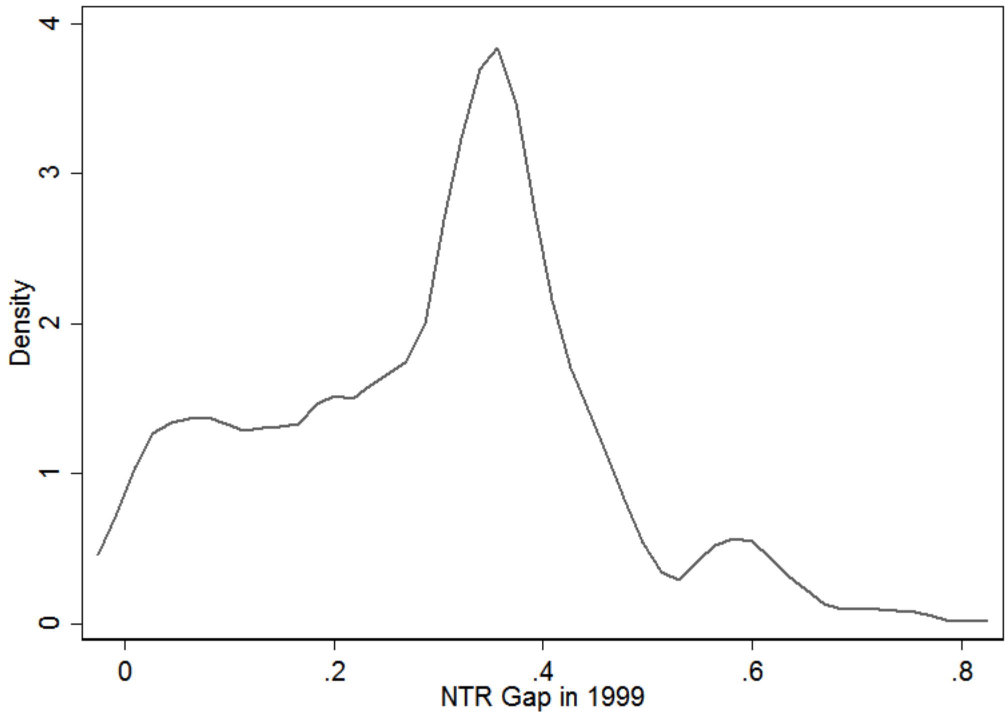
Notes: Time-series plots of the policy uncertainty indices from https://www.policyuncertainty.com/. The blue line represents overall economic policy uncertainy. The red line represents specifically trade policy uncertainty. These two indices differ on the term sets used to identify newspaper articles. For example, trade policy uncertainty looks specifically for terms like "import tariffs" and "import duty", while economic policy uncertainty looks for broader terms like "congress" and "white house".

Figure 2: Trade Policy Uncertainty and Stock Returns



Notes: Each bar represents a weighted average of returns on the value-weighted industry portfolios, where the weights are proportional to each industry's market capitalization in 1989. An industry is defined as exposed to trade policy uncertainty if its NTR gap in 1990 is above 0.3, approximately the median in our sample. The Pre period includes 1980-1989, the Uncertainty period 1990-2001, and the Post period 2002-2007. Each bar represents a weighted average of returns on the value-weighted industry portfolios, where the weights are proportional to each industry's market capitalization in 1989.

Figure 3: Distribution of NTR gaps



Notes: Unweighted Kernel density plot of the NTR gaps in 1999.

Table 1: Summary Statistics

Variable	Low-Gap	High-Gap	t-Statistic
NTR Gap in 1999	0.18	0.42	-62.39
Market Capitalization ($M)	$ 2,294.99	$ 2,268.93	0.05
EV/EBITDA	1.71	3.43	-0.06
Price / Earnings per Share	12.22	19.05	-1.07
Price / Book	3.36	7.74	-7.30
Return on Equity	-0.04	-0.10	1.98
Return on Invested Capital	0.20	0.45	-0.47
Dividend Yield	0.01	0.01	2.80
Total Sales	$ 2,254.54	$ 830.28	5.42
Current Ratio	2.25	3.93	-8.78
Debt / Equity	0.73	0.29	8.80

Notes: This table contains summary statistics on high and low gap firms in 1999, the year before China was granted conditional PNTR. A firm is classified as low-gap if it has a below median NTR gap in 1999. Each entry represents the un-weighted average within each group. The last column contains the t-Statistic from a difference of means test across groups.

Table 2: PNTR and Expected Returns

	(1)	(2)	(3)	(4)	(5)
$NTRGap_{i,y-1} \times PrePNTR_t$	0.016***	0.017***	0.020***	0.025***	0.021***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
$NTRGap_{i,y-1}$	-0.007	-0.010***	-0.011***	-0.009*	-0.009*
	(0.005)	(0.003)	(0.003)	(0.005)	(0.005)
$PrePNTR_t$	-0.004***	-0.004***	-0.005***	-0.006***	
	(0.001)	(0.001)	(0.001)	(0.001)	
Observations	41,241	41,241	40,689	40,689	40,689
R-squared	0.001	0.001	0.002	0.003	0.091
Policy Controls	No	Yes	Yes	Yes	Yes
Matched Control Sample	No	No	Yes	Yes	Yes
Firm Controls	No	No	No	Yes	Yes
Month Fixed Effects	No	No	No	No	Yes
Industry Fixed Effects	No	No	No	No	No

Notes: This table contains selected estimates from the following regression, run at the industry(i)/month(t) level using data from 1980-2007:
$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 PrePNTR_t + \beta_3 NTRGap_{i,y-1} + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is between 1990 and 2001. The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, Return on Equity, EV/EBITDA, Debt/Equity, Current Ratio and lagged Market Capitalization. Some specifications include time δ_t fixed effects. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 3: Robustness I: Standard Errors and Weights

	(1)	(2)	(3)	(4)
$NTRGap_{i,y-1} \times PrePNTR_t$	0.021**	0.024**	0.021***	0.019***
	(0.009)	(0.010)	(0.006)	(0.005)
$NTRGap_{i,y-1}$	-0.009		-0.006	-0.010**
	(0.007)		(0.005)	(0.004)
Observations	40,689	40,689	41,144	39,776
R-squared	0.091	0.095	0.046	0.13
Firm Controls	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	No	Yes	No	No

Notes: This table contains selected estimates from versions of the following regression, run at the industry(i)/year(t) level using data from 1980-2007:
$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 NTRGap_{i,y-1} + \delta_t + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is between 1990 and 2001. The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, Return on Equity, EV/EBITDA, Debt/Equity, Current Ratio, and Lagged Market Capitalization. In Columns 1 and 2, we double cluster standard errors at the industry/month level. In Columns 3 an 4, we cluster standard errors at the industry level. In Columns 1 and 2, observations are weighted by industry market capitalization in January, 1989. In Column 3, they are weighted by 1-year lagged market capitalization, and in Column 4 they are weighted by market capitalization in January, 1979. Standard errors are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 4: Additional Robustness

	(1)	(2)	(3)	(4)	(5)
$NTRGap_{i,t-1} \times PrePNTR_t$	0.015***	0.020***	0.019***	0.021***	0.021***
	(0.006)	(0.005)	(0.005)	(0.004)	(0.006)
$NTRGap_{i,t-1}$	(0.001)	(0.007)	(0.006)	-0.009***	(0.010)
	(0.004)	(0.005)	(0.005)	(0.002)	(0.007)
Observations	34,259	39,405	35,553	40,689	35,793
R-squared	0.098	0.108	0.102	0.061	0.083
Policy Controls	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Specification	CRSP	No Elec	Beta	MF Resids.	Con50

Notes: This table contains selected estimates from versions of the following regression, run at the industry(i)/year(t) level using data from 1980-2007:
$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 NTRGap_{i,y-1} + \delta_t + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is before 2001. The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, Return on Equity, EV/EBITDA, Debt/Equity, Current Ratio, and Lagged Market Capitalization. In column 1, we use the industry definition in CRSP, instead of the industry definition in Compustat. In column 2, we remove industry famlies 409, 410, 411 and 412, which map to the 4-digit NAICS codes for Computer and Peripheral Equipment Manufacturing. In column 3, we compute industry betas using the previous 5 years of daily returns. In column 4, we use industry-level 5-factor residuals, computed with daily data from 1980-2007 as the left-hand-side variable. In column 5, we restrict to Pierce and Schott's constant manufacturing sample. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 5: Exogeneity

	NTR Gap 1990		NTR Gap 1999		IV (1990)		Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$NTRGap_{i,1990} \times PrePNTR_t$	0.015**	0.020***						
	(0.006)	(0.006)						
$NTRGap_{i,1990}$	-0.006	-0.008*					0.000754	-0.0197
	(0.005)	(0.005)					(0.003)	(0.025)
$NTRGap_{i,1999} \times PrePNTR_t$			0.017***	0.022***				
			(0.005)	(0.005)				
$NTRGap_{i,1999}$			-0.005	-0.004				
			(0.004)	(0.004)				
IV: $NTRGap_{i,y-1} \times PrePNTR_t$					0.017***	0.027***		
					(0.004)	(0.008)		
Placebo: $NTRGap_{i,y-1} \times PrePNTR_t$							0.00339	0.0032
							(0.005)	(0.005)
Observations	41,241	40,689	41,241	40,689	41,241	40,689	41,241	40,689
R-squared	0.089	0.091	0.089	0.092	0.089	0.093	0.09	0.095
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table contains selected estimates from the following regression, using data from 1980-2007:
$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 NTRGap_{i,y-1} + \delta_t + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is between 1990 and 2001. The regressions also include the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, return on Equity, EV/EBITDA, Debt/Equity, Current Ratio, and Lagged Market Capitalization. Columns 5 and 6 show the second stage of a 2SLS regression where we instrument $PrePNTR_t \times NTRGap_{it-1}$ with $PrePNTR_t \times NNTR_i$. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 6: International Stock Returns

	Japan	Korea	UK	France	Australia
$NTRGap_{i,t-1} \times PrePNTR_t$	0.003	0.008	0.008	0.003	0.007
	(0.007)	(0.015)	(0.010)	(0.009)	(0.011)
$NTRGap_{i,t-1}$	-0.003	-0.02	-0.011	-0.004	-0.019*
	(0.006)	(0.017)	(0.012)	(0.007)	(0.011)
Observations	18,372	3993	13129	7278	5173
R-squared	0.479	0.601	0.26	0.565	0.317
Policy Controls	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	No
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: This table contains selected estimates from the following regression, using data from 1980-2007:
$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 NTRGap_{i,y-1} + \delta_t + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is between 1990 and 2001. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 7: Portfolio analysis

	Low Gap	2	High Gap	TPU
$PrePNTR_t$	-0.004	-0.001	0.006**	0.010**
	(0.003)	(0.002)	(0.002)	(0.004)
Market	0.749***	0.952***	1.053***	0.305***
	(0.051)	(0.028)	(0.031)	(0.066)
Size	-0.074	-0.151***	0.129**	0.203**
	(0.064)	(0.041)	(0.055)	(0.097)
Value	0.420***	-0.272***	-0.343***	-0.763***
	(0.097)	(0.064)	(0.069)	(0.144)
Profitability	0.197**	0.075	-0.367***	-0.564***
	(0.082)	(0.054)	(0.061)	(0.127)
Investment	0.022	0.408***	-0.246***	-0.268
	(0.126)	(0.075)	(0.093)	(0.190)
Observations	336	336	336	336
R-Squared	0.502	0.854	0.905	0.574

Notes: This table contains selected estimates from the following regression, using data from 1980-2007:
$R_{pt} = \theta PrePNTR_t + \boldsymbol{F}'_t\lambda + \alpha + \epsilon_{pt}$
Where R_{pt} is the return on portfolio p in month t and $PrePNTR_t$ is a dummy equal to one if the year is before 2001. $\boldsymbol{F}'_t$ is a vector containing the 5 Fama-French factors: Market, Size, Value, Profitability and Investment. Robust standard errors are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 8: TPU and Realized Volatility

	(1)	(2)	(3)	(4)	(5)	(6)
$NTRGap_{i,y-1} \times PrePNTR_t$	0.009***	0.009***	0.009***	0.008***	0.005*	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$NTRGap_{i,y-1}$	0.005**	0.005**	0.005**	0.002	0.003	
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
$PrePNTR_t$	-0.001***	-0.001***	-0.001***	-0.001***		
	0.000	0.000	0.000	0.000		
Observations	41,241	41,241	40,689	40,689	40,689	40,689
R-squared	0.037	0.037	0.037	0.06	0.424	0.48
Policy Controls	No	Yes	Yes	Yes	Yes	Yes
Matched Control Sample	No	No	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	Yes	Yes	Yes
Month Fixed Effects	No	No	No	No	Yes	Yes
Industry Fixed Effects	No	No	No	No	No	Yes

Notes: This table contains selected estimates from versions of the following regression, run at the industry(i)/month(t) level using data from 1980-2007:
$Volatility_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 NTRGap_{i,y-1} + \delta_t + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is before 2001. Volatility is the sum of squared daily returns in month t. The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, Return on Equity, EV/EBITDA, Debt/Equity, Current Ratio, and Lagged Market Capitalization. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 9: PNTR and Realized Returns

	(1)	(2)	(3)	(4)
	PNTR Dates			Earnings ann.
	10/10/2000	12/11/2001	1/2/2002	
NTR gap 1999	-0.151**	-0.00462	0.04	
	(0.06)	(0.03)	(0.02)	
NTR gap 1999 x PrePNTR				0.0148**
				(0.01)
Observations	225	224	221	125,131
R-squared	0.11	0	0.022	0.011
Event Window	t-1 to t+3	t-1 to t+3	t-1 to t+3	t-5 to t+1

Notes: This table contains selected estimates from the following regression:
$R_{i,(t-n,t+m)} = \theta NTRGap_i + \alpha + \epsilon_{it}$
Where $R_{i,(t-n,t+m)}$ is the cumulative return for industry i from $t-n$ to $t+m$, where t is the event-date of interest. 10/10/2000 is the day President Clinton signed the law which gave China PNTR conditional on joining the WTO. 12/11/2001 is the day China joined the WTO. 1/2/2002 is the day PNTR went into effect. Regression 4 is run at the firm, rather than industry level, using data from 1990-2007. Observations are weighed by each firm's one year lagged market capitalization. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 10: Congressional votes

	Dependent variable: Returns in from t-1 to t+3						
MFN-Status Voting Date	10/18/1990	7/10/1991	7/21/1992	6/8/1993	8/9/1994	7/20/1995	6/27/1996
Vote Result:	House/Pass	House/Pass	House/Pass	House/Reject	House/Reject	House/Table	House/Reject
Lagged NTR Gap	0.0671***	-0.000897	-0.0156	-0.0322*	0.0401**	-0.0610**	-0.0298**
	(0.0217)	(0.0107)	(0.0100)	(0.0184)	(0.0198)	(0.0255)	(0.0118)
Observations	407	406	402	398	389	383	382
R-squared	0.054	0	0.01	0.025	0.055	0.092	0.047
MFN-Status Voting Date	6/24/1997	7/16/1997	7/22/1998	7/20/1999	7/27/1999	7/18/2000	
Vote Result:	House/Reject	Senate/Reject	House/Reject	House/Reject	Senate/Reject	House/Reject	
Lagged NTR Gap	-0.00937	0.0273	-0.0325	-0.0638***	-0.0246*	-0.00494	
	(0.0206)	(0.0305)	(0.0305)	(0.0208)	(0.0142)	(0.0326)	
Observations	379	380	372	371	371	358	
R-squared	0.005	0.018	0.024	0.139	0.035	0.001	

Notes: This table contains selected estimates from the following regression:
$R_{i,(t-1,t+3)} = \theta NTRGap_{it-1} + \alpha + \epsilon_{it}$
Where $R_{i,(t-1,t+3)}$ is the cumulative return for industry i from $t-1$ to $t+3$, where t is the event-date of interest. Event dates are all days where US Congress voted to revoke China's MFN status. Observations are weighed by each industry's one year lagged market capitalization. Robust standard errors are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 11: Robustness - Excluding event days

	All	Ex. PNTR	Ex. Votes	Ex. Earn	Ex. All
$NTRGap_{i,t-1} \times PrePNTR_t$	0.015**	0.016***	0.017***	0.012*	0.014***
	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)
$NTRGap_{i,t-1}$	-0.010*	-0.011*	-0.011*	-0.009*	-0.010*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
Observations	40,689	40,689	40,689	40,689	40,689
R-squared	0.379	0.38	0.383	0.365	0.371
Policy Controls	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: This table contains selected estimates from the following regression, run at the industry(i)/month(t) level using data from 1980-2007:
$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 NTRGap_{i,y-1} + \delta_t + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is between 1990 and 2001. Monthly industry returns are computed by adding up log daily returns. Column 1 replicates the baseline results using the aggregated log returns. In Column 2, we exclude the t-1 to t+3 window around the following dates when computing monthly returns: 10/10/2000, 12/11/2001 and 1/2/2002. In column 3, we also exclude the t-1 to t+3 window around all the dates where US Congress voted on revoking China's MFN status. In column 4 we also exclude the t-5 to t+1 window around each firm's annual and quarterly earnings announcement days. Column 5 excludes all the days excluded in columns 2 to 4.
The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, return on Equity, EV/EBITDA, Debt/Equity, Current Ratio and Lagged Market Capitalization. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 12: Different sample periods

	1980-2007	1990-2007	1980-2001	1980-2017
$NTRGap_{i,t-1} \times PrePNTR_t$	0.0215***	0.0222***	0.0181***	0.0114***
	(0.005)	(0.006)	(0.005)	(0.004)
$NTRGap_{i,t-1}$	-0.00910*	-0.00801	-0.00736	0.00373
	(0.005)	(0.007)	(0.007)	(0.004)
Observations	40,689	25,965	32,267	54,164
R-squared	0.091	0.119	0.093	0.075
Policy Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes

Notes: This table contains selected estimates from the following regression, run at the industry(i)/year(t) level using data between 1980 and 2017:
$R_{it} = \alpha + \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta_2 NTRGap_{i,y-1} + \delta_t + \boldsymbol{X}'_{it-1}\lambda + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one for the years 1990-2001. The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, return on Equity, EV/EBITDA, Debt/Equity, Current Ratio and Lagged Market Capitalization. Some specifications also include industry δ_i and time δ_t fixed effects. Each observation is weighed by industry i's market capitalization in 1989. Robust standard errors, clustered at the industry level, are in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 13: The China Shock and Stock Returns

	(1)	(2)	(3)
$NTRGap_{i,y-1} \times PrePNTR_t$	0.0243***	0.0253**	0.0252**
	(0.009)	(0.011)	(0.010)
$China_i \times PrePNTR_t$		0.0038	0.0027
		(0.011)	(0.012)
$NTRGap_{i,y-1} \times PrePNTR_t \times China_i$		-0.0126	-0.0134
		(0.033)	(0.033)
$\Delta IP_{i,t} \times PostPNTR_t$			-0.0001
			(0.000)
Observations	21,405	21,297	21,297
R-squared	0.108	0.108	0.108
Policy Controls	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Ind/Month Fixed Effects	Yes	Yes	Yes

Notes: Notes: This table contains selected estimates from the following regression, run at the industry(i)/month(t) level using data from 1991-2007:
$R_{it} = \theta PrePNTR_t \times China_i \times NTRGap_{iy-1} + \boldsymbol{X}'_{it}\lambda + \delta_i + \delta_t + \alpha + \epsilon_{it}$
where $PrePNTR_t$ is a dummy variable equal to one if the year is between 1991 and 2001. $China_i$ is a dummy variable equal to 1 if an industry had above median import exposure from China between 2002 and 2007. $\Delta IP_{i,t}$ is the change in the imports from China in industry i between 1991 and each year t, divided by the initial industry absorption in 1991. We average this measure across each SIC 4 industry within each Pierce and Schott industry. Column 1 replicates the baseline results in the matched sample. Column 2 includes the measure of future Chinese import penetration. Column 3 adds the interaction term between realized and the post period.
The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, return on Equity, EV/EBITDA, Debt/Equity, Current Ratio and Lagged Market Capitalization. The regression includes industry δ_i and time δ_t fixed effects. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 14: IO Linkages and Stock Returns

	China		US		
	(1)	(2)	(3)	(4)	(5)
China Upstream Exposure	0.040***				
	(0.011)				
US Downstream Exposure		-0.001			
		(0.027)			
US Upstream Exposure			0.04		
			(0.049)		
US Downstream Exposure (L)				0.040**	
				(0.016)	
US Upstream Exposure (L)					0.066***
					(0.020)
Observations	34,329	34,329	34,329	34,329	34,329
R-squared	0.087	0.086	0.086	0.087	0.087
Policy Controls	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Ind/Month Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: This table contains selected estimates from the following regression, run at the industry(i)/year(t) level using data from 1980-2007:
$R_{it} = \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta IOLinkages_{iy} + \boldsymbol{X}'_{it}\lambda + \delta_i + \delta_t + \alpha + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is less than 2001. The following terms are part of $IOLinkages_{it}$: $ChinaUpstreamExposure$ is a a weighted average of NTR gaps for upstream Chinese industries, where the weights are proportional to the share of input expenditures in those industries. US $Upstream/Downstream$ Exposure are also weighted averages of NTR gaps for upstream/downstream US industries, where the weights are proportional to the expenditures/sales in those industries. The (L) denotes Leontief, and those exposures are calculated from the inverse of the Leontief matrix for all upstream/downstream linkages.
The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, return on Equity, EV/EBITDA, Debt/Equity, Current Ratio, Dividend Yield and Market Capitalization. All these controls are lagged one year to prevent a look-ahead bias. The regression includes industry δ_i and time δ_t fixed effects. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 15: IO Linkages and Stock Returns (with NTR Gap)

	China		US		
	(1)	(2)	(3)	(4)	(5)
NTR Gap	0.012	0.026***	0.024***	0.019**	0.016*
	(0.009)	(0.007)	(0.009)	(0.007)	(0.009)
China Upstream Exposure	0.026				
	(0.017)				
US Downstream Exposure		-0.024			
		(0.024)			
US Upstream Exposure			-0.013		
			(0.045)		
US Downstream Exposure (L)				0.019	
				(0.015)	
US Upstream Exposure (L)					0.032
					(0.027)
Observations	34,329	34,329	34,329	34,329	34,329
R-squared	0.087	0.087	0.087	0.087	0.087
Policy Controls	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Ind/Month Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: This table contains selected estimates from the following regression, run at the industry(i)/year(t) level using data from 1980-2007:

$R_{it} = \beta_1 PrePNTR_t \times NTRGap_{i,y-1} + \beta IOLinkages_{it} + \boldsymbol{X}'_{it}\lambda + \delta_i + \delta_t + \alpha + \epsilon_{it}$

where $PrePNTR_t$ is a dummy equal to one if the year is less than 2001. The following terms are part of $IOLinkages_{it}$: $ChinaUpstreamExposure$ is a a weighted average of NTR gaps for upstream Chinese industries, where the weights are proportional to the share of input expenditures in those industries. US $Upstream/Downstream$ Exposure are also weighted averages of NTR gaps for upstream/downstream US industries, where the weights are proportional to the expenditures/sales in those industries. The (L) denotes Leontief, and those exposures are calculated from the inverse of the Leontief matrix for all upstream/downstream linkages.

The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, return on Equity, EV/EBITDA, Debt/Equity, Current Ratio, Dividend Yield and Market Capitalization. All these controls are lagged one year to prevent a look-ahead bias. The regression includes industry δ_i and time δ_t fixed effects. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 16: Industry Concentration and Stock Returns

	Matched	Top 8	HHI 50
$NTRGap_{i,y-1} \times PrePNTR_t$	0.023***	0.038***	0.028***
	(0.007)	(0.010)	(0.007)
Concentration Measure		0.001	0.034
		(0.006)	(0.050)
Concentration Interaction		-0.036**	-0.128**
		(0.018)	(0.062)
Observations	40,413	40,413	40,413
R-squared	0.399	0.4	0.4
Policy Controls	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Ind/Month Fixed Effects	Yes	Yes	Yes

Notes: This table contains selected estimates from the following regression, run at the industry(i)/year(t) level using data from 1980-2007:
$R_{it} = \theta PrePNTR_t \times NTRGap_{iy-1} + \beta Concentration_{i,t} \times NTRGap_{iy-1} \times PrePNTR_t + \boldsymbol{X}'_{it}\lambda + \delta_i + \delta_t + \alpha + \epsilon_{it}$
where $PrePNTR_t$ is a dummy equal to one if the year is less than 2001, and $LaggedNTRGap_{it}$ is the NTR gap for industry i in year $t-1$. Concentration measures are the share of sales going to the 8 largest firms in an SIC 4 industry, and the HHI of sales to the top 50 firms in an SIC 4 industry. The regression also includes the following controls in $\boldsymbol{X}'_{it}$: Price/Earnings, Price/Book, Return on Invested Capital, return on Equity, EV/EBITDA, Debt/Equity, Current Ratio, Dividend Yield and Market Capitalization. All these controls are lagged one year to prevent a look-ahead bias. The regression includes industry δ_i and time δ_t fixed effects. Each observation is weighed by industry i's market capitalization in January, 1989. Robust standard errors, clustered at the industry level, are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

7 Appendix: Construction of valuation variables

The price-to-earnings ratio (P/E ratio) is a valuation multiple, typically measured at the firm level as current share price relative to earnings per-share. To aggregate this to the industry level, we take the value-weighted average at the industry/fiscal-year level. We construct this using annual Compustat data.

The Price-to-book ratio is also a valuation multiple, typically measured at the firm level as the ratio of price to book value per-share. This is also a value-weighted average at the industry/fiscal-year level of market capitalization, divided by total book value of equity. We exclude firms with negative book values from this calculation.

The EV/EBITDA ratio is computed with annual Compustat data a value-weighted average at the industry/fiscal-year level of enterprise value divided by EBITDA.

The Return on invested capital (ROI), is a measure of the profitability, or the return earned on capital invested in operating assets. We compute this with annual Compustat data as a value-weighted average at the industry/fiscal-year level of net income divided by the total capital, measured as the book value of equity plus the book value of debt, minus cash and cash equivalents.

The Return on equity (ROE) measures profitability. We compute this with annual Compustat data as the value-weighted average at the industry/fiscal-year level of net income, divided by market capitalization.

The debt-to-equity ratio is a leverage metric which compares the company's debt to its stockholder equity. It is calculated as a total liabilities over stockholders' equity and indicates what proportion of shareholders' equity and debt a company is using to finance its assets. We compute this with annual Compustat data as a value-weighted average at the industry/fiscal-year level of the ratio of long-term debt to market capitalization.

The Current ratio is a liquidity ratio that measures a company's ability to pay short-term and long-term obligations. We compute this with annual Compustat data as a value-weighted average at the industry/fiscal-year level of current assets divided by current liabilities.

Aggregating controls to the industry level. In our baseline specification, our unit of observation is industry-month. As mentioned above, we computed the ratio for each firm/fiscal-year, and then taken a value-weighted average (weights proportional to the previous year's market capitalization) at the industry level. Alternatively, we could have constructed the controls by summing each part of each ratio within each industry/year, and then computing the ratio. In unreported results, we tried this and found it made little difference. We have also tried running the regressions at the firm level to avoid the issue of aggregating controls. Although the results go through, we do not think this exercise is well specified, as the NTR gaps are defined at the product level, rather than the firm level. Pierce and Schott (2016) aggregate the products produced by each industry to form the NTR gaps we use in the paper at the industry level, so we have no variation in NTR gaps among firms within a given industry.