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TRADE WAR AND PEACE:
U.S.-CHINA TRADE AND TARIFF RISK FROM 2015–2050

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ABSTRACT

We use U.S. import dynamics and a dynamic exporting model to estimate how expectations about U.S. tariffs on China have changed around the U.S.-China trade war. We find (i) there was 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 that 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 remains an issue in 2024.

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 time-varying probability of switching between regimes. We estimate these probabilities using indirect inference by aligning the simulated differences in import growth across products before and during the trade war in the model with the differences in the data.

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 actually began in 2018. 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—imports of products with high trade-war gaps grew at about the same pace as imports of products with low trade-war gaps—suggesting there was no anticipatory response to these tariffs.[2]

Second, during the first two years after the trade war began, the probability that tariffs

[1] In 1971 Nixon imposed a 10-percent surcharge on all dutiable imports but removed it four months later.

[2] Alternatively, the expected tariffs may have been uncorrelated with the realized tariffs. We show that this should be captured by a decline in the China-year fixed effect in advance of the tariff realization. We find no change is this fixed effect.

would return to NTR levels was very high—almost 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 17 percent. The dynamics of this transition 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, ever since China was granted access to NTR tariffs in 1980, 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 steadily 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 many 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 separate contributions of the Trump and Biden administrations to changes in those paths. We find, even though Trump raised tariffs and Biden only maintained those tariffs, Trump lowered the discounted expected mean tariff by 4.1 percentage points while Biden raised it by 4.7 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 accounts for the increase in expected future tariffs.

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

Our analysis also highlights clear 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 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.

Our paper contributes to the literature on the U.S.-China trade war (Fajgelbaum and Khandelwal, 2022, and Caliendo and Parro, 2023). Our novel approach explicitly considers the dynamics of trade substitution and recovers the trade-regime transition probabilities from theory. It 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 begin with an empirical analysis of the dynamics of U.S. tariffs on, and imports of, Chinese goods. We document several novel patterns of import substitution to two measures of good-level trade-policy risk.

2.1 Data

We use U.S. import data from the U.S. Census Bureau covering July 2014–May 2024, aggregated to the 6-digit subheading of the Harmonized System (HS-6). Imports of good g from country i are denoted by v_{igt}. Applied tariffs, denoted by τ_{igt}, are measured as duties divided by import values. We use a balanced sample—goods imported from China every year—and exclude goods that were affected by trade policies that were not China-specific. We annualize

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

the data to avoid concerns about stockpiling in advance of possible tariff changes.[5] To align with the timing of the trade war, we define a year as starting in July and ending in June.[6]

Figure 1(a) plots the paths of 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 and remains elevated.

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 additional tariffs that Chinese imports face outside of the NTR regime.[7] 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,

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

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, most country-level increases in U.S. tariffs have involved moving from NTR to NNTR tariffs or an outright embargo. For example, in 2022 Russia and Belarus were shifted to NNTR tariffs following Russia's invasion of Ukraine.[8]

Figure 1(b) plots the trade-war gap and NNTR-gap distributions and Figure 1(c) shows their correlation. 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 will play an important role in our measurement of how expected future tariffs have changed since the trade war began. Second, the two gaps are approximately orthogonal, with a correlation of only –0.08. This means, on average, goods that are exposed to one risk are not exposed to the other. This is what allows us to separately identify the probabilities of these risks from the trade data.

[5] 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.

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

[7] Country-specific tariff risk will be absorbed in country-year fixed effects as discussed in the appendix.

[8] Numerous proposals have sought to return China to the conditional NTR policy of the 1990s.

2.2 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,

$$\log v_{igt} = \sum_{t=2015}^{2024} \left(\beta_t^{NNTR} X_g^{NNTR} + \beta_t^{TW} X_g^{TW}\right) \mathbb{1}_{\{i=China \,\wedge\, t=t'\}} \\ + \delta_{gt} + \delta_{ig} + \delta_{iht} + \log c_{igt} + u_{igt}, \tag{2}$$

where δ_{ig} and δ_{gt} are country-good, and good-time fixed effects; δ_{iht} is a country-time fixed effect at the HS-Section level, and c_{igt} is a measure of shipping charges.[9] As is common in event-studies, we reference the source-good fixed effects to the year before the tariffs rise, 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 U.S. demand shocks, time-invariant bilateral trade barriers, and aggregate shocks to exporting countries.

Figure 1(d) plots the estimates of equation (2). The trade-war gap elasticity was statistically indistinguishable from zero during 2015–2017, suggesting a constant likelihood of a trade war during this period. During 2019–2020, the trade-war elasticity fell to about –2.8, likely reflecting the intensive-margin response to the increase in tariffs. From 2021 onward, it fell gradually by another 1.3 log points. There are two possible explanations for this growing substitution: (i) trade was gradually adjusting to the increase in tariffs, or (ii) the likelihood that these tariffs would be reversed was falling. This is because the trade-war gap has a dual meaning: it represents the size of the past tariff increase imposed at the onset of the trade war, but it also represents the potential future tariff reduction if the trade war ends. As AKKRS argue, a structural model is needed to disentangle these two channels.

The NNTR-gap elasticity was also statistically indistinguishable from zero during 2015–2017, indicating that the probability of going back to NNTR status was stable during this period as well. In 2019, it began to rise, and by 2024 was almost one log point higher than before

[9] Shipping *charges* are the difference between the CIF import value and the FOB import value. c_{igt} is the logarithm of one plus a good's charges divided by its FOB import value.

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 policy regimes. After the trade war began, the likelihood of going back to NNTR status fell and the uncertainty was now largely about moving between trade war and "trade peace."

In the appendix, we show that our results are robust to different levels of aggregation both across goods and time, an unbalanced sample, and a host of additional controls. Most importantly, our results are robust to a specification that includes European Union imports, which allows us to include exporter-good-time fixed effects to control for supply conditions in exporting countries.

3 Structural model

Our empirical findings are inputs to a structural model that 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 model builds on Alessandria et al. (2021) and AKKRS by introducing a rich stochastic process for trade policy featuring multidimensional tariff risk.

3.1 Environment

There are G goods that correspond to the HS-6 goods in our 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 status. 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 tariff 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 in period t expect the current matrix to remain in place going forward. This allows us to avoid taking a stand on how transition probabilities might evolve in the future. We study models in which firms anticipate future changes in Ω_t, including a model where transition probabilities can occur only alongside political transitions, in the appendix.

Trade costs. Firms pay variable costs of exporting (ξ) and fixed costs of entering the U.S. market (f_{g0}) and continuing in the market (f_{g1}). The variable cost takes three values ($\infty > \xi_{gH} > \xi_{gL}$) and follows a stationary first-order 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. The fixed costs are a function of the firm's export participation status. 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

$$V^1_{gt}(z, \xi, s) = -f_g(\xi) + \frac{\delta(z)}{1+r} \sum_{s'} \omega_t(s, s') \mathop{\mathbb{E}_{\mathrm{t}}}_{z',\xi'} V_{g,t+1}(z', \xi', s'), \tag{7}$$

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

$$V^0_{gt}(z, \xi, s) = \frac{\delta(z)}{1+r} \sum_{s'} \omega_t(s, s') \mathop{\mathbb{E}_{\mathrm{t}}}_{z'} V_{t+1}(z', \infty, s'). \tag{8}$$

Given these objects, the value of the firm is

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

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

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

This equation can be rewritten as

$$f_g(\xi) = \frac{\delta(z)}{1+r}\sum_{s'}\omega_t(s,s')\left\{\mathop{\mathbb{E}_{\mathrm{t}}}_{z',\xi'}\left[V_{t+1}(z',\xi',s')\right] - \mathop{\mathbb{E}_{\mathrm{t}}}_{z'}\left[V_{t+1}(z',\infty,s')\right]\right\}, \tag{11}$$

which says that, for the marginal firm, the fixed cost of exporting equals the expected gain in firm value from exporting in the future. Crucially, this latter object depends on the entire expected path of future tariffs, not 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 gap and NNTR-gap elasticities. Table 1 provides an overview of our 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 $\delta(a) = 1 - \max\left[0, \min\left(e^{-\delta_0 a} + \delta_1, 1\right)\right]$, which implies that 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 also 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 separately for each sector in both the model and the data; the partial-equilibrium nature of our model allows us to calibrate each sector independently. The empirical moments, which are taken from AKKRS, 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 dynamics of the elasticities of trade to the trade-war gap and the NNTR-gap. 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(d) shows that the model captures the dynamics of both the trade-war gap and NNTR-gap elasticities. The former falls sharply between 2018–2020, and 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 in 2019, the probability of moving from trade peace to NNTR was 11.8 percent. Once the trade war began, the probability of returning to trade peace was 70.0 percent in 2019 and 57.0 percent in 2020, but then fell sharply, reaching 16.7 percent in 2024.

Figure 1(d) also provides some intuition into the identification of these probabilities. The gold dashed line labeled "NNTR gap (constant probability)" depicts the evolution of the NNTR-gap elasticity when the probability of moving from trade peace to NNTR is constant at zero. Now the NNTR-gap elasticity barely changes; the slight increase is from the small negative correlation between the two gaps. The red dotted line labeled "TW gap (permanent)" shows how the trade-war gap elasticity evolves if the trade war is permanent, and the purple dash-dotted line labeled "TW gap (transitory)" shows how this elasticity evolves if firms always believe the trade war is certain to 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 can use our results to compare the changes in applied 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 uses the discount factor, $\beta = 1/(1+r)$, to weight expected future tariffs, and is equal to

$$\tau_t^E = \frac{1}{G}\sum_{g=1}^{G}(1-\beta)\left(\sum_{s=t}^{\infty}\beta^{s-t}\mathbb{E}_t[\tau_{gs}]\right). \tag{13}$$

While the average applied tariff rises by 17 percentage points during the Trump administration, the mean discounted tariff actually falls by 4.1 percentage points, because the trade-war regime has a lower average tariff than the NNTR regime, but also because 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 1.8 years. Under Biden, the average applied tariff does not change, but the mean discounted tariff increases by 4.7 percentage points given the decline in the likelihood of ending the trade war during 2021–2024. The expected duration of the trade war in 2024 is 6 years.

We can also use our model to study trade dynamics if firms had anticipated that tariffs could rise before the trade war.[10] The green dash-dotted line labeled "TW gap (correlated anticipation)" in Figure 1(d) shows that if firms believed ahead of time there was a chance the trade-war tariffs could be implemented (i.e., $\omega_t(P,W) > 0$ for $t < 2019$), the trade-war gap elasticity would have begun to fall earlier. This anticipation is not in the data. Alternatively, 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 (δ_{iht} for $i = China$) rather than the gap elasticities. The blue dashed line labeled "Pre-war uncorrelated anticipation" Figure 1(e) shows that these fixed effects would fall before the trade war, whereas they would remain flat in our baseline scenario. The figure also shows that there are no statistically significant movements in these fixed effects in the data.

Similarly, we can ask our 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 very small and would not materially affect

[10]See the appendix for more details on our experiments with anticipation effects.

our estimates 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 (red dotted line labeled "Post-war uncorrelated anticipation"). There is no evidence of this sort of 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 the evolution 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 17 percent, but this probability rises over time and eventually surpasses the unconditional probability in 2031. In the long-run, there is a 58.6 percent probability 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 state, 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 levels off at a higher level, reflecting the decline in this probability 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 several percentage points higher than the post-war long-run average because the NNTR regime has higher average tariffs than the trade-war regime.

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 17 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 almost 60 percent 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 an 11 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 ending up in the NNTR regime.

In the "permanent trade war" scenario, firms initially operate under the original pre-trade war transition matrix, Ω^P, but, when the trade war starts, they believe it will be permanent. As seen in Figure 2(d), on impact, trade falls by the same amount as in our baseline trade-war scenario, but then continues to fall further. In the long run, aggregate trade stabilizes at –0.85 log points below the pre-trade war level—a 60 percent larger drop than in the baseline case—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 (section 4.2), but grow more later, ultimately converging to 25 percent 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 as well as the possibility of moving to the NNTR regime.

Our last approach is to consider 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. Average trade grows from its 2024 level, but the long-run aggregate trade declines about 20 percent from 2018.

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. We now show that 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 equation (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 4. 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 to invest 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 state rise, we should expect to see further substitution away from Chinese goods.

The 1980 trade liberalization can help us understand the trade war. The perceived credibility of both reforms was initially low, and for the earlier reform, grew as time passed. 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

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

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 point of contention in the subsequent Carter-Reagan election. Reagan campaigned on restoring relations to 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 certain industries. Finally, in May of 2024, in the review of the trade-war tariffs, the Biden administration expanded some tariffs.

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 actually 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 shifted upwards.

Our approach to estimating the trade-policy process leverages heterogeneity across goods in observed tariffs, tariff risk, and trade dynamics. We interpret this heterogeneity using a forward-looking model of exporting in which firms that are the most affected by changes in trade-policy risk respond the most. Alternative processes that allow for other risks could yield different model outcomes, but should be disciplined by the dynamics of trade to these new 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 and trade relations 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.

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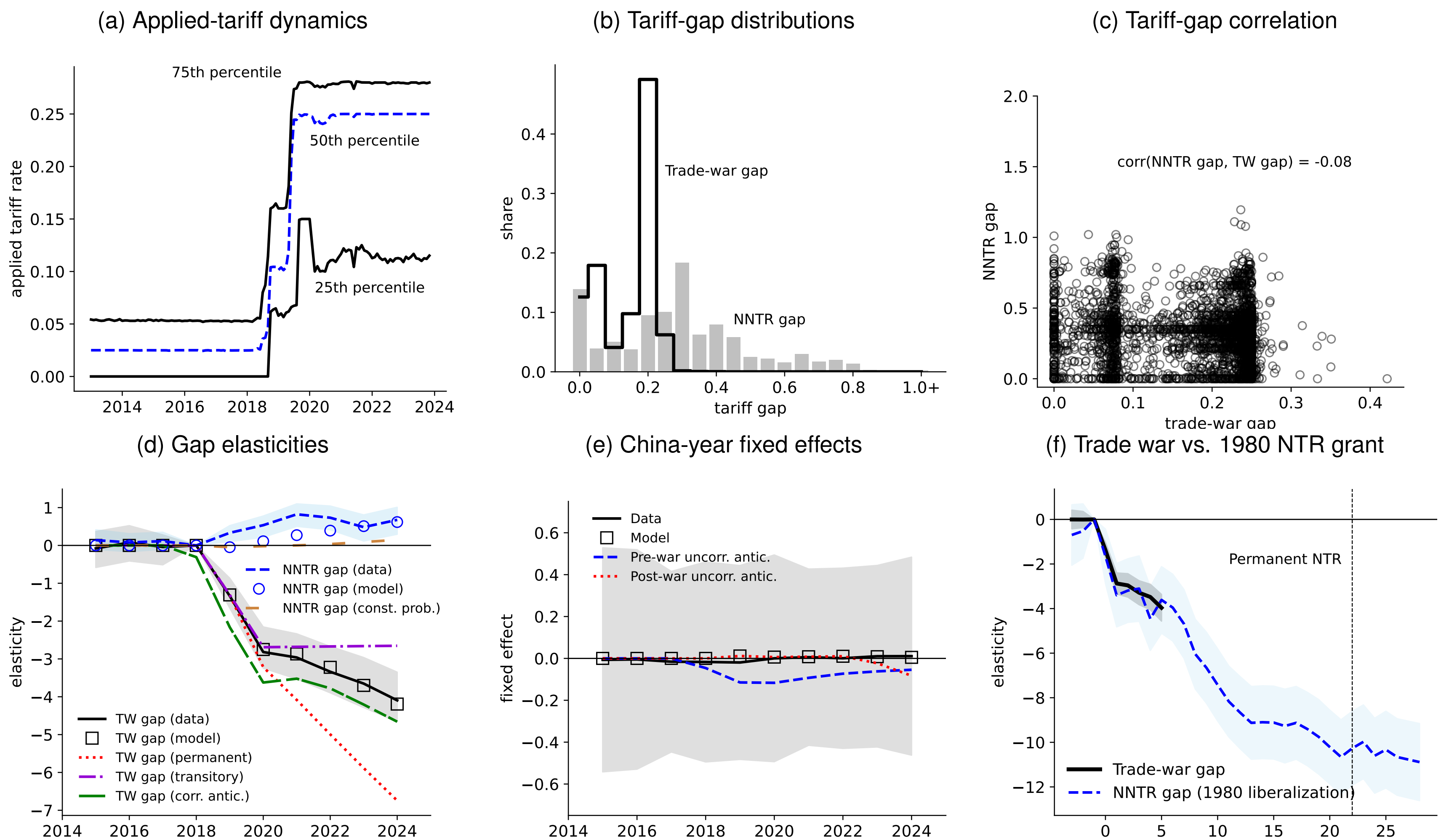
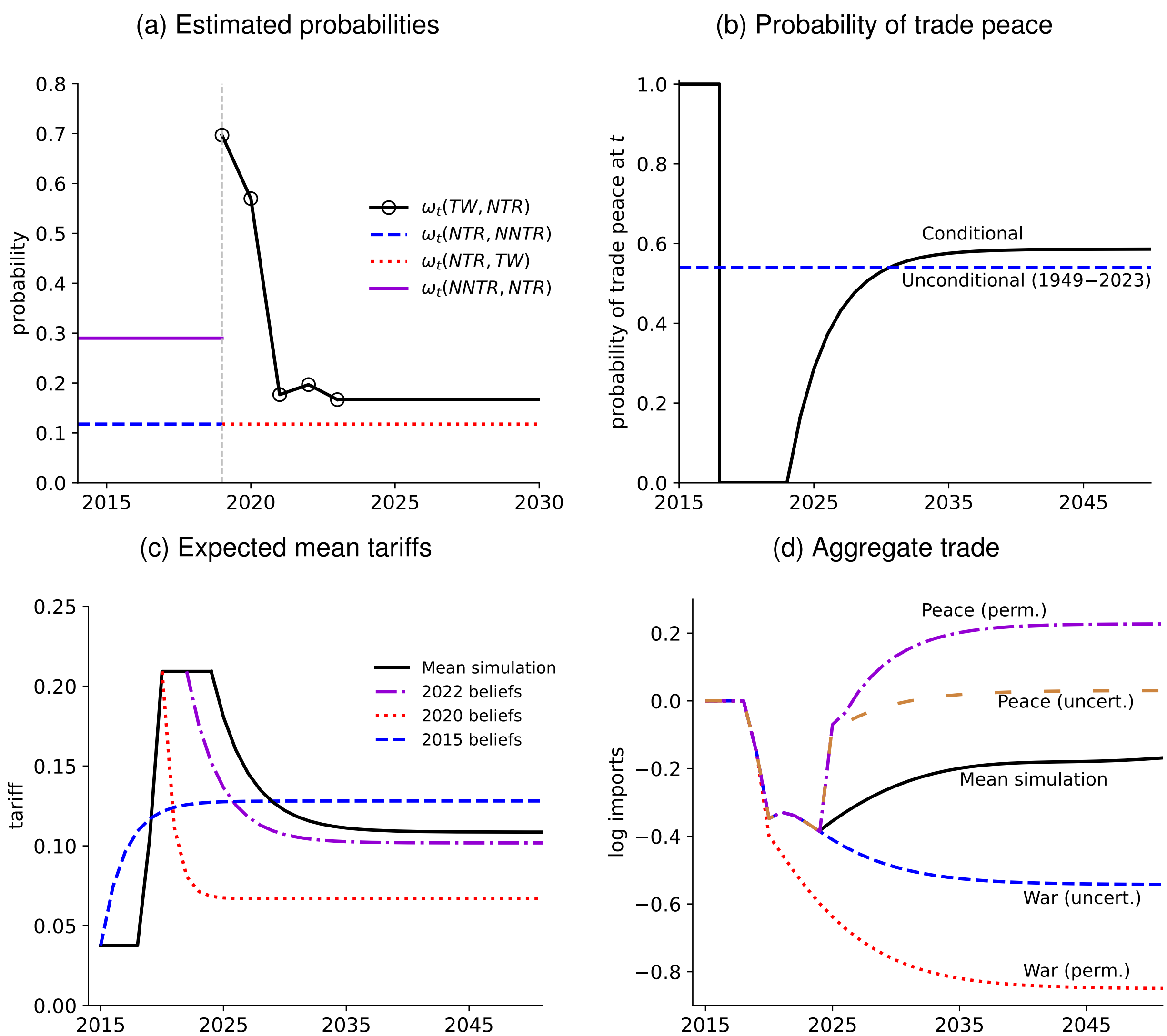
Figure 1: Dynamics of U.S. trade policy and imports from China



Notes: (a) Median and interquartile range of applied tariffs by year. (b) Trade-war gap and NNTR-gap distributions. (c) correlation of trade-war gap and NNTR gap. (d) β_t^{NNTR} and β_t^{TW} from (2). (e) Average China-HS-section fixed effect ($\frac{1}{H}\sum_{h=1}^{H}\delta_{CHN,h,t}$) from (2) with bootstrap confidence interval indicated by shaded area. (f) β_t^{TW} versus NNTR-gap elasticity from Alessandria et al. (2024b) normalized to zero in 1979. Years begin in July and end in June, e.g., 2024 is July 2023–June 2024. Applied tariff calculated at HS6 level as duties collected divided by f.o.b. trade value. Trade-war gap defined as average applied tariff during 2020–2023 minus average applied tariff during 2013–2017. NNTR gap defined as NNTR tariff rate minus average applied tariff during 2013–2017.

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 onwards. 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	Prob. of keeping iceberg cost	0.91	Alessandria et al. (2024b)
$\omega(N,P)$	Prob. of staying in NNTR	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	Exit rate
$\xi_{\gamma(g)}$	High iceberg cost	Varies by sector	Incumbent premium
$\sigma_{\gamma(g)z}$	Productivity dispersion	Varies by sector	CV of log sales
(c) Determined during the trade war			
$\omega(P,N)$	Prob. trade peace to NNTR	11.8%	Δ NNTR-gap elasticity, 2018–2024
$\omega(W,P)_{2019}$	Prob. trade war to peace in 2019	69.7%	Trade-war gap elasticity in 2020
$\omega(W,P)_{2020}$	Prob. trade war to peace in 2020	57.0%	Trade-war gap elasticity in 2021
$\omega(W,P)_{2021}$	Prob. trade war to peace in 2021	17.7%	Trade-war gap elasticity in 2022
$\omega(W,P)_{2022}$	Prob. trade war to peace in 2022	19.7%	Trade-war gap elasticity in 2023
$\omega(W,P)_{2023}$	Prob. trade war to peace in 2023	16.7%	Trade-war gap elasticity in 2024
(d) Implied trade-policy expectations			
τ^E_{2018}	Mean discounted tariff in 2018	11.8%	Tariff data and estimated probabilities
τ^E_{2019}	Mean discounted tariff in 2019	7.1%	Tariff data and estimated probabilities
τ^E_{2020}	Mean discounted tariff in 2020	7.7%	Tariff data and estimated probabilities
τ^E_{2021}	Mean discounted tariff in 2021	12.2%	Tariff data and estimated probabilities
τ^E_{2022}	Mean discounted tariff in 2022	11.7%	Tariff data and estimated probabilities
τ^E_{2023}	Mean discounted tariff in 2023	12.4%	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	3.13	0.14	0.05	6.12	0.84
Textile & clothing	45	10	1.99	1.06	3.17	0.27	0.01	3.41	0.97
Wood & straw	24	13	2.05	1.09	2.79	0.45	0.03	6.45	0.99
Paper & printing	12	17	3.10	1.30	3.43	0.17	0.06	5.95	1.01
Energy & chemicals	19	15	3.23	1.48	2.99	0.39	0.05	8.28	1.12
Rubber & plastic	29	10	2.69	1.08	3.16	0.29	0.01	5.45	0.93
Non-metallic mineral	16	18	2.26	0.85	2.85	0.15	0.07	7.13	0.85
Base metal	12	21	3.96	1.15	3.04	0.13	0.08	8.95	0.96
Calendered metal	29	10	2.48	1.24	2.73	0.54	0.01	7.15	1.06
Other machinery	23	13	3.33	1.54	3.74	0.27	0.03	4.59	1.11
Computer & electronic	48	7	4.82	1.94	3.18	0.48	0.00	5.92	1.28
Electrical equipment	32	10	3.35	1.55	3.27	0.41	0.01	5.74	1.13
Vehicles	23	12	4.07	1.31	3.01	0.35	0.03	8.70	1.03
Furniture & other	59	7	1.76	0.95	3.26	0.29	0.00	2.48	0.95
Non-manufacturing	28	13	2.99	1.25	2.96	0.40	0.03	7.06	1.02

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 expoerters 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(d), are robust to a range of alternative approaches. In Appendix 2 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.

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, Hungary, Mongolia, Romania, Soviet Union,

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, we use a country-product (ig) fixed effect that captures trade relative to the year before the trade war. We use a good-time (gt) fixed effect to control for changes in U.S. demand 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 i-HS sections-t fixed effect. In columns 2 and 3 of Table A1, we show that imposing less restrictive it or more restrictive i-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 exported to the United States in every year of our sample period and 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. Nevertheless, between 2020 and 2023, the elasticity grows by almost 60 percent compared with the corresponding 45-percent growth between 2021 and 2024 in our baseline.

China supply effects and other destinations. In our baseline, we focus on U.S. imports only and use imports from other countries to control for U.S. good-specific demand shocks

[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).

through the gt fixed effects. In order to control for Chinese good-specific supply shocks (i.e., jgt fixed effects) we extend our sample with import data from Chinese exports to all 27 countries of the European Union ("EU-27"), aggregate over all other countries except China.[13] and estimate the analogous equation to 2:

$$\log v_{ijgt} = \sum_{t=2015}^{2024} \left(\beta_t^{NTR} X_g^{NTR} + \beta_t^{TW} X_g^{TW}\right) \mathbb{1}_{\{i=China \,\wedge\, j=US \,\wedge\, t=t'\}} \tag{14}$$
$$+ \delta_{igt} + \delta_{jgt} + \delta_{ijg} + \delta_{ijht} + u_{ijgt},$$

where j indexes the destination country. Column 7 of Table A1 reports the results. The results are almost identical to the baseline indicating that Chinese supply shocks were unrelated to the U.S. trade policy shocks. As a placebo test to further rule out unobserved supply shocks that spuriously correlate with the tariff gaps, we estimate (2) using EU-27 imports as the dependent variable instead of U.S. imports. Column 8 shows there is no significant pattern in response to either of the gaps.

Gap measures. Our baseline trade-war gap, X_g^{TW}, is calculated as the log of 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 log of 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, increase slightly when we use the simple average over HS-10 products to calculate the average applied tariff to China in 2020–23 and 2013–2017. Column 3 shows that our baseline results are very similar when instead of applied tariffs we use the statutory tariff increases obtained from Fajgelbaum et al. (2020) as the trade-war gap.

Finer aggregation. Our baseline level of aggregation of goods is at the 6-digit HS level, the level commonly used in the literature (Handley et al., 2020). Columns 4 and 5 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, although the second quarter of 2024 is based on data through May.

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

[13]We use CIF import values since Eurostat does not report FOB values (and thus exclude controls for shipping costs)

[14]This data has been widely used to study Chinese manufacturing growth between the late 1990s and 2000s (see, for example, Bai et al. (2023)). We thank Dan Lu for sharing the data with us.

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 but yields a higher estimates 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 that more of the trade response is captured by the China year fixed effect. Finally, we consider the case of administration-specific transition probabilities that last for four years with certainty and then change with the electoral cycle. We show that a set of expectations with Trump being more likely to maintain the trade war than Biden, and other future administrations, is inconsistent with the trade dynamics.

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 TableA3. 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 4.7 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 2.3 percent during the Trump administration and rose 1.7 percent during the Biden administration, compared to 4.1% and 4.7%, 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 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

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

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 three values of ρ: (i) zero (random tariff hikes are uncorrelated with actual trade-war tariffs); (ii) one-half (partial correlation); and (iii) one (full correlation). We also analyze a scenario where all goods get a common tariff increase of 17.2%, which is the average changed in applied tariffs between 2018 and 2020.

Figure A3 shows the results. Panel (a) show 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 four 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 three 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 get 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 three 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); (ii) one-half (partial correlation, which is a mix of deepening and broadening); and (iii) one (full correlation, or pure deepening). Again, we also look at a

[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.

common-tariff scenario.

Figure A4 shows the results in the same format as in the previous exercise. Aggregate trade begins to decline more 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 three 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 partial correlation case is in between; the only notable thing here is that the gap elasticity essentially does not change at all.

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 as saying that 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 stastically insigificant decline consistent with anticipation of further tariff increases.

Political cycles. An open question is whether these fluctuations in policy expectations are specific to an administration.[17] In particular, what would happen in a model where changes in the trade-policy transition process are associated with political transitions? Could such a model fit the data? The motivating ideas here are: (i) Trump is hawkish towards China whereas Biden is dovish; (ii) ending the trade war under Biden was perceived as a high-probability event that nevertheless failed to materialize; (iii) the small initial adjustment to the trade-war tariffs was because firms were looking forward to the possibility of better times soon under Biden; and (iv) the additional adjustment under Biden was because the possibility of a second Trump term was getting nearer. Such an approach abstracts from the year to year variation in circumstances that influence these policy decisions.

To answer these questions and make these ideas concrete, we consider a version of our model with three time-invariant transition matrices: (i) the "pre-trade war" matrix Ω_{Pre}, which is the same as that estimated in our baseline model for the pre-trade war period; (ii) the "Trump" matrix Ω^{Trump}, which has a high (75%) chance of starting a trade war and a zero percent

[17] We thank Marc Melitz for suggesting this line of exploration.

chance of ending one; and the (iii) "post-Trump" matrix Ω^{Post}, which has a high (90%) chance of ending a trade war and a low (10%) chance of restarting one. Formally,[18]

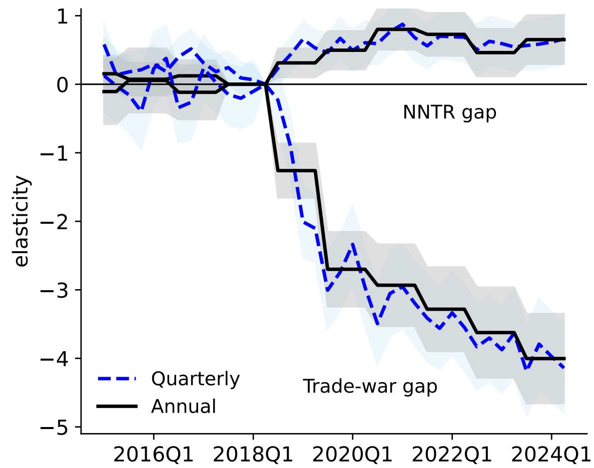
$$\Omega^{Pre} = \begin{bmatrix} 0.71 & 0.29 & 0 \\ 0.88 & 0.12 & 0 \\ 0 & 0 & 1 \end{bmatrix} \quad \Omega^{Trump} = \begin{bmatrix} 1 & 0 & 0 \\ 0 & 0.25 & 0.75 \\ 0 & 0 & 1 \end{bmatrix} \quad \Omega^{Post} = \begin{bmatrix} 1 & 0 & 0 \\ 0 & 0.9 & 0.1 \\ 0 & 0.1 & 0.9 \end{bmatrix} \tag{15}$$

The only time the economy can switch between these matrices is election years: 2016, 2020, 2024, etc. We assume that the election of Trump was a surprise (e.g. the switch to Ω^{Trump} in 2016 was unanticipated) which is consistent with polling and punditry at the time, and that the chance of switching parties in other election years is 50%. Trump can serve at most two terms, and if this happens the economy permanently switches to Ω^{Post} forever. We do two versions of the experiment, one where the trade war is anticipated (the economy switches to Ω^{Trump} in 2016) and another in which it is not (the economy switches to Ω^{Trump} in 2019). [Note that modeling firms' expectations about future electoral cycles requires us to use the perfect-foresight version of the model described at the beginning of this appendix.]

Figure A5 shows the results. The model without anticipation does reasonably well, but the trade-war gap elasticity falls too much at the onset of the trade war and then declines too little afterwards. The model with anticipation does worse, with the trade-war gap elasticity falling too soon and too sharply, and then completely flattening after 2020 (not surprising given our findings about lack of empirical evidence for anticipation). Both political-cycles models exhibit counterfactual increases in aggregate trade after 2020. In our experiments with this model, it is impossible to get both the small initial response to the trade-tariffs (the small trade-war gap elasticity in 2020) and the pace of adjustment in subsequent years (especially the larger elasticity in 2024) without allowing for changes in the intra-regime transition matrices. We interpret this as indicating that firms really did believe that Trump would end the trade war quickly and that Biden became increasingly less likely to do over time—i.e., motivating ideas (i) and (ii) above are false—and that taking these expectations into account is the only way to account for the dynamic response to the trade-war tariffs.

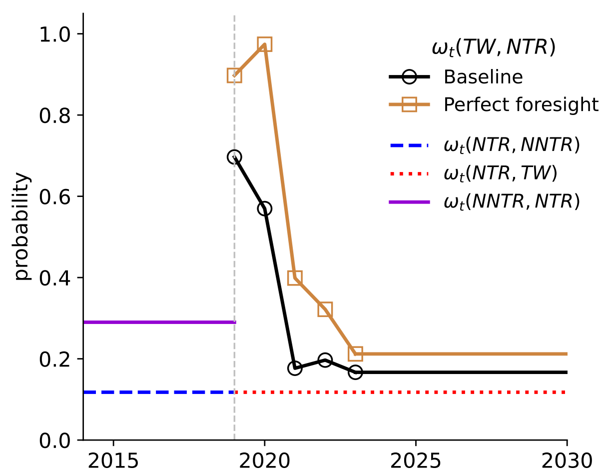
[18] For simplicity and without loss of generality, we treat zero-probability states (trade war pre-Trump and NNTR post-Trump) as absorbing.

Figure A1: Tariff gap elasticities at the quarterly frequency



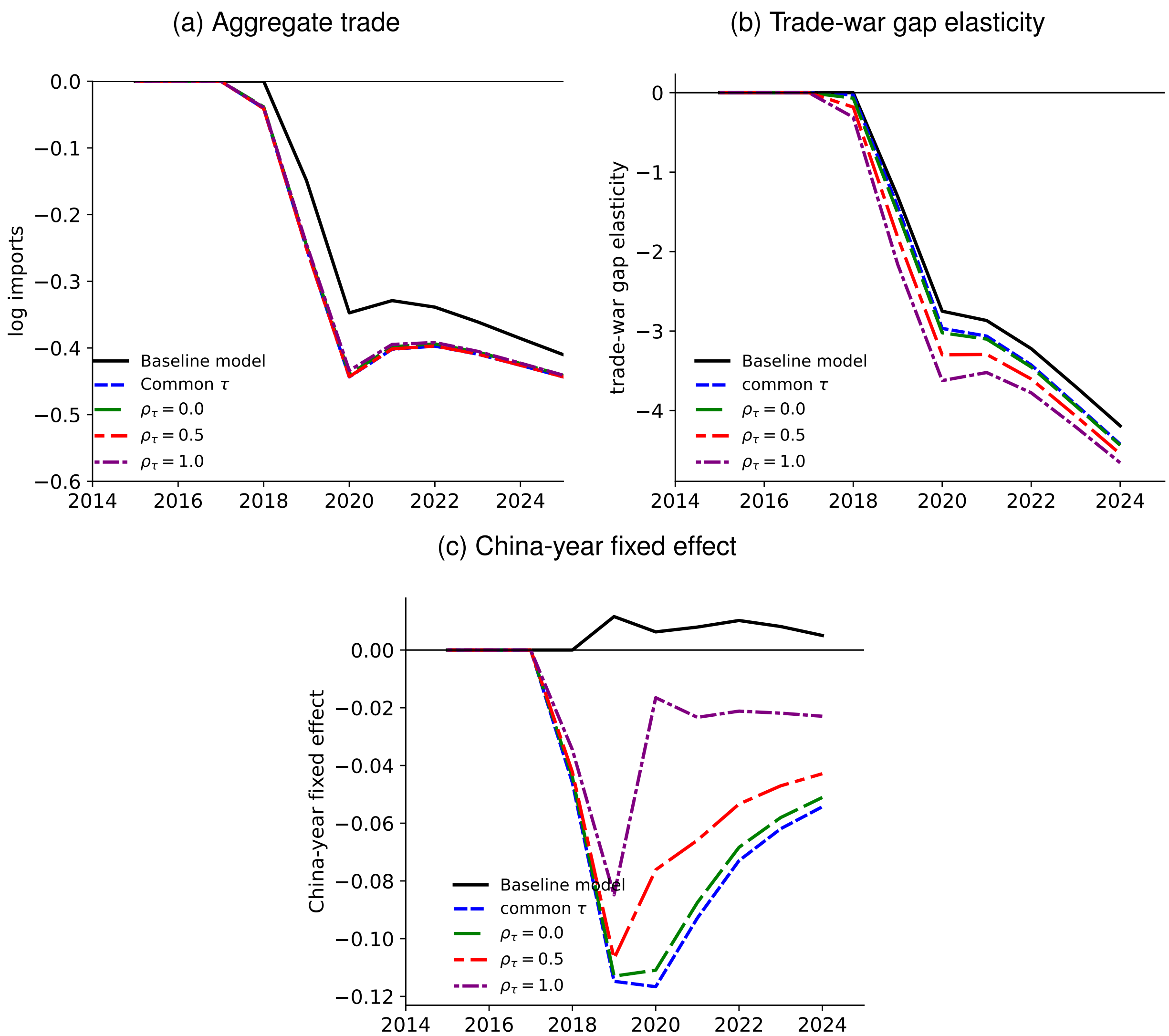
Notes: Figure shows estimates of β^{NNTR} and β^{TW} from (2). Black line: baseline estimates using annual data reported in main text. Blue line: Estimates using data aggregated to quarterly frequency.

Figure A2: Estimated probabilities in perfect-foresight model



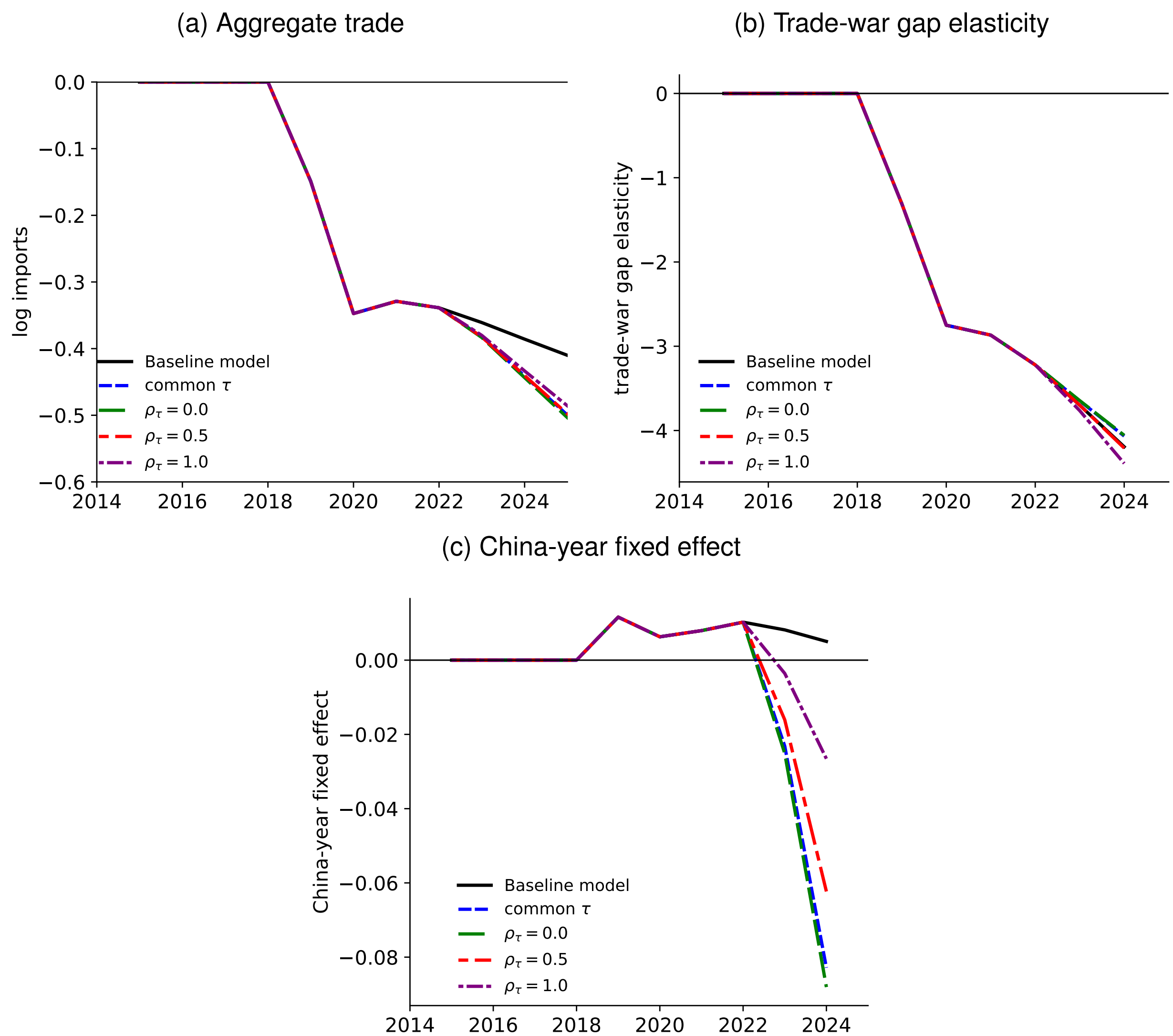
Notes: Figure compares baseline estimates of $\Omega_t(W, P)$ to estimates from perfect-foresight model where firms know entire path of $\{\Omega_t\}_{t=2019}^{\infty}$ when trade war starts.

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



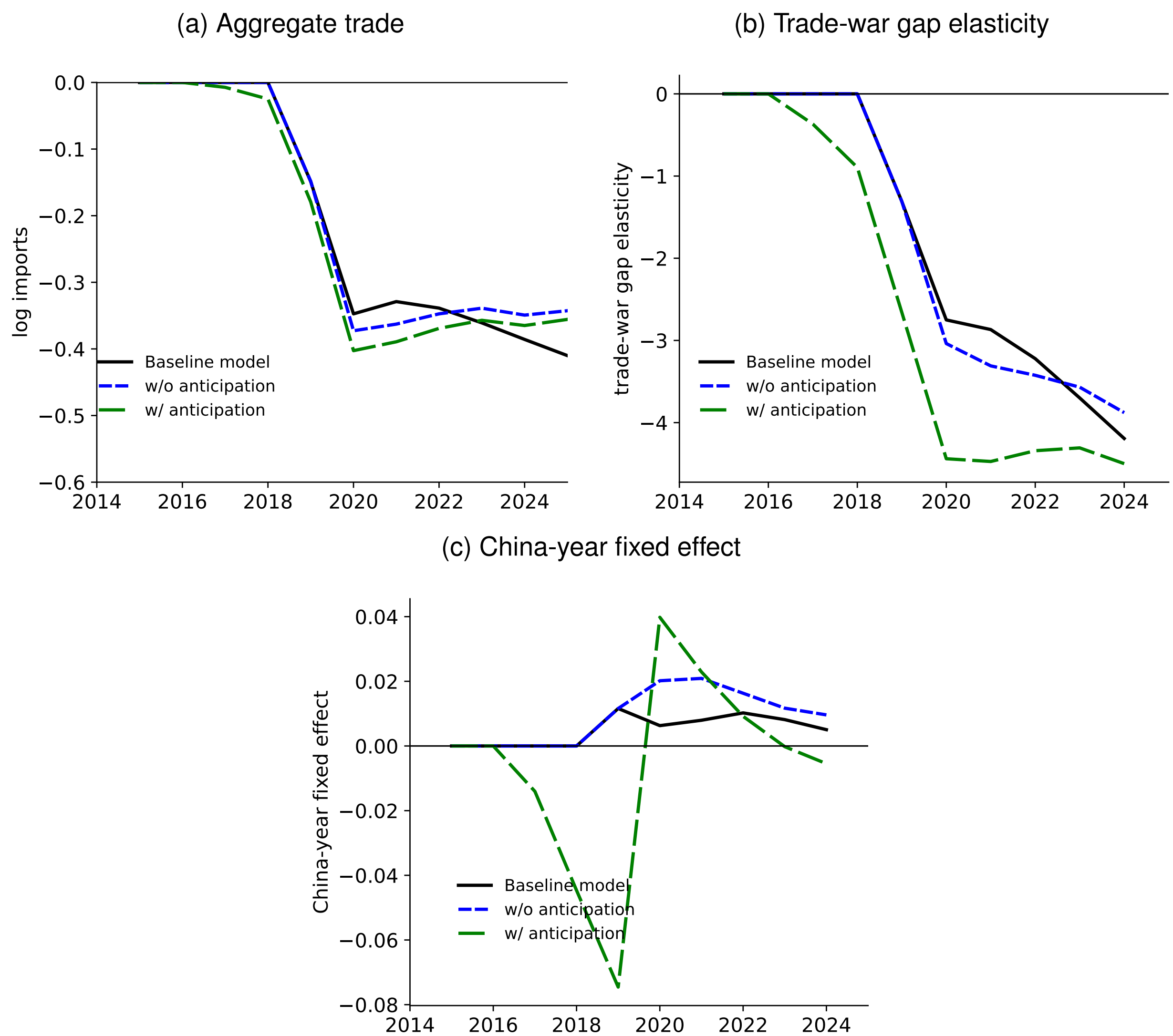
Notes: Figure compares baseline model to models where each good has a chance of a random tariff increase starting in 2016. $\rho_\tau = 0.0$: Model where tariff increases are uncorrelated with actual trade-war tariffs. Common τ: Model where all goods get the same tariff increase of 17.2 percentage points. $\rho_\tau = 0.5$: Model where tariff increases are partially 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 $i = China$.

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. $\rho_\tau = 0.5$: Model where tariff increases are partially 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 $i = China$.

Figure A5: Results for political-cycles model



Notes: Figure compares baseline model to models with political cycles. Assumptions in latter: (i) time-invariant transition matrices ("Trump" where TW is perfectly persistent, and "post-Trump" where TW ends with 90% chance and restarts with 10% chance); (ii) 50% probability of switching matrices after every election year; and (iii) firms have perfect foresight over Ω_t. Without anticipation: political cycles begin after trade war starts. With anticipation: political cycles begin when Trump is elected.

Table A1: Robustness: Gap elasticities

				Alternative Samples			w/Chinese Exports to	
	Baseline	Alternative FEs		Unbalanced	Full	Jan-Dec	US & EU-27	EU-27
Dep. Var. v_{igt}	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}\{^{t'}_{j=\text{CHN}}\}X_g^{\text{TW}}$								
2015	-0.11	-0.21	0.06	-0.05	-0.15	0.03	0.40	-0.55^{*}
	(0.24)	(0.22)	(0.30)	(0.25)	(0.28)	(0.22)	(0.37)	(0.29)
2016	0.06	-0.04	0.19	0.24	0.21	-0.03	0.27	-0.31
	(0.24)	(0.22)	(0.28)	(0.25)	(0.27)	(0.17)	(0.34)	(0.26)
2017	-0.12	-0.12	-0.12	-0.02	0.06	0.00	-0.16	-0.15
	(0.20)	(0.18)	(0.25)	(0.21)	(0.21)	$(.)$	(0.29)	(0.20)
2018	0.00	0.00	0.00	0.00	0.00	-0.38^{**}	0.00	0.00
	—	—	—	—	—	(0.17)	—	—
2019	-1.26^{***}	-1.26^{***}	-1.27^{***}	-1.18^{***}	-1.16^{***}	-2.35^{***}	-1.20^{***}	-0.02
	(0.20)	(0.19)	(0.26)	(0.21)	(0.22)	(0.23)	(0.29)	(0.21)
2020	-2.70^{***}	-2.59^{***}	-2.61^{***}	-2.65^{***}	-2.69^{***}	-2.82^{***}	-2.50^{***}	-0.12
	(0.28)	(0.25)	(0.34)	(0.28)	(0.28)	(0.29)	(0.36)	(0.24)
2021	-2.93^{***}	-2.75^{***}	-3.05^{***}	-2.74^{***}	-2.64^{***}	-3.23^{***}	-3.15^{***}	0.41
	(0.30)	(0.27)	(0.38)	(0.30)	(0.30)	(0.28)	(0.39)	(0.27)
2022	-3.28^{***}	-2.98^{***}	-2.96^{***}	-3.10^{***}	-2.97^{***}	-3.27^{***}	-3.05^{***}	0.02
	(0.31)	(0.28)	(0.39)	(0.32)	(0.32)	(0.30)	(0.43)	(0.30)
2023	-3.62^{***}	-3.29^{***}	-2.96^{***}	-3.53^{***}	-3.44^{***}	-3.78^{***}	-3.72^{***}	0.14
	(0.34)	(0.30)	(0.41)	(0.34)	(0.34)	(0.31)	(0.43)	(0.30)
2024	-4.00^{***}	-3.65^{***}	-3.58^{***}	-3.87^{***}	-3.75^{***}	-4.03^{***}	-3.99^{***}	0.49
	(0.33)	(0.30)	(0.40)	(0.33)	(0.33)	(0.32)	(0.44)	(0.31)
$\mathbb{1}\{^{t'}_{i=\text{CHN}}\}X_g^{\text{NNTR}}$								
2015	0.15	0.22^{**}	0.23	0.16	-0.01	0.15	0.24	-0.14
	(0.13)	(0.11)	(0.15)	(0.14)	(0.15)	(0.12)	(0.19)	(0.18)
2016	0.07	0.13	0.08	0.12	-0.04	0.18^{*}	0.09	-0.16
	(0.12)	(0.10)	(0.13)	(0.13)	(0.14)	(0.09)	(0.16)	(0.13)
2017	0.12	0.11	0.09	0.16	0.12	0.00	0.19	-0.11
	(0.12)	(0.09)	(0.12)	(0.12)	(0.12)	$(.)$	(0.16)	(0.11)
2018	0.00	0.00	0.00	0.00	0.00	0.07	0.00	0.00
	—	—	—	—	—	(0.10)	—	—
2019	0.31^{***}	0.22^{**}	0.37^{***}	0.36^{***}	0.30^{**}	0.44^{***}	0.26^{*}	0.02
	(0.11)	(0.09)	(0.12)	(0.11)	(0.12)	(0.13)	(0.16)	(0.11)
2020	0.50^{***}	0.44^{***}	0.36^{**}	0.64^{***}	0.61^{***}	0.63^{***}	0.48^{**}	-0.05
	(0.14)	(0.12)	(0.16)	(0.16)	(0.16)	(0.15)	(0.19)	(0.13)
2021	0.80^{***}	0.64^{***}	0.59^{***}	0.89^{***}	0.82^{***}	0.61^{***}	0.82^{***}	-0.09
	(0.15)	(0.13)	(0.17)	(0.16)	(0.15)	(0.15)	(0.20)	(0.14)
2022	0.73^{***}	0.50^{***}	0.61^{***}	0.66^{***}	0.64^{***}	0.58^{***}	0.71^{***}	-0.06
	(0.16)	(0.14)	(0.18)	(0.16)	(0.16)	(0.17)	(0.24)	(0.19)
2023	0.46^{***}	0.20	0.31	0.55^{***}	0.47^{***}	0.45^{***}	0.54^{**}	-0.18
	(0.18)	(0.15)	(0.19)	(0.18)	(0.17)	(0.17)	(0.23)	(0.17)
2024	0.65^{***}	0.43^{***}	0.37^{*}	0.70^{***}	0.64^{***}	0.55^{***}	0.83^{***}	-0.23
	(0.18)	(0.15)	(0.19)	(0.18)	(0.18)	(0.19)	(0.23)	(0.16)
log Shipping Cost	-2.54^{***}	-2.52^{***}	-2.59^{***}	-2.55^{***}	-2.55^{***}	-2.52^{***}		
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)		
gt, ig FEs	✓	✓	✓	✓	✓	✓		✓
i-HS Section-t FEs	✓			✓	✓	✓		✓
it FEs		✓						
i-HS2-t FEs			✓					
jgt, igt, jig FEs							✓	
ji-HS Section-t FEs							✓	
N	1,019,765	1,026,607	1,003,059	1,093,068	1,144,746	986,834	125,568	63,040
Adjusted R^2	0.88	0.88	0.88	0.88	0.88	0.88	0.95	0.95

Notes: The table reports estimates of (2). Columns 2 and 3 use less restrictive source-time and more restrictive source-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 includes Chinese exports to an aggregate of the EU-27. Column 8 is a placebo test that uses only EU-27 imports. Standard errors clustered at the ig-level (and ijg level in column 7) are reported in parenthesis.$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A2: Robustness: Gap elasticities

		Alternative Gaps Measures		Good Level Aggregation	
	Baseline	Simple Avg Gaps	Statutory TW Gap	HS-8	HS-10
Dep. Var. v_{igt}	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}\{_{j=\text{CHN}}^{t'}\}X_g^{\text{TW}}$					
2015	-0.11	-0.10	-0.13	0.34	0.34^{*}
	(0.24)	(0.25)	(0.31)	(0.21)	(0.18)
2016	0.06	0.07	-0.11	0.47^{**}	0.52^{***}
	(0.24)	(0.24)	(0.30)	(0.19)	(0.17)
2017	-0.12	-0.09	-0.04	0.24	0.32^{**}
	(0.20)	(0.21)	(0.26)	(0.17)	(0.15)
2018	0.00	0.00	0.00	0.00	0.00
	—	—	—	—	—
2019	-1.26^{***}	-1.31^{***}	-1.34^{***}	-1.29^{***}	-1.16^{***}
	(0.20)	(0.21)	(0.24)	(0.18)	(0.15)
2020	-2.70^{***}	-2.76^{***}	-3.36^{***}	-2.91^{***}	-2.83^{***}
	(0.28)	(0.29)	(0.32)	(0.22)	(0.19)
2021	-2.93^{***}	-2.97^{***}	-3.52^{***}	-3.26^{***}	-3.15^{***}
	(0.30)	(0.32)	(0.38)	(0.24)	(0.21)
2022	-3.28^{***}	-3.33^{***}	-4.04^{***}	-3.27^{***}	-3.21^{***}
	(0.31)	(0.33)	(0.36)	(0.25)	(0.21)
2023	-3.62^{***}	-3.69^{***}	-4.02^{***}	-3.78^{***}	-3.69^{***}
	(0.34)	(0.36)	(0.41)	(0.27)	(0.23)
2024	-4.00^{***}	-4.07^{***}	-4.69^{***}	-3.95^{***}	-3.86^{***}
	(0.33)	(0.35)	(0.40)	(0.27)	(0.23)
$\mathbb{1}\{_{j=\text{CHN}}^{t'}\}X_g^{\text{NNTR}}$					
2015	0.15	0.19	0.15	-0.01	-0.04
	(0.13)	(0.14)	(0.13)	(0.11)	(0.09)
2016	0.07	0.11	0.07	0.00	-0.08
	(0.12)	(0.13)	(0.12)	(0.09)	(0.08)
2017	0.12	0.14	0.12	0.02	0.01
	(0.12)	(0.12)	(0.12)	(0.09)	(0.08)
2018	0.00	0.00	0.00	0.00	0.00
	—	—	—	—	—
2019	0.31^{***}	0.35^{***}	0.30^{***}	0.22^{**}	0.26^{***}
	(0.11)	(0.11)	(0.11)	(0.09)	(0.08)
2020	0.50^{***}	0.57^{***}	0.45^{***}	0.25^{**}	0.27^{***}
	(0.14)	(0.15)	(0.14)	(0.11)	(0.10)
2021	0.80^{***}	0.88^{***}	0.75^{***}	0.50^{***}	0.50^{***}
	(0.15)	(0.16)	(0.15)	(0.12)	(0.10)
2022	0.73^{***}	0.77^{***}	0.67^{***}	0.50^{***}	0.56^{***}
	(0.16)	(0.17)	(0.16)	(0.13)	(0.11)
2023	0.46^{***}	0.50^{***}	0.41^{**}	0.40^{***}	0.47^{***}
	(0.18)	(0.18)	(0.18)	(0.13)	(0.11)
2024	0.65^{***}	0.72^{***}	0.59^{***}	0.55^{***}	0.45^{***}
	(0.18)	(0.19)	(0.18)	(0.14)	(0.12)
log Shipping Cost	-2.54^{***}	-2.54^{***}	-2.54^{***}	-2.52^{***}	-2.52^{***}
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
gt, gt, ig FEs	✓	✓	✓	✓	✓
i-HS Section-t FEs	✓	✓	✓	✓	✓
N	1,019,765	1,019,765	1,019,765	1,242,230	1,750,082
Adjusted R^2	0.88	0.88	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 average NNTR rate, instead of the median—and column 3 uses the simple averages of the pre- and post-war HS-10 tariffs, instead of the weighted average. Columns 4 and 5 define good g as an HS-8 and HS-10 code, respectively. Standard errors clustered at the ig-level are reported in parenthesis.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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

	Baseline		Perfect foresight	
	Trump	Biden	Trump	Biden
Expected duration (years)	1.8	6.0	1.0	4.7
Change in mean discounted tariff (%)	-4.1	4.7	-2.3	1.7
Change in applied tariff (%)	17.2	0.0	17.2	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.