Firm Finances and Responses to Trade Liberalization: Evidence from U.S. Tariffs on China

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Abstract

This paper examines the relationship between a firm's finances and its response to trade liberalization. Using a landmark change in U.S. tariff policy vis-à-vis Chinese imports and micro level data from the U.S. Census Bureau, I find *larger* manufacturing job losses in better capitalized firms - those with less leverage and more cash on hand. The effects concentrate in industries where weaker balance sheets are likely to lead to collateral and other borrowing constraints, helping rule out alternative explanations. Finally, domestic manufacturing job losses are not accompanied by greater reductions in sales or aggregate employment, but better capitalized firms do exhibit reduced input costs and increased productivity. These findings point to offshoring as the predominant firm response to trade liberalization and suggest a role for financial capacity in facilitating offshoring investments.

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I. Introduction

China's integration into global markets has induced wholesale changes in the U.S. economy and, consequently, in economists' understanding of the impacts of trade. While consumers and manufacturing workers stand out respectively as winners and losers, the implications for other economic entities are less clear. In particular, how trade liberalization affects domestic manufacturing firms, who are responsible for the mass layoffs, is still an open question. Increased import competition may represent a threat for many U.S. firms, especially those lacking the financial resources to properly respond.[1] However, the opening of Chinese labor markets and the possibility of offshoring likely represents an important opportunity for firms with the financial wherewithal to invest in overseas production and import networks. While both channels translate to large domestic job losses, the underlying mechanisms and the firms responsible for the losses are very different.

In this paper, I examine the relationship between a firm's finances and its response to the conferral of Permanent Normal Trade Relations (PNTR) status to China - a landmark reduction in trade barriers that occurred in 2001. My analysis has two primary objectives. The first is to assess whether a firm's financial standing shapes its response to the threats and opportunities precipitated by trade liberalization. The second is to shed light on the predominant mechanism of trade adjustment in the U.S. economy. Using restricted micro-data from the U.S. Census Bureau, I find larger employment reductions at well capitalized firms (those with higher cash reserves and lower debt ratios), consistent with offshoring representing the dominant response to the PNTR. Further supporting this interpretation, I find that job losses are accompanied by a reduction in input costs and increased productivity at surviving domestic operations. All together, these findings establish a material connection between a firm's financial policy and its response to liberalized trade.

China's economic growth serves as a unique laboratory for studying the effects of and responses to trade liberalization. The "China Shock" is both large and unexpected, rendering it perhaps the closest approximation to a natural experiment in trade economics (Autor et al. (2016b)). PNTR specifically represents a landmark reduction in trade barriers with China that has been directly linked to U.S. manufacturing job losses in the most affected industries (Pierce and Schott (2016)). The policy made permanent previously temporary tariff reductions on Chinese imports, removing

[1] A long-standing literature links financial fragility to competitive vulnerability. See, for example, Bolton and Scharfstein (1990), Zingales (1998), and Fresard (2010).

uncertainty about future trade costs.[2] Crucially, exposure to PNTR depends on industry specific tariffs set by the Smoot-Hawley Tariff Act of 1930, which are unlikely to be incidentally related to changes in firm policies around the time of PNTR's adoption. I therefore exploit this exogenous industry-level variation in the difference between the Smoot-Hawley tariffs and the normal trade relations (NTR) tariffs the PNTR made permanent (the *NTR Gap*) to measure the policy's effects.

I first examine how employment, which is perhaps the most useful gauge of a firm's domestic manufacturing footprint, adjusts to PNTR. Like Pierce and Schott (2016), I find that more exposed firms - i.e. those operating in higher NTR Gap industries - experience larger post-PNTR employment losses. On average, firms with above-median NTR Gaps exhibit employment declines that are 10% larger than below-median firms. Interestingly, I find that within the high exposure group, stronger balance sheets actually translate to larger employment declines. High NTR Gap firms with below (above) median industry-adjusted leverage (cash) ratios exhibit employment losses that are 6-8% larger than their more poorly capitalized peers. These estimates are robust to controlling for firm size and age, unobserved inter-industry heterogeneity, and employment pre-trends.

I also measure employment responses at the plant level. Among surviving plants, a 10% (5%) decrease (increase) in debt (cash) on average amplifies the sensitivity of employment to PNTR by 70% (120%). That same change in leverage (cash) is associated with a 20% (70%) higher likelihood of plant closure, consistent with a permanent reduction in domestic manufacturing activity.

What drives these employment results? It is unlikely that better financed firms are less suited to face import competition. However, offshoring is a plausible explanation. Trade economists agree that offshoring, whether through direct investment, partnerships with Chinese firms, or production contracting, requires significant upfront investment in physical capital, developing relationships, establishing distribution networks, training, monitoring, and regulatory compliance. Given the established literatures on financing frictions and investment (Fazzari et al. (1988)) and on capital structure and stakeholders (Titman (1984)), it is natural to posit that financially stronger firms are best positioned to capitalize on the offshoring opportunities presented by freer trade.

Of course, a causal inference of how financial frictions shape trade responses requires that the variation in financial ratios, which I measure two year prior to PNTR, be orthogonal to any other

[2] Throughout this paper, the term "Chinese imports" refers to U.S. imports of goods from China. Both arms-length as well as related-party imports from China are subject to tariffs.

unobserved characteristic that may dictate post-PNTR employment. Much of the intra-industry dispersion in firm finances for my sample of mature manufacturing firms plausibly meets this requirement. As Rajan and Zingales (1998) show, mature companies anticipate less incentive to actively manage their capital structures with respect to investment needs.[3] Therefore, owning to hysteresis stemming from adjustment costs (Flannery and Rangan (2006)) and shareholder-creditor conflicts (Admati et al. (2018)), firms with similar optimal capital structures may actually exhibit large and persistent differences in observed capitalization ratios at any given point in time.[4] Of course, absent a way to isolate this "clean" variation, I cannot rule out competing hypotheses in general. However, I perform a battery of tests to rule out multiple alternative explanations.

I begin by addressing the concern that firms adjust their capital structure in anticipation of future offshoring activity. My results are robust to using longer lags of leverage ratios and to using only firms whose pre-PNTR financial ratios are likely unaffected by any preexisting offshoring plans. Next, I address several specific omitted variable concerns. Weak balance sheets may proxy for economic distress. Additionally, capital intensity is robustly correlated with both debt (Titman and Wessels (1988)) and cash (Fresard (2010)) holdings. Finally, multi-national corporations may structure their finances differently than strictly domestic firms. Any of these factors may result in a spurious relationship between firm financing and PNTR-induced employment losses. However, controlling for proxies of each alternative does not materially change the results. Lastly, using a similar argument to Rajan and Zingales (1998), I show that financial capacity leads to different post-PNTR employment responses predominantly for external finance dependent firms - i.e. those that require large-scale investments or that exhibit longer cash-harvest periods. Financially dependent and poorly capitalized firms are more likely to be subject to collateral and other borrowing constraints that severely depress offshoring ability. Therefore, to the extent that inter-industry differences in collateral requirements and project gestation periods are plausibly exogenous, this cross-sectional relationship helps rule out alternative explanations more generally.

To further validate the offshoring hypothesis, I examine several other firm outcomes. First, I show that better capitalized firms do not exhibit sharper reductions in sales in response to PNTR.

[3] The average age of manufacturing firms in my sample as of 1999 is over 20 years. Almazan and Molina (2005) also argue that optimal capital structure calibration is most likely to occur for young firms and show that intra-industry dispersion in leverage is positively correlated with the industry's maturity.

[4] These differences could arise due to past mergers, market timing of security issuance, or changes in tax laws.

This test helps rule out the notion that employment reductions are the result of competitive retrenchment. I also show that the relationship between firm financial structure and post-PNTR employment holds only for domestic manufacturing workers, and not for international or domestic non-manufacturing employees, suggesting firms are substituting away from this labor input rather than instituting across-the-board cuts.

Additionally, I find evidence consistent with the offshoring of intermediate production by better capitalized firms. Using data on materials cost from the quinennial Census of Manufacturers (CMF), I find that a 10% (5%) decrease (increase) in debt (cash) is associated with X% (X%) lower input costs for inputs sourced from high NTR Gap industries. Better capitalized firms also exhibit a significant increase in domestic labor productivity, which is consistent both with firms offshoring lower-skilled tasks and with efficiency gains from offshoring. In contrast, I find no connection between firm finances, PNTR, and domestic machinery expenditures, suggesting automation does not play a significant role in this setting.

I close my analysis by relating my findings to the broader question of the effects of trade liberalization on the U.S. economy. Specifically, I argue that firms view offshoring as a benefit of, rather than a necessary competitive response to, PNTR. Anecdotal evidence of the warm corporate reception of the policy supports this interpretation. In addition, I find that firms operating in higher NTR Gap industries exhibit larger abnormal returns in response to both the initial trade agreement between China and the U.S. as well as the actual PNTR vote.[5] Across the two event dates, firms on average experience 0.45% higher abnormal returns for each 10% increase in the NTR gap. Also, I show that higher exposure to PNTR translates to greater Chinese import growth only in product markets open to foreign direct investment (FDI). A 10% increase in the NTR gap leads to 4.8% growth of Chinese imports for products in which the Chinese goverment permits FDI, but to (insignificantly) negative growth when FDI is restricted. This finding is more consistent with offshoring investments driving Chinese import growth rather than the other way around.

This paper contributes to the growing literature examining the economic effects of Chinese trade on the U.S. economy (Autor et al. (2013), Autor et al. (2014), Acemoglu et al. (2016), Pierce and Schott (2016)).[6] While manufacturing workers undoubtedly suffer from increased trade exposure,

[5] As I argue in Section Appendix C, both events yield material information regarding the effects of Chinese trade liberalization.

[6] See Autor et al. (2016b) for a survey of this literature.

the effect on firms is less conclusive. Autor et al. (2016a) and Hombert and Matray (2018) use supply-side shocks to Chinese manufacturers to argue that import competition drives employment losses in U.S. manufacturers. In contrast, Magyari (2017) shows that firms more exposed to Chinese trade exhibit larger employment growth and Bretscher (2018) finds lower risk premia for industries with higher offshoring potential. This paper also argues that firms benefit from freer trade and offshoring, but focuses on the role of firm finances, which neither Magyari (2017) nor Bretscher (2018) consider.

This paper also relates to the large vein of research documenting a negative association of investment with debt (Whited (1992), Lang et al. (1996)) and a positive association with cash (Blanchard et al. (1994), Lamont (1997), Rauh (2006)). This paper studies the relationship between financial capacity and offshoring, an investment decision with major implications for labor and product markets. Furthermore, by documenting this relationship, this paper provides empirical evidence of how firms with stronger balance sheets capitalize on opportunities resulting from industrial shakeup. While the role that financial resources play in firms' ability to capitalize on periods of industrial turmoil is theoretically well understood (Shleifer and Vishny (1992)), empirical evidence of this phenomenon is rarer.

Additionally, this paper contributes to the literature on firm finances and international trade.[7] Manova (2013) and Chaney (2016) show theoretically that financial frictions reduce export activity. The extant empirical research studies exporters from emerging markets and Europe to verify these theoretical predictions. This paper extends this logic to U.S. import market distortions as well and focuses on implications for employment rather than trade flows.

Finally, this paper contributes to the literature on firm finances and employment. Papers in this literature either examine the adverse effects of financial frictions on employment (Benmelech et al. (2011), Chodorow-Reich (2013), Duygan-Bump et al. (2015)) or or the relationship between leverage and the cyclicality of employment (Sharpe (1994), Giroud and Mueller (2017)). This paper examines responses to a permanent macroeconomic shock and relates stronger balance sheets to larger employment reductions.

[7] See Fritz Foley and Manova (2015) for a survey of this literature.

II. Institutional Setting & Hypothesis Development

This section describes the institutional setting and develops the hypotheses for the paper. I first detail the most salient features of the PNTR policy. I then discuss why financial frictions may distort a firm's trade response - both with respect to competitive adjustments and offshoring investment - and the implications for this dynamic on employment and other relevant outcomes.

A. *Details of PNTR*

I present an abridged discussion of the PNTR policy and refer interested readers to Pierce and Schott (2016) for further details. On May 24, 2000, the U.S. House of Representatives voted to make permanent the relatively low NTR tariff rate schedule for Chinese goods.[8] President Clinton signed the bill into law in October of that year. Prior to China receiving PNTR status, Chinese imports were subject to the Trade Act of 1974, which called for an annual review of the U.S.-Sino trade relationship and the potential for U.S. tariffs to revert to the much higher non-NTR rates. In this respect, PNTR is unique in that it did not actually reduce effective tariffs, Chinese goods had been taxed at the NTR rates since 1980, but rather it removed uncertainty regarding U.S. tax policy vis-à-vis Chinese goods. Industries with higher NTR Gaps, i.e. the difference between the NTR and non-NTR tariffs, experienced a bigger reduction in uncertainty. PNTR went into effect upon China's entry into the WTO in 2001. Accordingly, and in following with Pierce and Schott (2016), I use 2001 onwards as the post-PNTR period. To calculate industry-level NTR Gaps, I use the ad valorem equivalent NTR and non-NTR tariff rates from 1999 with data provided by Feenstra et al. (2002). These rates are available for product codes using the Harmonized Commodity Description and Coding System (HS). Industry NTR Gaps are averages of NTR Gaps for all of the 8-digit HS products mapped to the industry. Internet Appendix Table B1 lists the 10 highest and lowest NTR Gap industries. Firm-level NTR Gaps are a weighted average of the gaps for each industry in which the firm has domestic production.

Several factors support the conjecture that PNTR materially affects Chinese import growth and domestic manufacturing employment. First, the policy's effect is sufficiently large as to plausibly in-

[8]China's PNTR status required Senate approval as well, but support for the bill in the Senate had always been strong. The Senate passed the measure on September 19.

duce firms to respond. The average NTR Gap in 1999 is 33%.[9] Second, since the NTR Gap is largely driven by variations in non-NTR rates, which were set in the 1930s by the Smoot-Hawley Tariff Act, it is plausibly orthogonal to potential confounding factors. Third, the bill's passage was uncertain up until the day of the House's vote. As late as May 23 of 2000, the White House, which supported the bill, was unsure if it had marshaled enough "yes" votes (Keto (2000)). This uncertainty renders it unlikely that firms adjusted their behavior prior to PNTR's passage. Fourth, economic theory dictates that higher uncertainty hinders corporate investment. For example, Pindyck (1993) shows that the option value of waiting before making sunk investments is increasing in the uncertainty surrounding input costs and Handley and Limao (2015) apply this specific logic to a model of trade cost uncertainty.[10] Finally, as Pierce and Schott (2016) show, the NTR Gap is strongly and causally connected with higher rates of Chinese import growth and bigger drops in domestic manufacturing employment, suggesting it serves as a landmark reduction of trade barriers.

In addition to serving as a useful laboratory for examining the effects of trade liberalization on domestic employment and firm organization, PNTR also facilitates the study of which firms capitalize on the opportunities that arise in the wake of major industrial shakeup. While this dynamic is well understood in theory, empirical evidence - e.g. labor poaching or buying assets from distressed competitors - is relatively hard to obtain.[11] In contrast, the offshoring opportunities that manifested as a result of PNTR - which I discuss in greater detail below - represent a tangible and detectable benefit for firms with financial slack.

In closing this discussion, it is worth noting that PNTR is only one of the relevant components contributing to China's tremendous economic growth, many of which predate the policy. Autor et al. (2013) highlight a myriad of supply-side shocks that increase Chinese export productivity. These include a mass migration from rural to urban areas, the introduction of previously-banned production technologies, and reduced restriction on foreign inputs and capital goods. As both Pierce and Schott (2016) and Amiti et al. (2017) show, PNTR is unrelated to much of the Chinese productivity growth and therefore, the analysis that follows pertains only to the effects of easing

[9] Pierce and Schott (2016) reference several anecdotes of firms indicating that the uncertainty regarding China's NTR status materially affects their operations.

[10] It should be noted that, conditional on the NTR level, a higher NTR Gap implies higher future expected tariffs, so that there exists a first-order effect as well.

[11] Shleifer and Vishny (2011) provide a summary of the relatively scarce empirical evidence.

Chinese trade barriers.[12] In addition, the results in Section IV apply to the set of large public manufacturers that comprise my analysis sample, although Fort et al. (2018) show that these firms may be responsible for the bulk of manufacturing employment losses since the 1970s.

B. *Hypothesis Development*

Each margin of adjustment, be it escaping foreign competition or offshoring, requires significant investment. Given the long-established link between financial capacity and investment, it is reasonable to surmise that poorly capitalized firms are less able to optimally adjust to trade liberalization. Furthermore, as I detail below, market-share-preserving and offshoring investments carry dichotomous implications for domestic employment. Therefore, looking at the trade-induced employment responses at well versus poorly capitalized firms yields important insight not only into how firm finances shape trade responses, but also into the primary mechanism of trade adjustment for domestic manufacturing.

Firms looking to preserve market share in the face of foreign competition may need to make "active" investment in perceived (marketing) and actual (R&D) product differentiation or "passive" investment in the form of price competition. If financial frictions prevent firms from making these investments, one would expect weakly capitalized firms to shed workers as they lose market share to low-cost foreign rivals. In concert with this logic, theoretical (Bolton and Scharfstein (1990)) and empirical (Fresard (2010)) work in corporate finance establishes a link between firms' balance sheets and their resilience to product market competition - with the latter paper examining import competition specifically.[13] Thus, if financial frictions predominantly impair competitive resilience, one should expect larger trade-induced job losses at more poorly capitalized firms.

Alternatively, firms may respond to trade liberalization by offshoring - i.e. substituting from domestic production to arms-length or related-party imports - part or all of their domestic manufacturing facilities. For firm finances to generate meaningful cross-sectional employment predictions under the offshoring hypothesis, two crucial assumptions must be met. The first is that offshored production substitutes for domestic production. The second is that financial frictions may preclude firms from engaging in offshoring.

[12] I discuss the competitive and offshoring implications of supply-side shocks further in Section V.A.

[13] See also Chevalier (1995), Zingales (1998), and Khanna and Tice (2000).

Several recent papers find that firms substitute offshored production for domestic employment. Ebenstein et al. (2014) show that offshoring has put significant downward pressure on U.S. wages. Boehm et al. (2017) show that multinationals account for an out-sized share of domestic manufacturing job losses and connect this finding to increased offshoring. Finally, Monarch et al. (2017) find that offshoring firms exhibit large domestic employment declines relative to a matched sample of non-offshorers. These results lend credence to the hypothesis that offshored production displaces domestic workers.[14]

Firm finances may affect offshoring investments in two ways. First, capital market frictions may inhibit the ability to finance entry into international trade markets. The presence of trade market entry costs, which include capital expenditures, costs of developing supply chains, training costs, costs associated with regulatory compliance, monitoring costs, costs associated with integrating different cultural and business norms, is a hallmark of the "heterogeneous firm" class of models popularized by Melitz (2003). The classical motivation for the high-entry cost Melitz-style models is the strong selection of firms into trade markets. Importing and exporting firms tend to be larger, more productive, and more profitable even prior to engaging in trade. To demonstrate this "offshoring premium," Internet Appendix Table B3 presents summary statistics for the offshoring and non-offshoring manufacturers in the Compustat universe. Similar to the analysis in Monarch et al. (2017), this table depicts differences in firm characteristics, measured in 1999, between firms who subsequently are identified as offshorers and non-offshorers using the data from the Trade Adjustment Assistance (TAA) program (see Appendices A and B). While these univariate correlations come with the standard caveats, they are informative in presenting several stylized facts regarding offshoring firms. Consistent with the entry-cost-induced selection framework, offshoring firms tend to be ex-ante larger, more profitable, more productive, and more capital intense.[15]

The large entry costs emphasized in Melitz (2003) suggest a role for capital market frictions. Manova (2013)) and Chaney (2016) extend the Melitz framework to include financial frictions and show that they interact with these costs to materially distort the patterns of trade. Furthermore,

[14]It should be noted that using BEA data, Desai et al. (2009) and Kovak et al. (2017) do not find that FDI leads to reduced domestic employment. However, they do not use establishment-level data and so cannot distinguish between manufacturing and non-manufacturing employment.

[15]In terms of univariate correlations between financial capacity and (future) offshoring activity, the sample of offshorers hold lower cash balances, and higher leverage ratios. However, accounting for industry drastically reduces even this univariate disparity.

theoretical (Antràs and Helpman (2004)) and empirical (Monarch et al. (2017)) work establishes the existence of similar entry costs into import markets, and anecdotal evidence confirms that the magnitude of these costs is substantial. For example, Lexmark International, a printer manufacturer, estimated costs of $35-$45 million to outsource a portion of its laser printer production to third party contractors in Mexico and China (Bronfenbrenner et al. (2001)). Even more strikingly, Igami (2018) estimates that offshoring costs for hard disk manufactures in the 1990s ranged into the billions of dollars. Given the much-documented relationship between firm finances and investment, financial frictions may very well preclude certain firms from investing in offshoring.

The second mechanism by which firm finances may impact offshoring activities is by impairing investment incentives of foreign counter-parties. Based on the logic articulated in Titman (1984) and Maksimovic and Titman (1991), both Kale and Shahrur (2007) and Banerjee et al. (2008) show that firms reliant on strong stakeholder relationships maintain lower leverage ratios. Building up foreign production and import networks very likely requires just such stakeholder relationships. Therefore, precariously-financed firms at the time of PNTR's passage are in a disadvantageous position in terms of establishing and maintaining these requisite relationships. Thus, if financial frictions impair trade response primarily via the offshoring rather than competition channel, one should expect (weakly) larger trade-induced job losses at better capitalized firms.

The implications for firm finances and trade liberalization should also extend beyond manufacturing employment. If firm finances shape trade responses via the competition channel, then manufacturing employment losses should be accompanied by market share losses as well. Additionally, competitive pressures should lead to employment losses across the entire firm, not just in manufacturing. Both of these conjectures are easily testable.

Alternatively, if firm finances shape trade responses via the offshoring channel, then these effects should manifest in their domestic operations as well. First, since firms presumably offshore to take advantage of lower costs of production (namely labor), offshoring of intermediate production should result in lower input costs for domestic plants. Second, offshoring should also lead to measurable gains in domestic productivity. This effect occurs for two reasons. First, since low-skill and labor intensive tasks are easier to offshore, domestic productivity may obtain via the changing composition of domestic tasks. Second, by allowing it to produce at a lower cost, reduced input costs act as a de-facto productivity gain for domestic labor (Grossman and Rossi-Hansberg (2008)). I rely on confidential production data form the U.S. Census to test these predictions.

To summarize, the existing theoretical and empirical literature leads to the following set of hypotheses regarding the interaction between firm finances and PNTR:

H1A: If financial frictions impair competitive resilience, poorly capitalized firms - those with lower cash holdings or higher leverage - should experience larger manufacturing employment declines in response to PNTR.

H1B: If financial frictions impair competitive resilience, firms with the largest declines in manufacturing employment declines should also experience larger sales and aggregate employment reductions in response to PNTR.

H2A: If financial frictions impair offshoring ability, well capitalized firms - those with higher cash holdings or lower leverage - should experience larger manufacturing employment declines in response to PNTR.

H2B: If financial frictions impair offshoring ability, firms with the largest manufacturing employment declines should also experience larger domestic productivity gains in response to PNTR.

III. Data & Empirical Strategy

A. Data Sources

A.1. Census Bureau Data

I obtain a time-series of domestic manufacturing employment using restricted-access microdata from the U.S. Census Bureau. The longitudinal business database (LBD) contains an annual snapshot of employment, annual payroll, location, primary industry, and owning firm for virtually every establishment with employment in the non-farm private U.S. economy.[16] With this data I calculate a time series of domestic manufacturing employment at either the firm or plant level. The precise quantification of domestic manufacturing employment in the Census data renders it superior to employment data from Compustat, which includes both international and non-manufacturing (e.g. sales and administrative) workers.

I also rely on detailed production data from the Census. The quinquennial Census of Manufacturers (CMF), conducted in years ending in 2 and 7, surveys the universe of manufacturing

[16] See Jarmin and Miranda (2002) for an overview of the LBD.

establishments in the U.S.[17] I use output data from 1997 to construct a firm-level measure of the NTR Gap

$$NTRGap_f = \frac{NTRGap_p * w_p}{\sum_{p \in f} w_p}$$

Where $NTRGap_p$ is the NTR Gap for the industry of each plant p owned by firm f and w_p is the plant's output from the 1997 CMF.

In addition, I obtain costs and quantities of the firm's intermediate inputs and use this data to construct a price index of materials (see details in Internet Appendix A) for each industry from which the firm sources inputs. Firms presumably offshore to take advantage of lower labor costs. Therefore, firms that offshore intermediate good production should exhibit lower material costs. I also use capital and employment data from the CMF to calculate two other relevant statistics. I calculate value add per employee as a measure of labor productivity (Bernard et al. (2003)). Offshoring should increase domestic productivity either through a reorganization towards more skill intensive tasks or through efficiency gains at existing operations. Finally, since automation requires significant capital investment, I collect data on machinery capital expenditures from each CMF extract. I use this data to test for alternative margins of trade adjustment. I deflate all nominal quantities using industry price indices from Becker et al. (2013).

A.2. Financial Data

I obtain firm financial information from Compustat. I match the Census's firm ID to Compustat identifiers using an internally-provided bridge. I measure all financial variables as of 1999. This practice of using financial ratios measured in advance of the shock (PNTR was ratified in October of 2000 and went into effect in 2001) is common in the literature - see Opler and Titman (1994), Fresard (2010), and Babina (2017). I detail variable construction in Internet Appendix A.

I use three proxies for financial capacity: net book leverage, net market leverage, and cash-to-assets ratio. I use these proxies for several reasons. First, the literature has documented a robust negative (positive) relationship between firm leverage (cash holdings) and investment. Second, debt and cash are commonly used as proxies for financial capacity in the international economics literature (e.g. Manova and Yu (2016)). Finally, the financial constraint indices proposed by Kaplan

[17] I exclude from my sample any administrative records, as values for those observations are often times imputed.

and Zingales (1997) (as constructed in Lamont et al. (2001)), Whited and Wu (2006), and Hadlock and Pierce (2010) all rely on firm size in their construction. Since firm size is the primary proxy for productivity in Melitz-style trade models, using these indices risks conflating productivity with financial capacity.

A.3. Additional Data

I obtain data on U.S. imports by destination country from the USA Trade Online website managed by the Census Bureau. Imports at the product level are defined using the 6- and 8-digit HS codes (HS6 and HS8 respectively). The HS codes undergo frequent revisions. Therefore, I create a consistent classification of products using the concordance from Pierce and Schott (2009).

I obtain country-level income data from the World Bank. I define low income countries as all countries with per-capita gross national income (GNI) $\leq$ \$2,995 in 1999.[18] This classification includes the low ($GNI \leq \$755$) and low-middle ($\$756 \leq GNI \leq \$2,995$) income groups.

I obtain industry-level data on capital and skill intensity, value add per employee, and pre-PNTR employment and shipment growth from the NBER-CES dataset. The data is derived from restricted Census microdata such as the CMF and and Annual Survey of Manufacturers (ASM) and is aggregated to the industry level for public dissemination by Becker et al. (2013).

B. Sample Construction

The baseline analysis period is 1992-2007. This time frame concords with other studies that measure the impact of Chinese trade on U.S. employment (e.g. Autor et al. (2013)). Over the course of the analysis window, the Census switches from the standard industrial classification (SIC) system to the North American Industrial Classification System (NAICS). This change poses several complications. The first is that the mapping between SIC and NAICS industries is often times many-to-many. The second, is that several industries classified as manufacturing under SIC (such as logging) are dropped from the manufacturing sector under NAICS. To overcome these complications, I use the algorithm developed in Pierce and Schott (2016) which creates a consistent set of manufacturing industries bridging across the SIC-NAICS switch.

[18] Historical classification of low income countries can be found at `http://databank.worldbank.org/data/download/site-content/OGHIST.xls`.

The baseline firm level sample includes all domestic manufacturers with available financial data. I further restrict the sample to observations with leverage and cash-to-asset ratios on the unit interval. The plant level data includes all manufacturing establishments whose owners comprise the firm level sample.

C. Empirical Design

Since PNTR represents a policy change with heterogeneous exposure, it lends itself to a difference-in-difference analysis. To gauge the relationship between financial status and firm response to PNTR, I estimate a triple-difference regression where the coefficient of interest is the triple interaction term between the post-PNTR time period, the NTR-gap, and measures of the firm's financial capacity. The baseline empirical specification is as follows

$$\begin{aligned}\ln(Emp)_{f,t} =& \theta_1 PostPNTR_t \times NTRGap_f \times FC_f + \theta_2 PostPNTR_t \times NTRGap_f \\ &+ \theta_3 PostPNTR_t \times FC_f + \beta_1' X_f \times PostPNTR_t \times NTRGap_f \\ &+ \beta_2' X_f \times PostPNTR_t + \delta_f + \delta_{i,t} + \varepsilon_{f,t} \end{aligned} \tag{1}$$

Where f indexes firm, i indexes industry, and t indexes year. If the regression is properly specified, θ_1 identifies the effect of firm financing on employment responses to PNTR accounting for differences in employment growth over that time between high and low NTR Gap firms (θ_2) and financially weaker versus strong firms (θ_3). A positive (negative) sign for θ_1 when financial capacity (FC_f) is measured using leverage (cash) suggests greater post-PNTR employment losses in better capitalized firms.

In addition to my main variables of interest, I include a vector of controls X_f, comprised of the firm's pre-PNTR size, age, and employment growth. As mentioned above, Melitz-style models yield a monotonic relationship between size and productivity. Age helps control for the life-cycle of the firm. Finally, I include controls for pre-PNTR employment growth since employment growth tends to generally lag at more leveraged firms (Opler and Titman (1994)). In my robustness analysis I experiment with different specifications for X_f, including a non-linear specification and one that allows for interactions between each of the firm's pre-PNTR characteristics. I also include a firm fixed effect, δ_f, to control for any time invariant firm characteristics, and an industry-year fixed effect, $\delta_{i,t}$, to control for industry-level shocks. In all of the difference-in-difference analyses that follow, I cluster standard errors at the level of variation for the NTR Gap.

To parse out the temporal dynamics of employment responses to PNTR, I follow Monarch et al. (2017) and divide the sample into four time periods: 1) the long-run pre-period ($LR-pre$ 1992-1996); 2) the short-run pre-period ($SR-pre$ 1997-2000); 3) the short-run post-period ($SR-post$ 2001-2004); and 4) the long-run post-period ($LR-post$ 2005-2007). I then estimate the following regression

$$\begin{aligned}\ln(Emp)_{f,t}=\sum_{k\in K}\Big[&\theta_{1,k}1\{t\in k\}\times NTRGap_f\times FC_f+\theta_{2,k}1\{t\in k\}\times NTRGap_f\\&+\theta_{3,k}1\{t\in k\}\times FC_f+\beta_{1,k}'X_f\times 1\{t\in k\}\times NTRGap_f\\&+\beta_{2,k}'X_f\times 1\{t\in k\}\Big]+\delta_f+\delta_{i,t}+\varepsilon_{f,t}\end{aligned} \tag{2}$$

Where $K\equiv\{LR-pre,SR-post,LR-post\}$ leaving $SR-pre$ as the reference category. In this specification, $\theta_{1,LR-pre}$ tests for pre-period parallelism and $\theta_{1,SR-post}$ and $\theta_{1,LR-post}$ measure the relative sort-term and longer-term post-PNTR responses respectively. The remaining controls and fixed effects are the same as in (1).

IV. Empirical Results

A. PNTR & Manufacturing Employment - Baseline Results

I begin with a simple graphical representation of the baseline tests. Panel (a) of Figure 1 reports the average percentage change in domestic manufacturing employment for firm above and below the sample median of $NTRGap_f$. As Pierce and Schott (2016) show, employment drops much more precipitously for firms operating in industries more exposed to the PNTR policy. Comparing the growth rates across the two subsamples yields a difference in difference estimate of the Policy's effect of about 10.2%. In panel (b) ((c)) of Figure 1, I further break the high and low $NTRGap_f$ groups into firms with book leverage (cash) ratios above and below their industry's median ratio. As the figures show, in the high $NTRGap_f$ group, firms who were better capitalized at the time of PNTR's passage, i.e. those with lower leverage or high cash balances, exhibit 6.5%-8.3% *larger* employment declines. This result contrasts with the low $NTRGap_f$ sample, where better capitalized firms exhibit *smaller* job losses. Comparing the difference between poorly and well capitalized firms in the high and low $NTRGap_f$ groups yields a triple difference estimate of the interaction between firm financing and PNTR on the order of about 11%.[19]

Next, I present the results from estimating (1) in Table 1. In column (1) I estimate the difference in difference effect without conditioning on firm finances and find that higher NTR Gaps lead to lower post-PNTR employment. Importantly, I find a statistically and economically meaningful relationship between a firm's pre-PNTR financial standing and its response to the policy. The positive and significant θ_1 coefficient in the leverage regressions (columns (2) and (3) of Table 1) suggests that firms with lower leverage ratios relative to their industry peers also exhibit greater PNTR-induced employment losses. Specifically, a 10% decrease in leverage is associated with employment losses that are 22%-29% more sensitive to changes in the NTR Gap.[20] The negative and significant coefficient for θ_1 in column (4) suggests that likewise, a 5% increase in a firm's cash holdings is associated with an average amplification of 57% of the unconditional effect of the NTR gap on employment losses. These findings indicate that the results in Figure 1 are robust to controlling for firm size and age, employment pre-trends, unobserved inter-industry heterogeneity, and firm fixed effects.

In Figure 2, I explore the dynamics of this relationship. The coefficient $\theta_{1,LR-Pre}$ is insignificant and positive (negative) in the leverage (cash) specification, allowing me to further rule out any contaminating pre-trends. The Figure further shows a significant reduction in employment in the $SR - Post$ period that remains persistent, albeit statistically insignificantly so, over the longer horizon as well.

B. PNTR & Manufacturing Employment - Additional Tests

I conduct several robustness tests, which I report in Table 2. First, I confirm that results are robust to sales (column (1)) and assets (column (2)) as the measure of firm size. Second, I restrict all firms with primary employment in the same industry to have the same NTR gap (column (3)). Finally, I measure PNTR exposure via just the non-NTR rates rather than the NTR Gap (column

[19] In compliance with the Census confidential disclosure policy, I do not report the results for net market leverage, which are qualitatively similar.

[20] These estimates are generated by multiplying the coefficient on net book (market) leverage by 10% and then comparing it to the unconditional effect of the PNTR policy. For example, for net book leverage, the estimate is $\frac{-2.26*0.1}{-0.78} = 0.29$.

(4)). This last test forces variation in PNTR exposure to come from the more plausibly exogenous Smoot-Hawley tarrif rates. I present the robustness tests in Table 2 and find no material qualitative or quantitative change in the results. The θ_1 coefficients are consistently significant and of a similar magnitude to the baseline results in all of the robustness tests.

In addition to firm level regressions, I also take advantage of the granularity of the Census microdata and estimate the employment regression at the plant level. Specifically, I run the following regression

$$\ln(Emp)_{f,p,t}=\theta_1 PostPNTR_t \times NTRGap_p \times FC_f + \theta_2 PostPNTR_t \times FC_f + \delta_p + \delta_{c,i,t} + \varepsilon_{f,p,t} \tag{3}$$

Here f indexes the firm, p indexes the plant, t the year, and i indicates the plant's industry. In this specification, rather than include continuous measures of the controls in X_f, I include indicators for the decile of the firm's size and pre-PNTR growth rate and whether or not it is above the median age in the sample. The fixed effect $\delta_{c,i,t}$ represents the interaction of each of the firm characteristic bins with each other as well as the plant's industry and the current year. Owing to $\delta_{c,i,t}$, this specification forces the comparison to occur between two plants operating in the same industry (i.e. same NTR Gap) and owned by firms of similar size, age, and pre-PNTR employment growth rates. Therefore, the regression generates estimates on based on variation in employment outcomes between narrowly defined groups of plants and provides evidence of adjustments across intensive margins of employment.

To capture plant-level adjustments along the extensive margin as well, I conduct two additional tests. First, following Davis et al. (1996), I calculate the symmetric growth rate from the pre to post-PNTR periods

$$\Delta\{Emp_p\} \equiv \frac{Emp_{p,post} - Emp_{p,pre}}{.5(Emp_{p,post} + Emp_{p,pre})}$$

As Davis et al. (1996) show, this growth rate approximates log changes in employment with the added benefit of accommodating plant closures (i.e. $Emp_{post} = 0$). I also define $\mathbb{1}\{Death_p\}$ as an indicator of plant closure in the post-PNTR analysis window. I then estimate the following regressions

$$\mathbb{1}\{Death_p\}=\theta_1 NTRGap_p \times FC_f + \theta_2 FC_f + \delta_{f,i} + \varepsilon_p \tag{4}$$

$$\Delta\{Emp_p\}=\theta_1 NTRGap_p \times FC_f + \theta_2 FC_f + \delta_{f,i} + \varepsilon_p \tag{5}$$

I report the results from estimating (3)-(4) using net book leverage, net market leverage, and cash in panels A-C of Table **??** respectively. The plant-level tests conform with the takeaways from the firm-level regressions. For each 10% increase in the NTR gap, firms with lower leverage and higher cash balances reduce plant employment by between X%-X% (columns (2) and (4)). Better capitalized firms are also X%-X% more likely to close plants in higher NTR gap industries (column (6))).[21]

C. Identification Concerns

The identifying assumption behind an economic (i.e. causal) relationship between firm finances and trade responses is that firm financial ratios, measured two years prior to PNTR, are uncorrelated with any other unobserved factor that would lead more exposed firms to shed employment in the policy's wake. As mentioned in the introduction, this type of "clean" variation should exist in my sample of mature manufacturing firms, but absent a way to isolate it, ruling alternative explanations in general is difficult. In this section I address several specific identification concerns and present cross-sectional tests that help rule out numbers alternative explanations.

C.1. Reverse Causality

The post-PNTR period coincides with the time-frame immediately following China's long-awaited WTO ascension. Furthermore, corporate lobbying in favor of PNTR (see Section IV.E) rouses suspicions that firms supporting the bill were poised to invest in offshoring regardless of its passage and had structured their finances accordingly. This reverse causality would invalidate the analysis only if the firms poised to offshore regardless of PNTR were systematically operating in higher NTR Gap industries. Since the NTR Gap is largely driven by variations in non-NTR

[21] Due to loss of power from sample restrictions needed to comply with Census disclosure policy, the coefficient for plant closures is not significant in the leverage specifications. Regressions on the unrestricted sample yield significant estimates.

rates, which were set in the 1930s, it is plausibly orthogonal to current determinants of offshoring. Additionally, as Pierce and Schott (2016) show, high NTR Gap industries in Europe, which was unaffected by PNTR's passage, do not experience larger employment drops in the post-NTR period. This evidence alleviates concerns that some unobserved industry characteristics correlated with the NTR Gap also drive offshoring decisions. Nonetheless, I examine the correlation between the NTR Gap and relevant industry characteristics to empirically assess this threat.

Internet Appendix Table B2 reports the correlation between the NTR Gap and various other industry characteristics. The NTR Gap is negatively correlated with capital intensity at the industry (column (1)) and firm (column (14)) levels. It is also negatively correlated with skill intensity (columns (2) and (3)). It is therefore possible that the anticipated replacement of labor intensive and lower skilled domestic manufacturing, which happens to also exhibit higher NTR Gaps, with Chinese imports, leads firms to maintain more financial slack. However, as Autor et al. (2013) document, the initial stages of Chinese import penetration growth and domestic manufacturing job losses were already occurring in the 1990s. If the NTR Gap was simply a proxy for the type of manufacturing most easily replaced by Chinese imports, then one would expect high NTR Gap industries to exhibit larger import growth rates and bigger employment declines even prior to the policy's passage. The lack of correlation between the NTR Gap and pre-PNTR changes in industry employment (column (4)), output (column (5)), or Chinese import growth (column (6)) therefore assuage concerns of reverse causality.

I conduct two further tests to rule out reverse causality due to China's anticipated WTO entry more definitively. First, I instrument leverage ratios from 1999 with their 1994 levels and find qualitatively similar results (results unreported). Second, I reestimate (1) after excluding firms whose 1999 financial ratios may reflect adjustments for future offshoring plans. On November 15, 1999 China and the U.S. entered into a bilateral trade agreement seen as paving the way to China's WTO entry (see Internet Appendix C). This agreement was unexpected as it was the product of tense negotiations which were officially called off prior to the agreement being signed (Devereaux and Lawrence (2004)). Furthermore, as previous negotiations regarding China's WTO ascendancy had stalled, it is unlikely that firm could predict the success of the November talks. It is therefore likely that firm finances measured prior, or shortly after, this agreement are not affected by any future considerations pertaining to China's entry into the WTO. However, firms with fiscal years ending several months after the agreement do have time to begin adjusting financial policy. In unreported tests, I exclude from my sample all firms whose fiscal year ended after December 31, 1999 and find similar results.

As Internet Appendix Table B2 further shows, the NTR Gap is also negatively correlated with firm size (columns (7) - (9)) age (column (10)) and operating margins (columns (11) and (12)).[22] This relationship raises the question of whether firms in higher NTR Gap industries are fundamentally less able to withstand increased competition from China. I address whether my results are driven by economic distress, along with additional concerns, below.

C.2. Alternative Mechanisms

Even if reverse causality concerns are addressed, a second, potentially more pernicious issue remains. It is possible that other factors simultaneously govern both firm finances and employment responses to PNTR. Ruling out this concern in general requires "clean" variation in firm finances. Without such variation, the best one can do is identify and address the most immediate manifestations of this omitted variable issue. To that end, I focus on the following alternative explanations.

The first alternative is that precariously capitalized firms are also economically distressed. These firms would likely forgo offshoring investment even absent financial frictions. The second alternative is that the results obtain due to differences in capital intensities between firms. Capital intense firms tend to hold more leverage (Titman and Wessels (1988)) and less cash (Fresard (2010)). Since the primary benefit of offshoring to China is access to cheap and ample labor, more capital intense firms may have less incentive to do so. The third alternative is that the results obtain due to differences in financial resources between multinational and strictly domestic companies. Multinational companies (MNCs) are presumably better positioned to take advantage of an improved offshoring environment, having already established international operations. A spurious relation will arise if MNCs also have higher cash holdings or lower leverage.

To rule out these specific concerns, I reestimate (1), augmenting X_f with controls for proxies for firm fundamentals, capital intensity, and multinational operations. I use return on assets (ROA), net profit margin (NPM), market to book (M2B) ratio, total factor productivity (TFP), and value add

[22]Given the size discrepancy, it is unsurprising that the NTR Gap is also negatively correlated with leverage, and positively correlated with higher cash balances (columns (16) through (18)).

per employee as proxies for fundamentals (i.e. economic distress). I measure ROA, NPM, and M2B as of the firm's 1999 financial year end. I measure the firm's TFP and value add per employee from the 1997 CMF. As proxies for capital intensity, I use the firm's ratio of tangible assets, also measured as of 1999, as well as its capital-to-labor ratio from the 1997 CMF. Finally, I construct an indicator of whether the firm reports multiple geographic segments or income from foreign operations as proxies for multinational operations. I report the results incorporating additional controls in Table 4. As the analysis shows, the baseline results are robust to the alternative explanations, as measured by my proxies. Of course, since I cannot capture all potential unobservables and alternative explanations, this analysis is admittedly imperfect. However, the consistent resilience of the θ_1 coefficients in the face of additional controls instills confidence in the validity of the estimates.

C.3. Cross-Sectional Evidence

Another alternative to mitigating the omitted variable concern is to use plausibly exogenous inter-industry variation to relax the identifying assumption. One relevant cross-sectional split is the firm's level of external financial dependence (EFD). Rajan and Zingales (1998) argue variation in EFD occurs due to cross-industry differences in "initial project scale, the gestation period, the cash harvest period, and the requirement for continuing investment..." (p. 563). As such, it could interact with firm finances to exacerbate the underinvestment problem in two ways. Firstly, if EFD is driven by the project's scale (i.e larger amounts of financing needs), then collateral constraints (or other borrowing constraints such as covenants) may prevent more poorly capitalized and highly dependent firms from obtaining sufficient financing. Secondly, capital intensive projects with long project gestation and cash harvest periods likely also require significant relationship-specific investment by foreign partners. This specificity may exacerbate any stakeholder disincentives created by poor firm capitalization.

Ideally, EFD captures only differences in the dependence on external sources of capital. In practice, such differences are often a product of the complexity of the manufacturing process. Consequently, high-tech industries such as pharmaceutical and computing manufacturing are also high-EFD while low-tech industries such as apparel manufacturing tend to be low-EFD. To the extent that high-tech industries are less likely candidates for offshoring, these differences should bias the results against my proposed channel. Furthermore, as long as the relationship between financial position and any other relevant unobservable (e.g. productivity) does not depend on a firm's degree of EFD, then a more acute employment response in the high-dependence subsample would be strongly suggestive of the financing channel. That is, by comparing the θ_1 coefficient across the two subsamples I no longer require well and poorly capitalized firms to exhibit no other unobservable differences. Rather, the assumption becomes that any difference in unobservables that is correlated with strength of balance sheet does not vary across the high and low EFD subsamples.

The classical Rajan and Zingales (1998) measure of EFD is the ratio of investment that cannot be funded by operating cash-flow.

$$EFD = \frac{INVEST - OCF}{INVEST}$$

Following the standard methodology, I calculate the firm-level EFD by summing all investment (Compustat CAPEX) and operating cash-flows from 1992-1999. I then define the industry-level EFD as the median firm-level EFD in the industry. I use only single-segment firms in this calculation. I first take the median EFD for each 3-digit SIC industry and then merge that to the 3-digit SIC of each firm's industry segment from Compustat. I then calculate the firm level EFD in a similar manner to the firm level NTR Gap, by weighting each industry's EFD by its sales.

$$EFD_f = \frac{EFD_i * w_i}{\sum_{i \in f} w_i}$$

I divide the baseline sample into high versus low EFD firms and present the results in Table 5. Firm financing has no material effect on post-PNTR employment outcomes for the low-EFD group. However firms with 10% less leverage in the high-EFD group reduce employment between 4% for each 10% increase in NTR gap (coefficient in column (1) $\times 0.01^2$), a statistically and economically different response. Similar results hold when examining cash balances. Firms in the high-EFD sample with 5% more cash on hand reduce employment by about 3% more for each 10% increase in the NTR gap ($6.2 \times 0.05 \times 0.1$). This increased sensitivity is economically significant considering that unconditionally a 10% increase in the gap leads to a 7% (-0.705×0.1) drop in employment for high-EFD firms. Furthermore, the effect is statistically different from the relationship between cash balances and post-PNTR job losses for low-EFD firms. The analysis therefore confirms my results are robust to relaxing the identifying assumption in the manner described above. Caveats notwithstanding, the tests examining domestic manufacturing employment present evidence uniformly consistent with financial frictions preventing certain firms from offshoring operations in the wake of PNTR.

D. Additional Evidence

In this section, I look beyond employment outcomes and examine additional firm outcomes that are consistent with either the competition or offshoring mechanism. My findings present a mosaic of evidence in favor of better capitalized firms offshoring operations as a result of PNTR.

D.1. Additional Firm Behavior

I begin by testing hypothesis 1B. To do so, I use Compustat data on sales and company-wide employment. If better capitalized firms shed more workers due to larger (relative) market share losses, they should also exhibit (relatively) larger declines in sales. To test this prediction, I estimate the following regression

$$\begin{aligned}\ln(Sale)_{f,i,t} = {} & \theta_1 PostPNTR_t \times NTRGap_f \times FC_f + \theta_2 PostPNTR_t \times FC_f \\ & + \beta_1' X_f \times PostPNTR_t \times NTRGap_f + \beta_2' X_f \times PostPNTR_t \\ & + \delta_f + \delta_{i,t} + \varepsilon_{f,t} \end{aligned} \tag{6}$$

The unit of observation is the natural log of sales from each industry segment i operated by firm f in year t. The coefficient on the interaction between the NTR Gap and the post PNTR period is subsumed by the industry-year dummy $\delta_{i,t}$. The remaining controls are the same as in (1) except they are all sourced from Compustat. The results from this exercise, summarized in Panel A of Table 6, are inconsistent with the market share loss hypothesis. Unconditionally, the NTR Gap is not associated with sales declines (column (1)). Conditioning on firm finances further dispels the notion that job losses materialize due to competition. The coefficients in columns (2) and (4) suggest that if anything, high NTR Gap firms with less book leverage (more cash) at the time of PNTR exhibit relatively larger sales growth post-PNTR. The coefficient in column (3) is positive, but this effect is small and insignificant. In Internet Appendix Table B6, I repeat the analysis using aggregate firm rather than segment sales. While PNTR is unconditionally negatively related to firm sales in this specification, I still find no evidence of more prominent market share losses for better capitalized firms.

Similarly, if the more pronounced manufacturing job losses are a product of downscaling, rather than adjusting away from domestic labor, one should observe similar declines in the firm's non-domestic-manufacturing employment. To test this prediction I re-estimate (6) using Compustat data on industry segment employment. I report the results in Panel B of Table 6. As with sales, the unconditional relationship between PNTR, firm finances, and employment becomes statistically insignificant, and economically much less relevant, when aggregate, not just manufacturing, employment is the outcome variable (column (1)). This fact holds true for each of the triple interaction terms in columns (2)-(4). These results, in conjunction with the manufacturing employment results from Tables 1-5, suggest that firms are substituting away from domestic manufacturing rather than instituting across the board cuts.

I next turn to tests of hypothesis 2B. I first examine the connection between PNTR, offshoring, and input costs. Internet Appendix Figure B1 shows that unconditionally, industries with higher upstream exposure to PNTR, i.e. those with higher NTR Gap inputs, exhibit a sharp drop in materials cost in the post-PNTR period. To test if these cost savings concentrate in better capitalized firms, I use input-cost data from the quinquennial CMF survey and estimate the following regression

$$\begin{aligned}\ln(Matcost)_{f,i,t} = &\theta_1 PostPNTR_t \times NTRGap_i \times FC_f + \theta_2 PostPNTR_t \times NTRGap_i \\ &+\theta_3 PostPNTR_t \times FC_f + \beta_1' X_f \times PostPNTR_t \times NTRGap_i \\ &+\beta_2' X_f \times PostPNTR_t + \delta_{f,i} + \delta_{f,p} + \varepsilon_{f,t}\end{aligned} \tag{7}$$

The unit of observation $\ln(Matcost)_{f,i,t}$ is the price index (i.e. total costs divided by total quantities) for each firm's f input industry i in 1992, 1997, 2002, and 2007. Along with the control set X_f, I also include segment ($\delta_{f,i}$) and industry-period ($\delta_{f,p}$ where $p \in \{Pre - PNTR, Post - PNTR\}$) fixed effects. I report the results in panel A of Table 7 The positive (negative) sign for θ_1 in the leverage (cash) regressions indicates that for each 10% increase in the NTR Gap of an input industry, better capitalized firms are able to reduce material costs by X%-X%.

To test whether offshoring leads to measurable gains in domestic productivity, I reestimate (7) with $LaborProd$ (value add per employee) as the dependent variable. Internet Appendix Figure B2 shows that the unconditionally, higher NTR Gap industries experience an insignificant increase in labor productivity. I report the results conditioning on firm finances in panel B of Table 7. The unit of observation is the value add per employee for each domestic manufacturing industry the firm operates. The negative (postive) sign for θ_1 in the leverage (cash) regressions indicate that better capitalized firms realize significant productivity gains along the order of X%-X% as a result of PNTR.

Finally, firms may respond to Chinese competition by investing in labor-saving technology (automation). Since the CMF data provide explicit breakouts of domestic capital expenditures for machinery, I re-estimate (7) using machinery outlays ($MachCapex$) as the outcome variable and report the result in panel C of Table 7. I find no meaningful relationship between the NTR Gap, firm finances, and investment in machinery.

E. *Effects of PNTR on U.S. Economy*

I close the analysis by relating my findings to the broader question of the effects of trade liberalization on the U.S. economy. Specifically, I ask whether firms view offshoring as a desirable benefit of, rather than a necessary response to, PNTR. Anecdotal evidence squares much more neatly with the former interpretation. The strong support for and warm corporate reception of PNTR's passage is incongruent with the notion that firms viewed the policy as facilitating more intense competition. There also exists anecdotal evidence that firms welcomed PNTR precisely because it facilitated investment in China. Joseph Quinlan, an economist at Morgan Stanley, describes PNTR as a "[D]eal about investment, not exports." in the Wall Street Journal (Cooper and Johnson (2000)).[23] That same article continues to state the following regarding the business community's response to PNTR's passage

> [B]usiness, which spent millions of dollars on advertising and lobbied vigorously for this outcome... played down its likely impact on investment, leery of sounding supportive of labor union arguments that the deal would prompt companies to move U.S. production to China.

[23] I provide further anecdotal evidence for corporate support of PNTR in Internet Appendix B.

To gauge whether markets shared similarly favorable sentiments regarding trade liberalization with China, I conduct an event study analysis of both the PNTR vote as well as the preceding bilateral trade deal signed by the U.S. and China in November of 1999. As I show in Internet Appendix C, these two events served as material stepping stones towards the liberalization of Sino-U.S. trade, and while they are inherently related, they contain sufficiently different information as to elicit a significant market response. The bilateral deal was first announced to the market on November 15, 1999 and the PNTR vote took place after market close on May 24, 2000. Therefore I use November 15, 1999 and May 25, 2000 as the respective event dates for the deal and the vote.

Table 8 provides the results for abnormal (net of market) returns of manufacturing firms for each event separately as well as over both events. The baseline results include only the event days (t), but I also consider an event window of $[t-1, t+10]$ for robustness.[24] Both events are associated with positive abnormal returns on average - the average combined abnormal return for the two events is 1.93%. Importantly, firms operating in higher NTR Gap markets exhibit larger abnormal returns for both the initial agreement (column (1)), the PNTR vote (column (2)), and their sum (column (3)). A 10% increase in the NTR Gap corresponds to abnormal returns that are 0.45% higher (column (3)). This effect is statistically significant and economically meaningful as compared to the mean abnormal return over the two events. The effects for the $[t-1, t+10]$ window (columns (4)-(6)) are even more striking. The event study analysis is consistent with markets anticipating that firms will take advantage of new offshoring investment opportunities and cheaper labor abroad.

Finally, I present evidence consistent with foreign investment serving as a driver of the post-PNTR growth in Chinese imports. Figure 3 graphs the annual flows of foreign direct investment (FDI) into China from 1995-2006. Aggregate FDI in China grew from about \$80 billion in 1995 to around \$200 billion in 2006. Furthermore, the composition of FDI shifted from investment in joint ventures to the establishment of foreign-owned entities (i.e. foreign subsidiaries) starting in 2000, the year of PNTR's passage. Figure 4 graphs the percentage of total U.S. imports sourced from China (blue series) and the percentage Chinese imports sourced from a related party from 1992-2007. Importantly, the Figure shows that as China's share of the U.S. import market grows, more of those imports come from a related party - that is a foreign subsidiary. Taken together,

[24]Results are qualitatively similar using a $[t-1, t+5]$ window as well. They are also robust to estimation using FGLS with an exponential heteroskedasticity specification.

Figures 3 and 4 suggest that the investment surge occurring around the time of PNTR's passage (and China's WTO entry) may help explain a material part of China's import growth.

Systematic evidence of the effects of FDI on PNTR-induced import growth further confirms this hypothesis. Using Chinese microdata, Pierce and Schott (2016) show that the strongest response in PNTR-related export growth, both economically and statistically, comes from foreign owned firms operating in China. This fact underscores the necessity of foreign investment for Chinese import growth. To complement their analysis, I investigate whether regulatory restrictions on FDI dampen the relationship between the NTR Gap and Chinese imports. Evidence that with FDI restrictions, the NTR Gap is unrelated to import growth speaks to the essential role that foreign investment plays in spurring Chinese import growth.

I implement my tests using data on FDI regulations by the Chinese Government. The Chinese Ministry of Commerce periodically releases a Guidance Catalog for Foreign Direct Investment.[25] This catalog divides foreign investment categories into four groups: prohibited, restricted, permitted, and encouraged. The catalog explicitly lists only prohibited, restricted, and encouraged investments. All other projects are presumed to be permitted. Investment in restricted projects is subject to strict governmental examination, case-by-case approval, and may be limited to joint ventures under which the Chinese partner shall hold majority interests. Permitted projects do not require special approval and face no ownership limitations. Encouraged projects provide additional incentives such as tax breaks. I use the mapping from Blonigen and Ma (2010) to link the catalog categories to HS6 product codes. I focus on the restricted and permitted categories only because the prohibited category is limited to a small set of projects such as ivory carving and herbal medicines, and as I discuss below, projects in the encouraged category are less likely to be FDI targets. Since disaggregated data on FDI is not readily available, I use these regulatory restrictions on FDI as a proxy for reduced FDI flows.[26]

Using UTO data on U.S. imports from China from 1997-2004 (when the Chinese Ministry of Commerce released another revision of the catalog), I estimate the following regression separately

[25] See Davies and Balve-Hauff (2003) for more information.

[26] The BEA provides restricted-use microdata on Chinese subsidiaries of U.S. multinationals. However, that data cannot be used in conjunction with Census microdata on domestic activities.

for products in the restricted and permitted categories

$$\ln(Val)_{p,t} = \sum_{k=1998}^{2004} \theta_k 1\{k=t\} \times NTRGap_p + \delta_p + \delta_t + \varepsilon_{p,t} \tag{8}$$

Where $\ln(Val)$ is the natural logarithm of Chinese imports and δ_p and δ_t represent product and year fixed effects respectively. I graph the θ_k coefficients (with 1997 as the reference year) in Figure 5. Restricted products (panel (a)) with higher NTR Gap do not experience higher Chinese import growth rates in the post-PNTR period, while import growth is significantly related to the NTR Gap in permitted products (panel (b)).[27] To verify that these differences are statistically significant I estimate the following diff-in-diff and triple difference regressions

$$\ln(Val)_{p,t} = \theta_1 PostPNTR_t \times NTRGap_p + \delta_p + \delta_t + \varepsilon_{p,t} \tag{9}$$

$$\begin{aligned} ln(Val)_{p,t} = {} & \theta_1 PostPNTR_t \times NTRGap_p + \theta_2 1\{c=Per\} \times PostPNTR_t \times NTRGap_p \\ & + \delta_p + \delta_{c,t} + \varepsilon_{p,c,t} \end{aligned} \tag{10}$$

I estimate (9) for the the entire sample as well as for the restricted and permitted subsamples separately. I report the results in Table 9. A 10% increase in the NTR Gap is associated with 4.3% larger Chinese import growth in the full sample (column (1)). For permitted projects (column (2)), the growth rate is 4.8%, while it is negative and insignificant for restricted projects (column (3)). The triple difference results (column (4) in Panel A of Table 9) indicate that this difference is significant at the 10% level. These findings are consistent with foreign investment playing a significant role in the China's post-PNTR export growth.

An important caveat to the analysis above is that the investment guidelines are not exogenous. However, a plausible argument exists as to why OLS estimates understate the effects of FDI restrictions on import growth in restricted products. A casual inspection of the Catalog suggests that China encourages investment in higher technology manufacturing fields and restricts it in lower technology markets (see for example Internet Appendix Table B5). Lower technology manufacturing is arguably better suited to take advantage of China's large, cheap, and (relative to the U.S.) unskilled labor force. Under this argument, the low-technology restricted projects are actually a

[27] Consistent with all other tests, I use 2001-2004 as the post period in this analysis, the results are qualitatively similar if I use 2002-2004.

better natural fit for the legal and economic environment in China. This logic then suggests that the estimation of θ_2 in (10) understates the dampening effect of regulatory investment restrictions. A similar argument regarding the higher technology manufacturing found in the encouraged category suggests FDI flows may lag in those projects despite the investment incentives.[28]

To test for the existence of systematic differences in the natural fit of investment for permitted and restricted projects, I calculate the percentage of total U.S. imports that originate from lower-income countries (excluding China) in 2000. Schott (2008) shows that China's factor endowments, e.g. skilled labor and capital, most closely resemble those of other developing economies. The classical Heckscher-Ohlin (HO) model predicts that with global trade markets, each country concentrates production in products whose factor intensities match most closely with the country's factor endowments. HO models therefore suggest that if lower income countries, including China, are endowed with labor forces more suitable for low-technology manufacturing (relative to the U.S. and other developed economies), then they should account for a larger share of U.S. imports in those products. I present the results of this hypothesis in column (1) of Panel B of Table 9. 13.3% of U.S. imports for restricted products are sourced from other low-income countries in 2000. That same ratio is about 9.6% for permitted products.[29] This difference is both economically and statistically significant and suggests that restricted projects do indeed exhibit a natural advantage for Chinese production relative to permitted projects. The insignificant association between the NTR Gap and import growth in the restricted category therefore speaks to the material role that FDI plays in promoting post-PNTR imports.

As additional validation, I also check for differences in country "HHI" between permitted and restricted projects. I calculate country "HHI" using the same sum-of-squared-shares methodology as traditional HHI calculations, but using country share of total imports. These indices don't measure competition or concentration in the traditional sense, but are still useful for examining whether significant differences in product markets exists across the two investment categories. For example, if one category of products exhibits a significantly higher concentration of importing countries, it is possible that production in those markets concentrates in a certain region due to some natural advantage. Likewise, significant differences in dispersion across categories may be indicative of

[28]The difference in difference coefficient for (9) is positive yet insignificant in the encouraged sub-sample. The results do not materially change if I include encouraged projects in the full sample and triple difference regression.

[29]Encouraged products have an even lower share of lower-income sourcing than permitted projects.

more competition. Therefore, it is comforting, as column (2) in Panel B of Table 9 shows, that no significant difference in importing country "HHI" exist across the categories.

The favorable corporate and market reception of PNTR's passage, as well as the connection between FDI allowances and the growth in Chinese imports attributable to the policy, paint a picture in which investment opportunities, rather than import competition, play a primary role in determining outcomes for domestic firms and product markets. These results, in conjunction with the evidence regarding offshoring and employment outcomes, shed new light on how the U.S. economy adjusts to freer trade with China.

V. Discussion & Conclusion

In this final section, I include a discussion of how my results compare to recent research examining the competitive effects of Chinese trade and provide concluding remarks.

A. Relation to Previous Literature

Recent papers also examine how Chinese trade impacts domestic firms and employment. Both Autor et al. (2016a) and Hombert and Matray (2018) argue that increased competition from Chinese producers erodes firm profitability leading to decreased investments in innovation and employment losses. There are several reasons why my results point in a different direction. First and foremost, the primary identification strategy of trade effects used by these two papers relies on reform-induced productivity growth stemming from China's transition into a market economy, while my analysis focuses on reductions in trade barriers. Trade liberalization is plausibly more of a shock to investment opportunities than the increased competitiveness of the Chinese manufacturing sector. In the extreme, consider a developing country with a large, idle labor force but no real organizational (or entrepreneurial) capital to utilize it. A reduction in trade costs with said country should not matter absent any foreign investment to establish a manufacturing sector. On the other hand, even if trade costs remain the same, as the developing nation becomes more market-oriented, it is likely to spur new ventures that will compete with producers in developed countries.

It is also possible to interpret the primary findings in Autor et al. (2016a) and Hombert and Matray (2018) within the context of offshoring. Autor et al. (2016a) show that firms in industries with larger Chinese import penetration rates reduce their innovation investments. However, Bena and Simintzi (2017) show that cheap foreign labor is a substitute for process innovations. Likewise, the result in Hombert and Matray (2018) that firms with a higher stock of R&D experience smaller trade-induced employment declines can be attributed to said firms being less willing to offshore production, e.g. due to concerns of intellectual property theft.

B. Conclusion

In this paper, I empirically examine the relationship between firm finances and responses to PNTR - a landmark reduction of Chinese-U.S. trade barriers. I document that better capitalized firms exhibit larger manufacturing employment declines in response to liberalized trade with China and connect this result to offshoring. While caveats regarding a causal link remain, I conduct a litany of tests that are all consistent with financial capacity determining firms' ability to invest in offshoring.

My results have three central implications. First, by establishing a robust connection between firm finances and trade responses, they speak to previously unexplored, yet material, effects of financial distress. Second, they point to offshoring as the primary driver of PNTR-induced employment losses at large public firms. This fact not only informs optimal trade assistance policy, but also sheds light on how developed economies adjust to trade with developing countries. Third, they document a concrete mechanism by which financially stronger firms capitalize on the opportunities that arise in the wake of industrial shakeup. Further exploring the industrial reorganization, e.g. mergers and acquisitions, precipitated by Chinese trade is a promising are for future research.

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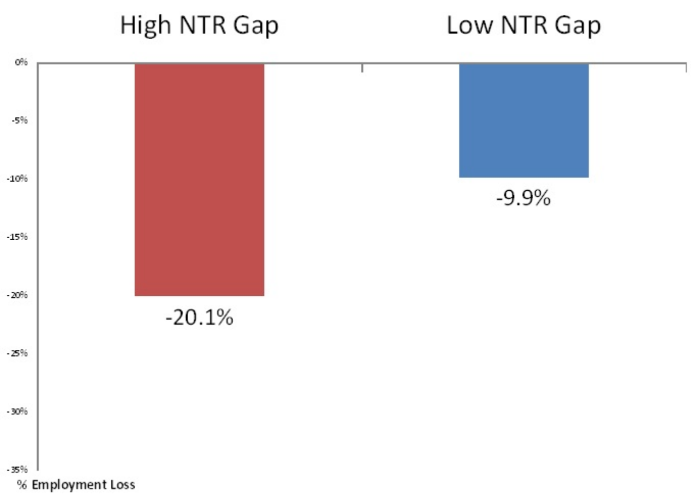
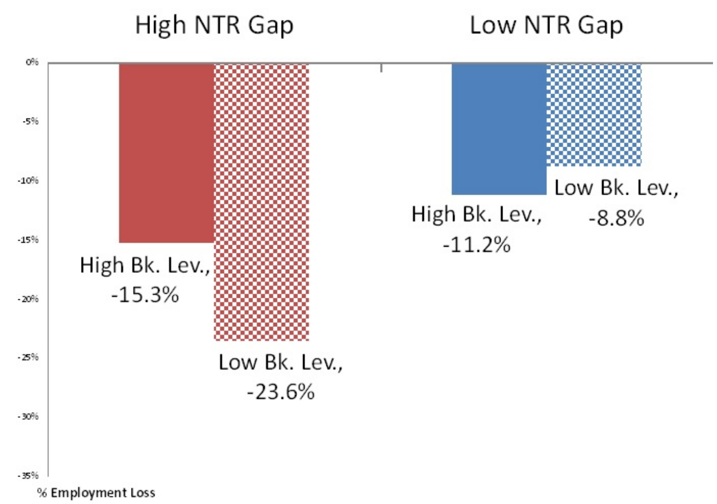
Figure 1: **PNTR, Firm Financing, and Employment - Graphical Depiction**

This figure presents the average reduction in firm employment for firms over 1992-2007. Bars below the x-axis indicate job losses. Panel (a) presents the average employment growth from for firms above and below the median NTR Gap - i.e. the difference-in-difference. Panels (b) and (c)further break up the sample to firms who are above and below their industry's median financial ratio - i.e. the triple difference. Note that the figure is displayed for exposition purposes only. The underlying numbers do not represent actual figures generated with confidential data and the vertical axis is intentionally left blank.

(a) Difference-in-Difference



(b) Triple Difference Net Book Leverage



(c) Triple Difference Cash Ratio

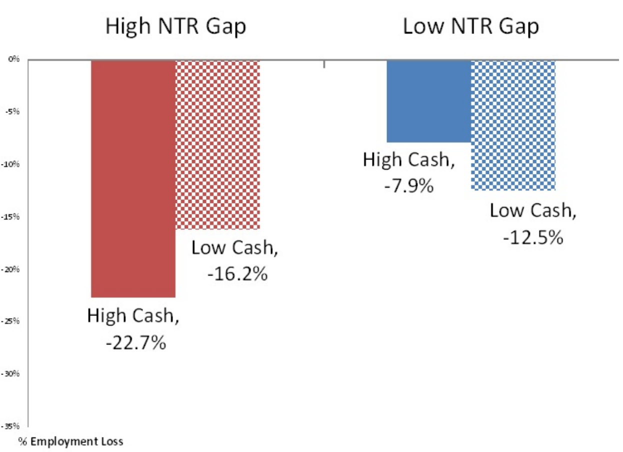


Figure 2: **PNTR, Firm Financing, and Employment**

This Figure graphs the $\theta_{1,k}$ coefficients from the following regression:

$$\ln(Emp)_{f,t} = \sum_{k \in K} \Big[\theta_{1,k} 1\{t \in k\} \times NTRGap_f \times FC_f + \theta_{2,k} 1\{t \in k\} \times NTRGap_f + \theta_{3,k} 1\{t \in k\} \times FC_f + \beta'_{1,k} X_f \times 1\{t \in k\} \times NTRGap_f + \beta'_{2,k} X_f \times 1\{t \in k\} \Big] + \delta_f + \delta_{i,t} + \varepsilon_{f,t}$$

I divide the sample into four sub-periods: 1) the long-run pre-period ($LR - pre$ 1992-1996); 2) the short-run pre-period ($SR - pre$ 1997-2000); 3) the short-run post-period ($SR - post$ 2001-2004); and 4) the long-run post-period ($LR - post$ 2005-2007) and denote the set $K = \{LR - Pre, SR - Post, LR - Post\}$ with $SR - Pre$ as the reference period. The remaining variables and standard error clustering are the same as in (1).

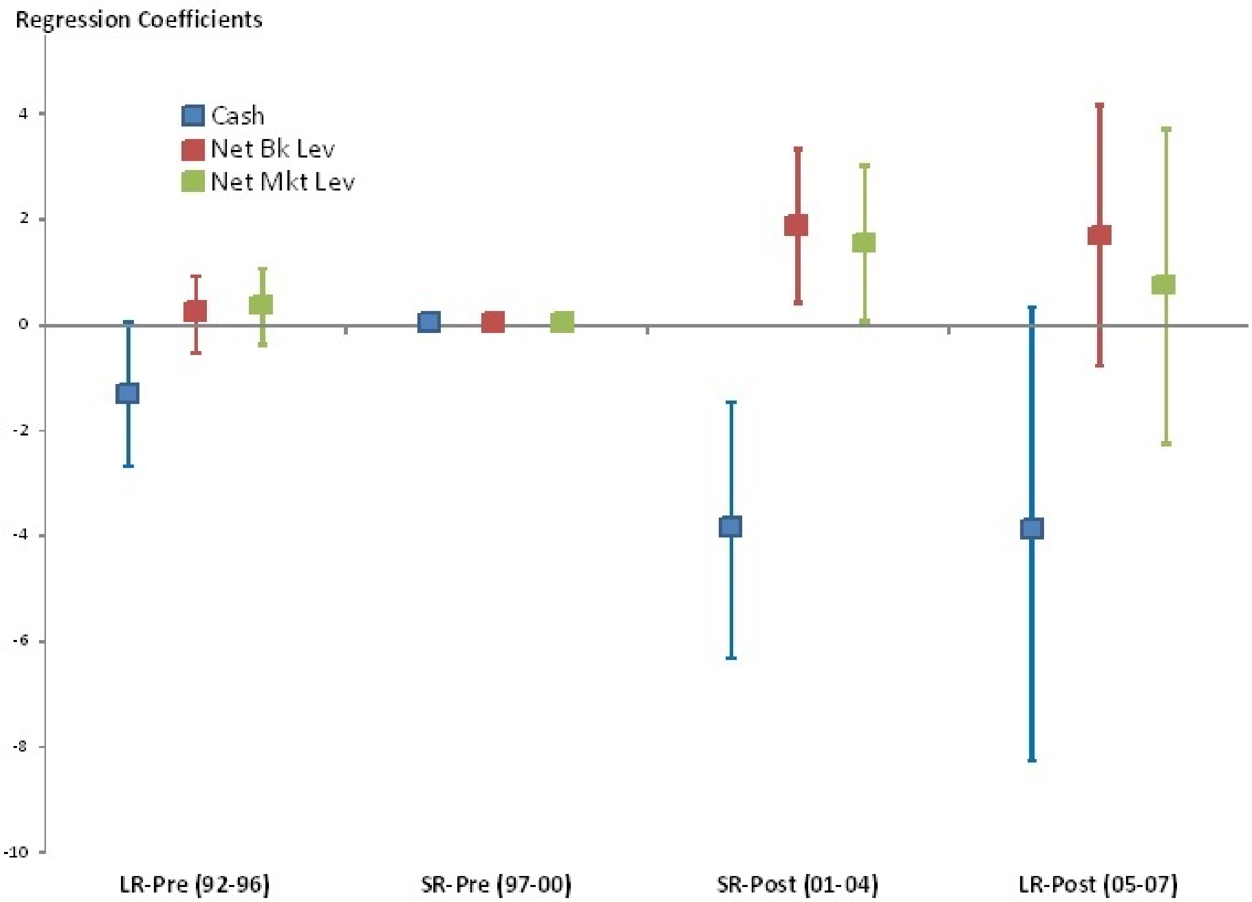


Figure 3: **China FDI**

This figure graphs annual foreign direct investment in China split out by joint ventures (JV) and foreign-owned entities (FOE) from 1995-2006. Source: National Bureau of Statistics of China.

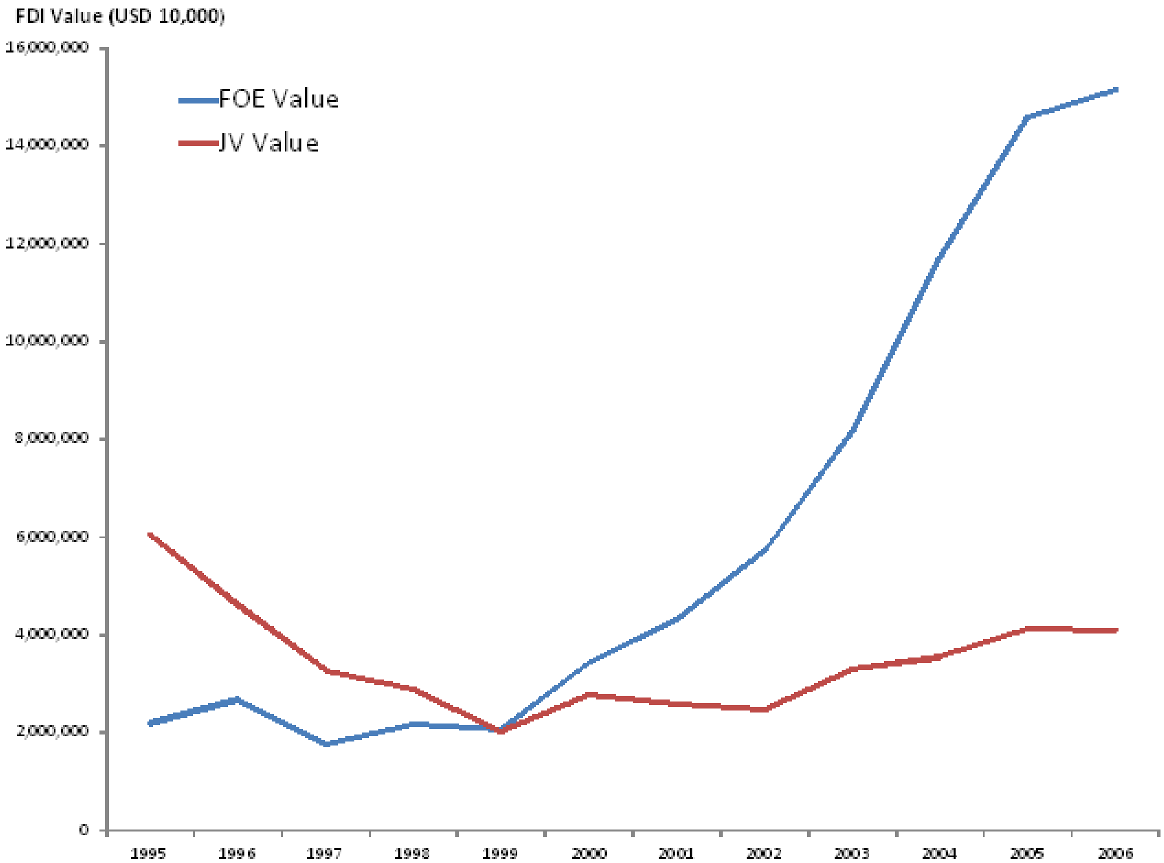


Figure 4: **China Related Party Imports**

This figure graphs the percent of all annual U.S. imports arriving from China and the percentage of Chinese imports from related parties from 1992-2007. Source: U.S. Census Foreign Trade Statistics.

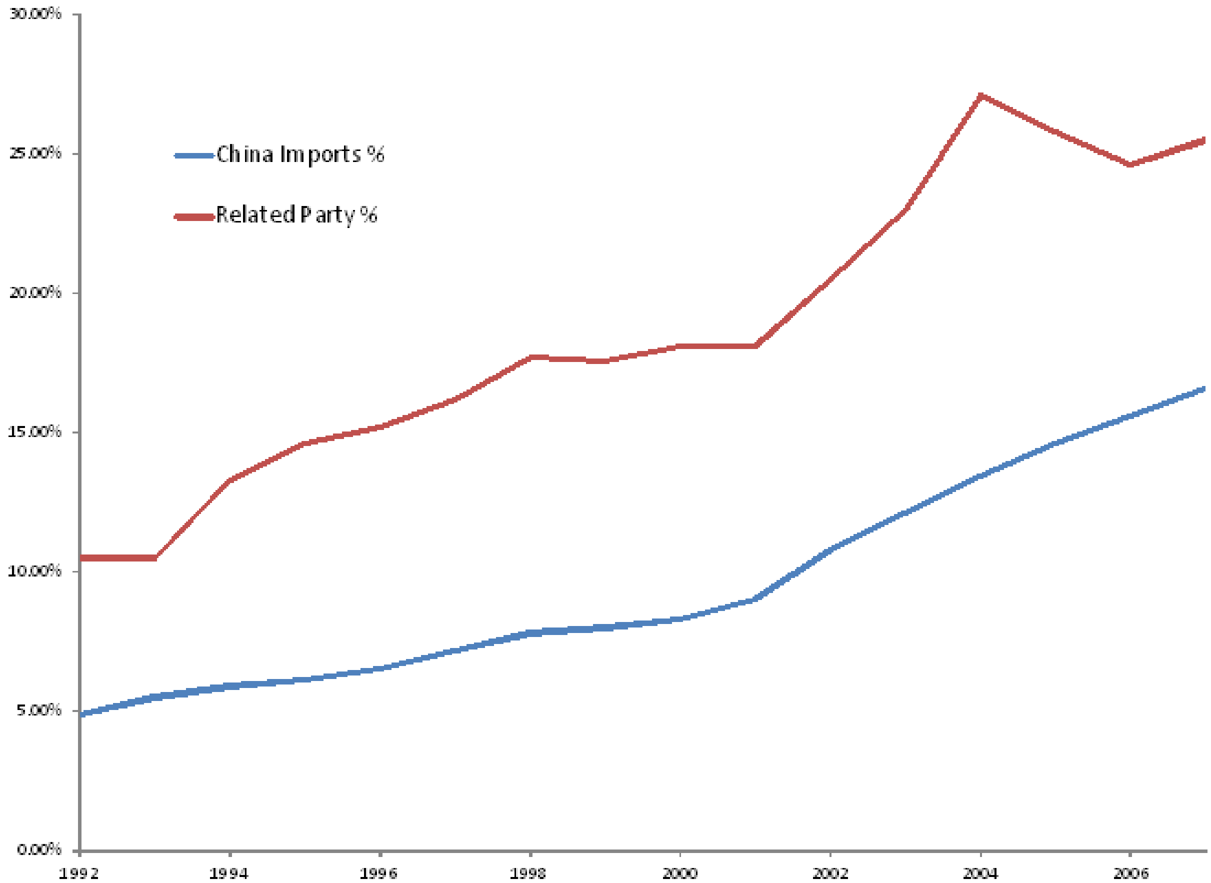
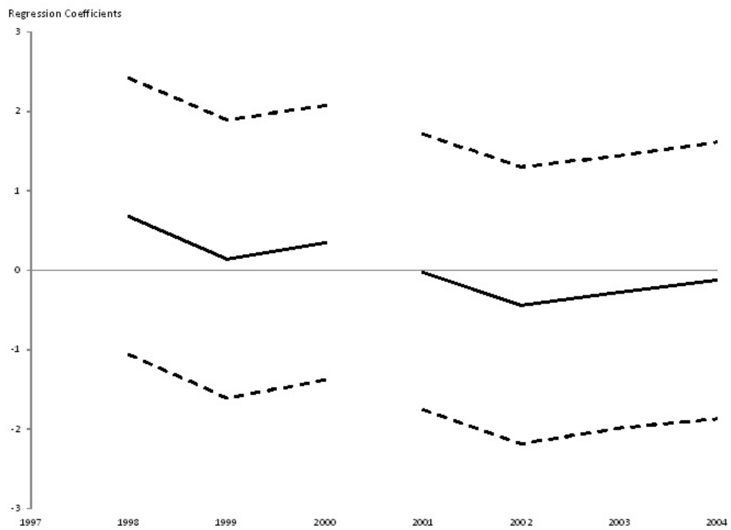


Figure 5: **FDI and Import Growth**

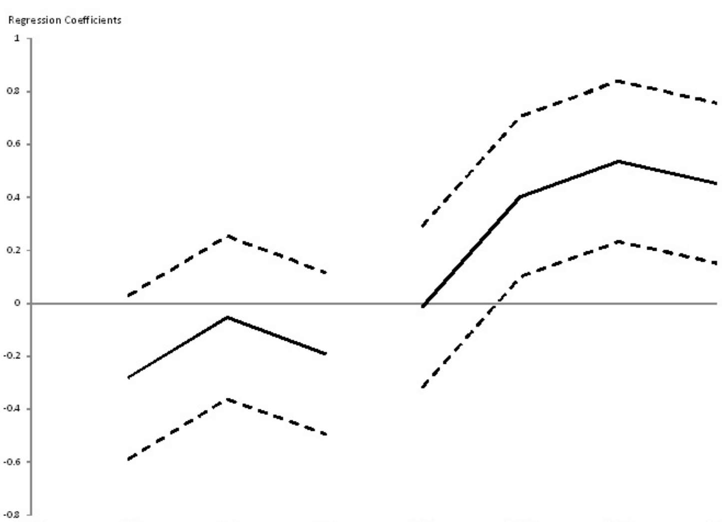
This figure graphs the θ_k coefficients from the following regression:

$$\ln(Val)_{p,t} = \sum_{k=1998}^{2004} \theta_k 1\{k=t\} \times NTRGap_p + \delta_p + \delta_t + \varepsilon_{p,t}$$

Where $\ln(Val)$ is the natural logarithm of Chinese imports for products defined at the six-digit Harmonized System (HS6) level. δ_p and δ_t represent product and year fixed effects respectively. Panel (a) ((b)) represents a subset of products for which foreign investment is restricted (permitted) based on foreign investment guidelines from the China Guidance Catalog for Foreign Investment. Product categories are from Blonigen and Ma (2010). The solid lines represent the point estimates and the dashed lines are 95% confidence intervals based on standard errors clustered by product.



(a) Restricted Projects



(b) Permitted Projects

Table 1: **PNTR, Firm Financing, and Employment**

Panel A of this table presents results for the following regression:

$$\begin{aligned}\ln(Emp)_{f,t}=&\theta_1 PostPNTR_t \times NTRGap_f \times FC_f + \theta_2 PostPNTR_t \times NTRGap_f\\ &+\theta_3 PostPNTR_t \times FC_f + \beta_1' X_f \times PostPNTR_t \times NTRGap_f\\ &+\beta_2' X_f \times PostPNTR_t + \delta_f + \delta_{i,t} + \varepsilon_{f,t}\end{aligned}$$

Where $\ln(Emp)$ is the logarithm of manufacturing employment. f indexes firm, i indexes industry, and t indexes year. FC_f is either net book leverage (column (2)), net market leverage (column (2)), or cash-to-assets ratio (column (2)), X_f includes controls for pre-PNTR firm size, age, and pre-PNTR employment growth (ΔEmp), δ_f is a firm fixed effect and $\delta_{i,t}$ is an industry-by-year fixed effect. All other variable construction is detailed in the Appendix. Standard errors clustered by firm are included in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%. The economic magnitude of θ_1 represents the average amplification a 10% decrease in leverage (5% increase in cash) on employment's sensitivity to the NTR Gap.

	(1) Diff-in-Diff	(2) FC=NBL	(3) FC=NML	(4) FC=Cash
$PostPNTR_t \times NTRGap_f$	-0.781***	-0.058	-0.396	0.608
	(0.243)	(1.458)	(1.416)	(1.539)
$PostPNTR_t \times NTRGap_f \times FC_f$		2.260**	1.735**	-4.490***
		(0.878)	(0.858)	(1.451)
$PostPNTR_t \times FC_f$		-0.839***	-0.670**	1.698***
		(0.306)	(0.298)	(0.550)
$PostPNTR_t \times NTRGap_f \times Size_f$		-0.065	-0.003	-0.087
		(0.212)	(0.210)	(0.212)
$PostPNTR_t \times Size_f$		-0.047	-0.069	-0.040
		(0.071)	(0.070)	(0.071)
$PostPNTR_t \times NTRGap_f \times Age_f$		0.026	0.028	0.036
		(0.054)	(0.053)	(0.052)
$PostPNTR_t \times Age_f$		-0.008	-0.008	-0.011
		(0.017)	(0.017)	(0.017)
$PostPNTR_t \times NTRGap_f \times \Delta Emp$		-1.674	-1.551	-1.508
		(2.570)	(2.585)	(2.484)
$PostPNTR_t \times \Delta Emp$		-2.973***	-2.938***	-2.931***
		(0.863)	(0.869)	(0.839)
Econ Mag of θ_1		29%	22%	57%
N	11,500	11,500	11,500	11,500
adj. R^2	0.919	0.928	0.928	0.928
Year FE	X			
Firm FE	X	X	X	X
Ind × Year FE		X	X	X

Table 2: **PNTR, Firm Financing, and Employment - Robustness**

This table presents several robustness test for the specification in (1). I report the results for net book leverage, net market leverage, and cash in Panels A-C respectively. In column (1) ((2)) I use assets (sales) to measure firm size. In column (3) I require that all firms with the same primary industry classification be assigned the same NTR Gap. In column (4) I measure exposure to the PNTR policy using only the Non-NTR rates. Standard errors clustered by firm (industry in column (3)) are included in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%. The economic magnitude of θ_1 represents the average amplification a 10% decrease in leverage (5% increase in cash) on employment's sensitivity to the NTR Gap.

	(1) Size= Assets	(2) Size= Sales	(3) Gap= Primary	(4) Gap= NNTR
Panel A: Net Book Leverage				
$PostPNTR_t \times NTRGap_f$				0.613
				(1.455)
$PostPNTR_t \times NTRGap_f \times FC_f$	1.694**	1.732**	2.212***	2.333**
	(0.789)	(0.790)	(0.777)	(0.914)
$PostPNTR_t \times FC_f$	-0.684***	-0.689**	-0.792***	-0.904***
	(0.276)	(0.276)	(0.262)	(0.333)
$PostPNTR_t \times NTRGap_f \times Size_f$	0.244*	0.230*		
	(0.137)	(0.137)		
$PostPNTR_t \times Size_f$	-0.105**	-0.108**		
	(0.044)	(0.045)		
Panel B: Net Market Leverage				
$PostPNTR_t \times NTRGap_f$				0.352
				(1.429)
$PostPNTR_t \times NTRGap_f \times FC_f$	1.409*	1.438*	1.577**	1.607*
	(0.768)	(0.766)	(0.783)	(0.859)
$PostPNTR_t \times FC_f$	-0.600**	-0.607*	-0.617**	-0.656**
	(0.272)	(0.272)	(0.266)	(0.312)
$PostPNTR_t \times NTRGap_f \times Size_f$	0.287**	0.273**		
	(0.137)	(0.138)		
$PostPNTR_t \times Size_f$	-0.122***	-0.124***		
	(0.044)	(0.044)		

Table 2 – continued

	(1) Size= Assets	(2) Size= Sales	(3) Gap= Primary	(4) Gap= NNTR
	Panel C: Cash			
$PostPNTR_t \times NTRGap_f$				0.325
				(1.429)
$PostPNTR_t \times NTRGap_f \times FC_f$	-3.367**	-3.319**	-4.351***	-4.487***
	(1.330)	(1.360)	(1.206)	(1.456)
$PostPNTR_t \times FC_f$	1.372***	1.329***	1.648***	1.749***
	(0.504)	(0.513)	(0.453)	(0.570)
$PostPNTR_t \times NTRGap_f \times Size_f$	0.235*	0.216		
	(0.137)	(0.140)		
$PostPNTR_t \times Size_f$	-0.104**	-0.104**		
	(0.044)	(0.045)		
N	11,500	11,500	11,500	11,500
adj. R^2	0.927	0.927	0.928	0.928
Firm FE	X	X	X	X
Ind $\times$ Year FE	X	X	X	X
SE Cluster	Firm	Firm	Ind.	Firm

Table 3: **PNTR, Firm Financing, and Employment - Plant Level**

This table presents plant-level estimates of regressions (3)-(4). All regressions include fixed effects for the interaction of firm size, age, pre-PNTR employment growth, and plant industry. Panel A reports results from estimating (3), panel B reports results from estimating (5), and panel C reports results from estimating (4). Standard errors clustered at the industry level are included in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%. The economic magnitude of θ_1 represents the average amplification a 10% decrease in leverage (5% increase in cash) on employment's or plant closure's sensitivity to the NTR Gap.

Panel A: Intensive Marging - ln($Emp_{p,f,t}$)

	(1) Diff-in-Diff	(2) FC=NBL	(3) FC=NML	(4) FC=Cash
$PostPNTR_t \times NTRGap_f$	-0.351***			
	(0.081)			
$PostPNTR_t \times NTRGap_f \times FC_f$		2.397**	2.523**	-8.478***
		(0.986)	(1.251)	(3.288)
$PostPNTR_t \times FC_f$		-0.825***	-0.877**	2.644**
		(0.305)	(0.429)	(1.097)
Econ Mag of θ_1		68%	72%	121%
N	62,500	62,500	62,500	62,500
adj. R^2	0.923	0.934	0.934	0.934
Year FE	X			
Plant FE	X	X	X	X
$\delta_{c,i,t}$		X	X	X

Panel B: Intensive & Extensive Margins

	$\mathbb{1}\{Death_p\}$				$\Delta\{Emp_p\}$			
	(1) Diff-in-Diff	(2) FC=NBL	(3) FC=NML	(4) FC=Cash	(4) Diff-in-Diff	(6) FC=NBL	(7) FC=NML	(8) FC=Cash
$NTRGap_f$	0.390***				-0.996***			
	(0.149)				(0.307)			
$NTRGap_f \times FC_f$		-0.872	-0.772	5.433***		3.608**	3.244*	-13.71***
		(0.818)	(1.007)	(2.087)		(1.418)	(1.938)	(3.948)
FC_f		0.556*	0.516	-1.885***		-1.738***	-1.594**	4.912***
		(0.283)	(0.391)	(0.714)		(0.478)	(0.738)	(1.274)
Econ Mag of θ_1		22%	20%	70%		36%	33%	69%
N	5,500	5,500	5,500	5,500	5,500	5,500	5,500	5,500
adj. R^2	0.011	0.307	0.306	0.306	0.016	0.287	0.286	0.286
$\delta_{c,i}$		X	X	X		X	X	X

Table 4: **PNTR, Firm Financing, and Employment - Alternative Explanations**

This table addresses various alternative explanations by re-estimating variants of (1) that control for various proxies for alternative mechanisms. I report the results for net book leverage, net market leverage, and cash in Panels A-C respectively. The proxies in columns (1)-(9) respectively are: return on assets (ROA), net profit margin (NPM), market to book ratio (M2B), TFP, value add per employee ($LaborProd$), asset tangibility (Tang), capital intensity ($\ln(CI)$), and indicator that the firm reports earnings from foreign operations (PIFO), and an indicator that the firm reports multiple geographic segments (MNC). Variable definitions are in Internet Appendix A Standard errors clustered by firm are in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

	(1) Proxy= ROA	(2) Proxy= NPM	(3) Proxy= M2B	(4) Proxy= TFP	(5) Proxy= $LaborProd$	(6) Proxy= Tang	(7) Proxy= $\ln(CI)$	(8 Proxy= PIFO	(9) Proxy= MNC
Panel A: Net Book Leverage									
$PostPNTR_t \times NTRGap_f \times FC_f$	2.478***	2.295***	2.381***	2.308***	2.182**	2.707***	2.144**	2.165**	2.002**
	(0.832)	(0.845)	(0.874)	(0.845)	(0.887)	(1.007)	(0.895)	(0.864)	(0.827)
$PostPNTR_t \times NTRGap_f \times Proxy_f$	7.222***	4.230*	0.045	0.455	-0.120	-2.388	0.338	0.521	0.736
	(2.534)	(2.338)	(0.231)	(0.395)	(0.224)	(1.788)	(0.300)	(0.547)	(0.523)
Panel B: Net Market Leverage									
$PostPNTR_t \times NTRGap_f \times FC_f$	2.154***	1.912**	1.727**	1.725**	1.645*	2.110**	1.718**	1.707**	1.499*
	(0.806)	(0.849)	(0.856)	(0.810)	(0.855)	(0.928)	(0.871)	(0.852)	(0.825)
$PostPNTR_t \times NTRGap_f \times Proxy_f$	7.345***	4.559*	0.019	0.429	-0.143	-1.997	0.372	0.604	0.789
	(2.582)	(2.400)	(0.241)	(0.397)	(0.228)	(1.718)	(0.302)	(0.562)	(0.536)
Panel C: Cash									
$PostPNTR_t \times NTRGap_f \times FC_f$	-4.342***	-4.469***	-4.571***	-4.373***	-4.354***	-5.952***	-4.422***	-4.353***	-3.969***
	(1.321)	(1.390)	(1.402)	(1.349)	(1.463)	(1.744)	(1.469)	(1.448)	(1.370)
$PostPNTR_t \times NTRGap_f \times Proxy_f$	6.068**	4.067*	0.028	0.399	-0.141	-2.980	0.358	0.535	0.685
	(2.456)	(2.279)	(0.226)	(0.388)	(0.223)	(1.825)	(0.295)	(0.549)	(0.523)
N	11,500	11,500	11,500	11,500	11,500	11,500	11,500	11,500	11,500
adj. R^2	0.928	0.928	0.928	0.928	0.928	0.928	0.928	0.928	0.928
Firm FE	X	X	X	X	X	X	X	X	X
Ind × Year FE	X	X	X	X	X	X	X	X	X

Table 5: **PNTR, Firm Financing, and Employment - Cross Sectional Test**

This table results for estimating (1) for firms with high and low levels of external finance dependence levels (EFD) using the measure from Rajan and Zingales (1998). To generate the estimates, I interact each coefficient and fixed effect in (1) with an indicator for whether the firm is above the sample median for the EFD measure ($\mathbb{1}\{High\_EFD\}$). The estimates in column (1) are the sum of the coefficients of θ_1 and $\theta_1 \times \mathbb{1}\{High\_EFD\}$. The p-value in column (3) is therefore the p-value on the t-stat of the interaction term of the θ_1 coefficient in (1) with the high-EFD indicator. F-tests of that coefficient yield very similar results. Standard errors clustered by firm are in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

	(1) High EFD	(2) Low EFD	(3) P-val: High - Low
Diff-in-Diff	-0.705	-0.757	0.947
θ_1 NBL	4.634	-0.160	0.009 (***)
θ_1 NML	4.039	-0.633	0.019 (**)
θ_1 Cash	-6.912	0.198	0.047 (**)
N	8,300		

Table 6: **Measures of Firm Scale**

This table presents results for the following regression:

$$y_{f,i,t} = \theta_1 PostPNTR_t \times NTRGap_f \times FC_f + \theta_2 PostPNTR_t \times FC_f \\ + \beta_1' X_f \times PostPNTR_t \times NTRGap_f + \beta_2' X_f \times PostPNTR_t \\ + \delta_f + \delta_{i,t} + \varepsilon_{f,t}$$

Where f indexes the unit of observation, i indexes industry, and t indexes year. $y_{f,i,t}$ is either the logarithm of sales (Panel A) or employment (Panel B) by industry segment. The unit of observation is industry-segment-year. FC_f is either net book leverage, net market leverage, or cash-to-assets ratio, X_f includes controls for pre-PNTR firm size, age, and employment growth, $\delta_{f,i}$ is a segment fixed effect, and $\delta_{i,t}$ is an industry-by-year fixed effect. All other variable construction is detailed in the Appendix. Standard errors clustered by industry (firm) in Panel A (B) are included in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

Panel A: Industry Segment Sales				
	(1) DiD	(2) NBL	(3) NML	(4) Cash
$PostPNTR_t \times NTRGap_f$	0.106			
	(0.275)			
$PostPNTR_t \times NTRGap_f \times FC_f$		-0.787**	0.199	0.466
		(0.372)	(0.401)	(1.065)
$PostPNTR_t \times FC_f$		0.058	-0.083	0.288
		(0.139)	(0.158)	(0.389)
N	26,619	26,619	26,619	26,619
adj. R^2	0.919	0.934	0.934	0.934
Controls		X	X	X
Year FE	X			
Year x Ind FE		X	X	X
Segment FE	X	X	X	X

Panel B: Industry Segment Employment				
	(1) DiD	(2) NBL	(3) NML	(4) Cash
$PostPNTR_t \times NTRGap_f$	0.004			
	(0.252)			
$PostPNTR_t \times NTRGap_f \times FC_f$		-0.630	0.522	1.404
		(0.673)	(0.387)	(1.412)
$PostPNTR_t \times FC_f$		0.025	-0.200	-0.171
		(0.237)	(0.174)	(0.490)
N	12,434	12,434	12,434	12,434
adj. R^2	0.933	0.941	0.941	0.941
Controls		X	X	X
Year FE	X			
Year x Ind FE		X	X	X
Firm FE	X	X	X	X

Table 7: **PNTR, Firm Financing, and Production**

This table presents a qualitative disclosure of the following regression

$$
\begin{aligned}
y_{f,i,t} = & \theta_1 PostPNTR_t \times NTRGap_i \times FC_f + \theta_2 PostPNTR_t \times NTRGap_i + \theta_3 PostPNTR_t \times FC_f \\
& + \beta_1' X_f \times PostPNTR_t \times NTRGap_i + \beta_2' X_f \times PostPNTR_t + \delta_{f,i} + \delta_{f,p} + \varepsilon_{f,t}
\end{aligned}
$$

In Panel A, y is the material cost index for each input industry i used by firm f at time t. In Panel

B, y is the labor productivity (value add per employee) for each industry segment i operated by firm f at time t. In Panel C, y is the natural logarithm of investment in machinery (automation). X_f includes firm size, age, and pre-PNTR employment growth. $\delta_{f,i}$ is a firm-industry fixed effect and $\delta_{f,p}$ is an industry-period fixed effect. Standard errors clustered at the industry level are included in parenthesis. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

	(1) FC=NBL	(2) FC=NML	(3) FC=Cash
Panel A: Input Costs			
θ_1	P	P*	N*
Panel B: Labor Productivity			
θ_1	N*	N*	P
Panel C: Automation			
θ_1	P	P	N
N			
adj. R^2			
Controls	X	X	X
Firm × Ind FE	X	X	X
Ind × Year FE	X	X	X

Table 8: **Event Study**

This table presents cross-sectional regressions of event returns for the November 15, 1999 China-US bilateral deal (columns (1) and (4)), May 25, 2000 PNTR vote (columns (2) and (5)), and their sum (columns (3) and (6)). $NTRGap_f$ is the NTR gap based on the firm's industry code in Compustat. Abnormal returns are the stock's excess return over the CRSP Value-weighted market return. Columns (1)-(3) present day-of abnormal returns and columns (4)-(6) present CAR over the [-1,10] window. Standard errors clustered at the industry level are included in parenthesis. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

	AR[Day Of]			CAR[-1,10]		
	(1)	(2)	(3)	(4)	(5)	(6)
Event=	11/15/99	5/25/00	Sum	11/15/99	5/25/00	Sum
$NTRGap_f$	0.022***	0.023*	0.045***	0.237***	0.127***	0.364***
	(0.008)	(0.013)	(0.014)	(0.061)	(0.044)	(0.096)
Constant	0.000	0.003	0.003	-0.064***	0.007	-0.058*
	(0.003)	(0.005)	(0.005)	(0.022)	(0.014)	(0.032)
N	1,977	1,977	1,977	1,977	1,977	1,977
adj. R^2	0.001	0.001	0.003	0.014	0.002	0.012

Table 9: **PNTR and Import Growth by FDI Category**

Panel A of this table presents results for the following regressions:

$$\ln(Val)_{p,t} = \theta_1 PostPNTR_t \times NTRGap_p + \delta_p + \delta_t + \varepsilon_{p,t}$$
$$\ln(Val)_{p,t} = \theta_1 PostPNTR_t \times NTRGap_p + \theta_2 1\{c = Per\} \times PostPNTR_t \times NTRGap_p + \delta_p + \delta_{c,t} + \varepsilon_{p,c,t}$$

Where $\ln(Val)$ is the natural logarithm of Chinese imports for products defined at the six-digit Harmonized System (HS6) level. $c \in \{Per, Res\}$ indicates whether the product is categorized as permitted (Per) or restricted (Res) for foreign investment based on the 2002 China Guidance Catalog for Foreign Investment. Product categories are from Blonigen and Ma (2010). δ_p, δ_t, and $\delta_{c,t}$ represent product, year, and category-by-year fixed effects respectively. The sample period is 1997-2004. Standard errors clustered at the product level are included in parenthesis. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

Panel A: Investment Restrictions, PNTR, & Chinese Import Growth				
	(1)	(2)	(3)	(4)
$y = \ln(Value)$	Full Sample	Permitted	Restricted	Triple Diff.
$PostPNTR_t \times NTRGap_p$	0.428***	0.479***	-0.512	-0.512
	(0.124)	(0.077)	(0.435)	(0.589)
$1\{c = Per\} \times PostPNTR_t$				0.991*
$\times NTRGap_p$				(0.602)
N	17,915	17,226	689	17,915
adj. R^2	0.872	0.873	0.810	0.872
Product FE	X	X	X	X
Year FE	X	X	X	
Category-Year FE				X

Panel B presents pre-PNTR the percentage of US imports generated from low income (LI) countries as well as the country import "HHI" for Permitted and restricted products in 2000.

Panel B: Pre-PNTR Characteristics By Investment Category		
	(1)	(2)
	LI Ratio	Country "HHI"
Res	0.133***	0.309***
	(0.018)	(0.016)
$Per - Res$	−0.037**	−0.003
	(0.018)	(0.017)
N	2,445	2,445