Business and Politics (2021), **23**, 330–343
doi:10.1017/bap.2020.21

CAMBRIDGE UNIVERSITY PRESS

RESEARCH ARTICLE

Do Targeted Trade Sanctions Against Chinese Technology Companies Affect US Firms? Evidence from an Event Study

Jeffrey S. Allen

Johns Hopkins University, School of Advanced International Studies, 1740 Massachusetts Avenue NW, Washington, DC, 20036, United States
Corresponding author: Jeffrey S. Allen, email: jallen71@jhu.edu

Abstract

This article asks how costly targeted trade sanctions imposed by the US government are for domestic firms. I argue that, as a result of sanctions, the firm value of US companies that have supply relationships with sanctioned entities is likely to suffer from lost revenue, reputational damage, and business model uncertainty. I test this expectation by applying an event study to the important case of targeted trade sanctions against Chinese technology companies. I find that sanctions against these companies reduced their US suppliers' risk-adjusted stock returns by 220 basis points. Firm-level cross-sectional analysis shows that businesses with stronger ties to the sanctioned entities are more negatively affected, which supports the direct connection between sanctions and relevant suppliers. Measuring the domestic economic ramifications of sanctions for the sender country has been elusive. These findings, which are statistically and economically significant, indicate that US companies face notable costs from sanctions against internationally active firms.

Keywords: economic sanctions; economic statecraft; international political economy; event study; US-China relations; great power competition

Introduction

Economic sanctions are a fixture of American foreign policy. They are deployed to address all manner of disputes in the international system. Though their record of coercing targets to change their behavior is fairly limited, US policymakers value sanctions as a non-kinetic option for signaling resolve and punishing bad behavior in the international arena.[1]

A key development in economic statecraft has been foreign policymakers' use of "targeted sanctions," which are imposed on firms, elites, and other entities, rather than on entire economies or economic sectors.[2] A policy innovation of the late 1990s, targeted sanctions emerged in response to the grim international consensus that comprehensive economic sanctions against Iraq earlier in the decade were responsible for more civilian causalities than the Gulf War itself.

As the "precision-guided munitions of economic statecraft," targeted sanctions are more humanitarian and better able to limit collateral damage in the sender and target countries than more comprehensive measures.[3] However, the perceived low cost and ease of implementing targeted sanctions have arguably lowered policymakers' sanctions inhibitions.[4] Targeted sanctions are so prevalent today that experts warn of their "re-comprehensivization."[5]

The US government's entity list, a targeted trade sanctions list managed by the Department of Commerce's Bureau of Industry and Security (BIS), embodies this acceleration in targeted sanctions imposition. Established in 1997, the entity list was originally limited to entities suspected of

[1]Pape, 1997; Hufbauer et al., 2009; Morgan, Bapat, and Kobayashi, 2014; Biersteker, Eckert, and Tourinho, 2016.
[2]Cortright and Lopez, 2002; Elliott, 2002; Drezner, 2011, 2015; Biersteker, Eckert, and Tourinho, 2016.
[3]Drezner, 2011, 96.
[4]Early and Schulzke, 2019.
[5]Biersteker, Eckert, and Tourinho, 2016, 274.

perpetuating weapons of mass destruction proliferation.[6] At the end of 2007, there were seventy entities on the list from eight countries.[7] Then, in 2008, the US government expanded the policy scope of the entity list to include "entities acting contrary to the national security or foreign policy interests of the United States."[8] By year-end 2019, there were 1,229 entities on the list from seventy-six international jurisdictions.

Beyond its rapid growth, another striking feature of the entity list is that it increasingly hosts large firms based in interstate rivals. For instance, since 2016, US policymakers have used the list to sanction a variety of multinational Chinese technology companies, such as Zhongxing Telecommunications Equipment (ZTE) Corporation, Huawei Technologies Co. Ltd. (Huawei), Dahua Technology, and Hikvision, among others. Similarly, many of Russia's largest nonfinancial companies, including Gazprom, Lukoil, and Rosneft, are on the list. This qualitative shift in entity list sanctions is consistent with US foreign policymakers' renewed emphasis on great power competition.[9]

Among their many potential effects, economic sanctions are expected to generate costs for the sender country. Indeed, sanctions work by denying domestic firms' ability to do business with targets. We know very little about the magnitude of the costs that sanctions generate for sender countries, perhaps because "it is just plain hard to quantify the costs to the sender country."[10] Still, there have been efforts to quantify lost or diverted trade in the sender country due to comprehensive or selective sanctions.[11] A recent study also explores how sanctions affect US firms' stock price volatility.[12] Less attention has been paid to the cost of targeted sanctions, which are the instrument of choice for sanctions policymakers worldwide.[13]

Targeted sanctions play out on a firm-to-firm or firm-to-individual level. The domestic effects of targeted sanctions in the sender country are felt most immediately by the firms that have business ties to sanctioned entities. Targeted sanctions undoubtedly incur lower costs for sender countries than more comprehensive measures, but this has arguably led to a sanctioning moral hazard.[14] The proliferation of targeted economic sanctions compels renewed efforts to quantify the compliance costs these measures incur for sender-country firms. A recent analysis of targeted sanctions calls for more research into this precise issue.[15] In this article, I answer that call for an important contemporary context by examining how the US government's addition of certain Chinese technology companies to the entity list has affected the sanctioned companies' American suppliers.

Quantifying the cost of entity list sanctions against Chinese technology companies to US firms is a timely and worthwhile undertaking. These measures have been a salient component of the US government's approach to what political scientists and technology analysts call the US-China technology Cold War.[16] Economic sanctions generally entail compliance considerations for sender-country firms, but the stakes are high and the potential blowback is significant when sanctions target internationally active firms from large, interstate rivals.

The urgency of this task is further supported by these instruments' projected efficacy. Unilateral targeted trade sanctions, such as those under investigation, are expected to be among the least effective

[6]Department of Commerce, 1997; Bureau of Industry and Security, n.d.a.

[7]Data on current entity list members are obtained from International Trade Administration (2020). Data on past entity list members are obtained from entity list Federal Register notices and 15 C.F.R. Supplement No. 4 to Part 744 – Entity List.

[8]Department of Commerce, 2008, 49331.

[9]United States, 2017, 2018.

[10]Hufbauer et al., 2009, 108.

[11]e.g., Hufbauer et al., 1997; DeRosa, 2009; Richardson, 1993. Comprehensive sanctions are those that are directed "indiscriminately at an entire population" (Biersteker, Eckert, and Tourinho, 2016, 2). An example is a blanket trade embargo. Selective sanctions are "less-than-comprehensive measures involving restrictions on particular products or financial flows" (Elliott, 2002, 172). An example is restricting high-technology exports to a certain country.

[12]Webb, 2020.

[13]Cortright and Lopez, 2002; Drezner, 2011, 2015.

[14]Early and Schulzke, 2019.

[15]See: Biersteker, Eckert, and Tourinho, 2016, 277.

[16]Muñiz, 2019; Segal, 2018, 2019; Triolo, Allison, and Brown, 2018.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

forms of sanctions because they are difficult to enforce and incentivize noncompliance.[17] Moreover, sanctions between great power adversaries are typically ineffectual because rivals expect frequent confrontations, and submitting to pressure is very costly.[18]

Policymakers also claim to care about the cost of sanctions. Though the BIS's first guiding principle makes clear that national security is its top priority, its second guiding principle revolves around "ensuring the health of the US economy and the competitiveness of US industry."[19] Considering the potential ramifications of unilateral targeted trade sanctions against Chinese technology companies, their projected inefficacy, and policymakers' stated objectives, measuring the cost of these sanctions to US firms has important scholarly and policy implications.

Using an event study methodology, I examine how the additions of ZTE, Huawei, and a group of artificial intelligence and surveillance companies to the entity list in 2016 and 2019 affected the firm value of these companies' US suppliers.[20] I find that the addition of these firms to the entity list led to a statistically significant 220 basis points reduction in their US suppliers' risk-adjusted stock returns. In aggregate, the sanctions wiped out about nineteen billion USD in market capitalization.

Complementary cross-sectional analysis of firm-level characteristics shows that US companies that maintained deeper ties with the targeted entities were more negatively affected by the sanctions. This supports the direct connection between entity list sanctions and the firm value of US suppliers. A supplementary finding of the cross-sectional analysis is that, even after accounting for revenue dependence, firms that allocate more resources to research and development are more negatively affected. This suggests that the sanctions are particularly harmful to the United States' own technology sector.

Collectively, these findings contribute to sanctions scholarship by advancing our understanding of the costs that targeted trade sanctions against internationally active companies incur for sender-country firms. Further, US policymakers, whose stated objectives include ensuring the competitiveness of US businesses, can use these findings to more thoroughly weigh the tradeoffs of targeted sanctions imposition. More broadly, the findings will hopefully spur wider research and theoretical development on the cost of sanctions for firms.

This article also makes a methodological contribution to the study of sanctions. Though data aggregation is challenging in this context and care must be taken to address threats to inference, the event study methodology is generally well-suited for measuring the cost of targeted sanctions to publicly traded companies. Future research could employ a similar framework to study the cost of targeted sanctions in other contexts.

I proceed by laying out the theoretical reasons why targeted trade sanctions are expected to incur costs for sender-country firms. Next, I outline the research design employed in this article, including case selection, data, application of the event study methodology, and the firm-level cross-sectional analysis. I then discuss the article's main findings. Finally, I summarize the key implications of this research.

How targeted trade sanctions affect sender-country firms

The mechanics of sanctions are such that, even though the sender intends to coerce a foreign target to alter its behavior, the specific measures it takes are geared toward economic agents in its own country.[21] This leads to the "general rule that sanctions entail costs for the sender country."[22] Yet quantifying the cost of sanctions to the sender has remained elusive. Nonetheless, a small body of literature has measured sanctions' domestic economic ramifications in certain contexts.

Hufbauer et al. (1997), for example, estimate that US economic sanctions imposed in 1995 likely reduced exports to target countries by fifteen to nineteen billion USD. They further calculate that lost exports would have, all else equal, resulted in about 200,000 lost jobs. Performing a similar

[17]Elliott, 2002; Drezner, 2015.
[18]Drezner, 1999, 2019.
[19]Bureau of Industry and Security, n.d.b.
[20]Fama et al., 1969; MacKinlay, 1997; Kothari and Warner, 2007.
[21]Morgan and Bapat, 2003.
[22]Hufbauer et al., 2009, 108.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

study of US economic sanctions in 2000, DeRosa (2009) finds more modest and statistically insignificant lost exports of 2.8 billion USD. Looking at a narrower group of exports, Richardson (1993) estimates that US export controls on high-technology items deter about 5 percent of such exports.

More recently, Webb (2020) examines how US sanctions affect domestic firms' stock price volatility. Focusing on eight cases in which the United States imposed or threatened to impose sanctions against seven different countries, he finds that select firms with economic exposure to these countries endured a spike in volatility, while comparable firms without exposure to the sanctioned countries did not. The present article seeks to advance the literature on the cost of sanctions by drilling down on the mechanism behind targeted sanctions, the predominant contemporary form of economic sanctions.

From a theoretical standpoint, targeted sanctions are likely to generate direct and indirect costs for certain firms in the sender country. When a government imposes a targeted sanction against a foreign entity, especially a foreign firm, sender-country firms that have business relationships with the sanctioned entity are most directly affected. Often, these firms must forgo the revenue stream or supply channel associated with the target. The forgone business can be diverted to international competitors or moved in-house at the target, especially in the case of unilateral targeted sanctions. Targeted sanctions can also inflict reputational damage on sender-country firms' reliability as suppliers and customers.

The imposition of certain forms of targeted sanctions may protect sender-country firms from these costs. Most notably, compared to targeted trade sanctions, targeted financial sanctions are "easier to enforce, harder to evade, and may spur market-reinforcing effects."[23] This is especially true for US-led targeted financial sanctions, due to the dominant position of the US financial system.[24] In effect, international economic agents have few alternatives to dealing with the US financial system and pay a high price for flouting its rules. Targeted trade sanctions, particularly of the unilateral variety, are not expected to shelter sender-country firms from sanction costs. They are difficult to enforce, incentivize trade diversion to a sender's international competitors, and often upset international economic partnerships.

Use of the entity list constitutes a targeted trade sanction, and list members are usually sanctioned unilaterally by the US government. Members of the entity list are typically banned from receiving items subject to the US Export Administration Regulations (EAR), which includes virtually all items exported from the United States and many foreign exports containing American components.[25] Without a license from the commerce department, US firms that have supply relationships with an entity placed on the entity list must sever ties with that entity, which leads to a loss of revenue.[26] The sanctioned entity will, in turn, likely seek to establish supplier relationships with the US firms' international competitors or aim for greater self-sufficiency.

The End-user Review Committee (ERC), the interagency body that determines which foreign entities are added to the entity list, has flexible authority to sanction foreign entities without consulting other relevant stakeholders.[27] Because entity list sanctions are often sudden and unexpected, US firms that have business ties with sanctioned entities may also suffer reputational damage and business model uncertainty. All else equal, entity list sanctions should have an adverse impact on affected US suppliers' bottom line and international competitiveness. With these potential consequences in mind, I establish two theoretical expectations:

***Expectation 1**: Placing entities on the entity list that have US suppliers is likely to reduce those US suppliers' firm value.*

***Expectation 2**: Firms that have deeper ties with targeted entities are likely to be more negatively affected by entity list sanctions.*

[23]Elliott, 2002, 177.

[24]Cortright, Lopez, and Rogers, 2002; Drezner, 2015.

[25]See: Export Administration Regulations, 15 C.F.R. § 734.3, Items subject to the EAR; 15 C.F.R. § 744.16, Entity List; and 15 C.F.R. Supplement No. 4 to Part 744 – Entity List.

[26]The vast majority of entities on the entity list are subject to a licensing policy of "presumption of denial" for all items subject to the EAR. See: International Trade Administration, 2020.

[27]The ERC comprises representatives from the departments of commerce, state, energy, defense, and, if necessary, treasury. The chair of the ERC sits within the BIS. The BIS also manages the entity list and the regulations that govern its use.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

Research design

I test my expectations regarding the cost of entity list sanctions for US suppliers by applying the event study methodology, which has been deployed extensively in finance and business research to examine how unanticipated events affect the value of a firm.[28] As in this article, event studies typically examine the behavior of firms' stock returns around an event. The granularity of daily stock market returns, their reflection of current and future business prospects, and their quick adjustment to relevant news allow for plausible inference of the effects of specific events on the value of a firm. In the following sections, I discuss case selection, data, application of the event study methodology, and the firm-level, cross-sectional method I use to extend the analysis.

Cases

I examine three entity list cases. These are the ERC's decisions to sanction ZTE in March 2016, Huawei in May 2019, and a group of technology firms in October 2019 that the US government believes is facilitating the Chinese government's monitoring of Muslim minority groups in the Xinjiang Uighur Autonomous Region (XUAR).[29] Though the October 2019 notice applies to a variety of firms and entities affiliated with the XUAR public security bureau, relevant data for this study are only available for Dahua Technology, Hikvision, IFLYTEK, and Megvii Technology. For ease of reference, I refer to these entities as the "surveillance firms" and the associated event as the "surveillance event." While the surveillance firms were sanctioned for human rights reasons, ZTE and Huawei were sanctioned for contravening the US government's export and reexport policies related to Iran.

A little over two weeks after ZTE was placed on the entity list, the commerce department issued a temporary general license (TGL), which effectively established the status quo ante licensing policy for ZTE.[30] In effect, US businesses could carry on doing business with ZTE. The TGL remained in place for the duration of ZTE's time on the entity list. A more limited TGL was also issued for Huawei five calendar days after it was placed on the entity list.[31] The commerce department consistently extended the Huawei TGL until 17 August 2020.[32]

It is important to emphasize that there is clear separation between the sanctioning of these firms and the issuance of the TGLs, and there is no evidence the TGLs were anticipated at the time of sanctioning. This separation allows me to identify the immediate effect of the sanctions on US suppliers' market value. The TGLs, among other factors, preclude me from analyzing the long-run effects of the sanctions. In this sense, my findings should be interpreted as the effect on US suppliers in the absence of a subsequent TGL. In any case, event studies are known to be far less reliable for longer-term studies.[33] As such, most event studies explore very short-horizon effects.

I focus on targeted trade sanctions against Chinese technology companies because these sanctions are increasingly probable and highly consequential. In other words, these are important cases. One limitation of these cases is they are not representative of typical entities placed on the entity list. Adding multinational Chinese technology companies to the entity list is likely to have more significant ramifications for US suppliers than adding other probable targets. For this reason, my findings should be thought of as an upper bound on the effects of adding US firms' business partners to the entity list. Data availability also contributes to my case selection. Supply chain data that are integral to this study are difficult to aggregate because there are few requirements internationally to disclose such information. The presently investigated firms have comparatively robust data availability.

[28]Fama et al., 1969; MacKinlay, 1997; Kothari and Warner, 2007.

[29]Department of Commerce (2016b, 2019a, 2019c). Note that the federal register notice dates do not correspond to announcement dates. See table 1 and the online Event Window Appendix for additional information.

[30]Department of Commerce, 2016c.

[31]Department of Commerce, 2019b.

[32]Department of Commerce, 2020.

[33]Kothari and Warner, 2007.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

Table 1. Entity List Events

Target Firms	Announcement Date	Effective Date	No. US Suppliers
ZTE	7 March 2016	8 March 2016	63
Huawei	15 May 2019	16 May 2019	80
Surveillance	7 October 2019	9 October 2019	11

Data

I examine the stock return behavior of publicly traded US firms that had supply relationships with ZTE, Huawei, and the surveillance firms around the date of their addition to the entity list. I obtain data on the Chinese companies' US suppliers through Bloomberg's supply chain data.[34] Because I focus on stock returns, I can only use US firms that have actively traded stocks throughout the estimation period and event window that I utilize. My sample comprises 103 unique US firms and 154 supply relationships. Some companies are suppliers to more than one of the Chinese companies. I obtain data on these US firms' stock returns from the Center for Research in Security Prices (CRSP).[35] Table 1 summarizes the entity list events I investigate.

Event study methodology

I implement the event study in roughly three phases. First, I establish the event window and estimation period. Second, I derive and aggregate abnormal returns. Third, I test whether aggregate abnormal returns in the event window deviate significantly from 0. I discuss these phases in the following sections.

Event window[36] and estimation period

Though the three events have different calendar dates, they all share a common event timeline. I designate $t = 0$, the event date, as the first full day of trading following the entity list announcements. For the ZTE and Huawei events, $t = 0$ also corresponds to the effective date of the sanctions. For the surveillance event, $t = 0$ is the day in between the announcement and the effective date.

For the Huawei and surveillance events, setting $t = 0$ as the first full day of trading following the announcements is clearly most appropriate. The announcements for these sanctions were made late in the day, and the financial news summarizing stock market developments discusses the effects of the sanctions on $t = 0$, not $t = -1$, the announcement date. For ZTE, setting $t = 0$ as the first full day of trading after the announcement is less straightforward. Stories addressing the forthcoming ZTE sanctions were published early enough on the announcement day to affect trading. Nevertheless, I believe the first full day of trading following the announcement is most appropriate. For all three events, the preponderance of news covering the events occurred on $t = 0$. As illustrated in the Event Window Appendix, there was five to six times more news coverage for the events on $t = 0$ than on $t = -1$.

I use an event window of eleven days, which corresponds to $+/-5$ days surrounding $t = 0$. The event window is excluded from the estimation period, which is used to estimate expected returns. I establish an estimation period of 253 days prior to the beginning of the event window, which corresponds to the average number of trading days in a year.

As documented in the Event Window Appendix, I conduct a search to determine if there are other events in the event window, particularly on $t = 0$, that could have abnormally affected the US suppliers'

[34]Bloomberg, 2019.

[35]Center for Research in Security Prices, 2020. The stock returns are adjusted to reflect splits and dividends.

[36]The supplementary Event Window Appendix (available online) documents much of the evidence that supports my approach to establishing the event window. The Event Window Appendix draws on government documents, a structured news search in Nexis Uni, and a consistent review of three widely used resources for news affecting financial markets. All citations supporting my discussion in this section are contained in the Event Window Appendix.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

returns. Though I find some evidence of other events on $t = 0$ that could have abnormally affected the firms' returns, I believe these events pose minimal threat to assuming the principal drivers of abnormal returns among these sets of US firms on $t = 0$ were the entity list actions.[37] The reason for this is the other events' connections to the US suppliers under investigation are more indirect than the entity list sanctions.

To substantiate my assumption, I conduct a magnitude check that examines the US firms' stock price reactions to events that are very similar to the alternative events on $t = 0$. The effects of these alternative events, which are reported after the main results, are statistically insignificant and substantially smaller than the effects I find in the main results.

I also find evidence of economic and political events that could have abnormally affected the US firms' returns at other days in the event window. These include the release of Chinese economic data and developments in the US-China trade dispute. Additionally, the Huawei TGL was issued during the event window, toward the end of the day at $t = 2$, with effects felt at $t = 3$. The presence of these other events during the event window contributes, among other reasons, to my focus on the effects of the entity list sanctions on $t = 0$.

It is also important to evaluate whether the events were anticipated. To examine this, I conduct an extensive search in Nexis Uni for all English language news reports dealing with the entity list actions. These searches are documented in the Event Window Appendix. Though there is evidence of limited references to the sanctions and related events prior to the announcement dates, the events were not anticipated in such a way that the effects of the sanctions would have been reflected in the US firms' returns in the days leading up to their announcement.

Abnormal returns

Next, I find abnormal stock returns for the individual firms. Abnormal returns are residual returns from a model of expected returns. They are used to measure the effect of the event on a firm's returns. To generate expected returns, I employ the market model, which measures risk-adjusted returns using the correlation of firms' returns with a market index.[38] Using the market model, I estimate the relationship between the US firms' returns and returns to the Nasdaq Composite Index[39] over the estimation period:

$$R_{it} = \alpha_i + \beta_i R_{mt} + \epsilon_{it}, \tag{1}$$

Where R_{it} are returns for an individual firm i at time t, R_{mt} are returns to the Nasdaq Composite Index at time t, and ϵ_{it} is a disturbance term. The Nasdaq Composite Index is more appropriate for generating expected returns than the S&P 500 index because the Nasdaq, like my sample, is more heavily weighted toward technology and communications stocks than the S&P 500 Index. Using the Nasdaq also helps extract intra-industry cross-correlation that could pose a threat to inference.

Using the estimated intercept and slope coefficient from equation [1], I estimate abnormal returns (AR) for firm i at time t as:

$$AR_{it} = R_{it} - (\hat{\alpha}_i + \hat{\beta}_i R_{mt}) \tag{2}$$

In this article, I analyze the average abnormal return (AAR) for all firms during the event window, focusing on the AAR at $t = 0$. For a given day, the AAR is:

$$AAR_t = \frac{1}{n}\sum_{i=1}^{n} AR_{it} \tag{3}$$

Alternatively, the cumulative AAR ($CAAR$) enables analysis of the effect of events over the entire event window, which is useful for testing market efficiency and when $t = 0$ is not precisely

[37]The nature of these alternative events is discussed extensively in the Event Window Appendix.
[38]MacKinlay, 1997.
[39]Nasdaq, 2020.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

known.[40] I do not investigate *CAARs* because I believe $t = 0$ is well identified, I am less interested in testing market efficiency, and there are other events in the event window that could have abnormally affected returns.

Tests of significance

Finally, I conduct statistical tests to determine whether average abnormal returns differ significantly from 0. The range of parametric and non-parametric tests proposed in the event study literature is extensive.[41] The most appropriate tests for my research design are the parametric and non-parametric tests developed by Kolari and Pynnonen (2010, 2011). These tests are robust to event-induced volatility and cross-sectional correlation due to event-date clustering, the two most serious threats to inference in my study.[42]

Both tests make use of the sample average cross-sectional correlation ($\bar{r}$), which is about 0.09 in my study. Kolari and Pynnonen (2010) have shown that even small amounts of cross-sectional correlation can lead to substantial over-rejection of the Null hypothesis of no price effects. To generate $\bar{r}$, I calculate pairwise correlations within event blocs and over the estimation period. The $\bar{r}$ for the full sample is the average of all firms after figuring pairwise correlations within each event bloc.

Kolari and Pynnonen's parametric test uses $\bar{r}$ to adjust the parametric standardized cross-sectional test proposed by Boehmer, Musumeci, and Poulsen (1991) ("BMP test").[43] The BMP test is a hybrid test that performs a cross-sectional t-test on standardized *ARs*. In discussing the results, I refer to Kolari and Pynnonen's parametric test as the "Adjusted BMP" test. The non-parametric test, referred to as the Generalized Rank (GRANK) test, performs a Rank test on generalized standardized *ARs*, which are adjusted for $\bar{r}$.[44]

Because the GRANK test is non-parametric, an added benefit is that it is robust to non-normality of stock returns. Simulations have shown that, consistent with the Central Limit Theorem, averages of abnormal returns are approximately normally distributed with a sample size of about fifty.[45] Though my study eclipses this threshold, there may be some threat to the application of the Central Limit Theorem due to the non-independence of firm selection.

Firm-level cross-sectional analysis

If entity list sanctions directly affect US suppliers by shutting off revenue streams with sanctioned entities, then we would expect firms that have deeper ties with targets to be more negatively affected by the sanctions. I test this expectation by exploring how supplier dependence affects abnormal returns on $t = 0$. I measure dependence as the share of revenue a given US supplier derives from the sanctioned Chinese entities. I obtain the revenue dependence data from Bloomberg.[46]

Analytically, I regress AR_{i0} on revenue dependence and a series of firm-specific control variables that account for size, profitability, leverage, research and development (R&D) spending, and growth. To measure these characteristics, I use market capitalization, return on assets (ROA), the debt to equity ratio, R&D spending as a percent of sales, and the price to book ratio, respectively. I obtain the market capitalization data from CRSP and the rest of the firm-level data from S&P Global Market

[40]Kolari and Pynnonen, 2010.

[41]e.g., Patell, 1976; Brown and Warner, 1985; Boehmer, Musumeci, and Poulsen, 1991; Corrado and Zivney, 1992; Cowan, 1992.

[42]Though some have proposed cluster-robust standard error approaches adapted to typical event study test statistics to deal with cross-sectional correlation (Jaffe, 1974; Cameron, Gelbach, and Miller, 2011), these methods are not well-suited to event studies in which one or a few events affect multiple firms because they reduce the degrees of freedom too dramatically and discard valuable variance information (Kolari, Pape, and Pynnonen, 2018).

[43]Kolari and Pynnonen, 2010.

[44]Kolari and Pynnonen, 2011.

[45]Brown and Warner, 1985; Cowan, 1992.

[46]Bloomberg, 2019. Revenue dependence data are not available for twenty-four supply relationships. Therefore, my sample size for the firm-level cross-sectional analysis is 130.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

Intelligence's Compustat database.[47] To account for cross-sectional correlation of returns, I cluster standard errors by event-date.

Results

Figure 1 depicts the $AAR(t)$ for the sample of 154 US suppliers over the 11-day event window ($t[-5, 5]$). Table 2 contains the precise $AAR(t)$ over the same period, along with the statistical significance of the Adjusted BMP and GRANK tests.

At −2.2 percent, $AAR(0)$ is by far the largest move over the event window and is strongly statistically significant according to both the Adjusted BMP and GRANK tests. In absolute terms, it is 2.8 times the next highest move at $AAR(4)$, which is the only other day in the event window that displays some statistical significance, according to the GRANK test. In the days leading up to the event, there are much more moderate fluctuations. The mean absolute AAR over $t[-5, -1]$ is 0.30 percent, while mean AAR is −0.10 percent over the same period. The mean absolute AAR and mean AAR over $t[1, 5]$ are 0.52 percent and −0.33 percent, respectively.[48] Using AR_{i0} and market capitalization data for each firm, I derive the aggregate reduction in market value from the entity list sanctions as 18.6 billion USD. The results show strong support for my expectation that entity list sanctions against Chinese technology companies have an adverse impact on their US suppliers.

To further examine the robustness of the results, I conduct a placebo test, in which I replicate the event study using alternative entity list sanctions that would, from a theoretical standpoint, have no adverse effect on these US suppliers. Specifically, I swap the event dates to correspond to the closest entity list action, temporally, to each event that does not target a Chinese entity. For the ZTE firms, I use the entity list action on 23 February 2016, in which eight entities from the United Arab Emirates were added to the list.[49] For the Huawei firms, I use the entity list action on 26 September 2018, in which fourteen entities from Belarus, Iran, Russia, and Singapore were added to the list.[50] For the surveillance firms, I use the entity list action on 13 November 2019, in which twenty-two entities from thirteen different countries were added to the list.[51]

Additionally, to benchmark the magnitude of $AAR(0)$ in table 2, I conduct a magnitude check, in which I replicate the event study using alternative events that are very similar to the alternative events that occurred on $t = 0$ of the entity list event study and could be expected to negatively affect the sample of US firms.[52] For the ZTE firms, I set the magnitude check date to 4 January 2016, on which, according to the financial media, US equity markets fell due to the release of weak Chinese economic data. For the Huawei and surveillance firms, I set the magnitude check dates to 6 May 2019 and 23 August 2019, respectively. Both of these dates correspond to escalations in the US-China trade dispute. The financial media reported both escalations as hitting technology and industrial stocks with Chinese exposure particularly hard.[53]

Table 3 displays the results of the placebo test and magnitude check. $AAR(0)$ is negative in both tests but statistically insignificant and small in magnitude. None of the days in either event window differs significantly from zero. In terms of magnitude, $AAR(0)$ in the magnitude check is only about 1 percent

[47]CRSP, 2019; S&P Global Market Intelligence, 2020a. Five firms, which account for eight supply relationships, are not covered by Compustat. For these firms, I use the best available data from NetAdvantage (S&P Global Market Intelligence, 2020b).

[48]For additional reference, the mean AAR over the estimation period is 0.

[49]Department of Commerce, 2016a. Though the entity list action on 21 March 2016 is actually closer to the ZTE event by one day, it occurs around the time of the ZTE TGL, which could affect the placebo test.

[50]Department of Commerce, 2018. Though the 13 November 2019 action is actually closer to the Huawei event, I use this alternative placebo date in order to limit overlap with the surveillance event placebo. Additionally, the 13 November 2019 entity list action occurs around the time of a renewal of the Huawei TGL, which could affect the placebo test.

[51]Department of Commerce, 2019d.

[52]See the Event Window Appendix for a detailed description of the alternative events.

[53]Further detail on these magnitude check events is contained in the Event Window Appendix.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

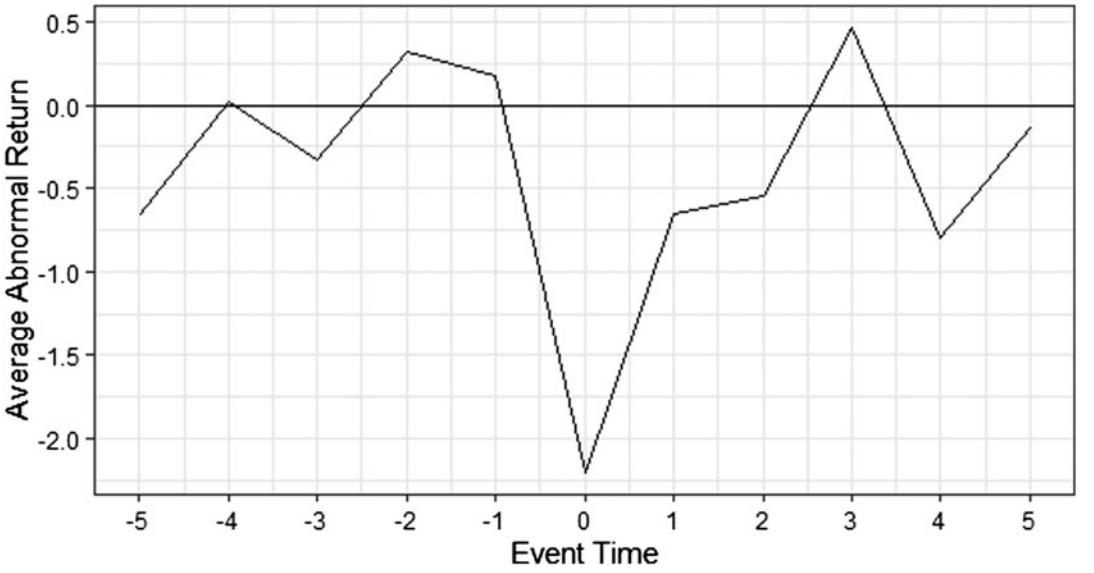

Figure 1. How Sanctions Against Chinese Technology Companies Affect Their US Suppliers.

Table 2. Average Abnormal Return Test Results

t	AAR	Adjusted BMP	GRANK
−5	−0.66		
−4	0.02		
−3	−0.33		
−2	0.32		
−1	0.17		
0	−2.20	**	***
1	−0.66		
2	−0.54		
3	0.47		
4	−0.79		*
5	−0.14		

Note: *p < 0.1; **p < 0.05; ***p < 0.01
The Adjusted BMP and GRANK tests are developed in Kolari and Pynnonen (2010) and (2011).

of the size of $AAR(0)$ in the entity list event study. These results reinforce that the −2.2 percent effect of the entity list sanctions on US suppliers is both economically and statistically significant. The results also reassure me that the entity list actions are the primary driver of abnormal returns at $t = 0$ in the entity list event study.

Turning to the firm-level cross-sectional analysis, table 4 captures the modeling results, in which I regress AR_{i0} on firm dependence and a series of controls.[54] Consistent with my expectations, the results show that there is a strong negative association between abnormal returns on $t = 0$ and the share of

[54]I generally use the natural log of all explanatory variables because they are skewed right in their raw form. I do not use the natural log of ROA because it contains negative values and is closer to normally distributed than the other variables. Additionally, I use cluster robust standard errors by event.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

Table 3. Placebo Test and Magnitude Check Test Results

	Placebo test			*Magnitude check*		
t	AAR	Adjusted BMP	GRANK	AAR	Adjusted BMP	GRANK
−5	0.31			0.31		
−4	0.36			0.41		
−3	0.62			0.17		
−2	0.17			0.16		
−1	−0.39			−0.18		
0	−0.24			−0.02		
1	0.11			−0.38		
2	0.15			−0.18		
3	−0.07			−0.40		
4	0.57			−0.45		
5	−0.22			−0.16		

Note: *p < 0.1; **p < 0.05; ***p < 0.01

Table 4. Firm-Level Cross-Sectional Modeling Results

Dependence	−0.926***
	(0.326)
Market Value	0.095
	(0.170)
Return on Assets	−0.009
	(0.013)
Debt to Equity	−0.321**
	(0.151)
R&D (% of sales)	−0.701***
	(0.242)
Price to Book	0.396
	(0.271)
Observations	130
Adjusted R^2	0.204

Note: *p < 0.1; **p < 0.05; ***p < 0.01

revenue a given US firm derives from the Chinese firms. Stated differently, US suppliers that are more dependent on the sanctioned entities are more negatively affected by the sanctions. This lends support to the argument that entity list sanctions directly affect US suppliers by cutting off revenue streams with sanctioned entities. The results do not speak to potentially powerful indirect effects of sanctions, such as reputational damage and additional business model uncertainty.

An interesting supplementary result captured in table 4 is the strong negative association between R&D spending as a percent of sales and entity list sanctions. After accounting for dependence, entity list sanctions still inflict greater harm on firms that spend more on R&D. This suggests that sanctions against multinational technology companies are particularly damaging to the United States' own tech sector.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

Conclusion

American firms bear notable costs from the sanctioning of Chinese technology companies. On average, I find that the US government's addition of select Chinese technology companies to the entity list reduced their American suppliers' market value by 2.2 percent on a risk-adjusted basis. The reduction likely occurs because the sanctions force US firms to sever ties with the sanctioned entity, thereby relinquishing a revenue stream to international competitors. This line of reasoning is supported by complementary firm-level cross-sectional analysis, which demonstrates that US firms that are more dependent on the sanctioned entities are more negatively affected by the sanctions. The US suppliers also likely suffer reputational damage for being unreliable, along with additional business model uncertainty.

Why does the US government impose sanctions that inflict steep costs on US companies? Many have found that US sanctions policymakers prioritize security issues over domestic business interests.[55] Mastanduno (1992) suggests that, in this issue area, relative neglect for domestic business interests owes to a powerful executive and the "absence of effective countervailing power from the US private sector."[56] This characterization certainly has relevance for entity list sanctions. The US government is typically required to seek formal public comment on regulations that affect US businesses. For national security reasons, the executive branch imposes entity list sanctions without seeking public comments, which makes it more difficult for the private sector to weigh in.

However, I also contend that the dearth of studies quantifying the domestic economic cost of sanctions prevents policymakers from thoroughly evaluating their tradeoffs. The shortage of such studies owes, in part, to the difficulty of measuring the cost of sanctions. The event study methodology I employ in this article is well-suited for capturing the effects of targeted trade sanctions on the public firms in the sender country that have business relationships with the sanctioned entities. The granularity of daily stock returns, their quick adjustment to relevant news, and their reflection of current and future business prospects, allow for plausible inference of the effects of sanctions.

The nature of the results presented in this article points to areas of potential future inquiry, particularly related to the longer run impact of sanctions. To this end, future research could explore whether the effects of sanctions persist over time and, relatedly, whether sanctions generate redistribution effects among groups of competing firms. Likewise, it would be valuable to examine whether sanctions prompt strategic or organizational changes for affected firms. Future research could also perform event studies akin to the one presented in this article to measure the effects of sanctions on firms in the sender country in other contexts.

Targeted sanctions are a valuable instrument of economic statecraft because, compared to more comprehensive sanctions and the use of force, they allow governments to signal resolve at a relatively low cost. As demonstrated, however, they are certainly not cost-free. This article expands our understanding of the cost of targeted sanctions, which is important for framing the domestic economic ramifications of these measures for relevant stakeholders. Sanctions policymakers, whose stated objectives involve ensuring the competitiveness of US businesses, can leverage the findings presented in this article to better calibrate sanctions policy.

Supplementary material. To view supplementary material for this article, please visit https://doi.org/10.1017/bap.2020.21.

Acknowledgements. I thank Charles Doran, David Steinberg, Dennis Yang, Pavithra Suryanarayan, Christopher Sands, Timothy Peterson, and the anonymous reviewers for their excellent comments.

Data Availability. The replication data for the analysis presented in this article are available at: Allen, Jeffrey. 2020. "Replication Data for: Do Targeted Trade Sanctions Against Chinese Technology Companies Affect US Firms? Evidence from an Event Study." https://doi.org/10.7910/DVN/NET3BA, Harvard Dataverse, V1.

References

Biersteker, Thomas J, Sue E. Eckert, and Marcos Tourinho. 2016. *Targeted Sanctions: The Impacts and Effectiveness of United Nations Action*. Cambridge: Cambridge University Press.

Bloomberg L.P. 2019. "Supply Chain Analysis <SPLC>." Retrieved from Bloomberg terminal, 10 December 2019.

[55]Mastanduno, 1992; Smith, 1984; Shambaugh, 1999.

[56]Mastanduno, 1992, 10.

Boehmer, Ekkehart, Jim Musumeci, and Annette B. Poulsen. 1991. "Event-Study Methodology Under Conditions of Event-Induced Variance." *Journal of Financial Economics* 30 (2): 253–72.

Brown, Stephen J., and Jerold B. Warner. 1985. "Using Daily Stock Returns: The Case of Event Studies." *Journal of Financial Economics* 14 (1): 3–31.

Bureau of Industry and Security. n.d.a. "Entity List." Accessed 1 September 2019. https://www.bis.doc.gov/index.php/policy-guidance/lists-of-parties-of-concern/entity-list.

Bureau of Industry and Security. n.d.b. "Mission Statement." Accessed 1 September 2019. https://www.bis.doc.gov/index.php/about-bis/mission-statement.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. 2011. "Robust inference with multiway clustering." *Journal of Business & Economic Statistics* 29: 238–49.

Center for Research in Security Prices. 2020. "Daily Stock File." The University of Chicago Booth School of Business. Retrieved from Wharton Research Data Service, 24 February 2020.

Corrado, Charles J, and Terry L. Zivney. 1992. "The Specification and Power of the Sign Test in Event Study Hypothesis Tests using Daily Stock Returns." *Journal of Financial and Quantitative Analysis* 27 (3): 465–78.

Cortright, David, and George A. Lopez. 2002. "Introduction: Assessing Smart Sanctions: Lessons from the 1990s." In *Smart Sanctions: Targeting Economic Statecraft*, edited by David Cortright and George Lopez, 1–22. New York: Rowman and Littlefield.

Cortright, David, George A. Lopez, and Elizabeth S. Rogers. 2002. "Targeted Financial Sanctions: Smart Sanctions that Do Work." In *Smart Sanctions: Targeting Economic Statecraft*, edited by David Cortright and George Lopez, 1–22. New York: Rowman and Littlefield.

Cowan, Arnold Richard. 1992. "Nonparametric Event Study Tests." *Review of Quantitative Finance and Accounting* 2: 343–58.

Department of Commerce. 1997. Final Rule. "Entity List." *Federal Register* 62, No. 22 (3 February 1997): 4910. https://www.govinfo.gov/content/pkg/FR-1997-02-03/pdf/97-2503.pdf.

Department of Commerce. 2008. Final Rule. "Authorization To Impose License Requirements for Exports or Reexports to Entities Acting Contrary to the National Security or Foreign Policy Interests of the United States." *Federal Register* 73, No. 163 (21 August 2008): 49311. https://www.govinfo.gov/content/pkg/FR-2008-08-21/pdf/E8-19102.pdf.

Department of Commerce. 2016a. Final Rule. "Addition of Certain Persons and Modification of Certain Entries to the Entity List; and Removal of Certain Persons From the Entity List." *Federal Register* 81, No. 35 (23 February 2016): 8825. https://www.govinfo.gov/content/pkg/FR-2016-02-23/pdf/2016-03745.pdf.

Department of Commerce. 2016b. Final Rule. "Additions to the Entity List." *Federal Register* 81, No. 45 (8 March 2016): 12004. https://www.govinfo.gov/content/pkg/FR-2016-03-08/pdf/2016-05104.pdf.

Department of Commerce. 2016c. Final Rule. "Temporary General License." *Federal Register* 81, No. 57 (24 March 2016): 15633. https://www.govinfo.gov/content/pkg/FR-2016-03-24/pdf/2016-06689.pdf.

Department of Commerce. 2018. Final Rule. "Addition of Certain Entities to the Entity List, Revision of an Entry on the Entity List and Removal of an Entity From the Entity List." *Federal Register* 83, No. 187 (26 September 2018): 48532. https://www.govinfo.gov/content/pkg/FR-2018-09-26/pdf/2018-20954.pdf.

Department of Commerce. 2019a. Final Rule. "Addition of Entities to the Entity List." *Federal Register* 84, No. 98 (21 May 2019): 22961. https://www.govinfo.gov/content/pkg/FR-2019-05-21/pdf/2019-10616.pdf.

Department of Commerce. 2019b. Final Rule. "Temporary General License." *Federal Register* 84, No. 99 (22 May 2019): 23468. https://www.govinfo.gov/content/pkg/FR-2019-05-22/pdf/2019-10829.pdf.

Department of Commerce. 2019c. Final Rule. "Addition of Certain Entities to the Entity List." *Federal Register* 84, No. 196 (9 October 2019): 54002. https://www.govinfo.gov/content/pkg/FR-2019-10-09/pdf/2019-22210.pdf.

Department of Commerce. 2019d. Final Rule. "Addition of Entities to the Entity List, Revision of an Entry on the Entity List, and Removal of Entities From the Entity List." *Federal Register* 84, No. 219 (13 November 2019): 61538. https://www.govinfo.gov/content/pkg/FR-2019-11-13/pdf/2019-24635.pdf.

Department of Commerce. 2020. Final Rule. "Addition of Huawei Non-US Affiliates to the Entity List, the Removal of Temporary General License, and Amendments to General Prohibition Three (Foreign-Produced Direct Product Rule)." *Federal Register* 85, No. 162 (17 August 2020): 51596. https://www.govinfo.gov/content/pkg/FR-2020-08-20/pdf/2020-18213.pdf.

DeRosa, Dean. 2009. "Impact of US Economic Sanctions on Trade." In Hufbauer, Gary Clyde, Jeffrey J. Schott, Kimberly Ann Elliott and Barbara Oegg. 2009. *Economic Sanctions Reconsidered*, 3rd edition. Washington, DC: Peterson Institute for International Economics. Appendix B.

Drezner, Daniel W. 1999. *The Sanctions Paradox*. Cambridge: Cambridge University Press.

Drezner, Daniel W. 2011. "Sanctions Sometimes Smart: Targeted Sanctions in Theory and Practice." *International Studies Review* 13 (1): 96–108.

Drezner, Daniel W. 2015. "Targeted Sanctions in a World of Global Finance." *International Interactions* 41 (4): 755–64.

Drezner, Daniel W. 2019. "Economic Statecraft in the Age of Trump." *The Washington Quarterly* 42 (3): 7–24.

Early, Bryan R., and Marcus Schulzke. 2019. "Still Unjust, just in Different Ways: How Targeted Sanctions Fall Short of Just War Theory's Principles." *International Studies Review* 21 (1): 57–80.

Elliott, Kimberly Ann. 2002. "Analyzing the Effects of Targeted Sanctions." In *Smart Sanctions: Targeting Economic Statecraft*, edited by David Cortright and George Lopez, 1–22. New York: Rowman and Littlefield.

https://doi.org/10.1017/bap.2020.21 Published online by Cambridge University Press

Fama, Eugene F., Lawrence Fisher, Michael C. Jensen, and Richard Roll. 1969. "The Adjustment of Stock Prices to New Information." *International Economic Review*. 10, no. 1 (February): 1–21.
Hufbauer, Gary, Kimberly Ann Elliott, Tess Cyrus, and Elizabeth Winston. 1997. "US Economic Sanctions: Their Impact on Trade, Jobs, and Wages." Working Paper. Institute for International Economics. https://www.piie.com/publications/working-papers/us-economic-sanctions-their-impact-trade-jobs-and-wages.
Hufbauer, Gary Clyde, Jeffrey J. Schott, Kimberly Ann Elliott, and Barbara Oegg. 2009. *Economic Sanctions Reconsidered*. 3rd edition. Washington, DC: Peterson Institute for International Economics.
International Trade Administration. 2020. "Consolidated Screening List." Retrieved from trade.gov, 15 January 2020. https://www.trade.gov/consolidated-screening-list.
Jaffe, Jeffrey F. 1974. "Special information and insider trading." *Journal of Business* 47: 410–28.
Kolari, James W., and Seppo Pynnonen. 2010. "Event Study Testing with Cross-Sectional Correlation of Abnormal Returns." *The Review of Financial Studies* 23, no. 11 (November): 3996–4025.
Kolari, James W., and Seppo Pynnonen. 2011. "Nonparametric rank tests for event studies." *Journal of Empirical Finance* 18: 953–71.
Kolari, James W., Bernd Pape, and Seppo Pynnonen. 2018. "Event Study Testing with Cross-Sectional Correlation Due to Partially Overlapping Event Windows." Mays Business School Research Paper No. 3167271.
Kothari, S.P., and Jerold B. Warner. 2007. "Econometrics of Event Studies." In *Handbook of Empirical Corporate Finance*, edited by B. Espen Eckbo. Amsterdam: Elsevier/North-Holland.
MacKinlay, A. Craig. 1997. "Event Studies in Economics and Finance." *Journal of Economic Literature* 35, no. 1 (March): 13–39.
Mastanduno, Michael. 1992. *Economic Containment*. Ithaca, NY: Cornell University Press.
Morgan, T. Clifton, and Navin A. Bapat. 2003. "Imposing Sanctions: States, Firms, and Economic Coercion." *International Studies Review* 5 (4): 65–79.
Morgan, T. Clifton, Navin Bapat, and Yoshiharu Kobayashi. 2014. "Threat and Imposition of Economic Sanctions 1945–2005." *Conflict Management and Peace Science* 31 (5): 541–58.
Muñiz, Manuel. 2019. "The Coming Technological Cold War." *Project Syndicate*. https://www.project-syndicate.org/commentary/us-china-technology-cold-war-by-manuel-muniz-2019-04?barrier=accesspaylog.
Nasdaq. 2020. Global Index Data Service. Yahoo! Finance, 24 February 2020.
Pape, Robert A. 1997. "Why Economic Sanctions Do Not Work." *International Security* 22, no. 2 (fall): 90–136.
Patell, J. M. 1976. "Corporate forecasts of earnings per share and stock price behavior." *Journal of Accounting Research* 14 (2): 246–76.
Richardson, J.D. 1993. "Sizing Up US Export Disincentives." Washington, DC: Institute for International Economics.
Segal, Adam. 2018. "Year in Review: Huawei and the Technology Cold War." New York: Council on Foreign Relations. https://www.cfr.org/blog/year-review-huawei-and-technology-cold-war.
Segal, Adam. 2019. "Year in Review 2019: The US-China Tech Cold War Deepens and Expands." New York: Council on Foreign Relations. https://www.cfr.org/blog/year-review-2019-us-china-tech-cold-war-deepens-and-expands.
Shambaugh, George E. 1999. *States, Firms, and Power*. Albany, NY: State University of New York Press.
Smith, Gordon. 1984. *The Politics of East-West Trade*. Boulder, CO: Westview Press.
Standard & Poor's (S&P) Global Market Intelligence. 2020a. Compustat. Retrieved from Wharton Research Data Service, 11 September 2020.
Standard & Poor's (S&P) Global Market Intelligence. 2020b. NetAdvantage. Retrieved from Wharton Research Data Service, 13 September 2020.
Triolo, Paul, Kevin Allison, and Clarise Brown. 2018. "The Geopolitics of 5G." Eurasia Group White Paper. November 15.
United States. 2017. "National Security Strategy of the United States of America." Washington, DC: President of the United States.
United States. 2018. "Summary of the 2018 National Defense Strategy of the United States of America." Washington, DC: Department of Defense.
Webb, Clayton. 2020. "Re-examining the Costs of Sanctions and Sanctions Threats Using Stock Market Data." *International Interactions* 46 (5): 749–77.

Cite this article: Allen JS (2021). Do Targeted Trade Sanctions Against Chinese Technology Companies Affect US Firms? Evidence from an Event Study. *Business and Politics* **23**, 330–343. https://doi.org/10.1017/bap.2020.21