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# Using equity market reactions to infer exposure to trade liberalization[☆]

Andrew Greenland[a,*], Mihai Ion[b], John Lopresti[c], Peter K. Schott[d,e]

[a] *North Carolina State University, United States of America*
[b] *Arizona State University, United States of America*
[c] *William & Mary, United States of America*
[d] *Yale School of Management, United States of America*
[e] *NBER, United States of America*

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ABSTRACT

We propose a method for identifying exposure to changes in trade policy based on asset prices that has several advantages over standard measures: it encompasses all avenues of exposure, it is natively firm-level, it yields estimates for both goods and service producers, and it can be used to study reductions in difficult-to-quantify non-tariff-barriers in a way that controls naturally for broader macroeconomic shocks. Applying our method to two well-studied US trade liberalizations provides new insight into service-sector responses and reveals dramatically different outcomes among small versus large firms, even within narrow industries.

## 1. Introduction

A firm's exposure to trade liberalization is typically measured via the average change in import tariffs among the set of goods it produces (Bernard et al., 2006). While easily observed, this metric limits our understanding of globalization in several ways. First, it focuses attention on manufacturing, neglecting the frequently much larger service sector. Second, it does not capture difficult-to-quantify but increasingly important changes in non-tariff barriers such as national treatment, product standards and intellectual property rights. Finally, it often ignores avenues of exposure beyond a firm's outputs, such as its major customers, suppliers and inputs (Amiti and Konings, 2007; Ding et al., 2019).

In this paper, we propose an alternate measure of firm exposure to policy changes derived from financial markets' reactions to key events associated with the new regime, such as the legislative votes by which they become law. We take our cue from the vast "event study" literature in financial economics that seeks to rationalize firms' abnormal stock returns on key trading days. In our case, however, we use these returns as "all-in", natively firm-level measures of policy exposure that can be used as an *explanatory* variable to predict and understand subsequent firm outcomes. Leveraging the "wisdom of the crowds" in this way addresses each of the limitations noted above: abnormal returns yield estimates of exposure for both goods and service producers; they capture information about the net impact of liberalizations on firms' operations that is known to analysts but difficult for the econometrician to observe;

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* Corresponding author.
*E-mail address:* agreenl@ncsu.edu (A. Greenland).

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and they can be used to study any change in policy that can be associated with one or more events. Moreover, "benchmark" firm returns computed on random days surrounding a liberalization offer a natural and conceptually appealing control for unobserved, firm-specific exposure to other shocks that is not available under traditional approaches.

Application of our method to two important changes in US trade policy – the granting of Permanent Normal Trade Relations (PNTR) to China in 2000 and the Canada–United States Free Trade Agreement (CUSFTA) in 1989 – yields a more complete picture of these liberalizations than has previously appeared in the literature. We highlight starkly different outcomes both across sectors and across firms, even within the same narrow industry, e.g., Apple and Dell versus Gateway. Comparison of firms' reactions to these two events also offers direct empirical support for the sort of cross-country variation in fixed supply-chain search costs implied by recent quantitative models of global sourcing, e.g., Antràs et al. (2017), as large firms exhibit disproportionate growth in sales and employment after PNTR relative to CUSFTA, consistent with greater fixed costs associated with accessing the Chinese market.

PNTR was a non-traditional trade liberalization in that it effectively eliminated the possibility that China would lose access to low US import tariffs.[1] For this application, we compute US firms' average abnormal returns ($AAR$s) across the five legislative events required for its passage: the introduction of the bill in the US House of Representatives, the House vote, Senate cloture, the Senate vote, and President Bill Clinton's signature. For goods-producers, we find that these $AAR^{PNTR}$ vary as expected with standard measures of exposure used in this literature – e.g., subsequent import growth – but provide explanatory power beyond them. For service producers, for which calculating standard measures is not possible, we provide *external* validation in two separate environments: NATO's accidental bombing of China's Belgrade embassy in 1999 and the election of Donald Trump in 2016 (Huang et al., 2018).

Employing a difference-in-differences (DID) approach, we use our measure to examine a wide set of firm outcomes, beginning with operating profit. We find that both goods *and* service producers with larger $AAR^{PNTR}$ are relatively more likely to survive and grow their operating profit after the change in policy versus before. Differences across firms, however, are stark: while almost all firms are predicted to have relative declines in operating profit after 2001, a small group of the very largest firms exhibit relative gains sufficient to outweigh the losses of all others. This finding is consistent with large firms' greater ability to absorb the relatively high fixed costs of sourcing internationally, allowing them to thrive while smaller firms contract or exit.

Examining employment, we find a similar, but flatter profile across the firm-size distribution – the largest firms expand, but by relatively less in terms of employment than in terms of profit. The implied relative increase in large firms' labor productivity (operating profit per worker) suggests a link between PNTR and the rise of "superstar" firms documented in Decker et al. (2014) and Autor et al. (2017c), as well as the substantial rise in US manufacturing productivity during the 2000s (Fort et al., 2018). Predicted relative growth of physical and intangible capital is similarly skewed, providing further support for the idea that industry "leaders" are able to invest more in response to rising import competition from China than followers (Gutierrez and Philippon, 2017).

Outside manufacturing, we find relatively greater predicted gains in operating profit and employment in Professional Services (e.g., accounting, law, engineering and R&D), consistent with an anticipated, post-PNTR shift in the United States towards the design, engineering and marketing of goods as physical production migrates to China (Fort et al., 2018; Ding et al., 2019). In Wholesale and Retail, by contrast, almost all firms are anticipated to shrink in relative terms. This result conforms with Wall Street's *ex ante* expectations that greater availability of Chinese goods would lead to an increase in competition among retailers, and thereby an erosion of markups (Kurtz and Morris, 2000). It suggests the relationship between the increasing "toughness" of competition and declining markups following trade liberalization developed in Melitz and Ottaviano (2008) may also apply to services.

Our second application, CUSFTA, considers a change in US trade policy with a closer and more similar trading partner, Canada. In contrast to PNTR, CUSFTA encompassed both traditional bilateral tariff reductions and a substantial loosening of restrictions on services trade via its inclusion of "national treatment", for which there is no standard tariff equivalent.[2] In this application, we compute abnormal returns for US firms during the 1988 Canadian federal election, which amounted to a referendum on the trade agreement (Breinlich, 2014). As with $AAR^{PNTR}$, we find that goods producers' $AAR^{CUSTFA}$ are correlated with the conventional measures of exposure: they fall with US tariff reductions and rise with Canadian tariff reductions. Intuitively, we find that average returns among service firms are higher for those in industries covered by national treatment.

Consistent with existing literature, but in contrast to the results for PNTR, we find no significant relationship between outcomes and exposure to CUSFTA among goods producers using either our measure or the standard measure of exposure, potentially due to CUSFTA's long time horizon or the subsequent implementation of NAFTA, unforeseen in 1988. We do, however, find that service providers with higher $AAR_j^{CUSFTA}$ exhibit greater operating profit after CUSFTA versus before, in line with the change in national treatment. We do not find as sharp a distributional impact across large and small firms as under PNTR, in line with relatively low fixed costs of sourcing from Canada relative to China.

A potential concern with our approach is that our estimates may reflect the response of firms to news more generally, rather than trade liberalizations specifically, resulting in biased estimates. For example, one might worry that market responses to the dot-com

[1] Handley and Limão (2017) estimate that the reduction in trade policy uncertainty associated with PNTR is equivalent to a reduction in tariff rates of approximately 13 percent. Pierce and Schott (2016) show that US manufacturing establishments facing greater reductions in expected tariffs exhibit relative declines in employment. Autor et al. (2013, 2014) find that US regions more exposed to Chinese import competition during this period experience relative declines in employment and earnings. In contemporaneous research Bianconi et al. (2021) show that industries with greater PNTR reductions in tariff rate uncertainty exhibit relatively lower stock returns.

[2] National treatment requires a country to treat foreign firms symmetrically to domestic firms. Trefler (2004) documents substantial reallocation among Canadian manufacturing sectors and plants following CUSFTA's passage. Breinlich (2014) demonstrates that changes in firm market value following CUSFTA are consistent with heterogeneous-firm models of international trade.

bubble or emerging technologies in 1999 are correlated firms' $AAR_j^{PNTR}$, and also predictive of their subsequent outcomes. In the final part of the paper, we show how this concern can be addressed using a control native to our method but infeasible in the traditional approach to assessing policy change. Specifically, we calculate "benchmark" $AARs$ on 1000 randomly chosen dates around each liberalization and include these returns as additional covariates in our DID specifications.[3] The resulting distribution of "benchmarked" DID coefficients identify the impact of each change in trade policy net of confounding information revealed about firms over the same period. In each case, we find that our benchmarked DID estimates remain essentially the same.

A final advantage of our approach is its ability to compare disparate policy changes using a common metric of exposure. We exploit this attribute to show that the impact of PNTR was both more immediate and more persistent than CUSFTA, a finding heretofore undocumented in the trade literature. This result obtains even after using the technique just described to control for potential variation in macroeconomic trends across the two periods, and using call option prices to account for potential market anticipation of PNTR's ultimate passage after its introduction in the House. We argue that one explanation for the disparity in the liberalizations' impact is greater-than-anticipated Chinese growth in the 2000s.

Our method has two caveats that must be kept in mind when interpreting results. First, because it is based on equity market reactions, it can be implemented only for firms whose shares are traded publicly. If the consequences for private firms are distinct from publicly traded ones, our approach will not capture the complete effect of the policy. Second, as with estimates from virtually all reduced-form empirical studies of environments where general equilibrium effects are relevant, ours may not capture a liberalization's systematic components. For instance, the impact of changes in interest rates, exchange rates or other aggregate prices are not captured due to the fact that abnormal returns are measured relative to the "market" impact of the change in policy. Our measure – like others – is thus better suited to analyzing variation in trade exposure across firms rather than the policy's level impact on a particular firm. Even so, we offer a means of assessing the systematic component, and demonstrate that our baseline results do not change substantially under plausible assumptions about its size.

Beginning with Ball and Brown (1968) and Fama et al. (1969), event studies have been used extensively in corporate finance to estimate the effect of new information on firm value.[4] Though not widely used within international trade, a number of papers examine the link between stock prices and exposure to trade, starting with Grossman and Levinsohn (1989a), who find a positive relationship between firm returns and the prices of competing import goods. More recently, Huang et al. (2018) report a negative relationship between firms' previous sales to China and their abnormal returns at the onset of the 2018 US–China trade war.[5] To our knowledge, we are the first to employ $AAR$s as an explanatory variable summarizing the effect of policy changes on firms, and to use that variable to predict and investigate subsequent firm outcomes. Our approach is conceptually similar to Kogan et al. (2017), who use firm returns after patent grants as a measure of *patent* value. Here, we show how $AAR$s can be used to gauge firms' exposure to changes in policy, and that this measure both predicts firm outcomes and sheds new light on their responses. Our measure is agnostic with respect to the underlying mechanism which ties trade policies to stock prices and future firm-level outcomes. In a related study, Amiti et al. (2021) propose such a mechanism based on Jones (1975) specific factors model and on Grossman and Levinsohn (1989b).

Our use of $AAR$s as "all-in", right-hand side explanatory variables contributes more broadly to the very large effort within trade to develop metrics of policy exposure. A popular approach, inspired by Bartik (1991), interacts agents' – generally firms' or regions' – activity shares with industry shocks, e.g., Topalova (2010). Such "direct" measures are often combined with additional industry-level information, such as input–output tables, to measure additional "indirect" channels of exposure, e.g., those associated with a firm's customers or suppliers (Amiti and Konings, 2007). A virtue of our approach is that it is *natively firm-level*. As a result, it captures variation across firms within industries and identifies the *net* impact of all channels of firm exposure without requiring any knowledge or assumptions on the part of the econometrician regarding firms' supply chains, managerial capabilities, or labor-market relationships.

Finally, our results with respect to PNTR and CUSFTA contribute to the very active literature in international trade studying the impact of import competition on workers and firms. Though researchers starting with Tybout et al. (1991) have examined plant and firm responses to greater openness, we are the first to use the same, "all-in" measure of firm exposure in two different trade liberalizations, and to compare a range of outcomes across them. Our finding that large firms exhibit larger growth in operating profit relative to employment following PNTR than following CUSFTA provides a clearer picture of the liberalizations' distributional effects than has previously existed in the literature. In this sense, we provide an additional rationale for why trade with China might be "different".

The paper proceeds as follows. Section 2 outlines the theory behind our approach, deferring details to Appendix. Section 3, validates and applies our method to PNTR. Section 4 applies our method to the Canada-US Free Trade Agreement. Section 5 highlights differences between the effects of both liberalizations. Section 6 details robustness exercises. Section 7 concludes.

[3] Breinlich (2014) uses a similar placebo exercise to show that his main results do not hold up in non-event days. The main difference in our benchmarking exercise is that we use the non-event AAR's as independent variables.

[4] Khotari and Warner (2006) document that this approach has been used in over 565 articles appearing in the top finance journals through 2006. For a recent discussion of this literature, see Wolfers and Zitzewitz (2018).

[5] Fisman and Zitzewitz (2019) go one step further, proposing that initial winner versus loser firms be tracked in the months after an event, such as Brexit, as a barometer of any revisions to initial expectations of the event.

## 2. Estimating firm exposure

In this section we briefly outline the conditions under which financial market reactions can be used to quantify firms' exposure to changes in policy, highlighting the key challenges for our method and outlining approaches to address them. We start with the assumption that markets are informationally efficient, i.e., that the impact of a particular event on a firm's market value can be estimated via the change in the firm's stock price during the event period, controlling for all other information relevant for firm value that may have been released at the same time.

We assume a firm's stock price at time $t$ is a function of a state space partitioned as $(X_t, e_t)$. Here, $e_t$ represents the information about the policy event of interest available at time $t$, and $X_t$ contains all other information relevant for firm value. This includes other firm-specific events such dividend announcements, or broader events such as the release of macroeconomic information. We assume that the policy event under consideration takes place at time $\tau$ and, as in our applications below, that the information released is whether the policy is approved ($e_\tau = Y$) or rejected ($e_\tau = N$). We assume that the event is unanticipated, deferring discussion of partial anticipation to Appendix Section A.

Let $P_{j,t}$ be the stock price of firm $j$ at time $t$, and $R_{j,t} = (P_{j,t} - P_{j,t-1})/P_{j,t-1}$ be the stock return of the firm during period $t$. Stock returns are commonly modeled as linear functions of shocks to macroeconomic (systematic) factors and firm-specific (idiosyncratic) shocks. Following this framework, we assume the true data-generating process for returns is:

$$R_{j,t}(X_t, e_t) = \alpha_j + \beta_j F_t(X_t, e_t) + \lambda_j D_t(e_t) + \eta_{j,t}(X_t) \tag{1}$$

where $\alpha_j$ is the expected return on the stock if no shocks are observed at time $t$, $F_t(X_t, e_t)$ is a vector of macroeconomic factors which affect all firms with varying sensitivities $\beta_j$, and $\eta_{j,t}(X_t)$ is a mean zero firm-specific shock independent of the event. Finally, $D_t$ is an indicator variable with $D_\tau(e_\tau = Y) = 1$, $D_\tau(e_\tau = N) = 0$, and $D_t(e_t) = 0$ for all $t \neq \tau$. Coefficients $\alpha_j$ and $\beta_j$ are assumed constant over time – in particular, they are not affected by the event of interest.

The true effect of the event on firm $j$'s value is given by the difference in the potential outcomes for its stock price $P_{j,\tau}(X_\tau, e_\tau = Y) - P_{j,\tau}(X_\tau, e_\tau = N)$. Expressed as a percentage of the initial price $P_{j,\tau-1}$, this becomes $R_{j,\tau}(X_\tau, e_\tau = Y) - R_{j,\tau}(X_\tau, e_\tau = N)$. We denote this term $AR^*_{j,\tau}$ – the (true) abnormal return caused by the event for firm $j$. Using Eq. (1), this effect can be expressed as:

$$AR^*_{j,\tau} = \beta_j (F_\tau(X_\tau, e_\tau = Y) - F_\tau(X_\tau, e_\tau = N)) + \lambda_j \tag{2}$$

Since the counterfactual $R_{j,\tau}(X_\tau, e_\tau = N)$ is not observed, this quantity must be estimated. The systematic factors $F_t$ are generally identified using either a statistical framework such as principal component analysis or an asset pricing model. In our applications below, we adopt the most common approach in the event-study literature, an economic model informed by the Capital Asset Pricing Model (CAPM) known as the "market model", which uses the market portfolio as the single factor.[6] We estimate the $\alpha_j$ and $\beta_j$ coefficients in Eq. (1) from firm-level time-series regressions of realized stock returns on the systematic factor(s) using data immediately prior to the event, with $D_t(e_t) = 0$:

$$R_{j,t} = \alpha_j + \beta_j F_t + \eta_{j,t}, \quad t < \tau \tag{3}$$

The effect of the event on the value of firm $j$ is then estimated by subtracting the fitted value from Eq. (3) from the realized return of the firm at time $\tau$:

$$AR_{j,\tau} = R_{j,\tau} - (\hat{\alpha}_j + \hat{\beta}_j F_\tau). \tag{4}$$

Under the mean independence assumption $E[\eta_{j,t}|F_t] = 0$, $\hat{\alpha}_j$ and $\hat{\beta}_j$ are unbiased estimates of $\alpha_j$ and $\beta_j$. Substituting Eq. (1) at $t = \tau$ into Eq. (4) yields $AR_{j,\tau}$ as an unbiased estimate of $\lambda_j + \eta_{j,\tau}(X_\tau)$. For this to be equal to the true effect of the event as in Eq. (2), two requirements must be met. First, there must be no other idiosyncratic shocks at the time of the event – that is, $\eta_{j,\tau}(X_\tau) = 0$. Second, the event must not affect systematic factors – $F_\tau(X_\tau, e_\tau = Y) = F_\tau(X_\tau, e_\tau = N)$.

In our estimations below, we follow the event study literature in trying to increase the likelihood that $\eta_{j,\tau}(X_\tau) = 0$ by using short windows surrounding the policy event and by excluding firms experiencing significant observable confounding events during the event window, such as dividend announcements. Avoiding the bias induced by the effect of the event on systematic factors such as, e.g., interest rates or exchange rates, is more challenging. While it is reasonable to assume this bias is close to zero for firm-specific events such a patent grant or an earnings announcement, this assumption is more tenuous for broad policy changes such as trade liberalizations. As a result, to the extent that we are successful in mitigating $\eta_{j,\tau}(X_\tau) \neq 0$, our abnormal returns estimates from Eq. (4) return the $\lambda_j$ term for each firm, and therefore must be interpreted as the effect of the policy on firms *relative* to its impact on systematic factors.

We note, however, that if one is willing to assume that no confounding systematic shocks occur at the same time as the change in policy, then the systematic component of the policy change can be estimated using the factor realizations themselves.[7] Such an approach might be reasonable for very short windows, during which it is unlikely that any other meaningful macroeconomic shock takes place. Towards that end, in Section 6 we explore the robustness of our results to narrower event windows. We also add a

[6] We show that our baseline results are robust to using multi-factor asset pricing models in online Appendix Section B.

[7] If no confounding systematic shocks occur at time $\tau$, this means that had the policy not been changed, there would have been no systematic shock at all. This, by definition, implies $F_\tau(X_\tau, e_\tau = N) = 0$, which in turn implies that $F_\tau = F_\tau(X_\tau, e_\tau = Y) - F_\tau(X_\tau, e_\tau = N) = F_\tau(X_\tau, e_\tau = Y)$. Amiti et al. (2021), for example, assume both $F_\tau(X_\tau, e_\tau = N) = 0$ and $\eta_{j,\tau}(X_\tau) = 0$ in their study of US firms' investment during the US–China trade war.

range of plausible values of $F_\tau(X_\tau, e_\tau = Y) - F_\tau(X_\tau, e_\tau = N)$ into our estimates of $AR^*$, and discuss the sensitivity of our DID results to this procedure.

While the caveats outlined in this section must be kept in mind, they should be weighed against our new measure's benefits, as well as the limitations of standard approaches in the trade literature, as discussed in the introduction.

## 3. PNTR

In this section we apply the method outlined above to measure US firms' exposure to the US granting of permanent normal trade relations (PNTR) to China in 2000.

The United States has two sets of import tariff rates. The first set, known as "normal trade relations" or NTR tariffs, are generally low and are applied to goods imported from other members of the World Trade Organization (WTO). The second set, known as non-NTR tariffs, were set by the Smoot–Hawley Tariff Act of 1930 and are often substantially higher than NTR rates. While imports from non-market economies such as China are by default subject to the higher non-NTR rates, US law allows the President to grant such countries access to NTR rates on a year-by-year basis, subject to potential overrule by Congress.

US Presidents began requesting that China be granted such a waiver in 1980. Congressional approval of these requests was uncontroversial until the Chinese government's crackdown on the Tiananmen Square protests in 1989, after which it became politically contentious and less certain. This uncertainty reduced US firms' incentive to invest in closer economic relations with China, and *vice versa*. Goldman Sachs, for example, wrote that "the annual debate has been a highly politicized process, posing a substantial threat to Chinese exporters and US importers" (Hu, 1999). It ended with Congress' passage of bill HR 4444 granting China permanent normal trade relations (PNTR) status in October 2000, which formally took effect upon China's entry into the WTO in December, 2001.[8]

At the time of PNTR's passage, investment bankers expected that China's entry into the WTO would benefit US firms in a variety of industries. Goldman Sachs expected US producers to have an easier time selling into the Chinese market and using China as an export platform, while US service providers, particularly in telecommunications, insurance, and banking, would be granted greater access to Chinese consumers via the loosening of restrictions on foreign direct investment (Hu, 1999). The $AAR$s computed in the next section are designed to aggregate investors' expectations regarding the impact of all of such channels.

### 3.1. Computing and describing $AAR^{PNTR}$

We choose events based on the US legislative process, calculating abnormal returns over the five steps by which a US bill becomes law: (1) introduction of the PNTR bill in the US House of Representatives on May 15, 2000; (2) the vote to approve PNTR in the House on May 24; (3) the successful cloture motion to proceed with a vote on PNTR in the US Senate on July 27; (4) the vote to approve PNTR by the Senate on September 19; and (5) the signature of PNTR into law by President Clinton on October 10.[9]

The salience of these events was noted among Wall Street analysts and in newspaper articles at the time.[10] Writing in early 2000, Goldman Sachs, for example, notes that

> "The event that deserves close watch is the forthcoming US Congressional debate on permanent normal trading relations (NTR) for China, which is required to bring current U.S. trade policies pertaining to China into conformity with the basic WTO principle of most favored nation (MFN) treatment for all members." (Kurtz and Morris, 2000)

Articles in the New York Times noted that the successful vote in the House represented a "stunning victory for the Clinton administration and corporate America" (Schmitt and Kahn, 2000), and that Senate Majority Leader Trent Lott's decision to proceed to a vote in the Senate removed a "major hurdle" to considering the policy change: while a majority of Senators were in favor of PNTR, Lott had been holding up a move of the bill to the floor to achieve greater leverage in budget negotiations with the Clinton administration (Reuters, 2000; Schmitt, 2000).

As noted in Section 2, to estimate each firm's abnormal returns, we first estimate its "normal" returns (i.e. returns unconditional on the event) using the standard "market model". Motivated by the CAPM, this model imposes the market portfolio return $R_{m,t}$ as the only systematic factor ($F_t$) in Eq. (3):

$$R_{j,t} = \alpha_j + \beta_j R_{m,t} + \eta_{j,t}. \tag{5}$$

We separately estimate this regression for every firm in our sample over all available dates in 1999, the year prior to PNTR. We choose this period to ensure that our coefficient estimates $\hat{\alpha}_j$ and $\hat{\beta}_j$ are not affected by periods when relevant legislative information

[8] PNTR was accompanied by several additional changes in policy in both the United States and China, including reductions in Chinese import tariffs, elimination of China's export licensing regime, production subsidies, and barriers to foreign investment, and the removal of US quotas on China's textile and clothing quotas as part of the phasing out of the global Multifiber Arrangement (Pierce and Schott, 2016).

[9] The full text of HR 4444 is available at https://www.congress.gov. The substantial gap between cloture and the vote in the Senate is due to that body's August recess.

[10] Appendix Figure A.1 tracks the number of articles appearing in major news outlets jointly containing the phrases "Permanent Normal Trade Relations", "China" and "United States" during 2000.

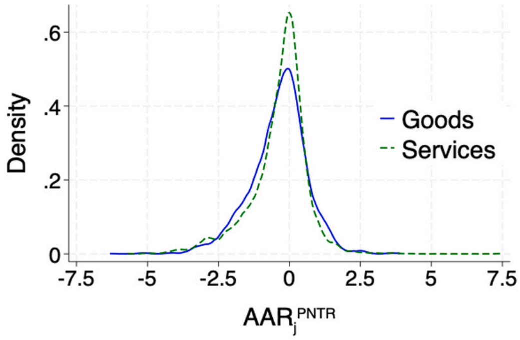


**Fig. 1.** PNTR average abnormal returns, by type of firm.
*Source:* CRSP and authors' calculations. Figure plots distribution of $AAR_j^{PNTR}$ for two mutually exclusive firm types: Goods producers, which have business segments in NAICS 11, 21, 3X, and service firms, which do not. Values below −7.5 and above 7.5 percent are dropped to improve readability. The means, standard deviations and inter-quartile ranges for the these two groups of firms are −0.38, 1.00 and 1.16 percent for goods producers and −0.35, 1.06 and 0.97 percent for service firms.

about PNTR became known.[11] Daily returns for these regressions come from the Center for Research in Security Prices (CRSP). We follow the literature and restrict ourselves to common shares (i.e. CRSP share code 10 or 11) of firms incorporated in the United States, traded on one of the three main exchanges – NYSE, AMEX, and Nasdaq (i.e. CRSP exchange codes 1, 2, or 3).[12]

In order to capture any anticipatory movements prior to each event, as well as any lagged response over the subsequent days, we use a 2-day window surrounding each of the legislative event days mentioned above, for a total of 5 days for each event, or 25 days across all 5 events. For each day $t$ in our event windows, we calculate normal returns for each firm $j$ as $\hat{\alpha}_j + \hat{\beta}_j R_{m,t}$ and subtract this sum from the return of the firm on that day to obtain its abnormal return: $AR_{j,t} = R_{j,t} - \hat{\alpha}_j + \hat{\beta}_j R_{m,t}$. Finally, we calculate our primary measure of the firm's exposure to the policy, hereafter $AAR_j^{PNTR}$, by taking an average of all the non-missing abnormal returns of the firm over the 25 days spanning all 5 events.[13] Our procedure yields $AAR_j^{PNTR}$ for 5378 firms that are present during 1999 (the pre-period used to estimate $\hat{\beta}_j$) and at least one of the five legislative events. Across all five events the mean $AAR_j^{PNTR}$ is −0.37 percent, with a standard deviation of 1.04 percent.[14]

We begin by documenting the heterogeneity in our measure of exposure along two important dimensions size and sector. Using data from COMPUSTAT, we classify firms into two mutually exclusive categories depending on the mix of 6-digit NAICS codes spanned by their major business segments. We define firms to be goods producers if their business segments include Manufacturing (NAICS 31 to 33), Mining, Quarrying, Oil and Gas Extraction (NAICS 21), or Agriculture, Forestry, Fishing and Hunting (NAICS 11). We classify all remaining firms as "service" firms.[15] This results in a sample consists of 2385 goods and 2993 service firms respectively.

Fig. 1 reports the *unweighted* distributions of these AAR. As the market-capitalization weighted average abnormal return across all firms is mean zero by definition, the greater left skewness of goods producers in the figure indicates these firms have a more positive correlation between market capitalization and $AAR_j^{PNTR}$. This outcome may reflect goods-producing firms' greater exposure to increased import competition from China following PNTR. The means, standard deviations and inter-quartile ranges for the these two groups of firms are −0.38, 1.00 and 1.16 percent for goods producers and −0.35, 1.06 and 0.97 percent for service firms.

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11 To minimize noise in our coefficient estimates, we keep only firms with at least 120 non-missing dates in 1999. We also show in Appendix Section B.2 that our results are robust to using "multi-factor" asset pricing models as well as alternate event windows. Finally, in unreported results, we find that our results are robust to utilizing $\hat{\alpha}_j$ and $\hat{\beta}_j$ coefficients estimated using the 250 days that end 30 days before each event.

12 Following convention, $R_{j,t}$ and $R_{m,t}$ are excess returns with respect to the risk-free rate, i.e., the one-month T-bill. Data on the daily market return and the risk-free rate are taken from Kenneth French's website. The market return is the value-weighted return for all firms meeting the criteria noted in the main text. Appendix Figures A.3 and A.4 report the simple return of the market ($R_{m,t}$) and the total volume of shares traded in the market across the PNTR event windows.

13 By averaging across events, we treat each day as an independent draw from the distribution of returns. In Appendix Section B.2, we demonstrate the robustness of our results to use of an alternate "buy-and-hold" average, i.e., the geometric mean of the cumulative abnormal return associated with purchasing firms' stock prior to the first event and holding them across all five events. In the asset pricing literature, the term "exposure" generally refers to factor loadings (i.e. elasticities to risk factors). Here, we refer to abnormal returns as a measure of exposure to trade liberalization, that is, as a measure of the expected impact of trade liberalization.

14 Appendix Figure A.2 reports the distribution at each stage of the legislative process.

15 COMPUSTAT reports firms' sales in up to 10, 6-digit NAICS business segments. In 2000, approximately 71, 16 and 7.5 percent of firms have 1, 2 or 3 segments listed, while the remaining 4 percent of firms have up to 10 segments listed. We classify the 57 firms with missing segment information as goods producers.

**Table 1**
$AAR_j^{PNTR} > 0$ size premia.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | (1) All | (2) Goods | (3) Services |
|---|---|---|---|
| Sales | 0.497 | 0.758 | 0.333 |
| | (0.134) | (0.230) | (0.127) |
| COGS | 0.371 | 0.607 | 0.226 |
| | (0.108) | (0.168) | (0.115) |
| Operating profit | 0.458 | 0.655 | 0.346 |
| | (0.117) | (0.195) | (0.123) |
| Employment | 0.421 | 0.599 | 0.314 |
| | (0.102) | (0.185) | (0.098) |
| PPE | 0.513 | 0.666 | 0.370 |
| | (0.128) | (0.212) | (0.143) |
| Intangibles | 0.374 | 0.509 | 0.284 |
| | (0.092) | (0.137) | (0.102) |
| Market capitalization | 0.712 | 0.877 | 0.602 |
| | (0.145) | (0.199) | (0.177) |

Table presents firm-level OLS regressions of the log of various measures of firm size on an indicator variable for whether $AAR_j^{PNTR} > 0$, a constant, and 6-digit NAICS fixed effects. Each cell represents the result of a separate regression. Each column focuses on a different set of firms. Goods firms have a business segment active in NAICS sectors 11, 21 and 3X. Service firms have no business segments in these sectors. The maximum number of observations are 5269, 2302, and 2967 for the regressions in columns 1, 2 and 3. Standard errors are reported below coefficient estimates and are clustered by 4-digit NAICS industries.

We find that firms with positive $AAR_j^{PNTR}$ are larger along almost every dimension than firms with negative relative returns, even within narrow industries, and that these premia are higher for goods-producers than for service firms.[16] These relationships are illustrated in Table 1, which summarizes the results of a series of OLS regressions of various measures of firm size on a dummy variable indicating whether $AAR_j^{PNTR}$ is greater than zero, as well as 6-digit NAICS industry fixed effects. Each cell in the table reports the coefficient and standard error for the dummy variable of interest from a different regression. The sample for results in the first column is all firms, while the samples for results in the second and third columns are goods producers and service firms, respectively. Standard errors are clustered at the 4-digit NAICS level. As indicated in the table, goods producers with positive $AAR_j^{PNTR}$ have size premia of 0.66, 0.60 and 0.88 log points in terms of operating profit, employment and market capitalization, with each of these relationships being statistically significant at conventional levels. The analogous premia for service firms are 0.35, 0.31 and 0.60.

To the extent that firm size is correlated with firm efficiency, the relationships displayed in Table 1 are consistent with models of international trade predicting that high-efficiency firms are better able to take advantage of reductions in trade costs (Melitz, 2003; Breinlich, 2014; Antràs et al., 2017; Bernard et al., 2018), for example because larger firms are more likely to be using the types of information and communication technologies that facilitate trade (Fort, 2017).

Finally, we find that firms' $AAR_j^{PNTR}$ vary widely even within 6-digit NAICS industries. Fig. 2 compares firms' $AAR_j^{PNTR}$ to their major industry's $AAR_i^{PNTR}$, i.e, the unweighted average abnormal return of all firms whose largest segment is 6-digit NAICS industry $i$. Results for goods-producing firms are in the left panel, while results for service firms are in the right panel, and the size of the markers is scaled to firms' market capitalization prior to the first PNTR legislative event. To the extent that import competition in firms' major business segments is the sole determinant of their exposure to PNTR, the points in this figure would be clustered along the 45 degree line. Instead, we find a broad cloud of points, potentially reflecting underlying heterogeneity in other forms of exposure to PNTR. For example, some firms within an industry subject to the same degree of import competition might be better able to take advantage of freer trade with China. Even in industries exhibiting a negative $AAR_i^{PNTR}$, many firms have a positive $AAR_j^{PNTR}$. This deviation from industry averages appears to be more pronounced among firms with a larger market capitalization – particularly in the goods-producing sectors.

"Electronic Computer Manufacturing" (NAICS 334111), for example, includes a number of firms with both positive and negative $AAR_j^{PNTR}$. Among them, Apple Computer Inc. and Dell Computer Corporation are positive, while Gateway Inc., also a supplier of PCs, is negative. The former two firms thrived after PNTR, in part by taking advantage of supply chains in China. Gateway, which attempted to produce computers solely within the United States, shrank steadily during the 2000s before abandoning its US operations altogether.

These differences are consistent with stock traders anticipating the firms' divergent post-PNTR business strategies.

[16] Griffin (2018) also finds that abnormal returns rise with firm size following the house vote on PNTR.

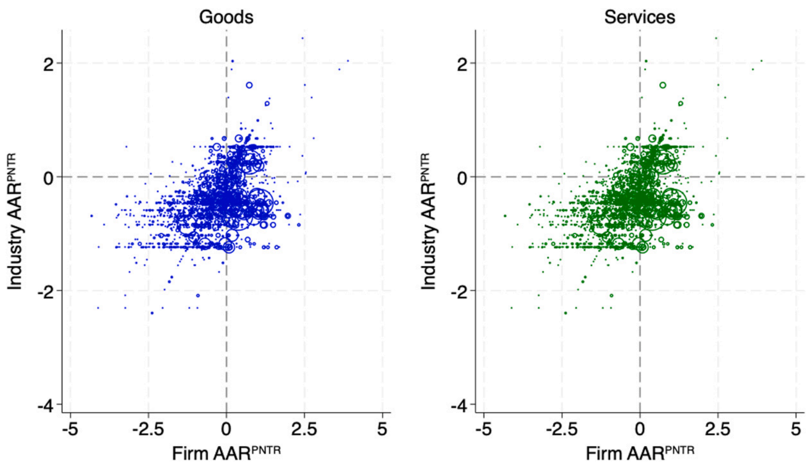


**Fig. 2.** Firm- versus industry-level average abnormal returns.
*Source:* CRSP, COMPUSTAT and authors' calculations. Figure compares firms' $AAR_j^{PNTR}$ to the unweighted average industry $AAR_i^{PNTR}$ of their primary 6-digit NAICS segment. Values below −5 and above 5 percent are dropped to improve readability. Each point's size is scaled to the firm's market capitalization in 2000.

### 3.2. Validity of $AAR_j^{PNTR}$

In this section we perform a proof of concept by using *contemporaneous*, *ex post* and *external* validity checks to demonstrate the correlation of $AAR_j^{PNTR}$ with standard measures of exposure to PNTR available at the time, subsequently, and from unrelated events.

*Contemporaneous validity*: We establish the contemporaneous validity of our measure by examining its relationship to the standard metric for PNTR used in the literature, the "NTR gap". This gap is defined as the difference between the higher non-NTR rate to which tariffs would have risen if annual renewal had failed, and the often much lower NTR rates permitted under temporary NTR status,

$$NTR\,Gap_i = Non\,NTR\,Rate_i - NTR\,Rate_i, \tag{6}$$

where $i$ indexes 6-digit NAICS industries. These gaps are computed for 1999, the year before the change in policy, using data on US import tariff rates reported in Feenstra et al. (2002).[17] Their mean and standard deviation are 0.29 and 0.15. We summarize their distribution visually in Appendix Figure A.5.

Specifically, we use an OLS specification of the form

$$AAR_j^{PNTR} = \delta NTR\,Gap_j + \epsilon_j, \tag{7}$$

where $NTRGap_j$ is the sales-weighted average of the industry-level NTR gap ($NTRGap_i$) in firms' major segments. As $NTRGap_j$ is not defined for service firms, estimation is restricted to firms with sales in at least one goods-producing industry, substituting a gap of zero for any service segments when computing the sales-weighted averages. To ease interpretation, all variables are de-meaned and divided by their standard deviation. Standard errors are clustered at the 4-digit NAICS level.

Results are reported in Table 2. As shown in column 1, we find a negative and statistically significant relationship between $NTR\,Gap_j$ and $AAR_j^{PNTR}$. A one standard deviation increase in the sales-weighted average $NTR\,Gap_i$ facing firms corresponds to a reduction in $AAR_j^{PNTR}$ of 0.20 standard deviations. That is, firms more exposed to PNTR via direct import competition are re-valued downward relative to less-exposed firms.[18]

Columns 2 to 4 examine firms' exposure to the change in policy via their supply chains, as proxied by their up- and downstream NTR gaps, $NTR\,Gap_j^{Up}$ and $NTR\,Gap_j^{Down}$.[19] To the extent that greater upstream exposure lowers firms' input costs, and greater

[17] Tariff rates are assigned according to 8-digit Harmonized System (HS) commodity codes. Following Pierce and Schott (2016), we take the average NTR gap across HS codes within each 6-digit NAICS code, using the concordance reported in Pierce and Schott (2012).

[18] In Table A.1 of Section C of the Appendix, we repeat this specification for each of the five events separately. We find a negative relationship for all events that is statistically significant for three: the House vote, Senate cloture, and Clinton's signing.

[19] Following Pierce and Schott (2016), we compute weighted averages of the NTR gaps across the firm's up- and downstream industries using the 1997 US Bureau of Labor Statistics input–output total-use coefficients as weights. Given the high correlation between an industry's own $NTR\,Gap_i$ and those of other industries within the same 3-digit sector, we omit from these averages all 6-digit industries within the same 3-digit NAICS root. The correlations between $NTRGap_i$ and $NTRGap_i^{Up}$ and $NTRGap_i^{Down}$ when we do not omit sectors are 0.55 and 0.08. The analogous correlations after removal are 0.38 and −0.01. For firms with multiple segments, we compute $NTR\,Gap_j^{Up}$ and $NTR\,Gap_j^{Down}$ as the sales weighted average of the respective industry-level gaps across segments.

**Table 2**
$AAR_j^{PNTR}$ versus the NTR gap and firm attributes.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | (1) $AAR_j^{PNTR}$ | (2) $AAR_j^{PNTR}$ | (3) $AAR_j^{PNTR}$ | (4) $AAR_j^{PNTR}$ |
|---|---|---|---|---|
| NTR $Gap_j$ | −0.202 | −0.244 | −0.139 | −0.075 |
| | (0.054) | (0.057) | (0.046) | (0.032) |
| NTR $Gap_j^{Up}$ | | 0.114 | 0.075 | 0.090 |
| | | (0.052) | (0.047) | (0.034) |
| NTR $Gap_j^{Down}$ | | −0.038 | −0.030 | −0.087 |
| | | (0.040) | (0.042) | (0.029) |
| MFA $Exposure_j$ | | | 0.006 | 0.009 |
| | | | (0.012) | (0.009) |
| $\Delta$ China $Licensing_j$ | | | −0.219 | −0.174 |
| | | | (0.064) | (0.038) |
| $\Delta$ China import $Tariffs_j$ | | | −0.075 | −0.041 |
| | | | (0.027) | (0.017) |
| ln(PPE per $Worker)_j$ | | | | 0.073 |
| | | | | (0.035) |
| ln(Mkt $Cap)_j$ | | | | 0.090 |
| | | | | (0.022) |
| $\frac{CashFlows}{Assets}_j$ | | | | 0.236 |
| | | | | (0.023) |
| Book $Leverage_j$ | | | | 0.037 |
| | | | | (0.030) |
| Tobins $Q_j$ | | | | 0.046 |
| | | | | (0.035) |
| Constant | −0.018 | −0.092 | 0.090 | 0.049 |
| | (0.058) | (0.074) | (0.092) | (0.052) |
| Observations | 2271 | 2271 | 2271 | 2271 |
| $R^2$ | 0.044 | 0.056 | 0.076 | 0.175 |

Table presents firm-level OLS regressions of $AAR_j^{PNTR}$ on $NTRGap_j$, other policy variables and a series of year-2000 firm accounting attributes that are winsorized at the 1 percent level. Policy variables are expiration of textile and clothing quotas under the global Multi-Fiber Arrangement (MFA), elimination of export licensing restrictions and decreases in Chinese import tariffs. All covariates are de-meaned and divided by their standard deviation. Goods firms have a business segment active in NAICS sectors 11, 21 and 3X. Service firms have no business segments in these sectors. Standard errors are reported below coefficient estimates and are clustered by 4-digit NAICS industries.

downstream exposure reduces customer demand, we expect the relationship between $AAR_j^{PNTR}$ and $NTR\,Gap_j^{Up}$ to be positive and that with $NTR\,Gap_j^{Down}$ to be negative. Estimates in column 2 are consistent with these expectations: greater Chinese import competition among firms' suppliers is associated with a relative increase in market value while greater import competition among firms' customers has an adverse impact on relative market value, though the point estimate for $NTR\,Gap_j^{Down}$ is not statistically significant at conventional levels.[20]

The third column of Table 2 considers variables capturing three other policy changes associated with China's entry into the WTO: decreases in Chinese import tariffs, elimination of export licensing restrictions, and the expiration of the global Multi-Fiber Arrangement (MFA).[21] Including these additional variables does not change the sign and statistical significance of the NTR gap variables, but it does reduce the magnitude of the own-gap estimate from −0.24 to −0.14. Among the new policy variables, we find negative and statistically significant relationships with respect to changes in China's import tariffs and export licensing, and a positive relationship with respect to MFA exposure. The negative associations between $AAR_j^{PNTR}$ and changes in Chinese import tariffs is consistent with higher expected profit in industries where it will be easier for US firms to export to China. The negative association between $AAR_j^{PNTR}$ and the share of Chinese firms eligible export is also intuitive, as removal of these restrictions may increase

[20] One concern with this regression is that most firms are observed to operate in just one business segment. A regression of the market-capitalization weighted average $AAR_j^{PNTR}$ across firms in each 6-digit NAICS industry on the industry-level $NTR\,Gap_j$ also yields a negative and statistically significant relationship of similar magnitude.

[21] Industry-level data on the change in Chinese import tariffs from 1996 to 2005 and the share of Chinese firms eligible to export are from Brandt et al. (2017) and Bai et al. (2015). As discussed in greater detail in Section D of the Appendix, we follow Pierce and Schott (2016) in using the import-weighted average fill rate of the quotas removed in each 6-digit NAICS industry as of the PNTR votes as a control. Fill rates are defined as actual divided by allowable imports; higher values indicate greater exposure to MFA quota reductions.

**Table 3**
$AAR_j^{PNTR}$ versus Chinese import growth.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | (1) $AAR_j^{PNTR}$ | (2) $AAR_j^{PNTR}$ | (3) $AAR_j^{PNTR}$ |
|---|---|---|---|
| $\Delta\ln(\text{Imports})_j^{2000-6}$ | −0.123 | −0.123 | −0.093 |
| | (0.045) | (0.045) | (0.030) |
| $\Delta\ln(\text{Imports})_j^{1990-00}$ | | 0.001 | −0.009 |
| | | (0.035) | (0.041) |
| $\ln(\text{PPE per Worker})_j$ | | | 0.000 |
| | | | (0.038) |
| $\ln(\text{Mkt Cap})_j$ | | | 0.113 |
| | | | (0.021) |
| $\frac{CashFlows}{Assets}_j$ | | | 0.232 |
| | | | (0.034) |
| Book Leverage$_j$ | | | 0.079 |
| | | | (0.034) |
| Tobins Q$_j$ | | | 0.027 |
| | | | (0.032) |
| Constant | −0.081 | −0.081 | −0.069 |
| | (0.052) | (0.052) | (0.042) |
| Observations | 1901 | 1901 | 1901 |
| $R^2$ | 0.016 | 0.016 | 0.121 |

Table presents firm-level OLS regressions of $AAR_j^{PNTR}$ on US import growth from China in firms' largest business segment and a series of year-2000 firm accounting attributes that are winsorized at the 1 percent level. Regression sample is restricted to firms operating in tradable industries – with a business segment in NAICS sectors 11, 21 and 3X. All covariates are de-meaned and divided by their standard deviation. Goods firms have a Standard errors are reported below coefficient estimates and are clustered by 4-digit NAICS industries.

competition for US producers in the exposed industries. The positive association between $AAR_j^{PNTR}$ and exposure to elimination of MFA quotas may reflect the ability of some goods-producing firms to take advantage of greater production in China.

Finally, the fourth column of Table 2 includes a set of firm attributes, based on accounting information, commonly included in regressions of abnormal returns in the finance literature as proxies for firms' investment opportunities and their ability to finance them. They are property, plant and equipment (PPE) per worker, firm size (as measured by the log of market capitalization), profitability (cash flows to assets), book leverage, and Tobin's Q.[22] As is common in the finance literature, we winsorize these accounting variables at the 1 percent level to reduce the influence of outliers, i.e., we replace observations below the first percentile and above the ninety-ninth percentile with the values at those percentiles.

With these additional covariates included, the coefficients on all three NTR gap variables retain their signs from previous columns. The own-gap coefficient drops further in magnitude, to −0.08, and all three gap controls are now statistically significant. Among the additional firm attributes, we find positive and statistically significant relationships for all except book leverage, which is positive but not statistically significant at conventional levels.

The results in Table 2 indicate that $AAR_j^{PNTR}$ is related to the standard metric of exposure used in the literature and indeed goes beyond it to capture additional dimensions of the change in policy. As a result, they highlight a key benefit of our approach, which is to provide an all-in measure of firm exposure. This attribute is particularly useful as gathering data on all possible dimensions of firm exposure is impractical.

*Ex Post validity:* Table 3 examines the link between firms' $AAR_j^{PNTR}$ and post-PNTR US import growth from China, an outcome not knowable in 2000, but useful for assessing the *ex post* validity of $AAR_j^{PNTR}$. For each firm, we calculate weighted average US import growth across observed business segments in 2000. Given that imports are not observed for service-firm industries, the sample for this analysis is restricted to firms with sales in at least one goods-producing industry. Among those firms, we assign zero import growth to all service segments in calculating the firm average. The sample period is from 2000 to 2006, from passage of PNTR until the year before the Great Recession. As above, all variables are de-meaned and divided by their standard deviation and standard errors are clustered at the 4-digit NAICS level.

As indicated in the first column of the table, we find a negative and statistically significant relationship between $AAR_j^{PNTR}$ and post-PNTR import growth. In column 2, we add the change in imports between 1990 and 2000 as a placebo exercise and find that the coefficient for *ex post* import growth remains as before while the coefficient for prior period is close to zero and statistically insignificant. In column 3, we find that these relationships are robust to the inclusion of the accounting attributes introduced in the

[22] In this section, all firm attributes are measured before the first legislative event we consider, and are drawn from COMPUSTAT. All columns in the table are restricted to the sample of firms for which all five controls are reported. Results using the full sample are very similar.

**Table 4**
$AAR_j^{PNTR}$ versus $AAR_j^{Belgrade}$.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | (1) $AAR_j^{PNTR}$ | (2) $AAR_j^{PNTR}$ | (3) $AAR_j^{PNTR}$ |
|---|---|---|---|
| $AAR_j^{Belgrade}$ | −0.082 | −0.051 | −0.121 |
| | (0.020) | (0.022) | (0.034) |
| Constant | 0.010 | −0.018 | 0.032 |
| | (0.063) | (0.074) | (0.089) |
| Observations | 5055 | 2269 | 2786 |
| $R^2$ | 0.007 | 0.004 | 0.012 |
| Firm type | All | Goods | Services |

Table presents firm-level OLS regressions of $AAR_j^{PNTR}$ on $AAR_j^{Belgrade}$. All covariates are demeaned and divided by their standard deviation. Goods firms have a business segment active in NAICS sectors 11, 21 and 3X. Service firms have no business segments in these sectors. Standard errors are reported below coefficient estimates and are clustered by 4-digit NAICS industries.

last section. The estimated coefficient estimate on post-2000 import growth from China in the final column, −0.093, indicates that a 1 standard deviation increase in subsequent imports from China is associated with a 0.093 standard deviation decline in average abnormal returns, or a loss in market value of about 2.4 percent.[23] Together, the results in Table 3 indicate that during PNTR's passage investors bid down the returns of firms that subsequently experienced greater import competition from China, and that this behavior is not the continuation of a prior trend.

*External validity*: We also include *external* validity tests which, unlike the previous two, can be performed for service producers. In these cases we examine the correlation between firms' AAR's and similarly constructed measures calculated during events that may *reverse* the effects of the liberalization. We consider two such events– the accidental NATO bombing of the Chinese embassy in Belgrade, Yugoslavia on May 7, 1999, and the election of President Donald Trump.[24] For brevity, we relegate our analysis of the 2016 Presidential Election to Appendix Section E. Given that the bombing was unanticipated, and that information about it unfolded slowly, we compute firms' $AAR_j^{Belgrade}$ across the seven trading days after the bombing occurred. We analyze the association between $AAR_j^{Belgrade}$ and $AAR_j^{PNTR}$ via the following OLS regression:

$$AAR_j^{PNTR} = \delta AAR_j^{Belgrade} + \epsilon_i. \tag{8}$$

Results are presented in Table 4. We find that the relationship between the $AAR$s is *negative* and statistically significant at conventional levels for all firms as well as goods and service providers separately, indicating that firms which are expected to benefit relative to the market from a potential breakdown of US–China relations due to the bombing in 1999 are expected to be harmed in relative terms by the trade liberalization in 2000.[25]

### 3.3. Using $AAR_j^{PNTR}$ to assess firm outcomes

Standard event studies in the finance literature focus on whether a particular event has a significant impact on stock returns. Hence, the object of interest is usually the cross-sectional average of abnormal returns.[26] In this paper we argue that abnormal returns provide an all-in summary of the impact of a change in policy on the firm. As such, they are a natively *firm-level* measure of exposure to trade liberalization that can be employed in the standard difference-in-difference identification strategies used in the trade literature.

We first examine the impact of exposure on firm survival and their sales, cost of goods sold and operating profit conditional on survival. As $AAR_j^{PNTR}$ represent traders' assessment of the effect of PNTR on firms' cash flows (and discount rates), we expect firms with relatively low $AAR_j^{PNTR}$ to be less profitable and less likely to survive. Then, following much of the "China shock" literature, we examine the link between $AAR_j^{PNTR}$ and firms' employment and capital.[27]

23 Multiplying the coefficient (−0.093) by the standard deviation of $AAR_j^{PNTR}$ (1.03 percent) provides the daily effect. Multiplying this number by 25 to account for all 25 days in our event windows yields 2.4 percent.

24 The bombing occurred during an 11-week NATO campaign intended to end Serbian aggression against ethnic Albanians in Kosovo, and was recognized at the time as a potential threat to China's entry into the WTO. Three days after the bombing, for example, the Wall Street Journal noted that "prospects for a speedy end to negotiations on China's accession to the World Trade Organization just got a lot worse" (Brauchli and Cooper, 1999).

25 Across goods firms, we find the expected *positive* relationship between the $AAR_j^{Belgrade}$ and the $NTR\ Gap_j$ in Section C of Appendix.

26 See for example the textbook treatment in Campbell et al. (1997).

27 We also show in Appendix Section B.2 that our results are robust to using "multi-factor" asset pricing models as well as alternate event windows when constructing AAR.

**Table 5**
$AAR_i^{PNTR}$ and firm exit, multinomial logit.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | Survival | Contraction/Bankruptcy | Merger | Other |
|---|---|---|---|---|
| | Panel A: All firms | | | |
| $\text{AAR}_j^{PNTR}$ | | −0.268 | 0.022 | −0.081 |
| | | (0.072) | (0.050) | (0.089) |
| Marginal effect | 0.016 | −0.026 | 0.011 | −0.001 |
| | (0.012) | (0.007) | (0.008) | (0.002) |
| Unconditional probability | 0.586 | 0.17 | 0.204 | 0.041 |
| $\Delta$ Prob. | 0.028 | −0.153 | 0.054 | −0.036 |
| Pseudo $R^2$ | .122 | .122 | .122 | .122 |
| Observations | 4377 | 4377 | 4377 | 4377 |
| | Panel B: Goods Only | | | |
| $\text{AAR}_j^{PNTR}$ | | −0.211 | 0.146 | −0.129 |
| | | (0.090) | (0.066) | (0.084) |
| Marginal effect | −0.006 | −0.018 | 0.028 | −0.003 |
| | (0.013) | (0.008) | (0.010) | (0.002) |
| Unconditional probability | 0.633 | 0.148 | 0.18 | 0.039 |
| $\Delta$ Prob. | −0.01 | −0.122 | 0.153 | −0.078 |
| Pseudo $R^2$ | .128 | .128 | .128 | .128 |
| Observations | 2266 | 2266 | 2266 | 2266 |
| | Panel C: Service only | | | |
| $\text{AAR}_j^{PNTR}$ | | −0.304 | −0.051 | −0.050 |
| | | (0.089) | (0.044) | (0.126) |
| Marginal effect | 0.032 | −0.034 | 0.001 | 0.000 |
| | (0.013) | (0.010) | (0.008) | (0.003) |
| Unconditional probability | 0.535 | 0.193 | 0.229 | 0.043 |
| $\Delta$ Prob. | 0.06 | −0.175 | 0.006 | 0.005 |
| Pseudo $R^2$ | .122 | .122 | .122 | .122 |
| Observations | 2111 | 2111 | 2111 | 2111 |

Table presents results of firm-level multinomial logit model of exit (i.e., de-listing from their exchange) between 2000 and 2006. De-listing codes are described in text and Appendix Table A.4. The base outcome (column 1) is survival through the end of 2006. Right-hand side variables included in the regression but whose estimates are suppressed are a series of year-2000 firm accounting attributes that are winsorized at the 1 percent level. All covariates are de-meaned and divided by their standard deviation. Standard errors are reported below coefficient estimates and are clustered by 4-digit NAICS industries.

### *3.3.1. Firm survival*

In this section, we examine the relationship between $AAR_j^{PNTR}$ and firm survival, where exit from our sample signifies a firm's de-listing from the stock exchange. We group the CRSP flags for these de-listings into three categories: (1) bankruptcy and contraction of firm assets, equity, or capital below the levels required to be listed; (2) merger; and (3) exit for other reasons, e.g., protection of investors and the public interest, or failure to meet equity requirements.[28]

Table 5 presents the results from a multinomial logit regression of exit,

$$Pr(Y_j = d) = \delta AAR_j^{PNTR} + \gamma \chi_{\mathbf{j}}^{\mathbf{2000}} + \epsilon_j, \tag{9}$$

where $Pr(Y_j = d)$ is the probability that firm $j$ exits between 2000 and 2006 due to de-listing category $d$, and $\chi_j^{2000}$ represents the vector of accounting variables employed in our earlier specifications, e.g., Table 2.[29] The latter are included because the fundamental attributes governing firms' success or failure during trade liberalization may affect their performance more broadly. For example, firms with higher productivity may earn greater profit after PNTR (Melitz, 2003), but they may also earn greater profit for other reasons, e.g., via their easier access to capital markets or their greater ability to achieve operational efficiencies from investments in technology. If ignored, these attributes would confound our ability to use $AAR_j^{PNTR}$ to predict subsequent changes in firm outcomes.

The base outcome is survival. As with our previous firm-level regressions, we standardize all variables by subtracting their mean and dividing by their standard deviations. We report both coefficients and marginal effects evaluated at the mean of all dependent variables for $\delta$; results for all other covariates are suppressed to conserve space.

[28] Appendix Table A.4 provides a more detailed breakdown of these flags. We observe 1814 firms de-list between 2000 and 2006. The distribution of these de-listings across the three categories is 743, 893, and 178, respectively.

[29] We cannot use a difference-in-differences specification to examine exit due to how our sample is constructed. That is, firms must be present in 2000 for $AAR_j^{PNTR}$ to be measured. Balance sheet information is missing for 771 firms in 2-digit NAICS sector 52 (Finance). This information is also missing for 221 firms in other sectors. All of these firms are excluded from the analyses in the remainder of the paper.

Panel A of the table focuses on the full sample of firms, and indicates that higher $AAR_j^{PNTR}$ is indeed correlated with reduced exit via contraction and bankruptcy. The marginal effects indicate that a one standard deviation increase in $AAR_j^{PNTR}$ is associated with a relative decrease in the probability of exit for these causes of 2.6 log points, an economically meaningful impact given that the unconditional probability of exit due to these causes, reported in the fourth to last line of the panel, is 16.9 percent. We do not find any significant relationships between $AAR_j^{PNTR}$ and "other" forms of de-listing.

In panels B and C, we estimate the multinomial logit separately for goods and service firms. We find that higher $AAR_j^{PNTR}$ are negatively associated with the likelihood of exit via bankruptcy and contraction for both types of firms, though the magnitude of the effect is comparatively larger for service firms. A one standard deviation increase in $AAR_j^{PNTR}$ is associated with a relative decline in exit of 1.8 and 3.4 log points, versus unconditional probabilities of exit of 14.8 and 19.3 percent. For manufacturing firms, we find a positive association with respect to de-listing due to merger, which may indicate the relative attractiveness of firms with a "China strategy" as acquisitions targets. Further research into such an explanation is warranted.

Overall, the results in Table 5 provide additional support for our approach, as they suggest investors anticipated future firm survival. The greater overall importance of $AAR_j^{PNTR}$ in explaining service firm survival may be due to their thinner profit margins. That is, to the extent that less profitable firms are more likely to exit in the face of negative economic shocks, one might expect the impact of PNTR on exit to be larger among these firms.

*3.3.2. Relative growth in operating profit, employment and capital*

This section explores the relationship between $AAR_j^{PNTR}$ and operating profit among surviving firms using a generalized difference-in-differences specification,

$$ln(OperatingProfit_{j,t}) = \delta Post \times AAR_j^{PNTR} + \gamma Post \times \chi_{\mathbf{j}}^{\mathbf{1990}} + \alpha_j + \alpha_t + \epsilon_{j,t}. \tag{10}$$

The sample period is 1990 to 2006. The left-hand side variable represents one of a range of firm outcomes available in COMPUSTAT, discussed in detail below. The first term on the right-hand side is the difference-in-differences term of interest – an interaction of firms' average abnormal return and an indicator variable ($Post$) for years after 2000 – which captures the relative change in outcomes among firms with differential exposure to the change in policy after versus before it occurs. The second term on the right-hand side again represents the vector of winsorized initial (here 1990) firm accounting attributes that may influence profitability through channels unrelated to PNTR.[30] The final terms on the right-hand side are the firm and year fixed effects required to identify the difference-in-differences coefficient. Firm fixed effects capture the impact of any time-invariant firm characteristics, while year fixed effects account for aggregate shocks that affect all firms. As above, all independent variables have been standardized so that the coefficients may be interpreted as the impact of changing the covariate by one standard deviation, and standard errors are clustered by 4-digit NAICS industry.

*Sales, Costs and Operating Profit*: Estimates for firms' worldwide sales, cost of goods sold (COGS) and operating profit (i.e., sales less COGS) are reported in Table 6. Columns 1, 4, and 7 contain results for all firms. In the first two of these columns, we find positive and statistically significant relationships between abnormal returns and both sales and cost of goods sold, indicating that firms with higher $AAR_j^{PNTR}$ expand after PNTR relative to firms with lower abnormal returns. The positive relationship between $AAR_j^{PNTR}$ and operating profit in column 7 suggests that firms with positive returns relative to the market during key PNTR legislative events do in fact exhibit relatively higher profits through 2006. The coefficient estimates in these columns imply that a one standard deviation increase in $AAR_j^{PNTR}$ is associated with relative increases in sales, COGS and operating profit of 13.0, 10.5 and 12.9 log points, respectively.

Columns 2, 5, and 8 report results for goods-producing firms, while columns 3, 6, and 9 are restricted to service firms. As indicated in the table, we find positive and statistically significant relationships for all three outcomes among both sets of firms. Magnitudes for sales and operating profit are larger for goods firms, while the opposite is true for COGS.[31] We discuss the implications of these results in Section 3.4 below.

*Employment, Physical Capital and Intangible Capital*: Estimates for firms' worldwide employment, physical capital and intangible capital are reported in Table 7. Physical capital is defined as the book value of property, plant and equipment, while intangible capital, following Peters and Taylor (2017), is measured as the sum of goodwill, capitalized research and development expenditures and capitalized "organizational" capital, defined as a fixed portion of selling, general and administrative expenses.

Both goods-producing and service firms with higher $AAR_j^{PNTR}$ exhibit relative increases in employment after the change in policy versus before. The coefficient estimate for all firms is 0.098, implying that a one standard deviation increase in $AAR_j^{PNTR}$ is associated with a relative increase in employment of 9.8 log points in the post period. Perhaps surprisingly, the magnitude of this point estimate is larger for service-producing firms – 10.2 log points – than goods firms – 8.6 log points. We return to the implications of this result in Section 3.4 below.

[30] For firms that enter the sample after 1990, we use their attributes upon entry in constructing $\chi_{\mathrm{j}}$.

[31] Results in Table 6 are restricted to firms with positive operating profit. We find qualitatively similar results using an inverse hyperbolic sine transform (e.g., $ln(OperatingProfit_{j,t} + (1 + OperatingProfit_{j,t}^2))^{0.5})$, which approximates the natural log but allows for values weakly less than zero. In Appendix Table A.6 we examine the relationship between operating profit and the average abnormal returns associated with each event, finding negative and statistically significant relationships except for the Senate vote. Appendix Tables A.7 and A.8 demonstrate that we find similar results when we add $NTRGap_j$, $NTR\,Gap_j^{Up}$ and $NTR\,Gap_j^{Down}$ as additional covariates to the baseline specification, suggesting that $AAR_j^{PNTR}$ captures the effects of PNTR through channels beyond direct import competition.

**Table 6**
$AAR_i^{PNTR}$ and firm sales, COGS and operating profit.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | $ln(Sales_j)$ | | | $ln(COGS_j)$ | | | $ln(Profit_j)$ | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Post*$AAR_j^{PNTR}$ | 0.130 | 0.150 | 0.095 | 0.105 | 0.097 | 0.103 | 0.129 | 0.143 | 0.098 |
| | (0.026) | (0.036) | (0.032) | (0.020) | (0.023) | (0.028) | (0.026) | (0.026) | (0.036) |
| Post*PPE per $Worker_j$ | 0.053 | 0.147 | −0.015 | 0.046 | 0.129 | −0.007 | 0.037 | 0.152 | −0.040 |
| | (0.041) | (0.055) | (0.028) | (0.035) | (0.050) | (0.023) | (0.044) | (0.054) | (0.031) |
| Post*ln(Mkt $Cap)_j$ | −0.068 | −0.091 | −0.062 | −0.076 | −0.097 | −0.072 | −0.074 | −0.105 | −0.058 |
| | (0.023) | (0.027) | (0.029) | (0.020) | (0.025) | (0.025) | (0.024) | (0.027) | (0.026) |
| Post*$\frac{CashFlows}{Assets}_j$ | −0.136 | −0.198 | −0.044 | −0.060 | −0.098 | −0.012 | −0.137 | −0.212 | −0.045 |
| | (0.031) | (0.033) | (0.029) | (0.020) | (0.021) | (0.028) | (0.035) | (0.040) | (0.027) |
| Post*Book $Leverage_j$ | −0.037 | −0.095 | 0.026 | −0.027 | −0.077 | 0.024 | −0.033 | −0.081 | 0.017 |
| | (0.019) | (0.021) | (0.023) | (0.020) | (0.024) | (0.025) | (0.023) | (0.024) | (0.025) |
| Post*Tobins $Q_j$ | 0.128 | 0.163 | 0.097 | 0.126 | 0.143 | 0.107 | 0.114 | 0.156 | 0.074 |
| | (0.023) | (0.042) | (0.024) | (0.021) | (0.035) | (0.025) | (0.025) | (0.040) | (0.028) |
| FE | j&t | j&t | j&t | j&t | j&t | j&t | j&t | j&t | j&t |
| Firm type | All | Goods | Services | All | Goods | Services | All | Goods | Services |
| Years | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 |
| $R^2$ | .924 | .926 | .921 | .927 | .93 | .922 | .913 | .92 | .906 |
| Observations | 51 121 | 28 694 | 22 427 | 51 205 | 28 778 | 22 427 | 48 551 | 26 928 | 21 623 |
| Unique firms | 4516 | 2340 | 2176 | 4517 | 2341 | 2176 | 4360 | 2237 | 2123 |

Table presents firm-level OLS DID panel regressions of noted firm outcomes on firms' PNTR average abnormal returns ($AAR_j^{PNTR}$) and a series of 1990 firm accounting attributes that are winsorized at the 1 percent level. Sample period is 1990 to 2006. All covariates are de-meaned and divided by their standard deviation. Standard errors are reported below coefficient estimates and are clustered by 4-digit NAICS industries.

**Table 7**
$AAR_i^{PNTR}$ and employment, PPE, and intangible capital.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | $ln(Employment_j)$ | | | $ln(PPE_j)$ | | | $ln(Intangibles_j)$ | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Post*$AAR_j^{PNTR}$ | 0.098 | 0.086 | 0.102 | 0.091 | 0.112 | 0.061 | 0.064 | 0.053 | 0.066 |
| | (0.018) | (0.023) | (0.030) | (0.024) | (0.025) | (0.038) | (0.019) | (0.019) | (0.030) |
| Post*PPE per $Worker_j$ | 0.036 | 0.102 | −0.008 | −0.062 | 0.012 | −0.129 | 0.007 | 0.074 | −0.021 |
| | (0.020) | (0.022) | (0.027) | (0.045) | (0.066) | (0.025) | (0.024) | (0.026) | (0.030) |
| Post*ln(Mkt $Cap)_j$ | −0.071 | −0.091 | −0.067 | −0.076 | −0.116 | −0.037 | −0.025 | −0.059 | 0.004 |
| | (0.016) | (0.017) | (0.024) | (0.025) | (0.030) | (0.025) | (0.019) | (0.016) | (0.037) |
| Post*$\frac{CashFlows}{Assets}_j$ | −0.024 | −0.056 | 0.033 | −0.030 | −0.044 | −0.003 | −0.037 | −0.062 | 0.003 |
| | (0.020) | (0.019) | (0.027) | (0.015) | (0.018) | (0.026) | (0.021) | (0.017) | (0.031) |
| Post*Book $Leverage_j$ | −0.052 | −0.092 | −0.010 | −0.050 | −0.109 | 0.022 | −0.056 | −0.077 | −0.043 |
| | (0.018) | (0.020) | (0.026) | (0.021) | (0.026) | (0.023) | (0.017) | (0.022) | (0.025) |
| Post*Tobins $Q_j$ | 0.119 | 0.166 | 0.084 | 0.169 | 0.227 | 0.130 | 0.189 | 0.232 | 0.146 |
| | (0.015) | (0.027) | (0.017) | (0.027) | (0.042) | (0.028) | (0.032) | (0.029) | (0.047) |
| FE | j&t | j&t | j&t | j&t | j&t | j&t | j&t | j&t | j&t |
| Firm type | All | Goods | Services | All | Goods | Services | All | Goods | Services |
| Years | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 | 1990-6 |
| $R^2$ | .935 | .941 | .927 | .944 | .948 | .939 | .917 | .943 | .886 |
| Observations | 51 007 | 28 779 | 22 228 | 51 227 | 28 968 | 22 259 | 49 468 | 28 782 | 20 686 |
| Unique firms | 4522 | 2347 | 2175 | 4523 | 2347 | 2176 | 4442 | 2337 | 2105 |

Table presents firm-level OLS DID panel regressions of noted firm outcomes on firms' PNTR average abnormal returns ($AAR_j^{PNTR}$) and a series of 1990 firm accounting attributes that are winsorized at the 1 percent level. Sample period is 1990 to 2006. All covariates are de-meaned and divided by their standard deviation. Standard errors are reported below coefficient estimates and are clustered by 4-digit NAICS industries.

The remaining columns of Table 7 indicate positive relationships between $AAR_j^{PNTR}$ and both forms of capital. Among goods producers, the coefficient for physical capital is more than twice as large as that for intangible capital, and both are statistically significant. For service firms, both associations are positive and of similar magnitude, but only the relationship with intangible capital is statistically significant at conventional levels. These positive relationships indicate a potential mechanism for the sort of product or process upgrading in response to low-wage country import competition found among US and European firms by Bernard et al. (2006), Khandelwal (2010) and Bernard et al. (2011). Bloom et al. (2016) and Ding et al. (2019). They are consistent with Gutierrez and Philippon (2017), who find relative increases in intangible investment and innovation among industry leaders in response to PNTR, but at odds with Autor et al. (2017b), who argue that increases in Chinese import penetration negatively affect patenting.

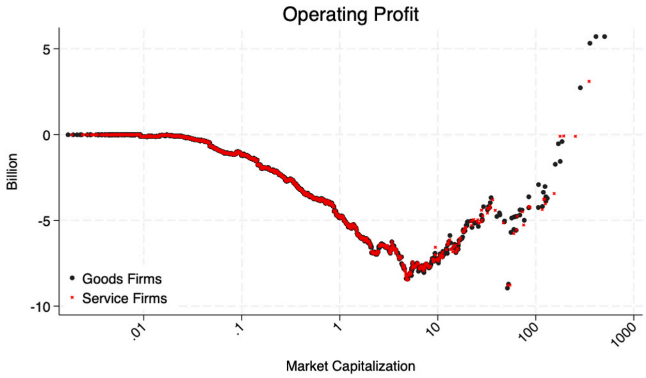


**Fig. 3.** Cumulative relative change in operating profit: Service firms highlighted. *Source:* CRSP, COMPUSTAT, and authors' calculations. Figure displays the predicted cumulative relative change in goods versus service firms' operating profit implied by the baseline difference-in-differences estimates in Table 6. Firms' market capitalization (in billions) is from 2000, prior to PNTR.

We note that while optimal employment and investment are functions of expected operating profits, there is no reason to believe that they are monotonically related. Expanding firms may invest in labor-saving technology, for instance, thereby reducing relative employment. Further, the relationship between profit and factor inputs may itself be affected by PNTR, for instance if PNTR causes general equilibrium changes in factor costs. Hence, the effect of PNTR on employment and investment is *a priori* unclear even if its impact on expected profits is not. Identifying the mechanisms through which PNTR's effect on expected profits alters employment and investment decisions is worthy of further inquiry but beyond the scope of this study.

### 3.4. The firm-level distributional implications of PNTR

In this section we use the results above to examine the firm-level distributional implications of PNTR. For each firm $j$, we employ the estimates of $\hat{\delta}$ from DID specifications analogous to Eq. (10), but estimated using non-standardized covariates, to compute predicted relative operating profit for 2001 to 2006:

$$\widehat{Op\ Profit}_j^{Post\ Period} = \left(exp(\hat{\delta} \times AAR_j^{PNTR}) - 1\right) \times Op\ Profit_j^{2000} \tag{11}$$

The product of $\hat{\delta}$ and $AAR_j^{PNTR}$ is the predicted growth in operating profit in the post-PNTR period relative to the pre-PNTR period, in log points. It is exponentiated and reduced by 1 to convert it into percentage terms, and then multiplied by operating profit in 2000 to convert it into levels. As we are focused on investors' expectations at the time of the policy change, we compute $\widehat{Op\ Profit}_j^{Post\ Period}$ for all firms, even if they subsequently exit the sample. In performing these calculations, we use the separately estimated $\hat{\delta}$'s for goods and service firms.

Fig. 3 plots the cumulative predicted relative operating profit across all firms in the post period, calculated by summing the fitted value from Eq. (11) along the firm size distribution, from low to high market capitalization. Goods producers are represented by large black dots, while service firms are indicated by the red x's.

As illustrated in the figure, cumulative profit declines with firm size until market capitalization reaches approximately 10 billion dollars. Firms larger than this threshold exhibit modest relative increases in expected operating profit until market capitalization reaches around 100 billion dollars, at which point it rises substantially. This reversal is driven by firms both inside and outside manufacturing, though the former are more prevalent as size grows: above 20 billion dollars, 57 percent of firms are goods producers, while above 50 and 100 billion dollars, their share is two-thirds.[32]

The variation in Fig. 3 is consistent with the existence of relatively high fixed costs to access Chinese suppliers. Antràs et al. (2017), for example, categorize China as one of the world's most attractive sources of imported intermediate inputs, with among the highest fixed costs. In such a setting, the largest US firms would have the greatest ability to access Chinese suppliers and thereby achieve lower costs and greater sales and operating profit. The results in Fig. 3 also suggest a potential role for trade liberalization in the rising share of economic activity attributed to large, old, "superstar" firms documented in Decker et al. (2014) and Autor et al. (2017c).

Fig. 4, which no longer differentiates goods and service producers to promote legibility, reveals a different trend for employment. As with operating profit, small firms exhibit relative declines. The largest firms, however, have relative employment growth that is

[32] As discussed further in Section F of the Appendix, large firms' size as well as their generally positive $AAR_j^{PNTR}$ contribute to their predicted relative growth *vis a vis* small firms in Fig. 3.

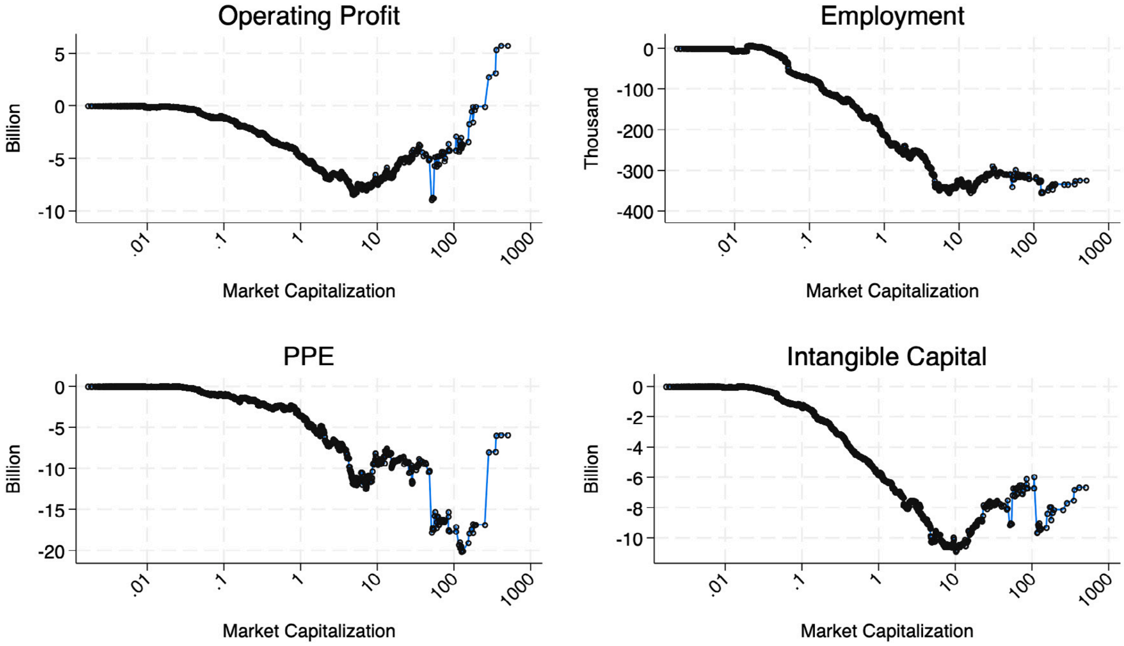


**Fig. 4.** Cumulative relative change in firm outcomes.
*Source:* CRSP, COMPUSTAT, and authors' calculations. Figure displays the predicted cumulative relative change in four firm outcomes implied by the baseline difference-in-differences estimates in Table 6. Firms' market capitalization (in billions) is from 2000, prior to PNTR.

either flat or moderately declining. The implicit relative increase in labor productivity among the largest firms and overall suggests a link between PNTR and the substantial rise in US manufacturing labor productivity observed during the 2000s (Fort et al., 2018).

The remaining panels of Fig. 4 provide analogous displays for physical and intangible capital. In both cases, the smallest firms show relative declines, and the largest firms show relative increases. The latter, however, are more modest than for operating profit, with the result that the relative gains of the largest firms do not offset the relative losses of the smaller firms. Even so, these outcomes are broadly consistent with recent research by Gutierrez and Philippon (2017) showing that industry "leaders" invest more in response to rising import competition from China than their followers.

Fig. 5 reports the cumulative relative change in each outcome for each 2-digit NAICS sector for which we observe a large number of firms. The $y$-axis in each panel of the figure reports the cumulative relative change in each outcome as a share of its initial (year 2000) level so that the four outcomes can be plotted against each other. Sectors vary substantially in their predicted relative changes. Almost all mining firms, for example, exhibit predicted relative increases in the four outcome variables, while the opposite is true in Wholesale/Retail. The latter accords with analysts' expectations at the time that China's entry into the WTO would reduce US wholesale and retail markups, and that these reductions would not be offset by greater profit in China, at least initially.[33] It also suggests the relationship between the increasing "toughness" of competition and declining markups following trade liberalization developed in Melitz and Ottaviano (2008) applies to services.

Two other sectors of note in Fig. 5 are Professional Services and Information. Professional Services, which includes business services such as accounting and law as well as engineering and research and development, exhibit a large cumulative relative gain. This increase may be driven by an anticipated, post-PNTR shift in the United States towards the design, engineering, sourcing, marketing and distribution of goods whose physical production would begin migrating to China (Ding et al., 2019). The Information sector, which includes publishing, motion pictures, broadcasting, telecommunications, and data processing, exhibits a large cumulative relative decline across all four outcomes, driven by negative average abnormal returns among 75 percent of the firms. The three largest firms (Microsoft, Oracle and AT&T) have positive $AAR$s and exhibit relative growth in all four outcomes. There is also a smaller cohort of relatively large internet and logistic firms, e.g., Ebay and I2 Technologies, which also exhibits relative gains.[34] These trends may be influenced by the fact that while China agreed to substantial liberalization of its telecommunications sector as part of its WTO accession, it was phased in gradually and subject to a number of limitations, such as temporary restrictions on foreign ownership shares, which may have affected different types of Information firms unevenly.[35] This

[33] For example, while Goldman Sachs anticipated a near tripling of Chinese sales for Wal-Mart in the first five years after PNTR, it predicted that this growth would not make a meaningful contribution to Wal-Mart's bottom line (Kurtz and Morris, 2000).

[34] These two firms both have market capitalization on the order of 10 billion dollars in our sample.

[35] For a detailed discussion of telecommunications liberalization in China, see Pangestu and Mrongowius (2002) and Whalley (2003).

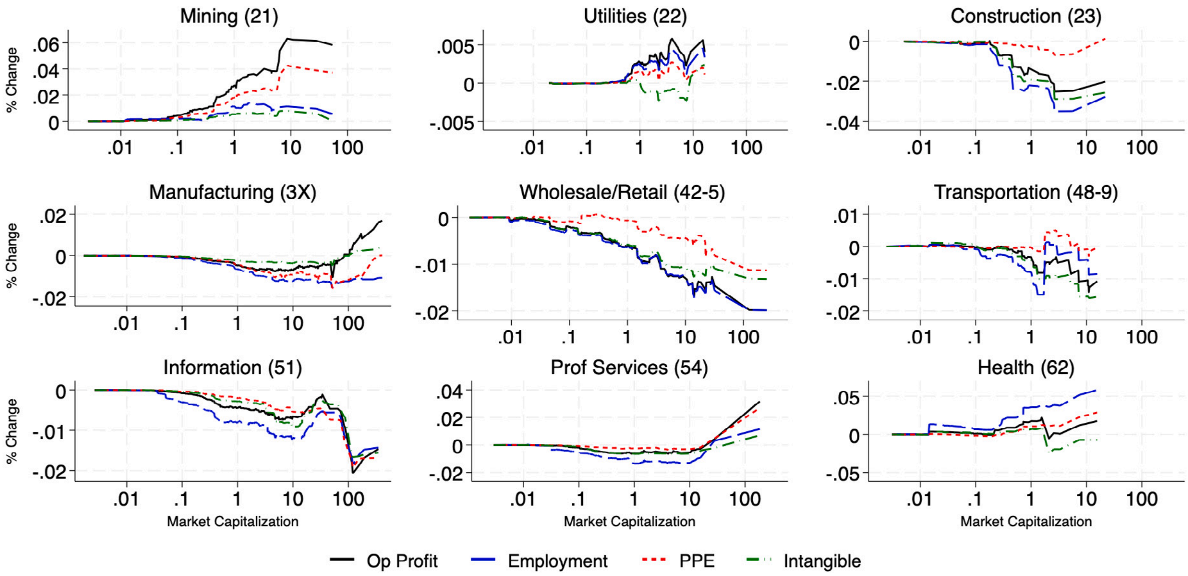


**Fig. 5.** Cumulative relative changes by sector

*Source:* CRSP, COMPUSTAT, and authors' calculations. Figure displays the predicted cumulative relative change in 4 firm outcomes implied by the baseline difference-in-differences estimates in Table 6 by noted 2-digit NAICS sector. $Y$-axis reports the cumulative predicted relative change as a share of the initial total of each outcome across firms in 2000, prior to PNTR. Each firm appears only in one panel, according to the NAICS code of largest business segment in 2000. Firms' market capitalization is from 2000, prior to PNTR. Note that y-axes vary across panels.

delay may have affected the timing of revenues versus costs more for some firms than others, substantially backloading operating profit beyond our time horizon. Further research here would be interesting.

## 4. CUSFTA

We now turn to a second application of our method, the 1989 Canada-US Free Trade Agreement, one of the largest bilateral trade agreements of its time. It is an attractive target for our approach due to the fact that one of its central provisions, "national treatment", required the US and Canada to treat each countries' service firms symmetrically, for instance with respect to professional licensing standards and market access.[36] Measures for such provisions are difficult to quantify, and as a result tend to be ignored in standard analyses. CUSFTA is also appealing because it mandated declines in *both* countries' tariffs, inducing potentially complicated responses among firms operating in or drawing inputs from both markets. Though CUSFTA's impact on Canada is well-studied, there is little research on either its US effects or on service sector responses.[37]

We follow Breinlich (2014) in focusing on the November 21, 1988 Canadian federal election as the key event associated with the policy change. CUSFTA was by far the most important issue debated in this election, and its outcome was uncertain in the weeks leading up to it. While Prime Minister Brian Mulroney and the Progressive Conservative party favored CUSFTA, his opponent John Turner and the Liberal Party proposed abandoning it.

### 4.1. Computing $AAR^{CUSFTA}$

We compute US firms' average abnormal returns around the Canadian election, $AAR_j^{CUSFTA}$, analogously to those calculated for PNTR. We divide firms into goods producers and service firms using the 1988 SIC classification system. The average AAR is −0.33% among the 2305 goods producers and −0.28% among the 2589 service firms.

We display the industry (SIC 4-digit) and firm-level AARs in Fig. 6. As with PNTR there is considerable variation in AAR among both goods and service firms and this variation occurs *within* narrow industries. We similarly observe that larger firms tend to have higher AAR than smaller firms within the same 4-digit SIC industry. This pattern holds both for goods and service firms.

[36] For example, in the years leading up to CUSFTA, AT&T, GTE and Rockwell International had complained to the US Trade Representative about favoritism shown towards Bell-Canada in public procurement (Chase, 2009).

[37] Trefler (2004) documents substantial reallocation between sectors and plants within Canadian manufacturing following its passage, while Breinlich (2014) and Thompson (1993) show that abnormal returns during CUSFTA are consistent with Canadian firms' and industries' *ex ante* characteristics.

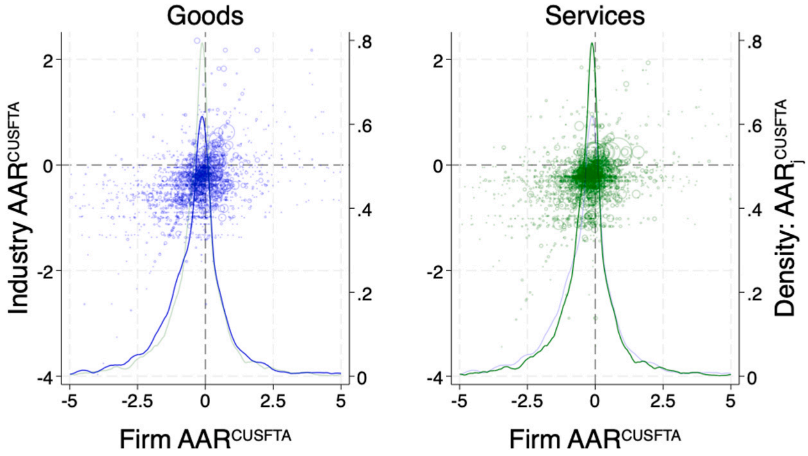


**Fig. 6.** Firm- versus industry-level average abnormal returns .
*Source:* CRSP, COMPUSTAT and authors' calculations. Figure compares firms' $AAR_j^{CUSFTA}$ to the unweighted average industry $AAR_i^{CUSFTA}$ of their primary 4-digit SIC industry. Values below −5 and above 5 percent are dropped to improve readability. Each point's size is scaled to the firm's market capitalization in 1988.

### 4.2. Validity of $AAR_j^{CUSFTA}$

In Table 8 we perform a contemporaneous validation of $AAR_j^{CUSFTA}$ by comparing them to the agreement's terms using the same specification employed in Table 2 for PNTR. In column 1 we explore the relationship between tariff changes and AARs among goods producers. For each US firm $j$, we compute the weighted average change in Canadian ($\Delta\tau_j^{Can}$) and US ($\Delta\tau_j^{USA}$) tariffs, using the firms' sales across its goods-producing business segments as weights.[38]

The results in the column indicate that a one standard deviation reduction in Canadian tariffs corresponds to an *increase* in US $AAR_j^{CUSFTA}$ of 0.048 standard deviations, while a commensurate reduction in US tariffs corresponds to 0.061 standard deviation *reduction* in US $AAR_j^{CUSFTA}$. These relationships are intuitive: US firms facing reduced Canadian tariffs are expected to benefit from increased market access, while those in industries in which the US is lowering tariffs are expected to suffer from increased import competition.

In the second column of Table 8 we perform a similar exercise including both goods firms and service firms, for which tariffs are not defined, by regressing their $AAR_j^{CUSFTA}$ on an indicator variable which takes the value of 1 for service industries covered by a change in national treatment as well as a separate dummy variable for goods firms.[39] As indicated in the table, we find that $AAR_j^{CUSFTA}$ are on average 0.1 standard deviations greater for firms in sectors experiencing a change in national treatment than those service firms operating in non-covered service sectors.

### 4.3. $AAR_j^{CUSFTA}$ and subsequent firm outcomes

We now explore the relationship between firms' $AAR_j^{CUSFTA}$ and subsequent firm outcomes. We estimates these relationships from 1978 to 1993 using the baseline difference-in-differences specification discussed in Section 3, and outlined in Eq. (10). The post period in this setting is defined as 1989 to 1993. Suppressed for space, we include the same controls as in our PNTR application, measured in 1978 and interacted with a post dummy.[40] Results are reported separately in Table 9 for all firms, goods producers, and service firms.

Two trends stand out. First, we find no statistically significant relationship between $AAR_j^{CUSFTA}$ and outcomes overall or among goods-producing firms. To understand this result, note that $AAR_j^{CUSFTA}$ reflect the effects of both Canadian and US tariff changes on firms' value. This result suggests that the two channels potentially offset one another. Consistent with this idea, we show in Fig. 7 that the US and Canadian tariff cuts exhibit a strong positive correlation. With few exceptions firms expected to benefit from increased Canadian market access were similarly exposed to the pro-competitive effects of reduced US tariffs in their segments.

Further, in Table A.11 of the Appendix, we show that $\Delta\tau_j^{Canada}$ and $\Delta\tau_j^{US}$ also fail to predict subsequent firm outcomes among goods producers. That neither $AAR_j^{CUSFTA}$ nor bilateral tariff changes explain subsequent economic outcomes for these firms

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[38] Sales are as of 1978 or the first year in which the firm appears in our sample. Business segments are recorded according to 4-digit SIC industries. All variables have been divided by their standard deviations.

[39] These industries are listed in Section 14, Annex 1408 of the CUSFTA. Transportation, basic telecommunications, doctors, dentists, lawyers, childcare, and government-provided services were not included.

[40] These are PPE per Worker, Log Market Capitalization, Cash Flows to Assets, Book Leverage, and Tobins Q. In contrast to our results for PNTR, we do not report results for intangible capital as those data, from Peters and Taylor (2017), are not available during the CUSFTA sample period.

**Table 8**
US firms' $AAR_j^{CUSFTA}$ versus tariff changes and firm attributes.
*Source:* CRSP, COMPUSTAT, Trefler (2004) and authors' calculations.

| | (1) $\text{AAR}_j^{CUSFTA}$ | (2) $\text{AAR}_j^{CUSFTA}$ |
|---|---|---|
| $\Delta\tau_j^{CAN}$ | −0.048 | |
| | (0.021) | |
| $\Delta\tau_j^{USA}$ | 0.061 | |
| | (0.024) | |
| National treatment $\text{Change}_j$ | | 0.107 |
| | | (0.049) |
| $\ln(\text{PPE per worker})_j$ | −0.012 | 0.040 |
| | (0.037) | (0.020) |
| $\ln(\text{Mkt Cap})_j$ | 0.024 | 0.018 |
| | (0.025) | (0.018) |
| $\frac{CashFlows}{Assets}{}_j$ | 0.102 | 0.083 |
| | (0.035) | (0.027) |
| Book $\text{Leverage}_j$ | 0.043 | −0.013 |
| | (0.031) | (0.019) |
| Tobins $\text{Q}_j$ | 0.003 | −0.020 |
| | (0.034) | (0.018) |
| $I(Goods_j)$ | | 0.021 |
| | | (0.044) |
| Constant | −0.035 | −0.050 |
| | (0.023) | (0.037) |
| Observations | 2065 | 3938 |
| $R^2$ | 0.017 | 0.012 |

Table presents firm-level OLS regressions of $AAR_j^{CUSFTA}$ on US and Canadian tariff changes between 1988 and 1996 and a series of 1988 firm accounting attributes that are winsorized at the 1 percent level. Tariffs are defined at the 4-digit SIC level, and are weighted by segment sales within firms. All covariates are de-meaned and divided by their standard deviation. Standard errors are reported below coefficient estimates and are clustered by 3-digit SIC industries.

**Table 9**
$AAR_i^{CUSFTA}$ and firm sales, COGS and operating profit.
*Source:* CRSP, COMPUSTAT and authors' calculations.

| | $\ln(Sales_j)$ | $\ln(COGS_j)$ | $\ln(OperatingProfit_j)$ | $\ln(Employment_j)$ | $\ln(PPE_j)$ |
|---|---|---|---|---|---|
| | Panel A: All firms | | | | |
| Post*$AAR_j^{CUSFTA}$ | 0.030 | 0.024 | 0.028 | 0.026 | 0.017 |
| | (0.021) | (0.020) | (0.017) | (0.016) | (0.017) |
| $R^2$ | .937 | .937 | .925 | .939 | .951 |
| Observations | 47 386 | 47 397 | 45 905 | 46 980 | 47 403 |
| Unique firms | 4144 | 4146 | 4068 | 4144 | 4155 |
| | Panel B: Goods producers | | | | |
| Post*$AAR_j^{CUSFTA}$ | −0.015 | −0.015 | −0.001 | 0.007 | −0.011 |
| | (0.021) | (0.021) | (0.019) | (0.020) | (0.019) |
| $R^2$ | .945 | .945 | .933 | .952 | .955 |
| Observations | 27 202 | 27 212 | 26 393 | 27 099 | 27 349 |
| Unique firms | 2256 | 2256 | 2210 | 2266 | 2269 |
| | Panel C: Service firms | | | | |
| Post*$AAR_j^{CUSFTA}$ | 0.092 | 0.078 | 0.067 | 0.047 | 0.059 |
| | (0.032) | (0.034) | (0.026) | (0.027) | (0.031) |
| $R^2$ | .924 | .924 | .913 | .921 | .946 |
| Observations | 20 184 | 20 185 | 19 512 | 19 881 | 20 054 |
| Unique firms | 1888 | 1890 | 1858 | 1878 | 1886 |

Table presents firm-level OLS DID panel regressions of noted US firm outcomes on firms' CUSFTA average abnormal returns ($AAR_j^{CUSFTA}$) and a series of 1978 firm accounting attributes that are winsorized at the 1 percent level. Sample period is 1978 to 1993. All covariates are de-meaned and divided by their standard deviation. Standard errors are reported below coefficient estimates and are clustered by 3-digit SIC industries.

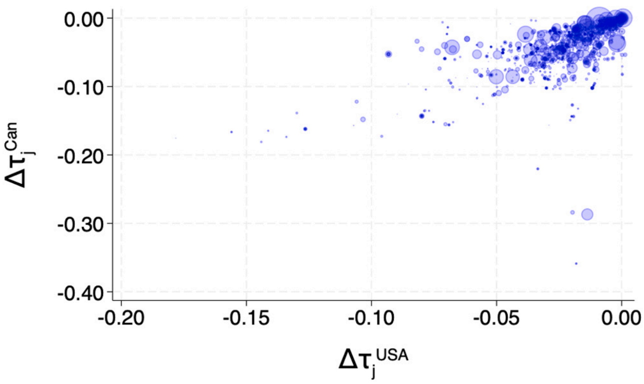



**Fig. 7.** Exposure to US and Canadian tariff cuts. *Source:* CRSP, COMPUSTAT and authors' calculations. Figure compares firms' sales-weighted average US and Canadian tariff cut exposure during CUSFTA. Averages are based on firms major 4-digit sic segments. Each point's size is scaled to the firm's market capitalization in 1988.

suggests that the cumulative effects of CUSFTA on manufacturing firms were small, or that any such effects take place outside of our period of analysis.[41]

By contrast, the final panel of Table 9 shows that the sales and operating profit of service firms do exhibit a strong relationship with $AAR_j^{CUSFTA}$. This relationship is consistent with the agreement's provisions with respect to national treatment of services noted above, as well as US comparative advantage in services more generally (Fort et al., 2018; Ding et al., 2019). Together, the results for goods and service firms suggest that a standard analysis of CUSFTA focused on manufacturing and relying on tariff-based metrics of exposure offers an incomplete picture of this liberalization.

### *4.4. Distributional implications*

In this section we use the procedure outlined in Section 3.4 to examine the impact of CUSFTA across firms. As in that section, we compute firms' cumulative relative predicted change in operating profit and employment using the (non-standardized) baseline DID coefficients from the last section, and plot these predictions against firms' market capitalization.[42] As indicated in Fig. 8 (versus Figs. 3 and 4), we find that while the reaction of operating profit to CUSFTA is qualitatively similar to that found for PNTR, the employment response differs starkly. In both liberalizations, cumulative relative predicted operating profit declines with market capitalization, in this case up to a threshold of about 1 billion. Employment also declines, up to a market capitalization of about 5 billion, but then begins *increasing*, as the largest firms' predicted relative employment turns positive.

Some intuition for this difference can be found in Fig. 9, which compares firms' labor productivity during the two liberalizations. As illustrated in the figure, the largest firms during CUSFTA exhibit relatively high levels of employment per operating profit than the largest firms during PNTR. Given this difference, the largest firms' expansion of operating expansion may have required relatively more employment after CUSFTA than after PNTR.

Fig. 10, analogous to Fig. 5 for PNTR, reports the cumulative relative change in operating profit and employment by groups of 2-digit SIC sectors for which we observe a large number of firms. Here, too, sectors vary substantially in their predicted relative changes, with firms in Telecommunications (48), particularly AT&T, exhibiting large relative increases in predicted employment and operating profit, consistent with the agreement's national treatment provisions. Reactions in manufacturing, by contrast, are very modest.

## 5. Comparing liberalizations

The DID coefficients presented in Tables 6 and 9 represent elasticities between abnormal returns – which capture the market's assessment of the *expected* impact of the change in policy on firm outcomes – and their *ex-post* realizations. They reveal that a 1 percent increase in abnormal returns is associated with a 12.9 versus 1.6 log point relative increase in operating profits for PNTR

[41] US and Canadian tariff reductions were to be phased in over ten years, and there is some evidence that most of the change in trade associated with the agreement occurred in the later years (Besedes et al., 2020). Assessment of post CUSFTA trends (after 1993), however, is complicated by the fact that during the CUSFTA phase-in period, the United States, Canada and Mexico negotiated and implemented the North American Free Trade Agreement (NAFTA).

[42] We focus on these outcomes because we find no statistically significant relationship with respect to physical capital. A version of this figure separately identifying goods versus service firms – indicating that the largest relative gains are experienced by service firms – is available upon request.

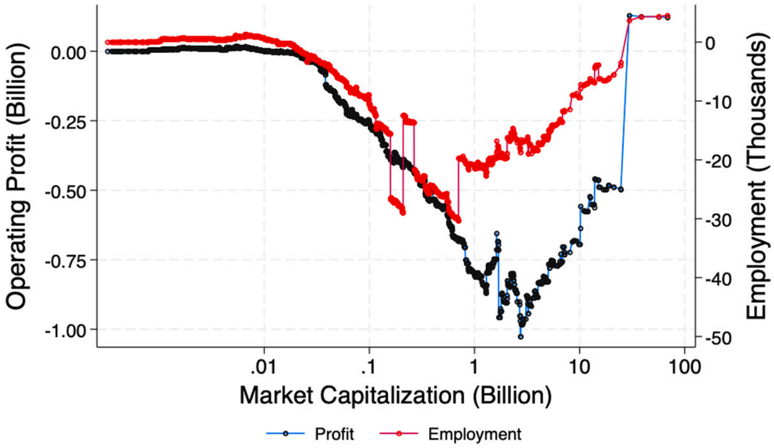



**Fig. 8.** Cumulative relative change in operating profit and employment.
*Source:* CRSP, COMPUSTAT, and authors' calculations. Figure displays the predicted cumulative relative change in goods versus service firms' operating profit implied by the baseline difference-in-differences estimates in Table 6.

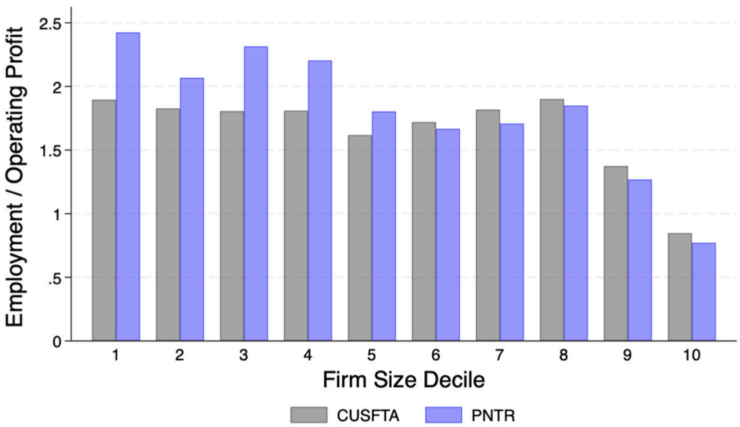



**Fig. 9.** Distribution of $\frac{Employment}{Operating\,Profit}$ by firm size decile.
*Source:* CRSP, COMPUSTAT, and authors' calculations. Figure displays the share of operating profit over employment accounted for by firms in each decile of firm size during CUSFTA and PNTR respectively. Firm size is the market capitalization of the firm prior to the relevant liberalization.

and CUSFTA, respectively.[43] As discussed in greater detail in Appendix G, these elasticities can vary across liberalizations due to the extent to which they impact cash flows versus discount rates, underlying heterogeneity in the speed with which policies' effects are realized, their persistence, and the degree to which the realized effects align with *ex ante* expectations. In this section we explore each of these explanations for greater insight into the relatively strong estimated effect of PNTR versus CUSFTA.

We begin by ruling out three issues associated with identification of the elasticities that can interfere with their comparability. First, we show that the disparity in the two policies' estimated effects is not driven by period-specific differences in the relationship between AARs and outcomes, e.g., future cash flows being discounted more heavily in the 2000's than in the early 1990's. Second, we demonstrate that this difference is not due to heterogeneous firm exposure to other events that might contaminate our estimates of exposure to the trade liberalizations. Third, we show that the estimated differences between PNTR and CUSFTA cannot be fully explained by partial anticipation of the former's passage prior to the first legislative event. Finally, we do find evidence that PNTR

[43] As noted above, Tables 6 and 9 report standardized coefficients (i.e., the data for each liberalization are de-meaned and divided by their standard deviation). Here, we restate our estimates using un-standardized data so the elasticities can be compared directly in terms of change in outcomes per 1 percent change in firm value.

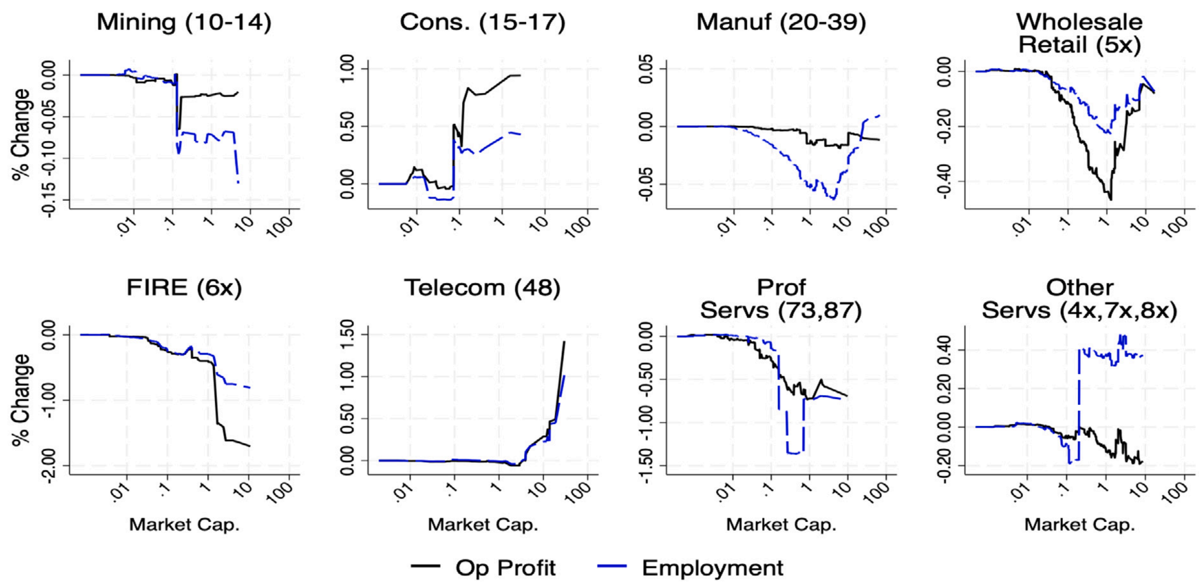
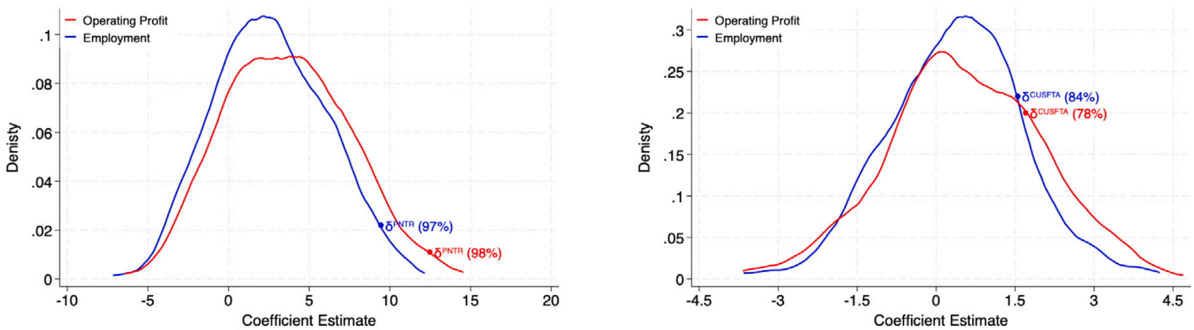



**Fig. 10.** Cumulative relative changes by sector .
*Source:* CRSP, COMPUSTAT, and authors' calculations. Figure displays the predicted cumulative relative change in 2 firm outcomes implied by the baseline difference-in-differences estimates in Table 9 by noted 2-digit SIC sectors. $Y$-axis reports the cumulative predicted relative change as a share of the initial total of each outcome across firms in 1988, prior to CUSFTA. Each firm appears only in one panel, according to the SIC code of largest business segment in Compustat. Firms' market capitalization is calculated from CRSP immediately preceding our event window on used in construction of AAR for CUSFTA. Note that y-axes vary across panels.



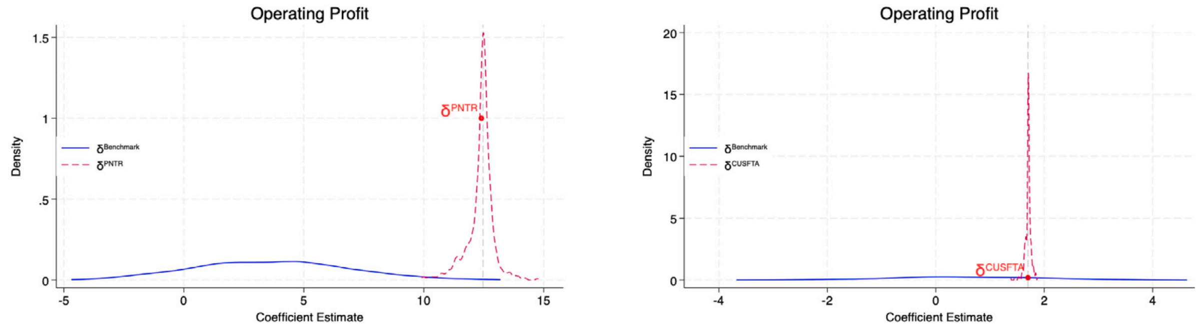

(a) PNTR

(b) CUSFTA

**Fig. 11.** Benchmark coefficient distributions. . (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
*Source:* CRSP, COMPUSTAT and authors' calculations. Left and right panels plot the distributions of $\hat{\delta}^{Benchmark}$ for PNTR and CUSFTA derived from the bootstrap-like procedure described in the main text. Highlighted points in each distribution show the location of the equivalent (non-standardized) baseline DID results reported in Tables 6 and 9. The means and standard deviations of the distributions for operating profit are 3.6 and 3.8 for PNTR and 0.57 and 1.44 for CUSFTA. The analogous statistics for employment are 2.5 and 3.5 for PNTR and 0.37 and 1.22 for CUSFTA.

had a more immediate and durable impact on operating profits than CUSFTA, and provide suggestive evidence that this discrepancy might have been driven by greater-than-anticipated Chinese growth.

*Macroeconomic Environment*: One possible explanation for the larger coefficients during PNTR is that we are simply picking up differences in the macroeconomic environment during these liberalizations, rather than the effects of the liberalizations directly. For example, fears of an impending recession due to the bursting of the tech bubble might induce investors to discount all future profits more heavily (not just those related to our trade liberalizations) which would result in a larger elasticity between AAR's and operating profits. In that case we might observe larger elasticities during PNTR, but this would have little to do with differences between PNTR and CUSFTA. To explore this possibility directly, we use a bootstrap procedure to compare the elasticity for each liberalization to a benchmark distribution of similarly constructed elasticities from each period.




**(a)** PNTR **(b)** CUSFTA

**Fig. 12.** AAR - Operating profit estimate stability .
*Source:* CRSP, COMPUSTAT and authors' calculations. Dashed red lines in each panel plot the distribution of (non-standardized) DID terms of interest for operating profit (i.e., $\hat{\delta}^{PNTR}$ and $\hat{\delta}^{CUSFTA}$) from an amended version of Eq. (10) which includes both the actual and random average abnormal returns for each policy, e.g., $AAR_j^{PNTR}$ and $AAR_j^{Benchmark}$. For comparison, blue lines in each panel plot the $\hat{\delta}^{Benchmark}$ for operating profit for each policy displayed in Fig. 11.

Specifically, for PNTR, we repeat the following steps 1000 times: (i) draw five random non-PNTR trading days in 2000; (ii) compute average abnormal returns from 2 days before until 2 days after these dates (25 days in all ensuring that none of the event windows overlap); (iii) substitute these $AAR_j^{Random}$ for $AAR_j^{PNTR}$ in our baseline DID regressions to estimate a series of $\hat{\delta}^{Benchmark}$ DID coefficients. For CUSFTA, we use an analogous procedure befitting that liberalization, i.e., we sample dates from 1988 and compute abnormal returns in the 5 day window centered on each date. We then highlight where our baseline point estimates fall in these benchmark distributions.[44]

Fig. 11 plots the $\hat{\delta}^{Benchmark}$ DID estimates for each liberalization for both operating profit (red line) and employment (blue line). As expected, $\hat{\delta}^{Benchmark}$ are predominantly positive: on average, higher $AAR_j^{Benchmark}$ are associated with a subsequent relative expansion of both operating and employment.

Consistent with operating profits being discounted more heavily during 2000 than 1988, we do see that the $\delta^{Benchmark}$ distributions are right shifted during PNTR as compared to CUSFTA. However, even within its own benchmark distribution, the elasticities for PNTR stand out. The actual DID coefficients for PNTR, noted by the red and blue dots at the 99th and 97th percentiles in the left panel, lie further to the right of their $\hat{\delta}^{Benchmark}$ distributions than those for CUSFTA, at the 85th and 77th percentiles in the right panel.[45] This difference indicates that the predicted relative changes in outcomes per unit of increase in firm value are stronger for PNTR than CUSFTA, even relative to the generally stronger elasticity of outcomes to $AAR_j^{Benchmark}$ exhibited during the year of PNTR's passage.

*Omitted Variables*: In Fig. 12, we address a related but distinct concern – that our measures of exposure to trade liberalization may be picking up firms' exposure to other events during the same year – a standard omitted variables problem. To address this possibility we re-estimate our baseline difference-in-difference specification but also control for each of the AAR benchmark draws sequentially in a distinct regression. We focus on operating profit, but note that results are similar for other outcomes. Each panel contains two distributions. The dashed red lines trace out the DID coefficients of interest from the modified specification containing one of the $AAR_j^{Benchmark}$ draws, with dots indicating the (non-standardized) baseline DID estimates from Tables 6 and 9. We interpret the estimates along these lines as the impact of each change in policy net of potentially confounding events in the surrounding period. For comparison, the solid blue distributions in each panel plot the coefficients on the $AAR_j^{Benchmark}$ controls across the regressions. As indicated in the figure, $\hat{\delta}^{PNTR}$ and $\hat{\delta}^{CUSFTA}$ remain tightly distributed around our baseline estimates, while $\hat{\delta}^{Benchmark}$ are diffuse. The stability of the former indicate that our baseline estimates do not spuriously capture the influence of confounding events, and that the disparity in the strength of $\hat{\delta}^{PNTR}$ versus $\hat{\delta}^{CUSFTA}$ is robust to exposure to unspecified events unfolding around them.[46]

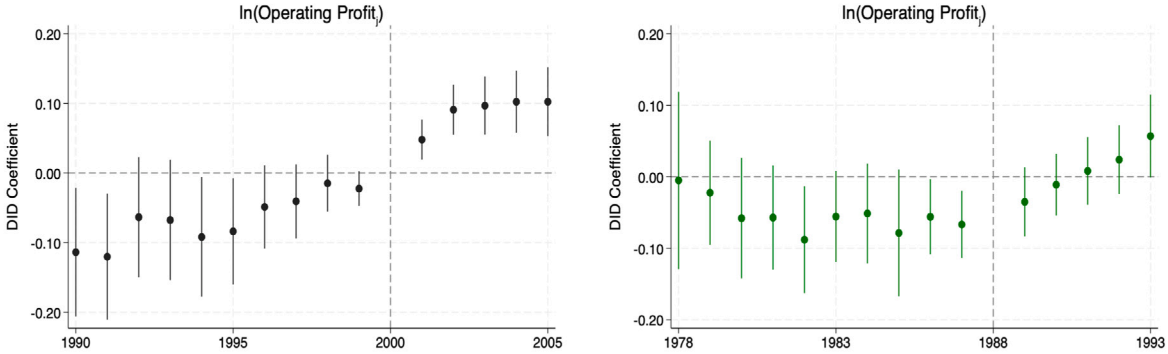
*Anticipation*: Another factor that could affect the relative size of $\hat{\delta}^{PNTR}$ and $\hat{\delta}^{CUSFTA}$ is partial anticipation of each policy's ultimate passage *prior* to our event windows, leading to underestimations of their true effects and thereby higher estimated DID coefficients. For PNTR, we are able to estimate the market's beliefs about the probability that the legislation would ultimately pass prior to the first event using options pricing data in the manner developed by Langer and Lemoine (2019), discussed in detail in Appendix A.[47]

[44] For CUSFTA this amounts to 244 benchmark dates.

[45] In contrast to the results reported in Tables 6 and 9, the DID coefficients displayed in Fig. 11 are derived from *non-standardized* covariates. This switch is necessary for an apples-to-apples comparison of the two sets of DID coefficients, since a one standard deviation increase in $AAR$ on days with a greater variance would represents a larger increase in $AAR$ *in levels* than a 1 standard deviation increase on days with lower variance. As a result, the DID coefficients in the figure should be interpreted as the impact of a 1 percent increase in $AAR$.

[46] We note that the inclusion of $AAR_j^{Benchmark}$ never makes $\hat{\delta}^{PNTR}$ statistically insignificant, nor do they make $\hat{\delta}^{CUSFTA}$ statistically significant.

[47] Unfortunately, unavailability of call option data as far back as CUSFTA prevents us from performing an analogous computation for that liberalization.




(a) PNTR (b) CUSFTA

**Fig. 13.** Operating profits and AAR: annual specification.
*Source:* CRSP, COMPUSTAT and authors' calculations. Figure displays a series of 95 percent confidence intervals for the difference-in-difference term of interest in Eq. (12). Each panel is from a separate, firm-level OLS regression of noted firm outcome on PNTR average abnormal returns ($AAR_j^{PNTR}$) interacted with a full set of year dummy variables as well as a series of initial (1990) firm accounting attributes, also interacted with year dummy variables and winsorized at the 1 percent level. Sample period is 1990 to 2006. Effects of CUSFTA are analogously estimated from 1978 to 1994. All covariates are de-meaned and divided by their standard deviations. The covariates for the year of the policy change are omitted. Standard errors used to construct confidence intervals are clustered at the 4-digit NAICS level and 3-digit SIC respectively.

We find that the market assigned a 12 percent probability to PNTR's passage prior to the introduction of the bill in the House. We can account for this anticipation by deflating estimates of $\hat{\delta}^{PNTR}$ in Tables 6 and 7 by (1/(1-.12)). For operating profit and employment, this procedure reduces the estimates from 0.129 and 0.098 to 0.113 and 0.086, respectively. While these adjustments are not trivial, they imply our PNTR estimates merely fall to the 98.6 and 95.9 percentiles of the benchmark distributions displayed in Fig. 12, still well above those for CUSFTA.

*Speed and Persistence*: We compare PNTR and CUSFTA in terms of speed of onset and duration using "annual" specifications that replace the single DID term in Eq. (10) with interactions of $AAR$s and a full set of year dummies, e.g., for PNTR:

$$ln(Outcome_{j,t}) = \sum_{y=1990}^{2006} \delta_y \times 1\{t=y\} \times AAR_j^{PNTR} + \sum_{y=1990}^{2006} 1\{t=y\} \times \mathbf{X_j} \boldsymbol{\gamma_y} + \alpha_j + \alpha_t + \epsilon_{j,t}. \tag{12}$$

Results for operating profit are displayed in Fig. 13 where, for PNTR, we report the results for all firms and for CUSFTA we focus on service firms, as the baseline estimates for goods-producing firms are statistically insignificant.[48] As indicated in the figure, we find that PNTR affects firms' relative operating profit both more quickly and more durably than CUSFTA, consistent with the sharp and persistent impact of Chinese imports on US industries and regions noted by Pierce and Schott (2016) and Autor et al. (2021).

*Expectations about Chinese Growth*: A plausible explanation for the relatively sharp reaction of US firms to PNTR displayed in Fig. 13 is that investors under-anticipated the magnitude of the effects of the trade liberalization with China compared to Canada. This under-anticipation might be driven by the relative difficulty of predicting a change in tariff rate uncertainty versus tariffs or national treatment (Handley and Limão, 2017), or the fact that China's unprecedented growth in the years after PNTR outpaced even the most informed forecasts. With respect to the latter, Fig. 14 reports the persistent gap between China's actual growth in real GDP as estimated by the IMF and World Bank versus one-year-ahead forecasts made by the IMF, from 1999 to 2007. If the anticipated effects of PNTR on firms relied on such forecasts, then the realized effects of PNTR on firms' operating profit and employment would have been more extreme than were priced in at the time of the change in policy. This explanation need not imply market inefficiency or uninformed market participants, but rather imperfect foresight. It receives further support from Bombardini et al. (2023), who find that US politicians underestimated the magnitude of PNTR at the time of its passage, a potential factor in the subsequent political backlash documented by Rodrik (2021), Che et al. (2016) and Autor et al. (2017a).

## 6. Robustness exercises

Our results for PNTR and CUSFTA are robust to a broad range of alternative specifications and assumptions. For the sake of brevity, we relegate these tests to the Online Appendix and briefly describe them here. In Section A we include a formal discussion of

[48] Figures for all other outcomes may be found in Figures A.8 and A.9. Results are qualitatively similar when including NAICS-2 by year fixed effects or additional controls.

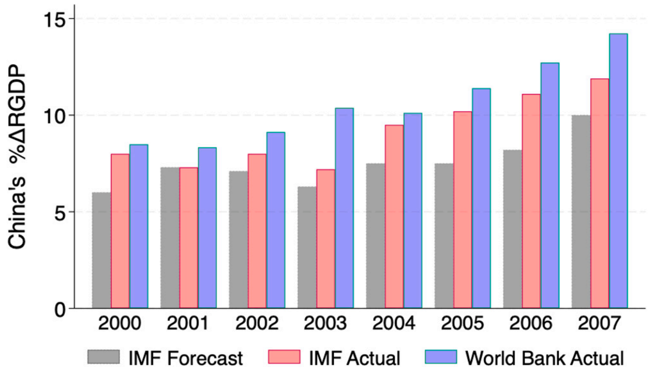



**Fig. 14.** Forecasted vs. realized Chinese RGDP growth.
*Source:* Forecasts are one-year forecasted RGDP growth for the indicated year taken from the IMF World Economic Outlook annual reports. Adjacent bars indicate realized RGDP growth calculated by the IMF and World Bank respectively for the indicated year (World Bank RGDP growth series can be found at https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?locations=CN).

the effect of partial anticipation of PNTR events on $AAR$s and show that such anticipation does not affect our main results. In Section B we demonstrate that our baseline difference-in-differences estimates are robust to a number of changes in estimation strategy, including: (1) re-estimation of Eq. (10) for each of our five policy events separately; (2) weighting each regression by the 1990 level of the dependent variable; (3) including 2-digit NAICS-by-year fixed effects; (4) using a one-day $[-1, 1]$ rather than two-day window around each event in computing $AAR_j^{PNTR}$; (5) estimating $AAR_j^{PNTR}$ using a popular alternative to the CAPM, the Fama and French (1993) three-factor model; (6) eliminating observations in our event windows that occur at the same time as earnings, dividend announcements, mergers and acquisitions (M&A), stock repurchases, and seasoned equity offering (SEO) announcements; (7) using buy-and-hold abnormal returns rather than average abnormal returns; (8) using bootstrapping to address sampling error in firms' estimated factor loading in the CAPM, $\hat{\beta}_j$s; and (9) allowing for non-zero systematic effects of PNTR on market returns.

## 7. Conclusion

We introduce a method for gauging firms' exposure to changes in trade policy based on abnormal equity returns, and use this method to measure US firms' exposure to trade liberalizations with China and Canada. With respect to China, we find that firms' average abnormal returns surrounding key legislative milestones associated with the liberalization vary widely within industries, that they are correlated with standard variables used to assess import competition, and that they provide explanatory power beyond these standard measures. Among both service and goods-producing firms, we find a strong relationship between firm size and predicted relative gains in operating profit, employment and capital. We also find stark differences in traders' assessment of subsequent relative operating profit across broad 2-digit NAICS sectors. For CUSFTA, we demonstrate that goods firms' average abnormal returns are correlated with US and Canadian tariff changes, while for service firms they are higher in industries subject to national treatment. For service firms, we also find that firms' average abnormal returns predict future operating profit, underscoring our method's ability to evaluate the removal of trade restrictions outside the manufacturing sector.

Our study highlights several important advantages to using equity market reactions to assess the impact of changes in trade policy. First, these reactions capture direct as well as indirect channels of exposure. Second, they are readily available for firms in all sectors of the economy in which firms are publicly traded. Finally, they can be used to quantify the effect of non-tariff barriers, which are notoriously difficult to capture using standard measures of exposure (Goldberg and Pavcnik, 2016). We hope use of the measure of exposure we propose will prove useful for further extending international trade research into these areas, and in examining impacts of policy exposure more broadly, e.g., in understanding firm responses to changes in the minimum wage.


## Declaration of competing interest

The authors have no relevant or material financial interests that relate to the research described in this paper. None of my close relatives or partners received financial support or have any paid or unpaid positions as officer, director, or board member of relevant non-profit organizations or profit-making entities. No party had the right to review this paper prior to its circulation. This paper did not require IRB approval.

## Data availability

Using Equity Markets to Infer Exposure to Trade Liberalization (Original data) (Mendeley Data)

## Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jinteco.2024.104000.

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