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

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Avishai Schiff

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Dedicated to my parents, who made getting a PhD seem like the normal
course of action.

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

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This dissertation examines the relationship between a firm's finances and its response to trade liberalization. Understanding how firms with different financial structures react to a major trade liberalization event sheds light both on an important real effect of financial frictions as well as on the underlying mechanisms of reorganization in the U.S. manufacturing sector. In the first chapter, I summarize the nascent yet growing literature at the intersection of corporate finance and trade economics and lay out the contribution of this research.

In the second chapter, I provide institutional details on the natural experiment that lies at the heart of my empirical analysis. The conferral of Permanent Normal Trade Relations (PNTR) status to China, effective 2001, reduced uncertainty about future trade costs and facilitated investment in setting up import and distribution networks in China. I discuss the validity of

the PNTR shock, arguing that it was an unexpected and material relaxation of trade barriers between the U.S. and China. I also lay out the main hypotheses relating firm finances and responses to freer trade. Increased import competition may represent a threat for many U.S. firms, especially those lacking the financial resources to properly respond. 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 the third and final chapter I use the PNTR shock, along with micro-level data from the U.S. Census Bureau, to empirically assess how firm finances relate to trade responses. 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, increased productivity, and are more likely to invest in establishing import networks in China. 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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Chapter 1

Introduction

1.1 Overview

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 dissertation, I examine the relationship between a firm's fi-

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

nances 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, increased productivity at surviving domestic operations, and a higher likelihood of offshoring to China. 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 uncertainty about

future trade costs.² Crucially, exposure to PNTR depends on industry specific tariffs originally 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 that PNTR made permanent (the *NTR Gap*) to measure the policy’s effects.

I begin by analyzing the internal validity of the PNTR experiment. While previous work has established that higher NTR Gap industries exhibit greater post-PNTR Chinese import penetration as well as steeper domestic manufacturing job loss, I focus on market responses as a validation mechanism. Specifically, I show firm abnormal returns covary significantly with their NTR Gap on days in which the market learns new PNTR-related information. I find that each 10% in the NTR Gap leads to abnormal returns that are about 45 basis points higher. Furthermore, I use implied volatility data to show that equity and options markets attached significant uncertainty to the PNTR vote, which helps confirm that stock price movements on the day of the vote are attributable to the resolution of uncertainty following PNTR’s passage.

Next I proceed to analyze how firms with different levels of financial capacity respond to the PNTR shock. I first examine how domestic employ-

²Throughout this dissertation, 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.

ment, 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) is on average associated with an amplified 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 relative to the baseline effect of PNTR. This finding is 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 years prior to PNTR, be orthogonal to any other 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, owing to hysteresis stemming from adjustment costs (Leary and Roberts (2005)) 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.

³The 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.

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

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 use data on reported offshoring in firms' 10-Ks (as col-

lected by Hoberg and Moon (2017) and Hoberg and Moon (2018)) to show that better capitalized firms increase offshoring investment in China. I find that each 10% increase in the NTR gap is associated with a 1.3% higher likelihood that firms report material offshore operations in China. Importantly, a 10% reduction in leverage is associated with an additional 0.98-1.4% increase on top of the unconditional affect. Comfortingly, I find no significant interaction between PNTR, firm finances, and offshoring to the rest of the world. I also show that lower leverage and cash-rich firms exhibit increased post-PNTR debt issuance, which is consistent with a superior ability to finance these offshoring activities.

Next, I show that better capitalized firms do not exhibit sharper reductions in sales in response to PNTR. 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.

Finally, I find additional 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 1.5% (8%) lower input costs for inputs sourced from higher 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 government 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.

⁵As I argue in Chapter 2.2, both events yield material information regarding the effects of Chinese trade liberalization.

1.2 Literature

Spurred by recent advances in data availability as well as geopolitical events (e.g. the rise of China on the global economic stage), the intersection of corporate finance and international economics has become a burgeoning area of research.⁶ Within this literature, this dissertation contributes to the research on firm finances and entry into international trade. 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 dissertation extends this logic to U.S. import market distortions as well and focuses on implications for employment rather than trade flows.

This dissertation also 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)).⁷ While manufacturing workers undoubtedly suffer from increased trade exposure, the effect on firms is less conclusive. Autor et al. (2016a) and Hombert and Ma-tray (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. A particularly related paper is Li

⁶See Fritz Foley and Manova (2015) for a survey of this literature.

⁷See Autor et al. (2016b) for a survey of this literature.

et al. (2018), who show that foreign-owned, rather than Chinese, firms grow more in response to the new investment opportunities created by PNTR. In addition, they link this result to financing capacity by looking at variation in “local finance” across different Chinese provinces. This research also argues that firms benefit from freer trade and offshoring, but unlike Li et al. (2018), it focuses on the domestic reorganization rather than foreign operations. In addition, it analyzes the role of firm finances, which neither Magyari (2017) nor Bretscher (2018) consider.⁸

This dissertation 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 dissertation 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 dissertation 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.

Finally, this dissertation contributes to the literature on firm finances

⁸Bloom et al. (2018) also show that offshoring firms lay off more workers in response to the Chinese trade shocks studied by Autor et al. (2013), but do not analyze its interaction with 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 the relationship between leverage and the cyclical nature of employment (Sharpe (1994), Giroud and Mueller (2017)). This dissertation examines responses to a permanent macroeconomic shock and relates stronger balance sheets to larger employment reductions.

Chapter 2

Institutional Setting & Hypothesis Development

This chapter describes the institutional setting and develops the hypotheses for the paper. I first detail the most salient features of the PNTR policy. I next proceed to show that firms and market participants all perceived the policy change as having a material effect on the U.S. manufacturing sector. Finally, I 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.

2.1 Details of PNTR

2.1.1 History of China - U.S. trade relationship

Chinese imports were historically subject to the “column 2” (non-NTR) tariff rates applicable to all “non-market” economies. These rates, which were originally established by the Smoot-Hawley Tariff Act of 1930, are for the most part substantially higher than the “column 1” tariffs that apply to countries with whom the U.S. has established normal trade relations - e.g. other World Trade Organization (WTO) members.

The Trade Act of 1974 established a mechanism by which non-market economies could have its imports taxed under the “column 1” schedule on a temporary basis. Prior to June 3 of each year, the Executive Branch can recommend to Congress that an individual non-market economy should receive an annual exemption from the “column 2” tariffs and instead have its imports taxed at the “column 1” rates. Upon receipt of this recommendation, Congress has 90 to pass a joint resolution in both the House and the Senate overriding the Executive Branch’s recommendation. If said resolution is not passed, then the recommendation stands until next year.

China had received the Executive’s exemption in each year since 1980 and through those first ten years, there was no dissent from Congress. However, following the Tiananmen Square incident in 1989, the House actually managed to pass a joint resolution against China’s exemption in 1990, 1991, and 1992. While the Senate failed pass its own joint resolution, and subsequent years saw neither chamber succeed in overriding the Executive’s exemption, the annual requirement for both Presidential and Congressional approval creates uncertainty about future exemptions being passed. I discuss the materiality of this uncertainty in further detail in Section 2.2.

On May 24, 2000, the U.S. House of Representatives voted to make permanent the relatively low “column 1” tariff rate schedule for Chinese goods.¹ PNTR is therefore unique in that it did not actually reduce effective tariffs,

¹China’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.

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. President Clinton signed the bill into law in October of that year. 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.

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.² 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.

2.1.2 The NTR Gap

Starting with Pierce and Schott (2016), the literature has used the difference between the “column 1” and “column 2” as a simple and intuitive measure for capturing exposure to PNTR. Industries with larger difference between the NTR and non-NTR tariffs, i.e. higher *NTR Gaps*, experienced a bigger reduction in uncertainty upon’s PNTR passage. To calculate industry-level NTR Gaps, I use the ad valorem equivalent NTR and non-NTR tariff

²Shleifer and Vishny (2011) provide a summary of the relatively scarce empirical evidence.

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. Table 2.1 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.

2.2 Materiality of PNTR

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 induce firms to respond. The average NTR Gap in 1999 is 33%.³ 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

³Pierce and Schott (2016) reference several anecdotes of firms indicating that the uncertainty regarding China’s NTR status materially affects their operations.

uncertainty surrounding input costs and Handley and Limao (2015) apply this specific logic to a model of trade cost uncertainty.⁴ 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.

Table 2.2 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 NTR Gap is simply a proxy for the type of manufacturing most easily replaced by Chinese imports. 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 this concern. Pierce and Schott (2016) further work to mitigate this concern by showing that

⁴It 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.

the NTR Gap is unassociated with large employment declines in the European Union, where China had benefited from NTR status prior to PNTR's passage.

2.2.1 Anecdotal Evidence

A skeptical reader may worry that since China's NTR status was granted in the twenty years prior to PNTR, the actual vote to grant China permanent NTR status would not have had any practical effect. However, anecdotal evidence from U.S. manufacturing firms and market participants prior to and immediately after the PNTR vote helps confirm its material affect. Below I present several examples of firms discussing how uncertainty regarding Chinese import tariffs affects their decision making:

[U.S. firms] cited uncertainty surrounding the annual renewal of Chinas most-favored-nation trade status as the single most important issue affecting U.S. trade relations with China.

...

The annual MFN review process may also be a negative factor for U.S. companies in securing financing for business transactions in China from the international lending community.

United States General Accounting Office (1994)

We view the imposition conditions upon the renewal of MFN as virtually synonymous with outright revocation... We cannot plan and run our businesses if we are wondering whether our most important source of supply is about to disappear.

Pearce (1996)

Further anecdotal evidence suggests that not only were firms worried about the uncertainty induced by China's temporary NTR status, they were actively pushing for the passage of PNTR. A multitude of individual corporations and industry groups lobbied for and hailed the bill's passage (Phillips (2000a)). For example, the Business Roundtable, a coalition of over 200 large corporations, spent over \$9 million promoting PNTR (Phillips (2000b)). Additional anecdotes depicting the strong support of PNTR's passage by corporate interests are below:

The fact that the United States does not accord China permanent NTR status creates uncertainty for America's toy companies and exposes them to unwelcome risk. While the risk that the United States would withdraw NTR status from China may be small, if it did occur the consequences would be catastrophic for U.S. toy companies given the 70 percent non-MFN U.S. rate of duty applicable to toys. As a result, Mattel strongly supports congressional approval of legislation granting permanent NTR status to China upon its WTO accession.

St. Maxens (2000)

High tech industry representatives Tuesday told House Republican leaders during a closed meeting that gaining permanent normal trade relations status for China is their top priority—and pledged to help garner congressional support for passage...

Congress Daily (2000)

Michael R. Bonsignore, Chief Executive Officer of Honeywell (NYSE: HON), today urged members of the U.S. House of Representatives Ways and Means Committee to grant Permanent Normal Trade Relations (PNTR) status to China this year. Bonsignore said doing so will enable U.S. companies and China to begin working toward realizing the broad, mutual benefits of the recent U.S.-China WTO (World Trade Organization) agreement.

Business Wire (2000)

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)). 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.

Not surprisingly, the reaction from organized labor groups was exactly antithetical to that of business groups:

With the AFL-CIO leading the way, organized labor has made defeating the China trade legislation its number one priority this year, and it is waging an aggressive effort to persuade Democrats not to abandon their annual vote on China's trade status.

Vita (2000)

While these anecdotes cannot substitute for systematic (i.e. economic and econometric) proof, they serve as a useful primer for establishing that PNTR materially impacts firms with domestic manufacturing operations.

2.2.2 Event Study

In search of systematic evidence that markets viewed the granting of PNTR status to be a material event, I conduct an event study analysis. The analysis considers two events that contained significant news about PNTR's possible passage. The first event is the November 15, 1999 signing of a bilateral trade agreement between China and the U.S. The deal outlined a series of concessions made by China, including a reduction in tariffs and easing of restrictions on foreign investment. In return, the U.S. pledged to support China's WTO bid and, crucially for my analysis, vote on PNTR (*New York Times* (1999)).

To understand the unexpected nature of the agreement, one should consider that previous talks in April of that year had stalled and there was no indication of a restart until the trade delegation was dispatched rather suddenly to China on November 9 (*Associated Press* (1999b)). Examination of

press articles around the agreement indicates that the first mention of the signing occurred at 1:03 AM on November 15, 1999. Reports from after the close of the market on November 14 suggest that no previous information leakage regarding deal closure existed prior to its signing (*Associated Press* (1999a)). In fact, the agreement was signed only after the U.S. trade representative had agreed to stop at the Chinese Trade Ministry on her way out of the country (Devereaux and Lawrence (2004)). Therefore, I use November 15 as the event day for the agreement.

While the deal served as the first material step towards liberalizing Sino-U.S. trade it would go into effect only upon China's entry into the WTO, and the Chinese had made clear their intention to link that step to the passage of PNTR (*Dow Jones Business News* (1999)). Therefore, the second event I consider is the Congressional vote on PNTR. President Clinton spear-headed the push to pass PNTR, but faced strong resistance from labor groups and many Democrats in the U.S. House of Representatives. Support for PNTR was widespread in the Senate, but the House vote, which took place after market close on May 24, 2000, was considered the largest obstacle to the policy's passage. Highlighting the uncertainty around the House vote, news reports from May 23rd, the day before it was set to take place, suggest neither side was confident it had secured enough support (Keto (2000)). Ultimately, the House voted 237-197 in favor of PNTR. I use May 25 as the event day since the vote took place after market hours.

Table 2.3 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.⁵ Both events are associated with significantly 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 thus consistent with markets ascribing materiality to PNTR-related news.

2.2.3 Evidence from Options Markets

The event study analysis points to markets considering news about PNTR to significantly affect firm valuations. However, there exists the concern that some factor unrelated to PNTR may be driving the results in Table 2.3. Two factors mitigating this concern are that markets react significantly to multiple (2) instances PNTR-related news and that market responses depend on the degree of exposure to PNTR (i.e. the NTR Gap).

⁵Results 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.

To further re-enforce the materiality of the policy change, I rely on evidence from options markets. The intuition of the analysis is as follows. If market participants anticipate a particular event, say the House vote on PNTR, to significantly move stock prices, then uncertainty about the outcome of the event should lead to higher expected volatility over any time horizon that includes that event. Put another way, comparing the implied volatility estimates from options whose maturity spans a political event to nearby options expiring outside of the event window can provide ex-ante evidence that markets attach significant uncertainty to the event.⁶ As Kelly et al. (2016) argue, options are uniquely suited to measuring the degree and materiality of political uncertainty because they allow one to compare market expectations around relatively short time horizons bracketing political events.

I operationalize the above logic in two ways. The first is running the following generalized difference-in-difference regression

$$ImpVol_{f,t} = \theta_1 PostPNTR_t \times NTRGap_f + \delta_f + \delta_t + \varepsilon_{f,t} \quad (2.1)$$

Where $ImpVol_{f,t}$ is the Black-Scholes implied volatility estimate (from OptionMetrics) for firm f on day t , $PostPNTR_t$ is an indicator for days after May 25, 2000, $NTRGap_f$ is the NTR Gap of firm f 's industry, and δ_f and δ_t are firm and day fixed effects respectively. The coefficient of interest is θ_1 which measures whether firms in higher NTR Gap industries (first difference) see

⁶This same logic underscores why the analysis is incompatible with the 11/15/99 event. For markets to form ex-ante expectations on volatility, they would need to be aware of the event, and be uncertain of its outcome, sufficiently in advance of it occurring.

bigger reductions in implied volatility after the PNTR vote (second difference). A significantly negative coefficient for θ_1 suggests that for higher NTR Gap industries, the implied volatility estimated over windows that cover the PNTR vote date is significantly higher than the implied volatility estimated over windows after the vote. This result would be consistent with markets ascribing more uncertainty due to the PNTR vote to higher NTR Gap firms. I present the result of the analysis in two ways. First, Figure 2.1 presents the daily coefficients ($\theta_{1,k}$) from the following regression

$$ImpVol_{f,t} = \sum_{k=5/1/00}^{6/30/00} \left[\theta_k 1\{t = k\} \times NTRGap_f \right] + \delta_f + \delta_t + \varepsilon_{f,t} \quad (2.2)$$

Where $ImpVol_{f,t}$ is the 30-day (interpolated) implied volatility measured as of day t over 5/1/00-6/30/00. As the Figure shows, implied volatility estimates for higher NTR Gap firms drops significantly after the PNTR vote (marked by the dashed red line), consistent with uncertainty being resolved on that day. In addition, in Table 2.4, I report estimates of θ_1 from (2.1) using 30- and 60-day horizons. In columns (1) and (2), I include all manufacturing firms with available data. In columns (3) and (4), I restrict the analysis to only firms with market caps that place them within the set of the 1,000 largest domestic companies. For both horizons - and in both sub-samples - θ_1 is significantly negative. The estimates suggest that a 10% increase in NTR Gap leads markets to expect an additional 0.84-1.01% volatility in returns due to the PNTR vote.

In addition to the difference-in-difference analysis, I also follow Kelly et al. (2016) and assess the uncertainty surrounding the the PNTR vote in the following way. First, I obtain implied volatility data for manufacturing firms for all at the money ($|\Delta|$ between .4 and .5) options that expire on 4/22/00, 6/17/00, and 7/22/00. These dates respectively correspond to dates a , b , and c in Kelly et al. (2016) where $a < \tau < b < c$ and τ is the date of the PNTR vote. Then, again following Kelly et al. (2016), for each firm f I calculate

$$\widehat{IV}_b^f = \{MeanIV_{b-s,b}^f : b - s \in [\tau - 20, \tau - 1]\}$$

That is \widehat{IV}_b^f is the average implied volatility for firm f over 20 different horizon windows each ending at date $b = 6/17/00$ and spanning the PNTR vote date τ . By averaging implied volatility estimates over multiple options and about one month's worth of trading day horizons, I minimize day-to-day noise. I then calculate

$$IVD^f = \widehat{IV}_b^f - \frac{1}{2}(\widehat{IV}_a^f + \widehat{IV}_c^f) \quad (2.3)$$

Where \widehat{IV}_a^f and \widehat{IV}_c^f are calculated in an analogous way to \widehat{IV}_b^f , except the date horizons end at date $a = 4/22/00$ and $c = 7/22/00$ respectively (and $a - s$ and $c - s$ are chosen to have the same number of trading days as between $b - s$ and b).

The idea behind (2.3) is that options expiring around windows close to event day τ , but not including it, should capture similar conditions for firm f without capturing any volatility that markets ascribe to the political event. They should therefore serve as valid controls for determining if expected

volatility is indeed higher over windows that do span τ . Kelly et al. (2016) argue that if markets indeed attach political uncertainty to the vote occurring on day τ , then IVD^f should be positive. Furthermore, it should be increasing in the degree of uncertainty, which in this case means it that it should be increasing in the firm’s NTR Gap. Confirming this analysis, I find that the average IVD^f in my sample is 0.025 (p-val of 0.001). This means that implied volatility for options that span event day τ is 2.5% higher than the control period. Furthermore, I find that high NTR Gap firms have IVD^f that is on average 3.44% higher (p-val of 0.031).⁷ These estimates corroborate the idea that market participants anticipated material uncertainty resolution as a result of the PNTR vote, and that this effect is larger for firms more affected by the policy.

In closing this discussion, it is worth noting that PNTR is only one of the relevant components contributing to the tremendous growth in Chinese import penetration, 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

⁷I define high NTR Gap firms as those above the sample median.

Chinese trade barriers.⁸ In addition, the results in Chapter 3 apply only 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.

2.3 Hypothesis Development

Having established PNTR as a material change in trade barriers between the U.S. and China, I proceed to discussing the main hypotheses of my analysis. Each margin of trade 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

⁸I discuss the competitive and offshoring implications of supply-side shocks further in Section 3.2.1.

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.⁹ 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.

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.

⁹See also Chevalier (1995), Zingales (1998), and Khanna and Tice (2000).

(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.¹⁰

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, and 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 international trade. To demonstrate this “offshoring premium,” Appendix Table A.1 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 Appendix A). While these univariate correlations come with the standard

¹⁰It 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.

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.¹¹

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, 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

¹¹In 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.

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 who rely 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, to the extent that it is possible to measure it, we should observe an increase in offshoring investment. Furthermore, the re-organization should manifest in changes to 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 from 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 report increased operations in China and experience larger domestic productivity gains in response to PNTR.

Chapter 3

PNTR, Firm Finances, & Offshoring

3.1 Data & Empirical Strategy

3.1.1 Data Sources

3.1.1.1 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, payroll, location, primary industry, and owning firm for virtually every establishment with employment in the non-farm private U.S. economy.¹ 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,

¹See Jarmin and Miranda (2002) for an overview of the LBD.

surveys the universe of manufacturing establishments in the U.S.² 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 Appendix B) 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).

²I exclude from my sample any administrative records, as values for those observations are often times imputed.

3.1.1.2 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 Appendix B.

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 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.

3.1.2 Offshoring Data

To measure offshoring investments in China (and other countries), I use the text-based measure of offshoring activity compiled by Hoberg and Moon

(2017) and Hoberg and Moon (2018). This data is constructed by searching 10-K filings for target words such as “DISTRIBUT*”; “SHIPMENT*”; “IMPORT*”; “VENDOR*”; “SUPPLIER*”; “PURCHAS*”; “SUBSIDIAR*”; “PLANT*”; “MANUFATUR*”; etc. (an “*” denotes a wild-card) within the same paragraph as (and within 25 words of) country names. The data thus allow me to observe firms that report substantial offshoring activity and the countries in which it takes place. The data is available starting in 1997.

3.1.2.1 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 1999.³ 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

³Historical classification of low income countries can be found at <http://databank.worldbank.org/data/download/site-content/OGHIST.xls>.

the CMF and Annual Survey of Manufacturers (ASM) and is aggregated to the industry level for public dissemination by Becker et al. (2013).

3.1.3 Sample Construction

The baseline analysis period is 1992-2007. This time frame concurs 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.

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.

3.1.4 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

$$\begin{aligned} \ln(\text{Emp})_{f,t} = & \theta_1 \text{PostPNTR}_t \times \text{NTRGap}_f \times \text{FC}_f + \theta_2 \text{PostPNTR}_t \times \text{NTRGap}_f \\ & + \theta_3 \text{PostPNTR}_t \times \text{FC}_f + \beta'_1 X_f \times \text{PostPNTR}_t \times \text{NTRGap}_f \\ & + \beta'_2 X_f \times \text{PostPNTR}_t + \delta_f + \delta_{i,t} + \varepsilon_{f,t} \end{aligned} \quad (3.1)$$

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 employ-

ment 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

$$\ln(Emp)_{f,t} = \sum_{k \in K} \left[\theta_{1,k} 1\{t \in k\} \times NTRGap_f \times FC_f + \theta_{2,k} 1\{t \in k\} \times NTRGap_f + \theta_{3,k} 1\{t \in k\} \times FC_f + \beta'_{1,k} X_f \times 1\{t \in k\} \times NTRGap_f + \beta'_{2,k} X_f \times 1\{t \in k\} \right] + \delta_f + \delta_{i,t} + \varepsilon_{f,t} \quad (3.2)$$

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 short-term and longer-term post-PNTR responses respectively. The remaining controls and fixed effects are the same as in (3.1).

3.1.5 PNTR & Manufacturing Employment - Baseline Results

I begin with a simple graphical representation of the baseline tests. Panel (a) of Figure 3.1 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 3.1, 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%.⁴

Next, I present the results from estimating (3.1) in Table 3.1. 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

⁴In compliance with the Census confidential disclosure policy, I do not report the results for net market leverage, which are qualitatively similar.

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 3.1) 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.⁵ 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 3.1 are robust to controlling for firm size and age, employment pre-trends, unobserved inter-industry heterogeneity, and firm fixed effects.

In Figure 3.2, 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.

⁵These 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 $\frac{-2.26*0.1}{-0.78} = 0.29$.

3.1.6 PNTR & Manufacturing Employment - Additional Tests

I conduct several robustness tests, which I report in Table 3.2. First, I confirm that results are robust to sales (column (1)) and assets (column (2)) as the measure of firm size. Second, rather than calculating individual NTR Gaps for each firms, I force 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 (4)). This last test forces variation in PNTR exposure to come from the more plausibly exogenous Smoot-Hawley tariff rates. I present the robustness tests in Table 3.2 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

$$\ln(Emp)_{f,p,t} = \theta_1 PostPNTR_t \times NTRGap_p \times FC_f + \theta_2 PostPNTR_t \times FC_f + \delta_p + \delta_{c,i,t} + \varepsilon_{f,p,t} \quad (3.3)$$

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

$$\mathbb{1}\{Death_p\} = \theta_1 NTRGap_p \times FC_f + \theta_2 FC_f + \delta_{f,i} + \varepsilon_p \quad (3.4)$$

$$\Delta\{Emp_p\} = \theta_1 NTRGap_p \times FC_f + \theta_2 FC_f + \delta_{f,i} + \varepsilon_p \quad (3.5)$$

I report the results from estimating (3.3)-(3.4) using net book leverage, net market leverage, and cash in panels A and B of Table 3.3 respectively. The plant-level tests conform with the takeaways from the firm-level regressions. Firms with 10% less leverage on average have PNTR-induced employment

reductions that are 33%-36% (columns (2) and (3) of panel B) larger than the unconditional effect (from column (1)). Better capitalized firms are also, on average, 20%-70% more likely to close plants in higher NTR Gap industries (columns (5) through (8) in Panel B).⁶

3.1.7 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.

3.1.7.1 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 3.1.9) rouses suspicions that firms supporting the bill were poised to invest in offshoring regardless of its passage and had

⁶Due 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.

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 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. Furthermore, as 2.2 reports, there is no significant relationship between the NTR Gap and pre-PNTR employment or import growth. Since unconditional import penetration and manufacturing employment declines preceded PNTR, the lack of pre-trends helps mitigate reverse causality issues.

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 (3.1) 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 Chapter 2.2). 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 Table 2.2 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)).⁷ 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.

3.1.7.2 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

⁷Given 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)).

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 (3.1), 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 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 3.4. 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.

3.1.7.3 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 3.5. 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.

3.1.8 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.

I begin by examining PNTR-related issuance of debt and equity. The inherent logic in the above analysis is that firms with less leverage and/or more cash are more easily able to marshal external (or rely on internal) resources to finance offshoring investments. One should therefore expect that better capitalized firms “draw down” on their financial capacity after PNTR is passed. To verify this argument, I re-estimate (3.1) using debt and equity issuance as the outcome variables and present the results in Table 3.6. I report the results for debt issuance in Panel A. In Columns (1)-(3), I check whether conditioning issuance activity on ex-ante financing is associated with increased debt issuance. The estimates on the triple interaction coefficient θ_1 suggest that ex-ante better capitalized firms do indeed issue more debt in response to PNTR. A one standard deviation reduction in leverage is associated with increased post-PNTR debt issuance of between 4-7% relative to the pre-PNTR period (Columns (2) and (3)). A standard deviation increase

in cash holdings is associated with increased post-PNTR debt issuance of between 11% relative to the pre-PNTR period (Column (4)). As Giroud and Mueller (2017) argue, these debt issuance results are consistent with ex-ante less levered and cash-rich firms drawing down on the debt capacity once new investment opportunities arise.

I look at equity issuance in Panel B. If offshoring opportunities are sufficiently lucrative, firms with exhausted debt capacity may try to issue additional equity to finance their investment. The results in Panel B are consistent with this hypothesis. I find at least some evidence that more levered firms increase equity issuance in response to the shock. A standard deviation increase in net book (market) leverage is associated with increased equity issuance of about 7% (3%).⁸

I next proceed in 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

$$\begin{aligned} \ln(\text{Sale})_{f,i,t} = & \theta_1 \text{PostPNTR}_t \times \text{NTRGap}_f \times \text{FC}_f + \theta_2 \text{PostPNTR}_t \times \text{FC}_f \\ & + \beta'_1 X_f \times \text{PostPNTR}_t \times \text{NTRGap}_f + \beta'_2 X_f \times \text{PostPNTR}_t \\ & + \delta_f + \delta_{i,t} + \varepsilon_{f,t} \end{aligned} \quad (3.6)$$

⁸If issuance costs for equity are higher than those for debt and offshoring exhibits decreasing returns to scale, then firms with unused debt capacity should be able to engage in greater offshoring investment than those forced to issue equity.

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 (3.1) except they are all sourced from Compustat. The results from this exercise, summarized in Panel A of Table 3.8, 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 un-tabulated tests, 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 (3.6) using Compustat data on industry segment employment. I report the results in Panel B of Table 3.8. 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 3.1-3.5, 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 check for evidence that better capitalized firms complement domestic manufacturing employment reductions with offshoring investment in China. This exercise is complicated by the fact that readily available investment data from Compustat usually comes at the aggregate level. Additionally, firms who outsource to third party manufacturers or engage in joint ventures may not report this activity directly in financial statements. To overcome this challenge, I rely on data of reported offshoring activity from the body of firms' 10-K filings. This data comes from collected Hoberg and Moon (2017) and Hoberg and Moon (2017) and I use it to estimate the following regression

$$\begin{aligned}
\mathbb{1}\{Offshore\}_{f,t}^x = & \theta_1 PostPNTR_t \times NTRGap_i \times FC_f \\
& + \theta_2 PostPNTR_t \times NTRGap_i \\
& + \theta_3 PostPNTR_t \times FC_f \\
& + \beta'_1 X_f \times PostPNTR_t \times NTRGap_i \\
& + \beta'_2 X_f \times PostPNTR_t + \delta_f + \delta_{i,p} + \varepsilon_{f,t} \quad (3.7)
\end{aligned}$$

Where $\mathbb{1}\{Offshore\}_{f,t}^x$ is an indicator that firm f reports offshore operations in country x during year t and $x \in \{China, ROW\}$. Along with the control

set X_f , I also include firm (δ_f) and industry-period ($\delta_{i,p}$ where $p \in \{Pre - PNTR, Post - PNTR\}$) fixed effects. Under the offshoring hypothesis, higher NTR Gap firms should see increased reporting in offshored Chinese operations after PNTR is passed. Furthermore a negative (positive) coefficient for θ_1 is consistent with less levered (cash rich) firms undertaking these investments at even higher frequencies. I report the results of this analysis in Table 3.7. The first row of Table 3.7 shows that unconditionally, a 10% increase in NTR Gap leads to about 1.3% higher likelihood of reporting offshored operations in China in the post-PNTR period (column (1)). This effect represents an increase of about 5% relative to the pre-PNTR probability of 27.2%. Examining the θ_1 coefficient in column (1) shows that a 10% reductions in leverage is associated a further 0.98%-1.46% increase in offshoring likelihood. While the estimate for cash holdings is smaller and statistically insignificant, overall the results are consistent with better capitalized firms shedding domestic employment in favor of offshored production to China. In column (2) I check whether PNTR, and its interaction with firm finances, is associated with increased offshoring outside of China. The coefficients for those regressions are all statistically insignificant and the sign suggests that if anything, in the post-PNTR period, firms substitute away from other foreign countries towards China. Finally, in column (3) I test whether the differential offshoring response to China versus the rest of the world (ROW) is statistically different. I find this to be the case for all instances except for the regression using pre-PNTR cash holdings as a measure of financial capacity.

I next examine the connection between PNTR, offshoring, and input costs. Figure 3.3 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

$$\begin{aligned}
\ln(\text{Matcost})_{f,i,t} = & \theta_1 \text{PostPNTR}_t \times \text{NTRGap}_i \times \text{FC}_f \\
& + \theta_2 \text{PostPNTR}_t \times \text{NTRGap}_i \\
& + \theta_3 \text{PostPNTR}_t \times \text{FC}_f + \beta'_1 X_f \times \text{PostPNTR}_t \times \text{NTRGap}_i \\
& + \beta'_2 X_f \times \text{PostPNTR}_t + \delta_{f,i} + \delta_{f,p} + \varepsilon_{f,t}
\end{aligned} \tag{3.8}$$

The unit of observation $\ln(\text{Matcost})_{f,i,t}$ 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 $p \in \{\text{Pre} - \text{PNTR}, \text{Post} - \text{PNTR}\}$) fixed effects. I report the results in panel A of Table 3.9 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 1.5%-8.5%.

To test whether offshoring leads to measurable gains in domestic productivity, I reestimate (3.8) with *LaborProd* (value add per employee) as the dependent variable. Figure 3.4 shows that the unconditionally, higher NTR Gap industries experience a small yet significant increase in labor productivity. I report the results conditioning on firm finances in panel B of Table

3.9. The unit of observation is the value add per employee for each domestic manufacturing industry the firm operates. The negative (positive) sign for θ_1 in the leverage (cash) regressions indicate that better capitalized firms realize significant productivity gains along the order of 1% for each 10% increase in the NTR Gap.

Finally, firms may respond to Chinese competition by investing in labor-saving technology (automation). Since the CMF data provide explicit break-outs of domestic capital expenditures for machinery, I re-estimate (3.8) using machinery outlays (*MachCapex*) as the outcome variable and report the result in panel C of Table 3.9. I find no meaningful relationship between the NTR Gap, firm finances, and investment in machinery.

3.1.9 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 (see Chapter 2.2) 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. In addition, the positive market reaction to PNTR-related news (See Table 2.3) is consistent with markets anticipating that firms will take advantage of new offshoring investment opportunities and cheaper labor abroad.

To complement these findings, I present evidence consistent with foreign investment serving as a driver of the post-PNTR growth in Chinese imports. Figure 3.5 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 3.6 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, Figures 3.5 and 3.6 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.⁹ 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.¹⁰

⁹See Davies and Balve-Hauff (2003) for more information.

¹⁰The 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.

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 for products in the restricted and permitted categories

$$\ln(Val)_{p,t} = \sum_{k=1998}^{2004} \theta_k 1\{k = t\} \times NTRGap_p + \delta_p + \delta_t + \varepsilon_{p,t} \quad (3.9)$$

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 3.7. 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)).¹¹ To verify that these differences are statistically significant I estimate the following diff-in-diff and triple difference regressions

$$\ln(Val)_{p,t} = \theta_1 PostPNTR_t \times NTRGap_p + \delta_p + \delta_t + \varepsilon_{p,t} \quad (3.10)$$

$$\begin{aligned} \ln(Val)_{p,t} = & \theta_1 PostPNTR_t \times NTRGap_p \\ & + \theta_2 1\{c = Per\} \times PostPNTR_t \times NTRGap_p \\ & + \delta_p + \delta_{c,t} + \varepsilon_{p,c,t} \end{aligned} \quad (3.11)$$

I estimate (3.10) for the the entire sample as well as for the restricted and permitted subsamples separately. I report the results in Table 3.10. A 10% increase in the NTR Gap is associated with 4.3% larger Chinese import

¹¹Consistent 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.

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 3.10) 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 Appendix Table 3.11). 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 better natural fit for the legal and economic environment in China. This logic then suggests that the estimation of θ_2 in (3.11) 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.¹²

¹²The difference in difference coefficient for (3.10) 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.

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 3.10. 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.¹³ 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”

¹³Encouraged products have an even lower share of lower-income sourcing than permitted projects.

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 more competition. Therefore, it is comforting, as column (2) in Panel B of Table 3.10 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.

3.2 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.

3.2.1 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.

3.2.2 Conclusion

In this dissertation, 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.

Table 2.1: **Highest and Lowest NTR Gap Industries**

Highest NTR Gap Industries			
Industry	NTR	Non-NTR	NTR Gap
Blank Magnetic and Optical Recording Media Manufacturing	0.000	0.800	0.800
Plastics Bottle Manufacturing	0.030	0.800	0.770
Toilet Preparation Manufacturing	0.015	0.748	0.732
Other Knit Fabric and Lace Mills	0.105	0.834	0.729
Cigarette Manufacturing	0.074	0.745	0.671
Other Hosiery and Sock Mills	0.145	0.803	0.658
Doll and Stuffed Toy Manufacturing	0.002	0.638	0.637
Game, Toy, and Children's Vehicle Manufacturing	0.002	0.638	0.637
Totalizing Fluid Meter and Counting Device Manufacturing	0.026	0.653	0.626
Schiffli Machine Embroidery	0.056	0.672	0.616

Lowest NTR Gap Industries			
Industry	NTR	Non-NTR	NTR Gap
Phosphatic Fertilizer Manufacturing	0.001	0.004	0.003
Malt Manufacturing	0.010	0.025	0.015
Pulp Mills	0.002	0.020	0.019
Cement Manufacturing	0.000	0.026	0.026
Farm Machinery and Equipment Manufacturing	0.001	0.034	0.033
Petroleum Refineries	0.012	0.048	0.036
Wood Preservation	0.003	0.048	0.045
Bottled Water Manufacturing	0.003	0.049	0.046
Ice Manufacturing	0.003	0.049	0.046
Soft Drink Manufacturing	0.003	0.049	0.046

Table 2.2: NTR Gap Correlations

This table presents correlations of the NTR gap with industry and firm characteristics. Industry characteristics are from the NBER-CES dataset. Import data are from the UTO. Firm financial variables are measured in 1999, the year prior to the PNTR vote. Variable definitions are included in the appendix. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

	(1)	(2)	Industry Char.			Imports			Firm Char.									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
$\ln(\text{CapitalIntense})$	-0.354***																	
$\ln(\text{SkillIntense})$		-0.077																
ValAdd			-0.182***															
$\frac{\text{Emp}}{\Delta \text{Emp}}$				-0.026														
ΔShip					0.0286													
ΔShip_{pre}						0.0286												
$\Delta \text{Imports}_{pre}$						-0.003												
$\ln(\text{asset})$							-0.190***											
$\ln(\text{emp})$								0.174***										
MktCap									-0.48**									
age										-0.108***								
$\max(\text{ROA}, 0)$											-0.088***							
$\max(\text{NPM}, 0)$												-0.0634***						
Tangibility													-0.274***					
$\ln(\text{CapitalIntense})$														-0.254***				
M2B														0.022				
NetBkLev															-0.004			
NetMktLev																-0.099***		
$\frac{\text{Cash}}{\text{Assets}}$																		0.123***

Table 2.3: **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%.

Event=	AR[Day Of]			CAR[-1,10]		
	(1) 11/15/99	(2) 5/25/00	(3) Sum	(4) 11/15/99	(5) 5/25/00	(6) Sum
$NTRGap_f$	0.022*** (0.008)	0.023* (0.013)	0.045*** (0.014)	0.237*** (0.061)	0.127*** (0.044)	0.364*** (0.096)
Constant	0.000 (0.003)	0.003 (0.005)	0.003 (0.005)	-0.064*** (0.022)	0.007 (0.014)	-0.058* (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 2.4: **PNTR and Implied Volatility**

This table presents results for the following regression

$$ImpVol_{f,t} = \theta PostPNTR \times NTRGap_f + \delta_f + \delta_t + \varepsilon_{f,t}$$

Where $ImpVol_{f,t}$ is firm f 's implied volatility across all at the money ($.4 \leq |\Delta| \leq .5$) puts and call options on day t . The analysis time frame is 5/1/00 - 6/30/00 and the post-PNTR period begins in 5/25/00. Implied volatility data is from OptionMetrics All regressions include year and firm FEs. Columns (1) and (2) include all manufacturing firms with available data. Columns (3) and (4) include only manufacturing firms from the set of the 1,000 largest firms (by market cap) in the U.S. Standard errors clustered by industry are included in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

Expiration	All Firms		Large Firms	
	(1) 30 Days	(2) 60 Days	(3) 30 Days	(4) 60 Days
$PostPNTR_t \times NTRGap_f$	-0.101*** (0.035)	-0.084*** (0.030)	-0.097*** (0.029)	-0.087*** (0.023)
N	34,751	34,751	12,394	12,394
adj. R^2	0.940	0.956	0.953	0.97

Table 3.1: PNTR, Firm Financing, and Employment

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

$$\ln(\text{Emp})_{f,t} = \theta_1 \text{PostPNTR}_t \times \text{NTRGap}_f \times \text{FC}_f + \theta_2 \text{PostPNTR}_t \times \text{NTRGap}_f \\ + \theta_3 \text{PostPNTR}_t \times \text{FC}_f + \beta_1' X_f \times \text{PostPNTR}_t \times \text{NTRGap}_f \\ + \beta_2' X_f \times \text{PostPNTR}_t + \delta_f + \delta_{i,t} + \varepsilon_{f,t}$$

Where $\ln(\text{Emp})$ 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
$\text{PostPNTR}_t \times \text{NTRGap}_f$	-0.781*** (0.243)	-0.058 (1.458)	-0.396 (1.416)	0.608 (1.539)
$\text{PostPNTR}_t \times \text{NTRGap}_f \times \text{FC}_f$		2.260** (0.878)	1.735** (0.858)	-4.490*** (1.451)
$\text{PostPNTR}_t \times \text{FC}_f$		-0.839*** (0.306)	-0.670** (0.298)	1.698*** (0.550)
$\text{PostPNTR}_t \times \text{NTRGap}_f \times \text{Size}_f$		-0.065 (0.212)	-0.003 (0.210)	-0.087 (0.212)
$\text{PostPNTR}_t \times \text{Size}_f$		-0.047 (0.071)	-0.069 (0.070)	-0.040 (0.071)
$\text{PostPNTR}_t \times \text{NTRGap}_f \times \text{Age}_f$		0.026 (0.054)	0.028 (0.053)	0.036 (0.052)
$\text{PostPNTR}_t \times \text{Age}_f$		-0.008 (0.017)	-0.008 (0.017)	-0.011 (0.017)
$\text{PostPNTR}_t \times \text{NTRGap}_f \times \Delta \text{Emp}$		-1.674 (2.570)	-1.551 (2.585)	-1.508 (2.484)
$\text{PostPNTR}_t \times \Delta \text{Emp}$		-2.973*** (0.863)	-2.938*** (0.869)	-2.931*** (0.839)
Econ Mag of θ_1		29%	22%	29%
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 \times Year FE		X	X	X

Table 3.2: **PNTR, Firm Financing, and Employment - Robustness**

This table presents several robustness test for the specification in (3.1). 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%.

	(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** (0.789)	1.732** (0.790)	2.212*** (0.777)	2.333** (0.914)
$PostPNTR_t \times FC_f$	-0.684*** (0.276)	-0.689** (0.276)	-0.792*** (0.262)	-0.904*** (0.333)
$PostPNTR_t \times NTRGap_f \times Size_f$	0.244* (0.137)	0.230* (0.137)		
$PostPNTR_t \times Size_f$	-0.105** (0.044)	-0.108** (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* (0.768)	1.438* (0.766)	1.577** (0.783)	1.607* (0.859)

Table 3.2 – continued

	(1) Size= Assets	(2) Size= Sales	(3) Gap= Primary	(4) Gap= NNTR
$PostPNTR_t \times FC_f$	-0.600** (0.272)	-0.607* (0.272)	-0.617** (0.266)	-0.656** (0.312)
$PostPNTR_t \times NTRGap_f \times Size_f$	0.287** (0.137)	0.273** (0.138)		
$PostPNTR_t \times Size_f$	-0.122*** (0.044)	-0.124*** (0.044)		

Panel C: Cash

$PostPNTR_t \times NTRGap_f$				0.325 (1.429)
$PostPNTR_t \times NTRGap_f \times FC_f$	-3.367** (1.330)	-3.319** (1.360)	-4.351*** (1.206)	-4.487*** (1.456)
$PostPNTR_t \times FC_f$	1.372*** (0.504)	1.329*** (0.513)	1.648*** (0.453)	1.749*** (0.570)
$PostPNTR_t \times NTRGap_f \times Size_f$	0.235* (0.137)	0.216 (0.140)		
$PostPNTR_t \times Size_f$	-0.104** (0.044)	-0.104** (0.045)		

Table 3.2 – continued

	(1)	(2)	(3)	(4)
	Size= Assets	Size= Sales	Gap= Primary	Gap= NNTR
<i>N</i>	11,500	11,500	11,500	11,500
adj. <i>R</i> ²	0.927	0.927	0.928	0.928
Firm FE	X	X	X	X
Ind × Year FE	X	X	X	X
SE Cluster	Firm	Firm	Ind.	Firm

Table 3.3: **PNTR, Firm Financing, and Employment - Plant Level**

This table presents plant-level estimates of regressions (3.3)-(3.5). 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 (3.3), panel B reports results from estimating (3.4) and (3.5). 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)	(2)	(3)	(4)
	Diff-in-Diff	FC=NBL	FC=NML	FC=Cash
$PostPNTR_t \times NTRGap_f$	-0.351*** (0.081)			
$PostPNTR_t \times NTRGap_f \times FC_f$		2.397** (0.986)	2.523** (1.251)	-8.478*** (3.288)
$PostPNTR_t \times FC_f$		-0.825*** (0.305)	-0.877** (0.429)	2.644** (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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Diff-in-Diff	FC=NBL	FC=NML	FC=Cash	Diff-in-Diff	FC=NBL	FC=NML	FC=Cash
$NTRGap_f$	0.390*** (0.149)				-0.996*** (0.307)			
$NTRGap_f \times FC_f$		-0.872 (0.818)	-0.772 (1.007)	5.433*** (2.087)		3.608** (1.418)	3.244* (1.938)	-13.710*** (3.948)
FC_f		0.556* (0.283)	0.516 (0.391)	-1.885*** (0.714)		-1.738*** (0.478)	-1.594** (0.738)	4.912*** (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 3.4: PNTR, Firm Financing, and Employment - Alternative Explanations

This table addresses various alternative explanations by re-estimating variants of (3.1) 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 Appendix B Standard errors clustered by firm are in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

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

Table 3.5: **PNTR, Firm Financing, and Employment - Cross Sectional Test**

This table results for estimating (3.1) 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 (3.1) 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 (3.1) 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 3.6: **PNTR & Issuance Activity**

This table presents results for the following regression:

$$y_{f,i,t} = \theta_1 \text{PostPNTR}_t \times \text{NTRGap}_f \times \text{FC}_f + \theta_2 \text{PostPNTR}_t \times \text{FC}_f \\ + \beta'_1 X_f \times \text{PostPNTR}_t \times \text{NTRGap}_f + \beta'_2 X_f \times \text{PostPNTR}_t \\ + \delta_f + \delta_{i,t} + \varepsilon_{f,t}$$

Where f indexes the unit of observation, i indexes industry, and t indexes year. $y_{f,i,t}$ is the % increase in total debt outstanding (Debt Issuances - Panel A) or total net proceeds from equity issuances scaled by total assets (Equity Issuances - Panel B). The unit of observation is firm-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, 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%.

Panel A: Debt Issuance			
	(1)	(2)	(3)
	NBL	NML	Cash
$\text{PostPNTR}_t \times \text{NTRGap}_f$	2.838** (1.317)	2.791** (1.340)	1.846 (1.315)
$\text{PostPNTR}_t \times \text{NTRGap}_f \times \text{FC}_f$	-1.976* (1.070)	-1.600* (0.967)	5.008** (2.160)
$\text{PostPNTR}_t \times \text{FC}_f$	0.312 (0.417)	0.048 (0.420)	-1.463* (0.819)
N	19,312	19,312	19,312
adj. R^2	0.849	0.849	0.849
Controls	X	X	X
Year x Ind FE	X	X	X
Firm FE	X	X	X

Panel B: Equity Issuance			
	(1)	(2)	(3)
	NBL	NML	Cash
$\text{PostPNTR}_t \times \text{NTRGap}_f$	-0.107 (1.089)	0.049 (1.106)	0.477 (1.249)
$\text{PostPNTR}_t \times \text{NTRGap}_f \times \text{FC}_f$	1.954* (1.147)	1.310** (0.615)	-2.014 (2.761)
$\text{PostPNTR}_t \times \text{FC}_f$	-0.418 (0.395)	-0.519 (0.707)	0.554 (0.942)
N	19,312	19,312	19,312
adj. R^2	0.231	0.231	0.231
Controls	X	X	X
Year x Ind FE	X	X	X
Firm FE	X	X	X

Table 3.7: **PNTR, Firm Financing, and Reported Offshoring**

This table results for estimating (3.7), that is the likelihood of firms to report offshoring to either China (column (1)) or the rest of the world (ROW - column (2)). Data on reported offshoring comes from Hoberg and Moon (2017) and Hoberg and Moon (2018). The coefficient in column (3) is estimated by interacting the θ_1 coefficient in (3.7) with the China indicator. Standard errors clustered by firm are in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

	(1) China Offshoring	(2) ROW Offshoring	(3) China - ROW
Diff-in-Diff	0.132* (0.075)	-0.028 (0.043)	0.160* (0.087)
θ_1 NBL	-0.981* (0.578)	0.357 (0.318)	-1.338** (0.659)
θ_1 NML	-1.456** (0.638)	0.348 (0.359)	-1.804** (0.731)
θ_1 Cash	0.073 (0.601)	-0.719 (0.437)	0.793 (0.743)
<i>N</i>	14,811	14,811	

Table 3.8: 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 are included in parentheses. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

Panel A: Industry Segment Sales				
	(1)	(2)	(3)	(4)
	DiD	NBL	NML	Cash
$PostPNTR_t \times NTRGap_f$	0.103 (0.274)			
$PostPNTR_t \times NTRGap_f \times FC_f$		-0.792** (0.372)	0.195 (0.402)	0.461 (1.067)
$PostPNTR_t \times FC_f$		0.060 (0.139)	-0.080 (0.159)	0.285 (0.390)
N	26,639	26,639	26,639	26,639
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)	(2)	(3)	(4)
	DiD	NBL	NML	Cash
$PostPNTR_t \times NTRGap_f$	0.034 (0.258)			
$PostPNTR_t \times NTRGap_f \times FC_f$		-0.613 (0.666)	0.548 (0.377)	1.467 (1.410)
$PostPNTR_t \times FC_f$		0.008 (0.235)	-0.219 (0.171)	-0.200 (0.488)
N	12,451	12,451	12,451	12,451
adj. R^2	0.932	0.939	0.939	0.939
Controls		X	X	X
Year FE	X			
Year x Ind FE		X	X	X
Segment FE	X	X	X	X

Table 3.9: **PNTR, Firm Financing, and Production**

This table presents the results for the following regression

$$y_{f,i,t} = \theta_1 PostPNTR_t \times NTRGap_i \times FC_f + \theta_2 PostPNTR_t \times NTRGap_i \\ + \theta_3 PostPNTR_t \times FC_f + \beta'_1 X_f \times PostPNTR_t \times NTRGap_i \\ + \beta'_2 X_f \times PostPNTR_t + \delta_{f,i} + \delta_{f,p} + \varepsilon_{f,t}$$

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	1.518 (1.368)	3.366** (1.486)	-17.160** (7.399)
N	3,800	3,800	3,800
Panel B: Labor Productivity			
θ_1	-1.110** (0.521)	-1.535*** (0.590)	2.339 (1.613)
N	15,500	15,500	15,500
Panel C: Automation			
θ_1	-0.177 (1.153)	0.591 (1.415)	0.780 (3.015)
N	15,500	15,500	15,500
Controls	X	X	X
Firm \times Ind FE	X	X	X
Ind \times Year FE	X	X	X

Table 3.10: **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*** (0.124)	0.479*** (0.077)	-0.512 (0.435)	-0.512 (0.589)
$1\{c = Per\} \times PostPNTR_t \times NTRGap_p$				0.991* (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”
<i>Res</i>	0.133*** (0.018)	0.309*** (0.016)
<i>Per - Res</i>	-0.037** (0.018)	-0.003 (0.017)
N	2,445	2,445

Table 3.11: **China FDI Guidelines: Examples of Restricted and Encouraged Projects**

Examples of Encouraged Projects	
<i>Textiles</i>	Production of special textile products for engineering applications Dyeing and finishing of high grade textile materials
<i>Chemical Fiber Manufacturing</i>	Production of high tech chemical fibers Manufacturing of synthetic rubber
Examples of Restricted Projects	
<i>Textiles</i>	Wool and cotton textile. Silk
<i>Chemical Fiber Manufacturing</i>	Production of conventional textile chemical fiber drawing
<i>Rubber Products Manufacturing</i>	Production of lower performance industrial rubber components

Figure 2.1: **PNTR and Implied Volatility**

This figure graphs the θ_k coefficients from the following regression:

$$ImpVol_{f,t} = \sum_{k=5/1/00}^{6/30/00} \left[\theta_k 1\{t = k\} \times NTRGap_f \right] + \delta_f + \delta_t + \varepsilon_{f,t}$$

Where $ImpVol_{f,t}$ is the daily implied volatility firm f 's at the money ($.4 \leq |\Delta| \leq .5$) put & call options with 30 day expiration from time t . The red line indicates 5/25/00, the event date for the PNTR vote. The analysis time frame is 5/1/00 - 6/30/00 and the X-axis is displayed in event time (5/25/00=0). Implied volatility data is from OptionMetrics. The solid lines represent the point estimates and the dashed lines are 95% confidence intervals based on standard errors clustered by industry.

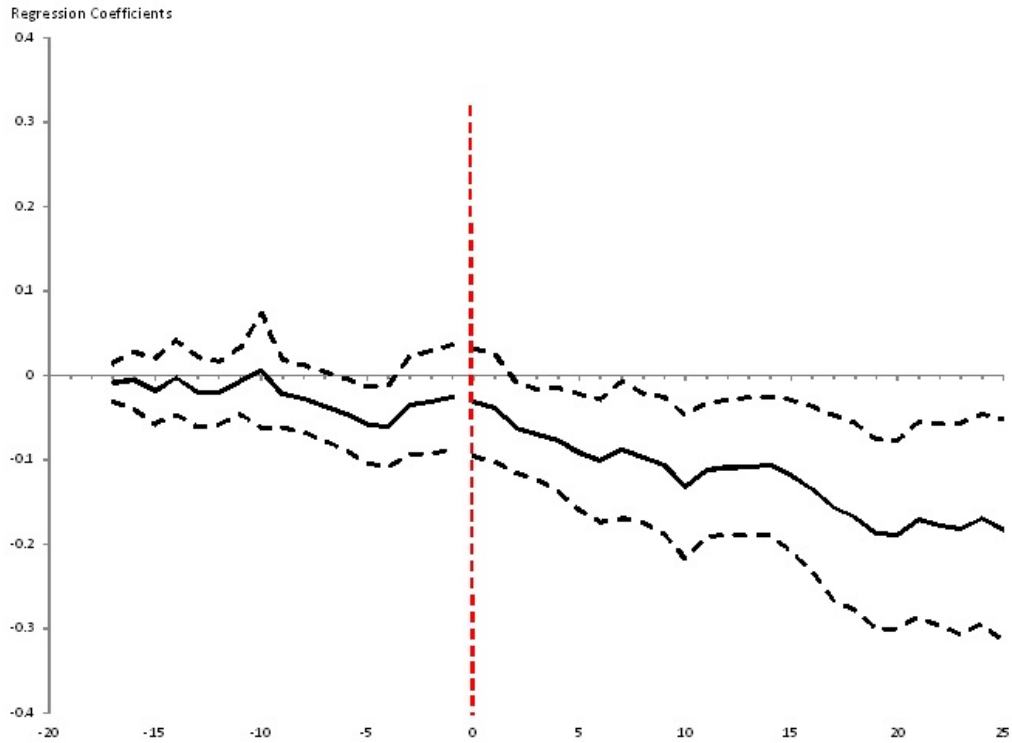
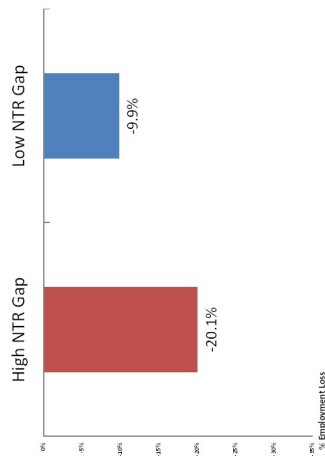


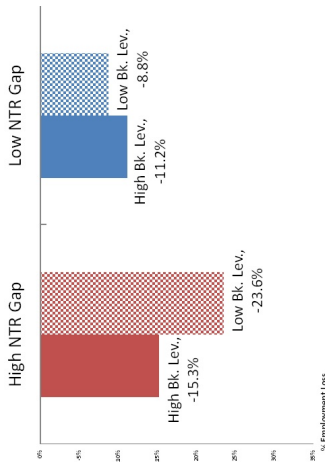
Figure 3.1: PNTR, Firm Financing, and Employment - Graphical Depiction

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 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

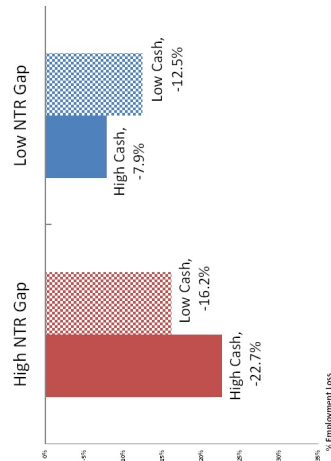


Figure 3.2: PNTR, Firm Financing, and Employment - Dynamics

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

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

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 (3.1).

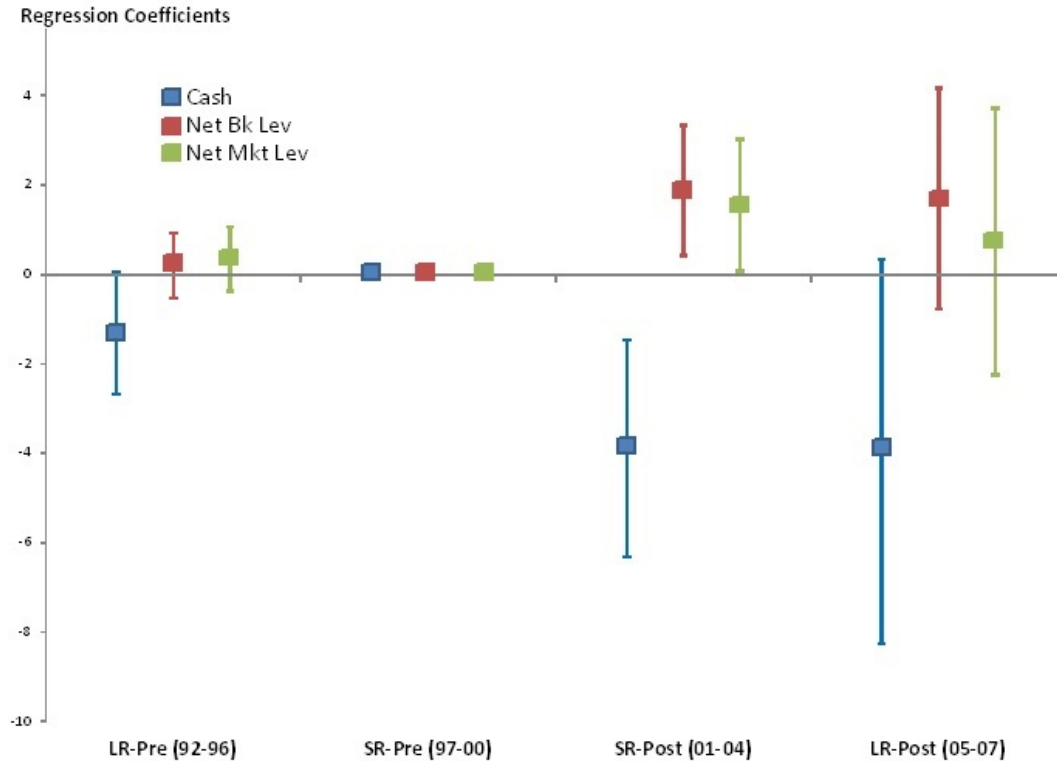


Figure 3.3: **PNTR and Materials Cost**

This figure graphs the θ_k coefficients from the following regression:

$$Matcost_{i,t} = \sum_{k=1993}^{2007} \left[\theta_k 1\{t = k\} \times UpstreamNTRGap_i \right] + \delta_i + \delta_t + \varepsilon_{i,t}$$

Where i indexes industry and t indexes year. The solid lines represent the point estimates (relative to 1992) and the dashed lines are 95% confidence intervals based on standard errors clustered by industry. Data are from the NBER-CES dataset.



Figure 3.4: **PNTR and Domestic Productivity**

This figure graphs the θ_k coefficients from the following regression:

$$LaborProd_{i,t} = \sum_{k=1993}^{2007} \left[\theta_k 1\{t = k\} \times NTRGap_i \right] + \delta_i + \delta_t + \varepsilon_{i,t}$$

Where i indexes industry and t indexes year. The solid lines represent the point estimates (relative to 1992) and the dashed lines are 95% confidence intervals based on standard errors clustered by industry. The regression is weighted by industry employment in 1992. Data are from the NBER-CES dataset.

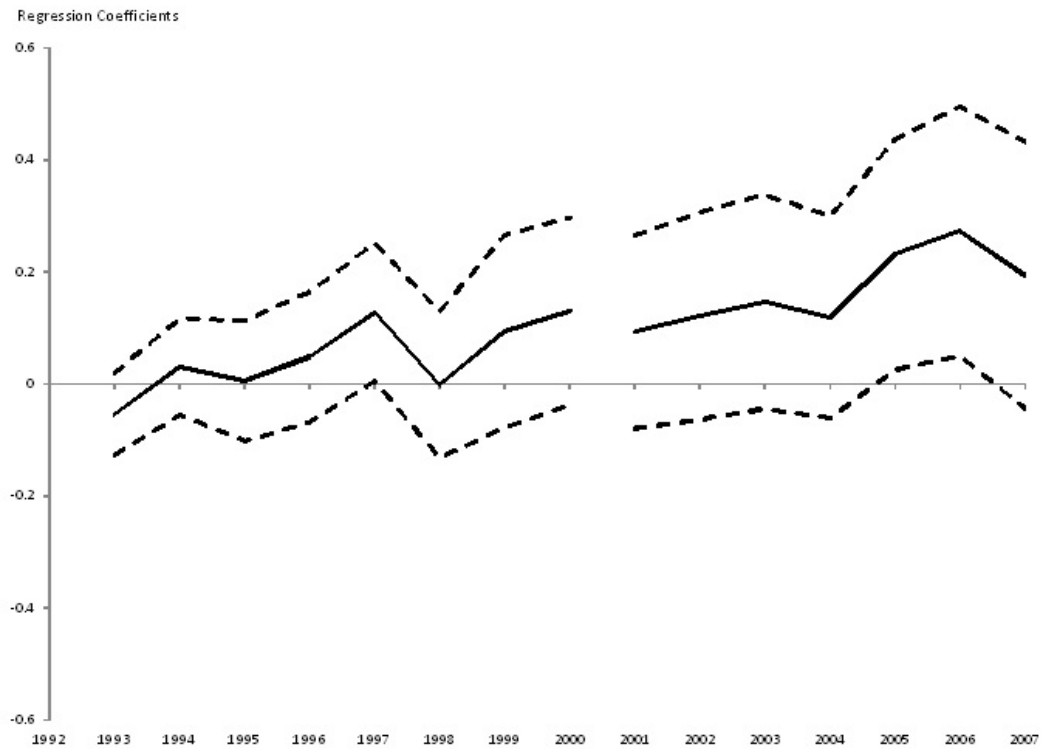


Figure 3.5: **China FDI**

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.

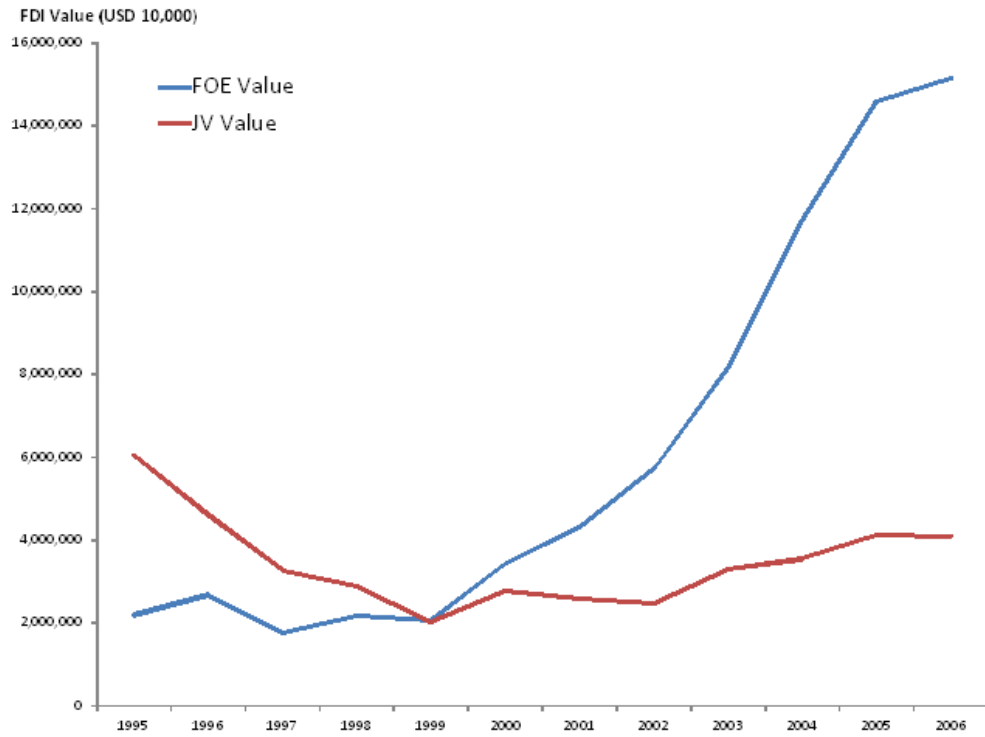


Figure 3.6: **China Related Party Imports**

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.

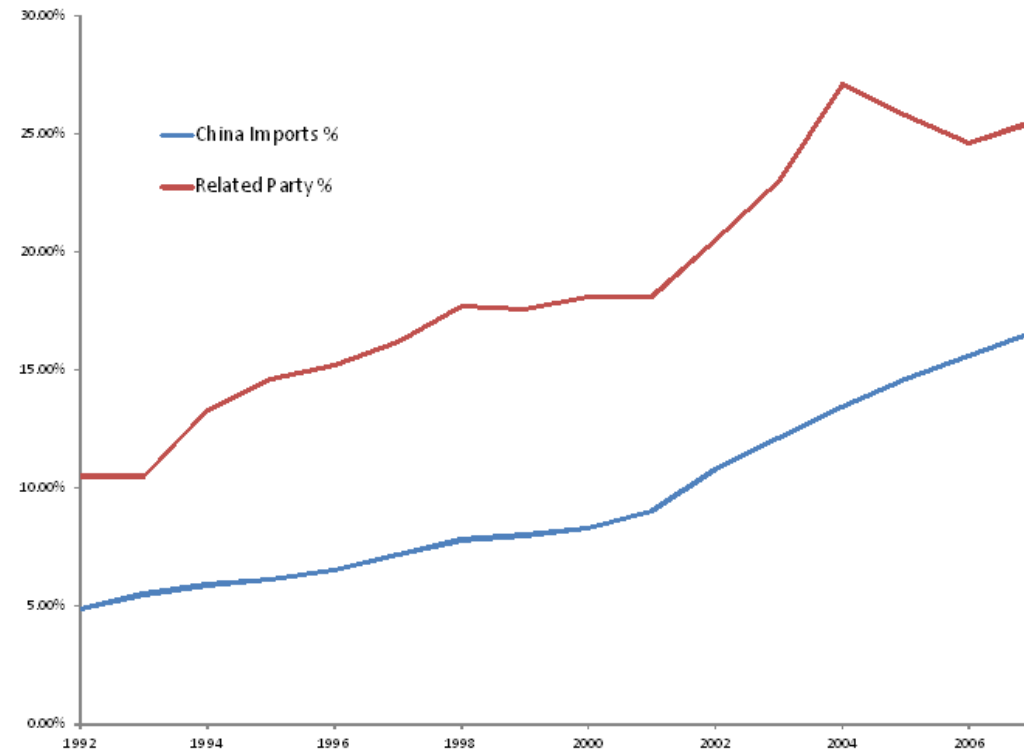
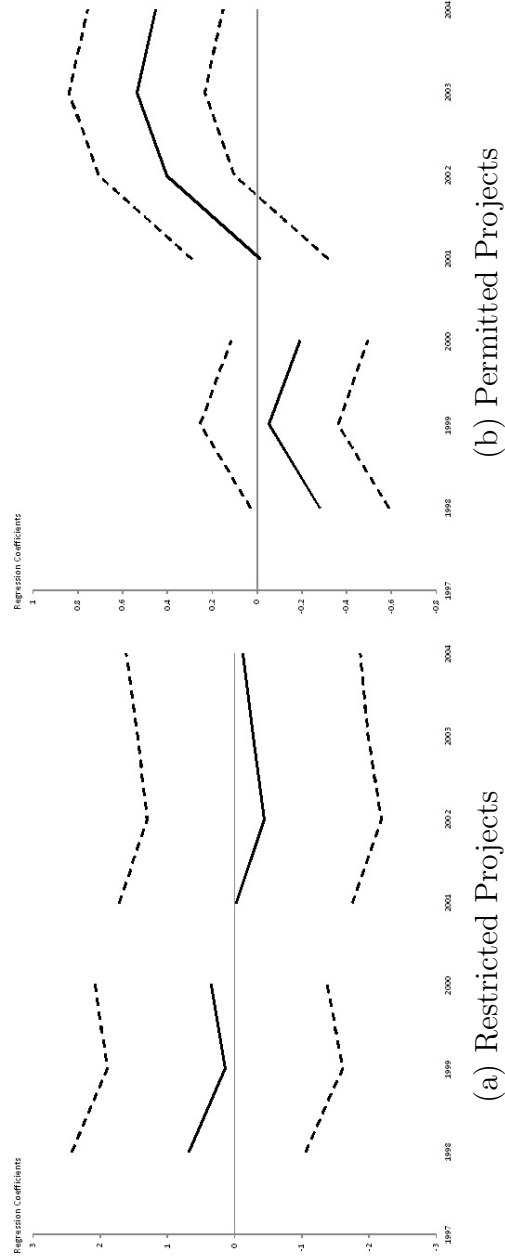


Figure 3.7: **FDI and Import Growth**

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.



Appendices

Appendix A

TAA Petitions

I obtain information on offshoring-induced layoffs from the petitions filed with the Trade Adjustment Assistance (TAA) program. The TAA is a Department of Labor (DOL) managed initiative created by the Trade Act of 1974. Under the program's mandate, workers who suffer trade-induced layoffs are eligible for benefits including job training, extended unemployment insurance (UI), and relocation and job search allowances. Petitions for these benefits are made at the establishment level and can be filed by workers, unions, their (previous) employer, or a state agency. The TAA data represent perhaps the most comprehensive source of explicit firm-level offshoring activities, but since parties are under no obligation to file TAA petitions, they likely underestimate offshoring activities.¹

Each petition is assigned a DOL investigator in charge of certifying that the layoff was indeed trade related. The investigator makes the determination after conducting interviews with employees, customers, and upstream/downstream plants, so that the certification is likely precise. All petitions include certification status, the date of the event, and the estimated

¹Per Autor et al. (2013), per-capita spending on the TAA program in 2007 amounted to just \$2.

number of affected workers. Petitions filed after the 2002 Trade Act also include the reason for the lay off and the countries to which production or customer demand has switched.

The data are made public by Public Citizen, a non-profit consumer advocacy group which receives monthly updates of TAA petitions from the DOL and compiles them into a raw dataset.² Following Monarch et al. (2017), I define all layoffs categorized as either *company imports* or *shift in production as offshoring*. This approach leads to just under 11,000 petitions totaling an estimated 1.1 million workers laid off between 1994 and 2016. Mapping the TAA to firm financial data is difficult since the raw company names from the petitions are ill-suited for algorithmic matching. For example, Fortune Brands is a holding company that owns a variety of brands such as Masterlock locks, Moen faucets, and Jim Beam bourbon whiskey and the petitions often reference the product name rather than the holding company. A further complication is that Fortune split into two companies in 2011 and the legacy financial data is listed in Compustat under Beam, Inc. In order to manage this complicated matching process without discarding too much data, I limit the TAA data to petition names that have at least one year where the estimated number of affected workers is greater than or equal to 100. This filter reduces the amount of unique petition company names to 2,651 (roughly 6,000 petitions), and these petitions account for an estimated 1 million affected workers. I map the petitions to Compustat identifiers using a combination of algorithmic and

²The data were downloaded from <https://tinyurl.com/ycc7z7jx> accessed 11/21/17.

manual matching. I am able to find Compustat matches for almost 5,000 of the petitions covering an estimated 700,000 affected workers. Consistent with the matching strategy, I define firms as offshorers in a given year if they lay off at least 100 workers in that year. Of the 2,510 active manufacturing firms in 1999, 294 are classified as offshorers between 2003-2016. Of those 294 firms, 80 list China as one of the offshoring destinations.

Table A.1 presents summary statistics for the offshoring and non-offshoring manufacturers in the Compustat universe. “Raw differences” (columns (1)-(3)) between offshorers and non-offshores are derived from t-tests of sample means. Differences adjusting for industry fixed effects (columns (4) and (5)) are estimated from a regression of each relevant variable on an offshoring dummy and industry fixed effects. In line with the extant literature, offshoring firms tend to be larger and more profitable. These differences exist across the raw and industry-adjusted measures. This “offshoring premium,” even within the sample of relatively larger and public firms, is congruent with the fixed-cost-induced selection mechanism promoted in Melitz-style models. Consistent with Monarch et al. (2017), offshorers are also more capital intense, hold a larger proportion of fixed assets, and have lower market-to-book ratios. 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.

To the extent that data on offshoring to China exists, one should also

expect the incidence of offshoring to positively correlate with exposure to PNTR. To test this hypothesis, I analyze whether Chinese offshoring propensity, as reported in the TAA data, is higher in industries with larger NTR Gaps. Since the TAA petitions prior to the 2002 Trade Act do not provide lay-off reasons or the responsible countries, I cannot conduct a traditional difference in difference test. However, I can examine whether NTR is more strongly associated with offshoring to China than to other countries. If the uncertainty reduction associated with PNTR indeed improved offshoring incentives, then we should observe a positive association between the NTR Gap and offshoring propensity to China. Furthermore, since PNTR was a China-specific policy, the NTR Gap should not be positively associated with increased offshoring to other countries. To test this claim, I use the TAA-Compustat matched sample to estimate the following regression

$$\mathbb{1}\{Offshore\}_f^x = \alpha + \theta_1 NTRGap_f + \varepsilon_f \quad (\text{A.1})$$

Where $\mathbb{1}\{Offshore\}_f^x$ is an indicator that firm f had at least one offshoring event to country x from 2003-2016. The petitions mostly mention multiple destination countries and so Chinese offshorers ($x = China$) are those who list China as one of the destination countries. Non-Chinese offshorers ($x = Ex - China$) are those who do not list China as a destination country. Firm-level $NTRGap_f$ is calculated as the weighted average of industry NTR Gaps across all of the firm's operating segments, with segment sales as the weights. I estimate (A.1) for each $x \in \{China, Ex - China\}$ jointly within a SUR system to allow for comparison of coefficients across specifications. The

sample includes all manufacturing firms with Compustat employment data in 1999. Table A.2 displays the results of the analysis. The NTR Gap is positively, yet statistically insignificantly, related with offshoring to China (column (1)). However, it is significantly negatively associated with offshoring to other countries (column (2)), which suggests firms shifted focus to Chinese offshoring. A χ^2 test strongly rejects equality of θ_1 across the two specifications.

Monarch et al. (2017) argue that the reduction in uncertainty should matter more for the largest employers, as they are the most likely to be able to absorb the fixed costs of offshoring. To capture this differential response, I estimate the following regression

$$\mathbb{1}\{Offshore\}_f^x = \theta_1 NTRGap_f \times \ln(Emp)_f + \theta_2 NTRGap_f + \theta_3 \ln(Emp)_f + \delta_i + \varepsilon_f \quad (\text{A.2})$$

$\ln(Emp)$ is measured as of 1999. I estimate (A.2) both without (columns (3) and (4)) and with (columns (5) and (6)) industry fixed effects. The coefficient of interest is again θ_1 which now measures whether larger firms are more likely to be offshorers in higher NTR Gap industries. Consistent with the hypothesis that PNTR is a uniquely Chinese offshoring shock, θ_1 is positive and significant when $x = China$ (columns (3) and (5)) and negative when $x = Ex - China$ (columns (4) and (6)). Equality of θ_1 is again strongly rejected. It should be noted that this analysis is subject to several caveats. The first is that offshoring activity is self-reported. Additionally, since the data starts in 2003, I cannot test whether PNTR leads to a shift in offshoring propensity from the pre-period. While this evidence should be taken with a grain of salt it

nonetheless comports with the anecdotes referenced above.

Table A.1: Offshoring Premium

This table presents firm-level summary stats for the sample of offshoring v. non-offshoring manufacturing firms. I define firms as offshorers if, per the TAA petition data, they lay off at least 100 workers in any year from 2003-2016. Financial variables are measured in 1999, the year prior to the PNTR vote. Variable definitions are included in the appendix. The sample includes all Compustat manufacturers with employment data in 1999. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

Variable	Raw Difference			Industry FE	
	(1) Offshoring	(2) Non- Offshoring	(3) P-Val Diff	(4) Offshoring Diff	(5) P-Val Diff
$\ln(asset)$	7.057	3.974	***	2.695	***
$\ln(emp)$	1.943	-1.240	***	2.675	***
age	25.517	12.884	***	9.879	***
Mkt_Cap	8.347	0.947	***	8.681	***
$\max\{ROA, 0\}$	0.151	0.090	***	0.050	***
$\max\{NPM, 0\}$	0.146	0.084	***	0.055	***
TFP	-0.214	-0.398	***	0.170	***
$Tangibility$	0.283	0.228	***	0.020	**
$\ln(CapitalIntense)$	3.710	3.382	***	0.247	***
$M2B$	2.159	4.411	***	-1.286	
$NetBkLev$	0.230	0.145		0.002	
$NetMktLev$	0.171	0.072	***	0.028	*
$\frac{Cash}{Assets}$	0.082	0.216	***	-0.065	***
N	294	2,216			

Table A.2: NTR Gap and Offshoring Propensity

This table presents results for the following regressions:

$$\mathbb{1}\{Offshore\}_f^x = \alpha + \theta_1 NTRGap_f + \varepsilon_f$$

$$\mathbb{1}\{Offshore\}_f^x = \theta_1 NTRGap_f \times \ln(Emp)_f + \theta_2 NTRGap_f + \theta_3 \ln(Emp)_f + \delta_i + \varepsilon_f$$

Where $\mathbb{1}\{Offshore\}_f^x$ is an indicator that firm f had at least one offshoring event to country x from 2003-2016. δ_i is an industry fixed effect. The sample includes all manufacturing firms with Compustat employment data in 1999. $\ln(Emp)$ is measured as of 1999. The regressions for $x \in \{China, Ex-China\}$ are estimated jointly in a SUR system to allow comparison of coefficients across the specifications. The last row contains a χ^2 test of $\theta_1^{China} = \theta_1^{Ex-China}$. Standard errors are included in parenthesis. *** indicates significance at 1% level, ** indicates 5%, and * indicates 10%.

$x =$	(1) <i>China</i>	(2) <i>Ex - China</i>	(3) <i>China</i>	(4) <i>Ex - China</i>	(5) <i>China</i>	(6) <i>Ex - China</i>
$NTRGap_f$	0.021 (0.033)	-0.107** (0.049)	0.107*** (0.033)	0.060 (0.046)	0.164 (0.145)	0.351* (0.200)
$\ln(Emp)_f$			0.011** (0.005)	0.062*** (0.007)	0.011* (0.006)	0.068*** (0.008)
$NTRGap_f \times \ln(Emp)_f$			0.030** (0.014)	-0.030 (0.020)	0.027* (0.016)	-0.041* (0.022)
$\theta_1^{China} =$ $\theta_1^{Ex-China} (\chi^2)$		6.77***		7.80***		8.09***
N	2,510	2,510	2,510	2,510	2,510	2,510
N Offshore	80	183	80	183	80	183
Industry FE					X	X

Appendix B

Data Construction

B.1 Construction of Materials Costs

The quinquennial CMF includes surveys on productions materials purchased by each establishment. Each (reported) purchased input is assigned an industry code - 4-digit SIC in earlier vintages of the CMF and 6-digit NAICS in latter vintages - as well trailing digits to identify individual products. The CMF also contains the total cost of said input and to a lesser degree, the quantity purchased. To generate a dataset of firm-level input cost indices I proceed in the following way. First I drop all inputs for which a quantity is unavailable. Next I aggregate all costs and quantities at the firm-industry level. I then map each input industry to its constant manufacturing family based on concordances in Pierce and Schott (2016). Finally, I construct the firm's input cost index for a given industry by dividing aggregated costs by aggregate quantities.

One complication in this process is certain products do not map to official SIC or NAICS industries. These instances often occur when establishments self-report production inputs that are not on the set of pre-specified products in the Census survey form. I allocate these products to their respec-

tive industries by iteratively matching on coarser (e.g. 3-digit SIC or 5-digit NAICS) codes using the available inputs as weights.

Table B.1: **Variable Construction**

Compustat Variables: Compustat variable names in italics.

Variable	Definition	Note
Net Book Leverage	$\frac{dltt+dlc-che}{at}$	
Net Market Leverage	$\frac{dltt+dlc-che}{at+prcc.f*csho-ceq}$	
Cash to Assets	$\frac{che}{at}$	
$\ln(emp)$	$\ln(emp)$	
$\ln(sale)$	$\ln(sale)$	Annual segment sales, replaced with firm sales for companies not in the Compustat segment files. Values are deflated by industry price indices from Becker et al. (2013).
ROA	$\frac{oibdp}{at}$	
NPM	$\frac{oibdp}{sale}$	
Tangibility	$\frac{ppent}{at}$	
$\ln(CapitalIntense)$	$\ln(\frac{ppent}{emp})$	
Market-to-Book	$\frac{prcc.f*csho+at-ceq}{at}$	
MNC Flag	Flag $\in \{0, 1\}$	= 1 if the company reports multiple geographic segments.
Foreign Income Flag	Flag $\in \{0, 1\}$	= 1 if the company reports $pi fo > 0$ in any pre-PNTR year.
<i>INVEST</i>	<i>capx</i>	
<i>OCF</i>	$f opt + \Delta invt + recch + apalch$	Δ indicates 1-year difference. When missing, <i>f opt</i> is replaced with $ibc + dpc + txdc + esubs + sppiv + fopo$.
<i>TFP</i>		Data from İmrohoroğlu and Tüzel (2014)

Census Variables

Variable	Definition	Note
$\ln(emp)$	$\ln(E)$	Employment data E are the March 12 headcount snapshots provided in the LBD.
TFP		Log of deflated revenue minus the log of inputs, weighted by the average cost share (β_i) for each input i across industries. Data are from CMF. Normalized by industry to $\mu = 0, \sigma = 1$.
$LaborProd$	$\frac{VA}{E}$	Value-add VA and employment E data are obtained from the CMF. VA is deflated by industry price indices from Becker et al. (2013). Normalized by industry to $\mu = 0, \sigma = 1$.
$\ln(CapitalIntense)$	$\ln(K/E)$	Capital K and employment E data are obtained from the CMF.
$MatCost$	$\ln(Q/P)$	Material quantity Q and cost P data are obtained from the CMF material trailer files. Cost is deflated by materials cost indices from Becker et al. (2013).
$MachCapex$	$\ln(M)$	Machinery expenditures M are obtained from the CMF. Values are deflated by investment cost indices from Becker et al. (2013).

Other Variables

Variable	Note
$AR[Day\ Of]$	Stock return net of CRSP value-weighted index.
$CAR[-1,10]$	Stock return net of CRSP value-weighted index - summed over $t - 1$ to $t + 10$.
$\ln(Val)$	Customs value of imports obtained from U.S. Census Bureau.

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