Trade War and Peace: U.S.-China Trade and Tariff Risk from 2015–2050[*]

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Abstract

We model trade policy as a Markov process. Using a dynamic exporting model, we estimate how expectations about U.S. tariffs on China have changed around the U.S.-China trade war. We find (i) no increase in the likelihood of a trade war before 2018; (ii) the trade war was initially expected to end quickly but its expected duration grew substantially after 2020; and (iii) the trade war reduced the likelihood that China would face Non-Normal Trade Relations tariffs in the future. Our findings imply the expected mean future U.S. tariff on China rose more under President Biden than under President Trump.

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1 Introduction

Donald Trump's election as U.S. President in 2016 raised questions about the future of U.S. trade-policy. Would he follow through on his campaign pledge to raise tariffs on China? If so, by how much? Would he shift China to the Non-Normal Trade Relations (NNTR) tariff schedule or choose something else? How long would these tariffs last? Would he reverse course quickly, as with President Nixon's import surcharge?[1] Or, would the tariffs remain in place for decades, as with President Truman's embargo on China? Once President Trump raised tariffs on China in 2018, the question of how long these tariffs would last was further complicated by the upcoming 2020 election and the subsequent Presidency of Joseph Biden. It remained an issue in 2024; in May, Biden renewed the tariffs and increased tariffs by 25 percent on almost 400 goods.

We answer these questions using disaggregated U.S. import data and a dynamic trade model with two key features: heterogeneous firms that make forward-looking export participation decisions, and tariff risk that varies across products and time. In the model, Chinese firms make investments in U.S. market access subject to idiosyncratic shocks, industry-specific variation in tariffs across policy regimes, and a common time-varying probability of switching between regimes. We estimate these probabilities using indirect inference.

We have three main findings. First, despite Trump's campaign rhetoric, there was no increase in the probability that U.S. tariffs on China would rise before the trade war began. The key data moment that identifies this probability is the *trade-war gap elasticity*: the elasticity of U.S. imports from China to the gap between the trade-war tariffs and Normal Trade Relations (NTR) tariffs. This elasticity was stable in the three years before the Trump tariffs were put in place, suggesting there was no anticipatory response to these tariffs.[2]

Second, during the first two years of the trade war, the probability that tariffs would return to NTR levels was very high—more than 70 percent. However, expectations about the end of the trade war began to shift when President Biden continued the trade war. By 2023, the probability of the trade war ending had fallen to 21 percent. The dynamics of this transition

[1] In 1971, Nixon imposed a 10-percent import surcharge but removed it four months later.

[2] Our findings suggest that there was no anticipation of a tariff increase, either correlated or uncorrelated with the tariffs imposed in 2018. See sections 2.1 and 4.2 for further discussion.

probability are also identified by the behavior of the trade-war gap elasticity, which fell in 2019 after the Trump tariffs were levied, and then stalled before beginning to fall again several years later.

Third, the trade war fundamentally shifted the nature of the uncertainty about U.S. trade policy towards China. Prior to the trade war, there existed a possibility of reverting to NNTR tariffs. This possibility still existed after China was granted Permanent NTR in 2001 and did not change with Trump's election, but it fell when the trade war began and a different tariff schedule was applied to China.[3] This shift is identified by the behavior of the *NNTR-gap elasticity:* the elasticity of U.S. imports from China to the gap between NNTR and NTR tariffs. Like the trade-war gap elasticity, the NNTR-gap elasticity was stable before the trade war, but began to rise after the trade war began. Because the trade-war gap and NNTR gap are orthogonal, this growth indicates a decline in the likelihood of reverting to NNTR. For perspective, the growth in the NNTR-gap elasticity during the trade-war period is about as large as the growth around China's 2001 WTO accession, which has been cited by Pierce and Schott (2016), Handley and Limão (2017), and others as evidence that this event eliminated policy uncertainty.

Our analysis yields a time-varying forecast of the path of trade and trade policy. We use this forecast to quantify the contributions of the Trump and Biden administrations to those paths. We find, even though Trump raised tariffs and Biden only maintained those tariffs, Trump lowered the discounted expected mean tariff by 5.3 percentage points while Biden raised it by 4.6 percentage points. The lower discounted expected mean tariff under Trump is a result of the reduction in the likelihood of reverting to the NNTR tariff schedule and the high initial probability of a short trade war. The shift in expectations to a long trade war under Biden increases expected future tariffs.

Our analysis also highlights parallels between the trade reform in 1980 and the increase in tariffs in 2018. The trade responses before and after these two reforms are similar in magnitude. Prior to both reforms, there was no material change in trade that was correlated with the change in tariffs. In the first two years following both reforms, trade changed suddenly by about three times the change in tariffs, and then stalled for two years before beginning to

[3]Similarly, Alessandria et al. (2024b) show the risk of losing NTR access did not materially change with the elections of Clinton, George W. Bush, or Obama. They argue that Reagan's 1981 election fundamentally changed the outlook on U.S. trade policy on China, raising the probability of losing NTR access.

change further. Statistically speaking, we cannot reject the hypothesis that these two episodes have the same trade-elasticity dynamics. This suggests that similar expectational dynamics were at work in both cases.

We contribute to the literature on the U.S.-China trade war surveyed in Fajgelbaum and Khandelwal (2022) and Caliendo and Parro (2023). Our novel approach builds on Alessandria et al. (2024b), henceforth AKKRS, by considering richer stochastic processes for trade policy and using them to forecast future trade dynamics. More broadly, our study relates to the trade-policy uncertainty literature, summarized by Handley and Limão (2022), and in particular, papers that use dynamic trade models to study the dynamics of trade policy.[4]

2 Reduced-form empirical analysis

We use U.S. import data from the U.S. Census Bureau (July 2014–June 2024, HS-6 level) and Eurostat import data for the 27 EU countries. We aggregate the EU countries into a single importer. For the United States and the European Union, China is treated as a separate exporter, while all other exporters are aggregated into a second group.[5] Imports of country j of good g from country i are denoted v_{ijgt}. We use a balanced sample—goods imported from China into the United States every year—and exclude goods that were affected by trade policies that were not China-specific. We use annual data to reduce concerns about stockpiling in advance of possible tariff changes.[6] To align with the timing of the trade war, we define a year as starting in July and ending in June.[7]

Figure 1(a) plots the 25th, 50th, and 75th percentiles of the applied tariff distribution. The median tariff rises from about 3 percent in January, 2018 to 10 percent by October, 2018. By August, 2019, it is about 25 percent. The lower and upper quartiles increased by similar amounts.

For each good, we define the NTR tariff rate as the average applied tariff on China during 2015–2017. We construct two measures of good-specific tariff risk that represent the addi-

[4] See Ruhl (2011), Alessandria et al. (2017), Handley and Limão (2017), Steinberg (2019), Alessandria et al. (2024a), and Hoang and Mix (2023).

[5] We use CIF import values, as Eurostat does not report FOB values.

[6] Alessandria et al. (2024a) find evidence of stockpiling in the 1990s prior to the July NTR renewal decision. Khan and Khederlarian (2021) show destocking occurred in advance of NAFTA tariff cuts.

[7] For example, 2019 covers 7/2018-6/2019. Our results are robust to using normal calendar years.

tional tariffs that Chinese imports face outside of the NTR regime.[8] The *trade-war gap* is the difference between the average applied tariff on China in 2020–2023 and the NTR tariff rate. The *NNTR gap* is the difference between the NNTR tariff rate, set by the Smoot-Hawley Tariff Act in 1930, and the NTR tariff rate. Formally,

$$X_g^j = \tau_g^j - \tau_g^{NTR}, \quad j = \{NNTR, TW\}. \tag{1}$$

Until the trade war, the NNTR gap represented the most relevant risk given the history of U.S. trade policy. Since the end of World War II, more than 20 countries were moved from NTR to NNTR tariffs or an outright embargo (see appendix for a list of countries). For example, in 2022, Russia and Belarus were shifted to NNTR tariffs following Russia's invasion of Ukraine. Numerous proposals have sought to remove China's permanent NTR status (e.g., 109th Congress, 2005; 118th Congress, 2023).

Figure 1(b) plots the trade-war gap and NNTR-gap distributions. There are two key observations. First, the NNTR-gap distribution has a fatter tail and higher average, indicating that moving to NNTR status would be a bigger policy change than beginning the trade war. This difference plays an important role in the evolution of expected future tariffs since the trade war began. Second, the two gaps are approximately orthogonal, with a correlation of only –0.08. On average, goods that are exposed to one risk are not exposed to the other. The orthogonality allows us to separately identify the probabilities of these risks from the trade data.

2.1 Elasticities of trade to the trade-war gap and the NNTR gap

We extend the approach in AKKRS by measuring the dynamics of U.S. imports with respect to both the NNTR gap and the trade war gap,

$$\begin{aligned} \log v_{ijgt} = \sum_{t'=2015}^{2024} \left(\beta_t^{NTR} X_g^{NTR} + \beta_t^{TW} X_g^{TW}\right) \mathbb{1}_{\{i=\text{China} \,\wedge\, j=\text{US} \,\wedge\, t=t'\}} \\ + \delta_{igt} + \delta_{jgt} + \delta_{ijg} + \delta_{ijht} + u_{ijgt}, \end{aligned} \tag{2}$$

where δ_{ijg}, δ_{igt}, and δ_{jgt} are exporter-importer-good, exporter-good-time, and importer-

[8] As discussed in the appendix, country-specific tariff risk will be absorbed in country-year fixed effects.

good-time fixed effects, and δ_{ijht} is an exporter-importer-time fixed effect at the HS-Section level. As is common in event studies, we reference δ_{ijg} to the year before the trade war, 2018. The coefficient β_t^{TW} measures the elasticity of U.S. imports from China to the trade-war gap, relative to all other countries, at time t, relative to 2018. Similarly, β_t^{NTR} is the NNTR-gap elasticity relative to the same benchmarks. The fixed effects control for good-level demand and supply shocks, time-invariant bilateral trade barriers, and aggregate shocks to exporting countries.

Figure 1(c) plots the estimates of (2). The trade-war gap elasticity, β_t^{TW}, was statistically indistinguishable from zero throughout 2015–2017. Our interpretation of this finding is that the likelihood of a trade war did not change during this period. An alternative possibility is that a tariff increase was expected, but it was uncorrelated with the increase that occurred in 2018. The expected-but-uncorrelated tariff would not affect products with high trade-war gaps differently than products with low gaps, so it would show up as a change in the China-US-section-time fixed effects, $\delta_{\text{China,US},h,t}$, rather than the trade-war gap elasticity. As Figure 1(e) shows, however, these fixed effects were stable throughout the pre-war period, which casts doubt on this possibility. We discuss the (counterfactual) implications of this alternative possibility in section 4.1.

During 2019–2020, the trade-war elasticity fell to about –2.5, likely reflecting the intensive-margin response to the increase in tariffs. From 2021 onward, it fell gradually by 1.8 points. There are two possible explanations for this growing substitution: (i) trade was gradually adjusting to the increase in tariffs, or (ii) the likelihood these tariffs would be reversed was falling. This is because the trade-war gap has two meanings: it represents the size of the past tariff increase at the onset of the trade war and it represents the potential future tariff reduction if the trade war ends. A structural model is needed to disentangle these two channels.

The NNTR-gap elasticity was also statistically insignificant during 2015–2017, indicating the probability of reverting to NNTR was also stable during this period. In 2019, it began to rise, and by 2024 was 0.6 points higher than before the trade war. This is notable because the NNTR gap is orthogonal to the trade-war gap; the trade war did not, on average, increase tariffs on goods with high NNTR gaps relative to goods with low NNTR gaps. Nevertheless, U.S. imports of Chinese goods with high NNTR gaps grew relative to imports of low-gap goods.

Our interpretation of this result is that the trade war fundamentally changed the nature of U.S.-China trade-policy uncertainty. Prior to the trade war, the uncertainty was about moving between the NNTR and NTR regimes. After the trade war began, the likelihood of reverting to NNTR fell and the uncertainty was now largely about moving between trade war and trade peace.

3 Structural model

Our empirical findings are inputs to the structural model we use to measure the dynamics of expectations about U.S. trade policy towards China and distinguish the trade effects of these dynamics from the gradual adjustment to the trade-war tariffs. The dynamic exporter model builds on Alessandria et al. (2021) and AKKRS by introducing a richer stochastic process for trade policy featuring multidimensional tariff risk.

3.1 Environment

There are G goods that correspond to the HS-6 goods in the data. Within each good g, there is a fixed mass of Chinese firms that produce differentiated varieties and face idiosyncratic shocks to productivity, trade costs, and survival. Accessing the U.S. market requires firms to pay a fixed cost that depends on their current export participation. There are three trade-policy regimes: NTR, or *trade peace* (P), NNTR (N), and trade war (W). The probability of switching between regimes varies over time.

Trade policy. The good-level tariff, $\tau_g(s)$, depends on the current tariff regime, $s \in \{$P, N, W$\}$. The regime follows a time-varying Markov process with transition matrix

$$\Omega_t = \begin{bmatrix} \omega_t(P,P) & \omega_t(P,N) & \omega_t(P,W) \\ \omega_t(N,P) & \omega_t(N,N) & \omega_t(N,W) \\ \omega_t(W,P) & \omega_t(W,N) & \omega_t(W,W) \end{bmatrix}. \tag{3}$$

The main objects of interest are $\omega_t(P,N)$, the probability of switching from trade peace to NNTR, and $\omega_t(W,P)$, the probability of switching from trade war to trade peace. We make three assumptions about these objects. First $\omega_t(P,N)$ is constant before the trade war begins ($t < 2019$) and zero afterwards ($t \geq 2019$). This assumption is motivated by the increase in the NNTR-gap elasticity during the trade-war period. Second, the probability of a trade war starting, $\omega_t(P, W)$, is zero before the trade war begins ($t < 2019$). This assumption implies the tariff schedule in 2019 was unanticipated and is motivated by the stability of the trade-war gap elasticity during the pre-war period. Finally, we assume that year-to-year changes in Ω_t are unanticipated, i.e., firms expect the current matrix to remain in place going forward.

Trade costs. Firms pay variable costs of exporting (ξ) and fixed costs of entering (f_{g0}) and continuing in the U.S. market (f_{g1}). The variable cost takes three values ($\infty > \xi_{gH} > \xi_{gL}$) and follows a stationary Markov process. When $\xi = \infty$, the firm is a nonexporter. When a firm enters the export market, $\xi = \xi_{gH}$, and switches to $\xi = \xi_{gL}$ with probability $\rho_\xi \in (0, 1)$. This specification implies exporters start with high variable costs and, with repeated investments and some luck, gain access to the low-cost technology and expand their exports. We summarize the fixed-cost structure as a function, $f_g(\xi)$, where $f(\infty) = f_{g0}$ and $f(\xi_{gL}) = f(\xi_{gH}) = f_{g1}$. This setup generalizes the sunk-cost model of Das et al. (2007) to capture the exporter life cycle documented by Ruhl and Willis (2017).

Production and demand. Firms produce using labor, $y = z\ell$. Productivity, z, is independent across firms and follows a stationary Markov process. U.S. demand for a firm's good, d_{gt}, is a downward-sloping function of the tariff and the firm's price, p,

$$d_{gt}(p, s) = (p\tau_g(s))^{-\theta_g} D_{gt}, \tag{4}$$

where D_{gt} is an aggregate demand shifter and θ_g is the price elasticity of demand.

3.2 Optimization

The firm's export status is determined in the prior period. The firm is a monopolistic competitor that maximizes current-period profits by choosing its price, taking as given its residual demand and the wage, w,

$$\pi_{gt}(z, \xi, s) = \max_{p,\ell} p\, d_{gt}(p, \tau_g(s)) - w\ell \tag{5}$$

$$\text{s.t.} \quad z\ell \geq d_{gt}(p, \tau_g(s))\xi. \tag{6}$$

The value of a firm that chooses to export at $t+1$ is

$$V_{gt}^{1}\left(z,\xi,s\right)=-f_{g}(\xi)+\frac{\delta(z)}{1+r}\sum_{s'}\omega_{t}(s,s')\underset{z',\xi'}{\mathbb{E}_{\mathrm{t}}}V_{g,t+1}\left(z',\xi',s'\right), \tag{7}$$

where r is the interest rate. The value of a firm that chooses not to export at $t+1$ is

$$V_{gt}^{0}\left(z,\xi,s\right)=\frac{\delta(z)}{1+r}\sum_{s'}\omega_{t}(s,s')\underset{z'}{\mathbb{E}_{\mathrm{t}}}V_{g,t+1}\left(z',\infty,s'\right). \tag{8}$$

Given these objects, the value of the firm is

$$V_{gt}\left(z,\xi,s\right)=\pi_{gt}(z,\xi,s)+\max\left\{V_{gt}^{1}\left(z,\xi,s\right),V_{gt}^{0}\left(z,\xi,s\right)\right\}. \tag{9}$$

The break-even exporter has productivity $\bar{z}_{gt}(\xi,s)$ such that

$$V_{gt}^{1}\left(\bar{z}_{gt}(\xi,s),\xi,s\right)=V_{gt}^{0}\left(\bar{z}_{gt}(\xi,s),\xi,s\right). \tag{10}$$

This equation can be rewritten as

$$f_{g}(\xi)=\frac{\delta(\bar{z}_{gt}(\xi,s))}{1+r}\sum_{s'}\omega_{t}(s,s')\left\{\underset{z',\xi'}{\mathbb{E}_{\mathrm{t}}}\left[V_{g,t+1}(z',\xi',s')\right]-\underset{z'}{\mathbb{E}_{\mathrm{t}}}\left[V_{g,t+1}(z',\infty,s')\right]\right\}. \tag{11}$$

For the marginal firm, the fixed cost of exporting equals the expected gain in firm value from exporting in the future. Crucially, this object depends on the entire expected path of future tariffs, not only the current tariff rate.

3.3 Calibration

Our calibration has four stages. First, we map the model to the data by grouping HS-6 goods into 15 sectors. Second, we assign standard values to several parameters. Third, we calibrate the parameters that govern exporter dynamics to match moments from Chinese firm-level data before the trade war. Fourth, we calibrate the trade-policy transition probabilities to match our estimated dynamics of the trade-war and NNTR-gap elasticities. Table 1 provides an overview of the calibration.

Mapping goods to sectors. We assign each 6-digit HS good to one of 15 2-digit sectors in the China Industrial Classification System. We denote this assignment by a function $\gamma(g)$. We assume that the demand elasticity, θ_g, productivity dispersion, σ_{gz}, and the export costs, f_{g0}, f_{g1}, ξ_{gH}, and ξ_{gL}, vary across sectors but are the same for all goods within a sector, e.g., $\theta_g = \theta_{\gamma(g)}$ and $\sigma_{gz} = \sigma_{\gamma(g)z}$.

Functional forms and assigned parameters. The model period is one year. We normalize the wage to one and set the interest rate to four percent. The productivity process is

$$\log a' = \rho_z \ln a + \varepsilon, \qquad \varepsilon \overset{iid}{\sim} N(0, \sigma^2_{\gamma(g)z}), \tag{12}$$

where $z = \frac{1}{\theta-1}\log a$. The persistence parameter, ρ_z, is common to all firms, while the variance of the innovations, $\sigma^2_{\gamma(g)z}$, differs across sectors. The firm survival probability is $\delta\left(a\right) = 1 - \max\left[0, \min\left(e^{-\delta_0 a} + \delta_1, 1\right)\right]$, which implies higher-productivity firms are more likely to survive. We take the values of ρ_z, δ_0, and δ_1 from Alessandria et al. (2021). The import demand elasticities, $\theta_{\gamma(g)}$, are from Soderbery (2018). The low idiosyncratic iceberg trade cost, $\xi_{\gamma(g)L}$, is normalized to one for all sectors without loss of generality. The persistence of this cost, ρ_ξ, is taken from AKKRS. Finally, we take the probability of switching from the NNTR regime to the trade-peace regime, $\omega_t(N, P)$, from AKKRS, as this parameter can only be identified by data from before 1980, when China had NNTR status. We assume this parameter is constant over time and set it to their estimate of 0.71.

Parameters determined before the trade war. The parameters that govern production and exporter dynamics, $\sigma_{\gamma(g)z}, f_{\gamma(g)0}, f_{\gamma(g)1}$, and $\xi_{\gamma(g)H}$, are chosen to match moments from Chinese firm-level data under the assumption that in 2018, the economy has been in the trade-peace regime for many years. The moments are: the dispersion in log export sales, the fraction of firms that export, the fraction of exporters that stop exporting next period, and the ratio of the average exports of incumbent exporters to new exporters. These moments are computed, by sector, in the model and the data; the partial-equilibrium nature of our model allows us to calibrate each sector independently. The empirical moments and the estimated parameters are reported in Table 2.

Parameters determined during the trade war. We calibrate the probabilities of switching trade-policy regimes to match our estimates of the trade-war gap and NNTR-gap elasticities. Given the assumption that NNTR is no longer possible once the trade war starts, the probability of switching from trade peace to NNTR during the pre-war period, $\omega_{t<2019}(P, N)$, is identified by the change in the NNTR-gap elasticity between 2018 and 2024. The higher this probability, the more imports of goods with high NNTR gaps will grow relative to imports of goods with low NNTR gaps once the trade war begins and going back to NNTR is no longer possible. The probability of switching from trade war to trade peace, $\omega_t(W, P)$, is identified by the dynamics of the trade-war gap elasticity in the subsequent periods. For example, $\omega_{2019}(W, P)$ is identified by the trade-war gap elasticities from 2020 onward and $\omega_{2020}(W, P)$ by the elasticities from 2021 onward.

4 Results

First, we discuss our model's ability to account for the trade dynamics around the trade war and the path of trade-policy expectations implied by these dynamics. Second, we study the implications of our estimates for the future of U.S.-China trade. Finally, we relate the current substitution patterns and risks to the trade liberalization in 1980.

4.1 Dynamics of trade flows and trade policy

Figure 1(c) shows that the model captures the dynamics of both the trade-war gap and NNTR-gap elasticities. The former falls sharply between 2018–2020, then continues to fall gradually over the following four years. The latter rises after 2018, albeit more slowly in the model than in the data; the model reproduces the cumulative change. Figure 2(a) plots our main finding: the implied probabilities of switching between trade-policy regimes. Before the trade war began, the probability of moving from trade peace to NNTR was 13.6 percent. Once the trade war began, the probability of returning to trade peace was 74.5 percent in 2019 and 71.6 percent in 2020, but then fell sharply, reaching 20.8 percent in 2024.[9]

Figure 1(d) provides some intuition into the identification of these probabilities. The line labeled "No NNTR" depicts the evolution of the NNTR-gap elasticity when the probability of mov-

[9]This figure is very similar to the probability of moving from the NNTR regime to trade peace estimated in Alessandria et al. (2024b).

ing from trade peace to NNTR is constant at zero. The NNTR-gap elasticity barely changes; the slight increase is from the small negative correlation between the two gaps. In the baseline calibration, where the probability of moving to NNTR falls at the onset of the trade war, the line rotates upward and we can now match the growth in trade in these products. This reaffirms the idea that NNTR continued to be viewed as a possibility after China was granted PNTR.

Turning to how the persistence of the trade war affects trade dynamics, the line labeled "permanent war" shows how the trade-war gap elasticity evolves if the trade war is permanent, and the line labeled "one-period war" shows how this elasticity evolves if firms always believe the trade war will end in the next period. In the permanent case, the elasticity falls further over time as export participation in goods with high trade-war gaps decreases more. In the temporary case, the elasticity is flat after 2020 because export participation is unchanged; the movements in the elasticity in 2019 and 2020 are due purely to the intensive-margin response to the two rounds of trade-war tariffs. The differences between these two extremes and the calibrated model reflect changes in policy expectations over time, which determine investments in market access.

We assumed that the trade war was a surprise. Here, we use our model to study trade dynamics when firms anticipate the trade war.[10] In Figure 1(c), the line labeled "Pre-war corr. antic." shows, if firms believed ahead of time that the trade war was possible ($\omega_t(P, W) > 0$ for $t < 2019$), the trade-war gap elasticity would have fallen earlier. This anticipation is not in the data. Alternatively, as discussed in section 2, if firms thought tariffs could rise, but did not anticipate the trade-war tariffs specifically (e.g., they anticipated a common tariff increase on all products), the anticipatory effect is captured in the country-section-time fixed effects rather than the gap elasticities. The line labeled "Pre-war uncorr. antic." in Figure 1(e) shows these fixed effects would fall before the trade war in this scenario, whereas there are no statistically significant movements in the data or our baseline model.

Similarly, we can model how trade would have evolved if firms anticipated further tariff increases after the trade war started. If these increases were expected to be correlated with the current trade-war tariffs, the effect would show up as a downward movement in the trade-war gap elasticity, but this movement would be small and would not materially affect our estimates

[10] The appendix contains more details on our experiments with anticipation.

of the probability of ending the trade war. If additional tariff increases were expected to be uncorrelated with the trade-war tariffs, Figure 1(e) shows that the effect would again appear as a decline in the China-year fixed effects ("Post-war uncorr. antic."). There is no evidence of this decline in the data, either.

4.2 Implications for the future of U.S.-China trade policy and trade

Our estimated model yields forecasts of U.S.-China trade policy and trade flows. We also consider some alternative paths of trade policy to illustrate the mechanics of the model and the role of expectations.

Figure 2(b) plots the probability of being in the trade-peace regime in the future, conditional on being in the trade-war regime in 2024. For reference, we include the unconditional probability that China is in the trade-war regime since 1949 (about 54 percent). The conditional probability of being in trade-peace regime in 2025 is 21 percent, and this probability rises over time, eventually surpassing the unconditional probability in 2031. In the long-run, there is a 60 percent probability that China is in the trade-peace regime.

Figure 2(c) plots the evolution of the expected mean tariff. The "mean simulation" line is the average NTR tariff until 2019, the average trade-war tariff from 2019–2024, and the average expected tariff from 2024 onward. The "2020 beliefs" line is the expected path of tariffs from 2020 onward, starting from the trade-war regime, and similar for the "2022 beliefs" line. The former falls sharply, reflecting the high initial probability of ending the trade war, whereas the latter falls more slowly and converges to a higher level, reflecting the declining probability of trade peace as the trade war continues. The "2015 beliefs" line is the expected mean tariff conditional on being in the trade-peace regime in 2015. This expectation uses the pre-war transition probabilities. The long-run expected average tariff is higher than the post-war long-run average because the NNTR regime has higher average tariffs than the trade-war regime.

We can use our results to compare the changes in trade policy during the Trump and Biden administrations with the changes in policy expectations. We calculate two measures of policy expectations for each President: the expected duration of the trade war and the change in the mean discounted tariff. The expected duration is just the inverse of the transition probability in the final full year of each Presidency. The mean discounted expected tariff is

$$\tau_t^E = \mathbb{E}_t \frac{1}{G} \sum_{g=1}^{G} \frac{r}{1+r} \left(\sum_{k=t}^{\infty} (1+r)^{t-k} \tau_g(s_k) \right). \tag{13}$$

While the average tariff rises by 17.1 percentage points during the Trump administration, the mean discounted tariff falls by 5.3 percentage points, because the trade-war regime has a lower average tariff than the NNTR regime and the trade war is expected to end quickly during 2019–2020. At the end of the Trump presidency, the expected duration of the trade war is 1.4 years. Under Biden, the average applied tariff does not change, but the mean discounted tariff increases by 4.6 percentage points because the likelihood of ending the trade war falls during 2021–2024. The expected duration of the trade war in 2024 is 4.8 years.

What do our estimates imply about the future dynamics of U.S. imports from China? In Figure 2(d), we plot aggregate trade under different scenarios. In the "uncertain trade war" scenario, the trade war continues indefinitely but firms continue to believe that the trade war has a 21 percent chance of ending. In this scenario, trade declines gradually as Chinese exporters adjust to the trade-war tariffs and the decreasing probability of trade peace. In the long run, the aggregate level of U.S. imports from China is 0.49 log points lower than before the trade war.

The "uncertain trade peace" scenario considers a realization of uncertainty in which the trade war ends in 2025, and never restarts, although firms believe it has a 14 percent chance of restarting. In this scenario, aggregate trade would completely recover, even though there is a chance the trade war could restart, because there is no longer a chance of reaching the NNTR regime.

In the "permanent trade war" scenario, firms initially operate under the original pre-trade-war transition matrix, but when the trade war starts, they believe it will be permanent. On impact, trade falls by the same amount as in the baseline trade-war scenario, then continues to fall further. In the long run, aggregate trade stabilizes 0.75 log points below the pre-trade-war level—double the baseline scenario's decline—despite identical tariff paths.

At the other extreme, in the "permanent trade peace" scenario, the economy follows the baseline case until 2025, at which point the trade war ends and is expected to never resume.

We assume that returning to the NNTR regime is impossible; this scenario is a deeper form of integration than the pre-trade war status quo. On impact, imports increase by the same amount as in the uncertain trade-peace scenario, but grow more later, ultimately converging to 0.19 log points above the pre-trade war level. The gap in imports between the permanent and uncertain versions of trade peace arises from the increase in export participation caused by the elimination of uncertainty, including the possibility of restarting the trade war and the possibility of moving to the NNTR regime.

Our last approach considers the distribution of possible future outcomes by simulating a large number of potential trade-policy sequences, $\{s_t\}_{t=2025}^{\infty}$, holding the policy transition matrix constant, i.e., $\Omega_t = \Omega_{2023}$ for $t = 2024, \ldots, \infty$. In Figure 2(d), we plot the mean path of U.S. imports from China in these simulations. On average, trade grows from its 2024 level, but falls relative to its 2018 level by 0.16 log points in the long run.

4.3 Parallels to U.S.-China integration

The trade war was a large change in U.S. tariffs on China. Another large change occurred in 1980, when the United States granted China conditional NTR, lowering tariffs dramatically, subject to annual renewal by the U.S. President. Here, we show trade is adjusting to the current reform in a way similar to the earlier reform, albeit in the opposite direction, and we discuss the role of policy expectations in the two episodes.

AKKRS use a version of (2) to estimate annual NNTR-gap elasticities during 1974–2008. Figure 1(f) plots their estimated NNTR-gap elasticities against our trade-war gap elasticities, each normalized to zero in the year before the relevant reform. The elasticity dynamics in the two episodes are similar. In both cases, five years following the tariff change the trade elasticity was about four. Looking ahead, growth in the NNTR-gap elasticity accelerated in the mid-1980s and the trade elasticity more than doubled in the next five years. The NNTR-gap elasticity rose to almost 11 in 2001, when China joined the WTO.

AKKRS attribute part of the slow adjustment of U.S. imports from China following the 1980 liberalization to low credibility of that policy change. As U.S.-China relations improved throughout the 1980s, the policy gained credibility and the probability of losing the low-tariff regime fell. The low initial credibility discouraged Chinese firms from investing in U.S. market access, but as the reform gained credibility, Chinese firms invested in market access, and trade grew rapidly. A similar adjustment is underway during the trade war. The new tariffs were initially perceived as temporary, but as time passed, the trade-war regime gained credibility and U.S. imports have increasingly substituted away from Chinese sources. If history repeats itself, and expectations of remaining in the trade war rise, we should expect to see further substitution away from Chinese goods.

The 1980 trade liberalization can help us understand the trade war. In both episodes, we find policy credibility to be intertwined with the political cycle in the United States and important geopolitical considerations in similar ways.[11] The 1980 reform followed the normalization of relations with China by President Carter and severed diplomatic relations with Taiwan. It was a large shift in foreign policy that did not involve Congress. Congress quickly and overwhelmingly passed the Taiwan Relations Act in 1979, which required military support of Taiwan. It was a shift in foreign policy that treated China and the USSR equally on trade and created significant uncertainty over the state of U.S.-China policy. It was an important issue in the subsequent Carter-Reagan election. Reagan campaigned on restoring relations with Taiwan and, in the early stages of his presidency, took steps in this direction. Only with Reagan's visit to China in 1984 did the relationship become more credible.

Similarly, the 2018 reform was a substantial shift in trade policy on imports from China. Nearly every U.S. presidential election since Carter-Reagan discussed trade restrictions on China, but ended with minor changes in trade policy. In the 2020 election between Trump and Biden, Trump supported his tariffs while Biden pushed to engage China on a multilateral basis. However, since Biden entered office, the trade-war tariffs have remained and industrial policy, in the Chips and Science Act and the Inflation Reduction Act in 2022, further restricted imports from China. In May 2024, in the review of the trade-war tariffs, the Biden administration proposed increasing tariffs by 25 percent on almost 400 goods; most went into effect in September.

[11] The appendix includes a timeline of key moments in U.S.-China relations.

5 Conclusion

The trade war between the United States and China that began in 2018 demonstrated that China's Permanent Normal Trade Relations status did not eliminate trade-policy risk, and that the nature of this risk had fundamentally changed. At the beginning of the trade war, the expected path of future tariffs fell because the trade-war tariffs were expected to be quickly reversed and the likelihood of Non-Normal Trade Relations had diminished. As the trade war continued, expected tariffs grew.

Our estimation of the trade-policy process leverages heterogeneity across goods in observed tariffs, tariff risk, and trade dynamics. We interpret this heterogeneity using a model of forward-looking firms. Alternative processes that allow for other risks could yield different model outcomes, but should be disciplined by the dynamics of trade to these new and old risks. Likewise, alternative models could be used to discipline the trade-policy process, but these should be forward-looking, dynamic models; static models are silent on trade-policy expectations and are inconsistent with the gradual substitution patterns in U.S. imports since the onset of the trade war. Existing work on the aggregate effects of trade policy in static versus dynamic models (Alessandria et al., 2021; Mix, 2023) suggests a need to revisit the aggregate effects of the trade war. Our estimates of the stochastic path of trade policy could be an input to such an analysis.

The dynamics of U.S.-China trade disintegration resemble the dynamics of integration following the normalization of relations in 1980, but in reverse. Owing to geopolitical considerations and political turnover in each country, prior reform took time to be viewed as credible, which depressed import growth. Similar dynamics are at play on the eve of the 2024 U.S. Presidential election.

Declaration of interests

George Alessandria has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper. Alessandria received support from the National Science Foundation, award #2214852.

Shafaat Khan has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Armen Khederlarian has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Kim J. Ruhl has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper. Ruhl is a co-editor of the *Journal of International Economics* and was not involved in the editorial review or the decision to publish this article. Ruhl received support from the National Science Foundation, award #2214852.

Joseph B. Steinberg has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper. Steinberg is an associate editor of the *Journal of International Economics* and was not involved in the editorial review or the decision to publish this article.

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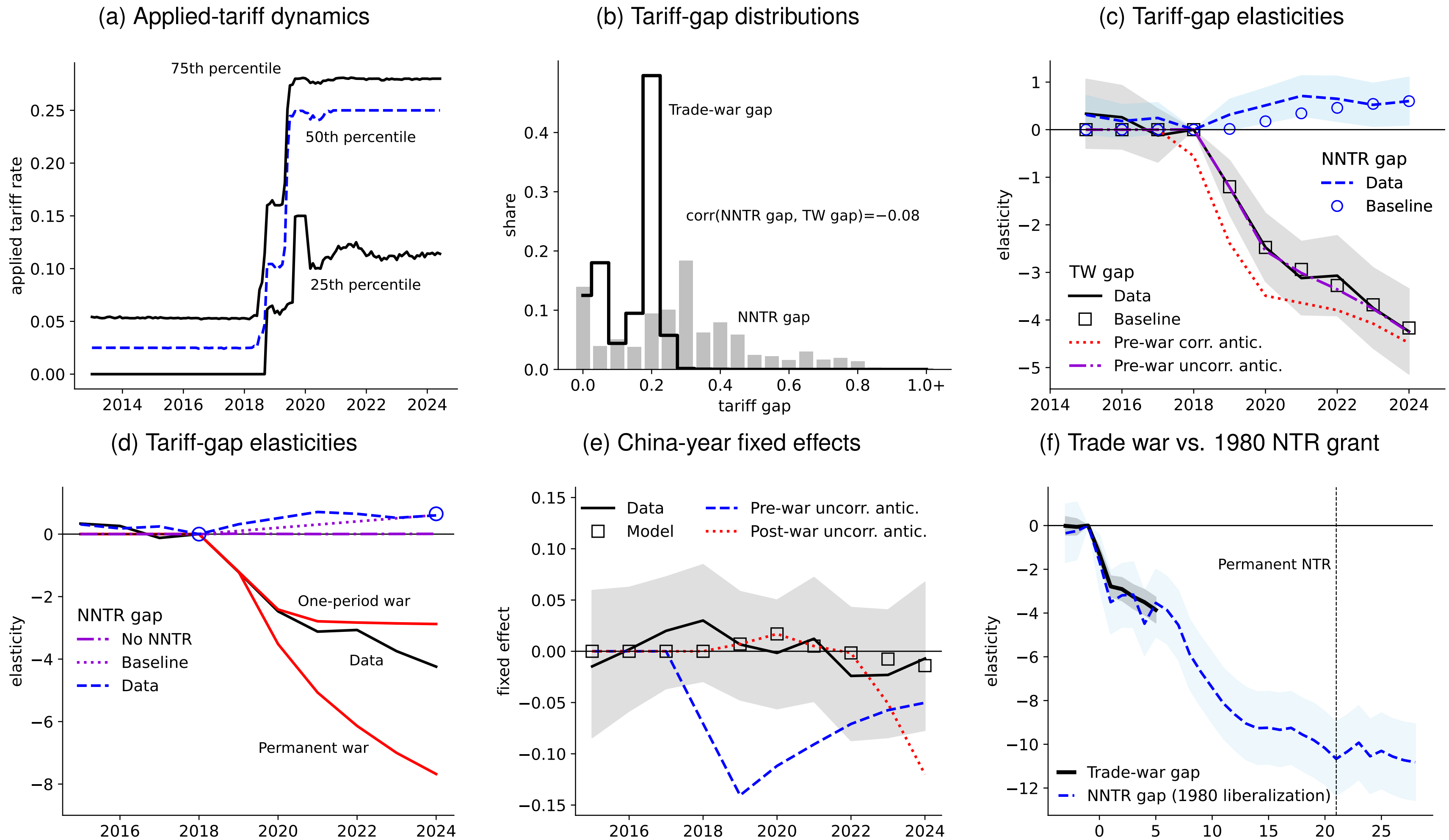
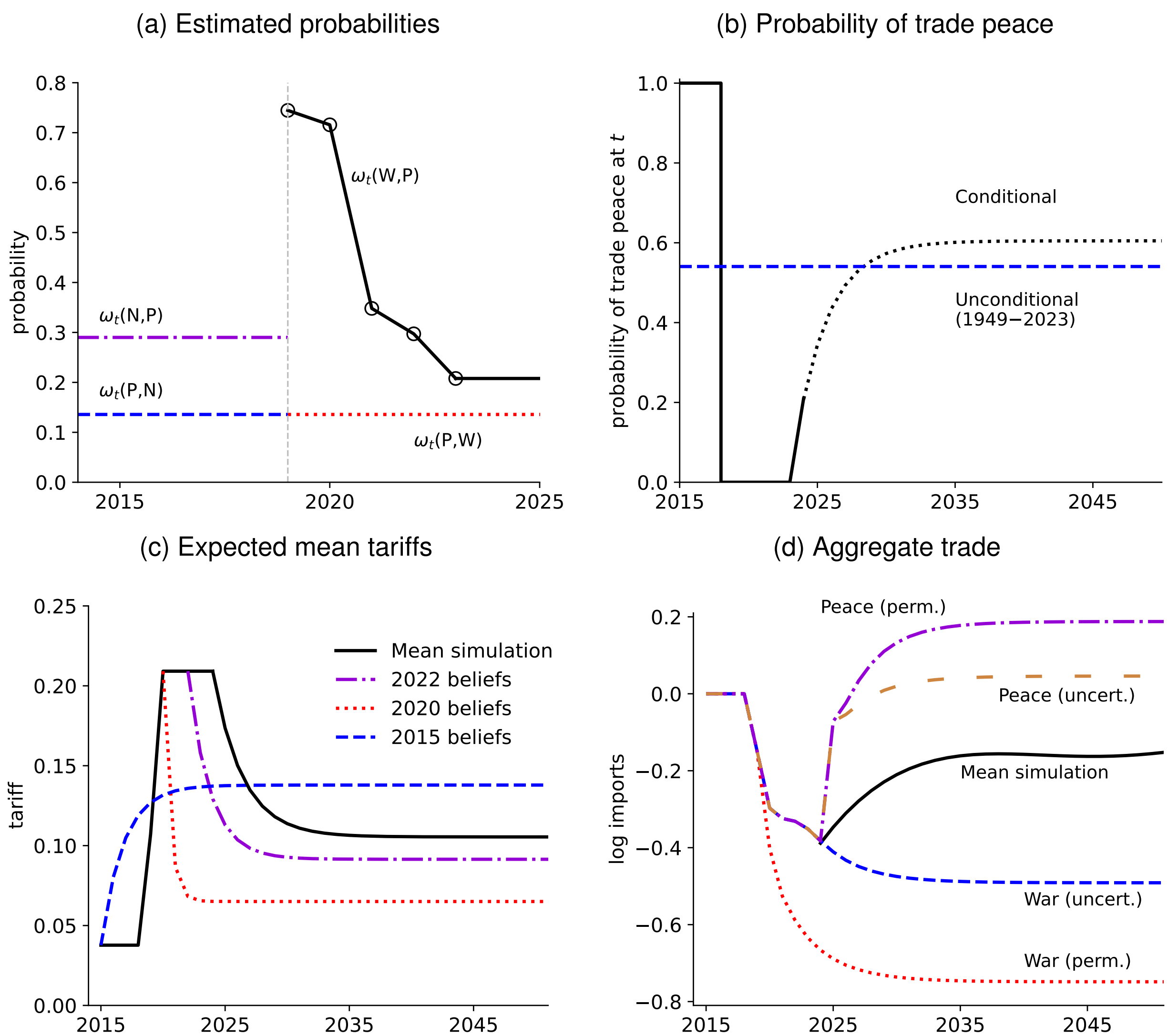
Figure 1: Dynamics of U.S. trade policy and imports from China



Notes: (a) Median and IQR of applied tariffs by year. (b) Trade-war gap and NNTR-gap distributions. (c) & (d) β_t^{NNTR} and β_t^{TW} from (2). (c) Data: 95-pct confidence intervals indicated by shaded areas. Baseline: calibrated model. Pre-war corr. antic.: model w/ anticipation of realized trade-war tariffs. Pre-war uncorr. antic.: model w/ anticipation of randomly-drawn tariffs uncorrelated with trade-war tariffs. (d) No NNTR: model w/ no chance of NNTR before trade war, i.e., $\omega_t(P,N) = 0\ \forall t$. One-period war: model w/ $\omega_t(W,W) = 0\ \forall t$. Permanent war: model w/ $\omega_t(W,W) = 1\ \forall t$. (e) Average China-HS-section fixed effect ($\frac{1}{H}\sum_{h=1}^{H} \delta_{\text{CHN},h,t}$) from (2). Data, Baseline, Pre-war uncorr. antic.: same as in (c); confidence interval constructed using bootstrap method. Post-war uncorr. antic.: model w/ anticipation of additional randomly-drawn tariffs after trade war begins. (f) β_t^{TW} versus NNTR-gap elasticity from Alessandria et al. (2024b) normalized to zero in 1979.

Figure 2: Model projections



Notes: (a) Trade-policy transition probabilities. (b) Probability of trade peace. Conditional: historical during 2015–2024 and model forecasts from 2025 onward. Unconditional: share of years in trade peace during 1949–2023. (c) Expected path of tariffs in 2015 versus 2020, alongside mean realized path from model simulations. (d) Aggregate trade projections under different scenarios. War (uncertain): remain in trade war with estimated transition matrix. Peace (uncertain): switch to trade peace in 2025 with estimated transition matrix. War (certain): remain in trade war with no chance of peace. Peace (certain): switch to trade peace in 2025 with no chance of war. Mean simulation: average path over 100 simulations of the model.

Table 1: Calibration summary

Parameter	Meaning	Value	Source/target
(a) Assigned			
r	Interest rate	4%	Standard
ρ_z	Persistence of productivity	0.65	Alessandria et al. (2021)
δ_0	Corr(survival, productivity)	21.04	Alessandria et al. (2021)
δ_1	Minimum death probability	0.023	Alessandria et al. (2021)
$\tau_g(N)$	NNTR tariff	Varies by good	Data
$\tau_g(P)$	NTR tariff	Varies by good	Data
$\tau_g(W)$	Trade-war tariff	Varies by good	Data
$\theta_{\gamma}(g)$	Demand elasticity	Varies by sector	Soderbery (2018)
ρ_ξ	Iceberg cost persistence	0.91	Alessandria et al. (2024b)
$\omega(N, P)$	NNTR persistence	0.71	Alessandria et al. (2024b)
(b) Determined before the trade war			
$f_{\gamma(g)0}$	Entry cost	Varies by sector	Export participation rate
$f_{\gamma(g)1}$	Continuation cost	Varies by sector	Export exit rate
$\xi_{\gamma(g)}$	High iceberg cost	Varies by sector	Incumbent premium
$\sigma_{\gamma(g)0}$	Entry cost	Varies by sector	CV of log sales
(c) Determined during the trade war (percent)			
$\omega(P, N)$	Prob. trade peace to NNTR	13.6	Δ NNTR-gap elasticity 2018–24
$\omega(W, P)_{2019}$	Prob. trade war to peace, 2019	74.5	Trade-war gap elasticity, 2020
$\omega(W, P)_{2020}$	Prob. trade war to peace, 2020	71.6	Trade-war gap elasticity, 2021
$\omega(W, P)_{2021}$	Prob. trade war to peace, 2021	34.8	Trade-war gap elasticity, 2022
$\omega(W, P)_{2022}$	Prob. trade war to peace, 2022	29.7	Trade-war gap elasticity, 2023
$\omega(W, P)_{2023}$	Prob. trade war to peace, 2023	20.8	Trade-war gap elasticity, 2024
(d) Implied trade-policy expectations (percent)			
τ^E_{2018}	Mean discounted tariff in 2018	12.7	Tariff data and estimated probabilities
τ^E_{2019}	Mean discounted tariff in 2019	7.2	Tariff data and estimated probabilities
τ^E_{2020}	Mean discounted tariff in 2020	7.3	Tariff data and estimated probabilities
τ^E_{2021}	Mean discounted tariff in 2021	9.8	Tariff data and estimated probabilities
τ^E_{2022}	Mean discounted tariff in 2022	10.4	Tariff data and estimated probabilities
τ^E_{2023}	Mean discounted tariff in 2023	11.9	Tariff data and estimated probabilities
τ^E_{2024}	Mean discounted tariff in 2024	11.9	Tariff data and estimated probabilities

Notes: The values of the parameters in panel (b) are reported in Table 2.

Table 2: Chinese exporter-dynamics statistics and sector-level model parameters

Sector	Target statistics				Parameters				
	Export part. (%)	Exit rate (%)	Incumbent prem.	Log CV exports	$\theta_{\gamma(g)}$	$f_{\gamma(g)0}$	$f_{\gamma(g)1}$	$\xi_{\gamma(g)H}$	$\sigma_{\gamma(g)z}$
Food & beverage	19	16	2.71	0.91	3.09	0.06	0.08	4.47	0.82
Textile & clothing	45	10	1.99	1.06	3.17	0.07	0.07	2.84	1.02
Wood products	24	13	2.05	1.09	2.79	0.13	0.12	4.95	0.99
Paper & printing	12	17	3.10	1.30	3.43	0.09	0.09	4.60	1.00
Energy & chemicals	19	15	3.23	1.48	2.99	0.12	0.12	6.49	1.11
Rubber & plastics	29	10	2.69	1.08	3.16	0.07	0.07	4.35	0.92
Non-metallic mineral	16	18	2.26	0.85	2.85	0.08	0.09	5.05	0.83
Base metal	12	21	3.96	1.15	3.04	0.06	0.09	6.93	0.88
Calendered metal	29	10	2.48	1.24	2.73	0.12	0.10	6.30	1.03
Other machinery	23	13	3.33	1.54	3.74	0.09	0.09	3.74	1.13
Computer & electronic	48	7	4.82	1.94	3.18	0.11	0.10	5.90	1.29
Electrical equipment	32	10	3.35	1.55	3.27	0.10	0.09	4.84	1.14
Vehicles	23	12	4.07	1.31	3.06	0.08	0.08	7.20	0.98
Furniture & others	59	7	1.76	0.95	3.26	0.07	0.07	2.18	1.01
Non-manufacturing	28	13	2.99	1.25	2.97	0.10	0.10	5.55	1.00

Notes: Exporter-dynamics statistics are calculated using Chinese firm-level data (see Alessandria et al., 2024b, for a detailed description). All statistics are sector-level averages during 2004 and 2007. Export participation: number of firms with positive export sales divided by total number of firms. Exit rate: number of firms that exported in $t-1$ but not in t, divided by number of exporters in t. Incumbent size premium: average sales of incumbent exporters divided by average sales of new exporters. Log CV of exports: natural log of coefficient of variation of export sales.

Appendix (For online publication)

In Appendix A, we include a timeline of key events in U.S.-China trade relations and a list of transitions between NTR and NNTR. In Appendix B, we show that the time-varying effects of the NNTR and trade-war gaps on China's exports to the United States, shown in Figure 1(c), are robust to a range of alternative approaches. In Appendix C, we describe the firm-level data used in our calibration of the model. In Appendix D, we explore alternative expectations of trade policy.

A U.S. trade-policy timeline

A.1 Key dates in U.S.-China relations

10/1949 People's Republic of China is established.

12/1950 The trade embargo on China begins.

06/1971 The trade embargo is lifted and China gains access to U.S. markets at NNTR rates.

02/1972 Nixon visits China and issues the Shanghai Communiqué.

01/1979 The United States and China normalize relations with the Joint Communiqué on the Establishment of Diplomatic Relations.

04/1979 The Taiwan Relations Act is passed by Congress and signed by Carter.

02/1980 China gains access to U.S. markets at NTR rates subject to annual renewal.

11/1980 Reagan is elected President of the United States.

07/1982 The Six Assurances are sent by the United States to Taiwan.

08/1982 The Third Communiqué between the United States and China is issued.

05/1984 Reagan visits China.

06/1986 China applies for observer status to the GATT.

10/2000 Bill is signed granting China Permanent NTR status upon joining the WTO.

12/2001 China joins the WTO.

11/2016 Trump is elected President of the United States.

03/2018 Broad tariffs are proposed on Chinese goods.

02/2020 Phase one of the trade deal between the United States and China begins.

11/2020 Biden is elected President of the United States.

5/2024 USTR 4-year review issued and new tariffs recommended.

A.2 Transitions from Normal to Non-Normal Trade Relations

1950 People's Republic of China and North Korea trade embargo

1951 Albania, Bulgaria, Czechoslovakia, East Germany, Hungary, Mongolia, Romania, Soviet Union

1954 North Vietnam

1960 Cuba embargo

1975 Vietnam, Cambodia, Laos trade embargo

1982 Poland

1986 Hungary

1989 Romania

1992 Serbia and Montenegro

2022 Belarus and Russia

B Robustness: Empirics

Alternative fixed effects. In our baseline specification, we use an exporter-importer-product (ijg) fixed effect that captures trade relative to the year before the trade war. We also use exporter-good-time (igt) and importer-good-time (jgt) fixed effects to control for demand and supply shocks for good g. These fixed effects are relatively standard in the literature. However, we also control for bilateral shocks at the sectoral level by including an ij-HS sections-t fixed effect. In columns 2 and 3 of Table A1, we show that imposing less restrictive ijt or more restrictive ij-HS 2-digit-t fixed effects yields similar results. In both cases, the time-varying path of the two gaps is very similar to our baseline (column 1), albeit slightly smaller in magnitude: the elasticities, on average, are 10 to 15 percent smaller than the baseline.

Alternative samples. Our baseline sample focuses on HS-6 goods that were (i) exported from China to the United States in every year of our sample period and (ii) were not affected by the tariffs the Trump administration imposed on countries other than China.[12] Column 4 of Table A1 relaxes the first restriction and allows for the sample of goods to be unbalanced. Column 5 further relaxes both restrictions, thus including the full sample of goods. Overall, the time-varying paths of elasticities are very similar. Column 6 reports results when we define the year as beginning in January and ending in December. In this case, we reference the effects to the year 2017. As expected, the 2018 effect is small, as tariffs had only been in place for part of the year. Hence, the jump in elasticities from the first to the second year is even larger under our baseline July to June definition of a year. Between 2020 and 2023, the elasticity grows by almost 30 percent compared with the corresponding 36 percent growth between 2021 and 2024 in our baseline.

[12] These were mostly steel and aluminum products targeted by the 2017 Section 232 tariffs and goods affected by the 2019 tariffs imposed on Mexico to deter migration. We obtain this set of goods from Fajgelbaum et al. (2020).

Placebo check with EU-27. Our baseline specification includes both the United States and the EU-27 as importers. In column 7 of Table A1, we conduct a placebo test to rule out unobserved supply shocks that may spuriously correlate with the gap measures, by using only the EU-27 as the importer. No significant pattern is found in Chinese exports to the EU related to any of the gap measures.

Gap measures. Our baseline trade-war gap, X_g^{TW}, is calculated as the difference between the average applied tariff to China between 2020–2023 and 2013–2017, at the HS-6 level. The NNTR-gap, X_g^{NNTR} is calculated as the difference between the six-digit NNTR rate and, again, the average applied tariff to China between 2013–2017, at the HS-6 level. Column 2 of Table A2 shows that both the NNTR-gap and trade-war gap elasticities are almost identical when we use the simple average over HS-10 products to calculate the average applied tariff to China in 2020–2023 and 2013–2017. Column 3 shows that our baseline results are very similar when we use the statutory tariff increases obtained from Fajgelbaum et al. (2020) as the trade-war gap, instead of applied tariffs.

Finer aggregation. Our baseline aggregation of goods is at the 6-digit HS level, which is commonly used in the literature (Handley et al., 2020). We also examine how the gap elasticities change when using more disaggregated definitions of goods at the 8- or 10-digit levels. However, since HS codes finer than the 6-digit level are not comparable across different importers, we restrict this analysis to U.S. imports and estimate the analogous equation to (2),

$$\log v_{igt} = \sum_{t'=2015}^{2024} \left(\beta_t^{NNTR} X_g^{NNTR} + \beta_t^{TW} X_g^{TW}\right) \mathbb{1}_{\{i=\text{China} \,\wedge\, t=t'\}} \qquad (14)$$
$$+ \delta_{gt} + \delta_{ig} + \delta_{iht} + \log c_{igt} + u_{igt}.$$

We also include a measure of shipping charges, c_{igt}.[13] In this specification, we do not aggregate the rest-of-the-world as a single exporter and include standard fixed effects for good-time, exporter-good, and exporter-HS section-time. Column 4 of Table A2 shows that our results are unaffected by restricting the analysis to U.S. imports and adding shipping charges as an additional control. Columns 5 and 6 of Table A2 show that our results are similar to our baseline estimates when we use a more disaggregate definition of goods, at the 8- or 10-digit level, respectively.

Quarterly frequency. The quarterly data are better suited to capture changes in trade flows at a higher frequency but require controlling for seasonal fluctuations that potentially differ by good and source. Figure A1 plots the elasticity of imports to the trade-war gap in the quarterly data. The quarterly data are through the second quarter of 2024.

[13]This is the difference between CIF and FOB trade values. We could not include this with the EU data, as Eurostat does not report FOB import values.

C Chinese firm-level data

The Chinese firm-level data is from an annual survey of manufacturing enterprises from the Chinese National Bureau of Statistics.[14] The dataset includes non-state firms with sales over 5 million RMB (about 600,000 U.S. dollars) and all state firms for 1998–2007. Information is derived from the balance sheet, profit and loss statements, and cash flow statements. The raw data consist of over 125,858 firms in 1998 and 306,298 firms in 2007 and includes sales, export revenues, value added, and number of employees. Firms are classified into industries according to the 4-digit Chinese National Industrial Classification (CNIC).

We follow the approach in our prior paper to concord these firms with our goods classified under the HS-6 goods. We proceed as follows. First, we apply the concordance between the 2-digit CNIC and the 3-digit ISIC (revision 2) reported in Table 2, obtained from Xie et al. (2020). Next, we apply the concordance between the 3-digit ISIC (revision 2) and the 4-digit SITC revision 2[15] and then a concordance to HS-6.

D Robustness: Model

We consider several alternative expectations in the model. First, we assume agents have perfect foresight over a time varying transition matrix. This set of expectations matches the qualitative pattern of transition probabilities from the baseline model, but yields a higher estimate that the trade war will end initially. Second, we assume there is an anticipated component to the trade war but that there is uncertainty over the good-specific tariff. Third, we explore the effect of a worsening of the trade war that has a good-specific component. These last two case show stronger responses in trade in the anticipation window. They also show that when the expected good-specific tariff is less correlated with the ultimate tariff, more of the trade response is captured by the China-year fixed effect.

Perfect foresight over transition probabilities. In the baseline model, we assume the year-to-year changes in the transition matrix Ω_t^W are unanticipated. Here, we assume instead that firms have perfect foresight over the entire path $\{\Omega_t^W\}_{t=2019}^{\infty}$ once the trade war begins. Figure A2 shows that this "perfect foresight" model yields qualitatively similar transition probabilities to our baseline model, but the likelihood of the trade war ending is consistently higher, especially in 2019 and 2020. The implications for policy expectations under Presidents Trump vs. Biden are shown in Table A3. At the end of the Trump presidency, the expected duration of the trade war is about one year and, in 2024, under Biden, it stands at 4.2 years. Despite the lower initial persistence of the trade war in the perfect-foresight model, the fact that firms know the persistence of the trade war will rise in the future leads to smaller differences in the changes in expected tariffs between the two administrations. In the perfect-foresight model, the discounted tariff fell 2.9 percent during the Trump administration and rose 1.5 percent during the Biden administration, compared to 5.3 percent and 4.6 percent, respectively, in the baseline model.

Anticipation of pre-war tariff increases. In the baseline model, we assume the trade war is unanticipated, which we argue is consistent with the empirical evidence. Here, we explore

[14] This data has been widely used to study Chinese manufacturing growth between the late 1990s and 2000s (see, for example, Bai et al., 2024). We thank Dan Lu for sharing the data with us.

[15] We obtain this concordance from Marc Muendler's website.

what happens when firms anticipate that tariffs could increase before the trade war begins, and that those increases may or may not be correlated with the actual tariffs that were implemented during the trade war. Starting in 2016, there is now a chance that each good g may draw a random tariff increase from the trade-war tariff distribution shown in Figure 1(b). We allow for the possibility that this hike may be correlated with a good's actual trade-war tariff in the following way. Using $\hat{\tau}_g$ to denote a good's random draw from the trade-war distribution, we set good g's tariff hike, which we denote by $\tilde{\tau}_g$, to a linear combination of that draw and its actual trade-war tariff: $\tilde{\tau}_g = \rho\tau_g(W) + (1-\rho)\hat{\tau}_g$. We do this experiment with two values of ρ: (i) zero (random tariff hikes are uncorrelated with actual trade-war tariffs); and (iii) one (full correlation). We also analyze a scenario where all goods get a common tariff increase of 17.1 percent, which is the average changed in applied tariffs between 2018 and 2020.

Figure A3 shows the results. Panel (a) shows clearly that trade begins to fall in anticipation of tariff hikes before the trade war actually begins. The decline is essentially the same in all three versions of the experiment, as the unconditional mean tariff hike is the same. However, the next two panels show that this same aggregate trade response is picked up differently by our estimation in the two scenarios. Panel (b) shows the trade-war gap elasticity and panel (c) the China-year fixed effect. In the common-tariff and zero-correlation ($\rho_\tau = 0$) scenarios, the anticipatory response is picked up almost entirely by the fixed effect, because this response is not correlated with the trade-war gap.[16] Conversely, in the full-correlation ($\rho_\tau = 1$) scenario, the gap elasticity picks up much more of the response and the fixed effect picks up less.

It is important to reiterate that we do not see any movement in the trade-war gap elasticity in the data until the trade war actually begins. Based on the results above, we can conclude that there is no evidence in the data of an anticipatory response that is correlated with the trade-war gap. On the basis of the (lack of) observed trade-war gap elasticity dynamics alone, it is not possible to rule out an anticipatory response that was uncorrelated with the trade-war tariffs (i.e., firms generally thought that tariffs could increase, but did not anticipate the specific tariffs that were ultimately put in place). However, recall that we also do not observe any statistically significant movements in the China-year fixed effects before the trade-war began. Based on that, we can conclude that there is no evidence of any kind of anticipation of future tariff hikes prior to the onset of the trade war.

Anticipation of post-war tariff increases. In the baseline model, we assume that once the trade war starts, there is no possibility that it could broaden or intensify. Here, we explore what happens when firms anticipate that additional tariff increases could happen, and that these increases may or may not be correlated with the trade-war tariffs, i.e., the trade war could broaden, intensify, or a mix of both. Starting in 2021, there is now a chance that each good may receive a random tariff increase modeled in the same way as before, i.e., a linear combination $\tilde{\tau}_g = \rho\tau_g(W) + (1-\rho)\hat{\tau}_g$ of a random draw $\hat{\tau}_g$ from the trade-war tariff distribution and that good's actual trade-war tariff $\tau_g(W)$. Again, We do this experiment with two values of ρ: (i) zero (random tariff hikes are uncorrelated with actual trade-war tariffs, which we interpret as a pure broadening of the trade war); and (iii) one (full correlation, or pure deepening). Again,

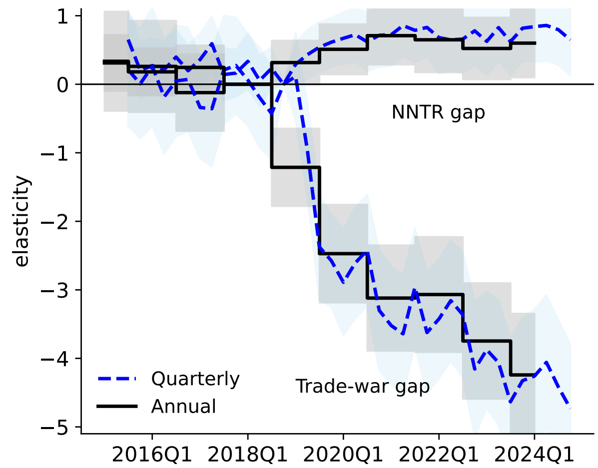
[16]The small movement in the gap elasticity in this scenario in Panel (b) of the figure is due to the fact that we have a finite number of goods, so we do not get a precisely zero correlation between the random tariffs and the trade-war tariffs.

we also look at a common-tariff scenario.

Figure A4 shows the results in the same format as in the previous exercise. Aggregate trade begins to decline sharply once the additional tariff hikes on top of the trade-war tariffs become possible in 2021. As before, the aggregate anticipatory response is similar across all the versions of the experiment, although there is a bit of nonlinearity in the model so the responses are not identical (e.g., the response to the potential of a given tariff increase is larger for a good with a low trade-war tariff than a high-trade-war tariff, and the former are more prevalent in the zero-correlation version). The same pattern as in the previous exercise emerges in terms of the way this aggregate response is picked up by the trade-war gap elasticity and the fixed effect. In the zero-correlation and common-tariff cases, the gap elasticity actually rises because goods with low trade-war tariffs are more impacted, and the fixed effect falls the most. In the full-correlation case, the gap elasticity falls the most and the fixed effect falls the least, because goods with high-trade war tariffs are most impacted.

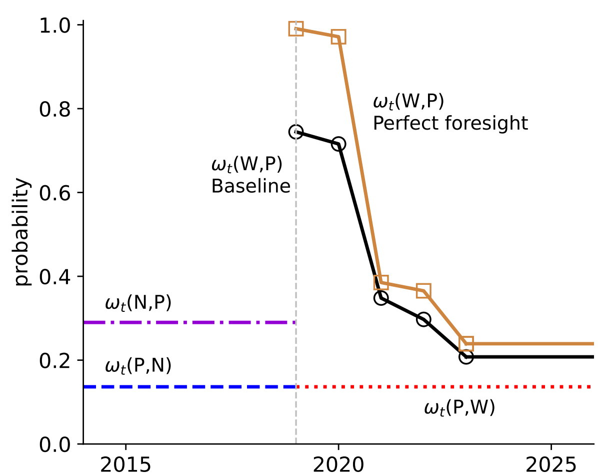
The most important takeaway from this exercise is that anticipation of the trade war broadening or intensifying does not materially affect the dynamics of the trade-war gap elasticity. Moreover, unless one has a strong prior that this anticipatory effect ought to be either completely uncorrelated or perfectly correlated with the original trade war tariffs, one should not expect to see any effect show up in the trade-war gap elasticity, anyway. We interpret these results to mean that our estimates of the probability of ending the trade war are not sensitive to whether this kind of anticipation exists or not. As in the previous exercise, if one wants to look for evidence of post-war anticipation the best place to look is in the aggregate trade response, or better yet the fixed effects from our specification, as aggregate trade movements are driven by lots of other factors that need to be controlled for. Again, we do not see any statistically significant movements in the fixed effects in the post-war period, although we do see limited evidence of a statistically insignificant decline consistent with anticipation of further tariff increases.

Figure A1: Tariff gap elasticities at the quarterly frequency



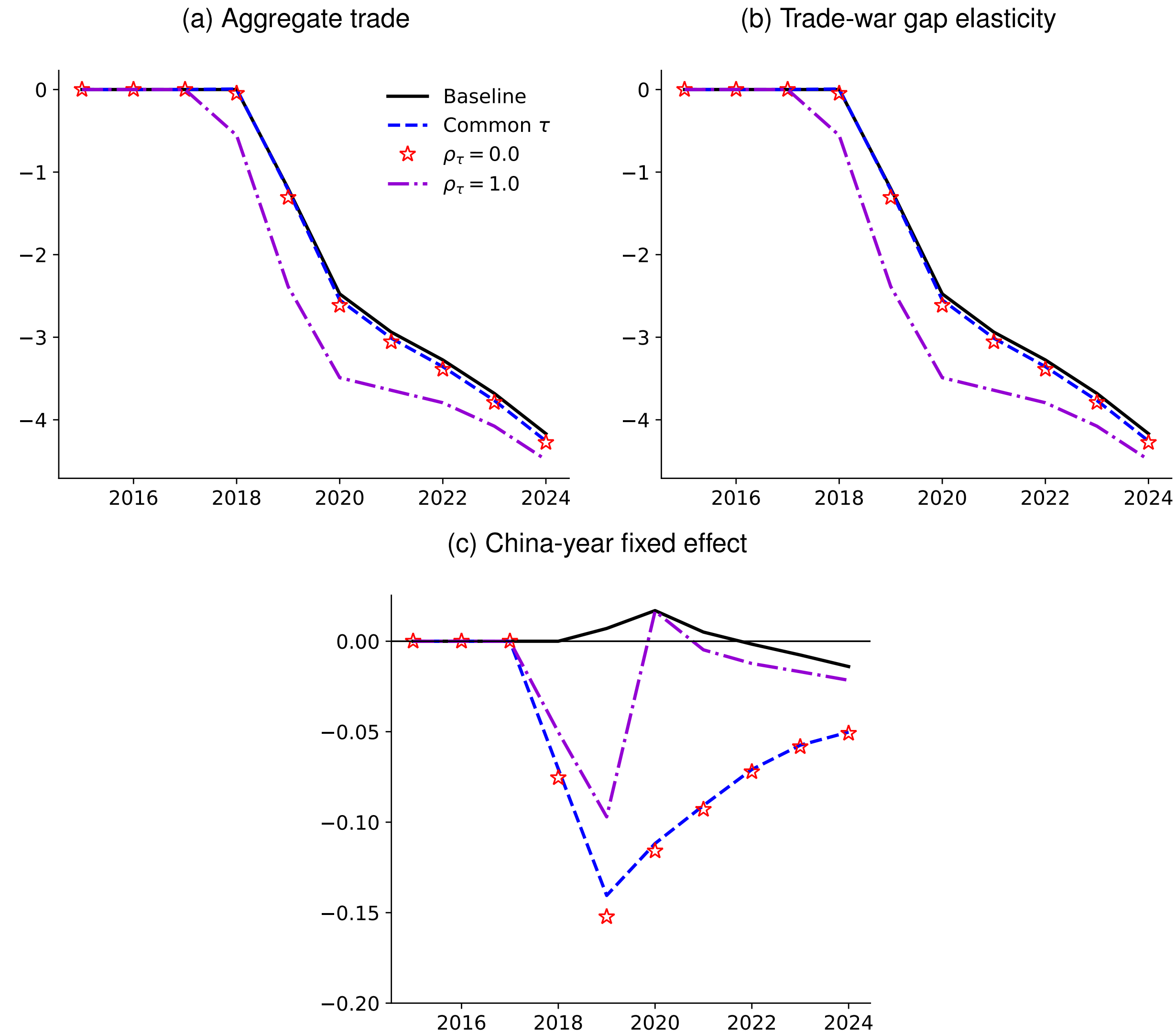
Notes: Figure shows estimates of β^{NNTR} and β^{TW} from (2). Black line: baseline estimates using annual data reported in main text. Blue line: Estimates using data aggregated to quarterly frequency.

Figure A2: Estimated probabilities in perfect-foresight model



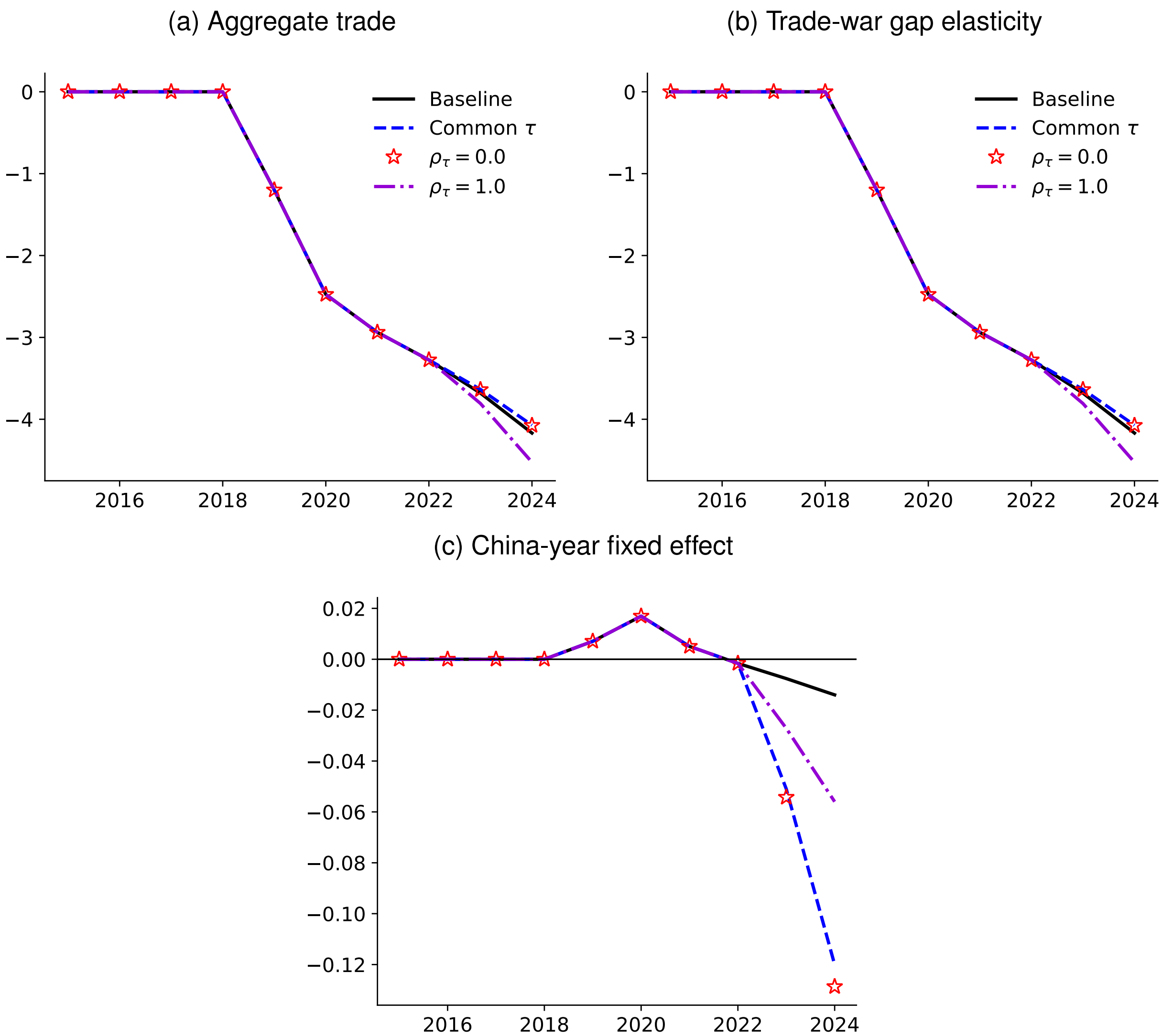
Notes: Figure compares baseline estimates of $\Omega_t(W,P)$ to estimates from perfect-foresight model where firms know entire path of $\{\Omega_t\}_{t=2019}^{\infty}$ when trade war starts.

Figure A3: Model results with pre-war tariff hike anticipation



Notes: Figure compares baseline model to models where each good has a chance of a random tariff increase starting in 2016. $\rho_\tau = 0.0$: Model where tariff increases are uncorrelated with actual trade-war tariffs. Common τ: Model where all goods get the same tariff increase of 17.2 percentage points. $\rho_\tau = 1.0$: Model where tariff increases are fully correlated with actual trade-war tariffs. Panel (a): Aggregate imports from China. Panel (b): coefficients β_t^{TW} from (2). Panel (c): mean across sectors $h = 1, \ldots, H$ of country-time fixed effects δ_{iht} for $i =$ China.

Figure A4: Model results with post-war tariff hike anticipation



Notes: Figure compares baseline model to models where each good has a chance of a random tariff increase starting in 2021. $\rho_\tau = 0.0$: Model where tariff increases are uncorrelated with actual trade-war tariffs. $\rho_\tau = 1.0$: Model where tariff increases are fully correlated with actual trade-war tariffs. Panel (a): Aggregate imports from China. Panel (b): coefficients β_t^{TW} from (2). Panel (c): mean across sectors $h = 1, \ldots, H$ of country-time fixed effects δ_{iht} for $i =$ China.

Table A1: Robustness: Gap elasticities

				Alternative Samples			
	Baseline	Alternative FEs		Unbalanced	Full	Jan-Dec	Placebo
Dep. var. v_{igt}	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\mathbb{1}\{^{t=t'}_{j=\text{CHN}}\}X_g^{\text{TW}}$							
2015	0.34	0.33	0.24	0.50	0.57	0.14	-0.56^{**}
	(0.37)	(0.35)	(0.44)	(0.42)	(0.43)	(0.32)	(0.29)
2016	0.26	0.10	0.36	0.43	0.54	-0.31	-0.31
	(0.34)	(0.32)	(0.41)	(0.40)	(0.41)	(0.23)	(0.26)
2017	-0.12	-0.10	-0.06	0.17	0.25	0.00	-0.16
	(0.29)	(0.27)	(0.34)	(0.34)	(0.33)	—	(0.20)
2018	0.00	0.00	0.00	0.00	0.00	-0.44^{*}	0.00
	—	—	—	—	—	(0.23)	—
2019	-1.21^{***}	-1.16^{***}	-1.16^{***}	-1.03^{***}	-0.92^{***}	-2.60^{***}	0.00
	(0.29)	(0.27)	(0.36)	(0.34)	(0.34)	(0.29)	(0.21)
2020	-2.47^{***}	-2.27^{***}	-2.52^{***}	-2.26^{***}	-2.39^{***}	-3.16^{***}	-0.13
	(0.36)	(0.33)	(0.44)	(0.39)	(0.39)	(0.36)	(0.24)
2021	-3.12^{***}	-2.75^{***}	-3.39^{***}	-2.72^{***}	-2.71^{***}	-3.34^{***}	0.42
	(0.39)	(0.36)	(0.46)	(0.42)	(0.41)	(0.37)	(0.27)
2022	-3.07^{***}	-2.71^{***}	-2.97^{***}	-2.61^{***}	-2.54^{***}	-3.44^{***}	0.03
	(0.43)	(0.40)	(0.52)	(0.46)	(0.45)	(0.38)	(0.30)
2023	-3.75^{***}	-3.26^{***}	-3.39^{***}	-3.25^{***}	-3.33^{***}	-4.10^{***}	0.17
	(0.43)	(0.40)	(0.51)	(0.46)	(0.46)	(0.41)	(0.31)
2024	-4.24^{***}	-3.86^{***}	-3.89^{***}	-3.69^{***}	-3.65^{***}	-4.44^{***}	0.74^{**}
	(0.46)	(0.43)	(0.54)	(0.51)	(0.50)	(0.44)	(0.33)
$\mathbb{1}\{^{t=t'}_{j=\text{CHN}}\}X_g^{\text{NNTR}}$							
2015	0.31	0.21	0.39^{*}	0.31	0.28	0.09	-0.22
	(0.21)	(0.18)	(0.22)	(0.22)	(0.22)	(0.16)	(0.19)
2016	0.18	0.06	0.34^{*}	0.18	0.19	0.22^{*}	-0.23
	(0.18)	(0.15)	(0.20)	(0.21)	(0.21)	(0.12)	(0.14)
2017	0.25	0.13	0.19	0.34^{*}	0.38^{**}	0.00	-0.18
	(0.16)	(0.13)	(0.18)	(0.18)	(0.18)	—	(0.13)
2018	0.00	0.00	0.00	0.00	0.00	0.03	0.00
	—	—	—	—	—	(0.13)	—
2019	0.32^{*}	0.26^{*}	0.29^{*}	0.39^{**}	0.34^{*}	0.41^{**}	-0.05
	(0.16)	(0.14)	(0.17)	(0.19)	(0.19)	(0.17)	(0.13)
2020	0.51^{***}	0.30^{*}	0.35^{*}	0.54^{**}	0.47^{**}	0.64^{***}	-0.01
	(0.19)	(0.17)	(0.20)	(0.21)	(0.21)	(0.20)	(0.13)
2021	0.71^{***}	0.38^{**}	0.44^{*}	0.78^{***}	0.70^{***}	0.45^{**}	0.01
	(0.22)	(0.19)	(0.23)	(0.23)	(0.23)	(0.19)	(0.16)
2022	0.65^{***}	0.21	0.42	0.55^{**}	0.51^{**}	0.66^{***}	-0.01
	(0.24)	(0.21)	(0.27)	(0.26)	(0.25)	(0.23)	(0.19)
2023	0.52^{**}	0.18	0.24	0.57^{**}	0.53^{**}	0.57^{**}	-0.10
	(0.23)	(0.20)	(0.25)	(0.26)	(0.25)	(0.23)	(0.17)
2024	0.60^{**}	0.46^{**}	0.31	0.72^{***}	0.71^{***}	0.56^{**}	-0.02
	(0.26)	(0.22)	(0.28)	(0.28)	(0.27)	(0.25)	(0.19)
jgt, igt, ijg FEs	✓	✓	✓	✓	✓	✓	
ij-HS Section-t FEs	✓			✓	✓	✓	
ijt FEs		✓					
ij-HS2-t FEs			✓				
gt, ig, i-HS Section-t FEs							✓
Observations	125,536	125,576	125,492	136,600	144,640	120,068	63,010
Adjusted R^2	0.94	0.94	0.93	0.93	0.92	0.94	0.95

Notes: The table reports estimates of (2). Columns 2 and 3 use less restrictive exporter-importer-time and more restrictive exporter-importer-HS2-time fixed effects, respectively. Column 4 uses an unbalanced panel and Column 5 uses the full sample, including goods that are part of trade disputes that do not discriminate only against China. Column 6 uses the conventional calendar year definition. Column 7 is a placebo test that uses only EU-27 imports. Standard errors clustered at the ijg level (and ig level in column 7) are reported in parenthesis.$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Robustness: Gap elasticities

		Alternative Gaps Measures		Good Level Aggregation		
	Baseline	Simple Avg Gaps	Statutory TW Gap	HS-6	HS-8	HS-10
Dep. var. v_{igt}	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{^{t=t'}_{j=\text{CHN}}\}X_g^{\text{TW}}$						
2015	0.34	0.37	-0.07	-0.12	0.28	0.33^{*}
	(0.37)	(0.39)	(0.48)	(0.24)	(0.20)	(0.18)
2016	0.26	0.33	-0.04	0.07	0.47^{**}	0.50^{***}
	(0.34)	(0.35)	(0.45)	(0.24)	(0.19)	(0.17)
2017	-0.12	-0.10	-0.18	-0.10	0.21	0.30^{**}
	(0.29)	(0.30)	(0.40)	(0.20)	(0.17)	(0.15)
2018	0.00	0.00	0.00	0.00	0.00	0.00
	—	—	—	—	—	—
2019	-1.21^{***}	-1.25^{***}	-1.41^{***}	-1.23^{***}	-1.27^{***}	-1.15^{***}
	(0.29)	(0.31)	(0.37)	(0.20)	(0.18)	(0.15)
2020	-2.47^{***}	-2.61^{***}	-3.55^{***}	-2.70^{***}	-2.90^{***}	-2.84^{***}
	(0.36)	(0.38)	(0.43)	(0.28)	(0.22)	(0.19)
2021	-3.12^{***}	-3.22^{***}	-4.09^{***}	-2.87^{***}	-3.16^{***}	-3.10^{***}
	(0.39)	(0.41)	(0.49)	(0.30)	(0.24)	(0.21)
2022	-3.07^{***}	-3.14^{***}	-4.17^{***}	-3.28^{***}	-3.29^{***}	-3.23^{***}
	(0.43)	(0.45)	(0.52)	(0.31)	(0.24)	(0.21)
2023	-3.75^{***}	-3.77^{***}	-4.55^{***}	-3.60^{***}	-3.76^{***}	-3.68^{***}
	(0.43)	(0.45)	(0.52)	(0.33)	(0.26)	(0.23)
2024	-4.24^{***}	-4.25^{***}	-5.22^{***}	-3.90^{***}	-3.89^{***}	-3.84^{***}
	(0.46)	(0.47)	(0.56)	(0.33)	(0.27)	(0.23)
$\mathbb{1}\{^{t=t'}_{j=\text{CHN}}\}X_g^{\text{NNTR}}$						
2015	0.31	0.35	0.29	0.17	0.00	-0.06
	(0.21)	(0.22)	(0.21)	(0.13)	(0.10)	(0.09)
2016	0.18	0.19	0.17	0.08	-0.01	-0.08
	(0.18)	(0.18)	(0.17)	(0.12)	(0.09)	(0.08)
2017	0.25	0.23	0.24	0.14	0.03	0.02
	(0.16)	(0.17)	(0.16)	(0.11)	(0.09)	(0.08)
2018	0.00	0.00	0.00	0.00	0.00	0.00
	—	—	—	—	—	—
2019	0.32^{*}	0.35^{**}	0.30^{*}	0.33^{***}	0.22^{**}	0.26^{***}
	(0.16)	(0.17)	(0.16)	(0.11)	(0.09)	(0.08)
2020	0.51^{***}	0.55^{***}	0.45^{**}	0.56^{***}	0.25^{**}	0.25^{***}
	(0.19)	(0.20)	(0.19)	(0.16)	(0.11)	(0.10)
2021	0.71^{***}	0.73^{***}	0.65^{***}	0.80^{***}	0.49^{***}	0.50^{***}
	(0.22)	(0.22)	(0.21)	(0.15)	(0.12)	(0.10)
2022	0.65^{***}	0.67^{***}	0.58^{**}	0.71^{***}	0.50^{***}	0.55^{***}
	(0.24)	(0.25)	(0.24)	(0.16)	(0.13)	(0.11)
2023	0.52^{**}	0.57^{**}	0.46^{**}	0.51^{***}	0.41^{***}	0.47^{***}
	(0.23)	(0.24)	(0.23)	(0.17)	(0.13)	(0.11)
2024	0.60^{**}	0.64^{**}	0.53^{**}	0.64^{***}	0.53^{***}	0.46^{***}
	(0.26)	(0.27)	(0.25)	(0.18)	(0.14)	(0.12)
log Shipping Cost				-2.53^{***}	-2.51^{***}	-2.52^{***}
				(0.03)	(0.03)	(0.02)
jgt, igt, ijg FEs	✓	✓	✓			
ij-HS Section-t FEs	✓	✓	✓			
gt, ig, i-HS Section-t FEs				✓	✓	✓
Observations	125,536	125,536	125,536	1,025,166	1,250,280	1,764,930
Adjusted R^2	0.94	0.94	0.94	0.88	0.86	0.85

Notes: The table reports estimates of (2). Columns 2 and 3 consider alternative definitions of the gap—column 2 uses the simple averages of the pre- and post-war HS-10 tariffs instead of the weighted average—and column 3 uses the statutory trade war tariff increases. Columns 4, 5, and 6 focus on the U.S. as the sole importer, using HS-6, HS-8, and HS-10 codes to define the good, respectively. Standard errors, clustered at the ijg-level in columns 1-3 and at the ig-level in columns 4-6, are reported in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Trade-policy innovations by administration in perfect-foresight model

	Baseline		Perfect foresight	
	Trump	Biden	Trump	Biden
Expected duration (years)	1.4	4.8	1.0	4.2
Change in mean, discounted expected tariff (%)	−5.3	4.6	−2.9	1.5
Change in applied tariff (%)	17.1	0.0	17.1	0.0

Notes: Expected duration is calculated as the inverse of the transition probability in 2020 for Trump and in 2024 for Biden. The change in the mean discounted tariff is based on changes in the mean discounted path from the start to end of each administration.