

# Recovering Credible Trade Elasticities from Incredible Trade Reforms\*

George Alessandria<sup>†</sup>, Shafaat Yar Khan<sup>‡</sup>, Armen Khederlarian<sup>§</sup>,  
Kim J. Ruhl<sup>¶</sup> and Joseph B. Steinberg<sup>||</sup>

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## Abstract

We recover the elasticity of trade in response to an unanticipated, once-and-for-all tariff reform by estimating a dynamic heterogeneous-firm model using data from anticipated, uncertain reforms. Using 1974–2017 U.S. import and tariff data, we show that within-schedule tariff changes are frequent and transitory, while transitions across schedules are rare, persistent, and elicit far larger trade responses. We calibrate the model to U.S. trade with China and Vietnam—the countries that experienced the most substantial and persistent tariff reductions—and jointly estimate the parameters governing tariff expectations and firm-level adjustment dynamics. Simulating an unanticipated, once-and-for-all reform yields a short-run elasticity of about three and a long-run elasticity of about 15. Our results imply that reduced-form estimates based on typical idiosyncratic, transitory tariff variation substantially understate the long-run effects of permanent liberalizations.

**JEL Classifications:** F12, F13, F14

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<sup>†</sup>george.alessandria@rochester.edu, University of Rochester and NBER

<sup>‡</sup>shafaatyar.k@gmail.com, Syracuse University

<sup>§</sup>armen.khederlarian@gmail.com, Hunter College, CUNY

<sup>¶</sup>ruhl2@wisc.edu, University of Wisconsin–Madison and NBER

<sup>||</sup>joseph.steinberg@utoronto.ca, University of Toronto and NBER

# 1 Introduction

The elasticity of trade volumes to trade policy is one of the most important concepts in international economics. It determines how trade reforms shift expenditures between domestic and foreign goods, how they reallocate production and labor across sectors, how they change the terms of trade, and how much tax revenue they generate. It is the key input in quantitative studies that evaluate the aggregate effects of policy changes and other shocks. Yet the literature reports a wide range of trade-elasticity estimates, and this dispersion is not simply noise.<sup>1</sup> The trade elasticity is inherently difficult to identify from reduced-form responses for two reasons. First, trade adjusts gradually, so the measured elasticity depends on the horizon over which it is evaluated. Second, exporting and sourcing decisions are forward-looking, so the response to today’s tariff change depends on beliefs about future policy.

For many applications, the relevant benchmark is the response to a *canonical reform*: an unanticipated, once-and-for-all change in tariffs. But policy changes observed in the data are never canonical: they are often anticipated to some extent, and there is always a chance they will be modified or reversed in the future. Consequently, reduced-form estimates are uninformative about how trade would respond to a truly permanent, unexpected reform. We recover the elasticity of trade to a canonical reform by estimating a structural model on data from two unique non-canonical episodes—the United States granting Normal Trade Relations (NTR) to China and Vietnam—where policy expectations can be cleanly modeled as time-varying probabilities of moving between pre-existing tariff regimes. In the estimated model, simulating a canonical trade liberalization generates a short-run trade elasticity of about three and a long-run trade elasticity of about 15. Adjustment is slow: one-half of the distance between the short- and long-run responses closes within five years.

Our approach is motivated by the dynamic trade literature, which emphasizes that export participation is a forward-looking decision because it involves up-front investments ([Baldwin and Krugman, 1989](#); [Roberts and Tybout, 1997](#); [Das et al., 2007](#)) and back-loaded returns

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<sup>1</sup>[Hillberry and Hummels \(2013\)](#) provide a survey of trade elasticity estimates that range from 1 to 35. [Head and Mayer \(2014\)](#) review 435 elasticities from 32 papers: they obtain a median estimate of 5.03 with a standard deviation of 9.3.

(Ruhl and Willis, 2017; Alessandria et al., 2021). These features imply that trade responds gradually to policy changes and depends on expectations about future tariffs as well as current tariffs (Ruhl, 2011; Handley and Limão, 2015). One important corollary is that the same observed tariff change can generate very different trade outcomes. The core challenge, then, is to disentangle the lagged effects of past reforms from the effects of expectations about future reforms. Our approach, which builds on Alessandria et al. (2025a), is to measure the trade elasticity by simulating a canonical reform in an estimated structural model.

We begin by documenting tariff dynamics and reduced-form trade responses using US import data for 1974–2017. Throughout this period, the United States maintained two main tariff schedules: Non-Normal Trade Relations (NNTR) and Normal Trade Relations (NTR). The NNTR schedule was largely set by the 1930 Smoot-Hawley Tariff Act and is considered exogenous to any specific trading partner (Handley and Limão, 2017; Pierce and Schott, 2016). The NTR schedule evolved gradually through multilateral negotiations that did not involve China or Vietnam, making it largely exogenous to them as well. Other US tariff schedules are the result of Preferential Trade Agreements (PTAs) and the Unilateral Trade Preference Program (UTPP). This structure implies that there are two kinds of tariff changes: (i) when a country transitions from one schedule to another, leading to simultaneous tariff changes across many goods; and (ii) when a schedule itself changes, generating good-specific tariff variation common across the countries trading under that schedule.

We compare the two types of tariff changes in terms of the dynamics of tariffs themselves and in terms of their effect on trade. Schedule transitions are rare but large and highly persistent, and are associated with much larger long-run responses than typical estimates in the literature. Within-schedule tariff changes are common but small and transitory outside of the GATT rounds, and generate low long-run responses that are similar to typical estimates. We also find that entering or exiting PTAs or the UTPP have higher short-run trade elasticities than other tariff changes, consistent with these switches being more predictable and therefore more anticipated.<sup>2</sup>

We then focus on the two most consequential transitions between the NNTR and NTR

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<sup>2</sup>PTAs are the product of years of public negotiations and involve a gradual phase-out of tariffs. Countries typically leave the UTPP when they cross predetermined income per capita thresholds, making these departures predictable.

schedules: China and Vietnam. China gained conditional NTR in 1980 (subject to annual renewal) and later received permanent NTR (PNTR) after joining the World Trade Organization (WTO) in 2001. Vietnam followed the same sequence, gaining conditional NTR in 2002 and PNTR upon WTO accession in 2007. Ex post, these liberalizations were the largest and most persistent in US trade history. Ex ante, there was substantial uncertainty about whether these reforms would occur and how durable they would be. Both countries were subject to a complete trade embargo prior to gaining access to the US market at NNTR tariff rates, which provides unusually clean initial conditions and allows us to study the entire transition from autarky to near-free trade. Moreover, the transition from embargo to PNTR took much longer for China (29 years) than Vietnam (12 years), which suggests that while the two countries experienced virtually identical observed changes in tariffs, there may have been material differences in policy expectations.

The trade responses to the China and Vietnam liberalizations share a common feature: cumulative import growth is roughly 10–11 times the cumulative change in tariffs, which is about 75 percent larger than the average response to a tariff-schedule switch. At the same time, the two transition paths differ markedly in ways that are informative about expectations. Vietnam exhibits a larger short-run response and a faster convergence to its long-run level than China. We interpret these differences as evidence that Vietnam’s NTR access was more anticipated and viewed as more credible early on, whereas China’s reform gained credibility more slowly. This interpretation is consistent with the historical context. China was one of the first non-market countries to gain NTR status and its relationship with the United States was fraught with political tension. Moreover, the trade agreement to grant NTR to the Soviet Union was blocked by Congress with the Jackson-Vanik amendment in the 1974 Trade Act several years earlier. Vietnam, by contrast, was one of the last countries to gain NTR status, and many of the Cold War concerns of the 1970s that drove uncertainty around trade policy had faded following the fall of the Soviet Union.

In the second part of the paper, we build on [Alessandria et al. \(2025a\)](#) to recover the canonical trade-elasticity dynamics by estimating a model in which firms make forward-looking export decisions under stochastic trade policy and then simulating a deterministic liberalization. The model has two key ingredients. First, firms face sunk costs of entering

export markets, implying that export participation depends on the entire expected path of trade policy. Second, an exporter’s sales capacity grows stochastically with tenure, allowing the model to match the speed of exporter growth and, in turn, the gradual aggregate adjustment of trade. These “technological” parameters, which are typically disciplined using firm-level data on entry, exit, and growth over the course of exporter life cycles (see, e.g., [Alessandria et al., 2021](#)), determine how trade responds to a canonical reform.

The central identification challenge is that the firm-level moments themselves are generated in an environment with policy risk, so they cannot be cleanly separated from beliefs about the tariff process. We address this by jointly estimating the parameters governing firm adjustment dynamics and the stochastic policy process to match both (i) firm-level moments and (ii) the time path of aggregate trade. The institutional structure of U.S. policy toward China and Vietnam is an important part of what makes this approach feasible: we can represent the policy process as a time-varying probability of moving between the NNTR and NTR tariff schedules.

We identify the time path of regime-transition probabilities using variation in NNTR-gap elasticities. The NNTR gap is the difference between a good’s NNTR tariff and its NTR tariff, and the elasticity of trade to this gap measures the differential import response across goods with higher versus lower gaps. When a country trades under NNTR, a rise in the probability of obtaining NTR increases expected profitability more for high-gap goods than for low-gap goods, pushing the gap elasticity up. When a country trades under NTR, a rise in the probability of reversion to NNTR harms high-gap goods more, pulling the gap elasticity down.

The implied transition probabilities for the two countries are similar at the start and end of the sample but differ importantly around the transitions to NTR. Early on, the estimated probability of gaining NTR access is low in both cases (roughly 5–15 percent). In Vietnam, the probability rises noticeably in the years leading up to NTR, consistent with anticipation, whereas it is comparatively stable for China. After NTR is granted, the probability of reversion falls immediately for Vietnam but temporarily rises for China before declining. Even after WTO accession, the estimated probability of losing NTR is low but not zero in both countries. Taken together, these estimates imply that neither episode was perceived as

an unanticipated, once-and-for-all liberalization; credibility evolved over time, and Vietnam featured a meaningful anticipatory component.

We also find similarities and differences in the technological parameters across the two countries. For example, the cost of starting to export is larger than the cost of continuing in both countries, but both costs are larger in Vietnam than in China, reflecting a lower level of export participation. The potential increase in export capacity that can occur over an exporter’s life cycle is larger in Vietnam as well, reflecting a greater degree of dispersion in export sales between entrants and incumbents.

We use the calibrated model to simulate a canonical reform: starting from a steady state with NNTR tariffs and no chance of switching, we move unexpectedly to NTR with no chance of reversal and trace the full transition to the new steady state. In these simulations, the long-run trade elasticity is about 15, and adjustment is gradual, with roughly half of the remaining distance to the long-run trade level closing every three to five years. Despite the differences in technological parameters, the model implies remarkably similar canonical adjustment dynamics across the two countries.

Our results imply that reduced-form estimates of the long-run trade elasticity are biased downward. Our empirical estimates for China and Vietnam are biased downward by about 25 percent and the biases for other reforms are much larger. The primary reason is that, under the estimated tariff process, the change in the permanent component of trade policy—what we call the discounted expected tariff—is smaller than the observed change in applied tariffs. When we rescale reduced-form responses using this discounted-expected-tariff measure, the implied long-run elasticity for China and Vietnam is also close to 15.

## 2 Relation to the literature

Our primary contribution is to estimate the canonical trade elasticity using a dynamic structural model with stochastic trade policy. While there is a vast literature measuring how trade responds to changes in trade policy, previous studies in this literature either use reduced-form empirical methods or static structural models.<sup>3</sup> We argue that neither of these approaches can recover the canonical trade elasticity.

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<sup>3</sup>There are several recent estimations of the trade elasticity using static trade models such as [Brancaccio et al. \(2020\)](#) and [Fieler and Eaton \(2025\)](#).

A key challenge in measuring how trade responds to changes in policy is the most-favored-nation principle, which implies that there is a dearth of cross-country variation in tariffs that can be used to identify these responses. Thus, several papers have leveraged tariff variation from PTAs. [Baier and Bergstrand \(2007\)](#) use a panel of PTAs to identify the dynamics of trade growth that start prior to the signing of the PTA and stretch 10-15 years after the PTA. [Kehoe and Ruhl \(2013\)](#), [Baier et al. \(2014\)](#), and [Alessandria and Avila \(2023\)](#) show that trade agreements lead to large increases in the extensive margin of trade. Focusing more narrowly on NAFTA, [Khan and Khederlarian \(2021\)](#) estimate a short-run elasticity of 2.7 and a seven-year response of close to nine for trade with Mexico.<sup>4</sup>

A potential concern with this approach is that the path of tariffs may be endogenous. While we do not think this is an issue with China and Vietnam, it could be an issue in general. To address this endogeneity, [Boehm et al. \(2023\)](#) focus on the effects of within-NTR tariff changes on minor trading partners, which are unlikely to play a material role in determining countries' strategies in setting NTR tariff schedules. Their trade-elasticity estimates are much lower than ours: less than one in the short run and less than two in the long run. However, when we focus on the same within-NTR tariff changes, we obtain similarly low estimates. This highlights an important external validity concern: estimates from small, transitory country-specific tariff changes within the NTR schedule are likely to be poor predictors of the effects of unanticipated, large, and persistent reforms like PTA formation, NTR access grants, or GATT rounds.

Numerous papers estimate the dynamic response of trade to changes in the exchange rate or other relative prices at the aggregate- or industry-level (e.g. [Houthakker and Magee, 1969](#); [Gallaway et al., 2003](#)). Much of this work also finds a delayed response, although these estimates tend to be much lower than those for tariffs. For example, [Alessandria and Choi \(2021\)](#) use US net trade flows, relative prices, and expenditures to estimate a quarterly short-run elasticity of 0.2 and a long-run elasticity of 1.1. They also show that purely empirical approaches that do not control for the source of the shock can substantially

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<sup>4</sup>[Romalis \(2007\)](#) uses tariff variation from NAFTA to estimate a trade elasticity of substitution between 6.2 and 10.9, based on a ten year change. Similarly, [Besedes et al. \(2020\)](#) find delayed effects of import growth in Mexico and Canada. [Caliendo and Parro \(2014\)](#), also studying NAFTA, estimate a trade elasticity of 4.5 for Mexico.

bias these estimates downward. Our paper also relates to a growing literature on the effects of future trade policy on the current level of trade, reviewed by [Handley and Limão \(2022\)](#). This literature attributes some trade growth to changes in the expected persistence of a trade policy, but largely abstracts from measuring how these changes bias estimates of the trade elasticity.

More broadly, our paper relates to earlier work that uses dynamic trade models and firm-level data on export participation to estimate the structure of trade costs ([Roberts and Tybout, 1997](#); [Das et al., 2007](#); [Alessandria et al., 2021](#); [Steinberg, 2023](#); [Morales et al., 2019](#)). Our contribution to this literature is to recognize that these data are generated from an environment in which trade policy is stochastic, and that trade costs must be jointly estimated alongside the policy process itself. We are the first to attempt such an estimation.<sup>5</sup>

### 3 Empirical evidence

We begin by documenting several facts about the dynamics of trade policy. We focus on the United States, owing to the availability of high-quality data over a long period. We emphasize that unanticipated, once-and-for-all trade reforms—the canonical reform studied in most models—do not exist in the data. Most tariff changes used in the estimation of trade elasticities are highly transitory, and more persistent reforms are often characterized by anticipation due to phase-outs or long negotiation periods. We identify two particular tariff changes that were extremely large, highly persistent ex-post, and have several other features that make them well-suited to use in our structural approach to recovering canonical elasticities.

#### 3.1 Overview and history

The United States has a unique tariff structure that creates tariff variation that facilitates the estimation of trade elasticities. Imports into the US generally enter under one of four tariff regimes: Normal Trade Relations (NTR, or Column 1); Non-Normal Trade Relations (NNTR, or Column 2); the Unilateral Trade Preference Program (UTPP); and Preferential

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<sup>5</sup>In [Alessandria et al. \(2025a\)](#) and [Alessandria et al. \(2025b\)](#), we estimated the technological parameters that govern firm-level trade dynamics separately from the aggregate policy process, in a steady state rather than along the transition.

Trade Agreements (PTA).<sup>6</sup>

Most US imports fall under the NNTR and NTR regimes. The NNTR tariff schedule is largely determined by the Smoot-Hawley Act of 1930 and is often treated as a source of exogenous variation (Pierce and Schott, 2016; Handley and Limão, 2017). The NTR schedule, formerly the Most Favored Nation schedule, has been adjusted over time through multilateral GATT rounds involving successively larger groups of developed countries. Three major GATT rounds—Kennedy, Tokyo, and Uruguay—had large impacts on tariffs, involved long negotiations, and were implemented in phase-out periods of 5–8 years. Prior to the Trade Agreements Extension Act of 1951, trade policy followed the most-favored-nation principle, and all US trading partners, save those for which the United States did not trade, received the NTR rate.<sup>7</sup> With the 1951 Trade Act, all non-market economies, except Yugoslavia, were transitioned from the NTR to the NNTR schedule. Of the 29 countries, including successor countries, subject to NNTR under the 1951 Trade Act or subsequent legislation, only North Korea and Cuba still have NNTR status at the end of our sample, although trade with both countries is subject to a complete embargo.

The Trade Act of 1974 established the UTPP regime through the General System of Preferences (GSP). This program provided tariff-free access to the United States for some goods from less-developed countries. Countries graduate from the program upon reaching development or market-share thresholds, acceding to other trade agreements, and other circumstances.<sup>8,9</sup> The program has lapsed several times, transitioning all participating countries to other regimes. Another key provision of the 1974 Trade Act was the Jackson-Vanik Amendment, which created a path for non-market economies to gain access to NTR rates subject to Congressional approval and annual renewal. PTAs came into being in 1985, with the most notable being the 1989 Canada-US Free Trade Agreement and the 1994 North American Free Trade Agreement (NAFTA). By 2017, the last year in our sample, the United States had 20 PTAs. PTAs often involve domestic content requirements, leading some goods

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<sup>6</sup>For a summary of US trade policy regimes see [CRS Report for Congress \(2005\)](#).

<sup>7</sup>Under the Trading with the Enemy Act of 1917, the United States imposed a trade embargo on China and North Korea in December of 1950.

<sup>8</sup>For instance, former non-market economies in the European Union lost access to GSP rates when they joined the European Union.

<sup>9</sup>While GSP is the best-known temporary trade program, there are several other programs, which we describe in the appendix.

to be imported under both PTA and NTR rates during the same year.<sup>10</sup>

## 3.2 Data

We use annual customs data from the US Census Bureau from 1974 to 2017.<sup>11</sup> The data for 1974–1988 are from the National Archives and Records Administration, while the data for the remaining years are from [Schott \(2021\)](#). To study trade over such a long horizon, we aggregate the data to the SITC 5-digit level.<sup>12</sup> To account for changes in country definitions, particularly with the fall of the Soviet Union and its satellites, we merge countries that at some point in our sample were united, e.g., the Czech Republic and Slovakia after 1989, and East and West Germany before 1990.

Our unit of observation is the triplet  $jgt$ , where  $j$  denotes the exporting country,  $g$  denotes an SITC 5-digit good, and  $t$  denotes the year. Our main variables of interest are the log FOB import value, denoted by  $v_{jgt}$ , and the applied tariff rate, denoted by  $\tau_{jgt}$ . We compute the latter as the log of one plus the ratio of total duty charges to the FOB import value. To classify triplets into different statutory tariff regimes, we use information on the rate of provision and country sub-code. The rate of provision and the country sub-code indicate the type of duty and special trade agreement under which a good from a country is imported, respectively. As our data features imports of the same good-country under different rates of provisions and country sub-codes, we assign goods to regimes if more than 50 percent of the value of the triplet is imported under it.<sup>13</sup>

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<sup>10</sup>See [Head et al. \(2024\)](#) for a discussion of this issue in the context of NAFTA’s rules of origin for the automotive sector.

<sup>11</sup>1974 is the first year that applied tariffs are available digitally from the National Archives. We end most of our analysis in 2017 to avoid dealing with the complications stemming from tariffs imposed during the 1st and 2nd Trump administrations, although we show some aggregated series for tariffs and trade through 2023 for completeness. The Trump tariffs fall outside of the schedules that had been set legislatively by Congress and do not fit into our “within-regime vs. across-regime” classification system. In [Alessandria et al. \(2025b\)](#) we study the evolution of trade and trade policy expectations in the window around the first US-China trade war that started in 2018.

<sup>12</sup>A challenge to analyzing trade over a long time period is the classification of goods into tariff lines changed in 1989, when the United States switched from the 7-digit TS-USA classification to the 8-digit Harmonized System (HS). Our approach is to use data aggregated to the 5-digit Standard International Trade Classification (SITC) level, for which concordances with the TS-USA and HS schedules are both readily available. The concordance between the TS-USA schedule and the SITC schedule is from [Feenstra \(1996\)](#) and the concordances with the HS schedule and its successive revisions are from [United Nations Trade Statistics \(2017\)](#). Other papers that use this product classification are, for instance, [Broda and Weinstein \(2006\)](#) and [Kehoe and Ruhl \(2013\)](#).

<sup>13</sup>We describe our classification scheme in detail with examples in the appendix.

Our final sample, summarized in Table 1, is an unbalanced panel with 44 years, 163 countries, 2,032 goods, and 2,105,521 observations. The median tariff is about 1.7 percent but this hides substantial differences across regimes. In the NTR regime, the median tariff is about 3 percent, while in the NNTR regime, it is 35 percent. In the other regimes, the median tariff is very close to zero. There is much dispersion in the NNTR and NTR states, but the dispersion in tariffs in the NTR state appears to be driven by some extreme outliers as when we winsorize at the 0.01 percent level it falls by more than half.

Figure 1 plots some features of US imports and tariffs by tariff regime. Panel (a) presents the share of US non-oil imports, by current regime of the importer, relative to the share in the NTR regime. Panel (b) presents the path of tariffs for those same countries.<sup>14</sup> In 1974, nearly all US imports were under NTR. In 1976, with the introduction of the GSP program, there is a discrete jump in the share of countries that gain UTPP and its associated zero tariffs. In 1989 and 1995, the PTA share jumps as US PTAs with Canada and Mexico takes effect. The share of trade with NNTR countries remains near zero throughout, and there are no NNTR imports after 2005.<sup>15</sup>

Panel (c) plots imports coming from countries that were initially in the NNTR and UTPP regimes in the 1970s relative to NTR imports. Imports from initially-UTPP countries rise from two percent of NTR imports in 1976 to almost ten percent in 2017, while imports from initially-NNTR countries rise from zero to nearly 25 percent. Panel (d) shows the decline in tariffs for these groups. Tariffs for initially-NNTR countries started substantially above NTR rates and then fell in several discrete steps, resulting from a country or set of countries being transitioned to NTR or UTPP. The rise in trade with countries that transitioned away from NNTR is a key component of US import growth that we seek to capture.

### 3.3 Tariff dynamics across vs. within regimes

We begin our empirical analysis by comparing the properties of tariff changes across versus within regimes. Table 2 reports the annual persistence of regimes at the country ( $j$ ) and country-product ( $jj$ ) levels. All four regimes are highly persistent, but we observe multiple

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<sup>14</sup>The statutory NNTR rates are essentially constant over time. The movements shown in the figure are driven by changes in the composition of goods that are imported under that regime.

<sup>15</sup>In 2022, Belarus and Russia were returned to the NNTR schedule, but our sample ends in 2017.

transitions away from each— no regime should be viewed as permanent.<sup>16</sup> Table 3 reports summary statistics for tariff changes. Most tariff changes are small, with a one-year mean of  $-0.17$  percentage points and a five-year mean of  $-0.74$  percentage points. However, tariff changes are much larger when we condition on regime changes. For example, the mean absolute tariff change during a transition into or out of NNTR is almost 30 percentage points.

Tariff changes involving a regime change are much more persistent than within-regime tariff changes. To illustrate this, we measure the autocorrelation of tariff changes by estimating a local projection of the horizon- $h$  tariff change on the initial tariff change following [Boehm et al. \(2023\)](#), but allow for different responses for changes across versus within regimes:

$$\begin{aligned} \Delta_h \tau_{jgt} = & \beta_h^{\tau, \text{within}} \Delta_0 \tau_{jgt} \mathbb{1}\{\text{regime}_{jgt} = \text{regime}_{jgt-1}\} \\ & + \beta_h^{\tau, \text{across}} \Delta_0 \tau_{jgt} \mathbb{1}\{\text{regime}_{jgt} \neq \text{regime}_{jgt-1}\} + \mathbf{Z}_{jgt} + \delta_{jt} + \delta_{gt} + u_{jgt}, \end{aligned} \quad (1)$$

where  $\mathbf{Z}_{jgt}$  is a vector that includes controls for pre-trends in tariff changes which we allow to vary for across and within regime changes, that is, we include  $\mathbb{1}\{\text{regime}_{jgt} = \text{regime}_{jgt-1}\} \Delta_{-1} \tau_{jgt}$  and  $\mathbb{1}\{\text{regime}_{jgt} \neq \text{regime}_{jgt-1}\} \Delta_{-1} \tau_{jgt}$ . The set of fixed effects is standard. In this setting,  $\delta_{jt}$  captures common tariff changes within a country, absorbing the average effects of regime changes, and  $\delta_{gt}$  captures common tariff changes across countries, absorbing tariff cuts resulting from GATT rounds and other multilateral negotiations.<sup>17</sup>

Figure 2(a) plots the paths of  $\beta_h^{\tau, \text{within}}$  and  $\beta_h^{\tau, \text{across}}$  for horizons  $h = [-5, 10]$ , along with estimates from a pooled specification without the interaction terms. Tariff changes across regimes are significantly more autocorrelated than within-regime changes. The within-regime autocorrelation is very similar to the pooled autocorrelation because the sample mostly consists of within-regime changes, which implies that the pooled estimates are poor predictors of the dynamics of across-regime tariff changes.

<sup>16</sup>If our data allowed us to go back further in time, we would observe more transitions. For example, starting in 1949 would add 20 more country-level transitions from NTR to NNTR and at least two from NNTR to NTR (Yugoslavia and Poland).

<sup>17</sup>In the appendix, we show that excluding these fixed effects magnifies the differences in tariff dynamics across versus within-regimes.

### 3.4 Case studies: China and Vietnam NTR access

We now describe the dynamics of US trade policy towards China and Vietnam, the case studies in our quantitative analysis. Figure 3(a) shows how tariffs on China and Vietnam changed over time by plotting the mean and interquartile range of the ratio of the NTR rate to the two countries' applied rates,  $\tilde{\tau}_{jgt} = (1 + \tau_{gt}^{\text{NTR}})/(1 + \tau_{jgt})$ . This measure is bounded between zero, during the embargo, and one, after NTR status is gained. In both countries, we see a long embargo followed by substantial tariff dispersion during the NNTR regime, which then subsides upon gaining NTR access.<sup>18</sup> When scaled this way, these two NNTR-to-NTR transitions look very similar to once-and-for-all trade liberalizations.

In fact, these two reforms were the most persistent in US trade history. To illustrate this, we estimate their autocorrelations using the following specification,

$$\begin{aligned} \Delta_h \tau_{jgt} = & \beta_h^{\tau, \text{CN}} \Delta_0 \tau_{jgt} \mathbb{1}\{j = \text{China}, t = 1980\} + \beta_h^{\tau, \text{VN}} \Delta_0 \tau_{jgt} \mathbb{1}\{j = \text{Vietnam}, t = 2002\} \\ & + \beta_h^{\tau, \text{other}} \Delta_0 \tau_{jgt} \mathbb{1}\{\text{other } jt\} + \mathbf{Z}_{jgt} + \delta_{jt} + \delta_{gt} + u_{jgt}. \end{aligned} \quad (2)$$

Figure 3(b) shows that the changes in tariffs on China and Vietnam that occurred when these countries gained NTR status were much more persistent than the typical across-regime tariff change; statistically, we cannot reject the hypothesis that there are no further changes in the next ten years for Vietnam, while there are some minor further increases for China.

In addition to being the closest to canonical reforms in recent US trade history, China and Vietnam's NNTR-to-NTR transitions were also two of the most important in terms of aggregate trade growth. Figure 3(c) shows that each country's share of US imports starts to rise sharply after gaining conditional NTR access and continues to rise all the way until the first US-China trade war begins in 2018, when China begins to lose US market share and Vietnam's growth accelerates. One other important feature of these reforms is that because both countries began in embargo, all cumulative trade growth is driven by the extensive margin. Figure 3(d) illustrates this by plotting the share of SITC goods with positive trade. This is another reason that these case studies are particularly well-suited to study using a

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<sup>18</sup>The trade embargo on North Vietnam started in 1964 and was extended to all of Vietnam in 1975. The trade embargo on China was in place from 1950 to 1971.

model of export participation dynamics.

Ex post, China and Vietnam’s NNTR-to-NTR transitions are the closest empirical counterparts to canonical unanticipated, once-and-for-all reforms in our sample, but this was not how these reforms were perceived ex ante. Uncertainty about the duration of China’s NTR status has been the focus of numerous studies. In [Alessandria et al. \(2025a\)](#) we show that this status was initially expected to be quickly revoked, and we show below that the same is true for Vietnam, although there are some material differences in the trade-policy risks the two countries faced. Thus, even for these reforms, reduced-form empirical estimates of trade responses may be quite different than the response to a truly canonical reform.

## 4 Reduced-form evidence on trade dynamics

We now turn to measuring the dynamics of the trade elasticity. Our conceptual object of interest is the path of the cumulative trade elasticity,

$$\varepsilon_h = -\frac{\Delta_h v_{jgt}}{\Delta_h \tau_{jgt}}. \quad (3)$$

We first estimate the dynamics of the trade elasticity to the average tariff change, and show that the responses are small and grow little over time. Next, we condition on regime changes and find larger, more gradual responses. Finally, we estimate trade dynamics for China and Vietnam following their NTR grants and find even larger responses. It is important to emphasize that these estimates are reduced-form responses that reflect agents’ beliefs about the stochastic process for tariffs as well as the tariff changes observed in the data. In light of the evidence in section 3, we interpret the differences in trade dynamics for tariff changes within versus across policy regimes as evidence of differences across regimes in their underlying tariff processes.

### 4.1 Trade dynamics across vs. within regimes

As we have shown, most tariff changes, especially those that occur within policy regimes, are transitory. To account for this, we estimate (3) using the local-projections approach of

Boehm et al. (2023),

$$\Delta_h v_{jgt} = -\beta_h^v \Delta_h \tau_{jgt} + \mathbf{Z}_{jgt} + \delta_{jt} + \delta_{gt} + u_{jgt}, \quad (4)$$

where  $\Delta_h \tau_{jgt}$  is instrumented by  $\Delta_0 \tau_{jgt}$  for  $h > 0$  in order to control for subsequent tariff changes that are correlated with the change in  $t$ . The vector of controls,  $\mathbf{Z}_{jgt}$ , includes the preceding tariff change,  $\Delta_{-1} \tau_{jgt}$ , as well as the preceding import growth,  $\Delta_{-1} v_{jgt}$ . We include the same set of fixed effects as in section 3, which is standard in the trade-elasticity estimation literature. The country-year fixed effects  $\delta_{jt}$  control for aggregate shocks such as exchange-rate movements or aggregate cyclical fluctuations. The good-year fixed effects control for changes in multilateral resistance, such as good-specific fluctuations in US demand as well as multilateral policy changes. By taking differences of the dependent variable, constant good-level effects, such as trends, are eliminated. We provide estimates for horizons  $h = [0, 14]$ .

We first estimate (4) for the full sample, shown by the black line in Figure 2(b). The short-run trade elasticity,  $\beta_0^v$ , is 2.14, and the long-run elasticity,  $\beta_{14}^v$ , is about 3.48. These pooled estimates not only restrict trade responses to be the same for within- and across-regime tariff changes, but also impose the same autocorrelation structure for tariffs in the first stage.

We then contrast these pooled estimates with a version of (4) that allows for different responses to tariff changes across and within regimes:

$$\begin{aligned} \Delta_h v_{jgt} = & -\beta_h^{v,\text{within}} \Delta_h \tau_{jgt} \{\text{regime}_{jgt} = \text{regime}_{jgt-1}\} \\ & - \beta_h^{v,\text{across}} \Delta_h \tau_{jgt} \{\text{regime}_{jgt} \neq \text{regime}_{jgt-1}\} + \mathbf{Z}_{jgt} + \delta_{jt} + \delta_{gt} + u_{jgt}. \end{aligned} \quad (5)$$

The first stage in this extended specification is (1). We allow for the pre-trend controls to vary with the interaction of interest. Figure 2(b) also plots the estimated  $\beta_h^{v,\text{within}}$  and  $\beta_h^{v,\text{across}}$  coefficients. The estimates for within-regime trade elasticities are lower than the pooled estimates, and there is little adjustment over time, with a short-run elasticity of 1.61 and a long-run elasticity of 2.11. The across-regime elasticities are noticeably higher and grow more over time, with a short-run value of 3.87 and a long-run value of 5.86. As with our estimates of tariff dynamics, these results indicate that the pooled estimates are

not predictive of how trade responds to large reforms that involve regime changes because within-regime tariff changes constitute the vast majority of the sample.

## 4.2 Case studies: China and Vietnam NTR access

We now turn our attention to the effects of the NNTR-to-NTR transitions that China and Vietnam underwent in 1980 and 2002, which we have argued are the most canonical reforms in US trade history. Leveraging the heterogeneity in tariff changes across goods shown in Figure 3(a), we estimate the dynamics of trade following these reforms, as well as in the years leading up to them.

Following [Alessandria et al. \(2025a\)](#), we estimate the relationship between trade flows and the difference between the NNTR tariff and the NTR tariff, which we refer to as the *NNTR gap*:

$$X_g = \log(1 + \tau_g^{\text{NNTR}}) - \log(1 + \tau_g^{\text{NTR}}), \quad (6)$$

where the tariffs are measured as the average over the 1999 scheduled NNTR and NTR rates at the HS-8 tariff line from [Feenstra et al. \(2002\)](#). The NNTR gap has been widely used in the literature to measure tariff risk, as it represents the good-specific tariff hikes that would occur if a country was shifted back to NNTR. However, it also measures the tariff reductions that occurred when NTR status was first granted. This dual meaning complicates the interpretation of the estimates discussed below, but does not preclude them from being used as inputs to a quantitative analysis, which is the strategy we ultimately follow. Figure 3(e) plots the NNTR gap distribution.

We use an event-study design to estimate the responses of imports from each country to NTR access:

$$\begin{aligned} v_{jgt} = & \sum_{t'=1974}^{2008} \beta_t^{v,\text{CN}} \mathbb{1}\{t = t'\} \mathbb{1}\{j = \text{China}\} X_g \\ & + \sum_{t'=1994}^{2017} \beta_t^{v,\text{VN}} \mathbb{1}\{t = t'\} \mathbb{1}\{j = \text{Vietnam}\} X_g + \delta_{jt} + \delta_{jg} + \delta_{gt} + u_{jgt}. \end{aligned} \quad (7)$$

The dependent variable is the log import value of good  $g$  from country  $j$  in year  $t$ . Our coefficients of interest,  $\beta_t^j$ , measure the elasticity of trade with respect to the NNTR gap,

relative to the other countries with NTR status, and relative to a country-specific base year. We focus on a window from 1974 to 2008 for China to avoid effects from the Great Recession, while for Vietnam we focus on the full sample ending in 2017.<sup>19</sup> As before, we include country-year fixed effects,  $\delta_{jt}$ , country-good fixed effects,  $\delta_{jg}$ , and good-year fixed effects,  $\delta_{gt}$ . This fixed-effects structure is common in the literature on China’s NTR access (Pierce and Schott, 2016; Handley and Limão, 2017).

It is important to emphasize that in these event studies, the country-year fixed effects,  $\delta_{jt}$ , absorb not only aggregate shocks in exporting countries, but also the responses to the end of the US embargoes on China and Vietnam. The lifting of the embargoes was effectively an infinite tariff reduction that triggered large, gradual trade adjustments that were still well underway when these countries later gained NTR status. As a result, our empirical approach measures the effect of NTR access on exports to the United States above and beyond this ongoing post-embargo growth, comparing products that experienced tariff reductions from NTR access with those that did not. In the appendix, we present an alternative specification that explicitly controls for aggregate fluctuations using data on GDP and exports to non-U.S. markets. That approach allows the gap elasticities ( $\beta_t^{v,CN}$  and  $\beta_t^{v,VN}$ ) to capture the combined effects of NTR access and embargo removal. Our model is consistent with both approaches, but our baseline estimation targets the specification with country-year fixed effects.

Figure 3(f) shows the estimates of  $\beta_t^{CN}$  and  $\beta_t^{VN}$ .<sup>20</sup> There are four key observations. First, both countries have similar long-run NNTR-gap elasticities that are substantially higher than the average long-run trade elasticities we previously estimated. From trough-to-peak, the elasticity is about 11.8 for China and 11.9 for Vietnam. Second, the Chinese response, on impact, was about half as big as the Vietnamese response. Third, the two countries have different trade dynamics in the lead-up to the tariff change. Trade with Vietnam in high-gap goods was growing faster than low-gap goods while, for China, high- and low-gap goods were

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<sup>19</sup>As a major US trade partner at that time, the Great Recession had a significant impact on US imports from China. Trade with Vietnam was still in the very early stages at this time, and was not significantly impacted.

<sup>20</sup>The appendix compares our event-study approach with a local-projections specification like (5) that focuses specifically on NTR access as in (2). While the latter works well for China, it significantly understates the long-run trade response for Vietnam due to the anticipatory trade growth in the years leading up to NTR access.

growing at about the same pace. Fourth, Chinese exports grew much slower than exports from Vietnam; it took more than twelve years to reach China’s long-run level, whereas Vietnam’s adjustment was more than 90 percent complete after four years (note that this is precisely the length of the period in which the Vietnamese reform remained conditional).

### 4.3 Discussion: robustness concerns

Our main empirical findings are that across-regime tariff changes, particularly the China and Vietnam NTR grants, are more persistent and lead to larger long-run trade responses than within-regime changes. Our supplemental appendix shows that these results hold across a wide range of robustness checks and sensitivity analyses, including different estimation approaches, sample designs, and fixed-effect structures. Before turning to our structural modeling exercises, a short discussion of two potential robustness concerns is warranted.

The first concern is what we call “supply factors:” exogenous shocks at the source-product level that shift exports to all destinations, not just the United States.<sup>21</sup> Controlling for supply factors using source-product-time fixed effects has been common practice in empirical trade since Romalis (2007). Our use of annual US import data over a long window allows us to observe numerous transitions between trade-policy regimes and exploit the relatively clean identification of product-level tariff risk and trade dynamics for China and Vietnam, but it prevents us from taking this approach. In order to control for supply factors, we would need to include data on tariffs and imports from multiple destination countries, not just the United States, so that source-product-time effects could be identified.

There are three reasons to doubt that controlling for supply factors would improve our identification. First, failing to control for them would only be a problem if they were systematically correlated with variation in US trade policy. Second, the United States is the largest economy in the world, so any change in US trade policy would likely affect source countries’ exports to third countries in general equilibrium. Third, owing to geopolitics, US trade policy is likely to be correlated with countries’ policies.<sup>22</sup> Note that the last two points

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<sup>21</sup>The word “exogenous” is key here. Endogenous supply-side adjustments caused by US tariff changes (e.g., entry, productivity, quality upgrading) are part of the total dynamic response we seek to measure and are a crucial part of our structural model. This concern only pertains to source-product shocks that would have raised exports absent variation in US trade policy but are nevertheless correlated with this variation.

<sup>22</sup>In the case of China, many other countries imposed an embargo in 1951; the United Nations General Assembly Resolution 500, which recommended a trade embargo on the People’s Republic of China and

do not necessarily imply that omitting source–product–time effects is innocuous (that’s the purpose of the first point), but they also caution against treating these fixed effects as a clean remedy. These effects can absorb endogenous supply-side adjustments that are induced by US policy—precisely part of the total dynamic response we aim to estimate and which our structural model is designed to capture.

Despite these conceptual reasons to doubt the benefit of controlling for supply factors in our setting, we address this concern in the appendix directly by using a more comprehensive dataset on worldwide bilateral trade flows in which we can control for the supply factors, similar to the Romalis (2007) approach. This comes at the cost of a higher level of aggregation (another robustness concern we discuss below) and a noisier measure of applied tariffs due to some inconsistencies between the trade flows in this dataset and those reported by the US census. Overall, the estimates obtained in this exercise are very similar to our baseline results, and our main results about the large long-run responses to across-regime tariff changes, especially the grants of NTR to China and Vietnam, hold up completely.<sup>23</sup>

The second robustness concern pertains to aggregation. As we discussed above, the 5-digit SITC level is the lowest level of aggregation at which trade and tariffs can be measured over a long time span, but US tariff rates are specified at a lower level of aggregation (7-digit TSUSA until 1989, then HS8 afterwards). Our baseline measure of applied tariffs is constructed by taking the weighted average of the scheduled rates within each good (a source country-SITC code pair), which means that some of the variation in bilateral tariffs in our sample is could be spurious, driven by changes in the composition of trade within goods rather than actual changes in tariff rates. However, as we show in the appendix, this does not drive our results.

Our first approach to dealing with the aggregation issue is to simply take the median or the simple average of tariffs within goods instead of the weighted average. In our second approach, we measure the dispersion of tariffs within goods and exclude goods with dispersion

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North Korea, passed 47–0 in May of that year. Canada maintained its embargo throughout the 1950s, did not formally recognize the People’s Republic of China at all until 1970, and imposed the Canadian General tariff (that country’s analogue of NNTR) until 1973. For Vietnam, many countries aside from the United States maintained embargoes until the Paris Peace Agreement of 1991. As we show in the appendix, this means that Vietnam’s multilateral export growth (both at the product level and the aggregate) during the 1990s was highly correlated with the growth of its exports to the United States.

<sup>23</sup>See, specifically, Figures A3(k) and A4(g)–(h) in the appendix.

above a threshold. Our results do not change materially in any of the variations of these approaches that we have tried. The autocorrelations of across- vs. within-regime tariff changes are largely unaffected and the long-run trade elasticities for both types of tariff changes rise slightly; if anything, the differences in tariff persistence and trade elasticities across vs. within regimes grow. There are no statistically significant changes to Vietnam’s NTR-gap elasticity and very few China, and those changes that are statistically significant make the elasticity larger in magnitude, not smaller.

## 5 Model

Our model is a partial-equilibrium version of [Alessandria et al. \(2021\)](#) in which a continuum of heterogeneous firms choose to start and stop exporting in the face of idiosyncratic technology shocks and aggregate shocks to trade policy. The model has two key ingredients that generate gradual adjustment to policy: sunk exporting costs and export capacity that grows stochastically with a firm’s tenure as an exporter. The first ingredient makes firms consider the entire future path of trade policy, not just its current state ([Das et al., 2007](#); [Alessandria and Choi, 2007](#)). The second ingredient generates exporter life-cycle dynamics ([Alessandria et al., 2021](#); [Fitzgerald et al., 2023](#); [Steinberg, 2023](#)). The model allows for potentially large differences between short- and long-run trade elasticities.

**Production and demand.** Firms use labor,  $\ell_t$ , to produce output according to a constant-returns to-scale technology,  $y_t = z_t \ell_t$ .<sup>24</sup> Productivity,  $z_t$ , evolves over time according to an AR(1) process in logs:  $\log z_t = \rho_z \log z_{t-1} + \sigma_z \varepsilon_t$ , where  $\varepsilon_t$  is i.i.d. across firms and time. Firms produce differentiated goods and operate in monopolistically competitive markets. Foreign demand for a firm’s good is a downward-sloping function of the price the firm charges,  $p_t$ , and the import tariff,  $\tau_t$ :  $d_t(p_t, \tau_t) = (p_t \tau_t)^{-\theta}$ , where  $\theta$  is the price elasticity of demand.

**Trade costs.** There are three types of trade costs: import tariffs, iceberg-type variable trade costs, and fixed trade costs. Import tariffs apply to all firms equally and are, at this stage, allowed to follow an arbitrary stochastic process. We index model objects by the time

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<sup>24</sup>We abstract from adjustment frictions on labor or capital inputs. Several papers show that these have a small impact on firm-level trade dynamics, especially when exports are a relatively small fraction of overall sales and firms can easily reallocate domestic sales to the export market ([Alessandria and Choi, 2014](#); [Alessandria et al., 2025a](#); [Kohn et al., 2020](#)).

period,  $t$ , which is a parsimonious way to encode firms' expectations about future tariffs. Variable trade costs can take three values,  $\infty > \xi_H > \xi_L$ , that encode a firm's status as an exporter. A firm with  $\xi = \infty$  is a non-exporter, a firm with  $\xi_H$  is a high-cost exporter, and a firm with  $\xi = \xi_L$  is a low-cost exporter. To begin exporting, a non-exporter must pay a fixed cost  $f_0$ . To continue exporting, a low- or high-cost exporter must pay a fixed cost  $f_1$ . Thus, the fixed cost of exporting can be expressed as a function,  $f(\xi)$ , of the variable cost, where  $f(\infty) = f_0$  and  $f(\xi_H) = f(\xi_L) = f_1$ .

**Exporter life cycles.** Firms are born as non-exporters with  $\xi = \infty$ . In each period, firms die with probability  $1 - \delta(z) = \max[0, \min(e^{-\delta_0 z} + \delta_1, 1)]$ , in which case they are replaced by newborn firms with productivities drawn from the ergodic distribution. When a non-exporter chooses to enter the export market, its variable trade cost falls to  $\xi_H$  in the next period. When a high-cost exporter chooses to continue exporting, its variable exporting cost falls to  $\xi_L$  in the next period with probability  $1 - \rho_\xi$ . Symmetrically, when a low-cost exporter chooses to continue exporting, the probability of switching to  $\xi_H$  is also  $\rho_\xi$ .

**Firm's problem.** The firm's state variables are its productivity,  $z$ , its variable trade cost,  $\xi$ , and its tariff,  $\tau$ . The firm's problem has a static component and a dynamic component. The static problem entails choosing a price to maximize the profits from exporting in the current period:

$$\pi_t(z_t, \xi_t, \tau_t) = \max_p \left\{ p d_t(p, \tau_t) - w_t \frac{\xi_t d_t(p, \tau_t)}{z_t} \right\}, \quad (8)$$

which is equal to zero if  $\xi_t = \infty$ . The dynamic problem entails deciding whether to export in the next period. The value of a firm that chooses to export in  $t + 1$  is

$$V_t^1(z_t, \xi_t, \tau_t) = -f(\xi_t) + \frac{\delta(z)}{1+r} \mathbb{E}_{z, \xi, \tau} [V_{t+1}(z_{t+1}, \xi_{t+1}, \tau_{t+1})], \quad (9)$$

and the value of a firm that chooses not to export is

$$V_t^0(z_t, \xi_t, \tau_t) = \frac{\delta(z)}{1+r} \mathbb{E}_{z, \tau} [V_{t+1}(z_{t+1}, \infty, \tau_{t+1})], \quad (10)$$

where

$$V_t(z_t, \xi_t, \tau_t) = \pi_t(z_t, \xi_t, \tau_t) + \max\{V_t^0(z_t, \xi_t, \tau_t), V_t^1(z_t, \xi_t, \tau_t)\}. \quad (11)$$

The solution to the export participation problem is characterized by a break-even level of productivity,  $\bar{z}_t(\xi)$ , such that  $V_t^0(\bar{z}_t(\xi), \xi, \tau_t) = V_t^1(\bar{z}_t(\xi), \xi, \tau_t)$ . Importantly,  $\bar{z}_t(\xi)$  depends on firms' expectations about future tariffs, not just the current tariff rate.

**Aggregation.** Aggregate exports are given by

$$X_t = \sum_{\xi \in \{\xi_L, \xi_H\}} \int^z p_t(z, \xi, \tau_t) d_t(p_t(z, \xi, \tau_t), \tau_t) d\varphi_t(z, \xi), \quad (12)$$

where  $\varphi_t(z, \xi)$  denotes the joint distribution of firms indexed by their productivity and variable trade cost. This distribution evolves according to the following law of motion:

$$\varphi_t(\mathcal{Z}, \infty) = \sum_{\xi} \left[ \int_0^{\bar{z}_{t-1}(\xi)} h(\mathcal{Z}, z) \varphi_{t-1}(z, \xi) dz + \int_0^{\infty} \bar{h}(\mathcal{Z}) \varphi_{t-1}(z, \xi) dz \right], \quad (13)$$

$$\begin{aligned} \varphi_t(\mathcal{Z}, \xi_H) &= \int_{\bar{z}_{t-1}(\infty)}^{\infty} h(\mathcal{Z}, z) \varphi_{t-1}(z, \infty) dz + \rho_{\xi} \int_{\bar{z}_{t-1}(\xi_H)}^{\infty} h(\mathcal{Z}, z) \varphi_{t-1}(z, \xi_H) dz \\ &\quad + (1 - \rho_{\xi}) \int_{\bar{z}_{t-1}(\xi_L)}^{\infty} h(\mathcal{Z}, z) \varphi_{t-1}(z, \xi_L) dz, \end{aligned} \quad (14)$$

$$\varphi_t(\mathcal{Z}, \xi_L) = (1 - \rho_{\xi}) \int_{\bar{z}_{t-1}(\xi_H)}^{\infty} h(\mathcal{Z}, z) \varphi_{t-1}(z, \xi_H) dz + \rho_{\xi} \int_{\bar{z}_{t-1}(\xi_L)}^{\infty} h(\mathcal{Z}, z) \varphi_{t-1}(z, \xi_L) dz, \quad (15)$$

where  $\mathcal{Z}$  is a typical subset of the space of productivity draws,  $h(\mathcal{Z}, z)$  is the probability of surviving and drawing a new productivity in  $\mathcal{Z}$  conditional on current productivity  $z$ , and  $\bar{h}(\mathcal{Z})$  is the probability of dying and being replaced by a new firm with productivity in  $\mathcal{Z}$ .

## 6 Illustrating expectational biases

We now use the model to illustrate how trade-policy expectations complicate trade-elasticity estimation. First, we show that less-persistent tariff changes have lower long-run trade elasticities, both because the level of trade is higher beforehand and because the response afterward is smaller, and that reduced-form estimation methods do not recover canonical trade elasticities from data generated by non-canonical reforms. Second, we show that the short-run trade elasticity is higher when tariff changes are preceded by reductions in persistence. Throughout this section, we use the model parameters calibrated in the case study of China in the next section, which are listed in Table 4. This may seem out of order to the reader, but

we believe that developing these ideas before explaining our identification strategy is useful, and that illustrating their quantitative significance as well as their qualitative significance is important.

## 6.1 Tariff persistence and the long-run trade elasticity

Consider a symmetric Markov process for tariffs with two values,  $\tau_L = 0$  and  $\tau_H = 30$  percent, and a symmetric probability of switching states,  $1 - \omega$ . When  $\omega = 0.5$ , tariffs are i.i.d. across time and the current tariff contains no information about future tariffs. When  $\omega = 1$ , tariffs are constant and the tariff cut is permanent. In between these extremes, tariffs are persistent but firms know there is a chance of switching states.

Figure 4 shows the dynamics of trade before and after a once-and-for-all tariff change for different values of  $\omega$ . In all versions of this experiment, the economy is in the high-tariff state for many periods, switches to the low-tariff state in period  $t = 0$ , and remains there forever. While this hypothetical is unlikely, especially when  $\omega$  is low and tariffs are expected to fluctuate frequently, it is useful to illustrate the economic forces at play; we discuss trade-elasticity estimation using simulated data with persistent vs. transitory tariff processes below. Panel (a) shows the discounted expected tariff,

$$\bar{\tau}_t^E = (1 - \beta)^{-1} \mathbb{E}_t \left[ \sum_{s=t+1}^{\infty} \beta^{s-t-1} \tau_s \right]. \quad (16)$$

Note that this measure excludes the current tariff rate, as only expectations about future tariffs matter for the export participation decision. Panel (b) shows export participation, panel (c) shows aggregate exports, and panel (d) shows the cumulative trade elasticity,  $\varepsilon_h$ , measured as in (3).

The  $\omega = 1$  case is the canonical trade reform found in virtually all quantitative and theoretical trade studies: an unanticipated, permanent reform. In period zero, the distribution of firms over trade costs and trade participation is predetermined, so trade only responds on the intensive margin, and the short-run trade elasticity is equal to the CES demand elasticity:  $\varepsilon_0 = \theta = 3.17$ . We refer to  $\varepsilon_0$  as the *short-run canonical trade elasticity*. In period one, export participation begins to rise, and converges to its new long-run value, which is

about 3.5 log points higher than before the reform. Trade follows a similar trajectory, and the long-run trade elasticity,  $\varepsilon_\infty$ , is about 15. This *long-run canonical trade elasticity* is a function of  $\theta$  as well as the parameters that govern firm-level trade dynamics,  $f_0$ ,  $f_1$ ,  $\xi_L$ ,  $\xi_H$ , and  $\rho_\xi$ , that we will recover in our quantitative exercise.

When  $\omega < 1$ , the discounted expected tariff is lower than the applied tariff before the reform. This raises export participation and trade in the high-tariff state relative to the canonical case. Conversely, the discounted expected tariff falls less than applied tariffs once the economy switches to the low-tariff state, which depresses the response of export participation and trade. To highlight these effects, figure 4 plots export participation and trade in each case relative to their values in the  $\omega = 1$  case. Both effects push the measured long-run trade elasticity below the canonical long-run elasticity. The  $\omega = 0.65$  case, which is very close to i.i.d., is particularly instructive. In this case, where the discounted expected tariff is essentially constant, export participation barely reacts to tariff shocks and the trade elasticity is virtually identical to the demand elasticity,  $\theta$ , at all horizons.

These once-and-for-all reforms illustrate how tariff persistence affects measured long-run trade elasticities, but they are inconsistent with the statistical properties of the assumed tariff process, especially for low levels of persistence where tariffs change frequently. To illustrate that this issue matters “in sample” under rational expectations, we apply the [Boehm et al. \(2023\)](#) empirical approach we used in section 4 to simulated model data. For each value of  $\omega$  shown in Figure 4, we simulate a panel of 1,000 goods for 1,000 periods, with tariff shocks drawn from the assumed distribution independently across goods. We then estimate tariff autocorrelations using (1) (with indicators for  $\omega$  values instead of regime switches) and trade-elasticity dynamics using the local-projections specification (4).

Panel (a) of Figure 5 shows the simulated tariff autocorrelations. Despite its simplicity, the two-state Markov process exhibits tariff dynamics that are similar to the data; within-regime and across-regime changes are approximated well by Markov processes with  $\omega = 0.65$ – $0.8$  and  $\omega = 0.9$ – $0.9$ , respectively. Panel (b) shows our simulated trade-elasticity estimates, normalized relative to the short-run elasticity for horizon  $h = 0$ . Overall, the “in-sample” elasticities estimated using local projections are similar to the calculations from the simple one-off reform episodes. For low values of  $\omega$ , the ratio of the long-run elasticity to the short-

run elasticity is barely above one, but once  $\omega$  exceeds 0.8, this ratio starts to rise sharply, reaching a value of 4.5 in the canonical  $\omega = 1$  case. Even when  $\omega = 0.99$ , however, which is virtually indistinguishable from the canonical case in the data unless one has an extremely long sample, the long-run elasticity estimate is materially lower than the canonical long-run elasticity. This confirms that reduced-form empirical approaches yield estimates of the long-run trade elasticity that are far below the canonical trade elasticity unless they are applied to extremely persistent reforms—much more persistent than the typical regime switch in our sample. This underscores the value of the China and Vietnam episodes as starting points for recovering canonical elasticities.

## 6.2 Persistence shocks and the short-run trade elasticity

We now use our model to illustrate what happens when changes in the expected discounted tariff precede changes in the applied tariff. This is a different form of anticipation than in the previous analysis. We now suppose that the economy rests in a perfectly persistent high-tariff steady  $\tau_H$  with  $\omega = 1$  for many years and then experiences a tariff cut to  $\tau_L$  that may be coupled with an unanticipated decline in persistence to  $\omega = 0.8$ . We present four versions of this experiment with different timings in Figure 6.

The first version (labeled  $\tau \downarrow$ ) is a drop in tariffs from  $\tau_H$  to  $\tau_L$  at  $t = 0$  without a change in persistence. This is the canonical surprise, once-and-for-all reform we have already discussed; we repeat it here to use as a benchmark against which to compare the other versions. The second version (labeled  $\omega \downarrow$ ) is an unanticipated drop in persistence from  $\omega = 1$  to  $\omega = 0.8$  without a tariff cut. Export participation begins to rise starting in the following period, generating an eventual increase in trade of about 1.5 log points, but since there is no change in tariffs, we cannot measure the trade elasticity.

The third version (labeled  $\tau, \omega \downarrow$ ) is a transition from  $\tau_H$  to  $\tau_L$  coupled with a simultaneous decline in persistence from  $\omega = 1$  to  $\omega = 0.8$  in  $t = 0$ . This lowers the current tariff, but since this change is expected to be less persistent than the canonical reform, the expected discounted tariff falls much less. The long-run response of trade is about halfway between the first two reforms' responses. Note that there is no anticipatory effect since the change in persistence occurs alongside the change in tariffs, so the short-run trade elasticity is still

equal to the demand elasticity  $\theta = 3.17$ .

The last version (labeled  $\omega \downarrow \rightarrow \tau \downarrow$ ) is an unanticipated drop in persistence from  $\omega = 1$  to  $\omega = 0.8$  in  $t = -1$  followed by a cut in tariffs from  $\tau_H$  to  $\tau_L$  in  $t = 0$ . The long-run response to this two-step reform is the same as in the third reform, but the short-run trade elasticity is now higher than the demand elasticity. This is because some firms make investments in market access in  $t = -1$  when they learn that a future tariff cut is more likely, pushing up export participation in  $t = 0$  when the tariff cut indeed materializes.

This kind of anticipatory effect is the mechanism that we believe to be at play in generating larger short-run responses to regime switches in section 4.1 and Vietnam’s NTR access in section 4.2. Indeed, tariff-schedule shifts often involve changes in expectations that precede changes in tariffs. For example, free-trade agreements typically involve long negotiation periods, are formally announced before being actually implemented, and phased in gradually over several years. We show how phased-in reforms can dramatically increase short-run trade elasticities in the appendix.<sup>25,26</sup> Of course, the case of China’s 1980 NTR access, which has a similar short-run trade elasticity to within-regime tariff changes, indicates that isn’t always true. Our quantitative analysis interprets the data as indicating that the likelihood of NTR access increased during Vietnam’s NNTR period but not during China’s.

## 7 Quantitative analysis

We now take our model to the data, which we have shown are contaminated by anticipation of and uncertainty about tariff changes, and use it to measure how trade would respond in the absence of these expectational biases. Our strategy is to jointly discipline the model’s technological parameters and its stochastic process for trade policy with empirical evidence from two trade reforms that we have argued are uniquely well-suited to this task: the United States granting NTR status to China in 1980 and Vietnam in 2002. We then use the model to simulate surprise once-and-for-all reforms to measure the canonical trade-elasticity path.

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<sup>25</sup>Khan and Khederlarian (2021) document evidence for these effects in NAFTA.

<sup>26</sup>Likewise, our analysis in this section assumes the economy starts from a steady state, but initial conditions can also complicate trade-elasticity estimation, particularly if the economy is still adjusting to an earlier reform like the lifting of an embargo. We discuss this issue in the appendix as well.

## 7.1 Environment and calibration

To exploit the heterogeneity in tariff and trade dynamics across goods documented above, we consider a multi-good version of the model in section 5. There are  $G$  goods, which correspond to the 5-digit SITC goods in our data. In each good  $g = 1, \dots, G$ , a continuum of firms sell differentiated varieties. The technological parameters—the productivity process and the variable and fixed costs of exporting—are the same across goods.<sup>27</sup> The basic structure of the economy is the same for both countries but we allow parameter values and timing to differ. For clarity, we omit country labels.

Tariffs,  $\tau_g(s)$ , differ across goods and trade-policy regimes,  $s \in \{\text{NNTR}, \text{NTR}\}$ . We take the tariff rates directly from the data. A country’s tariff regime, which is an aggregate state, follows a time-varying Markov process with transition probabilities

$$\Omega_t = \begin{bmatrix} \omega_t^{\text{NNTR}} & 1 - \omega_t^{\text{NNTR}} \\ 1 - \omega_t^{\text{NTR}} & \omega_t^{\text{NTR}} \end{bmatrix}, \quad (17)$$

where  $\omega_t^s$  is the persistence of state  $s$  in year  $t$ .<sup>28</sup>

Our calibration strategy builds on [Alessandria et al. \(2025a\)](#). First, we set several parameters externally. We take the interest rate,  $r$ , the survival-rate parameters,  $\delta_0$  and  $\delta_1$ , and the iceberg-cost transition probability,  $\rho_\xi$  from [Alessandria and Choi \(2021\)](#).<sup>29</sup> We set the demand elasticity,  $\theta$ , to the average estimate in [Soderbery \(2018\)](#) for US imports from China, which is 3.17. Soderbery’s estimate relies on cross-sectional price and quantity variation and is therefore closely related in a conceptual sense to our demand-elasticity parameter. We use this estimate rather than the reduced-form short-run trade elasticity because the latter also depends on changes in trade policy expectations as shown in section 6.2.<sup>30</sup> In the ap-

<sup>27</sup>We explore heterogeneity across goods in these parameters for China in [Alessandria et al. \(2025a\)](#). There are too few firms in many sectors in Vietnam to reliably estimate exporter-dynamics.

<sup>28</sup>We follow [Alessandria et al. \(2025b\)](#) and assume that firms believe the current transition probabilities will remain in force forever, and that firms are surprised each period when these probabilities change. In [Alessandria et al. \(2025a\)](#), we studied a model where firms have perfect foresight over the entire path  $\{\Omega_t\}_{t=0}^\infty$  and showed that the results are similar under both approaches.

<sup>29</sup>Estimating these parameters by matching moments from firm-level data requires a longer sample than we have for either China or Vietnam. The sensitivity analysis in the appendix shows that our results are not particularly sensitive to these parameters.

<sup>30</sup> $\theta$  cannot be separately identified from the probability of gaining NTR status,  $1 - \omega_t^{\text{NTR}}$ , because both parameters have the same directional effect on the initial jump in trade that occurs when that status

pendix, we show that there is a fairly narrow range of values for  $\theta$  that are compatible with the dynamics of the NTR-gap elasticity in these episodes, and re-calibrate our model using alternative estimates from the literature at the upper and lower ends of this range.

Second, we calibrate the other technological parameters,  $\sigma_z$ ,  $f_0$ ,  $f_1$ , and  $\xi_H$ , to match four moments computed from firm-level panel data: the export participation rate, the export exit rate, the incumbent size premium (the average sales of incumbent exporters relative to the average for new exporters), and the coefficient of variation of log exports. We measure these moments in the model in the same years that we observe them in the data (2004–2007 for China and 2010–2014 for Vietnam).<sup>31</sup> The entry cost,  $f_0$ , is mainly identified by the export participation rate. The continuation cost,  $f_1$ , is mainly identified by the exit rate. The initial iceberg trade cost,  $\xi_H$ , is mainly identified by the incumbent premium. Productivity dispersion,  $\sigma_z$ , is mainly identified by the coefficient of variation of log exports. Table 4 lists the target moments and calibrated values of the technological parameters for each country.

Third, we calibrate the sequence of trade-policy transition matrices  $\{\Omega_t\}_{t=0}^{\infty}$  to match the NNTR-gap elasticity dynamics described in section 4.2.  $\omega_t^{\text{NNTR}}$  is identified by the dynamics in the period before the country was granted NTR status. When the NNTR-gap elasticity grows more negative during this period, i.e., exports of high-gap goods grow faster than exports of low-gap goods, it indicates an increase in the likelihood of gaining NTR status.  $\omega^{\text{NTR}}$  is identified by the NNTR-gap elasticity dynamics after the NTR grant. A decrease in this elasticity indicates an increase in the likelihood of retaining NTR status. The main difference between our strategy and that of [Alessandria et al. \(2025a\)](#) is that we allow  $\omega_t^{\text{NNTR}}$  to vary over time, allowing us to capture changes in the probability of the NTR grant occurring in advance of the policy. Figure 7(a) shows the fit of the model against these target moments.<sup>32</sup>

Fourth, we start the model from the embargo, i.e., we set export participation to zero, and then lift the embargo unexpectedly in the appropriate year for each country.<sup>33</sup>

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is granted. Note that this externally-assigned  $\theta$  value works well for both countries despite the fact that the reduced-form short-run elasticity estimate is much higher for Vietnam, which our model interprets as a growing likelihood of NTR access, generating anticipatory growth in export participation.

<sup>31</sup>The appendix contains more details about the firm-level datasets.

<sup>32</sup>We HP-filter the NNTR-gap coefficients to smooth out temporary spikes. The calibrated model exactly matches this smoothed series.

<sup>33</sup>Even if firms believe there is a chance the embargo may be lifted, they do not enter before it is lifted

One material difference between our calibration strategy in this paper and in our previous work [Alessandria et al. \(2025a,b\)](#) is that here we jointly calibrate the technological parameters and the policy process, whereas in those papers, we separately calibrated the technological parameters in the long-run steady state with NTR tariffs and no risk of returning to the NNTR regime. In the case of China, which was the focus of those papers, this approach was relatively innocuous because the data moments are measured more than two decades after NTR access was granted, at a time when policy risk was widely believed to be minimal. In the case of Vietnam, it would be more problematic because the data moments are measured at a time when the economy was still adjusting to NTR access and, as we estimate, the risk of returning to NNTR was higher. This new approach is a methodological improvement.

Figure 7(b) plots the calibrated regime transition probabilities,  $1 - \omega_t^s$ . For China, the probability of gaining NTR status in the 1970s was low and did not change materially. When China gained NTR status in 1980, the pattern of trade growth is consistent with the view that this status was unlikely to be maintained, and only started to become credible in the mid-1980s. [Alessandria et al. \(2025a\)](#) discuss these considerations in detail. For Vietnam, the initial probability of gaining NTR status was similar to China's, but during the late 1990s and early 2000s this probability more than tripled. The model interprets the fast growth of high-gap exports relative to low-gap exports in advance of the Vietnamese NTR grant as evidence of an increase in the expectation that this reform would occur. After Vietnam gained NTR status in 2002, the fast growth in trade is consistent with the probability of losing this status falling faster than in China. As the long-run changes in the NNTR-gap elasticity are similar in the two cases, we estimate similar initial and final transition probabilities.

## 7.2 Discussion of US political timelines

Our calibration indicates that differences in the dynamics of US imports from China versus Vietnam reflect differences in expectations about US trade policy. While there are no direct measures of these expectations, specific details of the political process confirm that China's 1980 grant of conditional NTR was more of a surprise and less likely to be maintained than Vietnam's 2002 grant. Table 6 shows a timeline of the events in US politics leading up 

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 unless this probability is extremely high.

to the grants of conditional NTR and PNTR for each country and the outcomes of key Congressional votes. One high-level sign that these regime transitions were realizations from different underlying stochastic processes is the fact that it took China 29 years to gain PNTR from the end of the embargo compared to only 12 years for Vietnam.

The timing of the events leading up to the conditional NTR grants is consistent with our finding that the probability this would occur was low and constant for China versus rising over time for Vietnam. There were five procedural steps to granting conditional NTR:

1. Establishing diplomatic relations;
2. a Presidential waiver of the Jackson-Vanik (JV) Amendment;
3. Congressional approval of that waiver;<sup>34</sup>
4. negotiation of a Bilateral Trade Agreement (BTA); and
5. Congressional ratification of the BTA.

Starting the clock with the lifting of the embargo, the first of these five steps took much longer for China than Vietnam: eight years versus one and a half. The longer window for China meant that three presidents with sharply different geopolitical perspectives were involved (Nixon, Ford, and Carter), while a single president (Clinton) presided over the process for Vietnam. However, the next four steps were much faster for China: 4 months versus 43 months.<sup>35</sup> The longer window for Vietnam implied that there were actually four separate House votes on the JV waiver with increasing levels of support (initially 61 percent, rising to 78 percent just 3 months before the BTA vote).

The timelines are also consistent with our finding that Vietnam's conditional NTR grant was initially more likely to remain in place than China's. The votes approving the BTA with Vietnam passed almost unanimously (409–9 in the House and 88–12 in the Senate), while the BTA with China passed with slimmer margins and many abstentions (294–88 in the House and 74–8 in the Senate). The Vietnam BTA was signed in the first year of George W. Bush's presidency, while the China BTA was signed in Jimmy Carter's last year as president and

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<sup>34</sup>Formally, the lack of a successful disapproval measure.

<sup>35</sup>For China, President Carter issued the JV waiver on October 23, 1979, Congress ratified the BTA on January 24, 1980. For Vietnam, President Clinton issued the first waiver on March 9, 1998, but the BTA was not ratified until October 3, 2001.

the initial waiver was actually granted by his successor, Ronald Reagan, at a time of greater geopolitical tension related to Taiwan and the Soviet Union.<sup>36</sup> Another sign that the path to PNTR was more assured for Vietnam is that Congress voted on China’s JV waiver right up until PNTR access, whereas discussions about Vietnam’s waiver stopped reaching the House floor shortly after the BTA because they were no longer contentious.<sup>37</sup> Comparing the waiver votes 4 years in advance of the PNTR vote, Vietnam’s passed with 79 percent approval in the House of Representatives while China’s passed with only 67 percent.

### 7.3 Canonical trade elasticities

We now use the calibrated model to measure the trade elasticity following a canonical tariff reform that features neither anticipation nor uncertainty and is not affected by initial conditions. For each country, we first solve for the steady-state with permanent NNTR tariffs. We then introduce a surprise once-and-for-all switch to the NTR tariff schedule and solve for the transition to the new steady state. The canonical trade elasticity at horizon  $h$  is the difference in the canonical reform between the NTR-gap elasticity  $h$  periods after NTR access and its initial value. The long-run canonical trade elasticity is the difference between the gap elasticities in the new steady state with NTR tariffs and the initial steady state with NNTR tariffs. We find that this elasticity is 14.67 for China and 15.29 for Vietnam. We report the sensitivity of these estimates to the model parameters in the appendix.

Figure 7(c) compares the NTR-gap elasticity dynamics in the canonical NTR reforms to the baseline dynamics. There are three key differences. First, the pre-NTR gap elasticities in the baseline model are 4–6 log points smaller than the gap elasticities in the NNTR steady state. This is largely because firms in the baseline model have a greater incentive to begin exporting in advance of gaining NTR status, particularly firms that produce high-gap goods which have the most to gain from the reform. Moreover, in the data, the economy is not in a steady state but transitioning from the embargo, biasing our long-run estimates downward.

Second, the gap elasticities converge more slowly after the baseline NTR reforms than after the canonical reforms. This is because the probability of keeping NTR status is initially

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<sup>36</sup>Another anecdote in support of this finding is that the negotiated Trade Agreement between the Soviet Union and United States in 1972 to grant NTR was never ratified, in part because Congress put the Jackson-Vanik Amendment in place.

<sup>37</sup>China’s delay in joining the WTO led to a Congressional vote on the JV waiver after the PNTR vote.

low in both countries and rises gradually over time. It more noticeable in the case of China, where the gap elasticity stalls for several years after the baseline NTR reform and only starts to rise again several years later.<sup>38</sup> This implies that trade should continue to grow beyond our estimation window for both countries. In our canonical reform there is very little continued growth outside our estimation period.

Third, in the case of Vietnam, the gap elasticity begins to rise several years before the baseline NTR reform, whereas there is no change in advance of the canonical reform. This is an anticipatory effect driven by the increase in the probability of Vietnam gaining NTR status during 1999–2001 shown in Figure 7(b); note there is no anticipatory growth in the case of China, where the likelihood of gaining NTR status was more or less constant throughout the 1970s. This anticipation effect lowers the long-run trade elasticity and raises the short-run elasticity we recover from Vietnam.

Finally, we calculate the speed of adjustment of the canonical reform. Specifically, we measure the gap between the final level of trade and the current level of trade relative to the long-run change,  $\lambda_h = (\log V_\infty - \log V_{t+h}) / (\log V_\infty - \log V_{t-1})$ , where  $V_\infty$  is the level of trade in the new steady state. We plot this gap in Figure 7(d) and fit a geometric function on the latter half of the sample. 22–23 percent of the long-run change occurs in the first year, which captures the intensive-margin effect driven by the demand elasticity  $\theta$ . Another 40–45 percent occurs in the second year, reflecting the one-period delay on the extensive margin. After the first two years half the distance to the long-run closes every five years.

Table 5 compares the long-run canonical trade elasticities to our reduced-form estimates, both for US data from section 4.1 and the model simulation in section 6.1. The reduced-form gap-elasticities for China and Vietnam NTR access are 20 and 22 percent smaller than their canonical counterparts, respectively. A key reason that these estimates are biased downward is that the change in the mean discounted expected tariff is smaller than the observed change in tariffs. Indeed, we find that from 7 years before the reform to the final steady state, the change in the mean discounted expected tariff was 25 percent smaller than the actual change in applied tariffs for China and 23 percent smaller for Vietnam. If we scale the reduced-form estimates by this factor, they increase to 15.6 for China and 15.3 for Vietnam—very close

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<sup>38</sup>Alessandria et al. (2025a) discuss the geopolitical background of the “stalled” period.

to our canonical elasticities.

The estimates for other regime changes are about sixty percent smaller, and the within-regime estimates are about 80 percent smaller. In our simulations, even extremely persistent tariff changes ( $\omega = 0.99$ ) have long-run trade elasticity estimates ten percent smaller than our canonical values. We conclude that the typical transitory trade reforms observed in the data are uninformative about the canonical long-run trade elasticity, and that even highly-persistent reforms yield underestimates unless filtered through a structural model.

## 8 Conclusions

We estimate that the long-run trade elasticity to an unanticipated, once-and-for-all tariff change—what we have termed a *canonical* trade reform—is 15. This is five times what one finds when applying standard reduced-form empirical methods to U.S. data and about three times the values commonly used in quantitative work ([Simonovska and Waugh, 2014](#); [Caliendo and Parro, 2014](#)). This large response is hard to find in the data due to the stochastic properties of trade policy and firms’ forward-looking decisions related to anticipation and uncertainty that are not captured by existing empirical methods. Moreover, most previous empirical work focuses on shorter sample windows than the one we consider here.

Our estimated elasticities are recovered from a model-based analysis that leverages key elements of the structure of US trade policy that shifts countries across regimes with different product-specific tariff schedules. We consider two case studies, China and Vietnam, which underwent extremely large liberalizations that were highly persistent ex-post but were characterized by considerable uncertainty ex ante owing to these countries’ unique geopolitical relations with the United States. We model these reforms as two-state Markov processes with time-varying transition probabilities, and jointly calibrate these probabilities together with the technological parameters that govern trade adjustment dynamics.

Our results imply that there is a mismatch between reduced-form estimates of trade elasticities and the structural parameters used in quantitative research. The vast majority of country-specific tariff variation in the data is highly transitory, which implies that the elasticities identified using this variation are much lower than the elasticities that should be used to model canonical trade reforms, which is precisely the focus of most quantitative

research. Progress needs to be made on two fronts. First, the empirical literature should develop methods to estimate trade elasticities that control for anticipation and uncertainty. Our approach uses a structural model and we suspect more progress can be made by further integrating empirics with dynamic trade theory. Second, the quantitative literature should develop calibration strategies to account for the stochastic processes that tariffs follow in the data. Our approach is one example of such a strategy.

Our analysis uses the variation in trade dynamics across products to recover a common stochastic process for trade policy at the country level, leveraging the well-defined, exogenous structure of the United States' NNTR and NTR tariff schedules to specify this process as a non-stationary two-state Markov chain. A useful next step would be to recover the more complicated stochastic processes that govern tariff changes associated with the formation of PTAs, entry to and exit from the GSP program, transitory “within-regime” changes to MFN tariff rates, and other reforms that involve more product-specific outcomes.

If trade policy aims to foster deeper integration, then policymakers must find ways to make it credible. One possibility would be to adopt Milton Friedman’s suggestion of a constitutional amendment setting import tariffs to zero (Friedman and Friedman, 1980), mirroring the Constitution’s proscription on states taxing interstate commerce or imposing export taxes.<sup>39</sup> But if credible commitment to permanent trade reform proves impossible, then neither tariff reductions nor the trade expansions they cause should be expected to endure. This calls into question the value of the static models commonly used in the quantitative trade literature, which are typically interpreted as capturing the long run.<sup>40</sup> If trade policy is inherently dynamic and stochastic, then so too must be our models.

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<sup>39</sup>The commerce clause (Article 1, Section 8, Clause 3) gives Congress the power to regulate commerce between states. The export clause (Article 1, Section 9, Clause 5) states “No tax or duty shall be laid on articles exported from any state.”

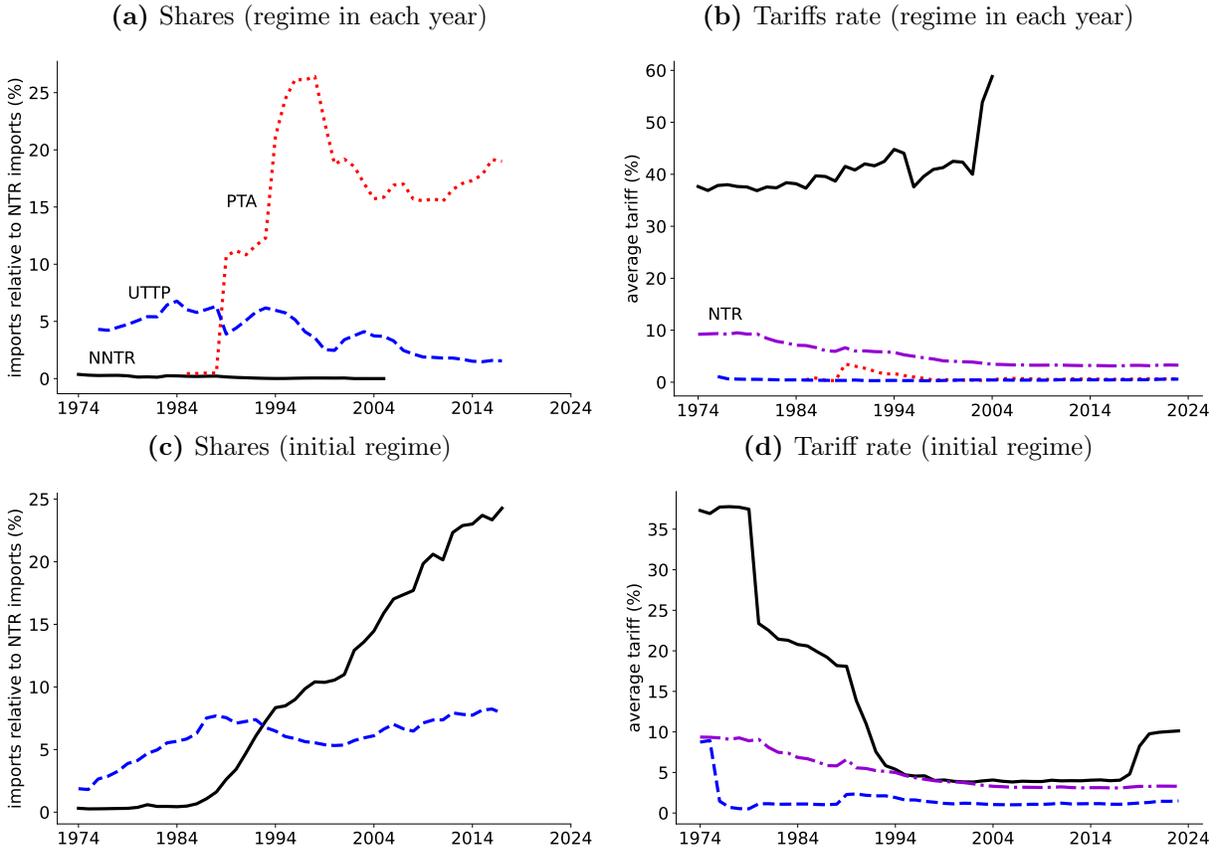
<sup>40</sup>For example, in their review of quantitative trade models Costinot and Rodríguez-Clare (2014) state that static models “aim to capture the long-run consequences of trade liberalization.” In a personal correspondence in 2026, Costas Arkolakis, editor of the *Journal of International Economics*, affirmed that this remains the consensus among international trade scholars.

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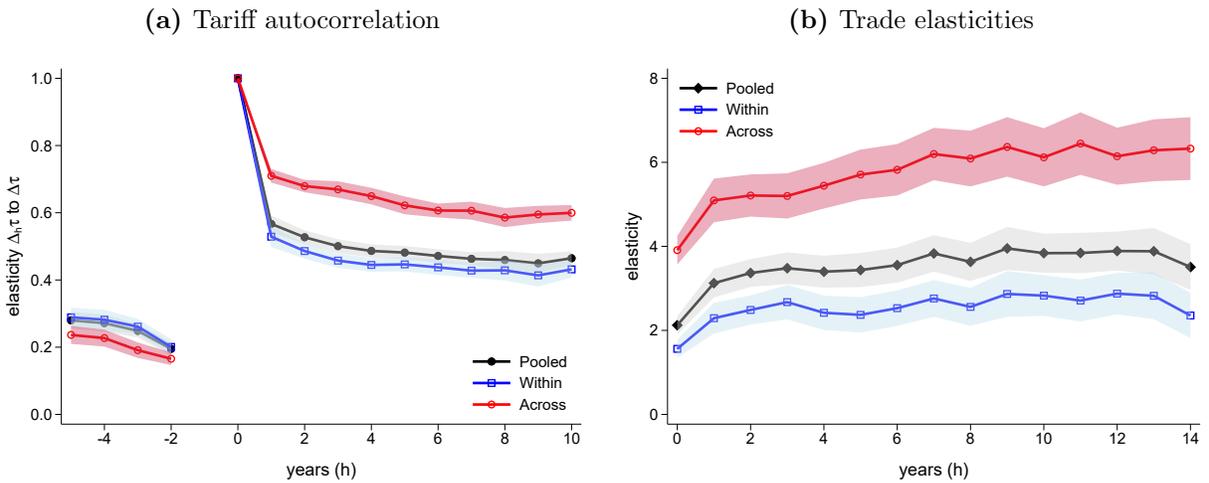
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**Figure 1: Import shares and tariffs by policy regimes**



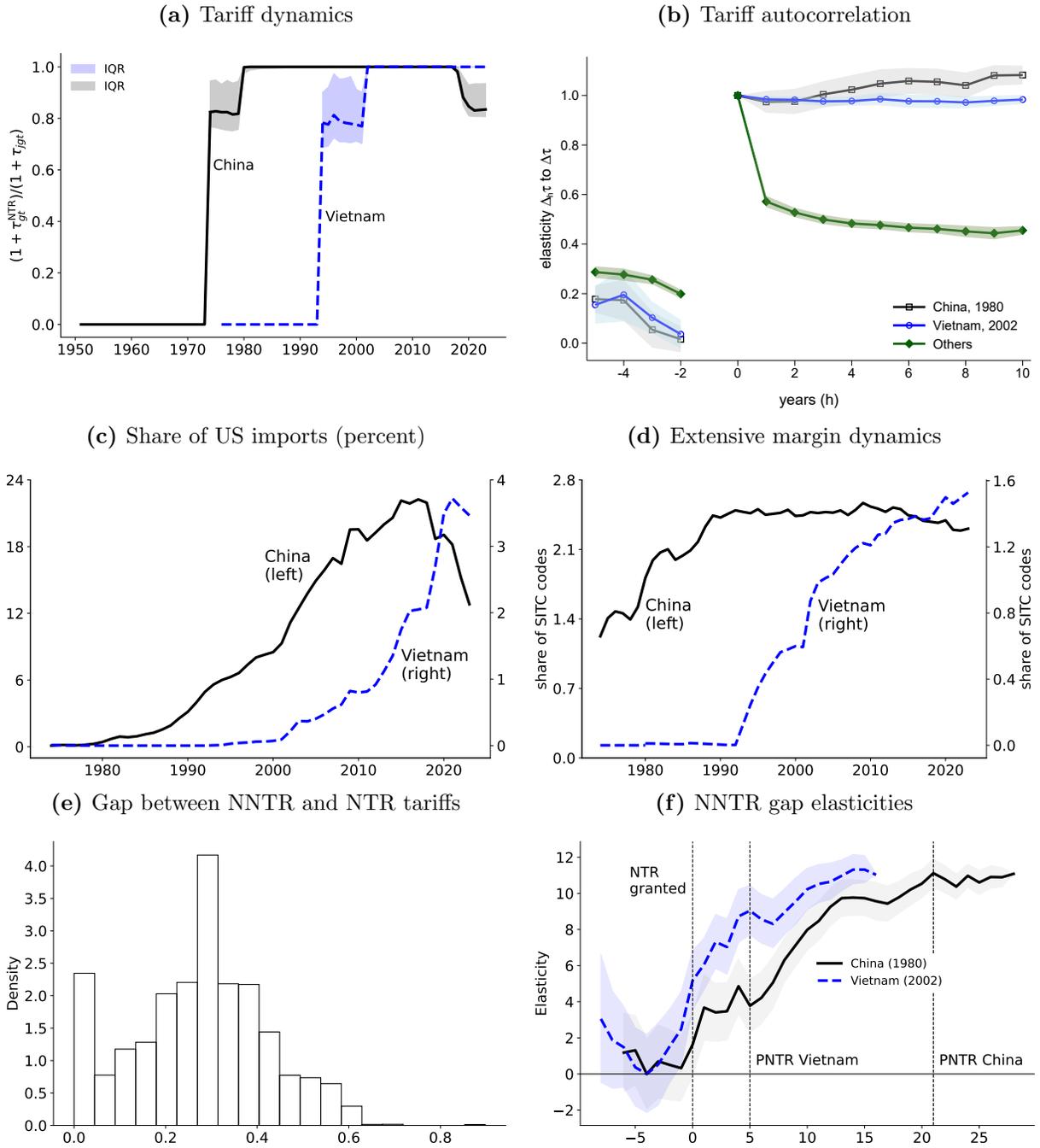
*Notes:* (a) Import share of each trade policy regime described in section 3 after dropping unclassified variety-years. (b) Average tariff rate by regime. (c) and (d) Analogues of (a) and (b) with each variety's regime fixed at its modal regime during 1970s.

**Figure 2: Tariff and trade dynamics: across vs. within regimes**



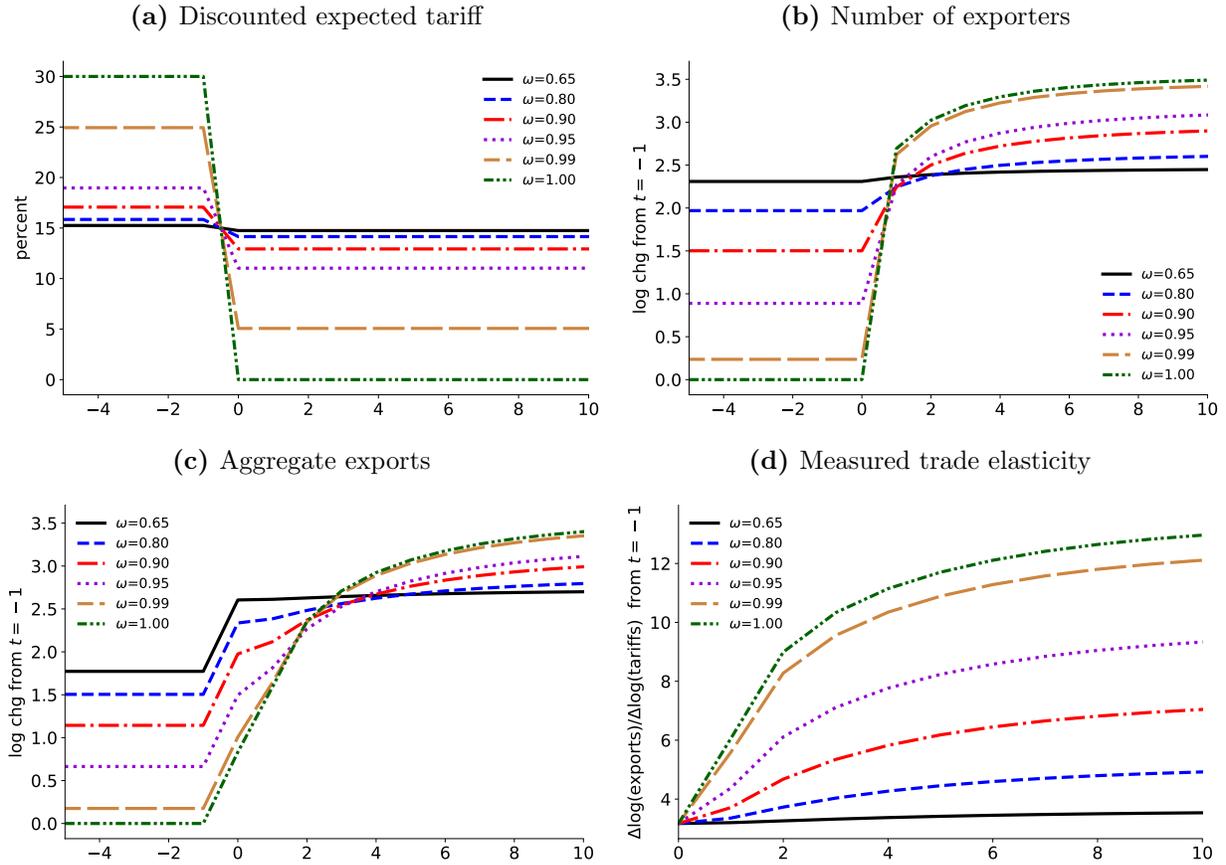
*Notes:* (a) Autocorrelation estimates from (1). (b) Elasticity estimates from (5). 95-pct. confidence intervals constructed using standard errors clustered at the  $jg$  level.

**Figure 3:** Tariff and trade dynamics: China and Vietnam



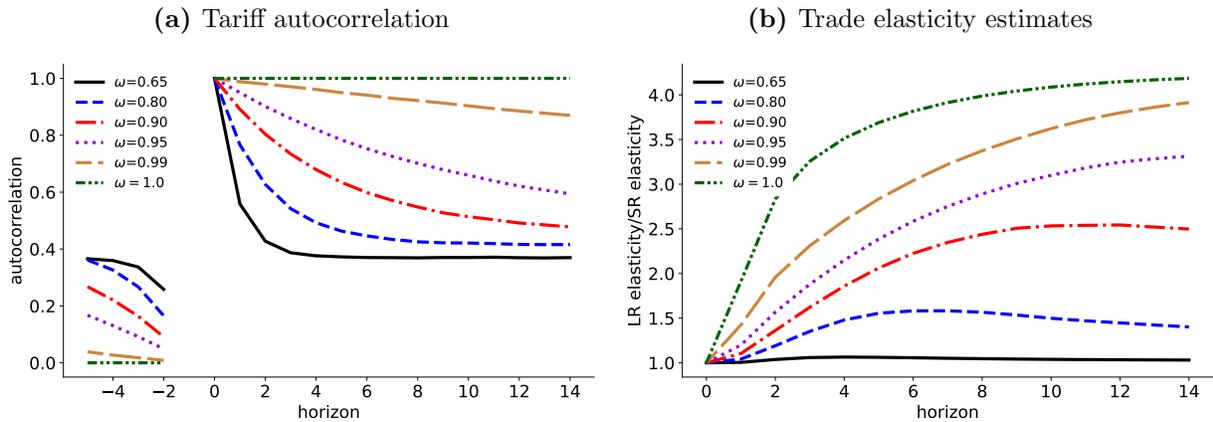
*Notes:* (a) Tariffs on NTR countries relative to tariffs on China and Vietnam. (b) Autocorrelation estimates from (2). (c) Share of US imports from China and Vietnam. (d) Share of SITC goods with positive imports from China and Vietnam. (e) Distribution of gap between NNTR and NTR tariffs in 1999. (f) Elasticity estimates from (7) with 95-pct. confidence intervals constructed using standard errors clustered at the  $ij$  level.

**Figure 4:** Model responses to persistent vs. transitory once-and-for-all reforms



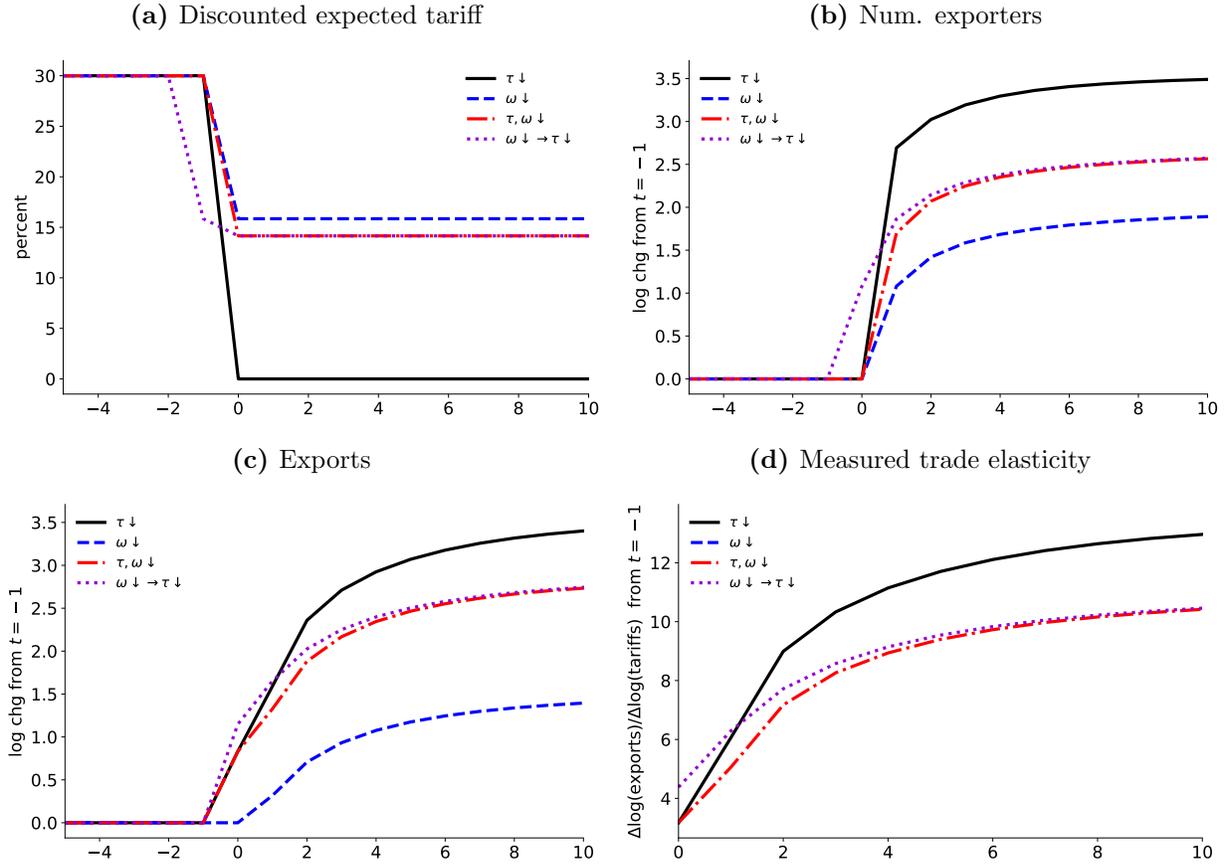
*Notes:* Transition dynamics from trade reform experiments described in section 6.1 in which the persistence of tariffs ( $\omega$ ) varies. (d) measured trade elasticity as defined in (3). (b)–(c) outcomes measured relative to period  $t = -1$  in calibration with perfectly persistent tariffs ( $\omega = 1$ ).

**Figure 5:** Local-projections estimates model simulations with Markov tariffs



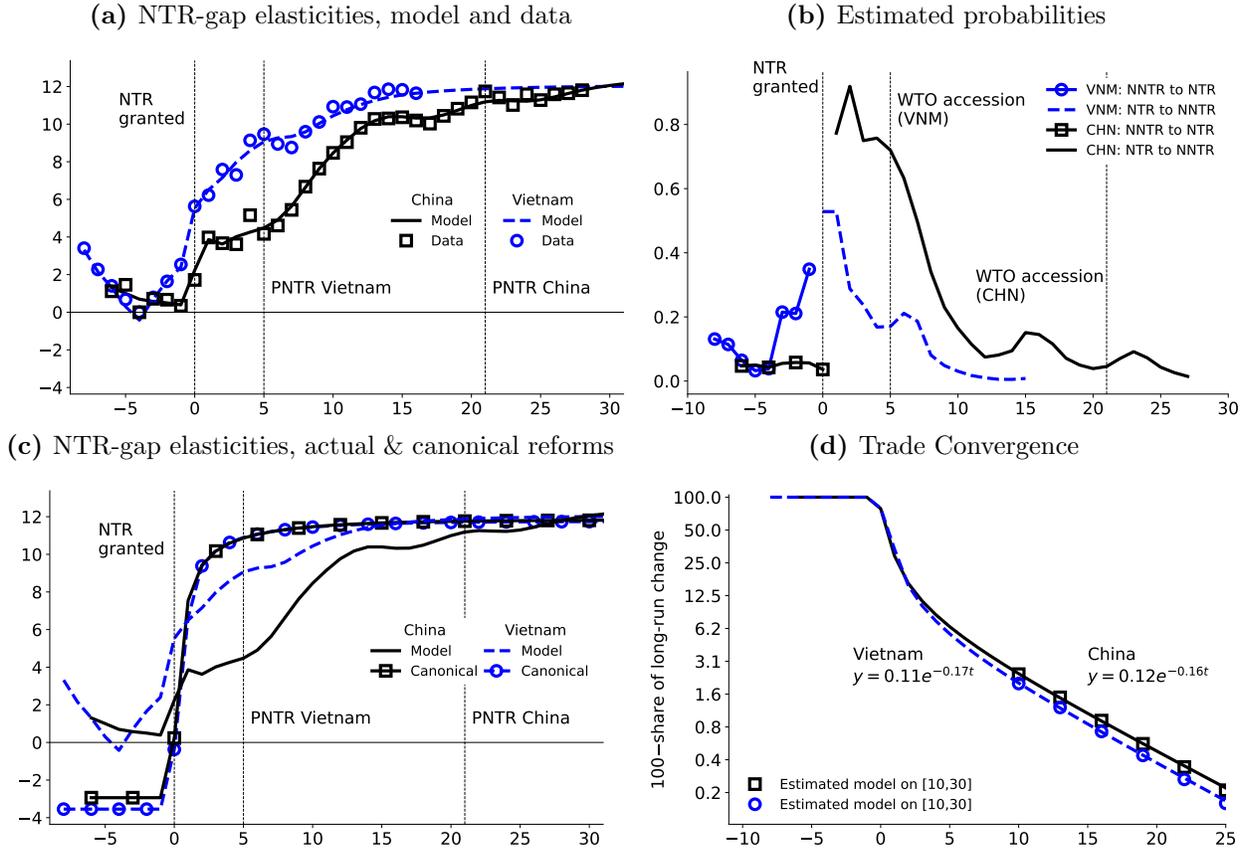
*Notes:* Estimates of tariff and trade dynamics in simulated data from models with different values of tariff persistence  $\omega$ . (a) tariff autocorrelation estimated using (1). (b) trade elasticities estimated using (4), normalized relative to elasticity for  $h = 0$ .

**Figure 6:** Model responses to changes in persistence vs. changes in tariffs



*Notes:* The figure plots the transition dynamics from the trade reform experiments described in section 6.2.  $\tau \downarrow$ : a drop in tariffs in  $t = 0$ .  $\omega \downarrow$ : a drop in persistence in  $t = 0$ .  $\tau, \omega \downarrow$ : a drop in tariffs and persistence in  $t = 0$ .  $\omega \downarrow \rightarrow \tau \downarrow$ : a drop in persistence in  $t = -1$  followed by a drop in tariffs in  $t = 0$ . In panel (d), the measured trade elasticity is defined in (3). In panels (b) and (c), outcomes are measured relative to period  $t = -1$  in the canonical case  $\tau_{HP} \rightarrow \tau_{LP}$ .

**Figure 7:** The NTR liberalizations of China (1980) and Vietnam (2002): Model



*Notes:* Panel (a): NTR-gap elasticities in data and baseline model. Panel (b): Estimated trade-policy transition probabilities. Panel (c) NTR-gap elasticities baseline model and the counterfactual canonical reform. Panel (d) The share of total adjustment remaining in the NTR-gap elasticities from the counterfactual canonical reform, as defined in (??).

**Table 1:** Tariff levels by regimes

Regime	Baseline Sample				Winsorized (99.99)			
	N # <i>jgt</i>	Mean (p.p.)	Median (p.p.)	SD (p.p.)	N # <i>jgt</i>	Mean (p.p.)	Median (p.p.)	SD (p.p.)
NTR	1,758,012	4.87	2.90	14.09	1,757,870	4.82	2.90	6.66
NNTR	19,507	38.61	35.00	22.56	19,423	38.16	35.00	21.07
PTA	97,548	0.78	0.05	2.21	97,545	0.78	0.05	2.08
UTPP	230,683	0.40	0.00	1.11	230,683	0.40	0.00	1.11
Unclassified	34,374	2.06	0.17	7.20	34,366	2.03	0.17	6.92
Total	2,105,750	4.50	1.70	13.58	2,105,521	4.46	1.70	7.38

*Notes:* This table reports some moments of the tariff levels by regime. Tariffs are calculated as the duties collected over FOB import values at the 5-digit SITC aggregation level, our definition of a good. Our classification of regimes is described in detail in Appendix A.1.

**Table 2:** Trade policy regime transition frequencies (percent)

$t - 1/t$		NNTR	NTR	PTA	UTPP
NNTR	$jg$	80.02	18.66	0.00	1.31
NNTR	$j$	89.00	9.29	0.00	1.70
NTR	$jg$	0.05	96.93	0.65	2.37
NTR	$j$	0.33	88.62	0.15	10.90
PTA	$jg$	0.00	8.58	91.42	0.01
PTA	$j$	0.00	0.00	100.00	0.00
UTPP	$jg$	0.01	16.60	0.93	82.46
UTPP	$j$	0.03	9.97	0.49	89.51

*Notes:* The transition frequencies are calculated as the average of the annual transitions between and within regimes over the full sample period at the  $j$  level. Each cell in the annual transition matrix is calculated as the ratio of sum of countries that moved from regime  $R'$  to regime  $R$  between years  $t - 1$  and  $t$  to the total number of countries in regime  $R$  in year  $t - 1$ , where  $R = \{NTR, NNTR, PTA, UTPP\}$ .

**Table 3:** Tariff changes across and within regimes

From	To	1-Year Changes				5-Year Changes			
		N # $jgt$	Mean (p.p.)	Median (p.p.)	SD (p.p.)	N # $jgt$	Mean (p.p.)	Median (p.p.)	SD (p.p.)
NTR	NNTR	466	27.36	26.96	19.02	913	28.03	29.26	14.90
NTR	PTA	10,281	-3.01	-1.80	4.56	16,838	-4.21	-2.70	5.64
NTR	UTPP	29,978	-4.02	-2.90	14.50	26,923	-5.22	-3.83	6.32
NTR	NTR	1,352,360	-0.15	0.00	9.47	901,653	-0.66	0.00	15.80
Tokyo	Tokyo	180,246	-0.45	-0.17	9.66	125,494	-1.81	-0.80	34.46
Uruguay	Uruguay	197,315	-0.25	-0.04	2.46	158,845	-0.84	-0.30	2.87
Non-GATT	Non-GATT	974,799	-0.07	0.00	10.29	442,584	-0.24	0.00	12.18
NNTR	NNTR	10,542	-0.25	0.00	9.25	3,012	-0.44	0.00	10.02
NNTR	NTR	1,523	-27.63	-26.17	24.04	3,343	-31.48	-30.03	21.46
NNTR	UTPP	72	-29.81	-32.93	16.20	735	-36.25	-34.99	18.12
PTA	NTR	8,432	2.47	1.30	3.95	8,911	1.39	0.00	3.57
PTA	PTA	75,876	-0.12	0.00	1.34	48,431	-0.50	0.00	2.07
PTA	UTPP	1	0.00	0.00					
UTPP	NNTR	12	23.04	24.08	17.77	75	34.67	34.96	16.24
UTPP	NTR	30,373	2.89	2.08	3.80	38,307	2.33	1.59	3.41
UTPP	PTA	1,848	0.05	0.00	1.36	7,057	-0.18	0.00	1.56
UTPP	UTPP	150,464	-0.03	0.00	1.04	82,515	-0.06	0.00	1.14
Total		1,672,228	-0.17	0.00	8.92	1,138,713	-0.74	0.00	14.40

*Notes:* Tariff changes are calculated as the average over  $\tau_{jgt} - \tau_{jg,t-h}$  with  $h = \{1, 5\}$ . For the 1-year change, GATT-Tokyo are averages for the years 1980-1987 and GATT-Uruguay 1995-2000. For the 5-year change, GATT-Tokyo are averages for the years 1984-1992 and GATT-Uruguay 1999-2004.

**Table 4:** Exporter-dynamics statistics and sector-level model parameters

<i>(a) Common assigned parameters</i>						
Parameter		Value		Target/Source		
$\theta$	Demand elasticity	3.17		Soderbery (2018)		
$r$	Interest rate	0.04		Standard		
$\rho_z$	Productivity persistence	0.65		Alessandria et al. (2021)		
$\delta_0$	Constant exit rate	21		Alessandria et al. (2021)		
$\delta_1$	Elasticity of exit to productivity	0.02		Alessandria et al. (2021)		
$\rho_\xi$	Trade cost transition persistence	0.92		Alessandria et al. (2021)		
<i>(b) Country-specific jointly calibrated parameters</i>						
Parameter		China	Vietnam	Target/Source	China	Vietnam
$f_0$	Sunk cost	0.49	1.29	Export part. (%)	28	11
$f_1$	Export cost	0.28	0.53	Exit rate (%)	11	15
$\xi_H$	High iceberg cost	4.34	6.63	Incumbent prem.	2.9	4.41
$\sigma_z$	Productivity shock dispersion	1.47	1.67	Log CV exports	2.27	2.91

**Table 5:** Long-run trade elasticities: canonical vs. reduced-form

<i>(a) Canonical reforms and event studies</i>		
Setting	China	Vietnam
Canonical reform	14.76	15.26
Event study	11.80	11.86
Event study (rescaled w/ discounted expected tariffs)	15.65	15.34
<i>(b) Across vs. within regimes (section 4.1)</i>		
Sample	Estimate	
Across regimes	6.33	
Within regimes	2.35	
<i>(c) Numerical examples (section 6.1)</i>		
Persistence ( $\omega$ )	Estimate	
0.65	3.24	
0.80	4.13	
0.90	6.84	
0.95	10.07	
0.99	12.93	

*Notes:* (a) Model simulation of canonical reform and NTR-gap elasticities from (7), both unadjusted and rescaled by ratio of change in mean discounted expected tariff to change in mean applied tariff. (b) Local-projections estimates from (5). (c) Local-projections estimates from model simulations with two-state Markov processes.

**Table 6:** The Path to PNTR/WTO for Vietnam and China

<i>(a) Timelines</i>				
Key Event	Year of Action		Time Since Lifting Embargo	
	China	Vietnam	China	Vietnam
Embargo	1950	1975	−21	−15
Embargo Lifted	1971	1994	0	0
Diplomatic Relations Established	1979	1995	8	1
First Jackson–Vanik Waiver	1979	1998	8	4
Bilateral Trade Agreement	1980	2001	9	7
PNTR Vote	2000	2006	29	12
Application to GATT/WTO	1986	1995	15	1
Final JV Waiver Vote	2001	2006	30	12
Entry into WTO	2001	2006	30	12

<i>(b) Jackson-Vanik waiver votes</i>				
Vote	China		Vietnam	
	Disapprove	Approve	Disapprove	Approve
1	247	174	163*	260*
2	223	204	130*	297*
3	258	135	91*	332*
4	105	318	91*	324*
5	75	356	91	338
6	107	321		
7	141	286		
8	173	259		
9	166	264		
10	170	260		
11	147	281		
12	169†	259†		

<i>(c) BTA and PNTR votes</i>				
	China		Vietnam	
	Disapprove	Approve	Disapprove	Approve
BTA (House)	88	294	9	409
BTA (Senate)	8	74	12	88
PNTR (House)	197	237	184	212
PNTR (Senate)	15	83	9	79

*Notes:* China Jackson-Vanik waiver votes started in 1990, ten years after BTA was approved. The final vote occurred after PNTR had been approved (denoted by †). For Vietnam, the first Jackson-Vanik waiver votes were before the BTA was approved (denoted by \*). There was only one more vote in the year following the BTA, while in other years votes did not make it out of committee.

# Appendix (For online publication)

Sections A–C present robustness checks, model experiments on anticipation and uncertainty, and additional empirical validation.

## A Data

### A.1 Regime classification

Here we describe our regime classification used in sections 3 and 4. To classify each triplet *jgt* into one of the four regimes, we incorporate information from the rate of provision and country subcode aggregation level of the census trade data. In particular, we classify *jgt*'s into each of the regimes when at least 50 percent of the import value of the triplet is attributed to that regime. We define regimes at the *jgt*-rate of provision-country subcode level as follows:<sup>41,42</sup>

- *NTR*: Rates of provision 10, 61 or country subcodes C, L, and K.
- *NNTR*: Rates of provision 62.
- *PTA*: Country subcodes from Table A2 or rates of provision 18, 19, or 64 for country-years in Table A2.
- *UTPP*: Country subcodes from Table A1 or rates of provision 18, 19, or 64 for country years with at least one observation under country subcode A, A\*, A+ or 4 (GSP).

This procedure allows classifies 98.4 percent of observations. Panel a of Figure A1 plots the import share and median applied tariffs for the unclassified *jgt*'s. It indicates that these represent a small fraction of imports and are mostly characterized by low tariffs until 2017, when the United States raised some tariffs on a set of goods and, especially, on imports on

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<sup>41</sup>Note the provision rate codes have changed over time. Using the Annual Import Data Bank Files between 1974 and 1989 from the National Archives we concord previous provision rates to the current ones. For instance, before 1989, provision rates distinguished ad valorem, specific, compound, and minimum rates, e.g. dutiable rates prescribed by column 2 tariffs were coded as 22 for specific rates, 32 for ad valorem rates, 42 for compound rates, and 52 for minimum rates. Currently all of these would fall under the provision rate 62 and are classified as the NNTR regime under our regime classification.

<sup>42</sup>For the current list of rates of provision and country sub-codes visit <https://www.census.gov/foreign-trade/reference/codes/rp.html> and <https://www.census.gov/foreign-trade/reference/codes/csc.html>, respectively.

China, was mostly carried out under provision rates 69 and 79, which are not used by any of our 4 regimes. This justifies our focus on the data until 2017.

The imports of most of the  $jgt$  triplets that are classified according to one of our four regimes are completely characterized by their regime. This can be seen in Panel b of Figure A1, which is a plot of the import share of the attributed  $jgt$  regime. The import share of the regime is always very close to 1, especially in the case of the NTR and NNTR regime. The fact that the regime of some  $jgt$ 's does not account for all imports is mostly due to product aggregation.

To clarify our classification, consider for example electric motors incl. universal A.C./D.C. motors (SITC code 71621) imported from Canada in 1990. We classify this triplet as “PTA”. However, the “PTA” import share of this  $jgt$  is only 65 percent, with the “NTR” share being 34 percent and 1 percent unclassified. Table A3 shines some light on this: There are 20 HTS-8 goods associated in this SITC code, of which some provide lower than NTR rates under the US-Canada FTA while others already have zero tariffs under the NTR regime.

## A.2 Robustness: Within vs. across tariff and trade dynamics

Our estimates of tariff autocorrelations in section 3.3 and reduced-form trade elasticities in section 4.1 are robust to a range of alternative estimation strategies and data samples that we describe below. The results are shown in Figures A2–A3.

**By regime transition** While in the baseline we focused on the distinction of across and within-regime transition, here we report the results of (1) and (5) when we consider specific regime transitions. In particular, we show the estimates for transitions from NNTR to NTR/UTPP, from NTR to PTA, and from NTR to UTPP, as well as the estimates for  $jg$ 's that were always entering under the NTR regime. Panel (a) of Figure A2 plots the autocorrelation of tariffs and panel (b) the results of the reduced-form trade elasticities. It confirms that within NTR tariffs are less persistent and lead to less substitution.

**Tariff autocorrelations with less restrictive fixed effects** In our baseline estimation of tariff autocorrelations, we include both country-time ( $jt$ ) fixed effects that absorb average tariff changes within countries over time, and good-time ( $gt$ ) fixed effects that absorb average

tariff changes within goods across countries. Figure A3(a) shows tariff autocorrelations estimated with only good-time ( $gt$ ) fixed effects and no fixed effects at all. In both alternatives, the within-regime autocorrelations barely change, while the across-regime changes become more persistent. Thus, our more restrictive fixed-effects structure yields a conservative estimate of the difference in persistence between across vs. within tariff changes.

**Definition of regimes** Our baseline classification into trade policy regimes considers that  $jgt$ 's are classified into one of the four regimes if 50 percent of its imports entered under the corresponding rate of provision and/or country subcode, as described in A.1. Here we consider two robustness checks. First, we restrict the import share to be 90 percent instead of 50 percent. Second, we consider only the first transition of any  $jg$  over our sample period, addressing potential concerns of pre-trends. The results, reported in Figure A3(b), indicate that across-regime elasticities increase slightly when we restrict the imports to 90 percent, while focusing on the first transition only has a minimal impact on the estimates, although it yields more precise estimates (standard errors are not shown).

**Estimation specification** Our baseline estimation of the reduced-form trade elasticities largely follows the local projection approach of Boehm et al. (2023), but the differences between across-regime vs. within-regime elasticities remain under alternative specifications. First, we consider a direct application of (3) to the data, regressing changes in trade after  $h$  periods on changes in tariffs after  $h$  periods while using fixed effects to control for confounding covariates (the  $h$ -on- $h$  specification):

$$\Delta_h v_{jgt} = -\beta_h^{hh} \Delta_h \tau_{jgt} + \mathbf{Z}_{jgt} + \delta_{jt} + \delta_{gt} + u_{jgt}. \quad (18)$$

where  $\Delta_h x_t = x_{t+h} - x_{t-1}$  and  $\beta_h^{hh}$  is the estimator of the  $h$ -horizon trade elasticity under the  $h$ -on- $h$  approach.

Our second alternative specification (the *ECM* specification), is a version of the error correction model (Gallaway et al., 2003; Alessandria and Choi, 2021) which recovers a parametric estimate of the long-run trade elasticity by specifying trade flows as an AR(1) process al-

lowing for lagged effects of tariffs:

$$\Delta_0 v_{jgt} = \beta_0^{ecm} \Delta_0 \tau_{jgt} + \gamma(v_{jg,t-1} - \beta_\infty^{ecm} \tau_{jg,t-1}) + \delta_{gt} + \delta_{jt} + \delta_{jg} + u_{jgt}, \quad (19)$$

The dependent variable is the one-year log difference in import values. The right-hand side includes the one-year change in tariffs, lagged tariffs, lagged log imports, and a set of fixed effects. The short-run, elasticity,  $\beta_0^{ecm}$ , is the coefficient on the contemporaneous tariff change. The long-run elasticity,  $\beta_\infty^{ecm}$ , is pinned down by the ratio of the coefficient on lagged tariffs to the coefficient on lagged imports. We add variety fixed effects,  $\delta_{jg}$ , to capture each variety’s average import level.<sup>43</sup>

Figure A3(c) compares the estimates under the *h-on-h* and the *ECM* approach to our baseline estimates. Panel (a) reports the results for the across- and within-regime elasticities. In the case of the across-regime elasticities the estimates are very similar. In contrast, in the case of the within-elasticities, the alternative specifications give higher estimates, especially the *h-on-h* specification. This indicates that Boehm et al. (2023)’s approach of controlling for the autocorrelation in the tariff process is critical for identification when that process has a low degree of persistence. Nevertheless, the differences between across- and within-regime elasticities, which is our main result in this part of the paper, are large under all specifications. Panel (b) plots the results for China and Vietnam. For China, the 1-year and 14-year elasticities are very similar under the three specification, although the *ECM*-specification misses out on the very slow adjustment path captured by the local projections. In the case of Vietnam, the long-run *h-on-h* and *ECM* elasticities are slightly larger.

**Alternative sample designs** Our baseline sample design excludes *jj*’s that were targeted by temporary trade barriers, such as antidumping duties, at some point throughout our sample period, as well as observations that are not classified by our regime classification. Here we show that our results are robust to this choice by re-estimating (5) with the full sample. Results are robust to excluding main trading partners from the sample, as is in Boehm et al. (2023). Note, as in Boehm et al. (2023), we define main trading partners the

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<sup>43</sup>This is warranted because, in contrast with the previous two specifications, *jj* fixed effects do not cancel out with the use of changes on the left hand side since once can rewrite the ECM specification in levels as  $v_{jgt} = \beta_0^{ecm} \tau_{jgt} + (1 + \gamma)v_{jg,t-1} - (1 + \gamma\beta_\infty^{ecm})\tau_{jg,t-1} + \delta_{gt} + \delta_{jt} + \delta_{jg} + u_{jgt}$ .

$jk$ 's that account for the largest import share of good  $g$  in period  $t$  and  $t - 1$  as well as the largest trading partner  $j$  overall in periods  $t$  and  $t - 1$ .<sup>44</sup> Figure A3(d) illustrates that both considerations have minimal effects on the reduced-form elasticities.

**Tariff measurement** Our baseline measurement of tariffs considers the weighted average of applied tariffs of the HS-8/TS-USA tariff lines at the 5-digit SITC level. Here we show that our results are very similar if we instead consider the simple average or the median over the HS-8/TS-USA tariff lines. Figure A3(e) plots the results of (5) with these different tariff measures. The differences between the across- and within-regime elasticities become slightly larger, as do Vietnam's, while China's remain mostly unchanged.

**Controlling for pre-trends.** In the baseline specification we control for pre-trends of import growth and tariffs. Figure A3(f) plots the results of (5) when we only control for lagged import growth and when we don't include any pre-trends at all. While the point estimate tend to decrease slightly in both cases and for all groups, their differences with respect to the pooled estimates and the differences with respect to each other remain largely unchanged.

**Good-level aggregation.** Our empirical results are based on goods defined by the 5-digit SITC (revision 2) classification. A potential concern with this good-level is that it is more aggregate than the tariff lines, which before 1989 were defined at the 7-digit TS-USA classification and afterwards according to the 8-digit HTS classification.<sup>45</sup> Given the lack of concordance between these two classifications, we perform the following robustness exercise. We calculate the dispersion in tariffs within SITC goods at the country-year level and then focus on the subsample of goods with relatively small dispersion. Precisely, we construct two measures of dispersion. First, we calculate the variance at the most disaggregate level possible, including the provision rate, country subcodes and district of entry and using monthly data. We then decompose this variance into the within- and across-TS-USA/HS products. We then re-estimate our results in Sections 3 and 4 using the standard deviation of the across-TS-USA/HS tariff variation, using thresholds of 2 percentage points (of tariffs). Our

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<sup>44</sup>The rationale for this exclusion discussed in Boehm et al. (2023) is that major trading partners were generally the ones negotiating the MFN rate reductions in the WTO's multilateral negotiation rounds.

<sup>45</sup>While using tariff lines avoids biases from aggregating over goods with different tariffs, it also exacerbates biases related to zero trade flows and the extensive margin response.

second approach is to calculate the annual weighted tariffs at the tariff line level and then consider the standard deviation for each SITC-country-year. Again, we consider a subsample of goods with a standard deviation below 2 percentage points.

The results of the autocorrelation of tariff changes are reported in Figure A3(g). The first approach corresponds to  $SD^1$  and the second to  $SD^2$ . The path of the autocorrelation of tariff changes is virtually unchanged when goods with dispersion in tariffs within SITC goods are excluded from the estimation. Figure A3(h) shows the results of estimating the reduced form trade elasticity under (5). Noticeably, both, the across- and within-regime elasticities increase, indicating that the aggregation level may lead to downward bias. Nevertheless, both elasticities increase by a similar amount, remaining statistically different throughout the estimated horizon, with a difference of around 2 to 3 log points.

**Tariff measurement concerns.** A possible concern is that measurement error in the applied tariffs at the SITC level might be driving our results. Here we show that when we trim the largest absolute tariff changes from our sample, the differences between the across- and within-regime elasticities remain significant and, if anything, grow in the long-run. Figure A3(i) and A3(j) plot the results of (1) and (5) when we trim the top 0.01%, 0.05%, and 0.1% of absolute tariff changes. While both elasticities tend to increase as more large tariff changes are dropped, their difference remains virtually unchanged.

**Supply factors.** Our baseline empirical analysis uses U.S. imports and 5-digit SITC goods in order to span the sample to cover the most important U.S. trade policy regime changes. A downside of this approach is that is challenging to control for supply shocks at the exporter-good level (e.g. productivity) using exporter-good-time fixed effects. We argue in the main text that due to the United States' outsized importance in the global economy, both as a buyer of exported goods and the geopolitical leader coordinating policy, including these fixed effects is not innocuous. Here we show the main results hold up well with this approach.

We use the World Trade Flows database by Feenstra et al. (2005) (1974-2000) and the BACI trade dataset (2001-17) (Centre d'Études Prospectives et d'Informations Internationales, 2023). This data poses two measurement concerns, which is why it is not our baseline sample. First, its is the more aggregate 4-digit SITC level, which reduces the number of

goods from around 2,000 to around 1,000, and also creates more potential for aggregation-related measurement error when constructing our applied tariff measure.

Second, this dataset does not contain data on duties paid needed to compute applied tariffs (it only includes trade flows). This means that we must still use the duties paid measure in our baseline dataset that originates from the US census. This measure is only available for the US, which means that we can only estimate elasticities of trade to US tariff changes; the data on other countries' imports are used only to control for supply factors in this estimation. Moreover, in some cases the trade flows reported in the world trade data are inconsistent with those reported in the US census data, which occasionally results in very large changes in applied tariffs that do not appear in the baseline data. We deal with this by trimming the largest tariff changes. Our results are not sensitive to the trimming threshold.

On both the importer ( $i$ ) and exporter ( $j$ ) dimensions, we include each of the top 50 countries based on exports in 2000, and a rest-of-the-world aggregate of the remaining countries. We estimate the following equation that allows us to control for supply factors:

$$\begin{aligned} \Delta_h v_{ijgt} = & -\beta_h^{v,\text{within}} \Delta_h \tau_{jgt} \{\text{regime}_{jgt} = \text{regime}_{jgt-1} \wedge i = \text{US}\} \\ & - \beta_h^{v,\text{across}} \Delta_h \tau_{jgt} \{\text{regime}_{jgt} \neq \text{regime}_{jgt-1} \wedge i = \text{US}\} + \mathbf{Z}_{ijgt} + \delta_{jgt} + \delta_{igt} + u_{ijgt}. \end{aligned} \quad (20)$$

where  $\delta_{jgt}$  are source-good-time fixed effects that control for supply shocks common across destination markets, and  $\delta_{igt}$  are destination-good-time fixed effects that control for demand shocks common across import sources. The results are plotted in Figure A3(k). The long-run trade elasticities are very close to our baseline estimates. The across-regime elasticity is statistically indistinguishable from our baseline estimate from horizon 8 onward. The same is true for the within-regime elasticity, except for  $h = 14$ . Thus, the main result—that across-regime tariff changes generate much larger trade responses than within-regime changes—is highly robust to controlling for supply factors.

Interestingly, the short-run elasticities for across vs. within tariff changes are more similar in this exercise than in our baseline results (in fact, for  $h = 0$  they are statistically indistinguishable). This implies that the trade growth for across-regime changes is more gradual than in our baseline estimation. We believe this is likely a result of the higher level of aggregation in

the world trade data. The lower the level of aggregation, the less scope there is for gradual adjustments. To see this, go the other way and suppose one were to use firm-level data with positive trade flows. In this case, there is no extensive-margin effect at all; the only scope for gradual adjustment is firm-level intensive-margin growth (which in our model is driven by switching stochastically from high to low iceberg costs). We believe that this suggests our 5-digit SITC level of aggregation may offer a good tradeoff between scope for gradual adjustment and potential for aggregation-related measurement issues (which we dealt with in several ways above).

### A.3 Robustness: China and Vietnam NTR access

Our approach to estimate the gap-elasticities laid out in section 4.2 and which we use in section 7 are robust to a range of alternative estimation strategies which we describe below. The results are shown in Figures A4–A5.

**Measurement of the gap** In the baseline we consider a common gap for China and Vietnam,  $X_g = \log(1 + \tau_g^{NNTR} - \tau_g^{NTR})$ , where we consider the simple average over the HS-8 1999 scheduled NNTR and NTR rates. We consider the following robustness checks to this choice. First, we consider the median over the HS-8 tariff lines. Second, we consider the 2001 rates. Third, we consider the applied NNTR and NTR rates at the time of the NTR access of each country, that is 1980 for China and 2002 for Vietnam. To calculate the average applied NNTR rates we consider the years 1974–1979 for China and 1994–2001 for Vietnam. We calculate the NTR rates using the two years after the access.<sup>46</sup> The results shown in Figure A4(a) and A4(b) show the gap-elasticities are quite similar.

**Alternative sample designs** Our baseline excludes goods subject to the Multi Fibre Agreement quotas since these goods faced non-tariff trade barriers. As robustness, we further exclude goods that were affected by temporary trade barriers such as antidumping duties. We also consider an approximate version of a balanced sample of goods, that is goods that were traded before the two countries’ respective NTR access. Finally, we consider a sample with only NTR countries (defined at the *jgt*-level) to allow a closer control of changes in MFN

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<sup>46</sup>Note, this definition of the gaps requires goods to be traded both before and after the access, thus we separate the gap-elasticities for China and Vietnam separately under this specification.

rates. The results are shown in Figure A4(c) and A4(d). None of these changes significantly affects our estimates.

**Gap elasticities vs. local projections** In the main text we focus our case-study analysis on elasticities of trade to the NNTR gap following Alessandria et al. (2025a). Here we show estimates for China and Vietnam using two local projections specifications similar to (5). The first specification estimates local projections specifically to NTR access:

$$\begin{aligned} \Delta_h v_{jgt} = & -\beta_h^{v,\text{CN}} \Delta_h \tau_{jgt} \{j = \text{China}, t = 1980\} - \beta_h^{v,\text{VN}} \Delta_h \tau_{jgt} \{j = \text{Vietnam}, t = 2002\} \\ & - \beta_h^{v,\text{others}} \Delta_h \tau_{jgt} \{j = \text{other}\} + \mathbf{Z}_{jgt} + \delta_{jt} + \delta_{gt} + u_{jgt}. \end{aligned} \quad (21)$$

These estimates are normalized to zero in the year before NTR access, and we re-normalize our gap-elasticities the same way to be consistent. Figure A5(a) shows that in the case of China, these LP estimates are closely aligned with the gap elasticities.

In the case of Vietnam, however, Figure A5(b) shows the LP estimates significantly understate the extent of long-run trade growth due to the anticipatory growth that occurs before NTR access. The value of the long-run LP estimate from (21) is similar to the long-run value of the gap elasticity, but the former only captures the growth in trade from the period before NTR access, which is two full log points higher than several periods earlier; recall that the long-run response of trade in the event-study specification is the difference between the highest gap elasticity (at the end of the sample) and the lowest (which occurs in period  $-4$ ). This shows that our event-study approach does a better job of dealing with this anticipatory behavior than the local-projections specification.

The second specification estimates local projections for the average tariff change:

$$\begin{aligned} \Delta_h v_{jgt} = & -\beta_h^{v,\text{CN}} \Delta_h \tau_{jgt} \{j = \text{China}\} - \beta_h^{v,\text{VN}} \Delta_h \tau_{jgt} \{j = \text{Vietnam}\} \\ & - \beta_h^{v,\text{others}} \Delta_h \tau_{jgt} \{j = \text{other}\} + \mathbf{Z}_{jgt} + \delta_{jt} + \delta_{gt} + u_{jgt}. \end{aligned} \quad (22)$$

This specification yields lower long-run trade elasticities than the first, especially for Vietnam. This is because the average tariff change for China and Vietnam is much less persistent than the changes associated with NTR access. Figure A5(c) illustrates this by plotting the

autocorrelation of the NTR-access tariff changes for China and Vietnam against the autocorrelation of these countries' other tariff changes. The latter look very similar to within-regime tariff changes discussed in the main text.

**Good-level aggregation.** As described in [A.2](#), our empirical results are based on goods defined by the 5-digit SITC (revision 2) classification. Here we show that using the subsample of SITC-country-years for which the dispersion in tariffs at the tariff line is below a threshold does not affect our baseline estimates of the gap-elasticities.<sup>47</sup> Figures [A4\(e\)](#) and [A4\(f\)](#) plot our baseline results in comparison to those when using subsamples that exclude observations with a standard deviation above 1, 2, and 3 percentage points under the first approach (using most disaggregate data) and above 2 percentage points under the second (using weighted tariffs at the tariff line level). In the case of Vietnam, estimates under these subsamples are nearly identical to the baseline. In the case of China, estimates are around 10-20 percent higher in the early period, with the peak elasticity going from around 11 to around 13.5.

**Supply factors.** As discussed in section [4.3](#), the economic and political might of the United States makes it particularly challenging to appropriately control for supply factors when considering the major reforms China and Vietnam underwent in 1980 and 2002, respectively. Nevertheless, here we illustrate that our results hold up well when accommodating controls for supply factors using the same world trade dataset as in section [A.2](#) above. Again, we consider the top 50 exporters in 2000 and the rest of the world, but exclude countries with which the United States signed a Preferential Trade Agreement before 2000 as well as countries receiving NNTR treatment. We then estimate the gap-elasticities controlling for supply factors as follows:

$$\begin{aligned}
 v_{ijgt} = & \sum_{t'=1974}^{2008} \beta_t^{v,\text{CN}} \mathbb{1}\{t = t'\} \mathbb{1}\{j = \text{China} \wedge i = \text{USA}\} X_g \\
 & + \sum_{t'=1994}^{2017} \beta_t^{v,\text{VN}} \mathbb{1}\{t = t'\} \mathbb{1}\{j = \text{Vietnam} \wedge i = \text{USA}\} X_g + \delta_{jgt} + \delta_{igt} + u_{ijgt},
 \end{aligned} \tag{23}$$

where  $\delta_{jgt}$  control for good-level supply shocks and  $\delta_{igt}$  controls for good-level demand shocks.

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<sup>47</sup>See the description of our dispersion measures in [A.2](#).

Figure A4(g) and A4(h) plot the result for China and Vietnam, respectively. In the figure, we also include the estimate of our baseline (7) when considering U.S. imports only in this sample. This allows us to disentangle the supply factor effects from the effects of using a more aggregate good (4- vs. 5-digit SITC). While the elasticities are not exactly equal to our baseline estimates, the supply factors do not seem to play an important role in determining the adjustment path to the lifting of the embargo and the NTR access. This can be seen by comparing the path of the estimates with US imports only (“4-digit SITC: US imports only”) and the one with supply factors (“4-digit SITC & Supply factors”), which are a very close throughout.

**Simultaneous reforms for Vietnam.** Here we show that controlling for supply factors is dubious for Vietnam’s NTR access because many other countries aside from the US significantly liberalized trade with Vietnam during the same period. We do this by looking at the distribution of midpoint growth in Vietnamese exports between 1991 and 1995. For a given destination-product pair  $(i, g)$ , midpoint growth is calculated as

$$\frac{\text{exports}_{i,g,1995} - \text{exports}_{i,g,1991}}{0.5 (\text{exports}_{i,g,1995} + \text{exports}_{i,g,1991})} \quad (24)$$

A value of 2 indicates that  $\text{exports}_{i,g,1991} = 0$ , i.e., country  $i$  had an embargo on Vietnam in 1991. Figure A6 (a) shows the distribution of destination-good midpoint growth for  $i = \text{USA, CAN, JPN, AUS, and EU-12}$ . Panel (b) shows the distribution of destination-level midpoint growth (calculated at the  $i$  level, not the  $i, g$  level) for all countries. In both cases, 2 is the most common value consistent with broad coordinated trade policy reforms on Vietnam.

## B Model

We now use the model to illustrate some additional principles related to anticipation and uncertainty, and measure the sensitivity of our canonical elasticity to parameters.

### B.1 Initial conditions and Temporary Lifting of Embargo

In the numerical illustrations in section 6, the economy was in a neighborhood of the steady state at the time of reform, where tariffs had been high (but finite) for many periods. In

China and Vietnam the transition from NNTR to NTR takes place while the economies are transitioning from the end of the embargo. To illustrate how the economy adjusts to lifting an embargo, and the implication of this adjustment for trade-elasticity measurement, we consider a scenario where the economy begins in embargo and then opens up permanently. We also consider a case in which there is a constant probability of returning to the embargo.

To align with our empirical approach, we apply these reforms for two goods that permanently go to different tariff levels of zero and 30 percent. We also estimate the trade elasticity from these two goods using a panel approach,

$$v_{gt} = \sum_{t'=1}^{25} \beta_{t'} \mathbb{1}\{t = t'\} \log(\tau_{gt}) + \delta_t + u_{gt}. \quad (25)$$

**Initial Conditions** Figure A7(a) plots trade dynamics following the embargo's end. Figure A7(b) shows that the good going to zero tariffs starts with more trade and exporters than the good going to 30 percent tariffs and the gap between the two goods grows over time. On impact, we find a large trade elasticity of about 10 from comparing the differences in trade across the two goods to the difference in tariffs, but this elasticity is smaller than the canonical elasticity of about 15 that we estimated in the paper. As trade grows further in the subsequent periods, the trade elasticity eventually converges to that canonical value. Importantly, five years after lifting the embargo we have only closed about two-thirds of the gap between the first year differences and the long-run differences across goods.

**Return to Embargo Possibility.** We also do a version of this experiment where there is a 10% chance of returning to embargo. For both goods, the level of trade when the embargo is lifted is lower and there is less adjustment over time. The trade elasticity still starts about at about 10, but does not grow in magnitude as much as in the case without uncertainty.

To connect with the empirical discussion about the how country-year fixed effects absorb part of the adjustment from the end of the embargo, we plot the path of the trade elasticity without the time effect  $\delta_t$ . Now, it starts out much larger in magnitude when the embargo is lifted, and shrinks over time instead of growing. As discussed above, this is similar to what we observe in the data for China and Vietnam and in our modeling of those episodes.

## B.2 Anticipation to phased-in reforms

Aside from the changes between NNTR and NTR/UTPP, many tariff changes involve a phase-out, particularly with GATT rounds and PTAs. We now discuss how trade evolves with these types of tariff reforms through the lens of our model and show that short-run and long-run trade elasticities can differ quite substantially from those from a canonical reform.

To study how anticipation affects trade adjustment dynamics, we consider several variations on the canonical scenario. We consider two kinds of variations: anticipated versus unanticipated and immediate versus phase-outs. Unanticipated reforms are announced in the period in which they occur, whereas anticipated reforms are announced ten periods in advance. In immediate reforms, tariffs change by 30 p.p. in one period, and in tariff phase-out reforms, tariffs fall by 3 p.p. per year for 10 years. The linear phase-out is a key feature of trade agreements, including the GATT and FTAs. In each variation, firms learn the entire path of tariffs when the reform is announced. Thus, phased-in reforms always feature some anticipation of future tariff cuts, even when the start of the reform is a surprise. None of the reforms considered in this section feature uncertainty about future tariffs. Figure A8 shows the results of these experiments. There are three main takeaways.

First, anticipation causes exports to rise before tariffs fall, increasing the short-run trade elasticity,  $\varepsilon_0$ . When the reform is anticipated, the value of exporting increases, which causes export participation to rise. When tariffs fall, aggregate exports increase from the intensive-margin effect and the pre-reform export entry. Compared with an unanticipated reform, anticipated reforms feature more trade in  $t = 0$ . Thus, the measured short-run trade elasticity is higher than the short-run canonical elasticity.

Second, phased-in reforms cause exports to grow slower than immediate reforms, but lead to higher trade elasticities in the short and medium run. When tariffs fall gradually, the value of exporting and the export participation rate rise gradually. But, exports rise faster, relative to the rate at which tariffs change, than in immediate reforms. Export entry depends on the entire path of future tariffs, not just the current tariff rate, and firms anticipate future tariff cuts in phased-in reforms. In unanticipated phased-in reforms, the trade elasticity in period  $t = 0$  is equal to the demand elasticity,  $\theta$  (as in the canonical reform), but in period  $t = 1$  it

jumps above its long-run value. In anticipated and phased-in reforms, the trade elasticity is high even in period  $t = 0$  and exhibits similar, albeit more muted, non-monotonic behavior. Third, anticipation does not affect the long-run level of trade but does affect the measured long-run trade elasticity. In all four reforms, export volumes converge to the same level, but each reform has a different long-run trade elasticity. This is because the level of trade in the period before the reform begins is higher when the reform is anticipated. This is particularly true in the anticipated & immediate reform, where more than three-quarters of the long-run growth in trade occurs before tariffs change at all, which results in a much lower long-run trade elasticity than in the other versions of the experiment. If we measure the change in trade relative to the announcement date, shown in panel (e), the long-run elasticity is the same across experiments. Empirical approaches that attribute this anticipatory growth to pre-trends will generate even smaller trade elasticities.

### B.3 Sensitivity to the demand elasticity $\theta$

We study the effects of the demand elasticity,  $\theta$ , on the policy transition probabilities and long-run canonical elasticity that we recover in our quantitative exercise.

While we have externally assigned  $\theta$  based on [Soderbery \(2015\)](#)'s estimate, the path of the NTR-gap elasticity in the data actually contains some identifying information about this parameter. If  $\theta$  is too low, even the canonical reform would generate less long-run trade growth than we observe in the data. As discussed in the main text of the paper, the level of the NTR-gap elasticity during the NNTR regime is influenced by the probability of gaining NTR access—a lower probability means less imports of goods with large NTR gaps relative to imports of low-gap goods. If  $\theta$  is sufficiently low, the difference between imports of high- and low-gap goods during this period in the model is too small even when the probability of NTR access is zero. Our experiments show that this lower bound is around 2.5.

Alternatively, if  $\theta$  is too high, the growth in exports of high-gap goods relative to low-gap goods when NTR access is granted in the model is too high, even if the probability of losing NTR access is 100% so that there is no change in export participation at all and this growth in trade is generated entirely from the  $\theta$ -driven intensive margin. There are some arguments for spreading this growth across two periods rather than one (e.g. China got access to MFN

rates in February of 1980, so there was less than a full year’s intensive-margin-driven trade growth), but even when one does this,  $\theta$  cannot possibly be higher than about 4.3.

We explore the sensitivity of our results to  $\theta$  in a re-calibrated model—both the technological parameters governing trade dynamics in the canonical reform and the trade-policy transition probabilities needed to generate the observed dynamics—using two alternative values for  $\theta$  from the literature that are close to this range’s upper and lower bounds. Our lower alternative value is 2.7, [Khan and Khederlarian \(2021\)](#)’s preferred estimate of the short-run response to NAFTA. Our upper alternative value is 4.1, [Simonovska and Waugh \(2014\)](#)’s preferred estimate and one of the most widely used values in quantitative trade modeling.

Figure [A9](#) shows the results of these sensitivity analyses for China and Vietnam. The first column shows the NTR-gap elasticity dynamics in the canonical reforms in the baseline calibration versus the alternatives, and the second column shows the estimated trade-policy transition probabilities. With the low  $\theta = 2.7$ , the NTR-gap elasticity in the canonical reform (green) during the NNTR phase is very close to the actual data, which requires a lower probability of gaining NTR access, especially for China, where the NTR access probability is less than 1 percent throughout the 1970s. Conversely, with the high estimate of  $\theta = 4.1$ , the NTR-gap elasticity in the canonical reform (red) during the NNTR phase is much larger, requiring a high probability of gaining NTR access. Once MFN is achieved, these alternative calibrations are close to the baseline, both in the dynamics of the NTR-gap elasticity in the canonical reform and the probability of going back to NNTR status.

The long-run canonical elasticity in the low- $\theta$  calibration is 12.83 for China and 13.51 for Vietnam, compared to the baseline elasticities of 14.67 and 15.29. The long-run elasticities for the high- $\theta$  calibration are 18.53 and 19.52.

## B.4 Sensitivity to other parameters

Here we investigate the sensitivity of the long-run canonical elasticity to the remaining parameters. For each parameter, we simulate the canonical reform when that parameter is set 10% higher (lower) than the baseline value. We do not recalibrate the entire model as in our sensitivity analysis to  $\theta$  above. Table [A5](#) shows the results.

**Entry cost  $f_0$ .** The canonical elasticity is increasing in the entry cost, but the effect is

small. An increase in the entry cost reduces export participation, but the effect is greater when export profits are lower, when the country is in the NNTR regime. Consequently, an increase in the entry cost amplifies the extensive-margin effect of a tariff cut.

**Continuation cost  $f_1$ .** The canonical elasticity is decreasing in the continuation cost, but again the effect is small. The reason is as follows. When the continuation cost is higher, the exit rate is higher, and firms are less likely to survive to reach the low-iceberg state  $\xi_L$ . A smaller mass of low-iceberg exporters means a smaller trade response.

**High iceberg cost  $\xi_H$ .** The canonical elasticity is increasing in  $\xi_H$ . The sensitivity here is a bit greater than to the fixed costs. The logic is that this parameter increases the firm-level long-run response to a tariff reform, conditional on making it to the low-iceberg state.

**Iceberg switching probability  $\rho_\xi$ .** This parameter can have a non-monotonic effect on the canonical elasticity: increasing or decreasing this parameter can lower the long-run response of trade owing two opposing forces:

- “Arrival” factor: how soon you move to  $\xi_L$  after entering with  $\xi_H$ . Decreasing in  $\rho_\xi$ .
- “Durability” factor: how long you enjoy  $\xi_L$  once you attain it. Increasing in  $\rho_\xi$ .

At very low value of  $\rho_\xi$ , arrival is fast, which makes exporting more attractive, but  $\xi_L$  spells are short, which makes exporting less attractive. Conversely, at a very high value of  $\rho_\xi$ ,  $\xi_L$  spells are very long, making entry more attractive, but also take a long time to begin, making entry less attractive. Intermediate values of  $\rho_\xi$  balance these two effects.

For both countries, the canonical long-run elasticity is quite sensitive to  $\rho_\xi$ . For China, the effect is indeed non-monotonic: increasing  $\rho_\xi$  lowers the canonical long-run elasticity quite a lot, and reducing it lowers the elasticity a small amount. For Vietnam, the effect is monotonic: increasing  $\rho_\xi$  lowers the elasticity, and increasing  $\rho_\xi$  raises it.

**Productivity shock dispersion  $\sigma_z$ .** The canonical elasticity is decreasing in  $\sigma_z$ . A more dispersed productivity shock distribution puts less mass near the entry threshold, making export participation less sensitive to tariff changes. The effect here is also fairly large.

**Productivity persistence  $\rho_z$ .** The canonical elasticity is decreasing in productivity persistence as well. The effect here is very large. The reason is the same as with  $\sigma_z$ . A higher

$\rho_z$  makes the ergodic productivity distribution more dispersed.

**Fixed death rate  $\delta_0$ .** This parameter has a negligible effect on the canonical elasticity. For China, the effect is positive but very small. For Vietnam, it is also quite small.

**Death Elasticity to productivity  $\delta_1$**  The canonical elasticity changes little with this parameter. For China, the effect is negative but small. For Vietnam, it is again quite small.

## C Model & data: Adjustment from embargo

The country-year fixed effects,  $\delta_{jt}$ , are typically included in trade-elasticity estimation frameworks. In our case studies, they do not just absorb aggregate shocks in exporting countries, they contain some useful information about the response of trade to the lifting of the U.S. embargoes on the two countries. To demonstrate this, we estimate versions of (7) without these effects, instead including other independent variables that more directly control for aggregate shocks without absorbing the effects of these reforms. Specifically, we estimate:

$$v_{jgt}/AS_{jt} = \sum_{t'=1974}^{2008} \beta_t^{v,\text{CN}} \mathbb{1}\{t = t'\} \mathbb{1}\{j = \text{China}\} X_g + \sum_{t'=1994}^{2017} \beta_t^{v,\text{VN}} \mathbb{1}\{t = t'\} \mathbb{1}\{j = \text{Vietnam}\} X_g + \delta_{jg} + \delta_{gt} + u_{jgt}. \quad (26)$$

where  $AS_{jt}$  are aggregate supply factors which are measured as total exports excluding exports to the U.S.<sup>48</sup> Note that with respect to (7) the  $jt$  fixed effects are dropped. In our baseline estimation (7) these fixed effects control for aggregate supply factors; however, as emphasized here they also capture the gradual adjustment to the lifting of the embargo. By dividing good-level exports by  $AS_{jt}$  we intend to control for the former but not for the latter.

Figure A10 shows the results. When country-year fixed effects are excluded and no other controls for aggregate shocks are added (the blue lines), the gap elasticities are larger in magnitude at the beginning of the sample as the elasticities now include the effects of productivity growth, population growth, etc. on exports to the United States, in addition to the effects of trade reforms. When we directly control for aggregate shocks that are

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<sup>48</sup>Results are similar if we use GDP or total exports including to the U.S.

unrelated to U.S. trade policy but still exclude country-year fixed effects (the red lines), the gap elasticities shrink substantially. In both cases, the red line starts well below the baseline elasticity (in black) and rises over time throughout the NNTR period. This is because the red line includes the growth of trade generated from the end of the embargo.

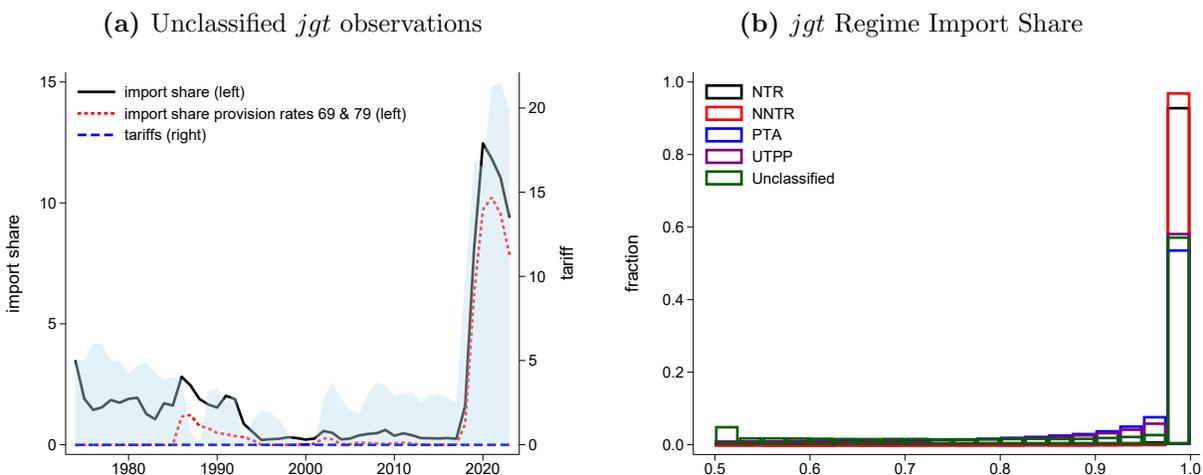
For China, the red line remains below the baseline elasticity throughout the sample. The red dashed line shows the gap elasticity estimate for China without country-time fixed effects in the model. It qualitatively mimics the pattern seen in the data, but it is not shifted downward as much relative to the black line as in the data. Experimenting with our model, we found that one way to explain this is that with a non-zero probability of returning to embargo from the NNTR state. This reduces the growth of trade stemming from the end of the embargo, and while this affects all goods, it disproportionately affects goods with low (and especially zero) NNTR tariff rates. These goods are not meaningfully exposed to the risk of losing NNTR status, and therefore do not contribute to the baseline gap elasticity.

Our experiments show that the larger the probability of returning to embargo, the larger the gap between the black and red lines. We can estimate this probability to match the red line from the data. The last panel shows the probability of embargo is low during the NNTR period, but rises to almost 20 percent shortly after NTR access is granted. It falls back near to zero throughout most of the late 1980s and early 1990s, before rising temporarily around WTO accession. The effects on the other probabilities are minor. Thus, our main results are not sensitive to allowing for a chance of returning to embargo.

For Vietnam, the red line is further below the black line during the NNTR period relative to China. This is because the end of the embargo is closer in time; for Vietnam, our first observation is the year after the embargo ended, whereas for China it is three years later. However, the red line rises more swiftly than in the Vietnamese case and is statistically indistinguishable from the black line starting a few years before NTR access. Even with no chance of returning to embargo, our model's version of the red line is slightly below the empirical estimate. This implies returning to embargo was less likely for Vietnam than for China, which we believe accords with our description of geopolitical events.

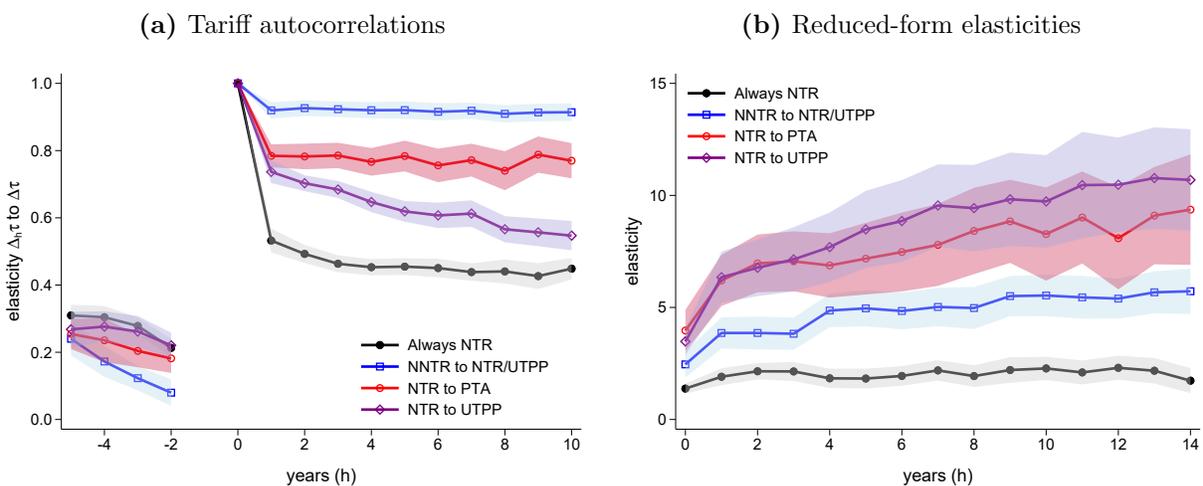
## D Additional figures

**Figure A1: Regime Classification**



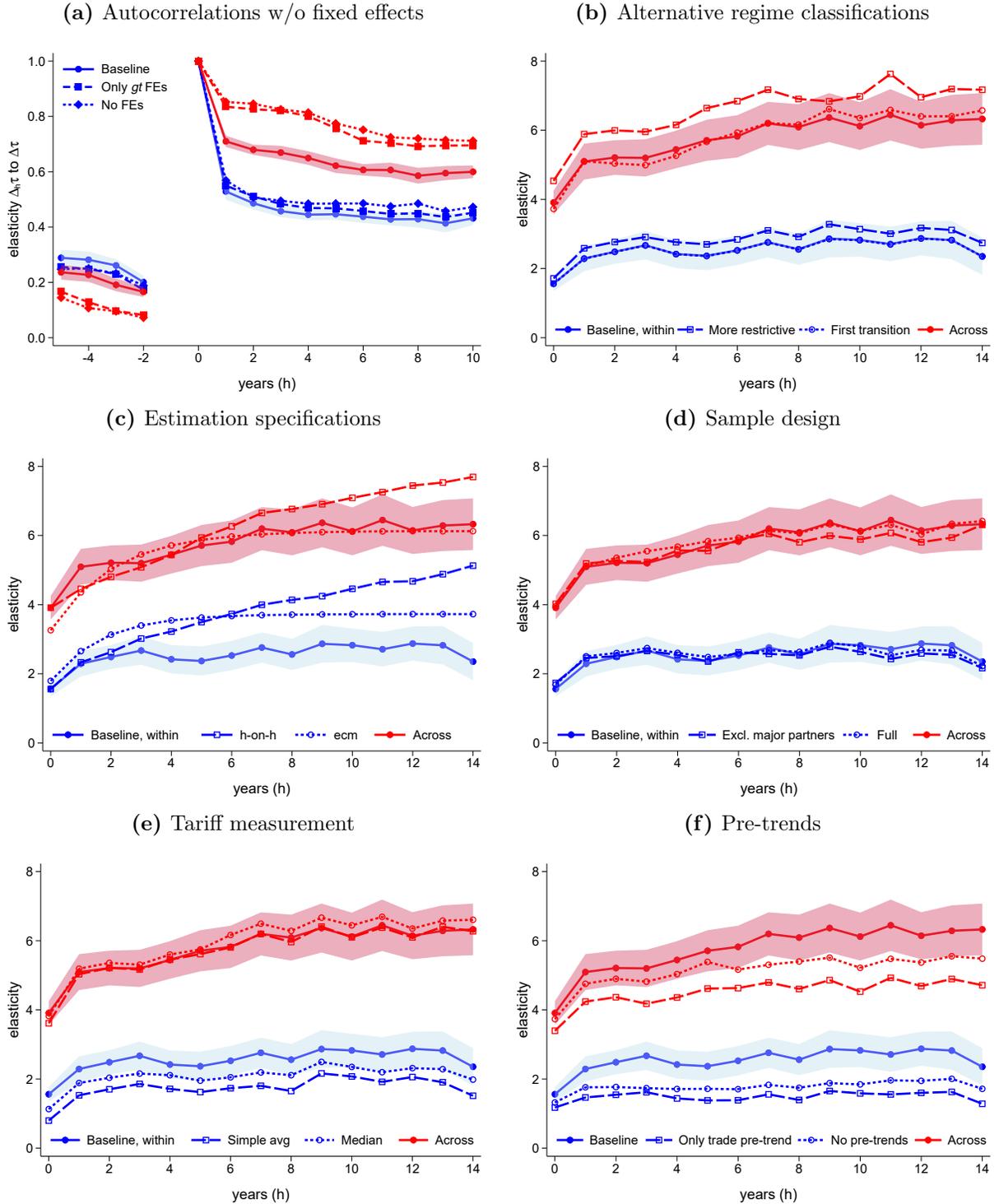
*Notes:* (a) import share and tariffs of unclassified observations (*jgt*'s). Also plots share specific to provision rates 69 and 79 excluded by our classification (dotted red line). The blue dashed line plots the median tariff and the shaded area is the 25<sup>th</sup> and 75<sup>th</sup> percentiles of the tariff distribution. (b) Distribution of import shares of the classified *jgt*'s (by regime) vs. unclassified observations. For illustrative purposes, we truncate unclassified *jgt*'s at 0.5.

**Figure A2: Tariff and trade dynamics across specific regime changes**



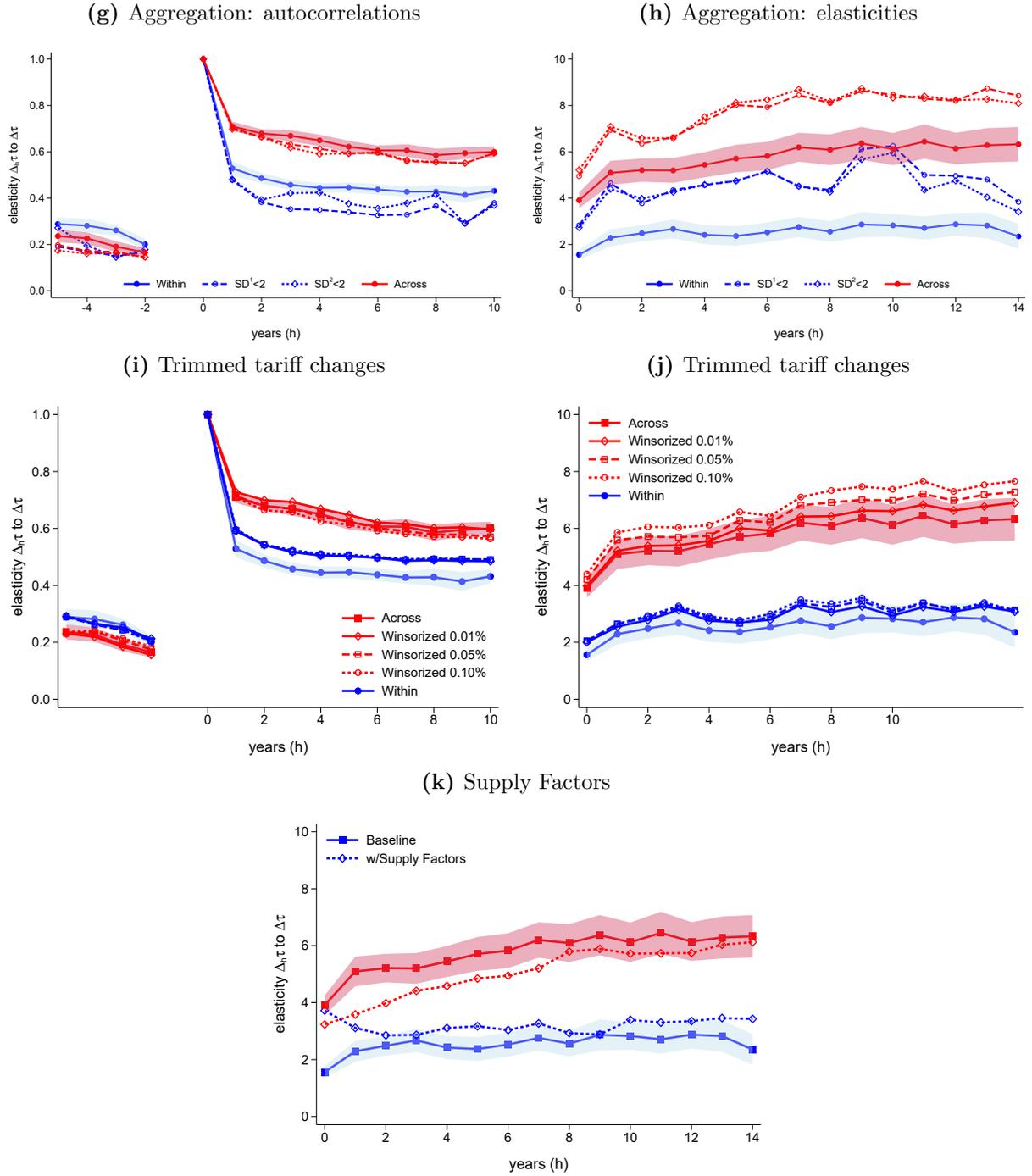
*Notes:* (a) Results of (1) when considering the regime transitions specified in the legend. (b) Same, but for (5). Shaded areas show 95-pct. confidence intervals constructed using standard errors clustered at *jg* level.

**Figure A3:** Robustness: within-vs. across tariff and trade dynamics



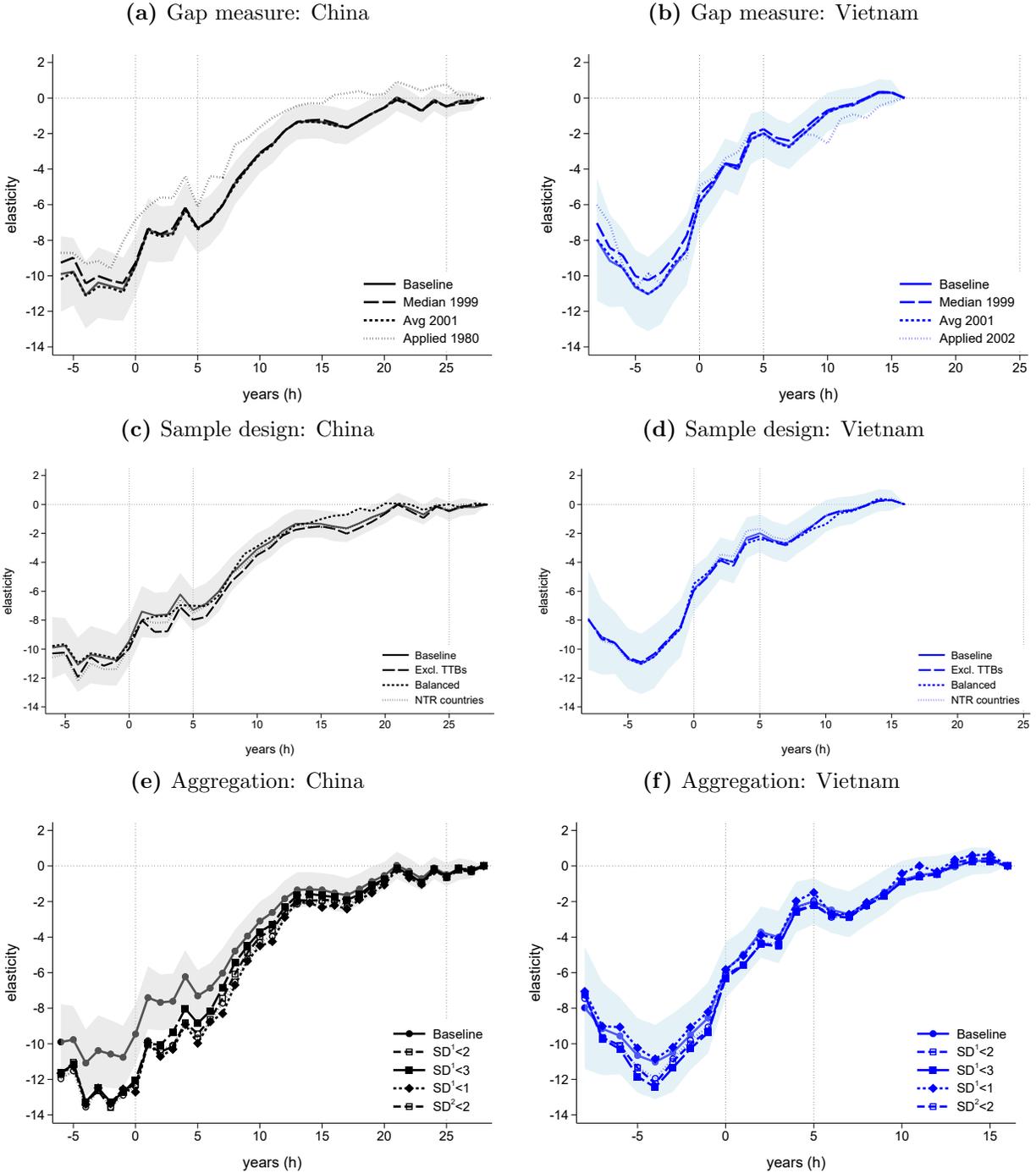
(a) Estimates of (1) with different fixed-effect specifications. (b) Estimates from (5) using alternative regime classifications. (c) Estimates using alternative regression specifications. (d) Estimates using alternative sample designs. (e) Estimates using alternative tariff measures. (f) Estimates with fewer pre-trend controls.

**Figure A3:** Robustness: within-vs. across tariff and trade dynamics, continued



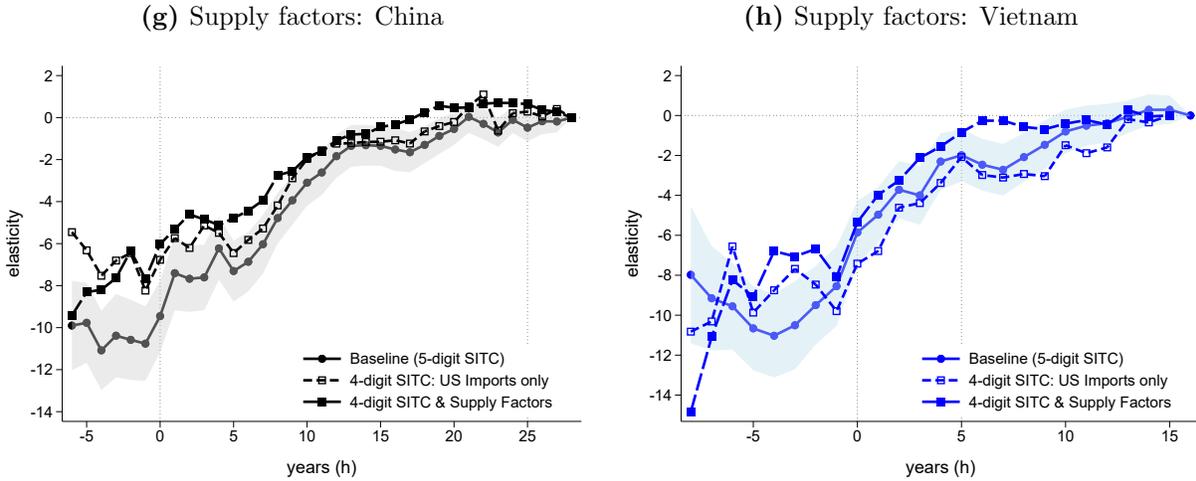
(g) Estimates of (1) excluding observations for which the standard deviation of tariffs (at more disaggregate levels) is above the thresholds indicated in the legends. (h) does the same estimating the trade elasticity under (5). (i) and (j) provide estimates of (1) and (5), respectively, when the sample excludes absolute tariff changes of the top 0.01, 0.05, and 0.1 percentiles. (k) estimates the trade elasticity using the dataset with global supply factors.

**Figure A4: Robustness: Gap elasticities**



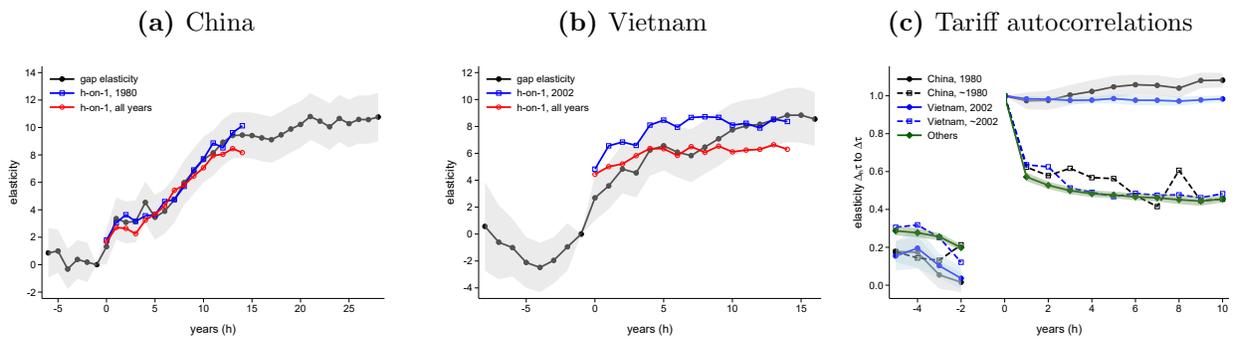
*Notes:* (a) and (b) Estimates from (7) under baseline approach (solid line and shaded area for the confidence interval) vs. alternative gap measurements. (c) and (d) Estimates of (7) under baseline approach vs. alternative sample designs. (e) and (f) Estimates of (7) under baseline approach vs. subsamples excluding observation for which the tariff line tariff variance is higher than the indicated thresholds. The period  $h = 0$  is 1980 for China and 2002 for Vietnam.

**Figure A4: Robustness: Gap elasticities, continued**



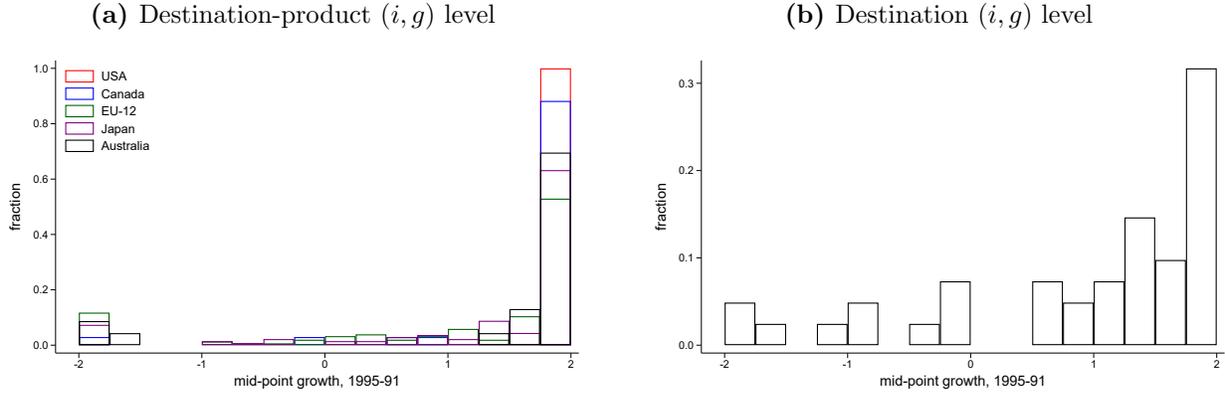
*Notes:* (g) and (h) Estimates from (23) using the 4-digit SITC level dataset with bilateral trade flows. The period  $h = 0$  is 1980 for China and 2002 for Vietnam.

**Figure A5: Gap elasticities vs. local-projections estimates**



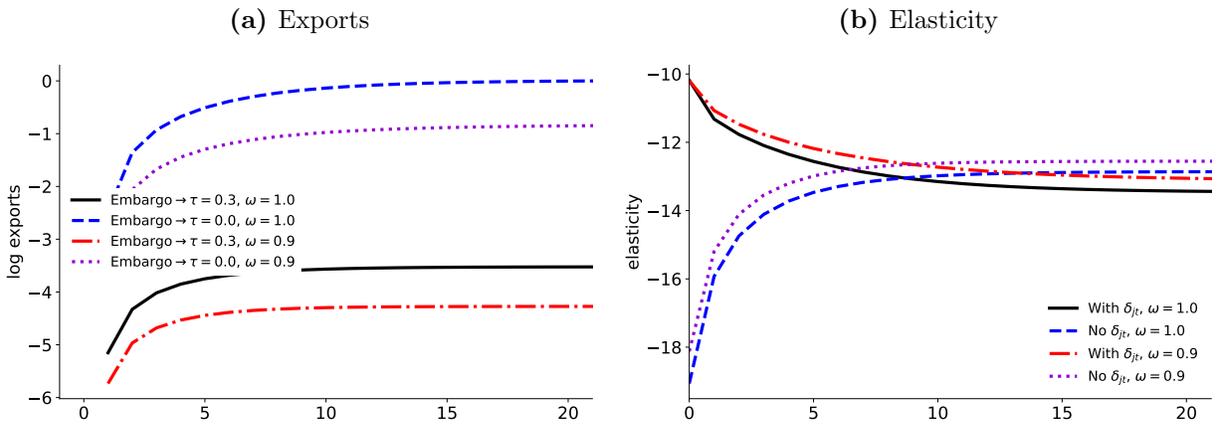
*Notes:* (a) and (b) plot the gap-elasticities estimated by (7) when normalizing to the period prior to the NTR access ( $h = -1$ ). Blue line plots local-projections estimates from (21) focusing on changes from the NTR access. Red line plots local-projections estimates from (22) that average over all years. Period 0 is 1980 in (a) and 2002 in (b). (c) plots estimates of (2) from Figure 3(b) together with estimates when considering only the NTR access years (1980 for China and 2002 for Vietnam) and when excluding those.

**Figure A6:** Distribution of midpoint growth in Vietnamese exports between 1991–1995



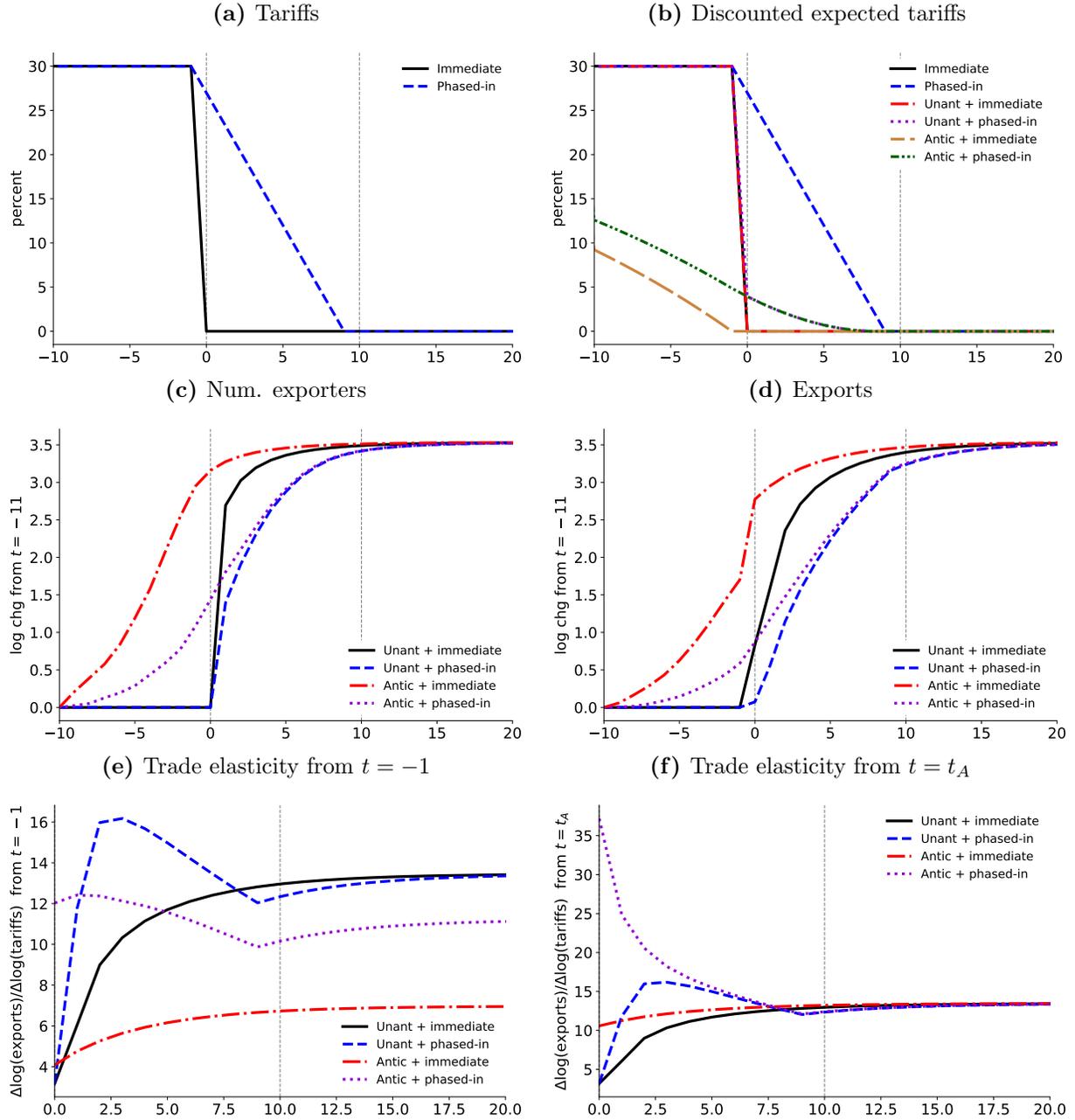
*Notes:* (a) Histogram of destination-good level midpoint growth of Vietnamese exports between 1991 and 1995 for US, Canada, Japan, Australia, and the EU-12 countries. (b). Histogram of destination-level midpoint growth of Vietnamese exports between 1991 and 1995 for all countries.

**Figure A7:** Role of initial conditions: Dynamics from an embargo's end



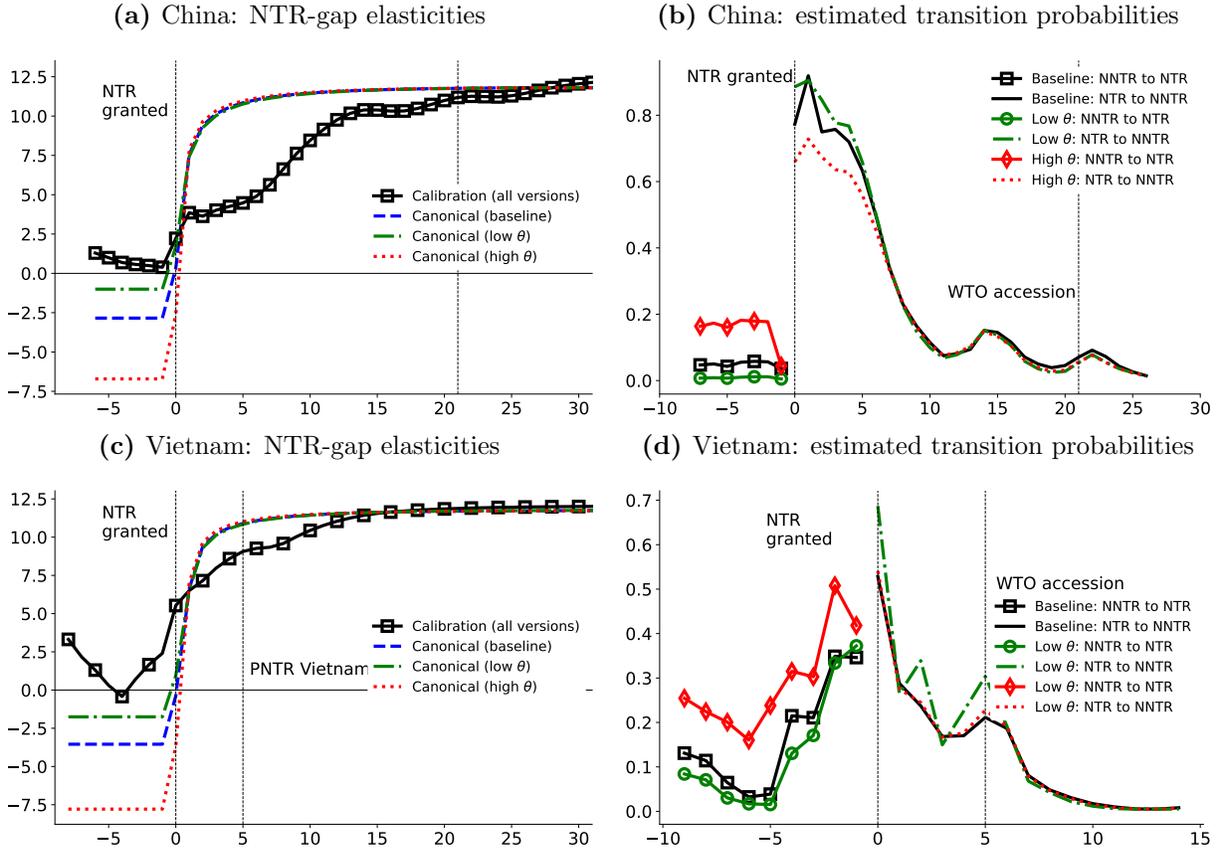
*Notes:* The figure plots the transition dynamics following the end of an embargo for two goods, one that goes to low tariffs and another to high tariffs. There are two cases, one where the embargo ends for good and another where there is a 10% chance of it restarting. Panel (a): Log exports. Panel (b) Time-varying elasticity of trade to tariffs, with and without time fixed effects.

**Figure A8:** Responses to anticipated and/or phased-in reforms



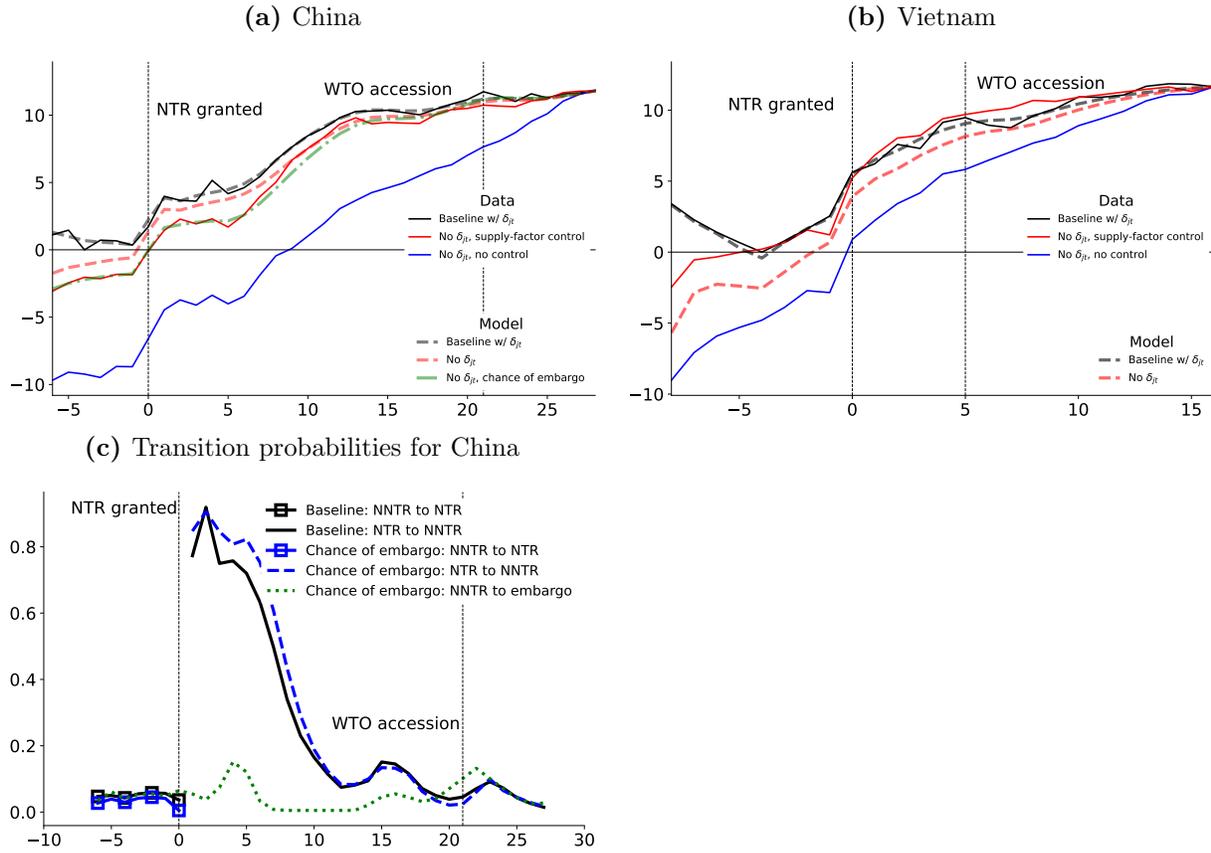
*Notes:* (Figure shows transition dynamics in experiments described in section B.2. (a): observed path of tariffs in immediate (solid black) and phased-in (dashed blue) reforms. (b): discounted expected tariff in unanticipated/immediate (solid black), unanticipated/phased-in (dashed blue), anticipated/immediate (dash-dotted red), and anticipated/phased-in (dotted purple) reforms. (c): number of exporters with same legend as (b). (d): aggregate exports with same legend as in (b). (e): trade elasticity measured relative to period before reform with same legend as (b). (f): trade elasticity measured relative to period firms learn about reform with same legend as (b).

**Figure A9: Sensitivity analysis to alternative values of  $\theta$**



*Notes:* (a) NTR-gap elasticities in the baseline model for China with trade-policy uncertainty (black with square markers), baseline canonical reform (blue dashed), canonical reform with lower value of  $\theta = 2.7$  (green dash-dotted), and canonical reform with higher value of  $\theta = 4.1$  (red dotted). (b) Same as (a) for Vietnam. (c) Estimated trade-policy transition probabilities for China in baseline calibration (black squares and black solid line), calibration with lower value of  $\theta = 2.7$  (green circles and dash-dotted line), and calibration with higher value of  $\theta = 4.1$  (red diamonds and dotted line). (d) Same as (c) for Vietnam.

**Figure A10:** Gap-elasticities: Robustness to country-year fixed effects and models with chance of embargo



*Notes:* (a) and (b) plot the results of (7) under the baseline approach (black line), without country-year fixed effects but with aggregate export control (red line), and without country-year fixed effects or control (blue line). Solid lines are data and dashed lines are model. For China, estimates without country-year fixed effects are also shown for the model with a probability of embargo (green line). The period  $h = 0$  is 1980 for China and 2002 for Vietnam. (c) plots the transition probabilities in the model with a chance of embargo versus the baseline probabilities. Black is the baseline, blue (NNTR to/from MFN) and green (NNTR to embargo) are the embargo model.

## E Additional tables

**Table A1:** List of Unilateral Trade Preference Programs

Year	Program	Code
1976	Generalized System of Preferences (GSP)	A, A*, A+, 4
1982	Caribbean Basin Economic Recovery Act (CBERA)	E, E*, 9
1992–2014	Andean Trade Preferences Act (ATPA)	J, J*, J+
2001	African Growth and Opportunity Act (AGOA)	D
2004	Caribbean Basin Trade Partnership Act Initiative (CBTPA)	R
1999	Duty-free Code for West Bank and Gaza Strip	N
2017	Nepal Preference Program	NP

*Notes:* The *Year* column is the first (and last) year in which we observe the corresponding country sub-code in the US Census trade data, reported in the *Code* column.

**Table A2:** List of countries in a PTA with the United States

Year	Country	Code	Year	Country	Code
1985	Israel	IL	2006	Singapore	SG
1988	Canada	X, CA	2007	Peru	PE
1994	Mexico	MX	2010	Costa Rica	P, P+
2001	Colombia	CO	2010	El Salvador	P, P+
2001	Jordan	JO	2010	Dominican Republic	P, P+
2004	Australia	AU	2010	Guatemala	P, P+
2004	Chile	CL	2010	Honduras	P, P+
2006	Bahrain	BH	2010	Nicaragua	P, P+
2006	Morocco	MA	2010	South Korea	KR
2006	Oman	OM	2012	Panama	PA

*Notes:* The *Year* columns report the first full year the agreement went into effect. The *Code* columns are the country sub-codes reported in the US Census trade data.

**Table A3:** Example of the Regime Classification

<i>Panel a: Census Data for SITC good</i>					
Provision Rate	Country Subcode	Tariff (%)	Import Share (%)	Regime	
10	0	0.00	5	NTR	
13	0	0.00	1	.	
16	0	0.00	0	.	
18	B	0.00	10	PTA	
18	C	0.00	0	PTA	
61	0	4.12	28	NTR	
64	0	3.87	23	PTA	
64	X	3.00	31	PTA	
79	0	0.00	0	.	
79	X	0.00	1	PTA	

<i>Panel b: 1990 HTS Tariff Schedule for corresponding HTS8 Products</i>		
HTS-8 Product	NTR rate (%)	PTA rate (%)
85011020	10	8
85011040	6.6	5.2
85011060	4.2	3.3
85012020	4.2	2.5
85012040	5	3
85012050	5	3
85012060	3.7	2.2
85014020	4.2	3.3
85014040	5	4
85014050	5	4
85014060	3.7	2.9
85015120	4.2	3.3
85015140	5	4
85015150	5	4
85015160	3.7	2.9
85015240	3.7	2.9
85015280	Free	
85015340	Free	
85015360	4.2	2.5
85015380	4.2	2.5

*Notes:* Based on imports of electric motors incl. universal A.C./D.C. motors (sitc code 71621) imported from Canada in 1990.

**Table A4:** Top five country-year transitions across regimes

From	To	$ig$ (# $g$ )
NTR	NNTR	PLD-1983 (232), PLD-1984 (78), PLD-1985 (43), AFG-1986 (46), ROU-1989 (119)
NTR	PTA	CAN-1989 (889), MEX-1994 (387), KOR-2012 (325), AUS-2005 (241), ISR-1986 (203)
NTR	UTPP	TWN-1976 (280), HKG-1976 (214), ISR-1976 (204), KOR-1976 (189), BRA-1976 (177)
NNTR	NTR	CHN-1980 (273), VNM-2002 (347), PLD-1989 (253), USSR-1992 (226), USSR-1993 (215)
NNTR	UTPP	ROU-1994 (32), CZE-1992 (31), CZE-1991 (28), BGR-1992 (26), PLD-1990 (23)
PTA	NTR	CAN-1999 (205), MEX-1999 (179), ISR-1999 (165), AUS-2009 (135)
UTPP	NTR	KOR-1989 (403), TWN-1989 (400), HKG-1989 (265), MYS-1997 (262), PRT-1986 (214)
UTPP	NNTR	ROU-1989 (7), ROU-1990 (6), ROU-1992 (5), ROU-1993 (5), YUG-1996 (5)
UTPP	PTA	ISR-1985 (354), MEX-1994 (342), PER-2007 (241), COL-2001 (229), DOM-2007 (176)

*Notes:* Regime transitions are based on consecutive good-country observations but need not be in consecutive years. For instance, Poland transitioned back to NNTR in October of 1982 and several goods traded under NTR were not traded in 1983 or 1984.

**Table A5:** Sensitivity of canonical long-run elasticity to model parameters

Baseline	China		Vietnam	
	14.67		15.29	
Parameter	Increase	Decrease	Increase	Decrease
$f_0$	14.99 ( 0.23)	14.39 ( 0.20)	15.66 ( 0.25)	15.29 ( 0.01)
$f_1$	14.24 (−0.31)	15.10 (−0.30)	15.10 (−0.14)	15.94 (−0.44)
$\xi_H$	15.52 ( 0.59)	13.86 ( 0.59)	16.05 ( 0.51)	14.85 ( 0.31)
$\rho_\xi$	14.13 (−1.50)	14.61 ( 0.16)	14.82 (−1.28)	15.45 (−0.40)
$\sigma_z$	12.48 (−1.70)	16.50 (−1.24)	13.81 (−1.07)	17.15 (−1.20)
$\rho_z$	9.99 (−4.03)	18.28 (−2.31)	10.56 (−3.89)	18.88 (−2.21)
$\delta_0$	14.70 ( 0.03)	14.63 ( 0.03)	15.53 ( 0.16)	15.47 (−0.12)
$\delta_1$	14.58 (−0.06)	14.76 (−0.06)	15.39 ( 0.06)	15.55 (−0.18)

*Notes:* Each row shows what happens when one parameter is increased or decreased by 10%, except for  $\rho_\xi$  where the change is 2.5% due to how close the parameter is to 1.0. Numbers in parentheses are elasticities of the canonical long-run elasticity with respect to the parameter.