

Better Safe than Sorry: Sanctions and Firm Overcompliance

Eric Jeong¹ Yajie Wang²

September 28, 2026

Abstract

Economic sanctions are a central instrument of economic statecraft, yet they can prompt firms to abandon trade that governments remain willing to permit. We develop a theory of precautionary overcompliance in which unequal access to credible political information shapes firms' ability to infer the state's enforcement resolve. When officials permit continued trade but their intentions remain uncertain, firms with weaker political standing may abandon activities beyond formal prohibitions. Better information can preserve commerce in this setting or encourage withdrawal when officials intend broader disengagement. We examine these implications using monthly Chinese customs records following North Korea's 2006 nuclear test and subsequent UN sanctions. Comparing firms' product-level exports to North Korea with exports to other destinations, we find that private firms withdraw more than state-owned enterprises (SOEs). This ownership gap persists after excluding products plausibly covered by formal sanctions and extends to food and humanitarian categories. Comparative cases of economic coercion involving China support the argument that clearer information about the government's enforcement resolve can encourage SOEs to comply more fully with demands to withdraw from trade. Our argument explains how unequal political access within states shapes the international effects of economic statecraft.

¹Perry World House, University of Pennsylvania. E-mail: kwjeong@upenn.edu

²Division of Global and Area Studies, Chinese University of Hong Kong, Shenzhen. E-mail:yajiewang@cuhk.edu.cn

1 Introduction

Economic sanctions are a central instrument of economic statecraft, allowing governments to turn cross-border commercial and financial ties into sources of political pressure (Farrell & Newman, 2019). Their economic impact depends in turn on how firms respond. Firms facing the same trade restrictions may nevertheless respond quite differently: some have narrower targeted responses while preserving other commercial ties, whereas others withdraw more broadly from the target market (Stępień et al., 2024; Wellhausen & Zhu, 2026). Such overcompliance can magnify the economic impact of sanctions beyond what governments intend to impose (Verdier, 2023). These divergent responses shape which commercial ties survive and how widely the costs of economic coercion spread. Why do some firms sustain trade while others withdraw beyond formal requirements?

If firms differ in their ability to infer the state’s enforcement intentions, their access to credible political information should help explain variation in compliance behavior. We identify *political standing*, which we define as a firm’s position in the relationships linking business and government, as a critical factor shaping this ability (Haveman et al., 2017; Hillman, 2005; Wei et al., 2023). When public communication does not fully reveal the state’s enforcement resolve, firms lacking access to credible information may withdraw beyond the formal restrictions rather than risk undercompliance. We refer to such behavior as *precautionary overcompliance*. Our argument builds on the growing literature on uncertainty-driven explanations of overcompliance (Smith & Poplin, 2023; Stępień et al., 2024; Verdier, 2023) by theorizing how unequal access to political information generates systematic variation in firms’ responses to the same sanctions.

Our theory rests on information asymmetry between sanction-implementing policymakers and firms. The same publicly available information may be consistent with either broader enforcement or a more permissive stance, leaving firms uncertain about the state’s true resolve. In such settings, firms with stronger political standing are more likely to obtain credible information about what officials intend to enforce. Holding commercial costs and protection constant, better information can therefore move behavior in either direction. When officials permit continued activity, political access can reduce precautionary withdrawal by providing credible reassurance. When policymakers instead intend to enforce broader disengagement, the same informational advantage can increase compliance by providing credible warning. This mechanism operates when the government can make defiance costly but firms remain uncertain about the scope of enforcement (Becker, 1968). Political access should matter especially where credible information is restricted and public communication provides only an incomplete guide to implementation (Haveman et al., 2017; Hollyer et al., 2011).

We test these predictions using the universe of Chinese exporting firms’ customs records following North Korea’s first nuclear test in October 2006 and the subsequent UN Security Council Resolution 1718. This context is particularly suitable for our theory because the intended scope of Chinese enforcement beyond the formal sanctions was ambiguous. Following the nuclear test, China supported targeted sanctions while seeking stability on the Korean Peninsula, and contemporaneous Chinese statements affirmed that normal trade could continue (Ministry of Foreign Affairs of the People’s Republic of China, 2006b). This diplomatic context makes a limited response plausible, but the public assurances alone do not establish whether officials would have permitted particular transactions in practice. We use monthly firm–product–destination customs records to compare each

firm's product-level exports to North Korea with its contemporaneous exports of the same products to other countries. A triple-difference design then examines whether adjustment varies with firms' political standing, which we proxy using state ownership.

Our findings show that private firms withdraw from North Korea substantially more than state-owned enterprises (SOEs), in both positive-trade probabilities and export values. At the extensive margin, the post-shock decline in the probability of positive trade with North Korea relative to comparison destinations is approximately 4.9 percentage points smaller for SOEs than for private firms. For export values, private firms exhibit a relative decline of approximately 28.5 percent on the transformed outcome scale, while the corresponding SOE response is indistinguishable from zero. We then examine whether this divergence extends beyond formally restricted commerce by mapping the UN sanctions documents onto four-digit product codes. The ownership gap persists even after excluding products plausibly covered by the sanctions and extends to food and humanitarian goods. These patterns are consistent with precautionary overcompliance among private firms rather than adjustment confined to the formal scope of the sanctions. We further examine the argument in comparative settings where the informational environment surrounding enforcement differs. We consider China's response to official Dalai Lama receptions, where the government's demand for commercial retaliation was comparatively clearer, and UN sanctions against Iran, which more closely resemble the North Korea setting. The ownership patterns differ across these cases: SOEs withdraw relatively more following Dalai Lama receptions but relatively less under the Iran sanctions, as they do following the North Korea sanctions. We also examine North Korea's second nuclear test in 2009, where prior experience with sanctions implementation should reduce the informational advantage associated with political standing. The ownership differential becomes substantially smaller. These comparisons are consistent with the central implication of the theory: political standing need not uniformly sustain commerce or encourage withdrawal. Its effect depends on what better-positioned firms learn about the state's underlying enforcement intentions.

We make several contributions to the literature. First, we contribute to research on sanctions implementation by explaining how unequal access to political information can shape the effects of sanctions on trade. Existing work emphasizes enforcement incentives, evasion, and economic incidence (Ahn & Ludema, 2020; Bapat & Kwon, 2015; Drezner, 2011; Early, 2015; Morgan et al., 2023), while research on firm responses emphasizes commercial resources and information about other firms' behavior (Bai & Li, 2026; Davis et al., 2024). Building on uncertainty-driven accounts of overcompliance (Verdier, 2023), we theorize unequal access to credible information about government enforcement intentions as a distinct mechanism linking political relationships to withdrawal decisions. The effects of sanctions on trade can therefore depend not only on the restrictions governments adopt and their incentives to enforce them, but also on which firms can accurately infer how those restrictions will be implemented.

Second, we contribute to research on state-business relations and the political economy of economic statecraft by explaining why political standing can either sustain commerce or encourage withdrawal. Building on research on political relations and trade (Davis & Meunier, 2011; Davis et al., 2019; Fuchs & Klann, 2013; Lin et al., 2019) and firms' responses to sanctions, we distinguish access to credible information about enforcement intentions from protection against punishment. Protection generally makes continued activity less costly. Better

information, by contrast, can move behavior in either direction: it can preserve commerce when officials permit continued trade but increase compliance when officials intend to punish continuation. This distinction helps explain why firms with stronger political standing need not respond uniformly to geopolitical pressure.

Third, we explain why the costs of sanctions can fall unevenly across firms. Research shows that trade agreements benefit firms unevenly (Baccini et al., 2017), that government ownership mediates adjustment to international commitments (Tan & Davis, 2023), and that domestic support shapes the design of coercion (Mangini, 2024). Our theory identifies an additional source of these domestic stakes: firms lacking political standing may bear the costs of precautionary withdrawal while better-informed firms retain permissible trading relationships. The North Korea ownership gap, including in food and humanitarian trade, is consistent with this implication. Sanctions can therefore redistribute commercial opportunities within the implementing economy while extending their economic consequences beyond formal prohibitions.

Section 2 develops the theory, while Appendix E presents the formal derivation. Sections 3 and 4 introduce the empirical setting and research design. Section 5 presents the main analysis and Section 6 examines alternative comparative cases. Section 7 discusses implications for sanctions research.

2 Theory of Precautionary Overcompliance

Our theory concerns a government implementing sanctions¹ and a firm deciding on the scope of its adjustment. We develop a theory of *precautionary overcompliance* in which firms rationally abandon commerce that officials would permit because they cannot confidently rule out punishment for continuing. This mechanism depends on officials' capacity to make defiance costly and firms' unequal access to credible information about the government's true enforcement resolve. Under such conditions, political standing can serve as a channel through which firms obtain or verify that information. Better resolve reading can preserve commerce when officials permit it and encourage withdrawal when officials demand disengagement. Appendix E formalizes the argument into a simple signalling game between one policymaker and one firm. The model is intended to clarify the logic of *resolve reading* and to motivate the empirical tests rather than to characterize the entire environment of sanctions implementation. The main role of the model thus is to show that ambiguity about the enforcement resolve can persist in equilibrium, in that under ambiguity, the firm's choices are reduced to a comparison of the cost of withdrawal and the expected cost of punishment.

2.1 Sanctions and Resolve Ambiguity

We define *policy resolve* as the policymaker's willingness to bear the costs of enforcing a particular demand (Kertzer, 2016). In the sanctions setting, we refer to this willingness as *enforcement resolve*. The relevant distinction is between a government that confines its demands to the formal restrictions and one willing to enforce a broader cutoff of economic ties with the target. Firms must infer whether officials will permit commerce beyond the formal restrictions or punish its continuation. Policy resolve is distinct from enforcement capacity.

¹Our argument applies to unilateral and multilateral sanctions, including formal restrictions and informal pressure on firms to disengage from certain commercial activities. The policymaker represents the authority responsible for the relevant implementation decision, or a coordinated position among implementing authorities.

A government may possess the ability to punish but choose not to do so, while another may prefer broader withdrawal without having the means to enforce it (Holland, 2016).

While sanctions make explicit the types of transactions or transacting entities that they prohibit, they also serve a performative purpose in foreign policy. Announcing or endorsing them can display toughness to domestic audiences, register disapproval abroad, or demonstrate cooperation with international partners. As such, there are benefits of taking a visible political position even when officials have little appetite for enforcing broader commercial withdrawal. Whang (2011) finds that imposing sanctions can increase domestic support for US presidents. In China, Weiss and Dafoe (2019) find that tough but vague rhetoric can increase public approval even without military action. F. Y. Wang (2021) shows how hawkish media campaigns can pacify domestic audiences while the government pursues a restrained foreign policy. Governments may thus value the appearance of a firm response while remaining reluctant to disrupt commercial relationships.

This gap between the government's publicly stated stance and its true enforcement resolve can arise whether a government acts alone or coordinates with others. For instance, S. Han (2025) shows how sanctions can enable governments to demonstrate strength while simultaneously pursuing sustained diplomatic relations. A government may also endorse a multilateral resolution to demonstrate international cooperation while adopting a narrower interpretation of the implementation scope (Duchâtel & Schell, 2013, pp. 54–56). Conversely, policymakers may seek broader withdrawal than they publicly claim. Zhang (2024) argues that China's use of unannounced sanctions seeks to preserve plausible deniability and avoid institutional constraints imposed by international trade rules. In our framework, such incentives can encourage officials to leave demands for broader commercial withdrawal implicit even when they intend to enforce them.

Clarifying the intended scope of enforcement can itself be costly. Explicitly reassuring firms that commerce may continue can be criticized as weakening pressure on the target, while specifying a broader demand can require detailed administrative guidance or reduce diplomatic flexibility. Officials may therefore leave their enforcement resolve partially ambiguous when the costs of clarification exceed the benefits of aligning firms' behavior with their preferred response. Private guidance about a particular activity may avoid some of these public commitments. This provides a possible basis for differentiated information, provided the guidance comes from officials able to speak credibly about implementation.

Whether strategic or not, a divergence between the government's public position and the scope of withdrawal it intends to enforce creates uncertainty for firms. A strong public demonstration of toughness may suggest high resolve to enforce broader withdrawal even when officials intend a limited response, while narrow formal restrictions may understate the withdrawal officials expect. Firms may therefore withdraw from commerce that officials would permit, or continue trading when officials expect them to stop. Sanctions often leave substantial areas of commerce legally permissible, while broader sanctions may preserve exemptions or authorized channels (Verdier, 2023). These residual areas require firms to infer officials' resolve to enforce further adjustment. Withdrawal beyond formal restrictions constitutes precautionary overcompliance only when officials would permit the activity and uncertainty motivates the withdrawal.

2.2 Resolve Reading

Given this information asymmetry, firms engage in *resolve reading*: they use the available information to infer the government's resolve to enforce broader commercial withdrawal. The government's true resolve and firms' inferred resolve are distinct. Even when the broader political objective is clear, firms may remain uncertain about the scope and the timing of the commercial adjustment expected of them. Thus, even strong public rhetoric alone need not reveal the government's willingness to bear the costs of broader enforcement (Weeks, 2008). When public communication alone does not resolve the information problem, firms must decide how much commerce to sacrifice to avoid a political risk they cannot confidently assess.

A firm therefore weighs two types of losses. Withdrawal imposes an economic cost from reduced or terminated commercial activities, while continuation risks punishment if officials demand broader disengagement and enforce that demand. This tradeoff between commercial opportunity and political risk also shapes firms' responses in other settings of political uncertainty (Jung et al., 2021). A firm withdraws when the expected loss from punishment exceeds the cost of withdrawal. That expected loss depends on its inferred enforcement resolve, officials' capacity to detect and punish continued trade, and the severity of punishment (Becker, 1968).²

When potential political penalties are substantial, a firm can rationally withdraw even if it considers broad enforcement relatively unlikely. Where continued trade is unlikely to be detected or penalized, firms have less reason to sacrifice profitable commerce. Precautionary overcompliance thus does not require firms to misunderstand the formal rules or believe that strict enforcement is the most likely outcome. It can arise from rational responses to uncertainty over the government's true enforcement resolve (Aytun et al., 2025; Smith & Poplin, 2023; Stępień et al., 2024; Verdier, 2023).³ Conversely, a fully informed firm may continue trading even when punishment is certain if withdrawal would be more costly. Information affects behavior when it changes the firm's assessment enough to alter this cost comparison.

Table 1 summarizes the argument by comparing policymakers' actual resolve with firms' responses to inferred resolve. High actual resolve means officials enforce broader withdrawal, while low actual resolve means they permit continued activity. Firms choose between withdrawal and continuation based on their inferred resolve and the cost comparison above. These choices need not reveal firms' beliefs directly: a modest perceived risk can induce withdrawal, while a sufficiently costly commercial relationship can be maintained despite certain punishment.

2.3 Political Standing as Information Channel

We focus on political standing as one factor that can improve resolve reading. We define *political standing* as a firm's position in the relationships linking business and government. Research on political connections and embeddedness motivates treating those relationships as a source of policy knowledge (Haveman et al., 2017; Henisz & Zelner, 2010; Hillman, 2005; Wei et al., 2023). Political standing can help a firm obtain or verify

²Our model holds enforcement capacity fixed by making punishment effective once chosen. The scope condition concerns applying that benchmark across countries and activities. The analysis does not endogenize monitoring or evasion.

³This mechanism is distinct from other forces that may also shape withdrawal decisions, including peer conformity, competitive pressures, bargaining, and host-government interests (Davis et al., 2024; Wellhausen & Zhu, 2026). Our focus is on firms' access to credible information about officials' enforcement resolve.

Table 1: Policymakers’ Actual Resolve and Firms’ Inferred Resolve

Firm’s inferred resolve	Policymakers’ actual resolve	
	High resolve Enforce withdrawal	Low resolve Permit continuation
High	Compliance The firm undertakes the withdrawal officials demand.	Precautionary overcompliance Uncertainty induces withdrawal from activity officials would permit.
Low	Undercompliance Continued commerce attracts punishment in the model.	Permitted continuation The firm maintains commerce that officials permit.

Notes: The table concerns activities outside established prohibitions, which remain constant across all four cells. Columns distinguish policymakers’ actual resolve to enforce broader withdrawal, while rows distinguish the firm’s inferred resolve. The precautionary-overcompliance cell assumes withdrawal is motivated by uncertainty about punishment. Firms compare the expected loss from continuation with the cost of withdrawal. Accurate resolve reading does not guarantee compliance when withdrawal is more costly than punishment.

information about the government’s true resolve through direct access to officials or administrative guidance.

The mechanism operates in three steps. First, public announcements leave firms unsure about the government’s true enforcement resolve. Second, political standing increases the likelihood that a firm learns whether officials have the resolve to enforce broader withdrawal or will permit continued commerce. Third, this information changes the firm’s inferred resolve and can alter its withdrawal decision. A company that learns that continued activity is acceptable can preserve a relationship it would otherwise abandon as a precaution. A firm that learns that broader withdrawal will be enforced can instead disengage when the expected punishment outweighs the cost of withdrawal. Political access can therefore reduce precautionary overcompliance or increase compliance, depending on the actual resolve the guidance reveals.

The value of political standing increases when firms have few alternative channels for political information. In centralized authoritarian systems, relevant authority may rest with a small set of officials, while government control can restrict public reporting (Gehlbach & Sonin, 2014; Hollyer et al., 2011). Business–government ties can provide knowledge of bureaucratic practices and trust (Haveman et al., 2017). Research on political connections under geopolitical uncertainty also motivates examining these relationships (Adarkwah et al., 2024).

Access improves interpretation only when contacts provide credible guidance. Stern and O’Brien (2012) show how even insiders can face uncertainty about political boundaries. Repeated dealings, identifiable administrative responsibility, and guidance specific to a firm’s circumstances are plausible foundations for trust. Reassurance from an official unable to speak for implementing authorities would not resolve the problem. The same specificity can limit how readily one firm’s guidance informs another, and firms may lack incentives to share commercially valuable information. Political standing therefore provides an informational advantage only when it offers credible clarification that other firms cannot easily obtain or verify.

Our model in the appendix formalizes this mechanism by assuming that firms with stronger political standing are more likely to obtain accurate information about the government’s true enforcement resolve. In order

to isolate the informational mechanism, the model takes the availability and credibility of political guidance as given rather than considering why some firms receive better information than others. Therefore, we do not model officials' choices over whom to inform or the conditions in which private guidance is credible. Our narrower aim is to show how unequal access to credible guidance, when available, leads to systematically different withdrawal decisions. The appendix models one firm with a fixed withdrawal cost known to both players and compares its behavior under higher and lower political standing in separate games. Withdrawal probabilities reflect whether that firm receives information. The information comparison holds policy preferences, prior beliefs, the withdrawal cost, and protection from punishment constant. Political standing therefore affects behavior through access to information, rather than because politically connected firms necessarily face lower costs of withdrawal. The model separately allows political standing to change protection, as discussed below.

Ordinary learning can substitute for political access. Relevant experience, broadly available guidance, or observable implementation can help firms that initially lack privileged information. Holding other conditions constant, the information advantage narrows as fewer firms remain uncertain, and fully informative public guidance that is equally credible and applicable to all firms removes it. These conditions vary across policy areas: authoritarian governments can offer accessible clarification, while sensitive policy areas in democracies can combine credible enforcement with restricted information. The mechanism's reach therefore depends on the availability and reliability of policy knowledge. The China evidence examines these institutional conditions within one political system.

2.4 Information Access versus Political Protection

Political standing can affect firm behavior not only through an informational channel but also via the channel of political protection. Political standing can confer greater bargaining power and access to government resources under uncertainty (Khwaja & Mian, 2005; Kubinec et al., 2021). Such resources may help firms sustain commerce through financing or other commercial advantages, even without better information. In some cases, political standing may even reduce responsiveness to government pressures (Naoi et al., 2022). These findings motivate allowing political standing to confer resources, bargaining power, and protection, but they do not directly establish protection from punishment for sanctions violations. Firms with stronger political standing may therefore suffer smaller losses when punished, even though political connections can also carry costs in other dimensions (Kubinec et al., 2024).⁴ In the case where officials permit continued commerce, information and protection can reinforce one another: better information reduces uncertainty about whether punishment will occur, while protection reduces the expected cost of punishment if it does. Thus, both channels can make politically connected firms less likely to withdraw.

Our model distinguishes these channels by comparing differences in information quality while holding protection fixed, and differences in protection while holding information fixed; commercial costs, prior beliefs, and public communication remain constant in both comparisons. The channels yield different predictions across

⁴The evidence concerns politically connected firms' reliance on informal dispute resolution and the resulting reluctance of less-connected investors. Here, protection refers to smaller losses from government punishment. Connections need not improve every aspect of a firm's commercial position.

enforcement settings. When policymakers' actual resolve is weak and they permit continued commerce, better information reduces precautionary withdrawal by revealing that continued activity is acceptable. When resolve is strong, the same informational advantage can increase compliance by revealing that continued trade will expose firms to punishment. Political protection does not generate this reversal: reducing losses from punishment makes firms more willing to sustain commerce under either level of resolve. Information can therefore shift behavior in either direction, whereas protection consistently favors continued activity. These differences affect withdrawal decisions when they are large enough to change which option the firm prefers.

When all firms clearly understand the government's enforcement demand, politically connected firms may comply more because they face stronger obligations to cooperate. Reciprocal firm-government exchanges and government control over SOE leadership can make political benefits conditional on cooperation (C. Han et al., 2022; Leutert & Vorthers, 2021). Defiance may be especially costly when sovereignty or regime survival is at stake, although strategic priorities also extend to economic policy (Hsueh, 2016). Connected firms are therefore more likely to withdraw when the costs of defying the government outweigh the protection their connections provide.

2.5 Observable Implications

Our theory yields three predictions about how political standing shapes firms' responses to uncertainty about enforcement resolve. These predictions concern activities for which formal prohibitions leave firms some discretion over whether to continue commerce. Officials have the capacity to make defiance costly, but public signals do not fully reveal whether they intend to enforce broader withdrawal. Firms must therefore decide whether to continue trading without knowing how officials will respond.

In this setting, political standing can provide access to credible guidance that remains unavailable to less-connected firms. The predictions below hold commercial stakes, protection from punishment, prior beliefs, and public communication constant. Better information changes a firm's decision when it shifts the expected cost of punishment enough to make withdrawal preferable to continued trade, or vice versa. The direction of the effect depends on what the guidance reveals: reassurance can preserve commerce that firms would otherwise abandon, while a warning can prompt withdrawal from commerce they would otherwise continue. This informational advantage narrows as other firms learn from experience and disappears when public guidance fully resolves uncertainty and is equally credible to all firms.

H1: Political standing as information channel. When officials have weak resolve to enforce broader withdrawal and permit continued trade, stronger political standing should reduce precautionary withdrawal among firms that remain uncertain about officials' intentions. Political access provides credible reassurance that continued commerce will not be punished.

H2: Precautionary overcompliance beyond formal restriction. The withdrawal difference in H1 can extend to activities outside formal prohibitions. Even when officials permit these activities, less-informed firms may abandon them out of concern about punishment. Holding other conditions constant, a higher perceived risk of broader enforcement can increase such precautionary withdrawal.

H3: Political standing under stronger enforcement resolve. When officials have strong resolve to enforce

broad withdrawal, stronger political standing should increase withdrawal among firms that remain uncertain about officials' intentions. Political access provides credible warnings that continued commerce will be punished.

H1 and H3 concern different decisions under uncertainty. H1 applies to activities that a firm would abandon without reassurance; H3 applies to activities that it would continue without a warning. The model therefore does not imply that political standing reverses the same firm's decision over the same activity across the two settings. Whether information changes behavior depends both on actual enforcement resolve and on the firm's balance between the costs of withdrawal and expected punishment.

We evaluate H1 and H2 primarily through North Korea's 2006 nuclear test and the sanctions that followed, examining ownership differences in monthly trading activity, trade values, and product scope. State ownership serves as a proxy for political standing, while product screens establish whether the ownership pattern extends beyond mapped restrictions. These measures connect the empirical analysis to the predictions, although neither directly measures access to information or precautionary motives. North Korea's 2009 nuclear test provides a further opportunity to examine whether the ownership difference narrowed after firms gained experience with sanctions enforcement.

Section 6 extends the analysis to Iran and official Dalai Lama receptions. Iran offers a close comparison to North Korea because both involved targeted UN sanctions over nuclear disputes. Official Dalai Lama receptions provide a setting for examining the conditional prediction in H3. Greater SOE withdrawal is consistent with the informational channel if political access clarified an enforcement demand that remained uncertain to other firms. Where the demand was equally clear to all firms, political obligations offer an alternative explanation for greater SOE compliance.

3 Empirical Context

3.1 North Korea's Nuclear Program and UN Export Sanctions

Our empirical context is North Korea's first nuclear test in October 2006 and the subsequent UN sanctions that followed. On October 9 of that year, North Korea conducted its first nuclear test. Five days later, the UN Security Council adopted Resolution 1718 with China's support. The resolution prohibited specified weapons, missile- and nuclear-related transfers, and luxury goods, and froze designated assets, but it did not impose a comprehensive trade embargo (United Nations Security Council, 2006a). During this time period, China had led the Six-Party Talks and remained North Korea's principal political and economic partner. Beijing sought to deter nuclear escalation while avoiding pressure severe enough to threaten stability on its northeastern frontier (Snyder & Wit, 2007; Twomey, 2008).

Contemporaneous official statements clarify China's publicly stated position on the scope of sanctions. On October 17, Foreign Ministry spokesperson Liu Jianchao affirmed China's implementation of Resolution 1718 while referring questions about normal financial and commercial dealings to the relevant Chinese financial authorities (Ministry of Foreign Affairs of the People's Republic of China, 2006a). A week later, he stated that normal trade was not restricted by the resolution and that the government did not interfere with banks' normal commercial activities conducted in accordance with it (Ministry of Foreign Affairs of the People's Republic

of China, 2006b). These statements establish an announced distinction between targeted sanctions and normal commerce under the resolution.

Two distinct sources of uncertainty could nevertheless remain. Firms might be unsure whether a particular transaction fell within the formal restrictions. Separately, even when a transaction fell outside those restrictions, firms might remain uncertain whether officials expected broader commercial withdrawal and would punish continuation. Our resolve-reading argument concerns this second question: whether compliance with the formal restrictions was sufficient to avoid punishment. The public statements do not establish either how implementing officials would treat particular transactions or how confidently firms understood their position. Similarly, the referral of operational questions to financial authorities indicates a need for more specific guidance without identifying which source of uncertainty that guidance would resolve.

Public assurances also do not, by themselves, identify weak actual enforcement resolve. In the equilibrium in Appendix E, officials who would punish broader commercial continuation issue the same routine announcement as officials who would permit it, leaving the uninformed firm's belief unchanged. China's diplomatic priorities make a limited response plausible, but neither the public statements nor the targeted legal scope establishes that officials would have permitted the particular trade from which firms withdrew. The ownership comparison therefore examines whether firms with weaker political standing withdraw more when public assurances leave the scope of enforcement uncertain.

Contemporary reporting indeed suggests that some traders responded with caution during this period. For instance, reporting from Dandong, ten days after the nuclear test, Li (2006) described an experienced trader becoming more selective about orders despite broadly normal port operations, while another shifted exports through an intermediary. Other traders continued substantial transactions, and many reportedly waited for greater clarity. These accounts are consistent with firms reducing their exposure to political and commercial risk even as cross-border trade continued. Traders also faced uncertainty surrounding North Korean border decisions, repayment, and the suspension of a trade-settlement arrangement at China Construction Bank's Dandong branch.

North Korea was also highly dependent on China for trade during this period, making the responses of Chinese firms economically consequential. Figure 1 shows that China's share of North Korea's recorded external trade rose over the study period. Figure 2 maps the geographic distribution of Chinese exports to North Korea during the twelve months before the sanctions. The pre-sanction geography is somewhat concentrated in the Northeast border region, but exporting prefectures span much of China, showing that exposure to North Korean trade extended well beyond the border regions.

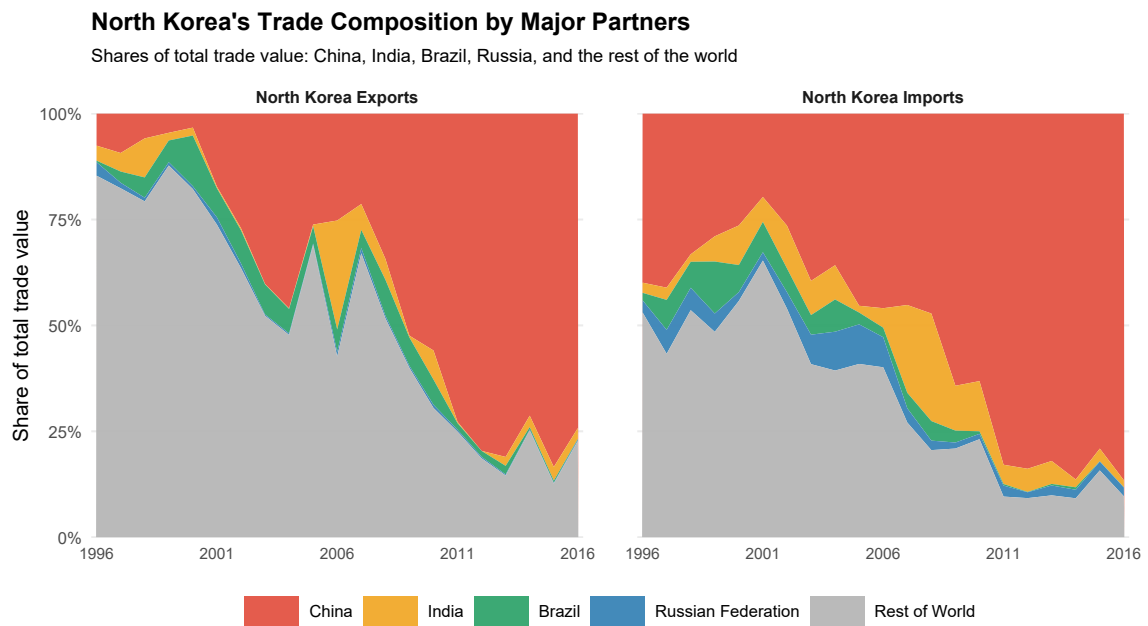


Figure 1: China's Share of North Korea's Recorded External Trade

Chinese exports to North Korea before the 2006 sanctions

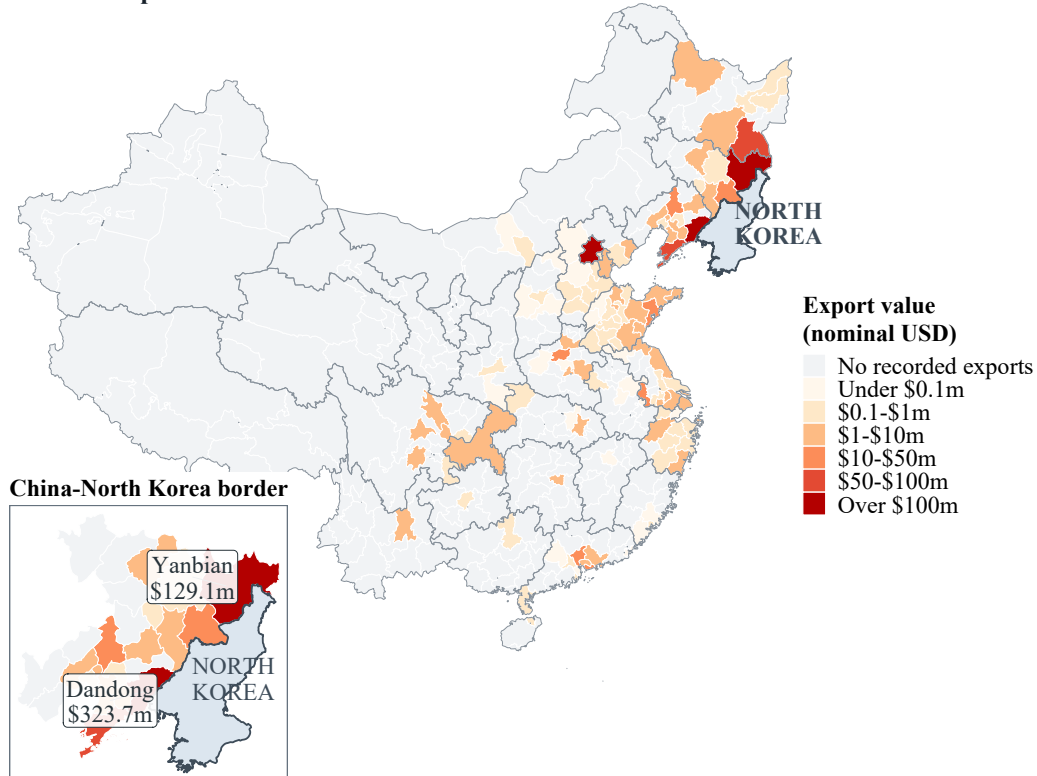


Figure 2: Geographic Distribution of Chinese Exports to North Korea before the 2006 Sanctions

Notes: The map aggregates transaction-level Chinese customs records by the prefecture associated with each exporter's Customs registration code from October 2005 through September 2006, the twelve complete months before the October 2006 sanctions. Total exports were approximately \$1.16 billion across 115 exporting prefectures. Shading reports nominal export value in fixed U.S.-dollar bins. Gray indicates no recorded exports. Legacy five-character Customs domestic-district codes are harmonized by geographic name to contemporaneous 2006 prefecture boundaries, including municipality districts, county-level ports, and special zones whose code prefixes do not identify the correct prefecture. The Yanbian total consolidates the Hunchun, Tumen, Yanbian, and Yanji code families.

3.2 Political Standing of Chinese Exporters

Firms' political standing may shape access to guidance about both the coverage of formal restrictions and officials' expectations for broader withdrawal. Our theoretical interest lies in the latter: whether political access helps firms determine when officials will permit continued commerce outside formal prohibitions. The Chinese setting is therefore particularly useful for our argument because firms differ systematically in their political standing and in the institutional channels through which policy information can reach them. We use state ownership as our principal measure of political standing. SOEs are embedded in the state through ownership, government oversight, and managerial appointments, creating institutional channels through which implementation information can reach firms (Bruton et al., 2015; Hu & Xu, 2022; Shi et al., 2014; Zhou et al., 2017). Private firms can also develop political relationships, but these ties vary considerably in their regularity and administrative reach (Chen & Dickson, 2008; Dickson, 2003). State ownership therefore provides a useful proxy for differences in access to credible political information.

It is important to note that our treatment of political standing is not limited to informational access. The same relationships that facilitate communication with the state may also provide political protection or create stronger obligations to respond to government preferences. Existing evidence illustrates these multiple channels. Davis et al. (2019) show how state ownership enables governments to use trade in pursuit of foreign-policy objectives, finding that Chinese SOE imports are more responsive to bilateral political relations than private-firm imports. Evidence on sovereign wealth funds similarly shows greater sensitivity to bilateral conflict and cooperation than among private acquirers (D. Wang et al., 2021). Building on this research on state influence over commercial decisions, we develop a complementary argument about firms' access to credible information. Political standing helps firms interpret uncertain enforcement demands, sustaining commerce when officials permit continued trade and encouraging withdrawal when they intend to enforce disengagement. As additional analyses, we examine alternative proxies for political standing, including registration in Beijing, registration in one of the four centrally administered municipalities, and pre-shock export scale.

4 Research Design

4.1 Data and Variables

We use transaction-level records from the China General Administration of Customs covering the universe of recorded transactions by China-based firms from 2000 to 2011. The data offer information on the trading firm, ownership type, location, HS8 product, destination or origin, trade value, and month of transaction. Although the source data extend through 2016, monthly reporting becomes inconsistent after 2011, so we restrict the analysis to the earlier period.

The empirical design compares a firm's exports to North Korea with its contemporaneous exports of the same product to other foreign destinations. For each firm-HS8-month, we therefore construct two destination-class observations. One records exports to North Korea, while the other records mean exports of the same product across the firm's remaining observed destinations. The resulting panel is at the firm-product-month-destination-

class level. The first firm-product observation enters sample when the firm first exports that product to North Korea and remain in the panel thereafter, including months in which North Korea exports fall to zero. This construction allows the analysis to capture both changes in shipment values within continuing relationships and adjustment through relationship exit. The sample time window runs from May 2006 through April 2007, centered on North Korea's October 2006 nuclear test. May–September provides five pre-treatment months. October is the treatment month, with the post indicator equal to one for October and all subsequent months (until April 2007), providing six post-treatment months.

Our main outcome captures the monthly value of exports at the firm–product–destination level. For each firm and product, we compare the total value exported to North Korea with the average value exported to the firm's other observed destinations. We log-normalize export values while retaining zero-trade observations. As a complementary outcome, we use a positive-trade indicator to capture whether a firm–product trade relationship remains active in a given month. The appendix reports these extensive-margin estimates alongside alternative transformations and comparison measures.

The post indicator identifies observations after the October 2006 nuclear test, while the North Korea indicator distinguishes exports to North Korea from exports of the same products to other destinations. Their interaction captures the post-shock change in exports to North Korea relative to the comparison destinations. We then interact this term with the state-ownership indicator, our primary proxy for political standing. This triple interaction measures whether the relative post-shock change in exports to North Korea differs between SOEs and private firms.

Firm-by-month fixed effects absorb all observed and unobserved time-varying firm characteristics that affect exports to North Korea and the comparison destinations equally. We therefore do not include additional firm-level controls in our preferred specification. As a robustness check, we allow the difference-in-differences effect to vary with four predetermined firm characteristics measured from January 2000 through June 2006: total export scale, the share of exports destined for North Korea, product concentration, and destination concentration.

Table 2 reports summary statistics for the sample, which contains 2,173,720 firm–HS8–month–destination-class observations from 2,963 firms. SOEs were substantially larger and more diversified before the shock, with mean log total exports of 16.724 compared with 12.233 for private firms and an average of 297.96 HS8 products compared with 112.45. Private firms, however, were more exposed to North Korea, directing 27.40 percent of their pre-shock exports there compared with 14.75 percent among SOEs. These baseline differences motivate our within-firm design, in which identification comes from changes in each firm's trade with North Korea relative to its contemporaneous trade with other destinations rather than from cross-sectional differences between SOEs and private firms.

Table 2: Descriptive Statistics for the Preferred Estimation Sample

<i>Panel A. Preferred estimation-sample observations</i>							
Ownership	Destination class	Observations	Firms	Value (thousands of dollars)		Mean log	Positive
				Mean	Standard deviation	$\ln(1 + y)$	trade (%)
Private	Other destinations	647,077	1,552	0.89	48.22	0.131	1.5
Private	NK	647,077	1,552	0.90	29.09	0.559	8.0
SOE	Other destinations	439,783	1,411	4.01	144.36	0.174	1.8
SOE	NK	439,783	1,411	0.83	96.12	0.135	1.8
<i>Panel B. Predetermined firm characteristics</i>							
Variable	Private			SOE			
	Firms	Mean	Standard deviation	Firms	Mean	Standard deviation	
Observed exporting before July 2006 (%)	1,552	86.4	34.3	1,411	98.9	10.3	
Pre-shock total exports, $\ln(1 + \text{export value})$	1,552	12.233	5.413	1,411	16.724	3.046	
Pre-shock NK export share (%)	1,552	27.40	40.22	1,411	14.75	30.93	
Pre-shock number of HS8 products	1,552	112.45	238.45	1,411	297.96	441.27	
Pre-shock number of destinations	1,552	18.69	29.18	1,411	48.15	42.06	
Pre-shock product HHI	1,552	0.282	0.312	1,411	0.260	0.278	
Pre-shock destination HHI	1,552	0.467	0.373	1,411	0.317	0.289	
Distance to Beijing, $\ln(1 + \text{km})$	1,400	6.767	1.108	1,336	5.633	2.387	
Registered in Beijing (%)	1,400	1.8	13.2	1,336	14.4	35.1	
Registered in a central municipality (%)	1,552	5.5	22.8	1,411	25.4	43.6	
Registered in Liaoning or Jilin (%)	1,552	36.8	48.2	1,411	31.2	46.3	

Notes: Panel A uses the estimation sample from column 3 of Table 4: May 2006 through April 2007, restricted to firms classified as private or state-owned using the frozen ownership measure. Each firm–HS8–month contributes an NK row and a comparison row. On the NK row, value is monthly exports to North Korea, while on the comparison row, it is the mean across other observed destinations, with missing comparison activity coded as zero. Values are in thousands of United States dollars. Positive trade equals one when value is greater than zero. Panel B uses one observation per firm. Pre-shock characteristics cover January 2000 through June 2006. Firms not observed exporting in that period receive zero for the constructed pre-shock measures. Location statistics use available registration information, so their sample sizes can be smaller. Entries are means and standard deviations. Indicator means are percentages.

4.2 Estimating Differential Adjustment

Our primary empirical strategy is a triple-difference design that extends the difference-in-differences comparison described above. We first compare trade with North Korea before and after the October 2006 nuclear test relative to trade with other destinations. We then examine whether this relative change differs between state-owned and private firms. Let f index firms, p HS8 products, c the NK and comparison destination classes, and t year-month. Our outcome variable, $Outcome_{fpct}$, is the log-normalized monthly export value for firm f , product p , destination class c , and month t . The indicator $Post_t$ identifies the post-shock period, NK_c identifies exports to North Korea rather than the comparison destinations, and SOE_f identifies state-owned firms.

$$Outcome_{fpct} = \alpha_c + \lambda_p + \gamma_{ft} + \theta(NK_c \times SOE_f) + \beta_P(Post_t \times NK_c) + \beta_D(Post_t \times NK_c \times SOE_f) + \varepsilon_{fpct}. \quad (1)$$

The specification includes destination-class fixed effects, α_c , HS8-product fixed effects, λ_p , and firm-by-month fixed effects, γ_{ft} . The firm-by-month fixed effects absorb all observed and unobserved time-varying firm-level factors that affect the firm's exports to North Korea and the comparison destinations equally. Identification therefore comes from within-firm changes in exports to North Korea relative to contemporaneous exports of the same products elsewhere. The interaction between $Post_t$ and NK_c , with coefficient β_P , captures the post-shock change in exports to North Korea relative to other destinations for private firms, the omitted ownership category. We then interact this difference-in-differences term with SOE_f . The coefficient on this triple interaction, β_D , measures how the relative post-shock change for SOEs differs from that for private firms. The corresponding post-shock effect for SOEs is therefore $\beta_P + \beta_D$. Our primary coefficient of interest is β_D : a positive estimate indicates that SOEs reduce their exports to North Korea less than private firms, relative to their respective exports to other destinations. Standard errors are clustered at the firm level (Bertrand et al., 2004).

For outcomes measured as $\ln(1 + y)$, we report percentage equivalents as $100[\exp(b) - 1]$, where b is the relevant coefficient or linear combination. We use unrounded estimates where available. These summarize multiplicative contrasts in geometric means of $1 + y$, rather than percentage changes in arithmetic mean trade values or counts. For an ownership interaction, this conversion describes the relative adjustment of SOEs to private firms, not a percentage-point difference between their converted effects or the percentage by which one group's decline is smaller. Linear-probability coefficients are multiplied by 100 and reported in percentage points.

The identifying assumption is that, absent the October 2006 shock, SOEs and private firms would have experienced similar relative changes in their trade with North Korea compared with other destinations. While we do not observe any noticeable pre-trends in the main analysis, we nonetheless check for parallel trends through a number of methods, such as examining pre-treatment dynamics among pre-October 2006 active firms, using alternative estimation windows and treatment dates, and allowing treatment effects to vary with predetermined firm size, North Korea exposure, and product and destination concentrations.

5 Results

5.1 Ownership and Post-Sanction Adjustment

We first examine whether Chinese exports to North Korea declined following the 2006 nuclear test and subsequent sanctions. Table 3 shows a clear trade contraction. In the preferred short-window specification, exports to North Korea decline by approximately 10.2 percent relative to contemporaneous exports of the same products to other destinations on the transformed outcome scale. This estimate is stable across the fixed-effect specifications in columns 1–3.

The central question is whether this adjustment varies by firm’s political standing. Table 4 shows that the contraction is concentrated among private firms. Following the shock, private firms exhibit a relative decline in exports to North Korea of approximately 28.5 percent on the transformed outcome scale, whereas the corresponding change among SOEs is indistinguishable from zero. The implied SOE–private contrast is approximately 42.5 percent on that scale. This ownership gap is stable across alternative fixed-effect specifications, remains positive in the broader window, and is robust to treatment interactions with predetermined firm characteristics. Thus, firms with weaker political standing respond substantially more strongly to the sanctions.

Such contraction appears in imports as well. In the short window, the point estimates imply declines in imports from North Korea relative to other origins of approximately 50.0 percent for private firms and 4.1 percent for SOEs on the transformed outcome scale. The SOE estimate is statistically indistinguishable from zero. The implied SOE–private contrast is approximately 91.8 percent on that scale, indicating a more favorable relative adjustment among SOEs. Appendix A reports the full import estimates and event paths.

Table 3: Difference-in-Differences Benchmark

Outcome:	(1) Trade Value	(2) Trade Value	(3) Trade Value	(4) Trade Value	(5) Trade Value
Post × NK	-0.108*** (0.0311)	-0.108*** (0.0311)	-0.108*** (0.0312)	-0.114*** (0.0293)	-0.327*** (0.0676)
Destination-class FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Absorbed	Absorbed	Absorbed
Firm FE	No	Yes	Absorbed	Absorbed	Absorbed
Firm-by-month FE	No	No	Yes	Yes	Yes
HS8-product FE	No	No	Yes	Yes	Yes
Predetermined profile interactions	No	No	No	No	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07
Observations	2,305,760	2,305,760	2,305,760	4,518,862	4,518,862

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and the mean across other observed destinations on the comparison row. *Post* equals one from November 2006 onward, and *NK* identifies the North Korea row. Columns 1–3 use May 2006–April 2007. Columns 4–5 use December 2005–October 2007. Column 5 fully interacts *Post* and *NK* with four mean-centered, January 2000–June 2006 firm characteristics: log total exports, North Korea’s share of firm exports, the product-export Herfindahl index, and the destination-export Herfindahl index. No other firm-level controls are included. Standard errors are clustered by firm.

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

Figure 3 examines the timing of this ownership divergence by replacing the post indicator with month-specific interactions. Private-firm exports to North Korea decline sharply beginning in January 2007, roughly three months after the October shock, while SOE exports change comparatively less. One plausible explanation for this delay is that firms adjust new orders before the change appears in realized shipments. Customs records

Table 4: Ownership Triple-Difference Estimates

Outcome:	(1) Trade Value	(2) Trade Value	(3) Trade Value	(4) Trade Value	(5) Trade Value
Post × NK × SOE	0.354*** (0.0563)	0.354*** (0.0563)	0.354*** (0.0564)	0.447*** (0.0503)	0.401*** (0.0527)
Post × NK	-0.335*** (0.0424)	-0.335*** (0.0424)	-0.335*** (0.0424)	-0.424*** (0.0402)	-0.562*** (0.0646)
Post × SOE	-0.0685 (0.0444)	0.0459 (0.0484)			
NK × SOE	-0.664*** (0.0688)	-0.664*** (0.0688)	-0.664*** (0.0689)	-0.693*** (0.0582)	-0.533*** (0.0657)
SOE	0.0718 (0.0542)				
Destination-class FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Absorbed	Absorbed	Absorbed
Firm FE	No	Yes	Absorbed	Absorbed	Absorbed
Firm-by-month FE	No	No	Yes	Yes	Yes
HS8-product FE	No	No	Yes	Yes	Yes
Predetermined profile interactions	No	No	No	No	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07
Observations	2,173,720	2,173,720	2,173,720	4,156,818	4,156,818

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and the mean across other observed destinations on the comparison row. *Post* equals one from November 2006 onward, and *NK* identifies the North Korea row. The first two rows report Post × NK × SOE and Post × NK. Columns 1–3 use May 2006–April 2007. Columns 4–5 use December 2005–October 2007.

Column 5 fully interacts *Post* and *NK* with four mean-centered, January 2000–June 2006 firm characteristics: log total exports, North Korea's share of firm exports, the product-export Herfindahl index, and the destination-export Herfindahl index. No other firm-level controls are included. Standard errors are clustered by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

capture completed trade, whereas underlying orders and commercial commitments may have been made earlier. For products outside the formal prohibitions, firms may therefore fulfill existing orders while reducing new commitments. Existing contracts and established commercial relationships can generate this inertia (Fuchs & Klann, 2013), causing an immediate reassessment of political risk to appear only gradually in monthly trade flows.

North Korea's trading institutions may further help explain why this adjustment differs across Chinese firms. Haggard and Noland (2018) document the importance of state entities in North Korean trade and show that trust is particularly concentrated in relationships between Chinese and North Korean SOEs. In this setting, private exporters with less reliable political guidance may be more reluctant to renew commercial relationships as existing commitments expire, while SOEs continue placing or accepting orders. Commercial inertia combined with unequal access to credible information therefore provides one plausible interpretation of the delayed ownership divergence.

Because the full post-entry panel adds newly observed firm–product relationships over time and retains inactive relationships as zero-trade observations, we treat these estimates as descriptive evidence on timing rather than as a formal pre-trend test. We therefore examine pre-treatment dynamics separately in the incumbent cohort. In that sample, the May–August ownership leads are jointly indistinguishable from zero ($p = 0.398$), and tests of differential pre-treatment linear trends show no systematic divergence between SOEs and private firms. Appendix C reports the underlying estimates and additional sensitivity checks.

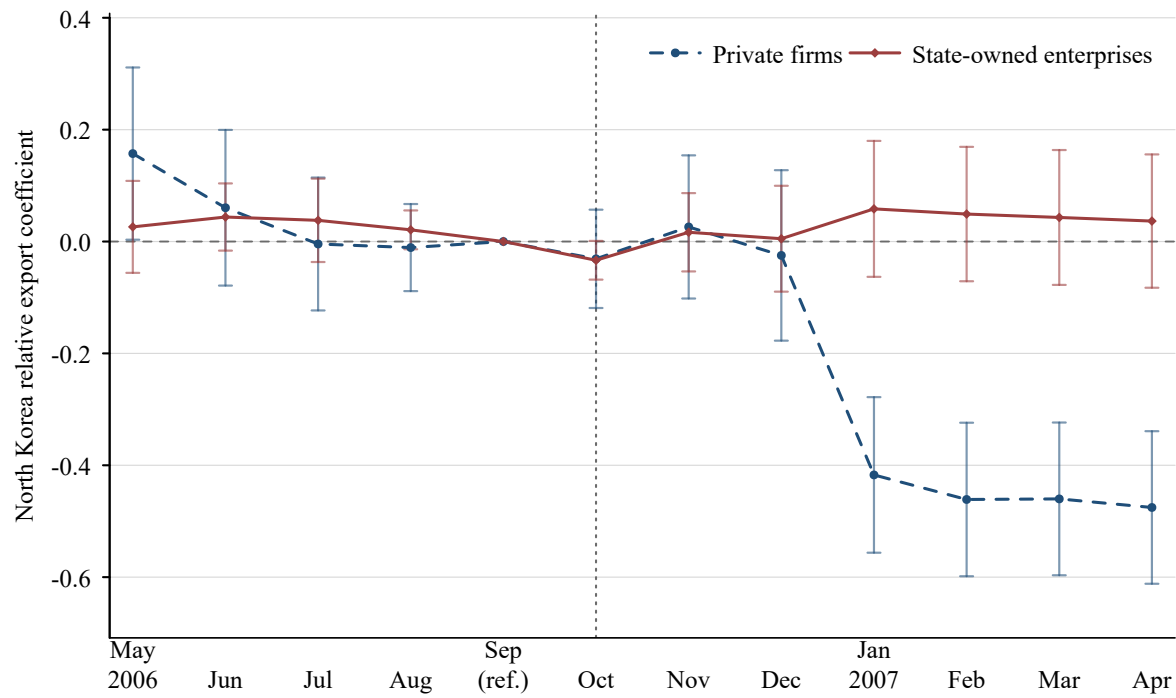


Figure 3: Export Event Paths by Firm Ownership

Notes: Monthly coefficients are plotted separately for private firms and SOEs. September 2006 is omitted, and the vertical line marks October 2006. Bars report 95-percent confidence intervals based on firm-clustered standard errors. The specification corresponds to column 3 of Table 4.

5.2 Withdrawal beyond Formally Restricted Products

We next explore whether the ownership divergence extends beyond products covered by the sanctions. This is the product-scope implication in H2: precautionary withdrawal can reach product categories outside formal prohibitions when their political acceptability remains uncertain. Such a pattern extends beyond compliance with listed product restrictions. A complication arises here as Resolution 1718 does clarify which products in the Harmonized System (HS) codes are included in the sanction. Paragraph 8(a)(ii) instead refers to UN documents identifying materials, equipment, goods, and technology related to nuclear, ballistic-missile, and other weapons-of-mass-destruction programs (United Nations Security Council, 2006a). Figure 4 reproduces the relevant passage.

Resolution 1718, paragraph 8(a)(ii): All items, materials, equipment, goods and technology as set out in the lists in documents S/2006/814 and S/2006/815, unless within 14 days of adoption of this resolution the Committee has amended or completed their provisions also taking into account the list in document S/2006/816, as well as other items, materials, equipment, goods and technology, determined by the Security Council or the Committee, which could contribute to DPRK's nuclear-related, ballistic missile-related or other weapons of mass destruction-related programmes.

Figure 4: Resolution 1718 Passage Identifying the UN Control-List Documents

The three documents cited in paragraph 8(a)(ii) describe sanctioned items in technical terms rather than customs classifications. We therefore construct three alternative screens that map these descriptions to HS codes.

First, we use a coarse HS2 screen that removes chapters 28, 29, 36, and 93, which cover inorganic chemicals, organic chemicals, explosives and pyrotechnic products, and arms and ammunition. This deliberately broad screen is likely to exclude many civilian products alongside potentially sanctioned goods. Second, we construct a lexical screen from the descriptions in the UN control lists. We extract normalized, non-generic words from sanctioned-item titles and match them to official HS4 descriptions, identifying 321 HS4 headings. Because a shared word does not establish that the corresponding customs heading is subject to the sanctions, this screen is also intentionally conservative. Third, in our preferred approach, we map the UN control-list records directly to candidate HS4 headings. We use GLM-5.2 through a locally run classification pipeline to classify each record based on its complete technical description and the HS 2002 reference. The model assigns an HS4 heading to 318 records, yielding 66 unique headings after duplicate assignments are removed. Three human reviewers independently assess blinded assignments drawn from a random sample of 130 unique products. Of these, 45 products receive two reviews, yielding 175 reviewer–item decisions. The AI coding and human review agree on assignability in 79.4 percent of the 175 reviewer–item decisions. Appendix B describes the classification and validation procedures in detail.

We re-estimate the same ownership specification after excluding the product categories that were matched in our screening, therefore examining export patterns outside the product categories mapped to the formal controls. Table 5 shows that the ownership gap persists across all three product screens, ranging from approximately 41.3 to 46.0 percent on the transformed outcome scale. Under the preferred AI-assisted screen, the implied SOE–private contrast is approximately 44.6 percent, compared with 42.5 percent in the full sample. Excluding products plausibly covered by the formal controls therefore leaves the central ownership result essentially unchanged,

supporting H2 on the breadth of adjustment.

Table 5: Ownership Effects under Alternative Sanction-Product Screens

Outcome	Baseline Trade Value	Coarse HS2 Trade Value	Unigram Trade Value	AI-assisted HS4 Trade Value
Post $\times$ NK $\times$ SOE	0.354*** (0.0564)	0.346*** (0.0586)	0.378*** (0.0603)	0.369*** (0.0578)
Post $\times$ NK	-0.335*** (0.0424)	-0.332*** (0.0434)	-0.370*** (0.0440)	-0.352*** (0.0429)
Excluded products	None	4 HS2 chapters	321 HS4 headings	66 HS4 headings
Destination-class FE	Yes	Yes	Yes	Yes
HS8-product FE	Yes	Yes	Yes	Yes
Firm-by-month FE	Yes	Yes	Yes	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07
Firms	2963	2878	2478	2850
Observations	2173720	2061718	1495590	1986814

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and the mean across other observed destinations on the comparison row. Post equals one from November 2006 onward, and NK identifies the North Korea row. Each column reports Post $\times$ NK $\times$ SOE and Post $\times$ NK from the same May 2006–April 2007 ownership model. All models absorb destination class, HS8 product, and firm-by-month fixed effects. The coarse screen removes HS2 chapters 28, 29, 36, and 93. The unigram screen removes official HS4 headings containing at least one normalized, non-generic title word that appears in no more than 12 of the 1,244 HS4 descriptions. The AI-assisted screen removes 66 unique headings from 318 assigned control-list records. Standard errors are clustered by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

The pattern extends to civilian necessities. Table 6 shows an SOE–private contrast of approximately 38 percent for food products and 39 percent for the broader humanitarian category on the transformed outcome scale. The broader category excludes all 66 mapped HS4 headings. Together with the alternative product screens, these results show that the ownership divergence extends well beyond the weapons-related products at the center of Resolution 1718. These results show why the breadth of commercial adjustment matters for sanctions implementation. Restrictions aimed at security-sensitive transactions can reduce firms’ willingness to supply ordinary civilian goods, including food and humanitarian products. Where the target country depends heavily on external supplies, such withdrawal can broaden the economic consequences of sanctions even when those goods are not formally restricted.

Note that the product mappings operate at the category level, so we cannot determine the legal status of every individual transaction, which may also depend on counterparties and end uses. The product screens show that the ownership divergence extends beyond categories mapped to the formal controls. They narrow an explanation based solely on uncertainty about restricted products, but do not distinguish uncertainty about transaction-specific coverage from uncertainty about demands for broader withdrawal. Nor do they establish that officials would have permitted the particular transactions firms discontinued.

5.3 Alternative Measures of Political Standing

State ownership provides our principal measure of political standing. We construct three additional proxies that capture different dimensions of firms’ access to the political center and their economic importance. First, we identify firms registered in Beijing, where proximity to central government institutions may facilitate access to political information. Second, we identify firms registered in one of the four centrally administered munici-

Table 6: Product Heterogeneity by Sanction and Humanitarian Scope

<i>Panel A. Sanction-mapped and food products</i>				
	(1)	(2)	(3)	(4)
Outcome	Trade value	Trade value	Trade value	Trade value
Sample	Full	Full	SOE	Private
Products	Sanction-mapped	Food	Food	Food
Post × NK × SOE	0.195** (0.0599)	0.320*** (0.0687)		
Post × NK	-0.151** (0.0488)	-0.362*** (0.0594)	-0.0414 (0.0346)	-0.362*** (0.0595)
NK × SOE	-0.682*** (0.0659)	-0.531*** (0.0872)		
Destination-class FE	Yes	Yes	Yes	Yes
Firm-by-month FE	Yes	Yes	Yes	Yes
HS8-product FE	Yes	Yes	Yes	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07
Observations	186,906	121,906	55,126	66,780
<i>Panel B. Unmapped humanitarian products</i>				
	(1)	(2)	(3)	
Outcome	Trade value	Trade value	Trade value	
Sample	Full	SOE	Private	
Post × NK × SOE	0.328*** (0.0675)			
Post × NK	-0.356*** (0.0490)	-0.0272 (0.0464)	-0.356*** (0.0491)	
NK × SOE	-0.573*** (0.0814)			
Destination-class FE	Yes	Yes	Yes	
Firm-by-month FE	Yes	Yes	Yes	
HS8-product FE	Yes	Yes	Yes	
Timeframe	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07	
Observations	449,122	169,742	279,380	

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and the mean across other observed destinations on the comparison row. Post equals one from November 2006 onward, and NK identifies the North Korea row. Panel A column 1 restricts the sample to the 66 mapped HS4 headings. Columns 2–4 cover food chapters 01–21. Panel B adds HS2 chapters 30, 39, 63, 90, and 94 and removes all mapped headings. All models use May 2006–April 2007, absorb destination class, HS8 product, and firm-by-month fixed effects, and cluster standard errors by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

palities—Beijing, Tianjin, Shanghai, and Chongqing—which occupy a privileged administrative position within China’s political system. Third, we use firms’ pre-shock export scale, measured both as an indicator for being above the sample median and continuously in logs, to capture economic importance and visibility.

Table 7 shows a similar pattern across these measures. On the transformed outcome scale, the post-shock relative adjustment in North Korea exports is approximately 42.8 percent more favorable for firms registered in Beijing than for other firms, and 41.8 percent more favorable for firms in the four centrally administered municipalities than for firms elsewhere. The corresponding contrast between firms above and below the pre-shock export median is approximately 22.4 percent. The interaction is also positive when firm size is measured continuously. The consistency of these results across ownership, administrative position, and firm scale supports the broader political-standing argument, although these measures may also capture differences in other organizational characteristics such as visibility and resources.

Table 7: Administrative Geography and Pre-Shock Firm Scale

Outcome	Beijing Trade Value	Four municipalities Trade Value	Large firm Trade Value	Log pre-shock exports Trade Value
Post $\times$ NK $\times$ Characteristic	0.356*** (0.079)	0.349*** (0.068)	0.202** (0.064)	0.106*** (0.016)
Post $\times$ NK	-0.115*** (0.032)	-0.122*** (0.032)	-0.276*** (0.052)	-0.441*** (0.039)
Destination-class FE	Yes	Yes	Yes	Yes
Firm-by-month FE	Yes	Yes	Yes	Yes
HS8-product FE	Yes	Yes	Yes	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07
Observations	2,305,760	2,305,760	2,305,760	2,305,760
Firms	4,069	4,069	4,069	4,069

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and the mean across other observed destinations on the comparison row. Post equals one from November 2006 onward, and NK identifies the North Korea row. Columns 1–3 use indicators for a Beijing registration, registration in Beijing, Tianjin, Shanghai, or Chongqing, and pre-shock exports above the sample median, respectively. Column 4 uses mean-centered $\ln(1 + \text{January 2000–June 2006 total exports})$. Its Post $\times$ NK coefficient is evaluated at mean pre-shock scale. Every model uses May 2006–April 2007, absorbs destination class, HS8 product, and firm-by-month fixed effects, and clusters standard errors by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

5.4 Additional Analyses

5.4.1 North Korea’s Second Nuclear Test

We examine whether the baseline pattern recurs around North Korea’s second nuclear test on May 25, 2009. We repeat the DD and DDD specifications in Tables 3 and 4, retaining the same outcome, comparison-destination construction, and fixed-effect structure while shifting the twelve-month estimation window to December 2008–November 2009. May is the transition month, and the post-treatment period begins in June 2009. Appendix Table C.4 reports the estimates.

The estimates reproduce the direction of the baseline results with substantially smaller magnitudes. The DD coefficient for all firms is negative (−0.0247, compared with −0.108 in the baseline twelve-month specification), indicating a decline in exports to North Korea relative to comparison destinations. The DDD coefficient is positive (0.00987, compared with 0.354 in the corresponding baseline specification), indicating

that SOEs withdraw less than private firms. Both 2009 coefficients are statistically distinguishable from zero. These results extend the baseline pattern to a subsequent sanctions episode. The smaller ownership differential is consistent with attenuation as firms gain experience with the sanctions regime.

5.4.2 Border Provinces and Shipment Route

We next examine whether the ownership divergence is concentrated among firms and transactions most directly exposed to the China–North Korea border. This allows us to assess whether the main result reflects a broader ownership pattern or is instead driven by the confounding geography and transport channels through which private firms differ from SOEs.

Panel A of Table 8 compares firms registered in Liaoning and Jilin with firms registered elsewhere. The SOE–private contrast remains positive in both groups, at approximately 44.3 percent in the border provinces and 85.5 percent elsewhere on the transformed outcome scale. The ownership divergence is therefore not confined to firms located closest to North Korea, suggesting that geographic exposure to the target market does not account for the main result. Panel B examines shipment mode. The corresponding SOE–private contrast is approximately 49.6 percent for rail and road shipments, whereas the river-and-sea estimate is close to zero and statistically insignificant. The divergence is therefore concentrated in land-based trade, where transactions are more likely to pass through border crossings at which firms directly encounter customs authorities.

5.4.3 Firm-Level Withdrawal versus Rerouting

An alternative explanation for the decline in North Korea trade is that firms simply rerouted their exports to other markets. If sales lost in North Korea were replaced elsewhere, firms’ total exports would be sustained even as their destination mix changed. We examine this possibility by tracing both the cessation of North Korea relationships and changes in the same firms’ exports across all destinations.

Figure 5 first documents the cessation of firm–HS8 export relationships with North Korea. A relationship is active when exports are positive and classified as an exit when positive exports in month $t - 1$ are followed by zero exports in month t . Active relationships rise from 6,344 in October 2006 to 7,634 in November and 7,329 in December, before falling to 3,524 in January 2007. Over the same period, exits increase from 5,210 in December to 7,156 in January. The contraction therefore includes firms stopping shipments of particular products to North Korea, not just reducing shipment values. The firm-level comparison below tests whether these lost sales were replaced in other markets.

To test firm-level rerouting, Table 9 compares firms that exported to North Korea before the shock with observably similar firms that did not. We use coarsened exact matching (CEM) to construct comparable groups based on province, trading-company status, pre-treatment export scale, product and destination scope, and product and destination concentration. We retain only strata containing both North Korea-exposed and unexposed firms and reweight comparison firms to balance the two groups within each stratum.

For the matched sample, we construct a balanced monthly panel from May 2006 through April 2007 and examine exports across all destinations. Firms previously exposed to North Korea experience substantially

Table 8: Ownership Effects by Border Exposure and Shipment Route

<i>Panel A. Firm registration province</i>		
Outcome	Other provinces Trade Value	Liaoning and Jilin Trade Value
Post $\times$ NK $\times$ SOE	0.618*** (0.158)	0.367*** (0.053)
Post $\times$ NK	-0.147 (0.095)	-0.433*** (0.043)
Destination-class FE	Yes	Yes
HS8-product FE	Yes	Yes
Firm-by-month FE	Yes	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07
Observations	2,169,948	2,169,948
Firms	2,965	2,965
<i>Panel B. Shipment mode</i>		
Outcome	River/sea Trade Value	Land (rail/road) Trade Value
Post $\times$ NK $\times$ SOE	-0.009 (0.037)	0.403*** (0.053)
Post $\times$ NK	0.143*** (0.016)	-0.507*** (0.045)
Destination-class-by-route FE	Yes	Yes
HS8-product-by-route FE	Yes	Yes
Firm-by-month-by-route FE	Yes	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07
Observations	4,339,896	4,339,896
Firms	2,965	2,965

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and the corresponding comparison-destination value on the comparison row. Post equals one from November 2006 onward, and NK identifies the North Korea row. Panel A compares firms registered in other provinces with firms registered in Liaoning or Jilin. Panel B compares river and sea shipments with land shipments by rail or road. Estimates are linear combinations from models that interact the Post $\times$ NK effect and ownership with the panel-specific group indicator. Panel A absorbs destination class, HS8 product, and firm-by-month fixed effects. Panel B absorbs destination-by-route, HS8-product-by-route, and firm-month-by-route fixed effects. Both models use May 2006–April 2007 and cluster standard errors by firm. Standard errors are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

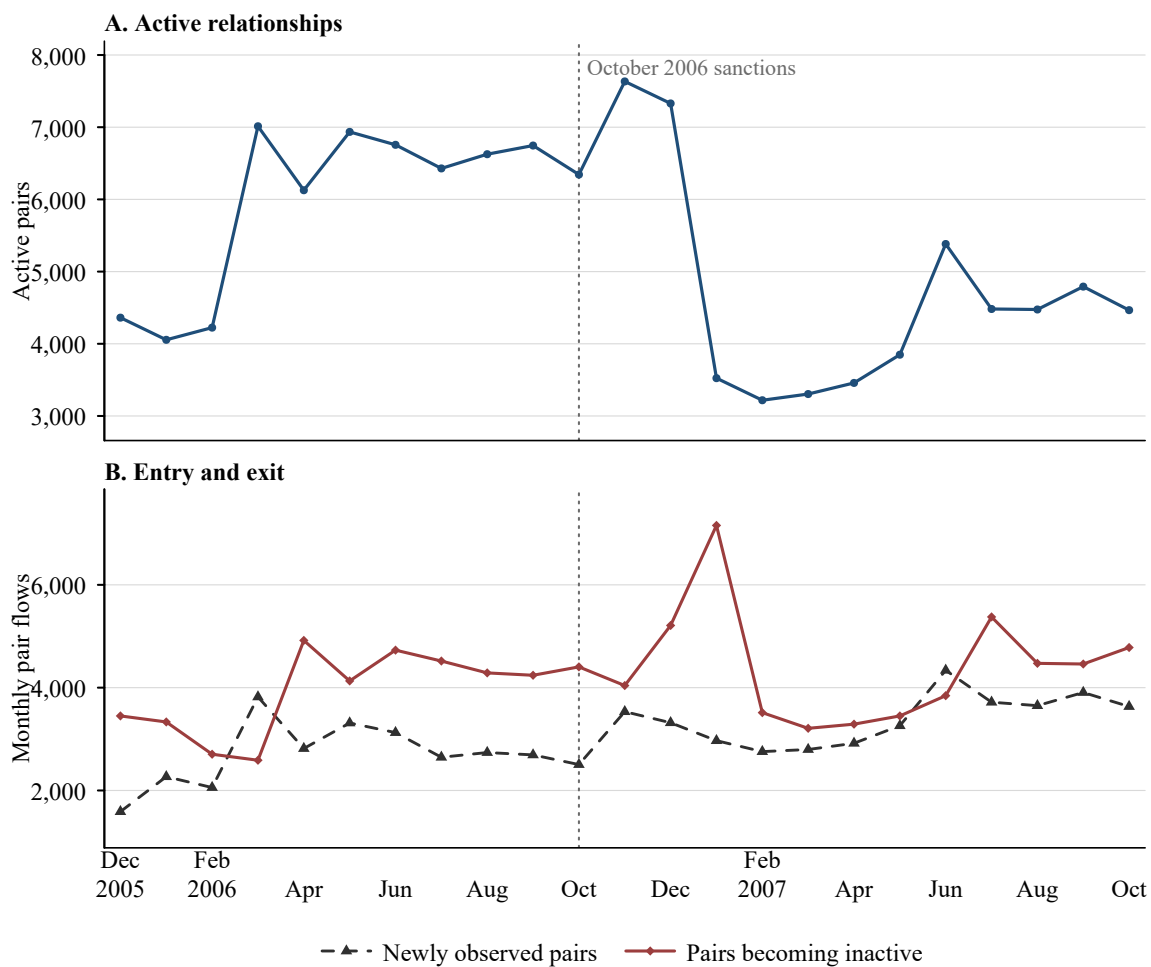


Figure 5: North Korea: Firm-Product Pair Activity and Turnover

Notes: The figure reports monthly counts of firm-HS8 relationships in the post-entry North Korea panel. Active relationships have positive exports to North Korea in a given month. Newly observed relationships record their first positive North Korea transaction. Exits occur when a relationship with positive exports in month $t - 1$ records zero exports in month t . The vertical line marks October 2006. Active relationships are measured on the left axis, while newly observed relationships and exits are measured on the right axis.

larger contractions than matched unexposed firms. Relative to matched unexposed firms, the estimates imply declines of approximately 56.9 percent in total exports and 15.0 percent in the number of active export destinations on the respective transformed outcome scales. Both results run against a simple rerouting account: firms lost international sales and served fewer markets, rather than replacing their North Korea business elsewhere.

The private firms driving the baseline contraction also entered the shock with smaller export operations and greater dependence on North Korea than SOEs. Table 2 shows that they exported fewer products to fewer destinations. Their average pre-shock North Korea export share was 27.4 percent, compared with 14.8 percent for SOEs. This concentration made access to the North Korean market especially consequential for private firms. The relationship cessations and firm-wide export declines together show that the adjustment involved a loss of international business, not simply a change in its destination.

Table 9: Export Contraction among Matched Exporters

Outcome	(1) Log total exports	(2) Log destination count
Post $\times$ NK-exposed	-0.842* (0.375)	-0.162* (0.080)
Firm FE	Yes	Yes
Calendar-month FE	Yes	Yes
Coarsened-exact-matching weights	Yes	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07
Observations	19,212	19,212
Firms	1,601	1,601

Notes: NK-exposed firms recorded positive exports to North Korea between January 2000 and June 2006. Matching strata combine province, a trading-company name indicator, and quartiles of pre-treatment log total exports, log product count, log destination count, product HHI, and destination HHI. Strata must contain both exposed and unexposed firms. Outcomes cover all export destinations from May 2006 through April 2007 and are measured as $\ln(1 + x)$. The reported coefficient is Post $\times$ NK-exposed, where Post begins in November 2006. Models absorb firm and calendar-month fixed effects, apply coarsened-exact-matching weights, and cluster standard errors by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. The estimation sample contains 1,601 firms and 19,212 firm-month observations.

6 Comparative Cases

Iran and official Dalai Lama receptions introduce variation in China's actual resolve to enforce economic disengagement while holding the implementing state constant. Our theory predicts that political standing can preserve commerce when credible guidance conveys permission to continue, but encourage withdrawal when it conveys a demand that officials intend to enforce. Iran extends the H1 comparison to another targeted UN sanctions regime. Meetings between the Dalai Lama and foreign government officials provide a setting for H3 in which Beijing initiated economic pressure over a core sovereignty interest. The information channel in both cases requires credible guidance to resolve uncertainty that less-connected firms still face, holding commercial stakes and protection from punishment comparable.

6.1 Iran Sanctions

Iran is the most similar case to North Korea. China supported targeted UN sanctions over a nuclear dispute in both cases, rather than a comprehensive trade embargo (United Nations Security Council, 2006a, 2006b). Resolution 1737 imposed nuclear- and missile-related controls in December 2006, leaving substantial commerce outside the formal prohibitions. For activities that Chinese officials permit, H1 predicts that credible reassurance reduces precautionary withdrawal among firms with better political access.

We apply the same firm–product triple-difference design used for North Korea, comparing pre–post changes in SOE and private exports to Iran relative to comparison destinations. Among firm–HS8 relationships with baseline-period exports to Iran, total SOE export value fell by 21.7 percent between July–December 2006 and January–June 2007, compared with 33.7 percent for private firms. The triple-difference estimate is positive and significant across models, indicating the same pattern that overcompliance is driven by private firms. Appendix D.2 provides the design and estimates.

6.2 Official Dalai Lama Receptions

Official Dalai Lama receptions provide a useful contrast to the North Korea and Iran sanctions. Beijing regarded high-level meetings with the Dalai Lama as challenges to its authority over Tibet and responded with diplomatic protests and threats of economic consequences (Fuchs & Klann, 2013). Unlike the multilateral sanctions examined above, China itself initiated economic pressure in these disputes over an issue it regarded as a core national interest (Swaine, 2011, p. 8).

This setting allows us to examine the opposite implication of our argument. When officials intend broader commercial retaliation, better political information should encourage rather than discourage withdrawal. We therefore expect SOEs to reduce trade more than private firms following an official Dalai Lama reception. This is the opposite ownership pattern from North Korea and Iran.

We examine Chinese imports during 2000–2006 using a staggered difference-in-differences design, with treatment beginning at each country’s first recorded official reception of the Dalai Lama. The panel covers 200 countries, including 23 treated countries in 17 cohorts whose receptions occurred between May 2001 and June 2006. Appendix D.3 reports the treatment cohorts and reception dates.

SOE importer participation declines relative to private-firm participation following an official reception. Across the two estimators, the results imply a decline of approximately 36.7–38.1 percent in SOE importer participation relative to private-firm participation on the transformed count scale. The participation contrast remains negative when allowing one month of anticipation or restricting the comparison group to never-treated countries. Table D.4 reports the full estimates and diagnostics.

The contrast is informative because political protection alone would generally make politically connected firms more willing to sustain trade. Instead, SOEs withdraw less than private firms under the North Korea and Iran sanctions but more following Dalai Lama receptions. This reversal is consistent with the informational mechanism: political access can sustain commerce when officials permit continued activity but encourage withdrawal when officials intend economic retaliation.

7 Conclusion

Our argument highlights the informational foundations of economic statecraft: how firms interpret governments' enforcement intentions shapes the economic impact of sanctions. Governments influence trade not only through the restrictions they adopt and the penalties they impose, but also through firms' access to credible information around what officials intend to enforce. When public communication leaves those intentions uncertain, firms facing the same written rules can make different economic decisions as firms differ in their ability to read the state's true resolve.

The North Korea evidence is consistent with this argument about political standing. Using state ownership as our primary proxy, we find that firms with weaker political standing withdrew from trade more than SOEs following the October 2006 nuclear test, with the divergence appearing in both export values and positive-trade probabilities. The pattern persists after accounting for products plausibly covered by the formal sanctions and extends to food and humanitarian categories. These findings are consistent with precautionary overcompliance if officials would have permitted the affected trade, a condition that the public statements and product screens alone cannot establish.

The theory predicts that political access can either preserve commerce or facilitate withdrawal, depending on what guidance reveals about officials' enforcement intentions while other firms remain uncertain. Guidance reduces precautionary withdrawal when officials permit continued trade and increases compliance when they intend to punish continuation. When the relevant demand is equally clear to all firms, this informational advantage disappears. The comparative cases document contrasting ownership patterns: SOE importer participation declined relative to private-firm participation after official Dalai Lama receptions, whereas SOE exports declined less in Iran, as in North Korea. The Dalai Lama pattern is compatible with H3 only if political access resolved uncertainty about an enforced demand to withdraw. Because the comparison does not observe firms' information, it cannot distinguish this explanation from political obligations or direct instructions.

Access to credible information is therefore an important distributional dimension of economic statecraft. Firms outside established political networks may forgo commercial opportunities that better-informed firms retain, magnifying the economic costs of sanctions beyond their intended scope. Our theory suggests that making reliable guidance more broadly available could reduce unnecessary disengagement while improving compliance when withdrawal is intended. Understanding sanctions therefore requires attention not only to what governments prohibit and how they punish defiance, but also to which firms can overcome the information asymmetry surrounding the state's true intentions.

References

- Adarkwah, G. K., Dorobantu, S., Sabel, C. A., & Zilja, F. (2024). Geopolitical volatility and subsidiary investments. *Strategic Management Journal*, 45(11), 2275–2306. <https://doi.org/10.1002/smj.3631>.
- Ahn, D. P., & Ludema, R. D. (2020). The sword and the shield: The economics of targeted sanctions. *European Economic Review*, 130, Article 103587. <https://doi.org/10.1016/j.euroecorev.2020.103587>.
- Aytun, U., Hinz, J., & Özgüzel, C. (2025). Shooting down trade: Firm-level effects of embargoes. *Journal of Economic Behavior & Organization*, 231, Article 106821. <https://doi.org/10.1016/j.jebo.2024.106821>.
- Baccini, L., Pinto, P. M., & Weymouth, S. (2017). The distributional consequences of preferential trade liberalization: Firm-level evidence. *International Organization*, 71(2), 373–395. <https://doi.org/10.1017/S002081831700011X>.
- Bai, K., & Li, Q. (2026). *Navigating targeted sanctions: A real options theory of American firm responses and financial performance* [Working paper]. Retrieved September 17, 2026, from https://gripe.polisci.ucla.edu/wp-content/uploads/sites/121/2026/08/Bai_Li_2026.pdf
- Bapat, N. A., & Kwon, B. R. (2015). When are sanctions effective? a bargaining and enforcement framework. *International Organization*, 69(1), 131–162. <https://doi.org/10.1017/S0020818314000290>.
- Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, 76(2), 169–217. <https://doi.org/10.1086/259394>.
- Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics*, 119(1), 249–275. <https://doi.org/10.1162/003355304772839588>.
- Bruton, G. D., Peng, M. W., Ahlstrom, D., Stan, C., & Xu, K. (2015). State-owned enterprises around the world as hybrid organizations. *Academy of Management Perspectives*, 29(1), 92–114. <https://doi.org/10.5465/amp.2013.0069>.
- Callaway, B., & Sant’Anna, P. H. C. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2), 200–230. <https://doi.org/10.1016/j.jeconom.2020.12.001>.
- Chen, J., & Dickson, B. J. (2008). Allies of the state: Democratic support and regime support among China’s private entrepreneurs. *The China Quarterly*, 196, 780–804. <https://doi.org/10.1017/S0305741008001124>.
- Davis, C. L., Fuchs, A., & Johnson, K. (2019). State control and the effects of foreign relations on bilateral trade. *Journal of Conflict Resolution*, 63(2), 405–438. <https://doi.org/10.1177/0022002717739087>.
- Davis, C. L., Li, J., & Miyano, S. (2024). Peer conformity and competition shape how business managers evaluate withdrawals from Russia amid the Ukraine War. *Proceedings of the National Academy of Sciences*, 121(37), Article e2406471121. <https://doi.org/10.1073/pnas.2406471121>.
- Davis, C. L., & Meunier, S. (2011). Business as usual? economic responses to political tensions. *American Journal of Political Science*, 55(3), 628–646. <https://doi.org/10.1111/j.1540-5907.2010.00507.x>.
- Dickson, B. J. (2003). *Red capitalists in China: The party, private entrepreneurs, and prospects for political change*. Cambridge University Press. <https://doi.org/10.1017/CBO9780511510045>.
- Drezner, D. W. (2011). Sanctions sometimes smart: Targeted sanctions in theory and practice. *International Studies Review*, 13(1), 96–108. <https://doi.org/10.1111/j.1468-2486.2010.01001.x>.

- Duchâtel, M., & Schell, P. (2013, December). *China's policy on North Korea: Economic engagement and nuclear disarmament* (SIPRI Policy Paper No. 40). Stockholm International Peace Research Institute. Solna. Retrieved September 13, 2026, from <https://www.sipri.org/sites/default/files/files/PP/SIPRIPP40.pdf>
- Early, B. R. (2015). *Busted sanctions: Explaining why economic sanctions fail*. Stanford University Press.
- Farrell, H., & Newman, A. L. (2019). Weaponized interdependence: How global economic networks shape state coercion. *International Security*, 44(1), 42–79. https://doi.org/10.1162/isec_a_00351.
- Fuchs, A., & Klann, N.-H. (2013). Paying a visit: The Dalai Lama Effect on international trade. *Journal of International Economics*, 91(1), 164–177. <https://doi.org/10.1016/j.jinteco.2013.04.007>.
- Gehlbach, S., & Sonin, K. (2014). Government control of the media. *Journal of Public Economics*, 118, 163–171. <https://doi.org/10.1016/j.jpubeco.2014.06.004>.
- Haggard, S., & Noland, M. (2018). Networks, trust and trade: The microeconomics of China–North Korea integration. *Asian Economic Journal*, 32(3), 277–299. <https://doi.org/10.1111/asej.12159>.
- Han, C., Li, X., & Oi, J. C. (2022). Firms as revenue safety nets: Political connections and returns to the Chinese state. *The China Quarterly*, 251, 683–704. <https://doi.org/10.1017/S0305741022000352>.
- Han, S. (2025). Symbolic sanctions and status signaling: Understanding China's use of sanctions as an emerging great power. *Pacific Focus*, 40(2), 392–422. <https://doi.org/10.1111/pafo.70002>.
- Haveman, H. A., Jia, N., Shi, J., & Wang, Y. (2017). The dynamics of political embeddedness in China. *Administrative Science Quarterly*, 62(1), 67–104. <https://doi.org/10.1177/0001839216657311>.
- Henisz, W. J., & Zelner, B. A. (2010). The hidden risks in emerging markets. *Harvard Business Review*, 88(4), 88–95.
- Hillman, A. J. (2005). Politicians on the board of directors: Do connections affect the bottom line? *Journal of Management*, 31(3), 464–481. <https://doi.org/10.1177/0149206304272187>.
- Holland, A. C. (2016). Forbearance. *American Political Science Review*, 110(2), 232–246. <https://doi.org/10.1017/S0003055416000083>.
- Hollyer, J. R., Rosendorff, B. P., & Vreeland, J. R. (2011). Democracy and transparency. *The Journal of Politics*, 73(4), 1191–1205. <https://doi.org/10.1017/S0022381611000880>.
- Hsueh, R. (2016). State capitalism, Chinese-style: Strategic value of sectors, sectoral characteristics, and globalization. *Governance*, 29(1), 85–102. <https://doi.org/10.1111/gove.12139>.
- Hu, H. W., & Xu, D. (2022). Manager or politician? Effects of CEO pay on the performance of state-controlled Chinese listed firms. *Journal of Management*, 48(5), 1160–1187. <https://doi.org/10.1177/01492063211015301>.
- Jung, Y. S., Owen, E., & Shim, G. S. (2021). Heterogeneity in how investors respond to disputes: Greenfield foreign direct investment and coindustrial disputes. *The Journal of Politics*, 83(4), 1260–1274. <https://doi.org/10.1086/715164>.
- Kertzer, J. D. (2016). *Resolve in international politics*. Princeton University Press.
- Khwaja, A. I., & Mian, A. (2005). Do lenders favor politically connected firms? Rent provision in an emerging financial market. *The Quarterly Journal of Economics*, 120(4), 1371–1411. <https://doi.org/10.1162/003355305775097524>.

- Kubinec, R., Lee, H. N., & Tomashevskiy, A. (2024). Why corporate political connections can impede investment. *Comparative Political Studies*, 57(11), 1804–1836. <https://doi.org/10.1177/00104140231204227>.
- Kubinec, R., Lee, H. N.-K., & Tomashevskiy, A. (2021). Politically connected companies are less likely to shutdown due to COVID-19 restrictions. *Social Science Quarterly*, 102(5), 2155–2169. <https://doi.org/10.1111/ssqu.13040>.
- Leutert, W., & Vortherms, S. A. (2021). Personnel power: Governing state-owned enterprises. *Business and Politics*, 23(3), 419–437. <https://doi.org/10.1017/bap.2021.5>.
- Li, H. (2006, October 19). Chaoxian heshi hou, Dandong bianmao shiri [Ten days of border trade in Dandong after North Korea's nuclear test] [In Chinese]. *Nanfang Zhoumo [Southern Weekly]*, A01.
- Lin, F., Hu, C., & Fuchs, A. (2019). How do firms respond to political tensions? the heterogeneity of the Dalai Lama Effect on trade. *China Economic Review*, 54, 73–93. <https://doi.org/10.1016/j.chieco.2018.10.009>.
- Mangini, M.-D. (2024). The economic coercion trilemma. *Journal of Conflict Resolution*, 68(6), 1226–1251. <https://doi.org/10.1177/00220027231191530>.
- Ministry of Foreign Affairs of the People's Republic of China. (2006a, October 17). *Foreign ministry spokesperson liu jianchao's regular press conference on october 17, 2006* [Chinese-language transcript; title translated]. Retrieved September 10, 2026, from https://www.mfa.gov.cn/web/gjhdq_676201/gj_676203/yz_676205/1206_676404/fyrygth_676412/200610/t20061017_7975974.shtml
- Ministry of Foreign Affairs of the People's Republic of China. (2006b, October 24). *Foreign ministry spokesperson liu jianchao's regular press conference on october 24, 2006* [Chinese-language transcript; title translated]. Retrieved September 10, 2026, from https://www.mfa.gov.cn/gjhdq_676201/gj_676203/yz_676205/1206_676404/fyrygth_676412/200610/t20061024_7975977.shtml
- Morgan, T. C., Syropoulos, C., & Yotov, Y. V. (2023). Economic sanctions: Evolution, consequences, and challenges. *Journal of Economic Perspectives*, 37(1), 3–29. <https://doi.org/10.1257/jep.37.1.3>.
- Naoi, M., Shi, W., & Zhu, B. (2022). “Yes-Man” firms: Government campaign and policy positioning of businesses in China. *International Studies Quarterly*, 66(4), Article sqac075. <https://doi.org/10.1093/isq/sqac075>.
- Shi, W., Markóczy, L., & Stan, C. V. (2014). The continuing importance of political ties in China. *Academy of Management Perspectives*, 28(1), 57–75. <https://doi.org/10.5465/amp.2011.0153>.
- Smith, A. M., & Poplin, C. M. (2023). Keeping sanctions “smart”: Calibrating U.S. sanctions policy to overcome overcompliance. *North Carolina Journal of International Law*, 48(3), 499–544. <https://journals.law.unc.edu/ncjil/wp-content/uploads/sites/3/2023/06/Smith-Poplin.pdf>
- Snyder, S., & Wit, J. S. (2007, February). *Chinese views: Breaking the stalemate on the Korean peninsula* (Special Report No. 183). United States Institute of Peace. Washington, DC. Retrieved August 4, 2026, from <https://www.usip.org/sites/default/files/sr183.pdf>
- Stępień, B., Early, B. R., Grauvogel, J., Preble, K. A., & Truskolaski, S. (2024). The impact of external pressure on companies' responses to sanctions: An international comparative study. *European Journal on Criminal Policy and Research*, 30(2), 301–326. <https://doi.org/10.1007/s10610-024-09576-y>.
- Stern, R. E., & O'Brien, K. J. (2012). Politics at the boundary: Mixed signals and the Chinese state. *Modern China*, 38(2), 174–198. <https://doi.org/10.1177/0097700411421463>.

- Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2), 175–199. <https://doi.org/10.1016/j.jeconom.2020.09.006>.
- Swaine, M. D. (2011). China's assertive behavior—part one: On “core interests”. *China Leadership Monitor*, (34), 1–25. <https://www.hoover.org/research/chinas-assertive-behavior-part-one-core-interests>
- Tan, Y., & Davis, C. L. (2023). The limits of liberalization: World Trade Organization entry and Chinese state-owned firms. *International Studies Quarterly*, 67(3), Article sqad056. <https://doi.org/10.1093/isq/sqad056>.
- Twomey, C. P. (2008). Explaining Chinese foreign policy toward North Korea: Navigating between the Scylla and Charybdis of proliferation and instability. *Journal of Contemporary China*, 17(56), 401–423. <https://doi.org/10.1080/10670560802000167>.
- United Nations Security Council. (2006a, October 14). *Resolution 1718 (2006)* (S/RES/1718 (2006)). United Nations. Retrieved August 4, 2026, from <https://docs.un.org/S/RES/1718%20%282006%29>
- United Nations Security Council. (2006b, December 23). *Resolution 1737 (2006)* (S/RES/1737 (2006)). United Nations. Retrieved August 4, 2026, from <https://docs.un.org/S/RES/1737%20%282006%29>
- Verdier, P.-H. (2023). Sanctions overcompliance: What, why, and does it matter? *North Carolina Journal of International Law*, 48, 471–498. <https://journals.law.unc.edu/ncjil/wp-content/uploads/sites/3/2023/06/Verdier.pdf>
- Wang, D., Weiner, R. J., Li, Q., & Jandhyala, S. (2021). Leviathan as foreign investor: Geopolitics and sovereign wealth funds. *Journal of International Business Studies*, 52(7), 1238–1255. <https://doi.org/10.1057/s41267-021-00415-4>.
- Wang, F. Y. (2021). Barking without biting: Understanding Chinese media campaigns during foreign policy disputes. *Security Studies*, 30(4), 517–549. <https://doi.org/10.1080/09636412.2021.1979843>.
- Weeks, J. L. (2008). Autocratic audience costs: Regime type and signaling resolve. *International Organization*, 62(1), 35–64. <https://doi.org/10.1017/S0020818308080028>.
- Wei, Y., Jia, N., & Bonardi, J.-P. (2023). Corporate political connections: A multidisciplinary review. *Journal of Management*, 49(6), 1870–1910. <https://doi.org/10.1177/01492063221136839>.
- Weiss, J. C., & Dafoe, A. (2019). Authoritarian audiences, rhetoric, and propaganda in international crises: Evidence from China. *International Studies Quarterly*, 63(4), 963–973. <https://doi.org/10.1093/isq/sqz059>.
- Wellhausen, R. L., & Zhu, B. (2026). Exiting Russia. *American Political Science Review*, 1–18. <https://doi.org/10.1017/S000305542610152X>.
- Whang, T. (2011). Playing to the home crowd? symbolic use of economic sanctions in the United States. *International Studies Quarterly*, 55(3), 787–801. <https://doi.org/10.1111/j.1468-2478.2011.00668.x>.
- Zhang, K. V. (2024). Just do it: Explaining the characteristics and rationale of Chinese economic sanctions. *Texas National Security Review*, 7(3), 16–38. <https://doi.org/10.26153/tsw/54044>.
- Zhou, K. Z., Gao, G. Y., & Zhao, H. (2017). State ownership and firm innovation in China: An integrated view of institutional and efficiency logics. *Administrative Science Quarterly*, 62(2), 375–404. <https://doi.org/10.1177/0001839216674457>.

Appendix Contents

A	Estimation, Inference, and Import-Side Results	36
A.1	The Extensive Margin	36
A.2	Alternative Aggregation and Clustering	36
A.3	Import-Side Ownership Response	36
B	UN Sanctions Document - Product Code Matching	38
B.1	Coarse (2-digit) and Unigram (4-digit) screens	38
B.2	Source records and AI-assisted mapping	38
B.3	Agreement statistics	39
C	Robustness and Additional Analyses	41
C.1	Cohort, Timing, and Seasonality	41
C.2	Robustness of Alternative Political-Standing Measures	41
C.3	Second Nuclear Test	43
D	The Iran Comparison and Dalai Lama Receptions	44
D.1	Case Context and Empirical Coverage	44
D.2	Most Similar Case: UN Sanctions on Iran	45
D.3	Most Different Case: Dalai Lama Receptions	47
E	Political Standing and Resolve Reading: A Formal Model	50

A Estimation, Inference, and Import-Side Results

A.1 The Extensive Margin

Table A.1 reports OLS linear-probability estimates for positive trade. The decline in active firm–product relationships is driven by private firms, while the corresponding SOE adjustment is close to zero. Relative to comparison destinations, the post-shock decline in positive-trade probability is 4.9 percentage points smaller for SOEs than for private firms in the short window and 6.4 percentage points smaller in the broad window. The ownership divergence therefore appears in both export values and the likelihood that firm–product relationships remain active.

Table A.1: Extensive-Margin Estimates

	(1)	(2)
	Any trade	Any trade
Post $\times$ NK $\times$ SOE	0.0494*** (0.00816)	0.0636*** (0.00751)
Post $\times$ NK	-0.0516*** (0.00648)	-0.0660*** (0.00622)
Estimator	OLS	OLS
Timeframe	May 06–Apr. 07	Dec. 05–Oct. 07
Destination-class FE	Yes	Yes
HS8-product FE	Yes	Yes
Firm-by-month FE	Yes	Yes
Firm-clustered SE	Yes	Yes
Observations	2,173,720	4,156,818

Notes: The outcome equals one when the destination-class row has positive trade. The coefficient rows report Post $\times$ NK $\times$ SOE and Post $\times$ NK. Post equals one from November 2006 onward, and NK identifies the North Korea row. Both OLS linear-probability models absorb destination class, HS8 product, and firm-by-month fixed effects and cluster standard errors by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

A.2 Alternative Aggregation and Clustering

Table A.2 replaces the comparison-destination mean with the sum across observed non-North-Korea destinations and re-estimates the models using both log and inverse-hyperbolic-sine transformations. Across these specifications, the main difference-in-differences effect remains negative, while the SOE interaction remains positive. The relative magnitudes indicate that the post-shock contraction is driven primarily by private firms, with SOEs exhibiting little corresponding decline. The pattern is robust to firm-clustered standard errors, two-way clustering by firm and month, and specifications that interact treatment with the four predetermined firm characteristics defined in Section 4.

A.3 Import-Side Ownership Response

We also examine whether the ownership pattern is mirrored in imports. Table A.3 shows a negative post-shock difference-in-differences estimate and a positive SOE interaction. The relative magnitudes again indicate that

Table A.2: Alternative Outcomes, Controls, and Clustering

	(1) Sum log	(2) Sum asinh	(3) Sum log	(4) Sum asinh	(5) Sum log	(6) Sum asinh
Post $\times$ NK $\times$ SOE	0.456*** (0.0520)	0.500*** (0.0570)	0.456*** (0.0731)	0.500*** (0.0802)	0.402*** (0.0674)	0.442*** (0.0739)
Post $\times$ NK	-0.419*** (0.0403)	-0.464*** (0.0445)	-0.419*** (0.0637)	-0.464*** (0.0704)	-0.553*** (0.0923)	-0.616*** (0.101)
Outcome	Log(1 + sum)	Asinh(sum)	Log(1 + sum)	Asinh(sum)	Log(1 + sum)	Asinh(sum)
Predetermined profile interactions	No	No	No	No	Yes	Yes
Clustering	Firm	Firm	Firm + month	Firm + month	Firm + month	Firm + month
Destination-class FE	Yes	Yes	Yes	Yes	Yes	Yes
HS8-product FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Timeframe	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07
Observations	4156818	4156818	4156818	4156818	4156818	4156818

Notes: The coefficient rows report Post $\times$ NK $\times$ SOE and Post $\times$ NK. Post equals one from November 2006 onward, and NK identifies the North Korea row. All columns use total non-NK exports on the comparison row and the December 2005–October 2007 window. The outcomes are $\ln(1 + y)$ in odd-numbered columns and $\text{asinh}(y)$ in even-numbered columns. Every model absorbs destination class, HS8 product, and firm-by-month fixed effects. Columns 1–2 cluster by firm. Columns 3–6 cluster by firm and calendar month. Columns 5–6 fully interact Post and NK with four mean-centered, January 2000–June 2006 firm characteristics: log total exports, North Korea’s share of firm exports, the product-export Herfindahl index, and the destination-export Herfindahl index. No other firm-level controls are included. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

the contraction is driven primarily by private firms, while the corresponding SOE adjustment is close to zero. Figure A.1 traces these differences over time, showing a pronounced decline in private-firm imports from North Korea following the shock and comparatively little adjustment among SOEs.

Table A.3: Ownership Triple Difference for Imports

	May 06–Apr. 07	Dec. 05–Oct. 07
Post $\times$ NK $\times$ SOE	0.651*** (0.1149)	0.743*** (0.1080)
Post $\times$ NK	-0.693*** (0.0810)	-0.852*** (0.0793)
Origin-class FE	Yes	Yes
HS8-product FE	Yes	Yes
Firm-by-month FE	Yes	Yes
Observations	157,162	300,362

Notes: The outcome is $\ln(1 + y)$, using imports from North Korea on the NK row and the mean across other observed origins on the comparison row. Post equals one from November 2006 onward, and NK identifies the North Korea row. The coefficient rows report Post $\times$ NK $\times$ SOE and Post $\times$ NK. Models absorb origin class, HS8 product, and firm-by-month fixed effects and cluster standard errors by firm. Ownership is frozen using the export-analysis rule. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

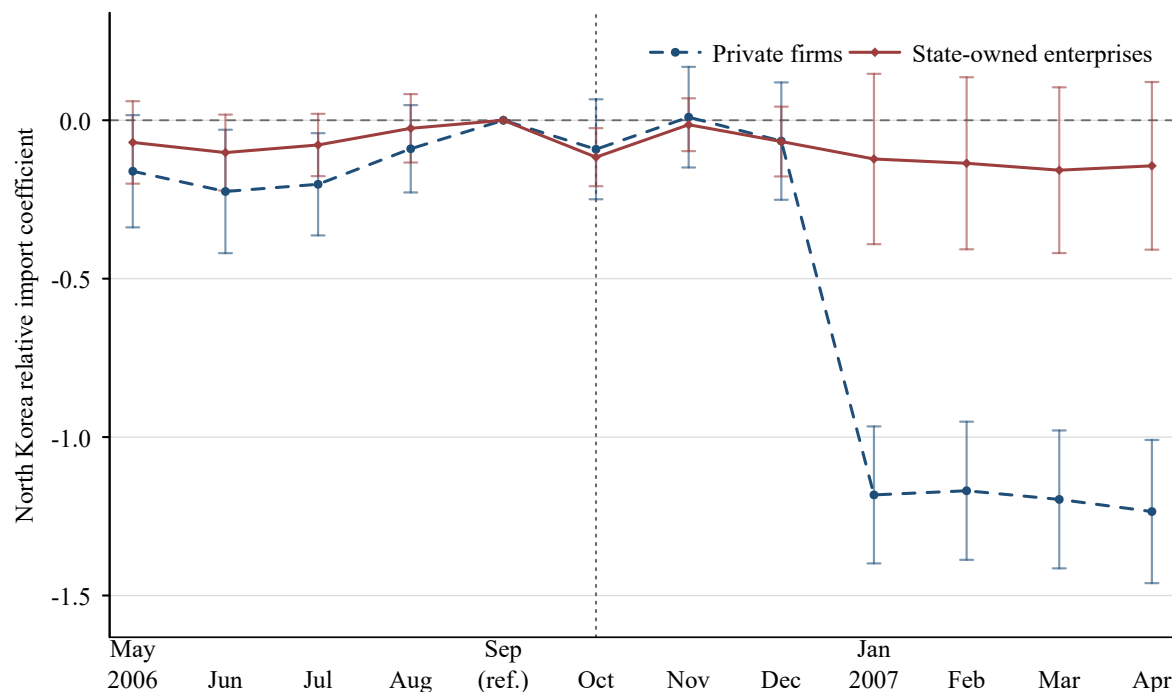


Figure A.1: Import Event Paths by Firm Ownership

Notes: Monthly North-Korea-versus-other-origin import coefficients are plotted separately for private firms and SOEs. September 2006 is omitted. Bars show 95-percent confidence intervals using firm-clustered standard errors.

B UN Sanctions Document - Product Code Matching

B.1 Coarse (2-digit) and Unigram (4-digit) screens

We construct three alternative screens to identify products plausibly covered by the UN sanctions. First, the coarse screen removes four HS2 chapters: inorganic chemicals (28), organic chemicals (29), explosives and pyrotechnic products (36), and arms and ammunition (93). This whole-chapter exclusion is intentionally broad and removes both potentially controlled and clearly civilian products.

Second, the unigram screen provides a more targeted but still deterministic lexical mapping. We lowercase and normalize the sanctioned-item titles, split them into single-word tokens, remove common English and generic classification words, and retain tokens with at least five characters. We then exact-match these tokens to the descriptions of the 1,244 official HS-2002 four-digit headings. To limit generic matches, a token must appear in no more than 12 headings, approximately one percent of the reference set. A heading is flagged if it contains at least one retained token. This procedure identifies 321 HS4 headings. Together, the coarse and unigram screens provide deliberately broad exclusion rules against which we compare the preferred AI-assisted mapping.

B.2 Source records and AI-assisted mapping

Third, we construct an AI-assisted mapping using the complete technical descriptions contained in the UN control lists. The classification begins with 540 control-list records extracted from S/2006/814, S/2006/815, and

Table B.1: Coarse and Civilian Product-Screen Definitions

Screen	HS chapters	Definition
Coarse sanction screen	28, 29, 36, 93	Entire chapters for inorganic chemicals, organic chemicals, explosives/pyrotechnics, and arms/ammunition
Food	01–21	Live animals, foodstuffs, agricultural products, and prepared foods
Broader humanitarian	01–21, 30, 39, 63, 90, 94	Food, pharmaceuticals, plastics, made-up textiles, medical/precision instruments, and basic household goods, after removing all 66 mapped HS4 headings

Notes: The coarse and humanitarian definitions operate at HS2 unless an HS4 exclusion is stated. These definitions specify the sample restrictions used in the product-scope analyses.

S/2006/816, containing a total of 536 unique normalized descriptions. Each record retains the source document, page, control reference, item title, complete description, and a stable identifier. The mapping evaluates each complete description against the HS 2002 reference and assigns one of four outcomes: *assigned HS4*, *multiple products requiring separate classification*, *non-physical software or technology*, or *insufficient information*. An assigned code contains four digits and correspond to unique official HS 2002 heading. Classification is based on the material itself rather than on end-use.

This procedure matches HS4 headings to 318 records, identifies 123 records requiring separation into multiple products, classifies 64 as non-physical software or technology, and finds 35 to contain insufficient information for assignment to a single heading. The matched 318 assigned records are then assigned to 66 unique HS4 headings, which we exclude as sanctioned products in the regressions.

We validate the AI-assisted mapping through an independent human-review exercise. We drew a random sample without replacement from the We drew a random sample of 130 products without replacement from the 536 unique normalized descriptions (using seed 20260808). Of these, 85 products were evaluated by a single reviewer, while 45 were evaluated independently by two reviewers to assess inter-reviewer agreement. The 45 double-reviewed products were divided evenly across the three possible reviewer pairs: 15 were assigned to Reviewers 1 and 2, 15 to Reviewers 1 and 3, and 15 to Reviewers 2 and 3. No product was reviewed by all three reviewers. This allocation resulted in Reviewer 1 evaluating 65 products and Reviewers 2 and 3 evaluating 55 products each, for a total of 175 reviewer–item evaluations.

The reviewer workbooks were designed to ensure that the human evaluations were independent of the AI-assisted classifications. Reviewers were not shown the AI-assigned HS4 code. Instead, they received only the original control-list description, associated source data, and the HS code (HS 2002) reference. For each product, reviewers first selected one of the same four mapping outcomes used in the AI-assisted procedure. When they judged that a single HS4 heading could be assigned, they recorded the corresponding four-digit code, official HS description, confidence rating, and a brief rationale. If a unique heading could not be assigned, they were told to classify the record as containing multiple products, non-physical content, or insufficient information.

B.3 Agreement statistics

Across the 175 reviewer–item decisions, human reviewers and the AI-assisted procedure agree on whether a product can be assigned to a single HS4 heading in 139 cases, which corresponds to 79.4 percent agreement. Agreement is slightly lower under stricter criteria, where reviewers and the AI assign the same four-category

Table B.2: AI-Assisted Sanctioned-Product Mapping by HS2 Chapter

HS2	Chapter description	Included HS4 headings
28	Inorganic chemicals	2805, 2811, 2812, 2813, 2826, 2830, 2837, 2844, 2845
29	Organic chemicals	2905, 2914, 2918, 2920, 2921, 2922, 2930, 2931, 2933, 2934
30	Pharmaceutical products	3002
36	Explosives and pyrotechnic products	3601, 3602, 3603
38	Miscellaneous chemical products	3801, 3815
39	Plastics and articles thereof	3914
68	Articles of stone, plaster, cement, and similar materials	6815
81	Other base metals; cermets; articles thereof	8104, 8106, 8108, 8109
84	Nuclear reactors, boilers, machinery, and mechanical appliances	8401, 8411, 8412, 8413, 8414, 8417, 8419, 8421, 8422, 8424, 8428, 8471, 8479, 8481, 8482, 8484
85	Electrical machinery and equipment	8504, 8505, 8526, 8532, 8540, 8542, 8543
87	Vehicles other than railway or tramway rolling stock	8705
88	Aircraft, spacecraft, and parts thereof	8802, 8803
90	Optical, measuring, medical, and precision instruments	9006, 9013, 9014, 9015, 9022, 9026, 9027, 9030, 9031

Notes: Repeated record-level assignments collapse to the 66 unique HS 2002 four-digit headings listed here. The source workbook records document, page, description, rationale, confidence, and authority web address for each assigned record.

status in 133 cases (76.0 percent) and agree on both the status and the recorded HS4 code in 125 cases (71.4 percent).

The 45 products evaluated independently by two reviewers allow us to benchmark AI–human agreement against human–human agreement. Across these double-coded products, reviewer agreement is 71.1 percent for assignability, 66.7 percent for the four-category status, and 57.8 percent for the strict status-plus-code criterion. The corresponding nominal Krippendorff’s alpha values are 0.418, 0.529, and 0.521, respectively. Pairwise agreement on the four-category status is 80 percent between Reviewers 1 and 2 and 60 percent for each pair involving Reviewer 3. Each reviewer pair evaluates 15 common products. The audit also identifies eight responses in which the entered HS4 code is inconsistent with the reviewer-selected status.

Table B.3: Human Review Sampling and Agreement Statistics

Statistic	Value	Denominator
<i>Panel A. Review design</i>		
Source control-list records	540	records
Unique normalized descriptions	536	products
Unique products sampled	130	products
Single-reviewed products	85	products
Double-reviewed products	45	products
Reviewer-item decisions	175	decisions
<i>Panel B. Human–AI concordance</i>		
Decision-status agreement	76.0%	175 decisions
Assignability agreement	79.4%	175 decisions
Strict status-plus-code agreement	71.4%	175 decisions
<i>Panel C. Human–human reliability</i>		
Decision-status agreement	66.7%	45 products
Assignability agreement	71.1%	45 products
Strict status-plus-code agreement	57.8%	45 products
Krippendorff’s alpha: decision status	0.529	45 products
Krippendorff’s alpha: assignability	0.418	45 products
Krippendorff’s alpha: strict status plus code	0.521	45 products

Notes: Assignability agreement means both coders reach the same conclusion about whether one HS4 heading can be assigned. Strict agreement requires identical status and identical entered code, including a blank code for non-assigned outcomes. Human–AI concordance measures agreement across 175 reviewer-item decisions. Human–human statistics use the 45 products assigned to two reviewers.

C Robustness and Additional Analyses

C.1 Cohort, Timing, and Seasonality

Table C.1 compares the full post-entry sample with incumbent firm–HS8 relationships established before December 2005, using the December 2005–October 2007 window and summed comparison-destination exports. The SOE–private coefficient falls from 0.456 in the full sample to 0.267 among incumbents, equivalent to contrasts of approximately 57.8 and 30.6 percent, respectively, on the transformed outcome scale. Among incumbents, the corresponding contrasts are approximately 30.8 percent when we drop October, 23.0 percent when we move the treatment date to July, and 33.5 percent when we add North-Korea-by-calendar-month fixed effects. The ownership divergence thus persists among established relationships, but its estimated magnitude is sensitive to the composition of the estimation sample. Because the incumbent restriction excludes relationships entering both before and after the sanctions, the attenuation cannot be interpreted as the share attributable to post-shock entry.

Table C.1: Cohort, Timing, and Seasonality Robustness

	(1) Baseline	(2) Oct. donut	(3) Baseline	(4) Oct. donut	(5) July date	(6) Seasonal
Outcome			Trade Value			
Post × NK × SOE	0.456*** (0.0520)	0.462*** (0.0522)	0.267*** (0.0386)	0.269*** (0.0395)	0.207*** (0.0345)	0.289*** (0.0416)
Post × NK	-0.419*** (0.0403)	-0.430*** (0.0414)	-0.194*** (0.0339)	-0.197*** (0.0348)	-0.153*** (0.0316)	-0.211*** (0.0364)
Sample	Full	Full	Incumbent	Incumbent	Incumbent	Incumbent
Treatment timing	Post-Nov.	Post-Nov.	Post-Nov.	Post-Nov.	Post-July	Post-Nov.
October 2006	Pre	Dropped	Pre	Dropped	Post	Pre
Seasonality controls	No	No	No	No	No	NK × month-of-year
Destination-class FE	Yes	Yes	Yes	Yes	Yes	Yes
HS8-product FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Timeframe	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07	Dec. 05–Oct. 07
Observations	4156818	3977482	2838798	2715372	2838798	2838798

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and total non-NK exports on the comparison row. All columns use December 2005–October 2007. The coefficient rows report Post × NK × SOE and Post × NK. Incumbents first exported the HS8 product to North Korea before December 2005. Baseline Post begins in November 2006. October-donut specifications drop October, and the July specification treats July 2006 onward as post. The seasonal specification adds NK-by-calendar-month-of-year interactions and their ownership interactions. All models absorb destination class, HS8 product, and firm-by-month fixed effects and cluster standard errors by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

Figure C.1 reports the ownership-difference path for incumbent relationships. The pre-treatment leads are jointly statistically insignificant. Table C.2 provides a complementary diagnostic and likewise shows no statistically significant differential pre-treatment trends across the broad, pre-July, or short pre-periods. These diagnostics apply to the incumbent sample and do not directly validate the larger full-sample estimate.

C.2 Robustness of Alternative Political-Standing Measures

Table C.3 reports additional robustness checks for the triple difference estimates using alternative measures of firms' political standing. As in the main analysis, we consider whether firms are registered in Beijing, whether they are located in one of the four centrally administered municipalities, and whether they are relatively large based on pre-shock export scale. These measures capture complementary dimensions of political access, administrative

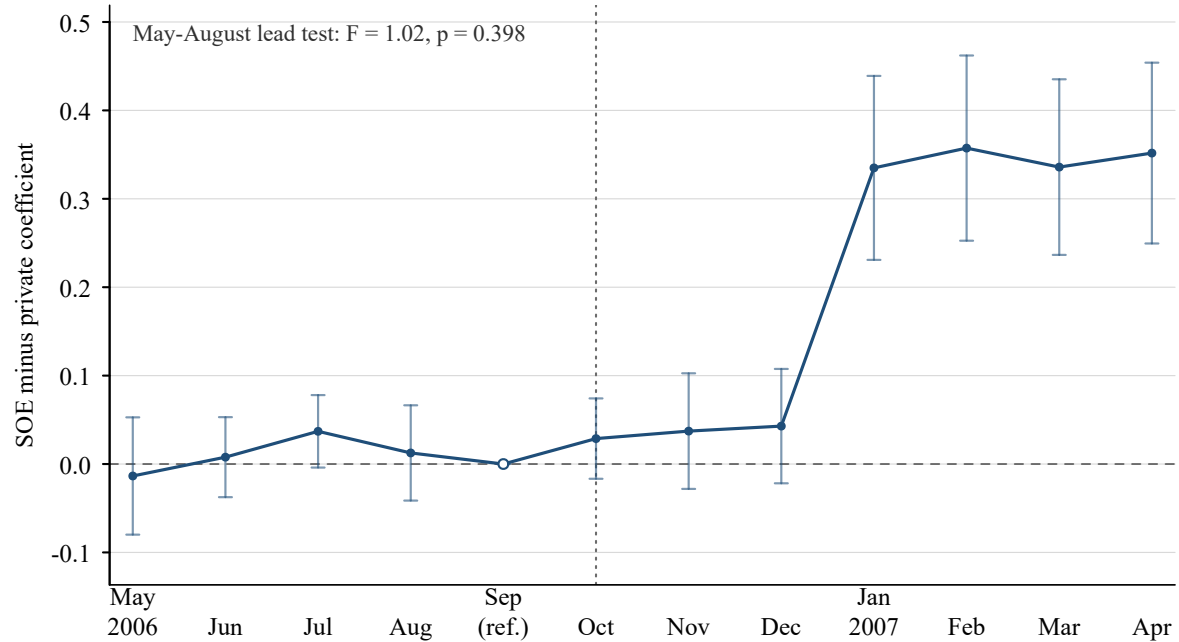


Figure C.1: Incumbent SOE-Minus-Private Event Path

Notes: The figure uses firm–HS8 relationships established before December 2005. September 2006 is omitted, October is retained as a transition month, and bars show 95-percent confidence intervals from firm-clustered standard errors.

Table C.2: Pre-Treatment Linear-Trend Diagnostics

Outcome	(1) Broad pre-period Trade Value	(2) Pre-July Trade Value	(3) Short window Trade Value
Overall slope	-0.002 (0.002)	-0.005 (0.004)	-0.000 (0.004)
Private slope	-0.002 (0.005)	-0.005 (0.008)	-0.003 (0.008)
SOE slope	-0.001 (0.002)	-0.004 (0.003)	0.001 (0.003)
NK × SOE × Month slope	0.001 (0.005)	0.001 (0.009)	0.003 (0.009)
Pre-period	Dec. 2005–Sep. 2006	Dec. 2005–June 2006	May–Sep. 2006
Sample	Incumbent	Incumbent	Incumbent
Estimator	OLS	OLS	OLS
Destination-class FE	Yes	Yes	Yes
HS8-product FE	Yes	Yes	Yes
Firm-by-month FE	Yes	Yes	Yes
Firm clustering	Yes	Yes	Yes
Overall observations	1,285,900	900,130	642,950
Ownership observations	1,234,260	863,982	617,130

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and total non-NK exports on the comparison row. The sample contains firm–HS8 relationships established before December 2005. Within each pre-period, Month is a linear index normalized to zero in the final month. The overall slope is the $\text{NK} \times \text{Month}$ coefficient estimated using all firms in the incumbent sample. The private and SOE slopes and their difference come from the ownership model. The last coefficient row reports that SOE-minus-private slope. All models absorb destination class, HS8 product, and firm-by-month fixed effects and cluster standard errors by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

position, and economic importance. Across these alternative proxies, the estimated ownership-style interaction remains positive across specifications. We also subject each measure to additional specification checks to assess whether the results are sensitive to alternative samples, controls, or treatment definitions. The results are largely consistent with our main findings.

Table C.3: Alternative-Measure Robustness

	Beijing	Four municipalities	Large firm
Mean-log DDD	0.356*** (0.079)	0.349*** (0.068)	0.202** (0.064)
Sum-log DDD	0.367*** (0.083)	0.368*** (0.071)	0.220*** (0.065)
Sum-asinh DDD	0.399*** (0.089)	0.400*** (0.077)	0.238*** (0.071)
Private/SOE-sample DDD	0.386*** (0.083)	0.362*** (0.070)	0.283*** (0.064)
Ownership-conditioned DDD	0.218* (0.085)	0.207** (0.074)	0.197** (0.065)
Continuous log-scale DDD			0.106*** (0.016)
Incumbent May–August lead-test p	0.949	0.831	0.289
Destination-class FE	Yes	Yes	Yes
HS8-product FE	Yes	Yes	Yes
Firm-by-month FE	Yes	Yes	Yes
Timeframe	May 06–Apr. 07	May 06–Apr. 07	May 06–Apr. 07
Full-sample observations	2,305,760	2,305,760	2,305,760
Ownership-sample observations	2,173,720	2,173,720	2,173,720
Incumbent observations	1,543,080	1,543,080	1,543,080

Notes: Each coefficient is the Post $\times$ NK $\times$ moderator interaction. Mean-log uses $\ln(1 + y)$ with the mean across other observed destinations on the comparison row. Sum-log and sum-asinh replace that mean with total non-NK exports. Private/SOE-sample estimates restrict mean-log regressions to firms with a frozen private or SOE classification. Ownership-conditioned estimates additionally include NK $\times$ SOE and Post $\times$ NK $\times$ SOE. The continuous log-scale row replaces the large-firm indicator with mean-centered log pre-shock exports. Lead-test p -values jointly test the May–August moderator-difference coefficients among firm–HS8 relationships established before December 2005, relative to September 2006. All models use May 2006–April 2007, absorb destination class, HS8 product, and firm-by-month fixed effects, and cluster standard errors by firm. Standard errors are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

C.3 Second Nuclear Test

Table C.4 repeats the baseline difference-in-differences and triple differences specifications around North Korea’s May 2009 nuclear test. The estimation window runs from December 2008 through November 2009, with May retained as the transition month and Post equal to one from June onward. Columns 1–3 reproduce the baseline DD fixed-effect progression, while column 4 introduces the ownership interaction using destination-class, HS8-product, and firm-by-month fixed effects. Standard errors are clustered by firm.

On the transformed outcome scale, the DD estimates for all firms imply an approximately 2.4 percent decline in exports to North Korea relative to comparison destinations. In column 4, the corresponding private-firm decline is approximately 1.4 percent, while the SOE–private contrast is approximately 1.0 percent. These effects are considerably smaller than the corresponding 2006 estimates of approximately 10.2 percent, 28.5 percent, and 42.5 percent, calculated using the same exponential conversion. The second nuclear test therefore produces the same directional pattern of greater withdrawal among private firms, but with a substantially attenuated ownership

divergence.

Table C.4: Second Nuclear Test: Baseline DD and DDD Replication

Specification	(1)	(2)	(3)	(4)
Outcome	DD	DD	DD	DDD
	Trade Value			
Post × NK × SOE				0.00987** (0.00368)
Post × NK	-0.0247*** (0.00227)	-0.0247*** (0.00227)	-0.0247*** (0.00227)	-0.0140*** (0.00284)
NK × SOE				-0.0773*** (0.0116)
Destination-class FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Absorbed	Absorbed
Firm FE	No	Yes	Absorbed	Absorbed
Firm-by-month FE	No	No	Yes	Yes
HS8-product FE	No	No	Yes	Yes
Timeframe	December 2008–November 2009			
Observations	4,622,032	4,622,032	4,622,032	3,180,554

Notes: The outcome is $\ln(1 + y)$, using North Korea export value on the NK row and mean exports to other observed destinations on the comparison row. All columns use December 2008–November 2009. May is the transition month, and Post equals one from June 2009 onward. Columns 1–3 estimate DD effects for all firms with progressively richer fixed effects. Column 4 estimates the DDD among firms with a frozen private or SOE classification. Ownership is measured using the same frozen classification as in the baseline. Standard errors clustered by firm are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

D The Iran Comparison and Dalai Lama Receptions

This appendix documents the comparative cases used in Section 6. Table D.1 contrasts two settings. Iran is similar to North Korea: China participated in multilateral sanctions but continued to permit commerce outside the formal restrictions. Official Dalai Lama receptions provide the opposite setting: Beijing itself initiated economic pressure and sought broader commercial retaliation. The first empirical subsection reports the firm–HS8 export estimates for Iran. The second describes the staggered reception design and reports the full-post estimates for 23 treated countries across 17 cohorts.

D.1 Case Context and Empirical Coverage

Table D.1 summarizes the cases analyzed in the paper. Panel A covers the North Korea setting and its closest collective-sanctions comparison, Iran. Panel B covers the official Dalai Lama receptions used in the comparison relevant to H3 in Section 6. The contrast is between participation in targeted UN sanctions and pressure initiated by Beijing over a sovereignty-related priority. Appendix Table D.3 lists every treated country, its exact reception date, and the official recorded in the staggered treatment chronology.

Table D.1: Analyzed Cases: Context and Empirical Coverage

Case	Trigger and response	Empirical coverage
<i>Panel A. Collective UN sanctions supported by China</i>		
North Korea, 2006	A nuclear test on October 9 was followed by the adoption of Resolution 1718 on October 14 with Chinese support. Specified weapons-related transfers and luxury goods were prohibited. China publicly distinguished normal trade from restricted activities (Ministry of Foreign Affairs of the People's Republic of China, 2006b; United Nations Security Council, 2006a).	Main firm–HS8 export design. Short window: May 2006–April 2007, with October as the transition month and November as the post start. Broader window: December 2005–October 2007.
Iran, 2006	Failure to suspend proliferation-sensitive nuclear activities led to the adoption of Resolution 1737 on December 23 with Chinese support. The resolution imposed nuclear- and missile-related controls and designated asset freezes (United Nations Security Council, 2006b).	Firm–HS8 exports, July 2006–June 2007, with December as the transition month and January as the post-period start. The comparison uses existing exporters. Estimates appear in Table D.2.
<i>Panel B. Dalai Lama receptions and China-initiated pressure</i>		
Official Dalai Lama receptions, 2001–2006	Meetings with serving government members provide repeated occasions for Chinese pressure over sovereignty and territorial integrity (Fuchs & Klann, 2013). Treatment dates the first recorded in-sample reception using the official dignitary chronology. ⁵	All-product country–month imports: 200 countries, 23 treated, and 17 cohorts. Recorded treatment events run from May 2001 to June 2006. Full-post estimates include all 23 treated countries. The event-study figure uses 20 countries observed through month 11.

Measurement notes. Panel A uses firm–HS8 export specifications with firm-clustered inference. Panel B uses country-clustered staggered difference-in-differences estimators. Outcomes are total import value and SOE-minus-private differences in log-plus-one import value and importer counts. Table D.4 reports the full-post estimates. Appendix D.3 provides the reception cohorts, event-study figure, and sensitivity discussion.

D.2 Most Similar Case: UN Sanctions on Iran

Resolution 1737 imposed binding nuclear- and missile-related controls on Iran in December 2006 (United Nations Security Council, 2006b). In descriptive totals for firm–HS8 relationships with exports to Iran during July–December 2006, export value fell 27.5 percent in January–June 2007: private-firm exports fell 33.7 percent, compared with 21.7 percent for SOEs. We apply the same firm–HS8 triple-difference design used for North Korea, comparing exports to Iran with exports to other destinations and examining whether the post-resolution adjustment differs by ownership.

Table D.2 reports the results. In the preferred specification, private-firm exports to Iran decline by approximately 17.9 percent relative to comparison destinations on the transformed outcome scale. The SOE–private contrast is approximately 40.7 percent on that scale, indicating a more favorable relative adjustment for SOEs. The pattern closely mirrors the North Korea results. Politically better-positioned firms withdraw less from a setting in which China participated in targeted multilateral sanctions while continuing to permit commerce outside the formal restrictions. The Iran comparison therefore provides a second setting consistent with the

⁵Office of His Holiness the Dalai Lama, “Dignitaries Met 2000–2004” and “Dignitaries Met 2005–2010”, both accessed September 10, 2026.

implications of our theory.

Table D.2: Iran: Ownership Differences under UN Sanctions

Outcome:	(1) Trade Value	(2) Trade Value	(3) Trade Value	(4) Trade Value
Post $\times$ Iran $\times$ SOE	0.342*** (0.0971)	0.342*** (0.0971)	0.342*** (0.0973)	0.342*** (0.0973)
Post $\times$ Iran	-0.197*** (0.0407)	-0.197*** (0.0407)	-0.197*** (0.0408)	-0.197*** (0.0408)
Post $\times$ SOE	-0.499*** (0.1001)	-0.499*** (0.1001)	-0.499*** (0.1004)	
Iran $\times$ SOE	-0.604*** (0.1399)	-0.604*** (0.1399)	-0.604*** (0.1404)	-0.604*** (0.1404)
SOE	0.825*** (0.1487)			
Destination-class FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Absorbed
Firm FE	No	Yes	Yes	Absorbed
HS8-product FE	No	No	Yes	Yes
Firm-by-month FE	No	No	No	Yes
Timeframe	Jul. 06–Jun. 07	Jul. 06–Jun. 07	Jul. 06–Jun. 07	Jul. 06–Jun. 07
Ownership lead-test p -value	0.004	0.004	0.004	0.004
Observations	506,112	506,112	506,112	506,112
Firms	6,640	6,640	6,640	6,640

Notes: The outcome is $\ln(1 + y)$, using Iran export value on the Iran row and the mean across comparison destinations on the comparison row. All columns use existing exporters: firm–HS8 relationships with positive Iran exports during July–December 2006, followed throughout July 2006–June 2007 with zero-trade months included. *Post* equals one from January 2007 onward, and *Iran* identifies the Iran row. Ownership is frozen in December 2006. Blank coefficient cells denote terms absorbed by fixed effects. Standard errors are in parentheses and clustered by firm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

D.3 Most Different Case: Dalai Lama Receptions

Official receptions of the Dalai Lama provide a contrasting setting in which China itself initiated economic pressure over an issue it regarded as involving sovereignty and territorial integrity (Fuchs & Klann, 2013). This case is useful for examining H3 because stronger political standing should be associated with greater withdrawal when officials intend commercial retaliation. The analysis does not directly observe firms' information, however, and political obligations provide an alternative explanation for greater SOE responsiveness. The reception chronology draws on the Dalai Lama's official dignitary lists.⁶ Treatment begins at each country's first recorded reception by serving government officials during 2000–2006. We exclude countries first treated in 2000 and India, which hosts the Dalai Lama in exile. Table D.3 reports the 23 treated countries across 17 monthly cohorts, with reception dates ranging from May 2001 through June 2006.

⁶Office of His Holiness the Dalai Lama, "Dignitaries Met 2000–2004" and "Dignitaries Met 2005–2010", both accessed September 10, 2026.

Table D.3: Official Reception Dates Used to Assign Treatment

Country	Reception date	Official recorded in the treatment chronology
Switzerland	2001-05-06	Ruth Dreifuss (Minister of Interior)
Estonia	2001-06-19	Mart Laar (Prime Minister)
Latvia	2001-06-22	Vaira Vike-Freiberga (President)
Lithuania	2001-06-24	Valdas Adamkus (President)
Bulgaria	2001-10-24	Simeon Saxe-Coburg-Gothe (Prime Minister)
Portugal	2001-11-28	Jorge Sampaio (President)
Italy	2001-11-30	Giovanni Alemanni (Minister of Agriculture and Forestry)
New Zealand	2002-05-28	Jim Anderton (Acting Prime Minister)
Slovenia	2002-07-04	Janez Drnovsek (Prime Minister)
Croatia	2002-07-08	Ivica Racan (Prime Minister)
Austria	2002-10-13	Benita Ferrero-Waldner (Foreign Minister)
Mongolia	2002-11-07	Nambaryn Enkhbayar (Prime Minister)
Peru	2003-10-12	Alejandro Toledo (President)
Canada	2004-04-23	Paul Martin (Prime Minister)
United Kingdom	2004-05-27	Jack Straw (Foreign Secretary)
Costa Rica	2004-09-27	Abel Pacheco (President)
El Salvador	2004-09-29	Antonio Saca (President)
Guatemala	2004-10-01	Oscar Berger (President)
Mexico	2004-10-05	Santiago Creel (Secretary of the Interior)
Jordan	2005-05-18	King Abdullah II (King)
Brazil †	2006-04-26	Gilberto Gil (Minister of Culture)
Chile †	2006-05-05	Paulina Urrutia (Minister of Culture)
Belgium †	2006-06-01	Guy Verhofstadt (Prime Minister)

Notes: Each row identifies the reception recorded for a treated country in the estimation chronology. Treatment begins in its calendar month. Dates identify official meetings, not the full duration of a visit. The 23 countries form 17 monthly cohorts. † marks Belgium, Brazil, and Chile, which enter the full-post estimates but lack complete follow-up through month 11. The other 20 countries also enter the event-study figure. India and countries first recorded receiving in 2000 are excluded. The panel includes 177 countries with no recorded reception in the estimation chronology. Subsequent receptions do not reset treatment. Sources: Office of His Holiness the Dalai Lama, “Dignitaries Met 2000–2004” and “Dignitaries Met 2005–2010”.

The import panel covers 200 countries over 84 months. We aggregate customs records to the country–ownership–month level and examine log-plus-one import value and importer participation. We estimate staggered treatment effects using the Callaway–Sant’Anna estimator, with never- and not-yet-treated countries as controls, and the Sun–Abraham estimator, using never-treated countries as the comparison group (Callaway & Sant’Anna, 2021; Sun & Abraham, 2021).

Table D.4: Staggered Effects of Official Dalai Lama Receptions: Full Post-Reception Average

Outcome	C–S Never/not-yet	C–S Never only	Sun–Abraham Never only
Total import value	-0.426 ⁺ (0.221)	-0.437* (0.219)	-0.437** (0.150)
Import value: SOE minus private	-0.699 (0.804)	-0.729 (0.845)	-0.729** (0.223)
Importer count: SOE minus private	-0.457*** (0.138)	-0.479*** (0.134)	-0.479*** (0.067)

Notes: C–S denotes Callaway–Sant’Anna. The country panel, treatment dates, and outcomes are identical across columns. The full-post average weights available cohort–time effects by cohort size, and all 23 treated countries enter. Earlier cohorts contribute more post-reception observations. The panel contains 200 countries over 84 months. Standard errors are in parentheses. C–S uses 999 country-clustered multiplier draws, and Sun–Abraham uses country score-sum covariance with a $G/(G - 1)$ correction and fixed weights. The latter has country/month effects and cohort-by-event-time interactions, with month –1 omitted. The full panel has 12 single-country cohorts out of 17. Ownership is measured at the transaction date. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Markers use nominal two-sided normal-reference tests for C–S and t_{G-1} tests for Sun–Abraham, consistent with their reported intervals.

Table D.4 reports the full-post estimates. Across both estimators, SOE importer participation declines relative to private-firm participation after an official reception. The estimates imply a decline of approximately 36.7–38.1 percent in SOE importer participation relative to private-firm participation on the transformed count scale. The participation result remains similar when allowing one month of anticipation or restricting the comparison group to never-treated countries. This pattern is the opposite of the North Korea and Iran results and is consistent with H3. When officials clearly intend broader commercial withdrawal, stronger political standing can increase rather than reduce withdrawal by providing more credible information about the expected response. The evidence does not directly identify differential access to information, however, and political obligations remain an alternative explanation.

Figure D.1 presents the Callaway–Sant’Anna event-time estimates for the SOE–private importer participation gap around official Dalai Lama receptions. The figure uses the 20 treated countries observed through month 11 and shows how the ownership gap evolves around the first recorded reception.

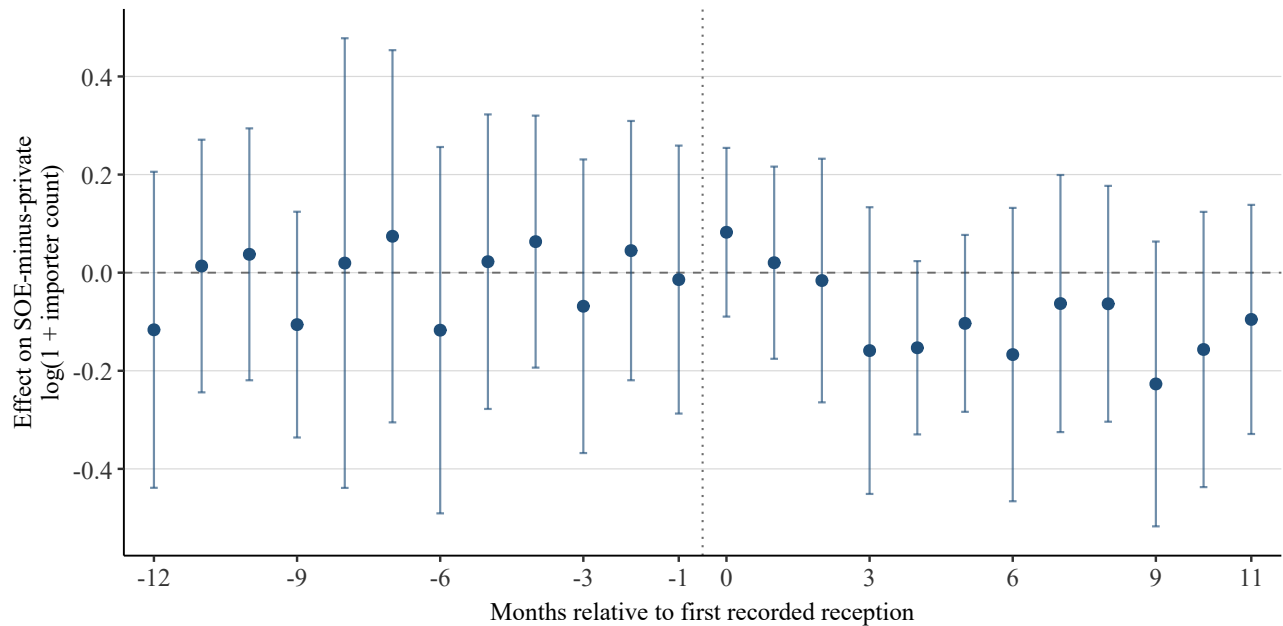


Figure D.1: Official Dalai Lama Receptions and the SOE–Private Importer Participation Gap

Notes: Callaway–Sant’Anna dynamic estimates for the SOE-minus-private difference in log-plus-one importer counts, using the fixed set of 20 treated countries observed through month 11 and never- and not-yet-treated controls. The 95 percent bands are simultaneous across the displayed event path, based on 999 country-clustered multiplier draws. Pre-reception points, including month –1, are varying-base placebo changes. Treatment begins at the first recorded in-sample reception and remains one thereafter. The outcome matches the importer-count row of Table D.4, whose full-post estimates use all 23 treated countries and all available post-reception months.

The two estimators parameterize pre-treatment dynamics differently. Under Callaway–Sant’Anna, the pre-treatment estimates are varying-base placebo changes between successive periods, whereas Sun–Abraham reports each event-time coefficient relative to month –1. The Callaway–Sant’Anna specifications do not produce an omnibus pre-trend test because the estimated covariance matrix is singular. We therefore assess pre-treatment dynamics using the individual event-time estimates reported in Figure D.1 and the replication files.

E Political Standing and Resolve Reading: A Formal Model

E.1 Setup and Equilibrium

The game formalizes the resolve-reading argument in Section 2. One firm decides whether to withdraw from a commercial relationship when the policymaker's willingness to punish continuation is uncertain. Established product prohibitions remain binding, and the decision concerns additional withdrawal. The model is deliberately simple. It isolates how unequal access to credible information about enforcement resolve shapes firms' withdrawal decisions under a common public signal. The signaling structure shows how public ambiguity can arise in equilibrium, while the propositions characterize firm behavior given that ambiguity. We take the availability and credibility of private guidance as given and do not model officials' decisions over whom to inform or strategic interactions among firms.

Players and information. There are two players: a policymaker and a firm. The policymaker's privately known type is $t \in \{S, W\}$. A strong type, S , enforces broader withdrawal, while a weak type, W , permits continuation. The firm initially assigns probability $p \in (0, 1)$ to the strong type. For example, $p = 0.6$ means that before receiving further information, the firm assigns a 60% chance to punishment for continued trade.

The firm's political standing is $j \in \{h, l\}$, where h means higher standing and l lower standing. We compare the same firm at these two standing levels in separate games. Its withdrawal cost $c > 0$ is fixed and known to both players. This cost can include forgone profits and fees for terminating a trading contract.

Political contacts, such as a private clarification from a ministry official, reveal the policymaker's type with probability λ_j , with $0 < \lambda_l < \lambda_h < 1$. Ordinary learning captures what firms can learn about enforcement intentions from publicly available information and accumulated experience, such as observing how earlier sanctions were implemented. These sources reveal the policymaker's type with probability $\ell \in [0, 1]$. A higher ℓ means that the firm is more likely to learn actual resolve without privileged political access, even if the government's current public announcement remains ambiguous. If ordinary learning fails, political contacts remain available. The probability that the firm remains uninformed is

$$u_j = (1 - \ell)(1 - \lambda_j). \quad (2)$$

Guidance is truthful and private, and its availability is independent of the policymaker's type and public signal. The baseline is $\ell = 0$. Comparing standing levels holds the prior, commercial cost, and policy preferences fixed.

Actions and payoffs. The policymaker chooses a public signal $s \in \{0, 1\}$. The routine announcement, $s = 0$, is costless. The alternative, $s = 1$, costs $K > 0$: greater administrative specificity or public commitments can constrain the government's diplomatic flexibility. For example, explicitly committing to a trade cutoff can make later diplomatic concessions more costly. Either type can choose either signal.

The firm chooses withdrawal, D , or continuation, C . Withdrawal costs c . After continuation, the policymaker chooses punishment, P , or tolerance, T . Punishment costs the firm $F_j > 0$. For example, continued trade might lead to license restrictions or exclusion from government contracts. Protection means $F_h \leq F_l$. Equal

protection means $F_h = F_l = F_0$. This allows standing to affect punishment losses as well as information access (Naoi et al., 2022).

The policymaker values obtaining or enforcing withdrawal at r_t and pays $\kappa > 0$ to punish. This enforcement cost can include administrative resources spent investigating and penalizing continued trade. Assume $r_W < 0 < \kappa < r_S$. Payoffs are:

Outcome	Firm	Policymaker
Withdrawal D	$-c$	$r_t - Ks$
Continuation, punishment (C, P)	$-F_j$	$r_t - \kappa - Ks$
Continuation, tolerance (C, T)	0	$-Ks$

Sequence of play. Nature selects the policymaker's type. The policymaker chooses its public signal. Nature then determines whether the firm learns the type, and the firm withdraws or continues. Withdrawal ends the game. Continuation leads to punishment or tolerance. The policymaker observes the firm's action but not whether it received information.

Optimal decisions. After continuation, the policymaker's gain from punishment is $r_t - \kappa$. Thus S punishes and W tolerates, regardless of what the firm knows. If the firm assigns probability μ to S , it withdraws when

$$c < \mu F_j, \quad (3)$$

and continues at equality. An informed firm has $\mu = 1$ under S and $\mu = 0$ under W . An uninformed firm updates from the public signal.

A pooling equilibrium. Suppose both types choose the routine announcement. The uninformed firm's belief remains p , so public communication leaves enforcement resolve uncertain. A simple sufficient condition for this equilibrium is

$$K \geq \max\{\kappa, -r_W\}. \quad (4)$$

Changing the signal can benefit S by at most κ , by avoiding punishment costs, and W by at most $-r_W$, by preventing unwanted withdrawal. The signal cost therefore rules out a profitable change for either type, whatever belief the firm assigns to the unused signal.

Together with the optimal firm and enforcement decisions, these strategies form a perfect Bayesian equilibrium. Beliefs after continuation follow Bayes' rule whenever continuation is possible. Where it is impossible, any belief about the firm's information supports the same optimal enforcement choice. Because the condition in Equation 4 does not depend on political standing or ordinary learning, it is sufficient for pooling for both higher-standing (h) and lower-standing (l) firms at every level of ordinary learning.

E.2 Results and Proofs

In the pooling equilibrium, the firm's withdrawal probabilities follow from three cost ranges. Let O_j denote withdrawal under weak resolve and $D_j(S)$ withdrawal under strong resolve. The three propositions below correspond to H1–H3 in Section 2.5.

Withdrawal cost	Weak resolve: O_j	Strong resolve: $D_j(S)$
$c < pF_j$	u_j	1
$pF_j \leq c < F_j$	0	$1 - u_j$
$c \geq F_j$	0	0

Each entry gives the firm's withdrawal probability conditional on the policymaker's actual resolve. The probability $u_j = (1-\ell)(1-\lambda_j)$ captures the chance that neither ordinary learning nor political contacts reveal the policymaker's type. Because both types send the same public signal in the pooling equilibrium, an uninformed firm still assigns probability p to strong resolve, even when actual resolve is weak. It therefore compares the withdrawal cost, c , with the expected loss from continuing, pF_j . An informed firm knows whether continuation will be tolerated or punished and compares c with zero or F_j , respectively. As specified above, the firm continues when the costs are equal.

When $c < pF_j$, an uninformed firm finds withdrawal less costly than continuing. Under weak resolve, the firm therefore withdraws unless it learns that continuation is permitted, giving $O_j = u_j$. Under strong resolve, it withdraws whether informed or uninformed, giving $D_j(S) = 1$. Information matters here by providing reassurance that prevents unnecessary withdrawal.

When $pF_j \leq c < F_j$, an uninformed firm continues because its expected punishment loss does not exceed the cost of withdrawal. Learning weak resolve confirms this decision, so $O_j = 0$. Learning strong resolve instead makes withdrawal worthwhile: certain punishment costs more than withdrawal. Thus $D_j(S) = 1 - u_j$, the probability that the firm becomes informed. Information matters here by providing a warning that induces withdrawal.

When $c \geq F_j$, withdrawal costs at least as much as certain punishment. The firm therefore continues regardless of what it learns, and both withdrawal probabilities are zero.

Proposition 1 (Political standing and overcompliance, H1). *At equal protection, higher standing weakly reduces withdrawal under weak resolve: $O_h \leq O_l$. The difference is strict when $\ell < 1$ and $c < pF_0$. The gap $\Delta = O_l - O_h$ is*

$$\Delta = \begin{cases} (1-\ell)(\lambda_h - \lambda_l), & c < pF_0, \\ 0, & c \geq pF_0. \end{cases} \quad (5)$$

Proof. If $c < pF_0$, the firm withdraws only when uninformed, so $O_h = u_h$ and $O_l = u_l$. Subtracting gives the stated gap, which is positive when $\ell < 1$ because $\lambda_h > \lambda_l$. If $c \geq pF_0$, the firm continues under weak resolve at either standing level, and both withdrawal probabilities are zero.

Proposition 2 (Withdrawal beyond formal prohibitions, H2). *Under weak resolve and equal protection, the standing gap extends to a permissible product with withdrawal cost c_r when $c_r < pF_0$ and $\ell < 1$. A higher perceived probability p of broad enforcement weakly increases this gap, while a higher cost c_r weakly reduces it. The change is strict when it changes whether $c_r < pF_0$, provided $\ell < 1$.*

Proof. Apply Equation 5 with the product's cost c_r in place of c . The gap is positive when $c_r < pF_0$ and $\ell < 1$. Increasing p raises the cutoff, while increasing c_r makes it harder to satisfy. Within either cost range, the gap stays unchanged.

The argument also applies to humanitarian goods whose trade is permitted: uncertainty about broader enforcement can induce withdrawal even when the government would tolerate continuation. With equal protection, incomplete learning, and $c_r < pF_0$, lower-standing firms withdraw more often because they are less likely to learn that continuation is permitted. This difference can also be averaged across separate product cases, using the same fixed, nonnegative product weights for higher- and lower-standing firms. Since the gap is nonnegative for every product, the average is positive whenever at least one product with positive weight has a positive gap. These results are conditional on weak actual resolve, when withdrawal is unnecessary. Before the policymaker's type is known, that state occurs with probability $1 - p$. Averaging across both types therefore multiplies the weak-resolve withdrawal probabilities and their standing gap by $1 - p$, because withdrawal under strong resolve does not constitute overcompliance. The unconditional gap need not increase with p , since a higher p also makes weak resolve less likely.

Proposition 3 (Compliance under strong enforcement resolve, H3). *At equal protection, higher standing weakly increases withdrawal under strong resolve: $D_h(S) \geq D_l(S)$. The difference is strict when $\ell < 1$ and $pF_0 \leq c < F_0$.*

Proof. In the middle cost range, the firm withdraws only after learning that punishment is certain. Its withdrawal probability is $1 - u_j$. Higher standing lowers u_j , so it raises withdrawal, strictly when $\ell < 1$. In the other two cost ranges, the firm either always withdraws or always continues, regardless of information access.

The strict effects in H1 and H3 arise in different cost ranges. When $c < pF_0$, an uninformed firm withdraws, so learning weak resolve provides reassurance that prevents unnecessary withdrawal. When $pF_0 \leq c < F_0$, an uninformed firm continues, so learning strong resolve provides a warning that induces withdrawal. These effects require incomplete learning and cannot both be strict for the same activity when the prior, withdrawal cost, and punishment loss are fixed. The withdrawal probabilities in the table above show this distinction. Within each proposition, the comparison between higher- and lower-standing firms holds the prior, withdrawal cost, protection, ordinary learning, and public communication fixed.

Political protection. At fixed information access, greater protection lowers F_j and therefore weakly reduces withdrawal in both states. In the weak-state column, it can move the firm from withdrawal with probability u_j to continuation. In the strong-state column, withdrawal can move from 1 to $1 - u_j$ and then to zero as F_j falls. Thus protection reinforces H1. H3 isolates information by holding protection equal.

Ordinary learning. Holding the prior, withdrawal cost, protection, and political access fixed,

$$O_j(\ell) = (1 - \ell)O_j(0), \quad \Delta(\ell) = (1 - \ell)\Delta(0). \quad (6)$$

This follows directly from the weak-state column: withdrawal is either zero or $u_j = (1 - \ell)(1 - \lambda_j)$. Learning leaves the cost range unchanged and scales each withdrawal probability, and hence their difference, by $1 - \ell$. This result allows protection to differ. Complete learning eliminates weak-state overcompliance, and the larger initial withdrawal probability falls more in absolute terms. This supplies the learning prediction for the comparison of North Korea's 2006 and 2009 episodes.

Public revelation. If public communication reveals the type, the firm withdraws under S exactly when $c < F_j$ and always continues under W . Information access then has no further effect. For comparison with pooling, suppose $c < F_h$ and the alternative signal is inexpensive: $0 < K \leq \kappa u_h$. A revealing equilibrium has S choose $s = 1$ and W choose $s = 0$. Imitating W saves S the signal cost but requires punishment whenever the firm remains uninformed, at expected cost $\kappa u_j \geq K$. Imitating S costs W the signal cost and induces unwanted withdrawal. Neither type gains by imitation.

A single-firm illustration. Let $c = 1$, $F_h = F_l = 3$, $p = 0.6$, $\lambda_h = 0.6$, and $\lambda_l = 0.2$. Set $r_S = 3$, $r_W = -0.5$, $\kappa = 2$, and $K = 2$, satisfying the pooling condition. Since $c < pF_j = 1.8$, the firm withdraws when uninformed and continues upon learning weak resolve.

All three rows below describe weak actual resolve: officials would permit continued trade. The parameter ℓ captures how informative publicly available information and accumulated experience are about the government's enforcement intentions. Formally, it is the probability that these ordinary sources reveal actual resolve to the firm. For example, following North Korea's second nuclear test in 2009, firms could draw on China's implementation of sanctions after the first test in 2006. To the extent that this experience remained informative about current policy, firms should have been better able to infer China's enforcement resolve without political contacts. The model represents this improvement as a higher ℓ . The subscript l instead labels lower political standing, while λ_j is the probability of learning through political contacts if ordinary learning fails.

Weak-state withdrawal probability	Higher standing	Lower standing	Gap
No ordinary learning, $\ell = 0$	40%	80%	40 pp
Partial ordinary learning, $\ell = 1/2$	20%	40%	20 pp
Complete ordinary learning, $\ell = 1$	0%	0%	0 pp

The columns compare the same firm at alternative levels of political standing; the rows vary only the availability of ordinary information. The gap is the lower-standing withdrawal probability minus the higher-standing probability, measured in percentage points (pp).

In the first row, $\ell = 0$, ordinary sources provide no clarification. This could represent an unfamiliar sanctions episode in which past experience offers little guidance. Only political contacts can reveal that trade is permitted. Those contacts succeed with probability 0.6 at higher standing and 0.2 at lower standing, leaving withdrawal probabilities of 40% and 80%, respectively.

In the second row, $\ell = 1/2$, experience or observed implementation gives the firm a 50% chance of learning that trade is permitted. If ordinary learning fails, political contacts can still supply reassurance. Withdrawal occurs only when both channels fail: its probability is $0.5 \times (1 - 0.6) = 0.2$ at higher standing and $0.5 \times (1 - 0.2) = 0.4$ at lower standing. Ordinary learning therefore halves the initial gap.

In the third row, $\ell = 1$, ordinary sources always reveal that continuation is permitted. This is a limiting case in which accumulated experience or observable implementation fully resolves uncertainty. The firm continues at either standing level, so political contacts provide no further informational advantage and the withdrawal gap disappears.

Raising c to 2 instead puts the firm in the middle cost range. Weak-state withdrawal is then zero, while strong-state withdrawal probabilities are 0.6 at higher standing and 0.2 at lower standing when $\ell = 0$.

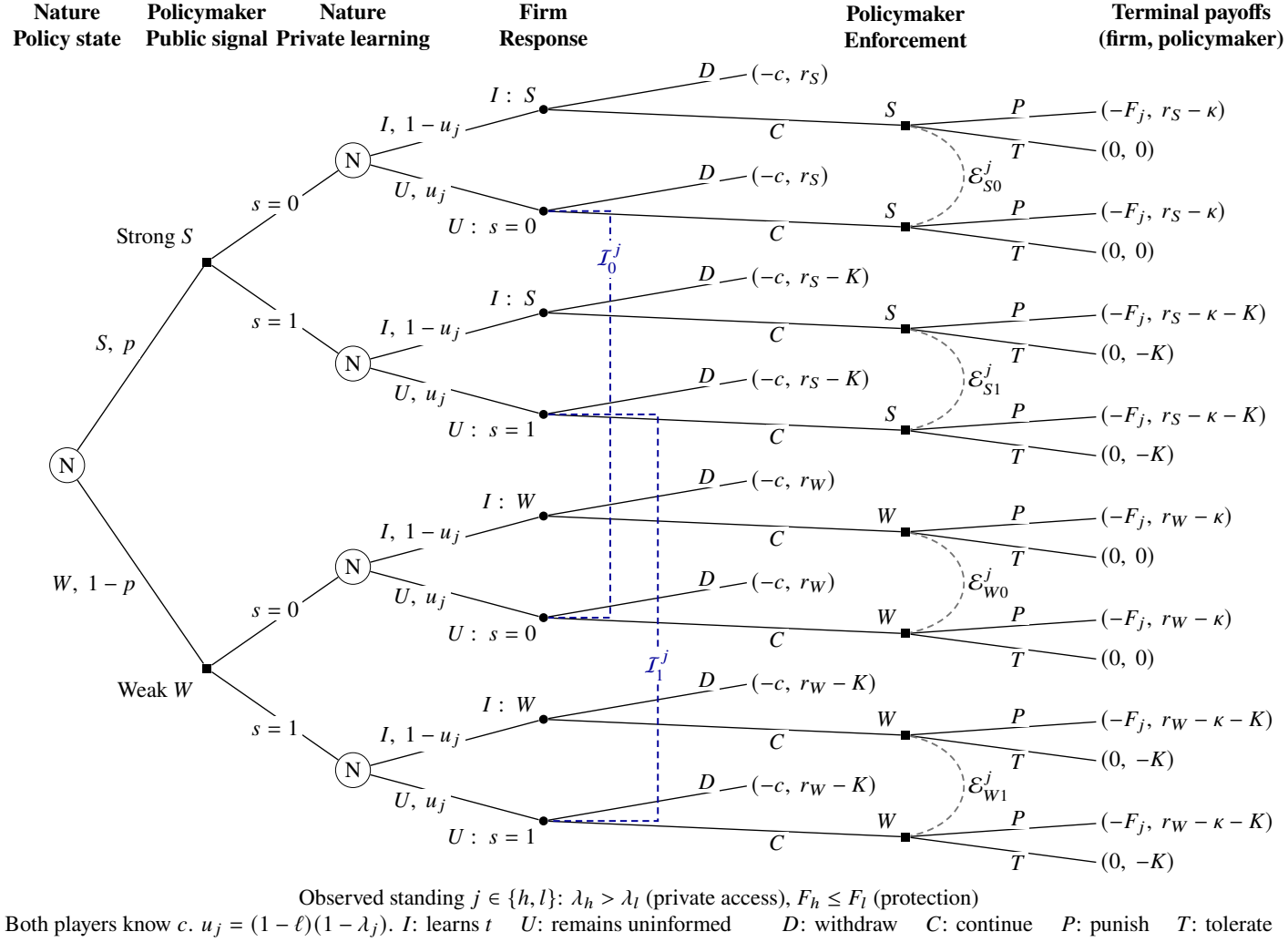


Figure E.1: Extensive Form: Political Standing, Learning, and Protection

Notes: The game contains one policymaker and one firm. Standing j and withdrawal cost c are fixed and known to both players. Learning reveals t with probability $1 - u_j$. Blue sets $\mathcal{I}_s^j$ connect the uninformed firm's nodes across policy states at a given signal. Gray sets $\mathcal{E}_{ts}^j$ connect enforcement nodes across unobserved learning outcomes. All choices are retained, including deviations, giving 24 terminal histories. Terminal pairs are (firm, policymaker).