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

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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.

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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.[1] 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.

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

[1] 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).

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),

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

We also include a measure of shipping charges, c_{igt}.[2] 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.

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.[3] 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[4] 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

[2] 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.

[3] 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.

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

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 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.[5] Conversely, in the full-correlation ($\rho_\tau = 1$) scenario,

[5]The small movement in the gap elasticity in this scenario in Panel (b) of the figure is due to the fact that we

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, 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

have a finite number of goods, so we do not get a precisely zero correlation between the random tariffs and the trade-war tariffs.

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.

References

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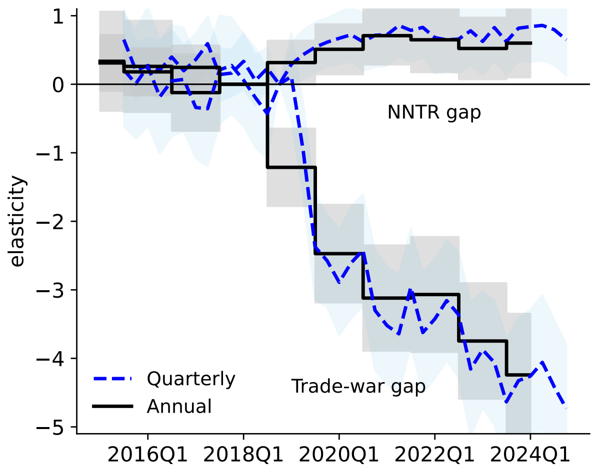
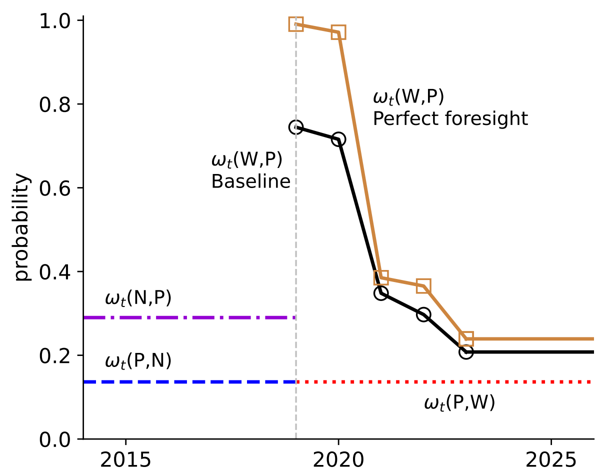
Figure A1: Tariff gap elasticities at the quarterly frequency



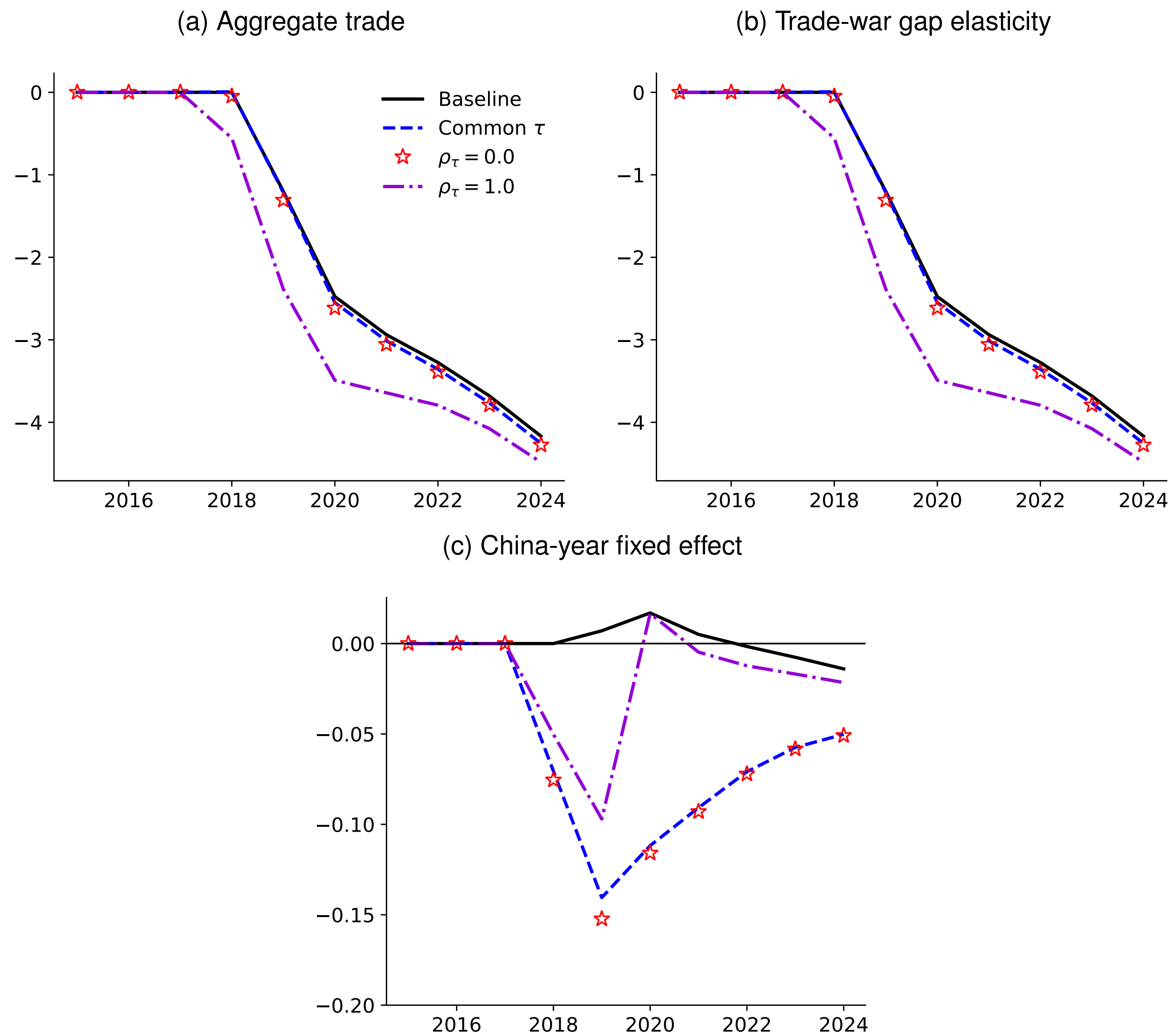
Notes: Figure shows estimates of β^{NNTR} and β^{TW} from (2). Black line: baseline estimates using annual data reported in main text. Blue line: Estimates using data aggregated to quarterly frequency.

Figure A2: Estimated probabilities in perfect-foresight model



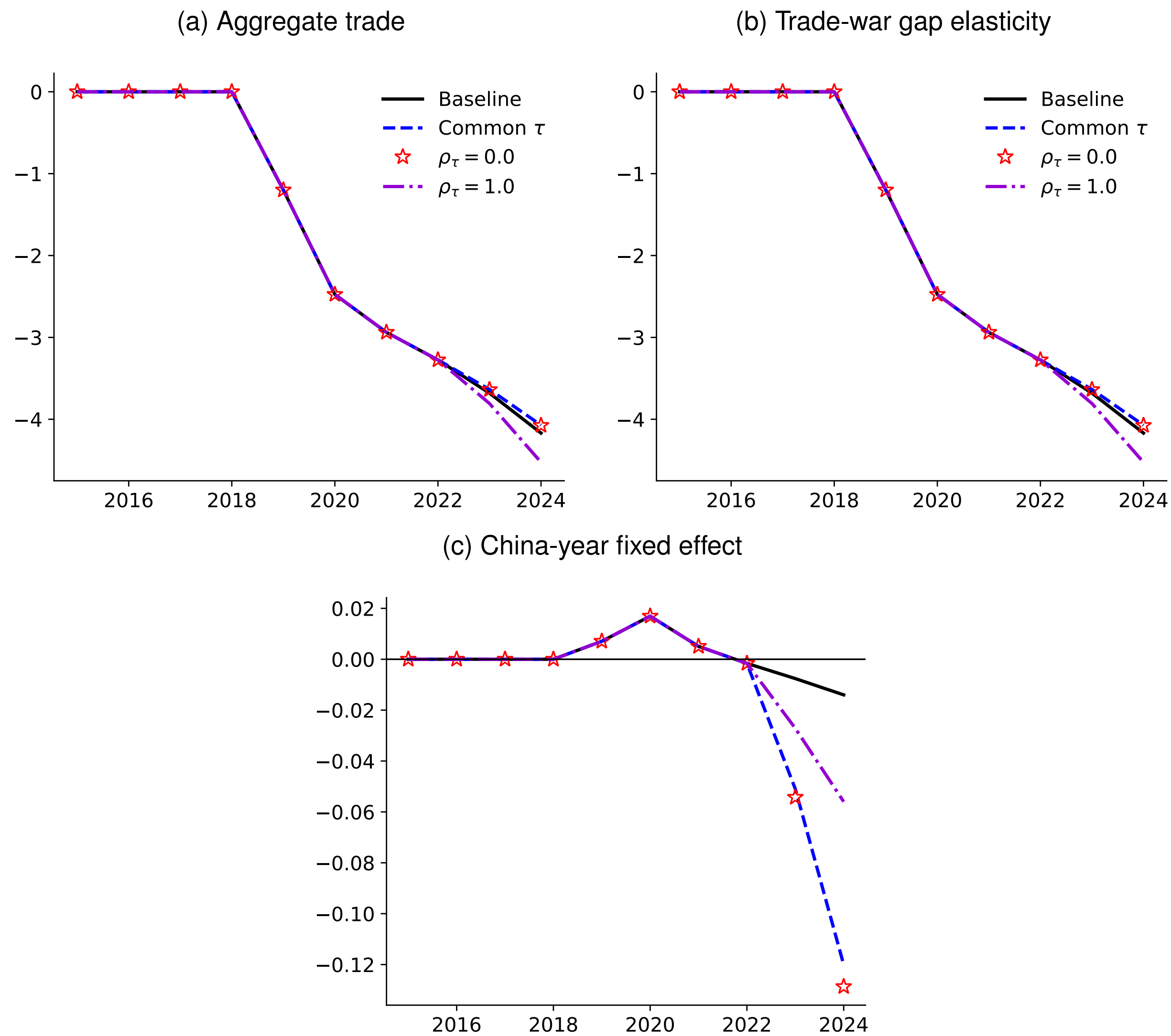
Notes: Figure compares baseline estimates of $\Omega_t(W, P)$ to estimates from perfect-foresight model where firms know entire path of $\{\Omega_t\}_{t=2019}^{\infty}$ when trade war starts.

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

	Baseline	Alternative FEs		Alternative Samples: Unbalanced	Alternative Samples: Full	Alternative Samples: 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)
$\mathbb{1}\{_{j=\text{CHN}}^{t=t'}\}X_g^{\text{TW}}$						
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)
$\mathbb{1}\{_{j=\text{CHN}}^{t=t'}\}X_g^{\text{NNTR}}$						
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.