Navigating Targeted Sanctions: A Real Options Theory of American Firm Responses and Financial Performance

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Abstract

Do targeted sanctions hurt American firms financially? Existing research offers competing arguments but little firm-level evidence on financial fundamentals. We shift the debate from aggregate sender costs to response-conditional performance. Our real options theory shows that partial, evolving sanctions present firms with binary choices depending on their starting position. Prior suppliers stay or exit; prior non-suppliers enter or stay out. Firms select among these options based on their resources and each option's expected value, so response-group performance reflects selection into each path. We test the theory using U.S. Entity List restrictions on Huawei. We examine 545 supplier and peer firms during 2015–2023 with probit and doubly robust difference-in-differences models. Outside options predict exit; financial flexibility predicts entry. Entrants achieve a persistent ROA advantage of 4–5 percentage points. Exiters and stayers show no statistically significant peer-relative underperformance. Treating exposed firms as a single group, therefore, obscures response-conditional financial outcomes of targeted sanctions.

Keywords: economic sanctions, Entity List, US suppliers, firm response, financial performance, supply chain, doubly robust difference-in-differences

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Introduction

Do US sanctions hurt American firms financially? The answer matters because theories of sanctions effectiveness and design build on assumptions about sender-firm costs. Sanctions that impose costs on sender countries or their domestic interest groups are seen as ineffective and difficult to enforce (Morgan and Schwebach 1997; Kaempfer and Lowenberg 1992, 1999; Drezner 2000; Early 2009); sanctions cheap for the sender and costly for the target are expected to succeed (Eaton and Engers 1992, 1999); and costs self-imposed by the sender signal credible commitment that enables multilateral cooperation (Martin 1992). Yet whether these costs are high or low remains contested, and the firm responses that produce them have received almost no theoretical attention.

Existing theories offer competing expectations about the magnitude of sender-firm costs.[1] Some studies posit that sender firms suffer little due to size disparity between sender and target economies, substitution toward third markets, and policies shielding domestic interests. Others argue that disrupted relationships, costly adjustment to outside options, and target retaliation impose real losses. Empirical evidence is mixed, with aggregate trade and investment studies finding effects ranging from substantial to negligible. A few recent firm-level studies show negative stock-market reactions. Yet existing research leaves two gaps. Empirically, we lack systematic evidence on how

[1]For reviews, see Morgan, Syropoulos and Yotov (2023); Özdamar and Shahin (2021). For low-cost arguments, see, e.g., Kaempfer and Lowenberg (1992); Early (2011, 2015); Lektzian and Sprecher (2007); McLean and Whang (2014); Barry and Kleinberg (2015); Bapat and Kwon (2015); Weber and Stępień (2020). For high-cost arguments, see e.g., Hufbauer et al. (2009); Özdamar and Shahin (2021); Le et al. (2022); Allen (2021); Weber and Stępień (2020); Farmer (2000); Early and Cilizoglu (2020); Lee, Lektzian and Biglaiser (2023). For aggregate evidence, see Hufbauer et al. (1997); Yang et al. (2004); Biglaiser and Lektzian (2011); Early and Peterson (2022); Farmer (2000); Besedeš, Goldbach and Nitsch (2021); Le et al. (2022). For firm-level evidence, see Webb (2020); Allen (2021); Crippa, Kalyanpur and Newman (Forthcoming).

sanctions affect sender firms' realized financial fundamentals, such as profitability. Theoretically, we lack a unified logic of how sender firms choose among available responses once a sanction is in place, and how selection into those responses shapes the financial outcomes observed afterward.

The rise of global supply chains and the spread of targeted sanctions make this firm-level question especially pressing. Historically, sanctions sought to change a target's behavior through comprehensive trade embargoes or financial blockades that prohibited commercial activities (Peksen 2019).[2] Contemporary targeted sanctions operate differently. We use "targeted sanctions" broadly to include government measures that restrict or condition customary trade or financial relations with named foreign entities. Entity List export controls, which restrict trade with named foreign entities, are an instance, and are treated as targeted sanctions by both scholars and the US government.[3] They do not impose a comprehensive embargo but condition covered exports, reexports, and transfers on government authorization. Our argument concerns this class of partial restrictions, not comprehensive embargoes.

The restrictions at the center of this paper, on the supply chains of Chinese technology firms such as Huawei, require a license for covered exports, reexports, and transfers of goods, software,

[2]This follows the conventional definition of economic sanctions as "the deliberate, government-inspired withdrawal, or threat of withdrawal, of customary trade or financial relations"(Hufbauer, Schott and Elliott 1990).

[3]Scholars have increasingly recognized such export control measures as a part of economic sanctions and statecraft. See, e.g., Morgan, Bapat and Kobayashi (2014); Drezner (2024); Egger, Syropoulos and Yotov (2024); Allen (2021). The US government itself uses the language of sanctions for Entity List actions. For instance, a BIS final rule added a person to the Entity List (2014-18579) to "impose additional sanctions implementing US policy toward Russia" (https://www.federalregister.gov/documents/2014/08/06/2014-18579/russian-oil-industry-sanctions-and-addition-of-person-to-the-entity-list).

and technology to the designated firms.[4] Because these measures restrict rather than completely sever commerce, they leave profit-making opportunities for American firms under permitted and licensed transactions, making staying, exiting, entering, and staying out meaningful responses and opening a choice space that comprehensive sanctions would foreclose.

What determines how firms respond to such targeted sanctions, and what financial outcomes follow from their responses? Prior scholarship has theorized firms as political actors whose lobbying capacity shapes the regime at the design stage (Kaempfer and Lowenberg 1992; McLean and Whang 2014; Bapat and Kwon 2015; Zhao 2026), but not as strategic commercial actors who, once the regime is in place, respond according to the economic resources and opportunities they hold. How firms navigate this implementation stage determines where the costs of economic statecraft actually fall within the sender economy, and therefore what domestic political pressure the policy generates. To address this gap, we develop a real options theory of sender-firm responses and performance under targeted sanctions.

In our theory, a firm's response is a real-options decision. Under an uncertain, evolving sanction, costly-to-reverse moves such as exit or entry are exercised only when their expected value exceeds that of preserving the current position, a logic we develop in Section 2. Pre-existing resources shape which options firms can feasibly exercise, while the expected value of each option determines whether exercising it is worthwhile. The financial outcomes observed within each response group therefore reflect the realized value for firms selected into that response, not the effect of sanctions on a randomly chosen sender firm.

Our argument considers two separate decisions: whether prior suppliers exit or stay, and whether prior non-suppliers enter or stay out. For prior suppliers, the choice between staying and exiting depends on outside options. A firm with many alternative customers can redeploy capacity and

[4]Bureau of Industry and Security, Addition of Entities to the Entity List, 84 Fed. Reg. 22961 (May 21, 2019). https://www.govinfo.gov/content/pkg/FR-2019-05-21/pdf/2019-10616.pdf

exit profitably, whereas one with few alternatives preserves the relationship and supplies the target under the restrictions. The same outside-option logic also shapes expected performance. Exiters should not systematically underperform peers because they are selected from firms whose outside options make redeployment viable. Stayers, by contrast, lack strong outside options and therefore maintain the relationship, possibly scaling target-related activity down when restrictions tighten or demand falls, then resuming it as conditions ease. Because stayers have low exit values, staying is preferable to exiting for them, but it need not yield returns above those of comparable peers. The theory therefore makes no directional prediction about stayers' profitability relative to peers.

For prior non-suppliers, the choice over whether to enter or stay out depends on financial flexibility. Entry requires funding an up-front commitment and bearing continuing regulatory exposure, so firms with greater financial flexibility are more likely to clear the funding constraint and enter. But entrant performance follows from a second selection layer. Among firms able to fund entry, only those expecting the new relationship to generate sufficiently high surplus enter. Entrants should therefore outperform peers because they are positively selected on the expected value of entering, not because financial flexibility itself raises performance. These distinct selection processes mean that pooled analyses may average over different response groups and time periods rather than reveal a representative sender-firm cost.

To test our argument, we use US Entity List restrictions on Huawei, one of the most salient targeted-sanction episodes of the past decade. The case fits the theory's scope conditions: the restrictions conditioned rather than prohibited commerce, their stringency changed over time, and Huawei remained a commercially viable buyer. It also offers the firm-level variation needed for a sharp test, including prior suppliers and non-suppliers in the United States and substantial heterogeneity in outside options and financial flexibility. Methodologically, we depart from the recent firm-level sanctions literature by measuring sender-firm outcomes through realized financial fundamentals such as return on assets rather than stock returns. Stock returns capture investor-anticipated risk and embed sentiment and noise, while fundamentals capture realized firm performance after firms execute their responses. Response choice analyses use binary probit on cross-sectional sam-

ples of prior suppliers and non-suppliers separately, modeling each population's response choice as a function of its focal resource variable. Outcome analyses use panel data on 545 American supplier and peer firms over 2015-2023 and employ difference-in-differences and dynamic event-study designs to estimate peer-relative effects for the full treated population and separately for each response group.

The findings support our theoretical expectations on both firm response and financial performance. On firm response, among prior suppliers, the probability of exit relative to stay is significantly higher for firms with more pre-treatment outside options (measured by customer count in Huawei's industry). Other resources do not predict the choice. Among non-suppliers, the probability of entering Huawei's post-sanction supply chain is significantly higher for firms with greater financial flexibility (lower leverage), while other resources do not predict the choice. Lobbying expenditures do not predict response choice in either decision. On financial performance, we find no statistically significant pooled effect on the treated firm population, but the pooled estimate masks substantial response-conditional heterogeneity. We find no statistically significant peer-relative underperformance for exiters, consistent with selection on outside options that facilitates successful redeployment. We find no statistically significant difference in stayers' profitability relative to peers. Entrants outperform their peers from the first full year after entry onward, consistent with positive selection on the firm-specific value of entry among firms that can clear the funding constraint.

Our research makes three contributions. First, we develop a real-options theory of the effects of targeted sanctions on sender firms, recasting the low- versus high-cost debate as a question of which response a firm chooses and how selection shapes its performance. Where prior firm-level work treats firms as undifferentiated recipients of a policy shock, we treat them as strategic commercial actors whose pre-existing resources shape their response choices, while observed outcomes reflect the value realized by selected response groups. Second, we analyze realized financial fundamentals like return on assets rather than the stock-market reactions prior firm-level work relies on. Thus, we capture how firms actually perform once they execute their responses. Third, we reveal systematic, response-conditional heterogeneity that aggregate analyses average away, including entry as a

response category that prior work has neither theorized nor measured, a pattern our theory predicts. Beyond sanctions, our findings extend research on firm responses to political shocks. While that literature emphasizes lobbying (e.g., Zhang 2025) and network entrenchment (e.g., Vortherms and Zhang 2024), we show that firms also adapt through strategic commercial choices.

1 The Puzzle of Sanction Effects on Sender Firms

Sanctions scholars offer two competing expectations of sender-firm costs, which we refer to as the low-cost and high-cost positions. Scholars holding the low-cost position suggest three mechanisms. First, because most sanction sender economies (e.g., the US and its allies) are significantly larger than those of target countries, the size disparity reduces sender dependence on the target and softens any negative impact (Farmer 2000; Morgan, Syropoulos and Yotov 2023). Second, sender firms often trade with third-party states and use them as outside options to minimize potential damage (Early 2011, 2015; Weber and Stępień 2020). Sender firms also invest in third-party countries to continue doing business with the target indirectly (Barry and Kleinberg 2015). Third, sender governments design and implement sanctions in ways that mitigate adverse impact on their citizens and businesses (Kaempfer and Lowenberg 1992; Farmer 2000; Lektzian and Sprecher 2007; McLean and Whang 2014; Bapat and Kwon 2015; Zhao 2026). Under pressure from interest groups that benefit from business with the target, the sender government may refrain from imposing sanctions, avoid actions that could trigger retaliation, or choose measures less economically harmful to domestic interests (e.g., targeting products with accessible substitutes).

In contrast, scholars who take the high-cost position propose three mechanisms by which sanctions harm sender firms financially. First, sanctions disrupt established trade relationships, causing firms that rely on the target for supplies or customers to experience declines in revenue and market share, layoffs, and business closures (Farmer 2000; Hufbauer et al. 2009; Özdamar and Shahin 2021; Le et al. 2022; Allen 2021). Second, sender firms complying with sanctions must seek and adjust to alternative markets or suppliers, and those with limited outside options

incur substantial adjustment costs (Weber and Stępień 2020; Farmer 2000; Morgan, Syropoulos and Yotov 2023). Third, sanctions invite target retaliation, including asset freezes and expropriation, and growing market uncertainty, harming sender firms (Early and Cilizoglu 2020; Lee, Lektzian and Biglaiser 2023; Morgan, Syropoulos and Yotov 2023).

Most existing empirical research on sanction senders focuses on country- or dyad-level trade and investment outcomes and reports mixed findings, ranging from substantial to negligible. On the one hand, studies of major power sanctions, such as US sanctions on China and European sanctions on Russia, indicate that sanctions impose high costs on the sender country. These costs include lost trade revenue (Yang et al. 2004; Crozet and Hinz 2020), job losses (Hufbauer et al. 1997), and reduced foreign direct investment and financial flows (Biglaiser and Lektzian 2011; Besedeš, Goldbach and Nitsch 2021). These aggregate costs are not evenly distributed, with some businesses bearing more than others (Barber 1979; Botterill and McNaughton 2008). On the other hand, several studies demonstrate that the economic costs of sanctions on the sender country are small. Using a general rather than partial equilibrium analysis, Farmer (2000) shows the costs of sanctions to the US are minimal after accounting for offsetting adjustments in the economy. American investments in the target country decrease before impending sanctions, but often return once sanctions are imposed (Biglaiser and Lektzian 2011). German non-financial firms reduce capital flows to sanctioned targets but expand business with third-party countries linked to the target, with no overall declines in employment or sales (Besedeš, Goldbach and Nitsch 2021). A broader dyadic gravity model analysis of sanctions' effects on global value chain activities among 68 countries during 2005–2015 shows that the negative impact is significant in developing countries but small or statistically insignificant in developed countries (Le et al. 2022).

Direct empirical evidence regarding sender firms is scarce. Webb (2020) finds that US sanctions and the threat of sanctions against China create market uncertainty, raising the stock price volatility of American firms with commercial interests in target states. Analyzing targeted sanctions against Chinese tech companies, Allen (2021) argues that US firms supplying them suffer revenue losses, reputational damage, and operational uncertainty. The risk-adjusted stock returns of American

suppliers decrease by 2.2%. Using an event study of the 2020 US ban on WeChat, Crippa, Kalyanpur and Newman (Forthcoming) find American and foreign firms with significant Tencent ownership experienced average abnormal losses of $1.5 billion. These studies advance firm-level analysis but capture investor expectations during the sanction period rather than realized firm performance. Stock returns also embed sentiment, industry news, and noise (Cutler, Poterba and Summers 1988; Brogaard et al. 2022).

A broader scholarship on firm responses to political shocks advances two further arguments. The first treats firms as political actors who lobby for favorable policy outcomes or coordinate collective political action in response to ongoing geopolitical pressure (Vekasi 2019; Zhang 2025). The other treats firms as commercially entrenched in existing trade and investment networks, with sunk costs, established ties, and firm size reducing the likelihood of exit during political tension (Davis and Meunier 2011; Vortherms and Zhang 2024). Both arguments illuminate important aspects of firm behavior under political shocks. But neither traces how selection into response groups relates to post-response financial outcomes nor maps the full range of strategic responses available under partial sanctions.

In sum, existing work on sanctions and on firm responses to political shocks leaves central questions about sender-firm sanction effects unresolved on both empirical and theoretical grounds. Empirically, mixed aggregate findings may reflect not contradictions among studies but the pooling of firms whose pre-existing characteristics, and therefore response choices, differ systematically. Moreover, neither aggregate nor stock return studies examine how sender firms' realized financial fundamentals like profitability respond to sanctions, or how those effects vary with the responses firms choose. Theoretically, the debate has focused on the magnitude of a cost borne by a sender economy or a representative sender firm. A separate literature recognizes that firms respond strategically rather than simply absorbing the shock, substituting toward third markets, withdrawing, or otherwise adjusting (Early 2015; Weber and Stępień 2020; Meyer and Thein 2014), yet it does not tie a firm's response to its subsequent financial performance. Addressing both gaps requires a

theory that explains both response choice and the peer-relative financial outcomes observed among firms selected into each response, a task we take up next.

2 A Real Options Theory of Firm Response and Financial Performance Under Targeted Sanctions

We develop a real-options theory of how targeted sanctions affect sender firms, with a stylized formal presentation in Online Appendix Section 1. Our premise is that a targeted, partial sanction does not simply impose a cost but presents a choice that depends on a firm's starting position. A current supplier decides whether to continue supplying the target or exit, whereas a firm not yet a supplier decides whether to enter or stay out. Real options theory, which we employ to build our argument, is more familiar in financial economics than in political science, so we first introduce it briefly below.

A call option is the right, but not the obligation, to buy an asset at a set price. An option is valuable because the holder buys only when the price has moved in its favor and otherwise walks away. Real options theory posits that many firm decisions share this structure whenever an action is costly to reverse and the future is uncertain (Dixit and Pindyck 1994). In the canonical treatment, the uncertain quantity is the price at which a firm can sell its output, which fluctuates unpredictably over time (Dixit 1989). Later works apply the same logic to other uncertain quantities, such as the prevailing interest rate (Dixit and Pindyck 1994, ch. 2), the exchange rate a multinational faces (Kogut and Kulatilaka 1994), or any payoff-relevant state variable that evolves unpredictably.

For illustration, consider a firm deciding whether to build a plant to produce a good whose future price it cannot foresee. Building is expensive and irreversible since the outlay cannot be recovered. Suppose the plant would just break even at today's price. The firm still does better to wait. If it builds now and the price drops, it is stuck. However, if it waits and the price rises, it builds then, with no regret. Waiting is itself worth something, and committing forfeits that value.

Therefore, the firm should build only when the price exceeds its cost by a margin wide enough to cover the value of the option it gives up (Dixit and Pindyck 1994). The same logic runs in reverse for shutting down. A firm whose output price falls below its operating costs need not close immediately because closing is costly and the price may recover. Hence, the firm exits only when the price falls below a lower threshold. The two thresholds sit on a single price scale with a wide range between them in which the firm makes no change, a range Dixit (1989) calls hysteresis. A firm that has not built stays out until the price climbs above the high threshold, whereas a firm already operating stays in until the price sinks below the low threshold. Therefore, each firm faces only the threshold its current position presents.

The value of waiting is the bad outcome it avoids, including being left with a plant that cannot run profitably if the price collapses, or scrapping a business just before the price recovers (Dixit and Pindyck 1994). The two decisions (entry and exit) are determined by different resources. Whether to build a new operation whose cost cannot be recovered depends on whether the firm can afford it. A firm with ample resources can make the outlay and absorb a loss, while a stretched firm cannot. Whether to give up an operation already running depends on how good a firm's alternatives are. A firm that can redeploy its capacity elsewhere loses little by leaving and can exit readily. Beyond timing the choice, the theory also speaks to firm performance that follows. Exercising an option well, by waiting before building or staying in before exiting, protects a firm against the worst outcome. Hence, the financial outcome that follows a choice reflects both the choice and relevant resources.

We apply the real options theory described above to explain sender firms' responses and performance under targeted sanctions. Our argument deviates from the canonical real options theory in two respects. In Dixit (1989), a single firm encounters the two triggers (for entry and exit) at different moments as the output price changes across the scale. In our argument, rather than the output price, the payoff-relevant state variable is the value of supplying a target, which changes as the sender state tightens or eases its restrictions and as the target's own demand moves. Furthermore, we investigate how two distinct populations of firms respond to the common shock

of a targeted sanction, each facing only one of the two triggers. Firms already supplying the target are incumbents positioned at the exit margin, weighing the value of supplying the target against the salvage value of leaving (i.e., the amount recovered by redeploying its capacity to its best alternative use). Firms not previously suppliers are outsiders positioned at the entry margin, weighing the value of supplying the target against the cost of entering. The sanction is the common shock that shifts the underlying value for both populations at once, moving each relative to its own trigger.

Applying real options theory in our analysis requires three assumptions. First, the sanction must be partial rather than total since a total embargo eliminates options. Second, the regulatory stringency a firm faces evolves in both directions under uncertainty. Firms cannot foresee whether restrictions will tighten or ease. This two-sided uncertainty gives value both to an outsider's staying out before entry and to an incumbent's staying in before exiting. Third, the target remains a commercially viable buyer. The prospect of continued sales sustains the value of both entering and staying, though the assumption is not needed to explain an exiter's outcome.

Within each population, the decision depends on its most relevant resource, and our argument connects each resource to a specific parameter of the option problem. For an incumbent supplier, the most relevant resource is its outside options, i.e., its alternative customers to which the incumbent could redeploy its capacity. These set the relationship's salvage value, which determines the exit trigger (Dixit and Pindyck 1994). The more an incumbent supplier can earn elsewhere, the less adverse conditions need become before leaving dominates staying. This is consistent with Hirschman's logic of asymmetric dependence (Hirschman 1945).[5] Relationship-specific and switching costs cut the other way for incumbents, widening the band of inaction and making continuation the default for firms whose ties are costly to unwind, so the incumbents we observe

[5]For Hirschman, a stoppage of trade forces a partner to find "alternative markets and sources of supply and, should this prove impossible, it forces upon them economic adjustments and lasting impoverishment" (Hirschman 1945, p. 16).

staying are those with both poor outside options and costly-to-sever ties (Kogut and Kulatilaka 1994).[6]

For an outsider firm, the most relevant resource is its financial flexibility, i.e., its capacity to fund an irreversible commitment to the target and to absorb downside risk without distress (Gamba and Triantis 2008). Entry requires a sunk, irreversible outlay under regulatory risk, and the option to wait raises the bar the opportunity must clear (McDonald and Siegel 1986; Dixit and Pindyck 1994). Only a firm with sufficient financial slack can act on it (Fazzari, Hubbard and Petersen 1987; Boyle and Guthrie 2003; Gamba and Triantis 2008), so the binding constraint on entry is the balance sheet, not the customer network.

The choice and the financial outcome are not two separate effects but one process of self-selection. A firm's resources determine which option it exercises, and the financial outcome we observe is the value that option realizes for that firm (Roy 1951; Heckman 1979). Observed outcomes by response group are therefore equilibrium values for selected subpopulations, not the sanctions' effects on a randomly chosen representative firm. This logic is familiar in research on exporting, foreign investment, and supply-chain participation, where firms sort into foreign activity by their underlying productivity and capacity to bear fixed costs (Helpman, Melitz and Yeaple 2004; Antràs and Chor 2022). Unlike those heterogeneous-firm trade models, however, where productivity often determines both the value of foreign participation and the ability to bear fixed costs, our argument separates these two roles. For incumbents, outside options shape the value of exit because they determine the salvage value the firm recovers on leaving. Stayers are selected by the same exit margin in the opposite direction. They remain because their outside options are weak, leaving their peer-relative profitability theoretically unsigned. For non-suppliers, financial flexibility determines the funding constraint at entry. Entrant performance, however, depends

[6] Kogut and Kulatilaka (1994, p. 124) are the management precedent for treating operating flexibility under policy uncertainty as a valuable real option, "the option to respond to uncertain events, such as government policies," and they stress that its value depends on the firm's freedom to exercise it.

not on financial flexibility itself but on the expected surplus from entering among firms able to clear that funding constraint. Attributes that raise performance uniformly, such as productivity, shift performance levels but do not sort firms, because they move both sides of the entry or exit comparisons simultaneously.

Based on the theoretical discussions above, we identify four testable hypotheses regarding firm responses and performance for prior suppliers and non-suppliers. The real options theory explains the logic of firm responses and, together with the selection argument, the variations in financial performance of each response group relative to its peers. The comparative statics in Online Appendix Section 1 formalize this response logic.

For prior suppliers, who choose between staying and exiting, the most relevant resource is outside options. As shown formally in Online Appendix Section 1, a firm with many alternative customers can redeploy its capacity at low cost and thus give up the relationship more readily. By contrast, a firm with few alternatives has little to gain from leaving and thus stays.

Hypothesis 1a Among prior suppliers, firms with more outside options are more likely to exit than to stay.

When firms with strong outside options exit, they redeploy capacity to alternative customers that make leaving worthwhile, recovering that capacity's value rather than suffering a loss. Because outside options raise the salvage value of exit, observed exiters are disproportionately firms for which redeployment preserves much of the value that would otherwise be lost. Exit, therefore, need not translate into peer-relative profitability losses. Rather than bearing the full cost of a severed relationship with the target, exiters are selected from firms better positioned to replace it with alternative businesses.

Hypothesis 1b Exiters do not systematically underperform comparable peer firms in profitability.

The incumbents that remain are selected by the same exit margin in the opposite direction. They have limited outside options and therefore preserve the relationship rather than exit. In real-

options terms, they remain inside the inaction region. The relationship may be scaled down or mothballed, but abandonment occurs only when expected continuation losses become sufficiently large.[7] Because a firm expecting a worse continuation value would have exited, staying reflects the value of preserving the relationship relative to a weak exit option. That comparison, however, is internal to the firm and does not imply a clear prediction regarding a stayer's peer-relative performance. A stayer's target-related activity changes with conditions. It falls when restrictions tighten, or the target's demand weakens, and recovers when restrictions ease or demand returns. The theory, therefore, predicts state-dependent target-related activity but makes no directional prediction about a stayer's profitability relative to peers.

For prior non-suppliers, who choose whether to enter or stay out, the most relevant resource is financial flexibility, as discussed earlier. Entry is an irreversible commitment to the target under regulatory uncertainty, and the option to wait raises the bar the opportunity must clear. Thus, only firms with the balance-sheet capacity to fund the commitment and bear its downside risk can act on it. As Online Appendix Section 1 shows, entry requires both a favorable value and a funding constraint that only a flexible balance sheet clears.

Hypothesis 2a Among non-suppliers, firms with greater financial flexibility are more likely to enter the target's post-sanction supply chain.

Entrants are not a random subset of non-suppliers. They pass two screens. First, they can fund entry under regulatory uncertainty. Financial flexibility is the observable margin clearing this screen, which is why it predicts who enters (Hypothesis 2a). Second, among those able to fund it, they enter only if they expect the new target relationship to generate enough surplus to cover the sunk cost and the option value of waiting. Observed entrants are therefore drawn disproportionately from firms for which entry has high expected value. By contrast, comparable peers include firms

[7] In Dixit and Pindyck's lay-up model, since "restarting is costly, there will be an option value of keeping the operation alive, and abandonment will be optimal only at a sufficiently large threshold level of operating losses" (Dixit and Pindyck 1994, p. 214).

that either cannot fund entry or do not expect enough surplus to enter. If firms' expectations are reasonably accurate and the target relationship is large enough to register in firm fundamentals, entrants should outperform peers on average. This expectation follows from positive selection on the expected value of entry, not from financial flexibility itself.

Hypothesis 2b Entrants outperform comparable peer firms in profitability.

Overall, these predictions form a falsifiable pattern rather than a retrospective rationalization of successful firm choices. Outside options should predict exit but not entry, financial flexibility should predict entry but not exit, and resources that raise profitability uniformly predict neither response. The outcome predictions follow from this sorting logic and should be assessed together with the response-choice predictions. Exiters should not underperform because exit is selected on redeployable outside options, stayers' peer-relative profitability remains indeterminate and unsigned, and entrants should outperform through positive selection on the value of entering.

3 US Sanction Measures on Huawei and Responses of American Suppliers

The Huawei Entity List restrictions provide a suitable test case because they match the theory's scope conditions. These restrictions limited rather than eliminated trade. They changed in stringency over time, and involved a target that remained commercially active. The broader US campaign against Huawei had two other parts. Initially, in December 2017 and August 2018, the US government restricted federal agencies from using Huawei equipment. Later, in November 2020, it blocked US investors from investing in Huawei. But those measures constrained US importers and investors rather than Huawei's American suppliers. The treatment we study is the export-side Entity List restrictions administered by the Bureau of Industry and Security (BIS), which required government authorization before covered exports, reexports, and transfers could go to Huawei and its listed

affiliates. Figure 1 summarizes the main timeline, with details in Online Appendix Sections 2 and 3.

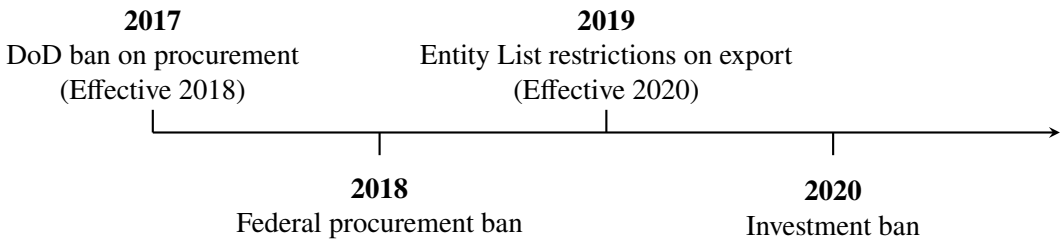

Figure 1: Timeline of Key US Sanctions on Huawei

Note: Indirect measures occurred in 2017–2018 (effective in 2018 and 2019 on imports from Huawei) and direct measures in 2019–2020 (effective in 2020 on exports to Huawei).

The direct supplier constraint began in May 2019, when BIS added Huawei and 68 non-US affiliates to the Entity List. The listing imposed a licensing requirement, but earlier BIS rules had little de facto impact because BIS repeatedly issued temporary general licenses authorizing broad classes of otherwise restricted transactions. That leeway narrowed in the second half of 2020. The list of Huawei affiliates expanded, the broad temporary general license lapsed, and a narrower permanent authorization replaced it. The foreign direct product rule then extended licensing requirements to foreign-made items built with controlled US technology and software. Thus, we treat 2020 as the onset of treatment.[8] Notably, Entity List actions bypassed the notice-and-comment requirement under the Export Control Reform Act's procedural exemption, limiting suppliers' formal opportunity to influence BIS policy implementation.[9] This feature informs our interpretation of the lobbying results below.

[8]We observe the regulatory environment and annual supply-chain relationships, not confidential firm-level license applications or approvals, so licensing enters our analysis as part of the sanction environment rather than a firm-level mechanism.

[9]The Federal Register notices for the Huawei-related Entity List actions state: "Pursuant to section 1762 of ECRA, this action is exempt from the Administrative Procedure Act (5 U.S.C. 553) requirements for notice of proposed rulemaking, opportunity for public participation, and delay in effective date." See 84 Fed. Reg. 22961 (May 21, 2019), Docket No. 2019-10616. Substantially

We use Bloomberg and FactSet supply chain data to track American firms' relationships with Huawei from 2015 to 2023. For each firm-year, we classify whether a firm supplied Huawei, exited relative to the prior year, continued supplying, or entered. Because the export-side restriction became operationally binding in 2020, the empirical analysis focuses on three post-2020 response groups. Stayers supplied Huawei before 2018 and continued supplying throughout the post-2020 window. Exiters supplied before 2018 but supplied for only part of the post-2020 window. Entrants had no prior supply relationship with Huawei and began supplying for the first time during 2020-2023. Online Appendix Figures 2-4 show the firm-level supply chain timelines, with first-time entrants and re-entries by former suppliers separated.

To evaluate financial performance, we compare these response groups with US firms that never supplied Huawei but were marked as product market peers before treatment. We build the peer group from FactSet competitor links observed from 2010 to 2017, which ended before any Huawei sanctions took effect. This comparison group is central to identification. Treated firms and peers operated in similar product markets and faced similar competitive pressure before the export-side restrictions. This design also separates Huawei-specific exposure from broader shocks to the technology or semiconductor sector.

4 Data and Methodology

Having identified the three response groups and peers, we turn to data and two sets of models, including probit models of firm responses and difference-in-differences models of financial performance.

similar language appears in subsequent Entity List modifications and Temporary General License extensions. For details, see Online Appendix Section 3.

4.1 Models of Response Choices

The theory's first predictions concern which firms choose which response (Hypotheses 1a and 2a). We test these hypotheses with two probit models, one per sample, estimated on pre-treatment characteristics. Summary statistics for the two samples are in Online Appendix Section 4 (Table 2 and 3).

The sample of prior suppliers combines the exiter-post-2020 and stayer-post-2020 firms. We model the choice to exit (coded as 1) versus to stay (coded as 0). The key explanatory variable is the firm's outside options. We measure it as the logged number of the supplier's customers in Huawei's industry. These are the buyers to which the supplier could redeploy the capacity now serving Huawei. We define the industry using FactSet sector code 1300, "Electronic Technology." All counts are measured in 2017, the last pre-sanction year. A supplier with more such customers can redeploy at lower cost, so it is more likely to exit (H1a).

The sample of prior non-suppliers includes entrant-post-2020 firms and their non-entrant competitor peers in the same industries, based on FactSet 2010–2017 data.[10] We model the choice to enter (1) versus stay out (0). The key explanatory variable is the firm's financial flexibility, measured by its debt ratio. The debt ratio (i.e., total debt over total assets) indicates a company's leverage and financial inflexibility (Boyle and Guthrie 2003). A low debt ratio indicates greater capacity to fund an irreversible commitment (H2a).

Both models include the same pre-treatment covariates as controls. Research-and-development intensity (R&D expenditures over sales) and advertising intensity (advertising expenditures over

[10]For this choice model, we restrict the non-entrant peer set to each entrant's own FactSet 2010-2017 industry peers. These firms form the relevant non-supplier choice set and provide a sharper comparison. This produces 12 entrants and 358 non-entrant peers. In the full model, the estimation sample is 223 firms (10 entrants and 213 peers). These firms are a subset of the 506 peer firm pool used in the DiD analysis.

sales) are the resources the theory predicts should *not* sort firms into responses, because they raise the value of every option together and cancel from the choice.

We also include lobbying expenditures, a proxy for political access, to control for the prominent argument that politically organized firms shape sanctions and their own treatment within the regime (Kaempfer and Lowenberg 1992; McLean and Whang 2014; Bapat and Kwon 2015; Zhao 2026). However, the Entity List's procedural exemption (see footnote 9) forecloses the formal channel for access to policy implementation, rendering political access ineffective at the implementation stage. Data on lobbying expenditure comes from LobbyView (Kim 2018), which compiles firm-level lobbying reports filed under the Lobbying Disclosure Act.

4.2 Models of Financial Performance Outcomes

Treatment, response and control groups. As noted earlier, the export-side Entity List restrictions became operationally binding in 2020. We compare the three response groups to never-supplier product-market peers identified before treatment, as shown in Table 1. Financial data are from COMPUSTAT, which covers primarily publicly traded firms. Subject to this availability, the estimation sample comprises 545 US firms from 2015 to 2023, including 39 Huawei suppliers (15 stayers, 14 exiters, 10 entrants) and 506 peers. The 39 suppliers are those of the 52 firms in the full treated set for which COMPUSTAT financial data are available. The unit of analysis is firm-year. Summary statistics for all variables are in Online Appendix Section 4 Table 4.

Table 1: Firm Type Composition in Estimation Sample

Firm Group	Total Firms	Public Firms	Private Firms	Estimation Sample
Stayer-post-2020	19	16	3	15
Exiter-post-2020	21	15	6	14
Entrant-post-2020	12	12	0	10
US peers	767	618	149	506

Note: Sample size is restricted by the availability of financial data in COMPUSTAT.

Variables and Estimation. The primary outcome variable is firm profitability, measured by Return on Assets (ROA), calculated as operating profit divided by average total assets, drawn from COMPUSTAT.[11] ROA is the outcome most directly tied to our theoretical expectations about whether sanctions leave firms financially worse off or better off relative to peers. The numerator captures the difference between operating revenue and the cost of goods and other operating expenses. As commonly used in business research, ROA measures how effectively a firm uses its assets to generate profits from core operations before accounting for depreciation and amortization (DesJardine and Durand 2020; Awaysheh et al. 2020; Berchicci and King 2022). ROA reflects the net effect on operating profitability of sanction-induced changes in revenues, costs, and firm response.

We also estimate models of operating revenue and operating expenses, the components of operating profitability, as diagnostics. Our theory's predictions concern response choice and peer-relative profitability, not these components, since each ROA hypothesis may be consistent with multiple combinations of revenue and cost changes. We report these models to check that no component moves in a way that would be difficult to reconcile with the ROA results.

We estimate peer-relative effects with the doubly robust difference-in-differences (DR-DiD) estimator of Callaway and Sant'Anna (2021), which combines an outcome regression (OR), fitted on never-supplier peers to predict each treated firm's counterfactual change, with inverse probability weighting (IPW) based on the estimated propensity of treatment given pre-treatment covariates. The estimator is doubly robust, meaning that the average treatment effect on the treated (ATT) is consistently estimated if either the outcome regression or the weighting model is correctly specified. For a cohort with treatment year g, the group-time ATT is:

[11] According to the SAS code for the measure in COMPUSTAT from WRDS, the denominator is the average of Total Assets (*at*) in the current and prior periods, and the numerator is Operating Income Before Depreciation (*oibdp*). If missing, the numerator value is replaced by gross Sales minus Operating Expenses (*sale-xopr*), and failing that by Revenue minus Taxes and Operating Expenses (*revt-xopr*).

$$ATT(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X)\,C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)\,C}{1-p_g(X)}\right]}\right)\left(Y_t - Y_{g-1} - m_{g,t}(X)\right)\right], \tag{1}$$

where G_g equals 1 for firms assigned treatment year g, C equals 1 for never-supplier peers, Y_{g-1} is the outcome in cohort g's last pre-treatment period, $p_g(X)$ is the propensity of belonging to cohort g rather than the peer group, and $m_{g,t}(X)$ is the change in Y OR-predicted counterfactual change. For response groups with a common treatment year, our reported ATT averages $ATT(g,t)$ over post-treatment years. For exiters, whose exit years vary, ATT averages the relevant cohort-year effects.

Identification rests on the conditional parallel trends assumption. Conditional on pre-treatment covariates, American suppliers' financial outcomes would have followed the same trend as those of firms that never supplied Huawei (Caetano and Callaway 2024). We condition on six pre-treatment covariates, including the number of customers in Huawei's industry (outside options), R&D expenditures as a share of sales (technological intensity and innovation capacity), advertising expenditures as a share of sales (marketing intensity and commitment to brand investment), the debt-to-assets ratio (overall financial inflexibility and balance sheet structure), total assets (firm size and resource capacity), and firm lobbying expenditures.

To keep covariates exogenous to both the 2018 import-side measures and possible anticipation of export-side restrictions, we hold each covariate at its 2017 value (the last year before any sanction took effect) for 2018–2023 and use its $t-1$ value in years before 2018. The outcome pre-period still includes 2018 and 2019 because the import-side measures affected Huawei's procurement environment but did not restrict US suppliers' exports to Huawei. These years therefore help trace pre-treatment outcome trends before the 2020 export-side treatment. The covariate-measurement date and the treatment-onset date thus reflect different considerations and do not coincide.

The 2020 treatment timing raises one additional identification concern because the post-treatment window overlaps with COVID-19, semiconductor-cycle shocks, and technology-sector

demand shifts. Common shocks do not threaten identification since they are differenced out in the treated-versus-peer comparison. The more serious threat is differential exposure. Huawei suppliers may be concentrated in product markets or supply-chain positions affected differently by these shocks, and because many are semiconductor and component firms, one may worry that entrant outperformance reflects the chip cycle rather than the sanctions. Our peer design addresses this concern. Each treated firm is compared against its pre-sanction FactSet competitors, which operated in the same product markets and faced similar competitive pressures, so broad semiconductor or technology shocks should move treated and peer firms together.

We further condition on pre-treatment firm characteristics and report pre-trend diagnostics for each response group. As shown in Online Appendix Section 4 Table 6, the parallel-trends assumption is not rejected for the ROA outcome. We also conduct sensitivity analysis across alternative peer definitions, including the 2017 FactSet snapshot and the combined FactSet–Orbis peer set. Results are robust to alternative peer definitions, as shown in Online Appendix Section 4 Table 5. These checks cannot rule out every differential shock over the post-2020 window, but they make the identifying assumption explicit.

Our empirical analysis proceeds in two steps. First, we establish a naive baseline by applying the estimator in Equation (1) to the pooled sample, treating all US suppliers as a single treated group relative to the peer group and ignoring response types, with a common treatment year $g = 2020$. The pooled ATT is the post-treatment time-average of the period-specific effects $ATT(2020, t)$ over $t \geq 2020$. It therefore captures the average post-2020 financial performance of US Huawei suppliers relative to never-supplier peers.

Second, we estimate response-specific peer-relative performance. For each response group, the primary estimate is the average ATT over the post-2020 window. These ATTs compare firms selecting into a given response with comparable never-supplier peers. Hence, they should be read as local estimates for specific response groups, not effects of randomly assigning firms to stay, exit, or enter. Under conditional parallel trends and no anticipation, the estimator identifies the ATT for

firms in each response group. Selection determines which firms enter the group, while the peer comparison identifies their counterfactual performance.

Stayers supplied Huawei throughout the post-2020 window and share the common 2020 treatment date (Online Appendix Figure 2). We therefore use the same specification as the pooled model, with the estimation sample restricted to stayers and never-supplier peers. Because the theory makes no directional prediction of profitability for stayers, we do not treat these estimates as tests of a directional hypothesis.

Entrants are concentrated in 2020, with only a thin 2021 entry cohort that is too small to support staggered estimation (Online Appendix Figure 4). We therefore anchor all entrants to the common 2020 treatment date, again with the estimation sample restricted to entrants and never-supplier peers, producing a conservative estimate that is robust to alternative estimation.[12]

Exiters differ because firms left in different years. Of the 14 exiters in the estimation sample, 3 exited in 2021, 5 in 2022, and 6 in 2023 (Online Appendix Figure 3). For exiters, we use the staggered doubly robust estimator on a sample restricted to exiters and never-supplier peers, estimating the ATT from cohort-by-year comparisons, each set against the peers. Firms that exit in later years do not serve as controls for earlier cohorts. The exiter model therefore identifies post-exit financial performance relative to peers, with treatment timed to each firm's exit year.

[12]Of the 10 entrants in the estimation sample, 9 entered in 2020 and 1 in 2021. For the 2021 entrant, this codes its 2020 pre-entry year as post-treatment, which adds a null pre-entry year and biases the entrant's ATT toward zero, making the estimate conservative. Dropping the 2021 entrant and re-estimating on 2020 entrants only leaves the results largely unchanged; the ROA effect is slightly larger (0.045 to 0.052) and remains significant.

5 Findings

We present the results in two parts. First, Tables 2 and 3 report the results from the response choice models. Second, Table 4 reports the pooled financial outcome estimates, and Tables 5, 6 and 7 report response-conditional estimates for stayers, entrants, and exiters, respectively.

5.1 Response Model Results

Table 2 reports the models for the choice to exit versus stay among prior suppliers. The coefficient on outside options, the number of same-industry customers, is positive and statistically significant, both on its own (0.571, $p < 0.05$) and with the full set of controls (0.664, $p < 0.05$). Thus, a supplier with more redeployable customer relationships is more likely to leave Huawei's supply chain. In the full model, a one-unit increase in the number of same-industry customers (in logs) is associated with an increase of about 21 percentage points in the probability of supplier exit. None of the other covariates, such as R&D intensity, advertising intensity, debt ratio, or lobbying, is significant individually or collectively. A joint test cannot reject the hypothesis that the four controls add no explanatory power. These findings match our expectations that exit is determined by outside options rather than the level of resources or political access.

Table 3 reports the models for the choice to enter versus stay out among prior non-suppliers. The coefficient of the debt ratio is negative and significant, on its own (-1.642, $p < 0.05$) and with controls (-1.549, $p < 0.05$). A firm with greater financial flexibility, i.e., lower leverage, is more likely to enter. A one-unit increase in the debt ratio is associated with a 13-percentage-point decrease in the probability of entry. Same-industry customer count, which predicts exit among prior suppliers, does not predict entry, exactly the cross-prediction the theory implies. Outside options set the salvage value of an existing relationship but play no role in the funding constraint at entry. R&D intensity, advertising intensity, and lobbying are likewise insignificant, individually and jointly.

Table 2: Probit Response Choice Models: Exit (1) vs. Stay (0)

	(1) Baseline	(2) R&D	(3) Adv.	(4) Debt	(5) Lobby	(6) Full
Log(Number of Customers in Huawei's Industry)	0.571**					0.664**
	(0.261)					(0.303)
R&D / Sales		0.987				1.000
		(1.782)				(2.300)
Advertising / Sales			0.261			0.082
			(0.655)			(0.699)
Debt Ratio				1.230		1.387
				(1.279)		(1.618)
Log(Lobbying Expenditures)					0.036	-0.023
					(0.032)	(0.047)
Constant	-0.786**	-0.244	-0.135	-0.626	-0.090	-1.661
	(0.399)	(0.371)	(0.263)	(0.612)	(0.240)	(0.924)
Observations	32	30	30	30	40	30
Pseudo R^2	0.120	0.007	0.004	0.023	0.024	0.173
Joint test (χ^2, df=4)						0.868
p-value						0.929

Notes: DV = 1 (exit), 0 (stay). Columns differ in N because each probit keeps non-missing observations. We conduct a Wald test that R&D, Advertising, Debt, and Lobbying coefficients are jointly zero. ** $p < 0.05$, *** $p < 0.01$.

In sum, the response choice models display the differentiated sorting pattern the theory predicts and could have falsified. Outside options predict exit but not entry, financial flexibility predicts entry but not exit, and other resources the theory predicts to be irrelevant predicts neither neither choice. The null coefficient of lobbying in both selection models is consistent with the procedurally insulated nature of the Entity List regime. The public-comment record for the Temporary General License reinforces this interpretation.[13] The docket drew 25 comments, but none came directly from the 52 treated firms; only four treated firms were indirectly represented through industry associations. Those comments accepted the Entity List restriction as given and sought operational continuity, such as longer license extensions and clearer licensing criteria, rather than a reversal of the restriction. In this implementation setting, firms sorted on economic resources, not measured political access.

[13] See Federal Register Docket No. 2020-05194.

Table 3: Probit Response Choice Models: Enter (1) vs. Stay Out (0)

	(1) Debt	(2) Customers	(3) R&D	(4) Advert.	(5) Lobby	(6) Full
Debt Ratio	-1.642**					-1.549**
	(0.644)					(0.623)
Log(Number of Customers in Huawei's Industry)		0.096				0.191
		(0.102)				(0.149)
R&D / Sales			-0.042			-0.121
			(0.067)			(0.160)
Advertising / Sales				-0.048		-0.043
				(0.034)		(0.032)
Log(Lobbying Expenditures)					-0.023	-0.022
					(0.020)	(0.026)
Constant	-0.957***	-1.941***	-1.690***	-1.583***	-1.748***	-0.931***
	(0.296)	(0.167)	(0.148)	(0.162)	(0.149)	(0.306)
Observations	241	370	223	223	370	223
Pseudo R^2	0.076	0.008	0.000	0.022	0.012	0.112
Joint test (χ^2, df=4)						5.838
p-value						0.212

Notes: DV = 1 (entrant), 0 (stay out). We conduct a Wald test that Customers, R&D, Advertising, and Lobbying coefficients are jointly zero. ** $p < 0.05$, *** $p < 0.01$.

5.2 Financial Outcome Model Results

As noted, ROA is the primary financial outcome of interest, with operating revenue and operating expenses serving as diagnostics for the ROA results. In each outcome table, Panel A reports the overall response-conditional ATT as our primary estimate, while Panel B reports dynamic or cohort-specific estimates. Because the treated groups are small, we use the period-by-period estimates to characterize the timing of effects and assess pre-treatment fit, rather than using them as the main basis for inference.

The validity of our findings depends on the conditional parallel trends assumption. The pre-treatment coefficients in the event studies are statistically insignificant for almost all models, and the joint pre-trend tests reported in Online Appendix Section 4 Table 6 do not reject the parallel-trends assumption for ROA or revenue in any model (for exiters, assessed under a common 2020 pseudo-treatment date). Since the assumption is rejected for the expense models for stayers and entrants, those estimates cannot serve the diagnostic purpose, and we do not interpret them.

Finally, our main findings are robust to two alternative peer definitions (2017 FactSet and FactSet-Orbis combined peers), as reported in Online Appendix Section 4 Table 5. Furthermore, as shown in Online Appendix Section 4 Table 7, ROA placebo ATTs based on fake 2017, 2018, and 2019 onsets show small, statistically insignificant effects.

5.2.1 Pooled Treated-Firm Effect

We first estimate a naive baseline model pooling all American suppliers into a single treated group. Table 4 shows the ROA ATT is near zero (-0.0002) and statistically insignificant. The revenue and expenses estimates are also small and insignificant, raising no difficulty for the ROA result. Panel B shows the same pattern in the dynamic estimates, with no statistically significant post-treatment coefficients. Overall, the pooled estimates suggest little average sender-firm effect. The response-conditional results below show that this average conceals systematic heterogeneity.

Table 4: Peer-Relative Financial Performance of American Suppliers, Pooled

	ROA	Revenue	Expenses
Panel A. Overall ATT			
ATT	-0.0002	0.0063	-0.0025
	(0.0126)	(0.0609)	(0.0736)
Panel B. Event Study (Dynamic ATT)			
t-4	-0.0018	0.0664	0.0825**
	(0.0068)	(0.0429)	(0.0368)
t-3	0.0059	0.0549	0.0347
	(0.0124)	(0.0918)	(0.0682)
t-2	-0.0008	-0.0148	0.0007
	(0.0184)	(0.0576)	(0.0306)
t-1	-0.0040	-0.0558	-0.0524
	(0.0089)	(0.0696)	(0.0738)
t	-0.0031	0.0085	0.0355
	(0.0094)	(0.0660)	(0.0536)
t+1	0.0023	0.0738	0.0388
	(0.0146)	(0.0848)	(0.0889)
t+2	0.0016	0.0291	-0.0058
	(0.0157)	(0.0926)	(0.1025)
t+3	-0.0015	-0.0929	-0.0882
	(0.0199)	(0.0933)	(0.0956)
Observations	4116	4194	4194

Notes: ** $p < 0.05$, *** $p < 0.01$.

5.2.2 Outcomes by Response Types

Stayers. Because the theory makes no directional prediction about stayers' profitability relative to peers, these estimates characterize the financial performance of firms selecting into staying rather than test a directional hypothesis. Firms that continued supplying Huawei throughout the post-2020 window show no significant ROA difference from peers (Panel A, Table 5). The overall revenue estimate is statistically insignificant, and the expense estimate is not interpreted given the failed pre-trend test.

The event study results are broadly consistent, except for one component result that requires reconciliation with the ROA findings. There is a statistically significant decline in operating revenue late in the post-sanction window, while peer-relative ROA remains statistically insignificant throughout. The two results are compatible. Whatever revenue pressure stayers faced did not translate into peer-relative profitability losses, and since ROA is operating income scaled by assets, stable profitability alongside declining revenue implies offsetting adjustment elsewhere in operations, the source of which our data cannot identify. The revenue decline is also compatible with, though not predicted by, the state-dependent target-related activity described in Section 2. Firm-level revenue reflects a firm's entire business rather than Huawei-specific sales, so this decline is at most an indirect signal.

Entrants. Firms that joined Huawei's supply chain in 2020 or later display a substantial profitability advantage over peers (Panel A, Table 6). The ATT on ROA is 4.5 percentage points ($p < 0.01$), economically substantial relative to a sample mean ROA near 0.06. Event-study estimates show that this advantage emerges in the first full year after entry ($t+1$, 5.1 percentage points, $p < 0.01$) and persists through 2023 ($t+3$, 6.3 percentage points, $p < 0.05$), suggesting a durable rather than transitory effect (Panel B). Pre-treatment ROA estimates are small and statistically indistinguishable from zero, consistent with parallel trends.

Table 5: Peer-Relative Financial Performance of Stayers

	ROA	Revenue	Expenses
Panel A. Overall ATT			
ATT	-0.0117	-0.1017	-0.0604
	(0.0190)	(0.0712)	(0.1268)
Panel B. Event Study (Dynamic ATT)			
t-4	-0.0028	0.0838	0.1177
	(0.0108)	(0.0814)	(0.0648)
t-3	0.0123	0.1256	0.0789
	(0.0236)	(0.2161)	(0.1544)
t-2	-0.0093	-0.0466	0.0085
	(0.0369)	(0.1329)	(0.0655)
t-1	-0.0040	-0.1998	-0.2128
	(0.0193)	(0.1439)	(0.1615)
t	-0.0100	-0.1438	-0.0191
	(0.0114)	(0.0831)	(0.0751)
t+1	-0.0052	0.0643	0.0189
	(0.0242)	(0.1325)	(0.1577)
t+2	-0.0085	-0.0924	-0.0643
	(0.0244)	(0.1398)	(0.1701)
t+3	-0.0232	-0.2350**	-0.1773
	(0.0281)	(0.1094)	(0.1300)
Observations	3925	4002	4002

Notes: ** $p < 0.05$, *** $p < 0.01$.

Estimates on revenue are statistically insignificant, and the expense estimates are not interpreted given the failed pre-trend test. Nothing in the interpretable component results is difficult to reconcile with the ROA findings. The persistent ROA advantage is consistent with Hypothesis 2b. Entrants self-selected into supplying Huawei on the expectation of profitable opportunities in the disrupted supply chain.

Table 6: Peer-Relative Financial Performance of Post-2020 Entrants

	ROA	Revenue	Expenses
Panel A. Overall ATT			
ATT	0.0453***	0.1119	-0.0062
	(0.0140)	(0.1228)	(0.1102)
Panel B. Event Study (Dynamic ATT)			
t-4	0.0037	0.0534	0.0907
	(0.0070)	(0.0546)	(0.0591)
t-3	-0.0078	0.0294	0.0478
	(0.0214)	(0.0784)	(0.0927)
t-2	0.0139	-0.0073	0.0011
	(0.0166)	(0.0490)	(0.0394)
t-1	-0.0099	0.0858	0.1044
	(0.0111)	(0.0959)	(0.0741)
t	0.0210	0.1392	0.0308
	(0.0143)	(0.1692)	(0.1377)
t+1	0.0510***	0.0972	0.0353
	(0.0147)	(0.1849)	(0.1540)
t+2	0.0492**	0.1220	-0.0341
	(0.0227)	(0.1704)	(0.1967)
t+3	0.0626**	0.0875	-0.0681
	(0.0269)	(0.2053)	(0.2284)
Observations	3861	3938	3938

Notes: ** $p < 0.05$, *** $p < 0.01$.

Exiters. Consistent with the selection logic, we find no statistically significant peer-relative underperformance among firms that left Huawei's supply chain (Panel A, Table 7). The overall ATT on ROA is small and statistically insignificant (0.0045), and the lower bound of its 95 percent confidence interval rules out peer-relative losses larger than 3.2 percentage points, so any exit-related underperformance is at most modest. Neither component estimate challenges the ROA

result. Firms exit when their outside options allow them to redeploy capacity at low cost, so observed exiters are those most likely to redeploy without significantly lower profitability than their peers.

The cohort breakdown in Panel B shows the 2021 and 2023 exit cohorts are indistinguishable from peers. But the 2022 cohort shows a statistically significant positive effect in ROA only. The result does not contradict our argument, but because these cohort estimates rely on very small groups, we draw conclusions from the overall exiter ATT rather than from any single cohort.

Table 7: Peer-Relative Financial Performance of Exiters (Staggered DiD; Treatment = Year of Exit)

	ROA	Revenue	Expenses
Panel A. Overall ATT			
ATT	-0.0045	-0.0527	-0.0200
	(0.0143)	(0.1193)	(0.0991)
Panel B. Cohort-Specific Effects			
Cohort 2021	-0.0139	-0.0342	-0.0498
	(0.0101)	(0.2200)	(0.2075)
Cohort 2022	0.0259**	-0.0560	-0.0847
	(0.0102)	(0.2110)	(0.1474)
Cohort 2023	-0.0175	-0.0722	0.0676
	(0.0348)	(0.1727)	(0.0939)
Observations	3896	3970	3970

Notes: Panel A ATT is a weighted average of the cohort effects (weight = cohort size × post-exit years). 2020 exiters omitted for insufficient data. ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

Whether economic sanctions impose substantial costs on the sender has long been debated, with mixed conclusions. Much of that debate was shaped by comprehensive sanctions and cast in terms of aggregate trade and investment flows or the cost to a representative sender firm. Increasingly salient targeted sanctions differ in that they are partial, evolving, and aimed at designated firms. Their effects on sender firms depend on how firms respond. A partial, evolving restriction does not

impose a single sender-firm cost but generates response-conditional outcomes. In our real-options theory, prior suppliers stay or exit, while prior non-suppliers enter or stay out. Exiters, driven by outside options and redeployment, should not underperform peers. Stayers, whose choice reveals only that continuation dominates a weak exit alternative, carry no clear prediction relative to peers. Entrants outperform peers because, among non-supplier firms that can fund entry, entrants are positively selected on the expected value of entering.

The evidence supports this argument. Using doubly robust difference-in-differences on 545 American supplier and peer firms from 2015 to 2023, together with two probit models of response choices, we find a pooled effect near zero that masks systematic heterogeneity across responses. Among prior suppliers, firms with stronger outside options are more likely to exit than to stay. Among prior non-suppliers, firms with greater financial flexibility are more likely to enter. Research intensity, advertising intensity, and lobbying predict neither choice.

The outcome models, estimated on realized financial fundamentals rather than stock returns, show distinct peer-relative patterns across response groups. Firms that enter Huawei's supply chain in 2020 or later have a sustained profitability advantage over comparable peers, on the order of four to five percentage points of return on assets. By contrast, we find no statistically significant peer-relative profitability losses for stayers or exiters. The pooled null finding is not evidence that targeted sanctions did nothing. It reflects divergent response-conditional outcomes that average to near zero. By the same logic, mixed findings in aggregate trade and investment studies may reflect differences in samples and in the mix of firm responses they capture, rather than disagreement over a single representative sender-firm cost.

Theoretically, we contribute a real-options framework that recasts the low- versus high-cost debate. That debate presumes a single representative sender-firm cost. Our theory suggests that the partial evolving restriction generates response-conditional costs, and it identifies entry, a response prior work neither theorized nor measured, as integral to any theory of sender-side effects.

Beyond the sanctions literature, our research also speaks to work on firm responses to political shocks. While those studies emphasize lobbying and collective action (Vekasi 2019; Zhang 2025) or entrenchment in existing trade and investment networks (Davis and Meunier 2011; Vortherms and Zhang 2024), we show that firms also adapt through strategic commercial choices. Although the null association between lobbying expenditures and firm response is suggestive rather than definitive, it implies possible refinement of political-access arguments. Once Entity List restrictions were imposed through insulated administrative procedures, firms' responses were associated with commercial resources rather than lobbying expenditures, even though political access may matter at the design stage or when implementation channels remain open.

Although our evidence comes from a single target, our theory applies to any partial restriction with evolving stringency on a commercially viable target. Contemporary instruments such as Russia-related sectoral sanctions, semiconductor export controls beyond Huawei, and other non-blocking OFAC measures that condition rather than prohibit specified transactions under IEEPA satisfy these conditions to varying degrees and offer natural settings for applying and testing our theory. By contrast, comprehensive embargoes and full blockade fall outside the theory's scope conditions unless licensing or other authorizations preserve meaningful commercial options.

The findings also help explain why domestic political resistance from US businesses toward sanctions may remain limited even as economic coercion has grown more frequent. Targeted sanctions need not impose a uniform national cost. They distribute risks and opportunities across firms according to the options firms can realistically exercise. Firms best positioned to redeploy capacity or finance new relationships have little reason to resist. Firms with few outside options have more reason to resist, but the procedural insulation of Entity List actions forecloses an important channel for doing so. For sanction design, the implication suggests caution. Domestic disruption depends not only on the severity of the restriction but also on the distribution of outside options and financial flexibility among exposed firms. Policymakers seeking to anticipate domestic costs should therefore pay attention to these firm-level adaptation margins.

Our research has several limitations that warrant caution and suggest possible directions for further research. As noted earlier, we observe the licensing environment, but not firm-specific license approvals, and our outcomes are firm-level totals rather than Huawei-specific activity. Incorporating firm-level license data and relationship-level sales would sharpen the within-group analysis, especially for stayers. The number of treated firms is small, so the response-group estimates, and especially the period-by-period and cohort breakdowns, should be treated with caution. Our research identifies effects on sender-firm fundamentals and does not consider target-side costs. Extending the analysis to other partial and evolving sanction regimes, longer panels that capture later adjustment, and target-side outcomes could lead to useful insights.

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