Effects of U.S. Export Controls on Chinese Firms*

Mark Albertson[†] Jacob Howard[‡] Miguel Sarzosa[§]

January 15, 2026

Abstract

The United States has increasingly relied on export controls as a central instrument of economic statecraft, particularly in its efforts to restrict diffusion of key technologies to firms in the People's Republic of China (PRC). Despite the rapidly expanding use of export controls against the PRC, little empirical evidence exists on how these policies affect the financial performance of the foreign firms that become subject to them. This paper utilizes Moody's Orbis database to construct the largest possible sample of both treated and control firms and applies modern difference-in-difference and event-study estimators to identify the causal effects of export-controls. Our results indicate that adding firms to the BIS Entity List causes significant declines in key measures of financial health such as revenue, employment, and assets but does not significantly impact indicators such as solvency ratio that may predict a firm's survival. These findings suggest that U.S. export controls reduce the size of listed PRC firms but, on average, do not force them out of business.

JEL Classification: F51, F14, F61, H56, L25

*We thank Nina Palmer, Graham Newell, Dan Brown, Chris Niessen, Roz Engel, Kyle Norris, Allison Sherlock and participants in seminars at MITRE's Economic Research Steering Group (McLean VA, 2025) for useful advice throughout the research process. This paper was supported by the MITRE Independent Research and Development Program.

[†]MITRE. Email: malbertson@mitre.org

[‡]MITRE. Email: jhoward@mitre.org

[§]MITRE. Email: msarzosa@mitre.org.

1 Introduction

Export controls have become a central instrument of U.S. economic statecraft, particularly in the context of strategic competition with the People's Republic of China (PRC) (Liu et al., 2024; Angelo, 2025). Over the past decade, the U.S. Department of Commerce's Bureau of Industry and Security (BIS) has expanded its use of technology-focused export restrictions to curb the transfer of sensitive U.S.-origin goods, software, and intellectual property to foreign firms (Pagano, 2023; Fergusson et al., 2021). These measures play a growing role in shaping global supply chains, access to advanced technologies, and the competitive landscape of advanced industries. Yet despite the growing prominence of export controls among policymakers, empirical evidence on their firm-level consequences remains limited. This paper aims to quantify the impact of export controls on a variety of financial outcomes for PRC firms and therefore further inform policymakers regarding their continued use.

The BIS Entity List is one of the more widely used and economically significant of these tools (Kilcrease and Frazer, 2023). Firms added to the Entity List face heightened licensing requirements for virtually all exports, reexports, and in-country transfers of U.S.-origin technology. Entity-List licensing requirements generally act as a limiting variable on international sourcing activities for listed entities, by introducing bureaucratic hurdles and potentially a prohibition on trade to named end users and users of certain named technologies. Because the United States leads in design, R&D, and intellectual property, and because the U.S. and Chinese economies became deeply integrated in the latter half of the twentieth century, placing Chinese firms on the Entity List has the potential to significantly affect Chinese companies in high-tech sectors.[1] Additionally, the BIS Entity List is the most prevalent economic statecraft tool that has been wielded against PRC firms with over 1,300 firms added since 2013.[2] High-profile desig-

[1]Khan et al. (2021), for example, find that in 2018 U.S. semiconductor firms accounted for more than half of global semiconductor R&D spending. They also find that U.S. firms devoted the highest share of semiconductor sales to R&D, nearly twice the share spent by Chinese companies.

[2]The Specially Designated Nationals and Blocked Persons list (SDN list) has been used at a similar

Figure 1: Firms Added to the BIS List by Year

Note: Data from International Trade Administration's Consolidated Screening List (CSL).

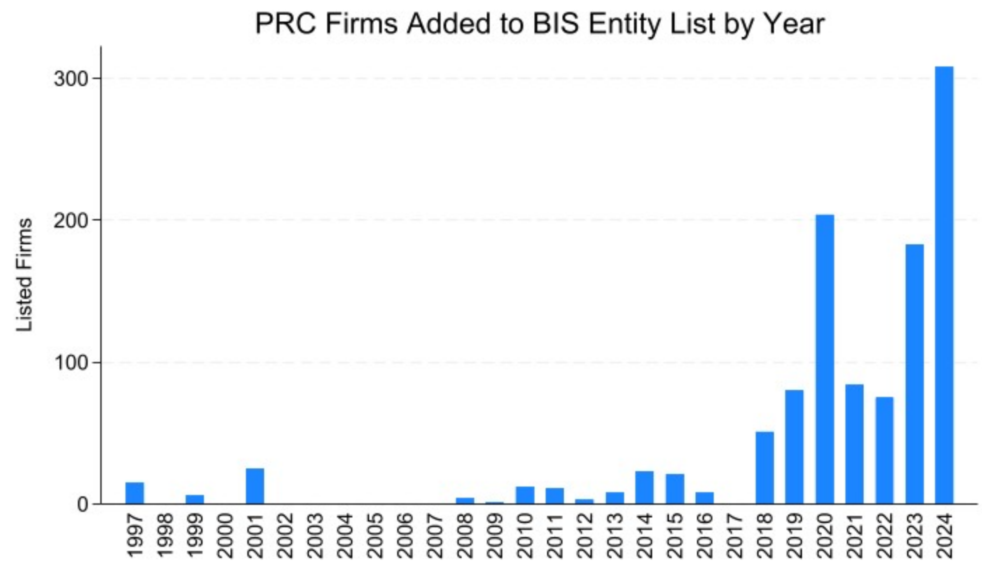
nations—including Huawei, ZTE affiliates, and Semiconductor Manufacturing International Corporation (SMIC)—reflect U.S. concerns over national security, military-civil fusion, and the diffusion of advanced dual-use technologies within the PRC. Figures 1 and 2 illustrate the increasing use of the Entity List both in general and against firms within the PRC.

Despite their extensive use, the economic effects of Entity List designations on Chinese firms are not well understood. On the one hand, naming a firm to the Entity List could materially constrain a firm's operations by raising input costs, disrupting supplier relationships, or preventing the acquisition of cutting-edge manufacturing equipment. Such constraints could translate into observable declines in revenue, profitability, asset growth, or investment. On the other hand, evasion tactics and the PRC's industrial policy ecosystem –including state-backed financing, local government subsidies, and rapidly expanding domestic production of advanced technology– may both reduce or

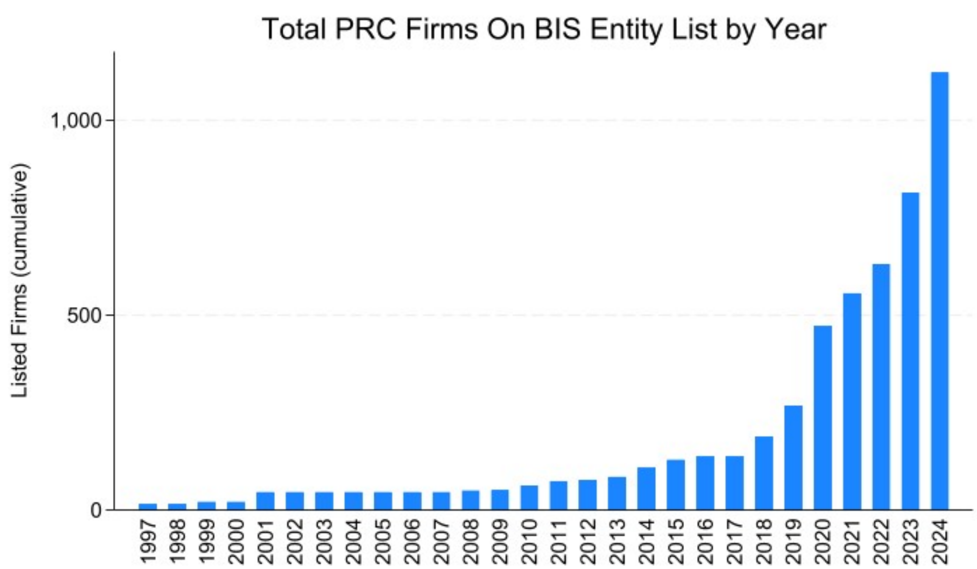
level over the same time frame but the Entity List has been used more extensively in China (Kilcrease and Frazer, 2023).

Figure 2: BIS Entity Listings Of PRC Firms Over Time

(a) PRC Firms Added by Year



(b) Total PRC Firms by Year



Note: Data from International Trade Administration's Consolidated Screening List (CSL).

delay the impact of the Entity List on Chinese firms. Additionally, PRC firms may endogenously respond to export-control shocks by accelerating localization, redesigning supply chains, or shifting to non-U.S. technologies, therefore insulating themselves from future U.S. actions. These competing narratives highlight the need for rigorous empirical evidence on the magnitude and persistence of Entity List impacts at the firm level.

Quantifying these effects is challenging for three main reasons. First, export controls are not randomly assigned: firms added to the Entity List often operate in strategic industries or face pre-existing geopolitical scrutiny. Second, Chinese firms vary widely in transparency, ownership structure, and access to state support, complicating efforts to isolate causal impacts. Third, the consequences of export controls may unfold gradually as firms exhaust inventories, lose foreign suppliers, or undertake costly technological substitution. This paper aims to address each of these and provide the first empirical evaluation of the impact of Entity List designation on financial health for firms in the PRC.[3]

The data used in this study consists of detailed financial information for the largest possible sample of both public and private Chinese firms and covers the period from 2012 to 2024. For each firm, we observe an unbalanced panel of indicators and focus our results on 10 core measures of financial health. We merge firm financial data with information on BIS Entity Listing available directly from the BIS to identify treated firms. We implement two parallel complementary empirical strategies to identify the effect of treatment. The first utilizes modern difference in difference methodologies for staggered treatment from Callaway and Sant'Anna (2021). The second, uses a permutation-based approach as in Heckman et al. (2010) to better estimate effects in small samples of treated firms. The latter allows U.S. to capture the effect of treatment

[3]This study is intended to inform evidence-based implementation of Export Administration Regulations (EAR) by quantifying the firm-level effects of Entity List designations in China that are relevant to BIS's national security objectives. The analysis is positive rather than normative and does not evaluate or comment on specific listing decisions.

on small subsets of our data such as by industry or firm size and the former provides accurate measures of the broad average treatment effect (ATE).

Our results indicate that adding a firm to the Entity List has a significant impact on core measures of financial health, and that most of these effects are persistent over time. We find particularly strong impacts on revenue (58%), employment (33%), and total assets (36%). Our findings are robust to model specification and hold for a sample that includes both public and private firms. This indicates that firms added the the Entity List have limited options to circumvent U.S. supply dependencies and points towards the BIS Entity List being valuable tool for economic statecraft.[4]

The remainder of the paper proceeds as follows. Section 2 reviews the literature on export controls and firm-level responses to technology restrictions. Section 3 delineates U.S. Export Control Policies. Section 4 describes the data. Section 5 outlines the empirical methodology. Section 6 presents the primary results. Section 7 shows heterogeneous treatment effects by industry. Section 8 discusses policy implications, and Section 9 concludes.

2 Related Literature

There is extensive literature exploring the various impacts of sanctions.[5] The vast majority of the existing literature focuses on sanctions tools like the Specially Designated Nationals and Blocked Persons list (SDN list) which rely on the proliferation of the U.S. financial system and most recent studies have focused on Russia following their aggressive actions in Ukraine (e.g., Egorov et al., 2025). A much smaller subset of the literature has centered on export controls and particularly those targeting China.

[4] One limitation is that our data do not allow us to systematically observe whether listed firms respond by creating new, related entities to develop workarounds or hold intellectual property. Assessing the prevalence and effectiveness of these responses is an important direction for future research.

[5] See Morgan et al. (2023) for a review of the literature.

Our work contributes to the literature on the effectiveness of economic sanctions, most of which has focused on the use of these policies in the wake of Russia's invasion of Ukraine in 2014. Findings are mixed. While some papers find that the Russian sanctions resulted in significant losses in operating revenue, asset values, and employment for sanctioned firms relative to their non-sanctioned peers (Ahn and Ludema, 2020), others find that those effects were small, of short duration, and some find that they affected less the sanctioned firms than the non-sanctioned ones (Gaur et al., 2023; Keerati, 2022). Nigmatulina (2025) finds that sanctions reduced total imports of targeted firms, but led to increases in revenues and capital. Authors try to reconcile these contradicting results by showing that government compensation to targeted firms increased through subsidies, contracts and loans, and that non-sanctioned firms were affected by crowding out and credit rationing among domestic borrowers.[6] We distinguish our analysis from the existing work on Russian firms by studying export controls imposed in a non-war setting on a much larger, more globally integrated economy, China. The 2014 measures against Russian entities were introduced as part of broad, rapidly escalating wartime sanctions, alongside substantial and contemporaneous policy shifts on multiple fronts. By contrast, U.S. export controls on Chinese firms arise in the context of ongoing strategic competition rather than open conflict, so both the policy design and the behavioral responses of firms and governments are likely to differ, reflecting China's economic size and the less acute nature of the conflict.

We also contribute to the literature on U.S.-China Strategic Competition and the role of export controls.[7] Much of the existing literature on export controls focus on the

[6] Additional work on the Russian sanctions include Gullstrand (2020) and Görg et al. (2024) who estimate the impacts of the Russian sanctions on exporters to Russia in Sweden and Germany, respectively. They both find that the impacts on exporters' overall performance were modest, but there were some negative impacts for a small set of firms that relied heavily on Russia as a lone export market. Itskhoki and Mukhin (2025) analyze the effects of the sanctions on Russia's exchange rate. Fernández-Villaverde et al. (2025) expose the role sanctions had on the rise of dark shipping—oil tankers disabling AIS transceivers to evade detection—and its macroeconomic consequences.

[7] Much of the past work on U.S.-China strategic competition has focused on the impacts of tariffs. See Fajgelbaum and Khandelwal (2022) for a literature review of the U.S.-China trade war focused on the impacts of tariff changes.

innovation push caused by export controls as affected firms cannot source key inputs. Alfaro et al. (2025) look at the impact of Chinese export controls of rare earth elements (REE) in 2010 as part of a response to a dispute over the Senkaku Islands. They find that export restrictions by China led to a global surge in innovation and exports in REE-intensive downstream sectors outside of China. Similarly, Liu et al. (2024) analyze Chinese firm-level import data and find that exposure to the 2007 U.S. "China Military Catch-All Rule," which restricted certain U.S. exports to China, increased the likelihood of firms reporting R&D, raised R&D spending by roughly 50%, and boosted patenting. Recent papers by Shen et al. (2024), Anwar et al. (2024), He et al. (2025), and Hu et al. (2024) find that the Entity Listings lead to increases in firm innovation performance, R&D intensity, and patent filing. Relatedly, He and Lyu (2025) find positive innovation spillovers onto firms that are indirectly affected by the listing of other firms in the same business group.

Papers analyzing the effects of export controls on outcomes outside innovation of Chinese firms are rare. Existing studies find links between export controls and increased financial distress, cash flow volatility and internal control deficiency (Huang et al., 2025).[8] To our knowledge, ours is the first paper to quantify the impacts of export controls on multiple dimensions of financial health of both publicly-traded and privately-owned Chinese firms. We do so using detailed longitudinal firm-level data from Orbis (Bureau van Dijk, 2025) and leveraging the Callaway and Sant'Anna (2021) estimator to exploit variation in the timing of firms being named to the BIS Entity List. We also provide estimates of the BIS Entity-List's heterogeneous impact on Chinese firm

[8] Some studies analyze the indirect effects of Entity Listing. For instance, Liu et al. (2025) find that following the emergence of listed firms in the industry, other firms in the same sector experience a significant increase in their environmental, social, and governance performance. Hu et al. (2025) study the BIS Entity Listings as a signalling mechanisms. They find that U.S. sanctions improve the effectiveness of Chinese government subsidy by releasing signals to alleviate firms' financing constraints and enhance external supervision. Crosignani et al. (2025) analyze the effect of adding a Chinese company to one of the BIS Entity List, the Military End use List, or the Unverified Entity List has on its U.S. suppliers. They show that U.S. exporters cease exporting to all Chinese entities, which results in negative impacts to the U.S. companies' market caps, profitability and employment. They also show that Chinese companies form new connections with Chinese and non-U.S. suppliers offsetting the loss of U.S. suppliers.

outcomes across industry groups using a permutation-based methodology to create estimates of the Entity Lists impact using samples of firms too small for estimators that rely on standard asymptotic theory for their inference.

We additionally contribute to the burgeoning literature on geoeconomics by quantifying firm-level effects of a specific economic statecraft tool, the BIS Entity List, using observed balance-sheet outcomes. In contrast to studies that rely on earnings calls and company filings for publicly listed firms, for example Clayton et al. (2025) and Alfaro et al. (2025), our approach uses financial data that allow U.S. to measure realized impacts and include privately-held firms, thereby complementing disclosure-based evidence with a broader view of firms' responses to sanctions. Within this literature we are closely related to those that study the impacts of economic sanctions, including: Drezner (2003), Morgan et al. (2009), and Felbermayr et al. (2020). Our analysis speaks directly to the research agenda outlined by Drezner (2024), who calls for better measurement of the dynamic effects of sanctions and for assessments of their systemic implications for the global political economy. We implement an event–study design that traces the heterogeneous impact of sanctions over time and across firms, focusing on policy actions between the United States and China, the two largest economies and central nodes in global production networks. By documenting differences in firms' responses across industries, we show how sanctions reallocate activity differently across sectors, to provide an initial understanding of the systemic consequences of sanctions emphasized by Drezner (2024).

3 Export Controls as a Policy Tool

Over the past two decades, the U.S. government has increasingly utilized export restrictions as an important tool for acheiving its objectives related to statecraft and economic security (Fergusson et al., 2021). The core principle underlying these policies is that the United States –by virtue of its technological leadership– can shape the international diffusion of dual-use technologies that have civilian and military applications. Although originally designed as a narrow administrative mechanism to control the export of sensitive items to foreign end users, modern export controls expanded dramatically over the past two decades—in scope, frequency of use, and strategic intent. The current export control regime reflects a significant shift in U.S. economic statecraft toward technology and firm-specific restrictions, particularly in the context of us–China competition (Fergusson et al., 2021). The following sections provide a brief review of U.S. export controls, their use against China, and the Current BIS list.

3.1 Brief History of Export Controls

Export controls have had a long history in the us, dating as far back as the Revolutionary War. Prior to the 1940s they were used primarily during times of war to limit the direct supply to enemy troops. In 1917, these early policies were formalized by the Trading with the Enemy Act (TWEA) when the U.S. entered World War I. TWEA allowed the President restrict economic activities, including exports, with designated enemy countries.

Modern U.S. export control policy originated in the early Cold War as a tool to restrict the transfer of militarily sensitive technologies to geopolitical adversaries.[9] The Export Control Act of 1949 and the subsequent establishment of the Export Administration Regulations (EAR) formalized a system focused on controlling specific goods and technologies—such as nuclear materials, missile components, advanced electronics, and cryptography—based primarily on destination country. During this period, export controls were largely multilateral and coordinated with allied governments through institutions such as the Coordinating Committee for Multilateral Export Controls (COCOM) (Congressional Research Service, 2023). Enforcement emphasized item-based

[9] See Bureau of Industry and Security, Office of Export Enforcement (1987)

restrictions and country coverage rather than specific firms or entities, reflecting a strategic environment dominated by state-to-state rivalry.

Following the end of the Cold War, export controls entered a period of relative contraction and reform. As former adversaries integrated into the global economy, U.S. policy increasingly emphasized trade liberalization, with export controls narrowed to a smaller set of proliferation-sensitive technologies (Fergusson et al., 2021). This period also saw administrative modernization of the EAR, including a greater emphasis on the license review process and on end-use and end-user certification, as well as expanded use of post-shipment verification. Although firm-level enforcement mechanisms existed, focus shifted to categories of goods, such as weapons of mass destruction, in the form of controls on chemical, biological, and missile components (Fergusson et al., 2021).

Beginning in the late 2000s and accelerating sharply in the late 2010s, U.S. export controls underwent a fundamental transformation in both scope and intent.[10] Policymakers increasingly adopted firm-specific, technology-targeted restrictions designed to operate through global supply chains rather than through broad country embargoes. Central to this shift was the expanded use of the BIS Entity List. Originally intended as a narrow administrative tool to identify end users posing diversion or proliferation risks, the Entity List evolved into a core mechanism of U.S. economic statecraft. Particularly, the BIS Entity List has been used in situations where traditional broad sanctions, such as those found on Iran, Syria, and North Korea, could be too escalatory or disrupt widespread economic linkages. This is the case with China where the U.S. has sometimes adopted the BIS Entity List to apply geoeconomic pressure without severing economic ties or invoking an oversized response.[11]

[10] See Pagano (2023) for a detailed overview of this transformation.

[11] An example of this includes the dispute over territory in the South China Sea. In 2020, the BIS End-user Review Committee (ERC) added new entities to the Entity List, citing their involvement in the dispute: "Specifically, the ERC has taken account of evidence that these entities enabled China to reclaim and militarize disputed outposts in the South China Sea. In particular, these entities have engaged in reclaiming land at Mischief Reef, which, per a July 12, 2016 ruling by an Arbitral Tribunal convened under the 1982 Law of the Sea Convention, was determined to be part of the Philippines' exclusive economic zone and continental shelf." See Bureau of Industry and Security, Department of

3.2 Export controls in the context of strategic competition with China

Over the past two decades—and especially since the mid-2010s—the United States has increasingly used a variety of export controls as a central instrument of strategic competition with China. Initially, theses controls were narrowly focused on nonproliferation and involvement with sanctioned states (i.e. Iran, North Korea, and Syria), but they have now shifted towards a much broader strategy of denying technological and strategic components.

The U.S. has used the full scope of available export control and sanctions regimes against firms in the PRC depending on the specific situation.[12] Given that the policies vary greatly in their authorities, implementation, and enforcement, a brief summary of the primary policies is provided below. Among these tools, the BIS Entity List, administered by the U.S. Department of Commerce, in most cases, requires licenses for any exports, reexports, or in-country transfers of items subject to the Export Administration Regulations to listed firms and has been applied to more than a thousand PRC entities at present.[13] Other Commerce-administered tools have been wielded in more recent years against PRC entities as well, including the Military End user List, created in 2020, which is narrowly focused on specific items for military end users in China.[14] The Unverified List is also administered by the Department of Commerce and currently contains just over one hundred Chinese companies and prohibits the use of license exceptions for exports, reexports, or in-country transfers to the listed parties and prescribes their addition to the Entity List if they refuse an end use check for 60 days.[15] In addition, the China Military Catch-All Rule, implemented in 2007 following a draft rule in 2006, expanded controls on exports to the PRC of certain items on the

Commerce (2020).

[12] Note that many policies are nonexclusive so multiple can be used against a single firm.

[13] Moreover, certain Entity List additions have a "footnote" that imposes additional restrictions on foreign-produced items and U.S. servicing.

[14] See link to listed entities named in Federal Register. Accessed December 29th 2025.

[15] See link to unverified entities at bis.gov. Accessed December 29th 2025.

Commerce Control List (CCL) based on Export Control Classification Numbers and established a process for Chinese companies to become "validated end-users," in which case they did not need a license.[16]

Beyond Commerce-administered export controls, several financial and investment measures have also been deployed in China. The U.S. Treasury Department's Specially Designated Nationals (SDN) List, which blocks access to U.S. assets and dollar-denominated transactions, has been applied to a substantial number of PRC firms, but in practice these designations have largely been based on nonproliferation or third-country sanctions authorities (e.g. related to North Korea, Iran, Russia, or broad WMD programs) rather than being explicitly framed as China-focused economic statecraft. The Non-SDN Chinese Military-Industrial Complex (CMIC) List, created in 2021 and covering several dozen PRC firms, targets U.S. portfolio investment in designated companies but does not itself impose export controls. Similarly, the Section 1260H list, also created in 2021 and currently covering roughly one hundred Chinese firms, restricts U.S. defense contractors from doing business directly or indirectly with designated entities, again without directly regulating exports.[17]

This study focuses on the BIS Entity List because it is has the broadest coverage in terms of its history of use, scope, and quantity among the policies that have been applied to PRC firms and because it directly regulates access to controlled U.S.-origin technology and components. The other Commerce tools (such as the Military End user List, the Unverified List, and the China Military Catch-All Rule) are either narrowly tailored to focus on PRC military end uses or cover too few firms to generate useful econometric estimates of effects. The CMIC and Section 1260H lists, while important in their own right, primarily operate through investment and procurement channels rather than through export licensing. The SDN List has been omitted from this study because it has primarily been applied to PRC firms for their involvement with other

[16] See link to federal register. Also see Liu et al. (2024) for a more detailed review.
[17] See link to federal register.

U.S. adversaries and therefore has not, to date, been used in a systematic way as an economic statecraft tool against the PRC itself.[18]

3.3 The Current State of the BIS Entity List

After starting with just four firms in 1997 and sparsely being used prior to 2008, the BIS Entity List has constantly grown over the past 15 years and has seen its largest annual expansions in 2022, 2024, and 2023 respectively.[19] Figure 1 displays how the use of the BIS Entity List has grown over time. The Entity List currently has $3,313$ firms from 88 countries. China has the most listed entities with $1,097$ which puts it slightly above Russia. Additions of Chinese firms to the Entity List closely follow periods of heightened goepolitical pressure between the two countries. Prior to 2018, no more than 25 Chinese firms were added in any year while the average since has been 140. Large additions of PRC firms occurred in 2020 (204), 2023 (183), and 2024 (308). Other countries with significant presence on the list include the UAE (186), Pakistan (181), and Iran (103).

Functionally, being added to the BIS Entity List imposes licensing requirements for any U.S. firm seeking to export to the listed firm. Technically the licensing requirements could only apply to a subset of items subject to the EAR. In practice, however, the licensing requirement is applied to all items subject to the EAR. In addition to the licensing requirement, each firm is given a "licensing policy" that determines the process for obtaining a license. The vast majority of PRC firms receive a "presumption of denial" policy.[20]

[18] This is not the case for Russia and is why the SDN List makes up the bulk of treatment in Ahn and Ludema (2020) and Nigmatulina (2025).

[19] Much of the increase in 2022 was driven by Russia's invasion of Ukraine and the subsequent U.S. response, but China accounts for a large share of the additions in 2023 and 2024.

[20] See Bureau of Industry and Security, Department of Commerce (2026).

4 Data

We use data from two primary sources: firm-level financial data from Moody's Orbis dataset and export-control designation data directly from the U.S. Department of Commerce Bureau of Industry and Security (BIS). The Orbis database provides standardized financial statements, ownership information, and descriptive metadata for firms globally, while the BIS Entity List contains the complete official record of firms subject to expanded export-licensing requirements. Merging these two datasets allows U.S. to construct a firm-year panel that tracks the financial outcomes of Chinese firms before and after Entity List designation.

The BIS Entity List does not have universal identifiers such as tax ID so we implement a multi-step process to properly link treated firms to the appropriate entity in the Orbis dataset. We first collect as much information as possible on treated firms from publicly available sources. In most cases, this allows U.S. to have English name, Chinese character name, alternate names, tax ID, and Unified Social Credit Code. We then utilize Orbis' built in matching algorithm on all of these characteristics to identify the proper observations for each treated firm. Of the $1,227$ Chinese firms on the Entity List, we identify 660 matches within Orbis that contain some financial data. Within those matched firms, 149 have sufficient data to perform our analysis.[21]

The resulting dataset includes annual observations for PRC-based firms from 2012 through 2024. Each observation corresponds to a firm-year, and the primary outcomes used in the analysis include revenue, employment, profitability, leverage, and various balance sheet indicators.

[21] In order to be included in our sample, firm must have at least five years of data and at least three post treatment observations.

4.1 Orbis

Orbis is a commercially available database complied by Moody's (formerly Bureau Van Dijk) and is one of the most widely utilized data sources for firm financial information both globally and in China (see Kalemli-Ozcan et al. (2024), Cravino and Levchenko (2017), Martin et al. (2014), Gopinath et al. (2017), and Ruane (2022). Orbis is particularly useful for studies focusing on firm performance in countries outside of North America and Europe where a variety of sources all have similar coverage.[22] The database is formed by harmonizing financial statements for millions of firms across the globe to create time series for every major financial indicator and ownership linkages (see Bureau van Dijk (2009) Bureau van Dijk (2025) for further information on the database structure). This study focuses solely on Chinese firms but Orbis data allows for an analogous study in almost any country.[23] The underlying data is collected via two primary methods. The first is directly from financial statements that firms share with Moody's and other rating agencies to receive credit ratings before borrowing on international markets. The second is by scraping and cleaning data from publicly available sources. This includes both national databases such as firm censuses as well as tax filings and news articles.

Despite being the leading choice among researches needing firm level financial data, the Orbis dataset does have known weaknesses. The first weakness is that data coverage and quality varies greatly between countries. While Orbis utilizes publicly available information to broaden its data beyond firms seeking credit ratings, data in countries where international financing is uncommon is often limited to what national authorities collect and publish. This is most often a limiting factor in emerging market and developing economies. Moody's has invested heavily in building their China dataset, currently covering 78 million firm records, so we are comfortable using it as a basis for our study. The second major weakness of the Orbis dataset is that it is not a

[22]See Bloomberg and S&P for two widely used alternatives for American and European markets.
[23]Orbis has data for over 100 countries but coverage coverage varies greatly.

representative sample of firms in any single country and skews toward larger, more internationally connected firms (Bajgar et al., 2020). In the context of our study, this is not a shortcoming because firms named the the Entity List are predominantly larger than the average firm and by definition if a company is importing U.S. origin goods, it is internationally connected. After evaluating multiple alternative sources, Orbis was the clear leader in terms of data coverage, quality, and ability to be merged with additional datasets.

We download data from Orbis using the native API and restrict our pulls to the 57 industries with at least one firm named to the Entity List. Restricting the industries results in more manageable queries and ensures that we do not bias our control set by including firms that are sufficiently different from those facing export controls (i.e. clothing manufacturers). After removing firms that have low quality data and lack a total of 6 years of observations with at least three post 2020, we are left with an unbalanced panel of 115,000 firms spanning 2012 to 2024 that becomes the core data for our analysis.

4.2 BIS Entity List

Data on which firms are subject to export controls via the BIS Entity List is obtained from the Consolidated Screening List published by the U.S. International Trade Administration. The Consolidated Screening List is a searchable database that combines multiple lists of individuals and entities restricted from certain exports, reexports, or transfers of items designed to assist businesses in due diligence related to complying with U.S. trade restrictions. The database contains over 25,000 entries and includes information on name, ID, address, date of inclusion, date of exit (if applicable), licence requirements, and link announcement documents. After removing entries for lists other than the BIS Entity List and further filtering to only firms with an address in China, we are left 1,227 firms that become our treated sample. Because the Consolidated

Screening List only contains English transliterations of firm name and a U.S. government specific firm ID, we search each firm in a variety of public databases to identify their Chinese character name, tax ID, and Unified Social Credit Code (usCC). These additional fields, particularly the Chinese character name, become extremely valuable when merging the treated firms into the Orbis data.

4.3 Descriptive Statistics

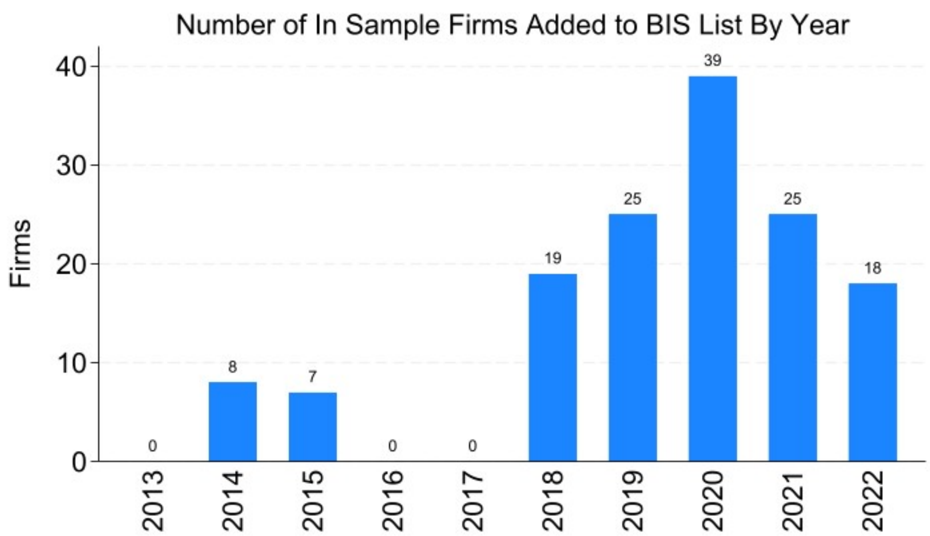
After downloading key financial series from Orbis and cleaning the Consolidated Screening List from the U.S. International Trade Administration we must merge the two datasets and determine how many treated firms have sufficient data in Orbis to perform our analysis. Unfortunately, there is not a commonly used unique identifier between the two data sources.[24] To match firms between the two sources, we utilize Chinese language name, English name, usCC, Tax ID, and address along with Orbis' confidence measure to identify financial information for treated firms. We restrict our sample to only firms that have at least 6 years with financial data. Treated firms must have a minimum of 3 years of post treatment data to be included and we require control firms to have 3 years of data post 2020 because that is when the bulk of treatment occurs in our sample.

The final dataset for this study contains observations for 149 treated firms and 115,333 untreated firms in an unbalanced panel spanning 2012 to 2024. Table 1 provides summary statistics for the financial variables we observe. A few important trends become apparent from Table 1. First, treated firms are orders of magnitude larger than the untreated average firm. For example, the mean operating revenue of a treated firm prior to being added to the Entity List was ¥700, 389, 900 while the average untreated firm had operating revenue of ¥142, 340, 700 or just 20% of the treated average. Similar differences are evident in other measures of size (employment and total assets)

[24]The Consolidated Screening List has an ID field but it does not map to any of the national IDs found within Orbis.

and profits. Second, our control sample (see Section 5.2 for information on how the control sample was constructed) roughly matches the treated sample in terms of mean and standard deviation. It is not surprising that the mean for certain variables, such as revenue, is still higher in the treated sample because treatment is heavily skewed towers larger firms. Finally, the standard deviations of our observed series are quite large, implying a significant degree of heterogeneity in our sample and the possibility that average outcomes may mask a more nuanced story when looking by industry or size.

Figure 3: In Sample Firms Added to the BIS List by Year



5 Empirical Strategy

Our choice of empirical strategy is guided by the three needs. First, that the treatment we analyze (i.e., U.S. Department of Commerce's BIS selection of Chinese firms to prevent them access to U.S.-origin goods) is staggered (i.e., BIS' firm selection occurs on different dates). Second, that the treatment state is absorbing (i.e., once included

Figure 4: BIS Firms by Industry

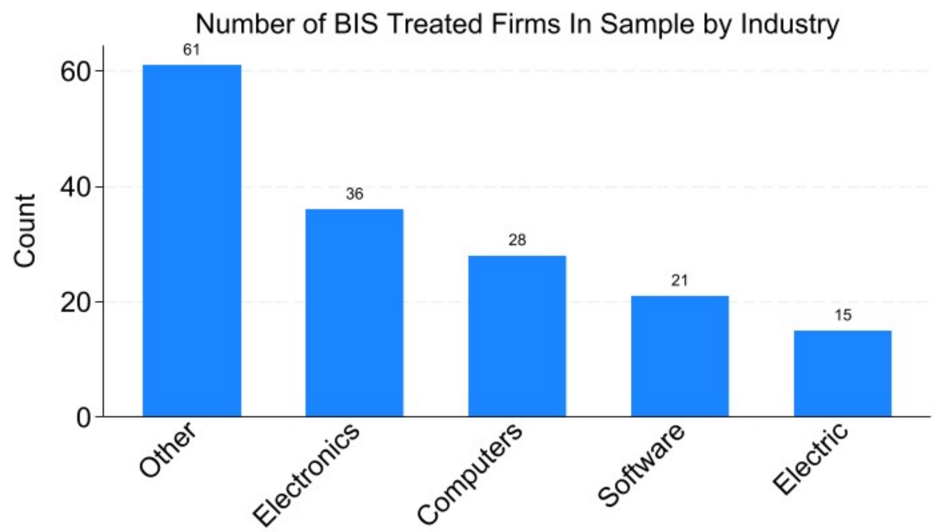


Table 1: Summary Statistics

	Treated Firms at $t < 0$		**Control Before 2020**		**Control in Sample**	
	Mean	SD	Mean	SD	Mean	SD
Operating Rev.†	700389.9	533569.3	142340.7	330842.5	578131.1	535420.1
Profit Bef. Tax†	44562.29	36841.56	8355.119	22019.27	33645.62	36880.55
Total Assets†	1165662	790319.9	169807.8	437789.1	857565.7	798206.9
Current Ratio	4.270949	3.827247	6.388694	4.241486	3.929159	3.852188
Solvency Ratio	46.63743	27.60448	39.21119	34.55087	43.39265	29.9875
Employment	636.8316	373.4525	384.2286	430.316	641.8688	377.3764
Intang. Assets†	16998.03	14015.17	6787.368	12009.11	15271.22	13872.63
Cash†	556774.3	357785	489550.9	426367.2	412167.8	394590.2
Current Liab.†	746458.4	580076.4	418281.4	597677.3	613903.4	588968.1
Turnover†	739102.8	563549.6	232923.2	450930.4	599406.9	562613.1
Costs†	809896.7	656137.5	517058.8	683380	699489	657150.2

Note: Data from Orbis. † Indicates units are thousands of RMB. There are 143 unique treated firms that amount to 763 firm-year observations. The control sample comprises 2131 unique firms amounting to 11,761 firm-year observations that stem from a pre-2020 untreated sample with 115,333 unique firms with 600,458 firm-year observations.

to the list the firms remain so).[25] Third, that we have special interest in obtaining

[25]Notionally, firms can be removed from the Entity List but this is rare and often due to clerical errors. In the few cases where firms are removed from the list, we continue to apply treatment to capture the lasting stigma of being treated.

treatment-effect dynamics and observing firm outcomes evolve several periods after treatment in order to understand if firms react and cope with the burden of being unable to source key inputs from America manufacturers. Therefore, to estimate how this staggered, absorbing treatment affects Chinese firms' performance and sustainability, we implement two complementary difference-in-differences estimators, Callaway and Sant'Anna (2021) (CSDiD) and a permutation-based estimator, designed for settings with heterogeneous treatment timing and rely only on valid comparisons to firms that are untreated at the time outcomes are measured.

5.1 Callaway and Sant'Anna Difference-in-Difference

The CSDiD estimator is built for settings exactly like the one we explore in this paper: an absorbing treatment imposed on at different times across firms, with impacts that plausibly vary by cohort and over time. CSDiD constructs cohort-time effects using only firms that are untreated at the evaluation date as controls, thereby avoiding the contamination and negative-weight problems of conventional two-way fixed effects. In addition, CSDiD's transparent aggregation of cohort-level effects delivers event-time dynamics, aligning with our goal of tracing the consequences of the BIS restriction for firm performance and sustainability.

Let Y_{it} denote an outcome for firm i in period t, and let D_{it} indicate whether firm i is treated in period t. Define the first treatment date $G_i = \min\{t : D_{it} = 1\}$, with $G_i = \infty$ for firms that are never treated. The object of interest is the cohort-by-time average treatment effect for the treated, $ATT(g, t)$, which measures the effect for firms first treated in period g evaluated at time t. Formally,

$$ATT(g, t) = E\big[Y_{it}(1) - Y_{it}(0) \mid G_i = g,\ t \geq g\big].$$

These group-time effects are identified by comparing the change in outcomes for the cohort $G_i = g$ from a pre-treatment baseline to period t against the contemporaneous change for firms that are untreated at t—either never-treated firms or firms not yet treated by t—and then aggregating $ATT(g,t)$ across cohorts and times to construct event-time dynamics and overall effects.

For each cohort-time pair (g,t), we form the analysis sample consisting of firms with $G_i = g$ and firms with $G_i > t$ (the valid controls), and we choose a pre-treatment baseline $s = g - 1$. The CSDiD estimator combines two components. The first is an inverse probability weighting (IPW) component in which CSDiD estimates cohort-membership probabilities to make the not-yet-treated controls resemble the cohort in terms of their pre-treatment covariates X_i. Concretely, CSDiD estimates $p_g(X_i) = P(G_i = g \mid X_i)$, the probability that firm i belongs to cohort g given its pre-treatment characteristics X_i. The second is an outcome regression piece that models the change in the outcome for controls between s and t as a function of X_i and uses the fitted change to impute the untreated counterfactual for cohort firms. Formally, within each group-time cell (g,t), CSDiD compares the average change in outcomes for the cohort between the baseline s and t to a reweighted average change for controls with $G_i > t$, where each control firm receives the weight $w_i = \frac{p_g(X_i)}{1-p_g(X_i)}$. These weights shift the covariate distribution of the control group to match that of the cohort, so that the weighted controls approximate what the cohort's untreated outcome change would have been in the absence of treatment under conditional parallel trends.

Let N_g denote the number of firms with $G_i = g$. A simple IPW estimator for the group-time effect is

$$\widehat{ATT}^{IPW}(g,t) = \frac{1}{N_g} \sum_{i:G_i=g} \big(Y_{it} - Y_{is}\big) - \frac{\sum_{i:G_i>t} \frac{p_g(X_i)}{1-p_g(X_i)} \big(Y_{it} - Y_{is}\big)}{\sum_{i:G_i>t} \frac{p_g(X_i)}{1-p_g(X_i)}}.$$

To summarize dynamics, we report event-study effects by relative time $k = t - g$. For each event time k, CSDiD aggregates across cohorts that contribute observations at

$t = g + k$ using weights proportional to cohort size:

$$\theta(k) = \sum_g w_{g,k}\, ATT(g, g+k), \qquad \sum_g w_{g,k} = 1.$$

Estimates at negative event times $k < 0$ serve as pre-trend diagnostics and should be close to zero under our assumptions; estimates for $k \geq 0$ trace short- and medium-run effects on performance and sustainability. We construct confidence intervals using a bootstrap that respects the panel structure and treatment timing, with resampling at the firm level and a multiplier scheme over group-time cells.

Identification rests on three conditions adapted to our setting. First, we assume no anticipation: outcomes prior to a firm's treatment do not depend on future treatment, so for $t < G_i$ we have $Y_{it} = Y_{it}(0)$. Second, we invoke conditional parallel trends for untreated potential outcomes. For each cohort g and post-treatment time $t \geq g$, the counterfactual trend in the absence of treatment would have been the same for the cohort and its controls, conditional on observed pre-treatment covariates X_i. Intuitively, in the absence of treatment, firms first treated in g would have followed the same untreated evolution as firms that are not yet treated at t. Third, we require overlap: within each cohort-time cell (g, t), the covariate profiles observed among firms first treated in g also appear among firms that are still untreated at t.[26] When overlap fails—for example, if all firms in an industry are already treated by time t—inverse-probability weights become unstable because the conditional probability that firm i is not yet treated by time t approaches zero, and outcome models must extrapolate into regions with no untreated observations, undermining identification. A practical implication is that we restrict attention to post-treatment horizons where sufficient numbers of untreated firms remain and covariate distributions overlap.

In this application, CSDiD yields interpretable event-time dynamics that are well suited

[26] Formally, for any x in the support of X_i among cohort-g firms, we require $P(G_i = g \mid X_i = x) > 0$ and $P(G_i > t \mid X_i = x) > 0$. This ensures that the counterfactual trend for cohort-g firms can be learned from comparable controls rather than extrapolated beyond the data.

to evaluating how loss of access to U.S.-origin goods affects firms' performance and sustainability.

5.2 Permutation-based Estimator

Given the nature of the BIS list, it might be important to measure its effects by comparing treated firms with similar firms within subsamples, like within industries. Such subsamples that are often too small to accommodate for estimators that rely on standard asymptotic theory for their inference. As a result, we base our inference on a permutation-based method that is valid in finite samples under a clearly stated null and a specified assignment mechanism for treatment (Heckman et al., 2010).

The central idea is to construct, for each listed firm, a sequence of treated-control pairs and calculate outcome differences over time. Because there is typically more than one plausible control firm for each treated firm, and the quality of the match is heterogeneous, the estimator averages event-study coefficients over many random draws of the matching assignment, where controls are drawn with probabilities determined by pre-specified similarity weights. This procedure delivers an estimator that is rooted in standard DiD/event-study logic while explicitly integrating over the uncertainty and arbitrariness in the choice of matched controls.

5.2.1 Nearest Neighbor Matching

Let firms be indexed by $i = 1, \ldots, N$ and calendar years by $s = 1, \ldots, T$. Let t^* denote the year in which BIS listings occur. A subset $\mathcal{T} \subset \{1, \ldots, N\}$ of firms is designated by BIS in year t^*. Define event time $k = s - t^*$. Let $\mathcal{C}$ denote the set of never-treated firms. For each treated firm $i \in \mathcal{T}$, define its admissible control set $\mathcal{J}(i) \subseteq \mathcal{C}$ as the collection of firms that satisfy pre-specified coarse comparability restrictions, like operating in the same (or a closely related) industry. Let X_i denote a $K \times 1$ vector collecting pre-treatment characteristics of firm i, measured strictly before t^*. For each treated firm $i \in \mathcal{T}$ and each admissible control $j \in \mathcal{J}(i)$, define the squared Mahalanobis distance between their pre-treatment covariate vectors as

$$d_{ij} = \sqrt{(X_i - X_j)' \hat{\Sigma}^{-1} (X_i - X_j)}.$$

where $\hat{\Sigma}$ is the empirical covariance matrix of the covariates. This distance is invariant to linear rescaling of the covariates and captures multivariate similarity in the space of pre-treatment characteristics: pairs (i, j) with smaller d_{ij} are closer in covariate space and thus more comparable. In the vector $X_.$, we include pre-treatment measures of operating revenue, total assets, and profit or loss before tax. In addition, we only allow matches within industries. The Mahalanobis distances are then transformed into non-negative similarity weights. For each treated firm i and admissible control firm $j \in \mathcal{J}(i)$, define unnormalized weights $\tilde{w}_{ij} = g(d_{ij})$, where $g : \mathbb{R}_+ \to \mathbb{R}_+$ is a strictly decreasing function. While in principle any decreasing function $g(\cdot)$ can be chosen to place rapidly declining weight on distant controls, we use $\tilde{w}_{ij} = 1/d_{ij}$.

The normalized similarity weights are then given by

$$w_{ij} = \frac{\tilde{w}_{ij}}{\sum_{l \in \mathcal{J}(i)} \tilde{w}_{il}}, \qquad j \in \mathcal{J}(i),$$

so that $\sum_{j \in \mathcal{J}(i)} w_{ij} = 1$ for each treated firm i. By construction, $w_{ij} \geq 0$ for all i and j, and larger w_{ij} indicates that control firm j is a higher-quality comparison for treated firm i in terms of pre-treatment covariates.

Based on the weights, we implement a stochastic matching design. For each treated firm $i \in \mathcal{T}$, exactly one control firm J_i is selected at random from the candidate set $\mathcal{J}(i)$ according to a probability given by w_{ij}. This produces a random matching $J = \{J_i : i \in \mathcal{T}\}$ of treated firms to controls, with exactly one control chosen for each treated firm.

5.2.2 Treatment Effect Estimation

Given any particular realization of the matching J, for each matched pair (i, J_i) and event time k, define $TE_{i,k} = Y_{i,k} - Y_{J_i,k}$ the treated-minus-control outcome difference. Let $k_0 = -1$ denote a chosen pre-treatment base period, the year immediately prior to listing. The dynamic DiD estimator conditional on J is obtained by regressing $TE_{i,k}$ on a full set of event-time dummies:

$$TE_{i,k} = \sum_{h \in \mathcal{H}_{k_0}} \beta_h(J)\, \mathbf{1}\{k = h\} + \varepsilon_{i,k},$$

where $\mathcal{H}_{k_0}$ is the set of observed event times with k_0 omitted. By construction, the coefficient on the dummy for event time $h \neq k_0$ is $\hat{\beta}_h(J) = \bar{TE}_h(J) - \bar{TE}_{k_0}(J)$, where $\bar{TE}_h(J)$ denotes the average of $TE_{i,k}$ over all matched pairs observed at event time $k = h$ under the matching J. That is,

$$\hat{\beta}_h(J) = \frac{1}{N_{T,h}} \sum_{i \in \mathcal{T}: k=h} \big(Y_{i,h} - Y_{J_i,h}\big) - \frac{1}{N_{T,k_0}} \sum_{i \in \mathcal{T}: k=k_0} \big(Y_{i,k_0} - Y_{J_i,k_0}\big),$$

with $N_{T,h}$ the number of matched pairs observed at horizon h.

To interpret $\hat{\beta}_h(J)$ as a causal effect, we impose standard matching–DiD assumptions at the pair level. For each treated firm i and its matched control J_i, potential outcomes under no treatment satisfy

$$E\big[Y_{i,k}(0) - Y_{J_i,k} \mid i, J_i\big] = \mu_i, \qquad \forall k \in \mathcal{H},$$

where μ_i is a pair-specific constant that does not depend on event time k. In words, absent treatment, the treated firm and its matched control would have followed parallel trends, up to a time-invariant difference in levels. Assuming further that $Y_{i,k} = Y_{i,k}(1)$

for $k \geq 0$ for treated firms, it follows that

$$E\big[\hat{\beta}_h(J) \mid J\big] = E\big[Y_{i,h}(1) - Y_{i,h}(0)\big] - E\big[Y_{i,k_0}(1) - Y_{i,k_0}(0)\big]$$

for $h \geq 0$ and $i \in \mathcal{T}$. Thus, conditional on the realized matching J, $\hat{\beta}_h(J)$ identifies the average treatment effect at horizon h relative to the base period k_0.

5.2.3 Monte Carlo and Stochastic Matching

Our empirical strategy acknowledges that, for each treated firm, multiple plausible controls exist, and any single choice of J is to some extent arbitrary. Rather than committing to one deterministic match per treated firm, we re-draw the matching according to the stochastic design described above and recompute the event-study coefficients for each draw. Let $J^{(r)}$ for $r = 1, \dots, R$ denote R independent draws from the matching distribution, and let $\hat{\beta}_h^{(r)} = \hat{\beta}_h(J^{(r)})$ be the corresponding event-study coefficients. The Monte Carlo estimator of the dynamic effect at horizon h is defined as the average of these coefficients:

$$\hat{\beta}_h^{MC} = \frac{1}{R}\sum_{r=1}^{R} \hat{\beta}_h^{(r)} \to E_J\big[\hat{\beta}_h(J)\big] \quad \text{as } R \to \infty,$$

Which converges to the expectation of the matched event-study estimator with respect to the matching design. This expectation has a convenient interpretation. Define, for each treated firm i, the design-implied synthetic control outcome at horizon k as

$$\tilde{Y}_{i,k}^{C} = \sum_{j \in \mathcal{J}(i)} w_{ij} Y_{j,k},$$

the weighted average of candidate control outcomes, where the weights w_{ij} are the matching-selection probabilities. Linearity implies that

$$\beta_h^{MC} = \big[\bar{Y}_{T,h} - \bar{\tilde{Y}}_{C,h}\big] - \big[\bar{Y}_{T,k_0} - \bar{\tilde{Y}}_{C,k_0}\big],$$

where $\bar{Y}_{T,h}$ and $\bar{\bar{Y}}_{C,h}$ are, respectively, the treated and synthetic-control averages at horizon h. In other words, the Monte Carlo matched event-study estimator is equivalent, in expectation, to an event-study DiD estimator that uses synthetic controls constructed as weighted averages of all candidate controls, with weights given by the matching design.

Identification of the dynamic treatment effect by β_h^{MC} requires an extension of the pairwise parallel-trends assumption to the design-weighted synthetic controls:

$$E\big[Y_{i,k}(0) - \tilde{Y}_{i,k}^{C} \mid i \in \mathcal{T}\big] = \mu, \qquad \forall k \in \mathcal{H},$$

with μ constant in k. Under this condition, the design-based target β_h^{MC} equals the dynamic treatment effect up to an additive constant that is eliminated by differencing relative to k_0.

The repeated sampling over matchings also provides a natural measure of the sensitivity of the estimated dynamic effects to the choice of control group. For each h, the empirical distribution $\{\hat{\beta}_h^{(r)}\}_{r=1}^{R}$ captures how much the event-study coefficient changes when the comparison firms are varied within the admissible sets. We summarize this distribution by its Monte Carlo mean $\hat{\beta}_h^{MC}$ and by the 5^{th} and 95^{th} empirical percentile bands of $\{\hat{\beta}_h^{(r)}\}_{r=1}^{R}$. These bands are not classical sampling confidence intervals derived from a large-sample variance formula; instead, they are design-based robustness envelopes that quantify the range of estimates implied by different plausible realizations of the matched control group. They answer the question of how much the estimated dynamic treatment effects would change if a different but comparably plausible set of controls had been chosen.

6 Main Results: Difference-in-Difference

To quantify the impact of U.S. export controls on PRC firm performance, we first estimate average treatment effects using Callaway and Sant'Anna (2021). This provides an unbiased difference-in-difference estimate of the policy's impacts, but given the relatively small sample of treated firms (149), it does not allow U.S. to further identify heterogeneous outcomes based on firm characteristics such as size or industry.[27] That is, does the effect of being added to the BIS list differ based on a firm's industry or size? To address the potential for heterogeneous outcomes, we next adapt the a permutation-based estimator, which is designed to work on small samples, to both provide a robustness check and determine if different firm characteristics result in greater treatment effects.

After applying the methodology outlined in Section 5.1, we see a clear negative impact of being added to the Entity List on some measures of financial health and no significant impact on others. Collectively, our results indicate adding a firm to the Entity List applies financial pressure and causes the firm to shrink in terms or revenue, employment, and assets but does not have a large impact on survivability measures such as solvency ratio.

We focus our analysis on 10 core measures of financial health.[28]. The core measures of financial health include operating revenue, total employees, profit, cost of goods sold, total assets, cash assets, intangible assets, current liabilities, current ratio, and solvency ratio. These variables were selected because they cover the three primary channels by which an export control could impact a firm: sales, costs, and assets/liabilities.

The Callaway and Sant'Anna (2021) methodology allows U.S. to create classic event

[27]This is strictly a sample size constraint and not an explicit shortcoming of the Callaway and Sant'Anna (2021) methodology. In cases with lager pools of treated firms, CSDiD can be run sequentially on each industry or size bin.

[28]Results for the other variables available in the Orbis dataset can be provided by the authors upon request

Table 2: Average Treatment Effect (5-Year Pre/Post Horizon)

	Natural Log			Per Dollar of Sales		
Outcome Variable	N_obs	Pre_avg	Post_avg	N_obs	Pre_avg	Post_avg
		-0.110*	-0.718***		0.028	-0.065
Cash and cash equivalent assets	977	(0.061)	(0.236)	960	(0.034)	(0.117)
		-0.028	-0.581***		0.004	-0.007
Costs of goods sold	1,069	(0.068)	(0.166)	1,053	(0.006)	(0.015)
		-0.061	-0.547***		0.030	0.086
Current liabilities	1,015	(0.041)	(0.145)	998	(0.037)	(0.085)
		0.015	0.021			
Current ratio	1,028	(0.030)	(0.126)			
		-0.062	-0.950***		-0.005	0.015
Intangible assets	880	(0.064)	(0.317)	872	(0.012)	(0.040)
		-0.006	-0.409**		-0.000	0.001***
Number of employees	1,292	(0.036)	(0.166)	1,218	(0.000)	(0.000)
		-0.364	1.561		-0.013	0.236***
Operating profit (loss) [EBIT]	1,320	(0.332)	(1.652)	1,273	(0.013)	(0.059)
		-0.061	-0.862***			
Operating revenue	1,432	(0.041)	(0.148)			
		-0.008	-0.074			
Solvency ratio (Asset based)	1,341	(0.027)	(0.146)			
		-0.032	-0.450**		0.108	0.925**
Total assets	1,501	(0.030)	(0.177)	1,405	(0.124)	(0.444)

Robust standard errors in parentheses

**** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$*

Note: Each Pre/Post average pair is from an independent regression using the methodology outlined in Section 5.1. Each row represents a single regression.

study figures with set specifications of pre/post treatment years but a useful starting place is to look at simple pre vs post treatment averages for each variable. Table 2 presents the pre vs post treatment average for each of our ten outcome variables when using either log or per dollar of sales transformations over a five year pre/post horizon. The simplest way to think about the estimates in the post treatment average column is as if they are the coefficients in an Ordinary Least Squares (OLS) regression of a dummy variable for treatment on the given outcome variable.[29] It is important to note that each row (and column block) in table 2 represents a separate regression, and not coefficients within a single regression as would be the case with OLS. The number of observations

[29]Note that the results in table 2 are derived from the Callaway and Sant'Anna (2021) methodology using the CSDiD function in Stata, not OLS. The pre and post treatment averages are calculated using five years of data, i.e. 5 years pre treatment and 5 years post treatment. Longer horizons become problematic due to limited data availability.

has been included to illustrate sample volatility between outcome variables. The pre treatment average column is included to demonstrate the parallel trends assumption is justified and indicate that the post treatment averages are not driven by underlying volatility in the data.

The results using log transformed variables show a strong negative effect on operating revenue, number of employees, cash assets, intangible assets, total assets, cost of goods sold, and current liabilities. Conversely, we find no statistically significant effect on solvency ratio, current ratio, or profit (measured as earnings before tax). The lack of a significant impact on solvency ratio and current ratio indicates that firms weather the effects of being placed on the Entity List by liquidating assets and reducing size but do not face significant bankruptcy or survivability risks. We attempt to measure the probability of a firm exiting the market but our data does not allow U.S. to confidently differentiate between firms that stop reporting data and those that go out of business. Given that our sample is restricted to firms that have at least 3 years of post treatment data, our results are biased toward firms that continue to both exist and report financial performance information.[30] Therefore, the lack of effect on solvency ratio is somewhat expected by virtue of continuing to report data.

In terms of magnitude, BIS listing has the largest impacts on intangible assets (61% reduction),[31] operating revenue (58% reduction), and cash assets (51% reduction) but the impacts are meaningful for the other four variables with statically significant effects. Explicitly, cost of goods sold (44% reduction), current liabilities (42% reduction), total assets (36% reduction), and employees (33% reduction). The fact that total assets falls less than cash and intangible assets, implies that physical capital stock is relatively

[30] Because the bulk of the Orbis database is collected from firms that voluntarily provide data to receive a credit rating, exiting the database could indicate a firm's exit or a decision to seek credit from non-international markets. The later is particularly important in the context of China.

[31] In most firm-level financial statements, 'intangible assets' is an aggregate category that includes goodwill and various forms of intellectual property (e.g., patents, trademarks, software, customer relationships, and licensing rights). Thus, the large decline in intangible assets following BIS listing is consistent with the impairment or loss of export-related licensing rights and other IP whose value depends on access to U.S. technologies. We do not observe the underlying components of the intangible asset line, this interpretation should be viewed as suggestive.

unaffected in the short run.[32] Collectively, the results indicate listed firms face both supply and demand side shocks after being added to the Entity List and are forced to liquidate assets and decrease operations to stay in business.

After establishing the average post treatment effect of Entity List designation, we can turn to the dynamics of treatment effect using event study graphs. Figures 5 and 6 show the dynamic effect of treatment on each of our 10 outcome variables with a horizon of 5 years pre/post treatment. The most notable outcomes are the large and persistent declines in operating revenue (turnover) and employment in the years after being added to the Entity List. After year-one declines of 35% and 17% respectively, the effect builds to 82% and 64% five years post treatment.[33]

At first consideration, the large declines in revenue and employment may seem puzzling for an export control policy that is by definition impacting the firm's ability to source inputs and not the demand they face, but we propose three explanations. First, if we assume firms are operating in a monopolistically competitive environment where their demand is a function of product quality, then the loss of U.S. origin components (without quality substitutes from international markets) could reduce their product quality and therefore demand, leading to shrinking revenue and employment. Second, a firm being added to the Entity List could act as a signal of risk to consumers and they could shift purchases to non-listed firms. Similar to the first case, this would result in reduction in demand for products from the listed firm and lead to falling revenue and employment. Third, the export control could limit the firms' ability to produce and therefore fill orders. In this case, the firm faces the same demand but their production becomes less efficient, leading to contractions in revenue and employment.

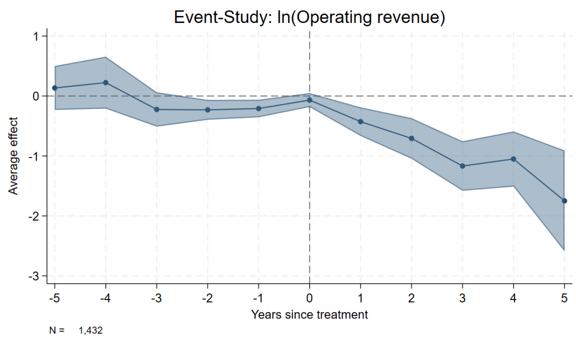
Figure 6 shows the dynamic effect of the policy on different classes of assets and liabilities, with strong negative impacts on each. Intangible assets likely fall due to lost

[32] We measure tangible fixed assets as an outcome but do not find a statistically significant effect.

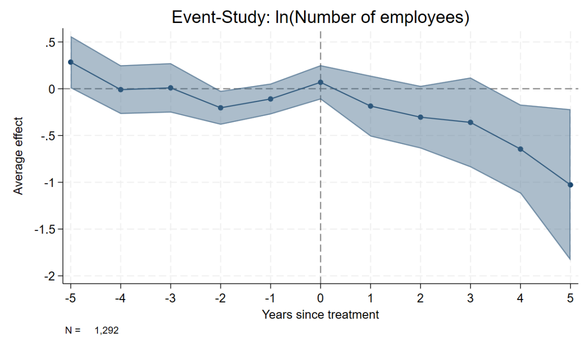
[33] Note that the firms with five years of post treatment data are those who were added to the Entity List prior to 2020 and therefore do not include the large expansion of listed firms that occurred since 2020.

Figure 5: Event-Study Results: ATE of Export Controls on Chinese Firms

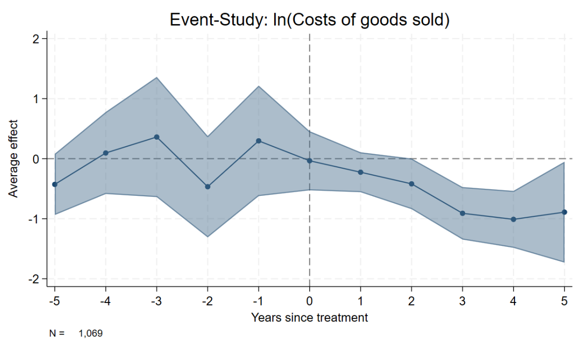
(a) log Operating Revenue



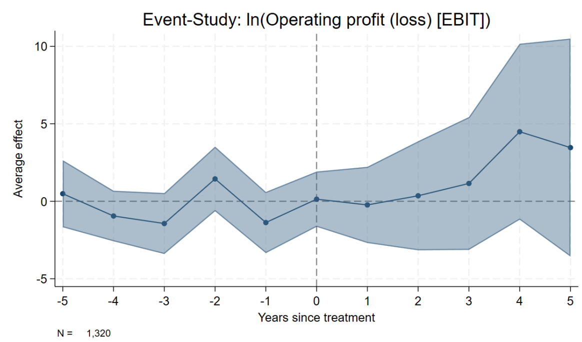
(b) log Employment



(c) log Costs

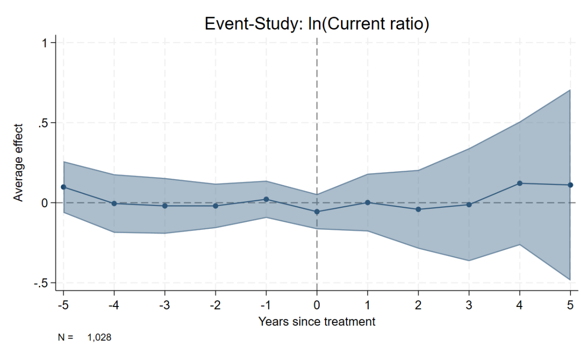


(d) log Profit



(e) log Solvency Ratio

(f) log Current Ratio

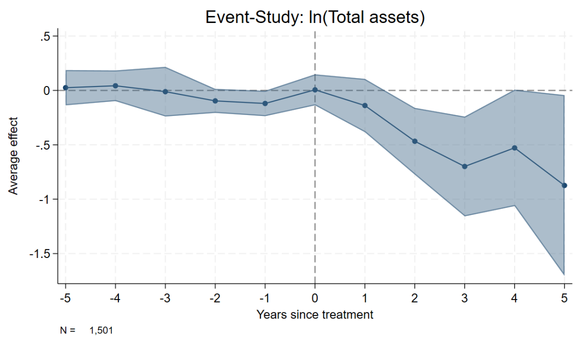


Note: Data from Orbis.

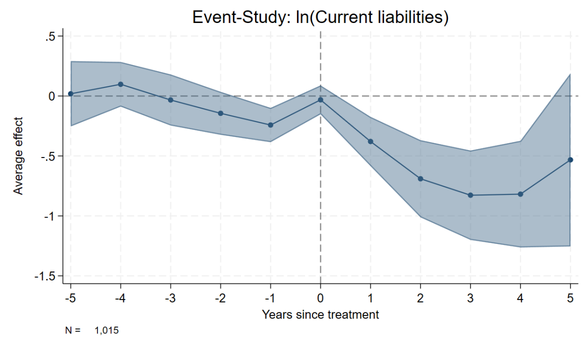
relationships with U.S. suppliers and diminished value of intellectual property without U.S. technological components. We hypothesise that the recovery of intangible assets in years four and five post treatment is driven by increased patent activity as was shown

Figure 6: Event-Study Results: ATE of Export Controls on Chinese Firms

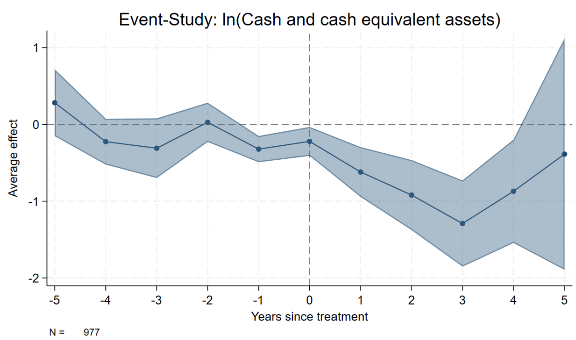
(a) Total Assets



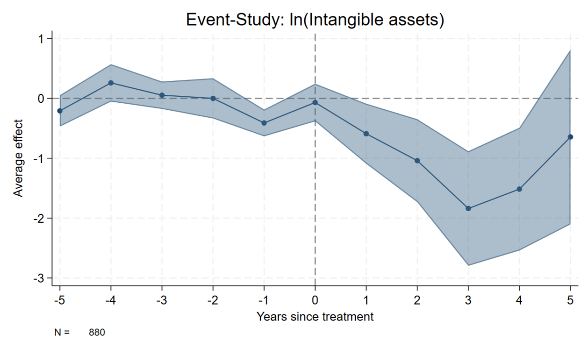
(b) Current Liabilities



(c) Cash Assets



(d) Intangible Assets

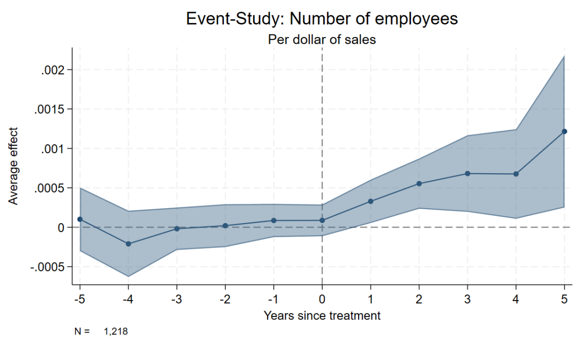


Note: Data from Orbis.

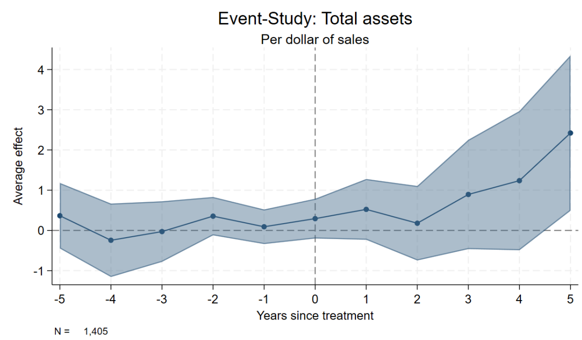
in Shen et al. (2024) and Liu et al. (2024). Cash assets also fall in the first three years following treatment before slightly recovering. The reduction in cash assets is expected as firms are forced to use their most liquid assets to pay down ongoing expenses and fixed/sticky costs (i.e. capital and employment costs). Largely as a function of declining cash and intangible assets, total assets fall in a similar manner. Finally, Liabilities decline in the years following treatment before stabilizing 3 years post treatment. The decline in liabilities is likely driven by the overall contraction of firms following being added to the Entity List, and therefore reduced borrowing. We see no evidence that firms are able to borrow their way out of the export control shock but given the lack of effect on solvency ratio, it also does not appear firms are unable receive credit. Instead, it appears firms are able to borrow less because they have fewer total assets to collateralize loans.

Figure 7: Event-Study Results: ATE of Export Controls on Chinese Firms (per unit of sales)

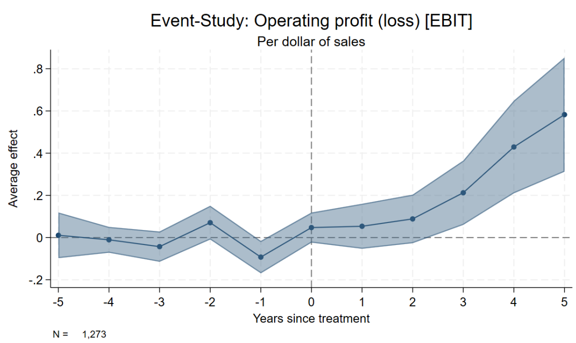
(a) Employment



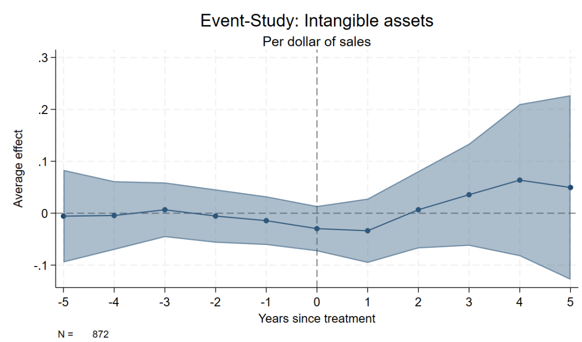
(b) Assets



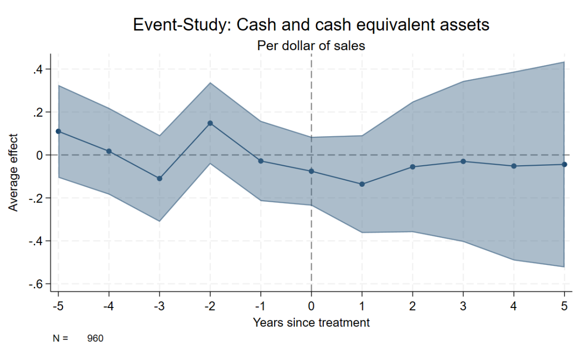
(c) Profits



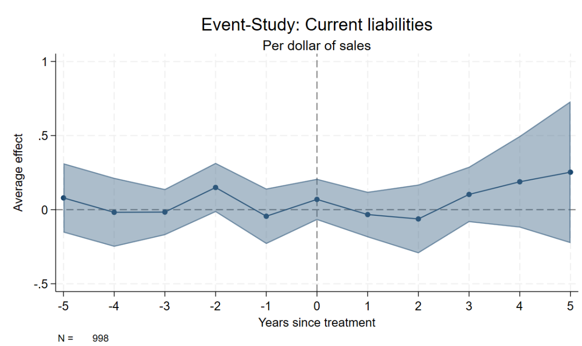
(d) Intangible Assets



(e) Cash Assets



(f) Liabilities



Note: Data from Orbis.

Given that many of our outcome variables are functions of sales, we also perform our analysis on variables that are deflated by total sales. This allows use to determine if the reductions we observe are driven primarily by falling sales or by other adjustment factors. Figure 7 displays these outcomes for variables that have logical interpretations.[34] We see strong positive relationships in employees per dollar of sales and profit margin per dollar of sales, as well as a weak positive relationship in total assets per dollar of sales. These positive relationships here indicate that sales are falling faster than these outcomes (employees, profit margin, and total assets). For intangible assets, cash assets, and liabilities, we detect no significant changes.

The results from Figure 7 suggest firms lose intangible assets in the form of relationships with U.S. suppliers following being named to the Entity List and then use cash assets to cover short term costs as revenue falls. The lack of a detectable change in cash assets per dollar of sales, in conjunction with the strong contraction when looking at log transformed variables, indicates that cash assets are falling at roughly the same pace as sales post treatment. Additionally, the positive effect on employment per dollar of sales in Figure 7 indicates firms retain more workers post treatment and the weakly positive trend in total assets suggests fixed assets are not liquidated at the at a rate proportional the the decline in sales. These outcomes are exactly what we would expect from a firm facing a temporary productivity or supply side shock. The results indicate that firms use their most liquid assets first and resist making costly structural changes such as fully reducing staff or selling fixed capital for as long as possible.

Collectively, the dynamic ATE results obtained from the Callaway and Sant'Anna difference-in-difference estimation show strong negative effects for firms named to the Entity List despite the exact transmission channel remaining unclear (i.e. initiated by a fall in demand, less efficient/inferior production, or a shortage of key components). The results show a lasting effect that firms are largely unable to mitigate over the five year post treatment horizon, leading to smaller and likely less efficient treated firms.

[34] Operating revenue, solvency ratio, and current ratio have been dropped from the analysis.

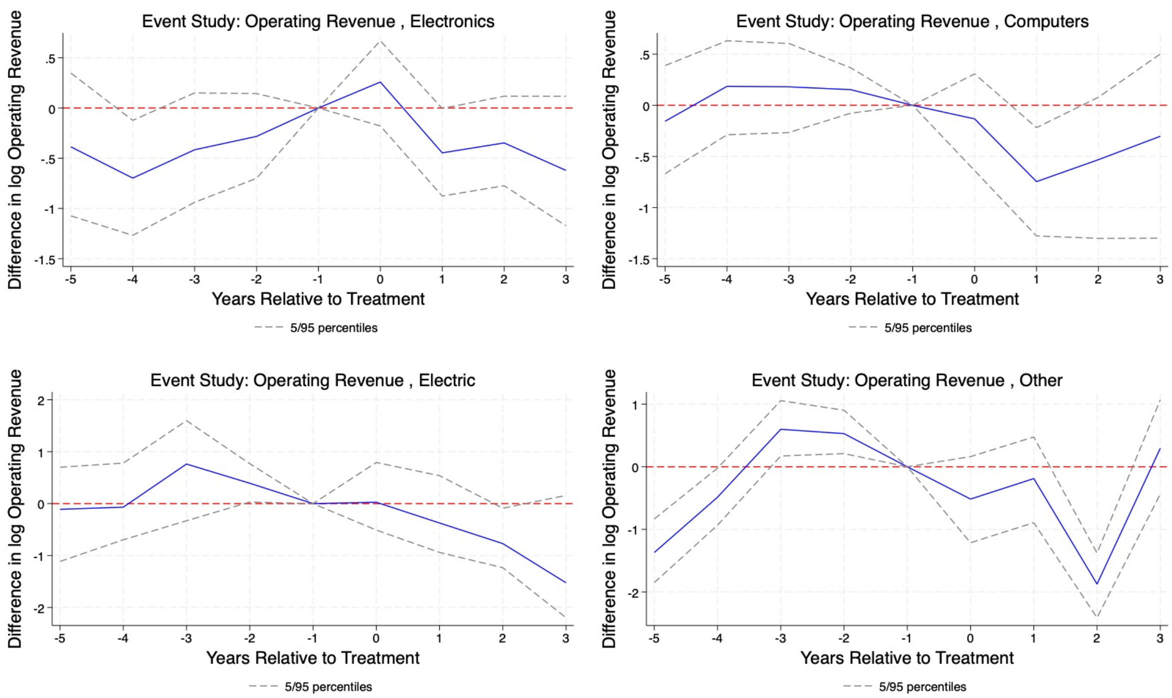
7 Heterogeneous Treatment Effects by Industry

We complement the dynamic ATE results obtained from the Callaway and Sant'Anna difference-in-difference estimation with sub-sample results obtained from a permutation-based approach designed for small samples. Our main goal is to use the latter results to explore possible heterogeneity of the treatment effects across industries. While identifying channels through which treatment drives heterogeneous effects on outcomes across industries is outside the scope of this paper, we do consider that identifying heterogeneous effects can shed light on which sectors are more exposed, how easily firms in different industries can adjust input sourcing and production, and hypothesize on the role of government protection for some industries. In addition, this information is useful for highlighting where the influence of BIS actions is likely to be strongest.

It is worth noting that the two estimands need not yield similar results, even if used on the same overall sample. This is because they aggregate heterogeneous firm-level effects over different target populations and with different weighting schemes: the Callaway and Sant'Anna estimand averages effects across all treated groups and cohorts in the sample, with weights that reflect the relative size and timing of each group, so it is naturally tilted toward the groups that are more numerous or observed over more post-treatment periods. By contrast, the permutation-based estimand averages effects only for treated firms that have close matches in the chosen sub-sample and weights those firms according to their similarity to specific controls, giving relatively more influence to high-quality comparisons. When treatment effects vary systematically with firm characteristics or industry, these differences in the underlying target populations and in the implicit weighting schemes can produce distinct numerical estimates, even though each estimator consistently recovers its own well-defined parameter.

Figure A.1 in the Appendix shows that the overall results stemming from the permutation-based estimator follow the findings reported by the Callaway and Sant'Anna difference-in-difference. Namely, they show that treated firms' operating revenue, employment, costs, total assets, current liabilities, cash holdings and the value of intangible assets fall relative to their counterfactual. Similarly, overall per-revenue results in Figure A.2 broadly follow the direction of the Callaway and Sant'Anna difference-in-difference results shown in Figure 5.[35]

Figure 8: Exact Permutation: ATE on Operating Revenue of Export Controls on Chinese Firms By Industry



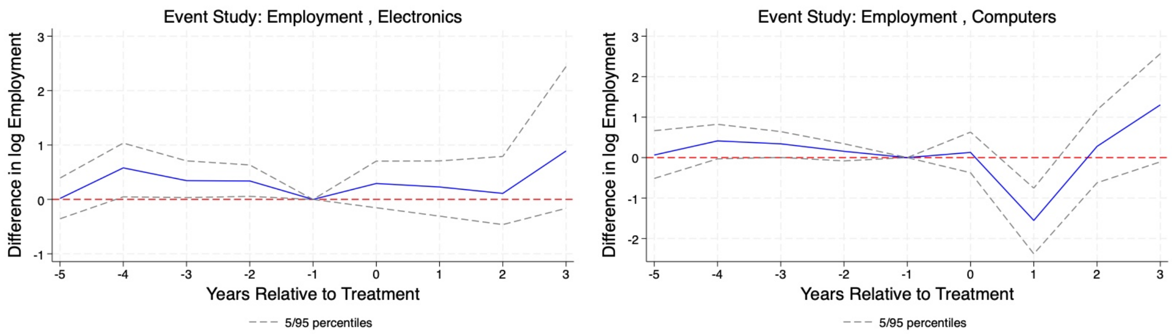
Note: Data from Orbis.

When using the permutation-based method to inquire about heterogeneous effects by industry, we find evidence that the fall in revenue occurred across the board. Figure 8 show that the trajectory of the operating revenue of firms changed after being added to the BIS list, especially among firms in the electronics and computer industries. The negative structural break appear to materialize faster in these industries than among firms in the remaining industries.

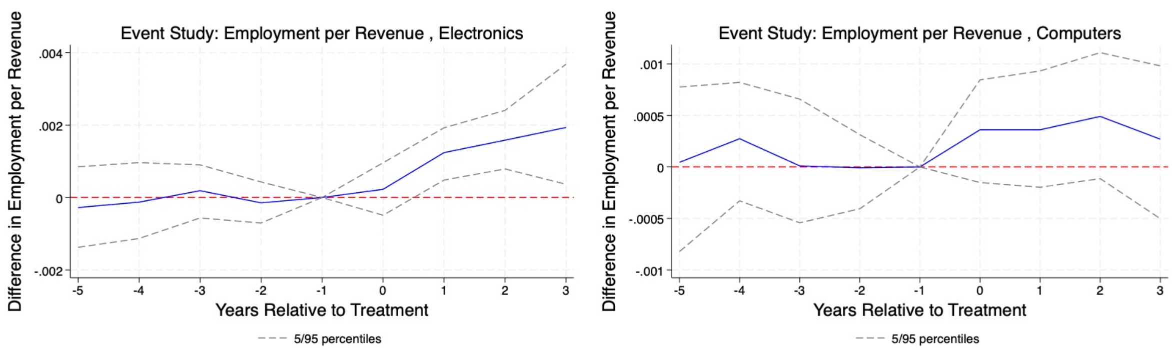
[35]While in the main results section, we report five post-treatment periods, in this section we cut our graphs at three post-treatment periods. We do so because this section is geared towards producing sub-sample estimates and the attrition we observe after post-treatment year 3 prevents us from having enough firms in the subsamples we analyze.

Figure 9: Exact Permutation: ATE on Employment of Export Controls on Chinese Firms By Industry

(a) Log of Employment



(b) Employment by Revenue



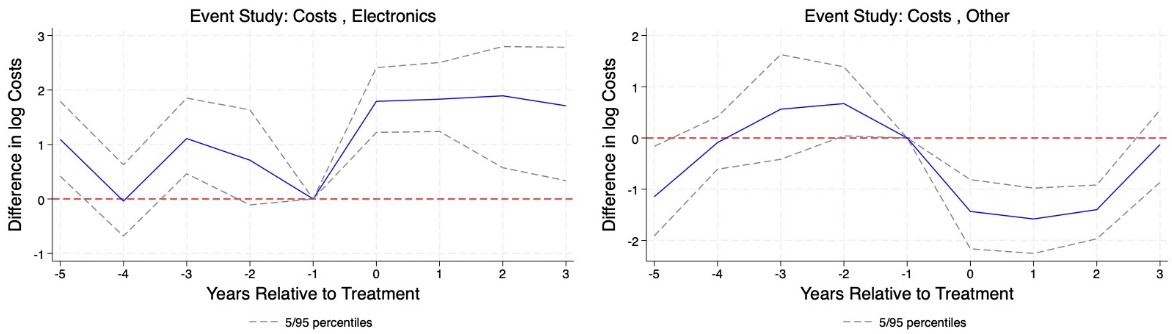
Note: Data from Orbis.

Unlike revenue, the employment losses did not occur across the board. Our results in Figure 9 indicate that the employment levels in computer industry firms dropped significantly one year after treatment. That drop was not mirrored by the average firm in other industries. In conjunction with the effects on revenue, the results in Figures 9b show that the drop in employment in the computer industry tracked the loss of revenue leaving the employment per revenue metric almost unchanged after treatment. That contrasts with the increase in employment per revenue among electronics firms.

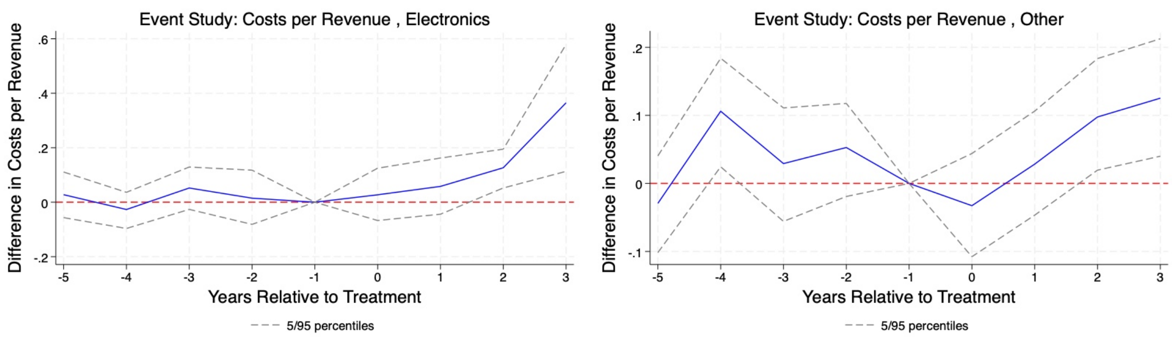
Cross-industry analyses also indicate that the overall drop in costs after treatment we observe is driven by the firms outside electronics, computer and electric industries. In fact, in Figure 10, we see that the costs faced by firms in electronics increase significantly after treatment. Cost-by-revenue, on the other hand, grew for both types of firms.

Figure 10: Exact Permutation: ATE on Costs of Export Controls on Chinese Firms By Industry

(a) Log of Costs



(b) Costs by Revenue



Note: Data from Orbis.

Finally, we find that firms in industries outside electronics and computers tend to be the one driving the overall effects we observe on assets and liabilities. We do not observe statistically significant changes in total assets, intangible assets, or current liabilities among firms in electronics and computer industries.

8 Policy Implications

The results in this paper highlight two main conclusions for policy makers. First, the BIS Entity List appears to impose material and lasting costs on targeted Chinese firms, even in an environment where substitution to non-U.S. suppliers, domestic industrial policy, and evasion are all plausible (Crosignani et al., 2025). This finding challenges the view that export controls rapidly become toothless as global supply chains adjust, and it supports the notion that U.S. technological centrality continues to translate into leverage at the firm level.[36]

Second, the pattern of results suggests that Entity List designations primarily work by reducing listed firms' size.[37] From a policy perspective, this is consistent with the stated goal of many U.S. controls: to slow the diffusion and operational capacity of sensitive firms without triggering widespread systemic instability.[38]

Third, our results highlight the heterogeneous impacts of the BIS Entity Listings across different Chinese industries. Our results found that listed firms across all industry types saw revenue fall after listing, but that employment fell, and costs rose, in the Computer and Electronics industries suggesting that the Entity List may be more impactful in these industries. While these findings deserve further exploration, they suggest that U.S. policy is raising the costs of production for listed firms in these industries likely by restricting their access to key inputs or software. Future work should explore the fundamental drivers of these differences in impacts across industries, reassessing prioritization, multilateral coordination, and periodic reassessment of the Entity Listing's impacts as they could evolve over time.

[36] It is worth noting that our analysis does not consider the general equilibrium impacts that Entity Listings might have on Chinese firms.

[37] Our sample does not include firms that exit the market after being listed. Orbis does not allow U.S. to distinguish whether a firm exits the market or turns domestically. Therefore our analysis cannot conclude whether the Entity List leads to listed entities going out of business.

[38] Each Entity List addition is published with a specific reason. Often these are very generic – most are to prevent PRC military modernization.

Before concluding it is important to point out two important caveats about our analysis in the context of the public debate surrounding export controls in recent years. Our results say nothing about stopping the diffusion of U.S. technology to the PRC. Our results highlight the impacts of the Entity List on PRC firm financials, but stop short of estimating the relationship between these financials and the listed firms' ability to innovate. There is public debate around the impact of export controls on innovation;[39] our results do not speak to this debate directly as we are focused on the financial impacts of the Entity List on listed firms. Second, our analysis does not explore the impacts of commodity-based export controls, such as the Commerce-Control List, which has largely been leveraged by the U.S. to restrict China's access to AI and semiconductor technologies and has received substantial media coverage in recent years (Allen and Goldston, 2025). Our analysis, rather, is focused on the impacts of a type of end-user export control in the Entity List. Nevertheless, U.S. use of export controls naming Chinese end-users has increased over the past decades, making our analysis relevant to an important piece of the current U.S. export control regime.

9 Conclusion

This paper has provided the first firm-level evidence on how U.S. export controls, implemented through the BIS Entity List, affect the financial performance of Chinese firms. using a large panel of public and private PRC firms and data on BIS designations, we find that being added to the Entity List is associated with large, persistent contractions in firm size, but without commensurate deterioration in standard balance-sheet measures of solvency. Our baseline results show that listed firms experience sharp and durable declines in operating revenue, employment, total assets, cash holdings, and current liabilities. The magnitudes are economically substantial: on average, treated

[39] Winter-Levy (2024) outlines the broad camps in this debate within the context of U.S. foreign policy.

firms lose on the order of one-half of their pre-treatment operating revenue and cash assets, and roughly one-third of their employees and total assets in the years following designation. At the same time, solvency and current ratios remain statistically unchanged, and we do not find clear evidence of heightened distress on these dimensions for the surviving firms in our sample. Viewed together, the evidence suggests that Entity List designations force firms to shrink and rebalance their balance sheets, rather than pushing them immediately into insolvency.

Event-study estimates and revenue-normalized outcomes indicate that firms respond by drawing down liquid assets. Intangible assets also contract sharply in the early post-treatment years, consistent with the destruction of relationships with U.S. suppliers and the loss in value of technologies that rely on U.S.-origin components. Over longer horizons, we observe a modest recovery of intangibles, which is in line with complementary evidence that export controls can spur domestic R&D and patenting activity among Chinese firms. Our results thus reconcile a short- to medium-run picture of financial contraction with the possibility of longer-run innovation responses emphasized in recent work. Our analysis further reveals that these average effects mask meaningful heterogeneity across industries. Firms in electronics- and computer-related sectors experience particularly pronounced and rapid revenue losses following listing, consistent with their heavy reliance on U.S.-origin technology and components. In contrast, firms in other sectors drive much of the observed decline in costs and liabilities, with more gradual adjustments and weaker evidence of sharp post-treatment breaks. At the same time, we do not find strong or systematic post-treatment changes in assets or liabilities among electronics and computer firms, suggesting that some highly exposed firms may be buffered by state support, privileged access to domestic finance, or strategic importance.

Our results underscore that the effectiveness of Entity List designations is uneven across sectors and firm types. Firms in technology-intensive industries that are tightly integrated into U.S.-centric value chains appear especially vulnerable, while firms in other sectors adjust more gradually or along different margins. This heterogeneity implies that the marginal impact of additional listings is likely to depend on the technological and network position of the targeted firm, and that one-size-fits-all expectations about the consequences of export controls are unwarranted.

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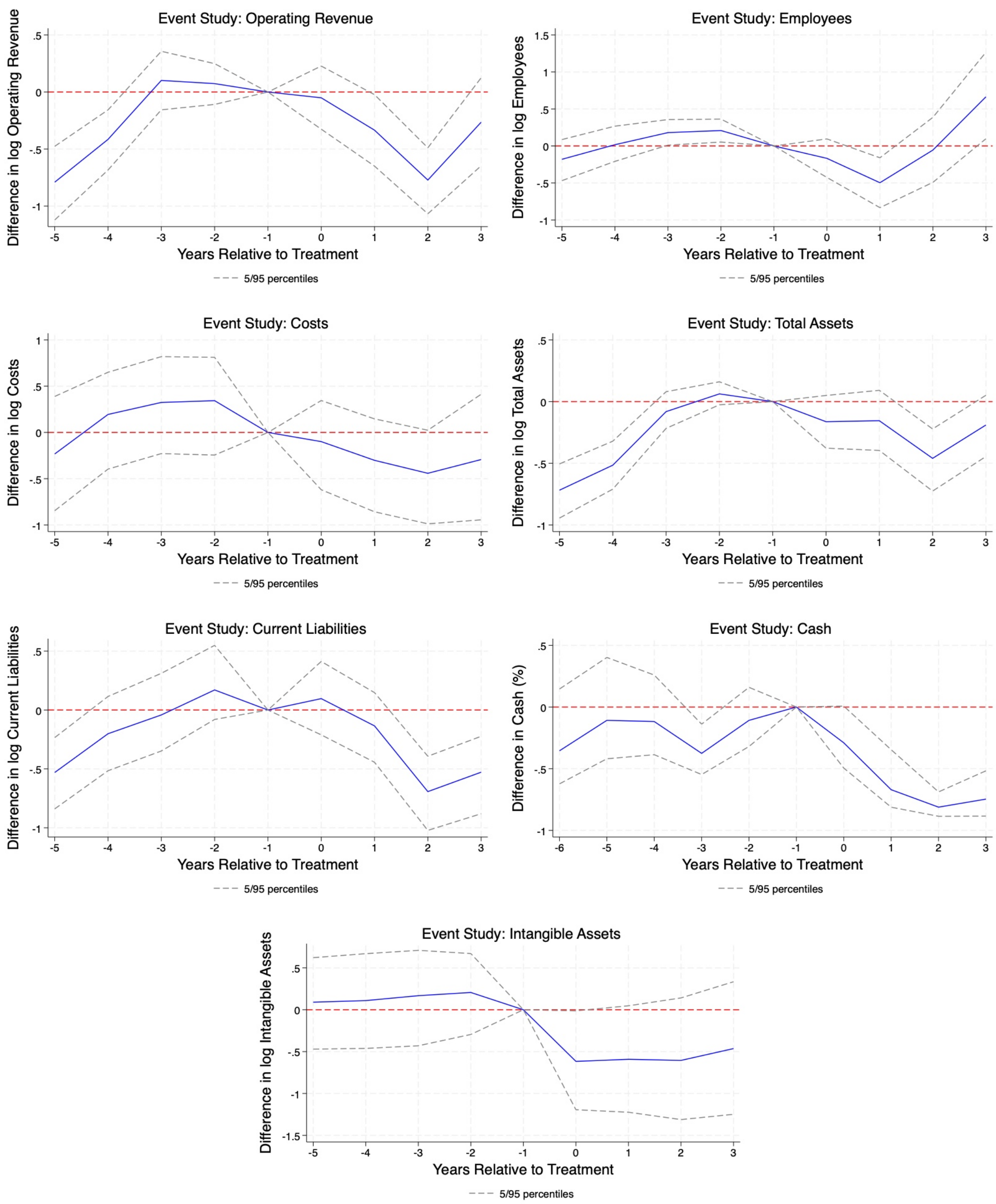
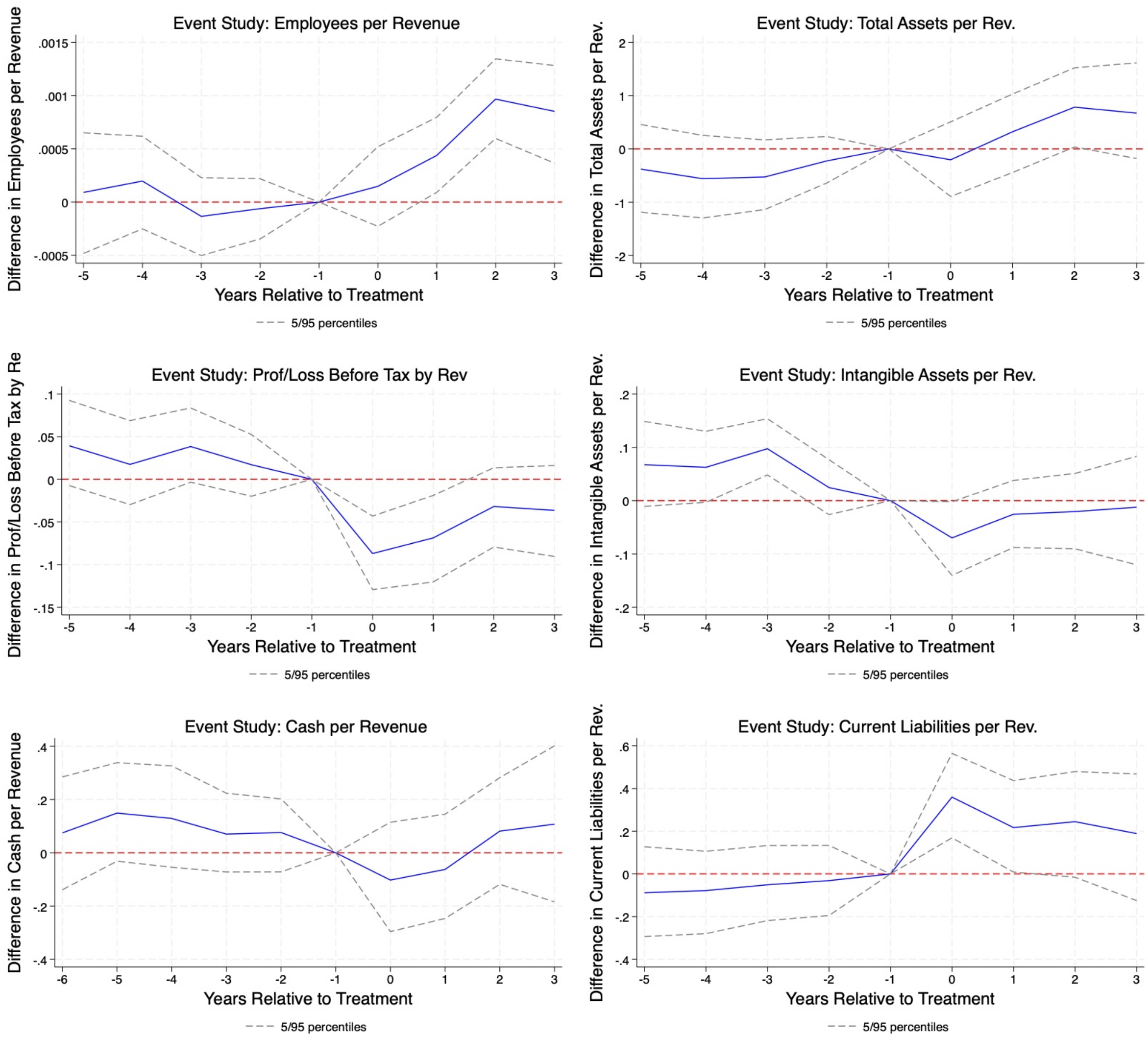
Appendix

Figure A.1: Exact Permutation: ATE of Export Controls on Chinese Firms



Note: Data from Orbis.

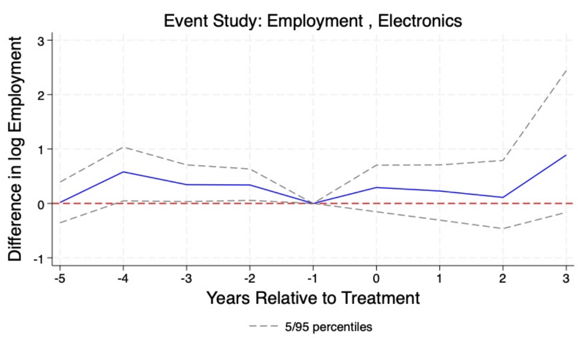
Figure A.2: Event-Study Results: ATE of Export Controls on Chinese Firms (per unit of sales)



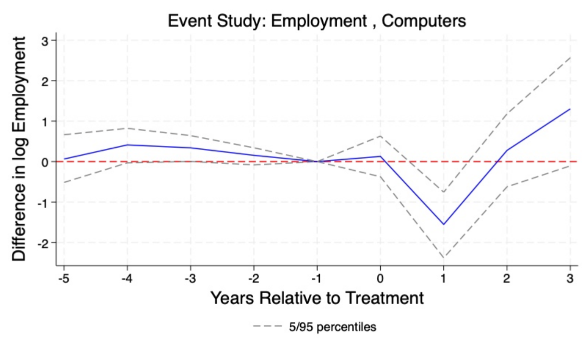
Note: Data from Orbis.

Figure A.3: Exact Permutation: ATE on Employment of Export Controls on Chinese Firms By Industry

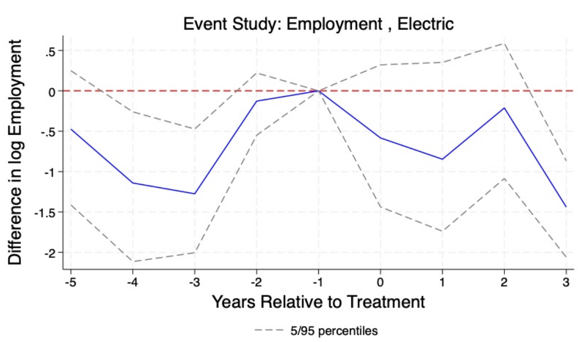
(a)



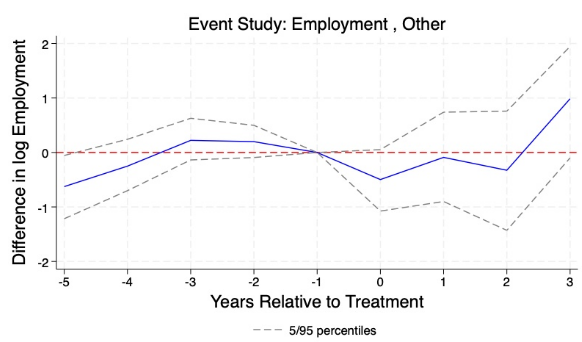
(b)



(c)

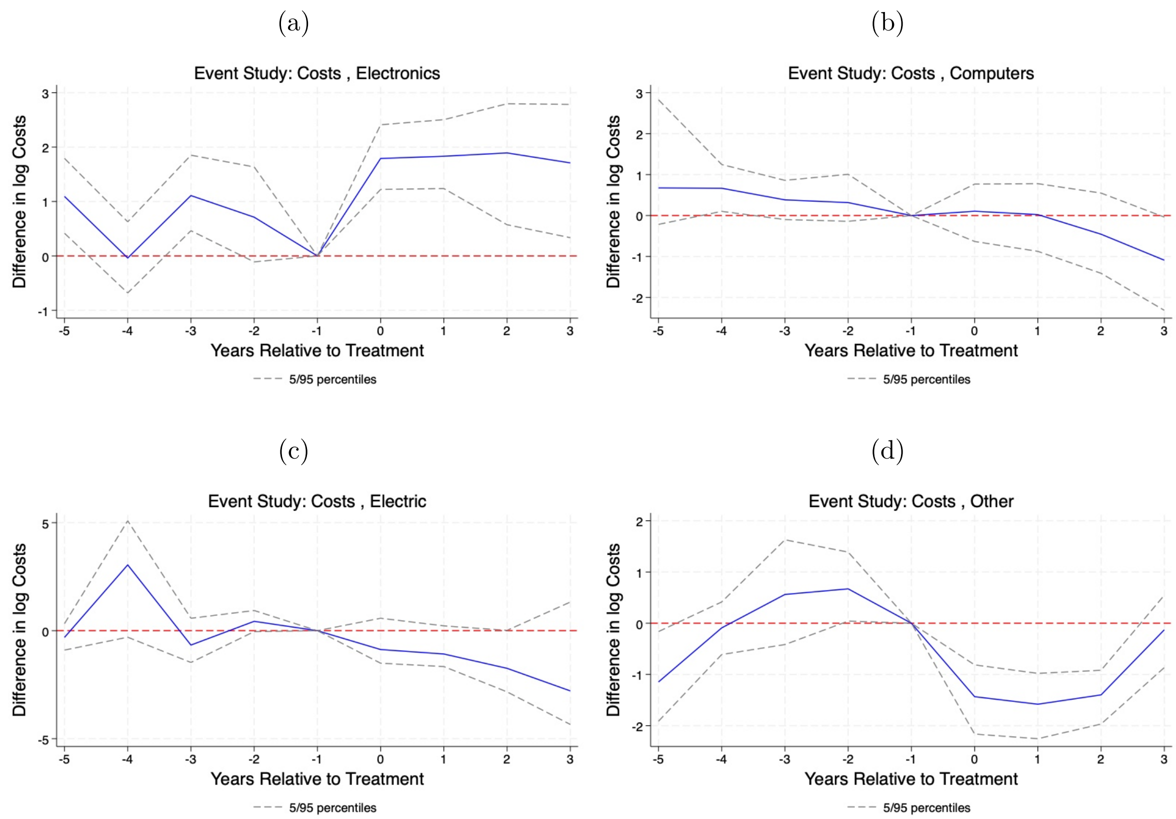


(d)



Note: Data from Orbis.

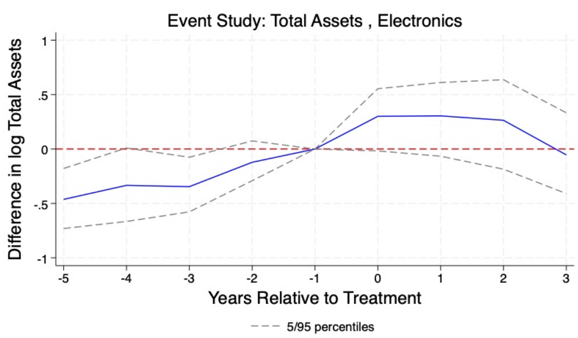
Figure A.4: Exact Permutation: ATE on Costs of Export Controls on Chinese Firms By Industry



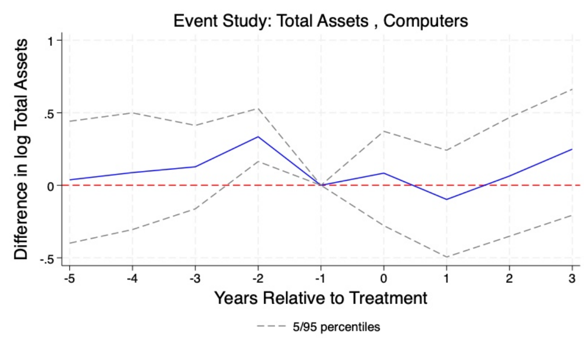
Note: Data from Orbis.

Figure A.5: Exact Permutation: ATE on Total Assets of Export Controls on Chinese Firms By Industry

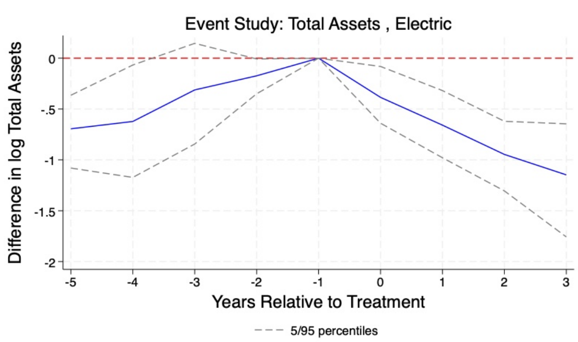
(a)



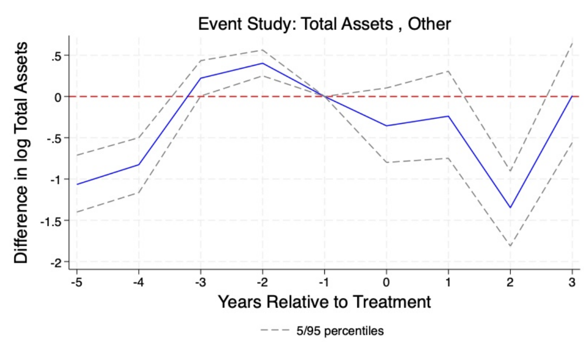
(b)



(c)



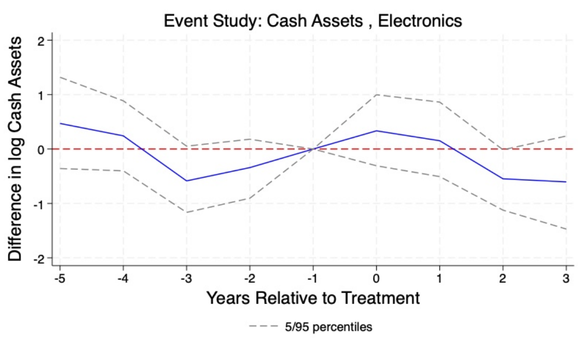
(d)



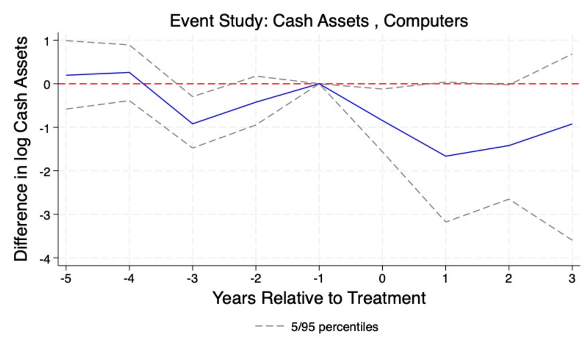
Note: Data from Orbis.

Figure A.6: Exact Permutation: ATE on Cash Assets of Export Controls on Chinese Firms By Industry

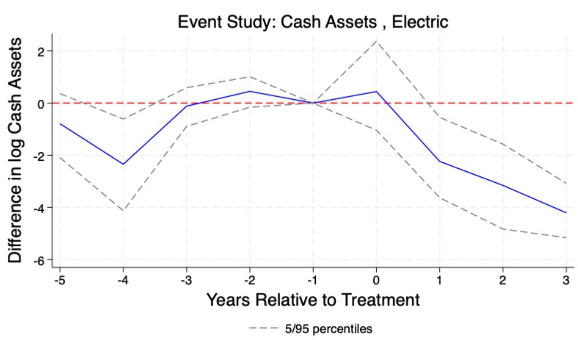
(a)



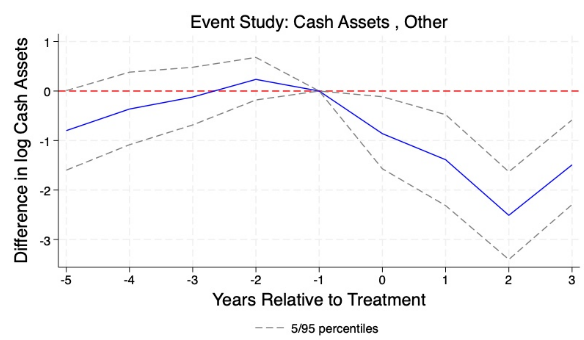
(b)



(c)



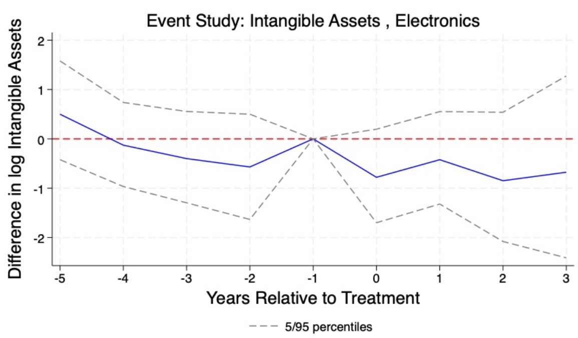
(d)



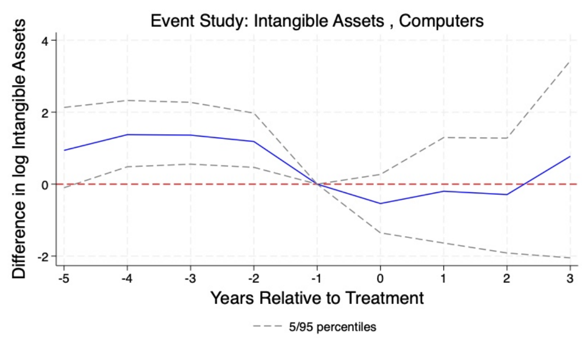
Note: Data from Orbis.

Figure A.7: Exact Permutation: ATE on Intangible Assets of Export Controls on Chinese Firms By Industry

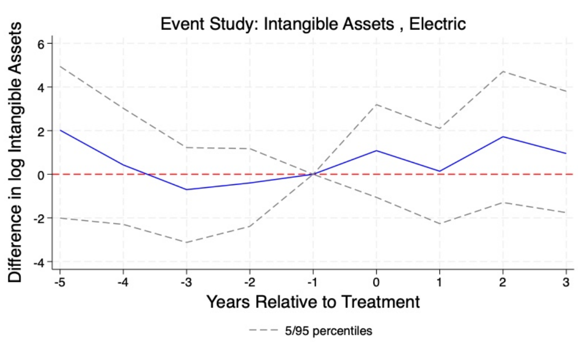
(a)



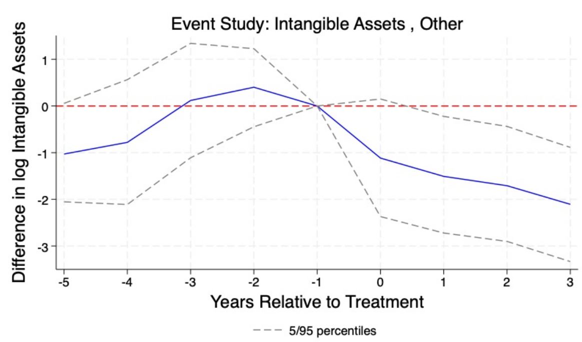
(b)



(c)



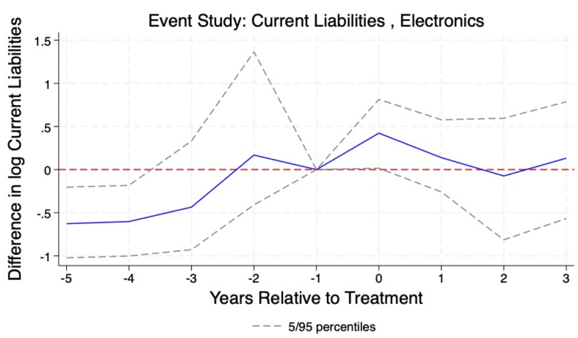
(d)



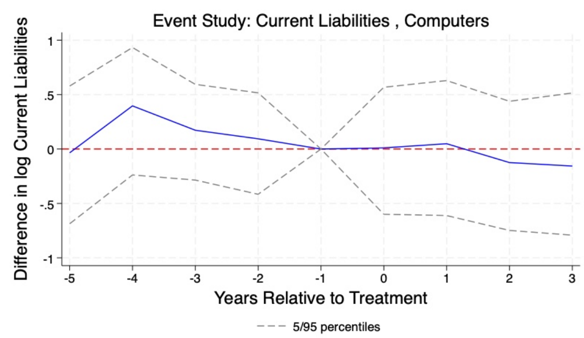
Note: Data from Orbis.

Figure A.8: Exact Permutation: ATE on Current Liabilities of Export Controls on Chinese Firms By Industry

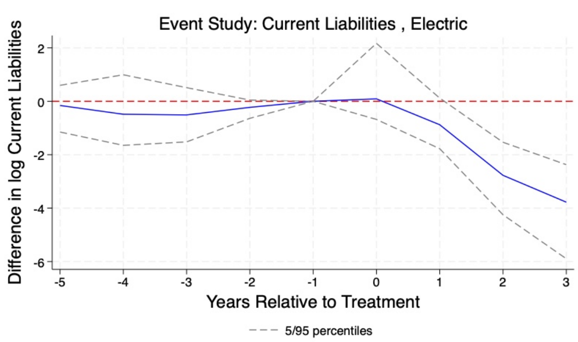
(a)



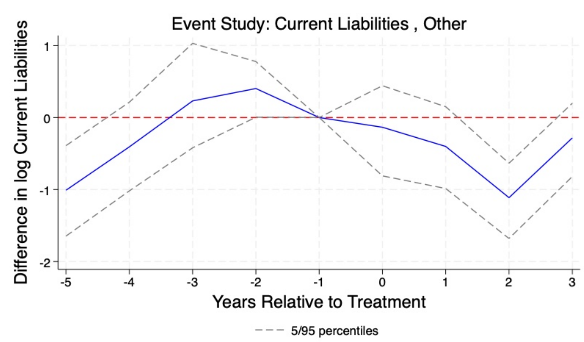
(b)



(c)



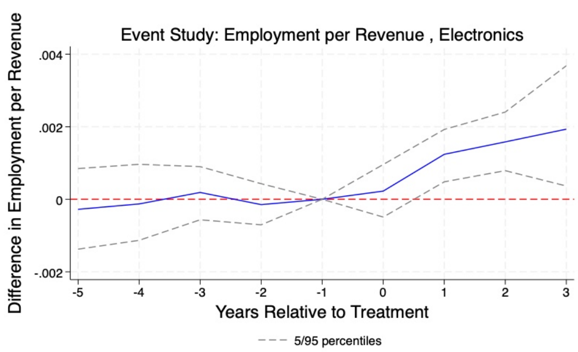
(d)



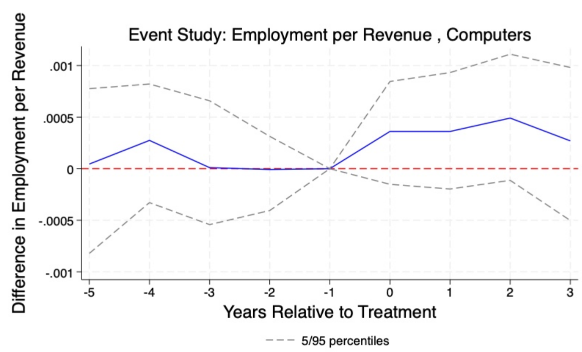
Note: Data from Orbis.

Figure A.9: Exact Permutation: ATE of Export Controls on Chinese Firms on Employment per Revenue By Industry

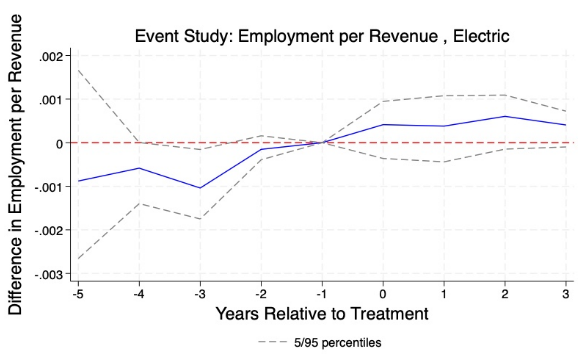
(a)



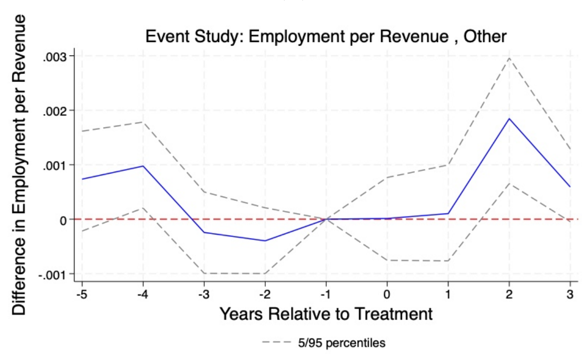
(b)



(c)



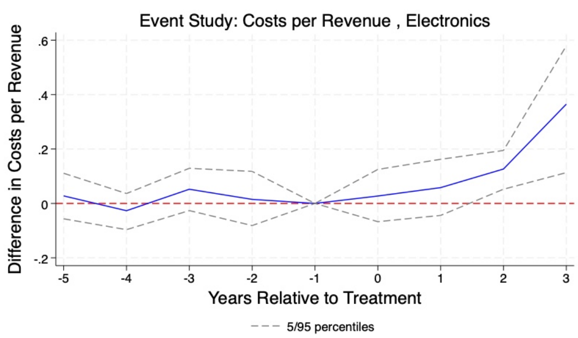
(d)



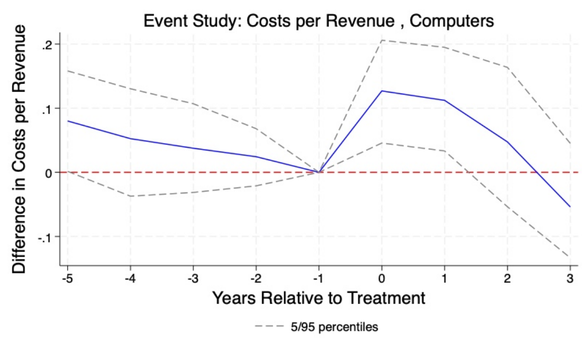
Note: Data from Orbis.

Figure A.10: Exact Permutation: ATE of Export Controls on Chinese Firms on Costs per Revenue By Industry

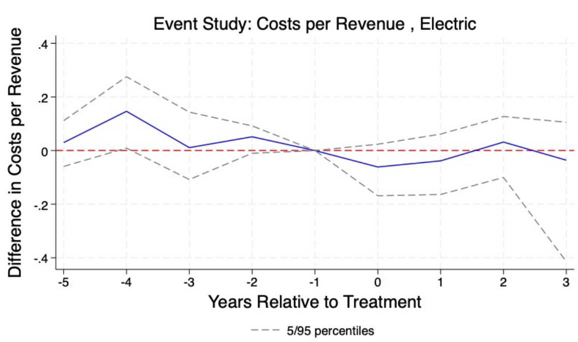
(a)



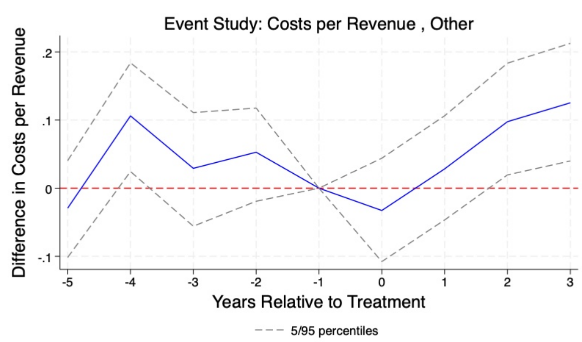
(b)



(c)



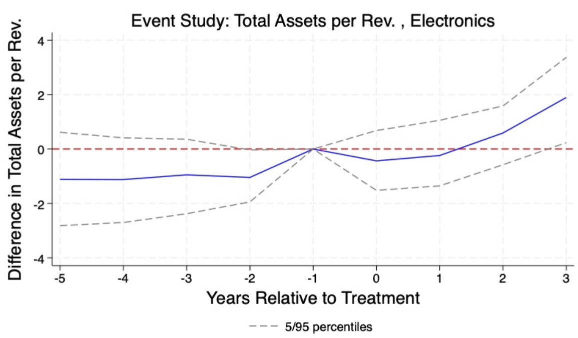
(d)



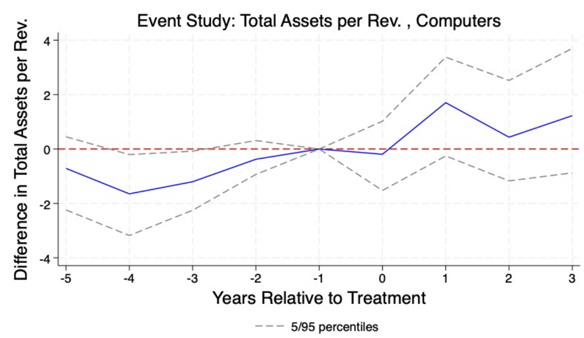
Note: Data from Orbis.

Figure A.11: Exact Permutation: ATE of Export Controls on Chinese Firms on Total Assets per Revenue By Industry

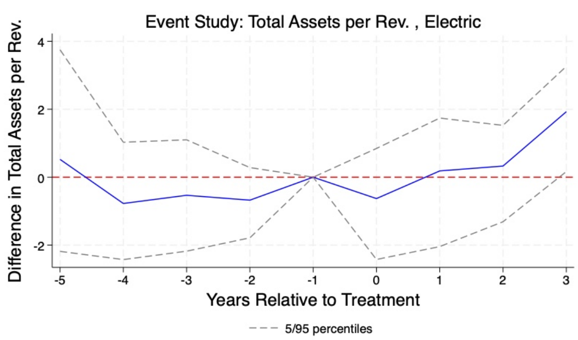
(a)



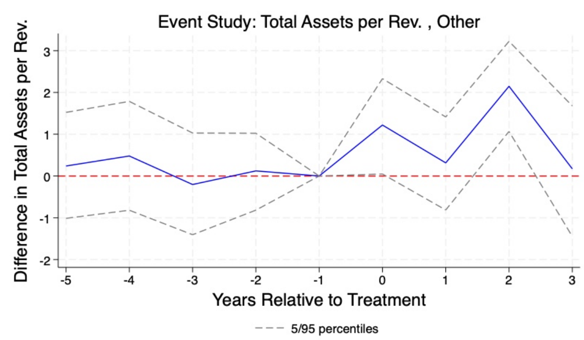
(b)



(c)



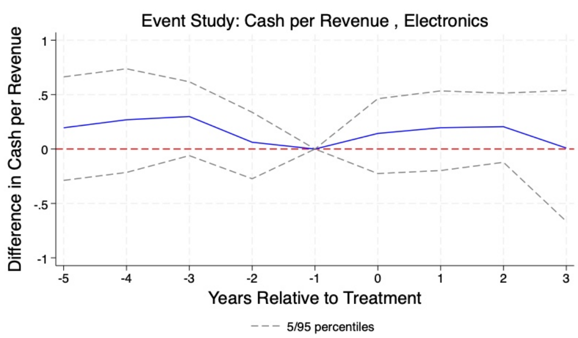
(d)



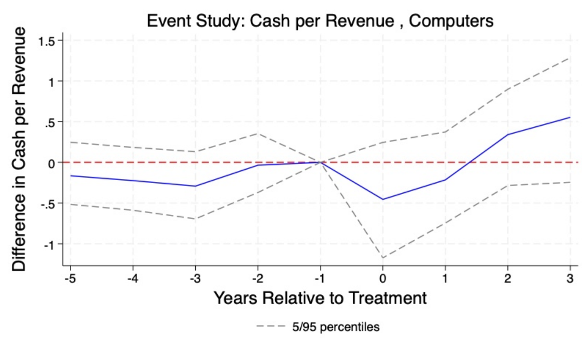
Note: Data from Orbis.

Figure A.12: Exact Permutation: ATE of Export Controls on Chinese Firms on Cash Assets per Revenue By Industry

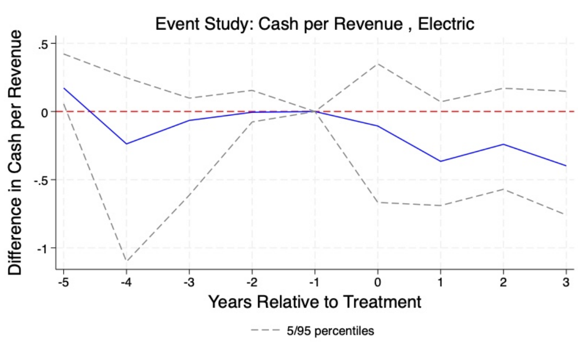
(a)



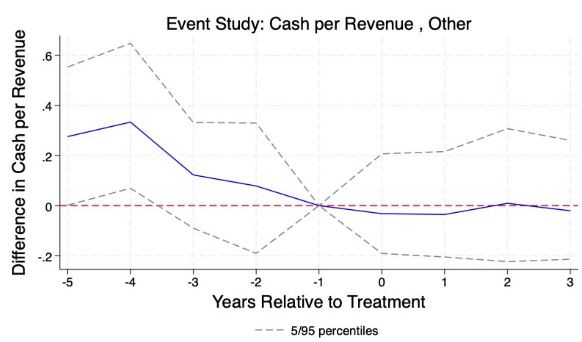
(b)



(c)



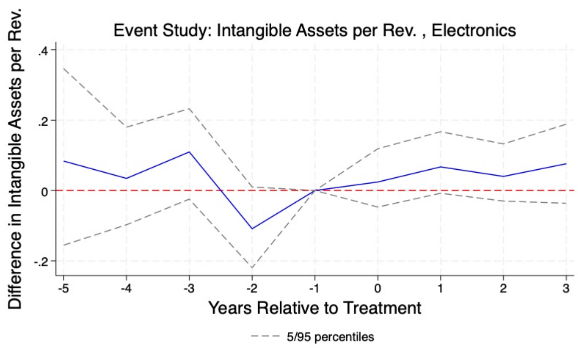
(d)



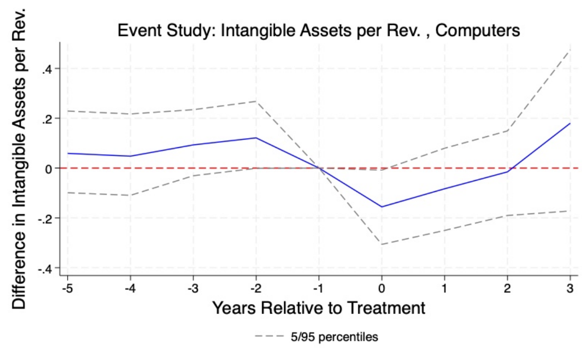
Note: Data from Orbis.

Figure A.13: Exact Permutation: ATE of Export Controls on Chinese Firms on Intangible Assets per Revenue By Industry

(a)



(b)



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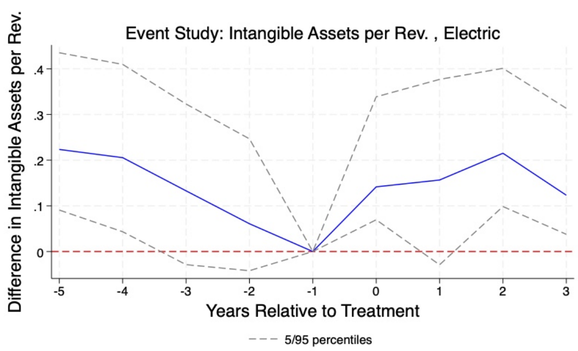
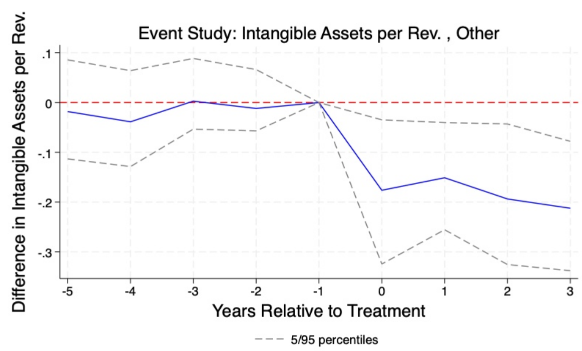


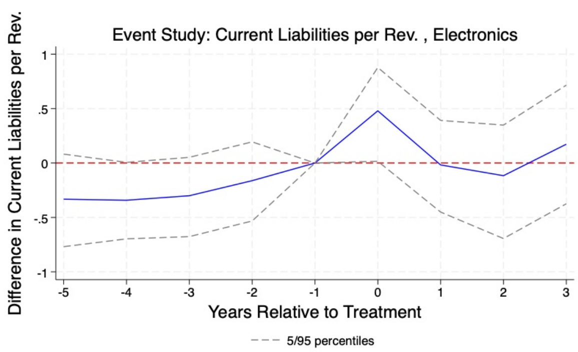
(d)



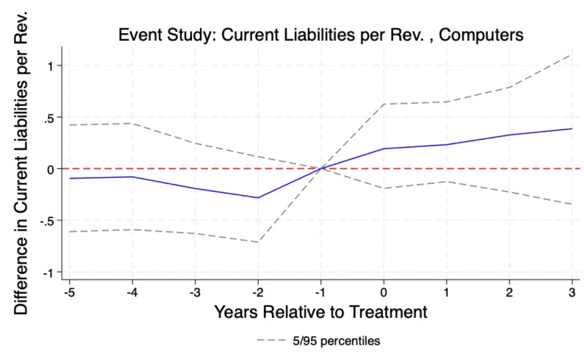
Note: Data from Orbis.

Figure A.14: Exact Permutation: ATE of Export Controls on Chinese Firms on Current Liabilities per Revenue By Industry

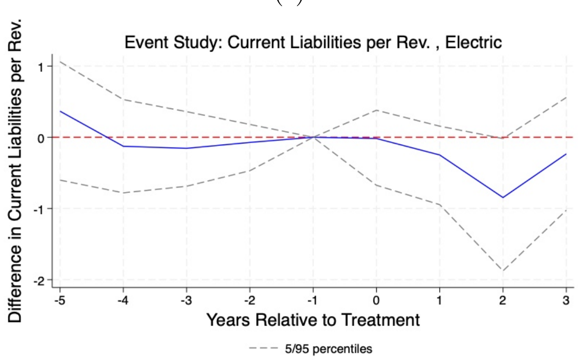
(a)



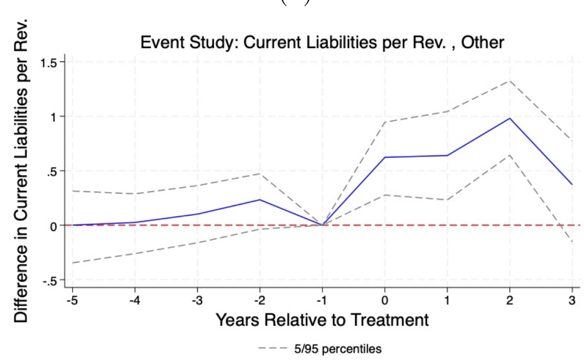
(b)



(c)



(d)



Note: Data from Orbis.