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ABSTRACT

The US trade war against China in 2018–2019 can either enhance or diminish the US soft power in China, depending on whether it is recognized as legitimate by Chinese citizens. We study how the viewership of US movies—an important element of the US soft power—is affected by the trade war, utilizing variations across Chinese cities in the exposure to the Trump tariffs. We find a significant reduction in US movie revenue in regions more exposed to the Trump tariffs, but no corresponding reduction in the consumption of non-US movies. This is corroborated by a decline in online search for US movies, US tourist destinations, and US branded sports shoes. The aversion to US movies appears to persist at least to 2021. The effect is somewhat milder for more affluent people.

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1 Introduction

It has been long recognized that the popularity of US movies in other countries constitutes a form of US soft power in influencing world affairs, extending and complementing its "hard powers" in military and economic might. In the pioneering work on the subject, Nye (1990, 2004) defines "soft power" as a country's "ability to attract or co-opt others to get desired outcomes rather than coercing with threats or inducing with payments" and specifically mentions the influence of Hollywood movies as an element of the US soft power. Indeed, Hollywood has been called "the little State Department," and US movies have been called "120,000 American ambassadors" by Hollywood executives for spreading US values or US interest (Swann, 1991; Moody, 2017). According to data from Internet Movie Database (IMDb) and Motion Picture Association of America (MPAA), US movies' share of global movie revenue was 70% in 2017, much higher than either the US share in world GDP (about 20%) or the US share in world military expenditure (about 28%). US movies tend to project US armed forces, US intelligence services, and US legal system in a positive light and help to increase an appreciation for the American way of life and American ways of looking at the world.

The trade war, launched by President Trump in March 2018 and escalated twice more in his presidency, greatly increased US tariffs on imports from China to a level last seen under the Smoot-Hawley Tariff Act before World War II. It was justified by the Trump administration as a penalty for China's unfair trade policies, theft of US intellectual property rights, and other deviations from international rules. As such, the trade war can either enhance or diminish the US soft power in China. If citizens in the target country regard it as a righteous and justified penalty for their government's unfair policies and other transgressions, they may respond to the trade war by increasing their appreciation for US movies.[1] Conversely, if they regard the Trump tariffs as a bullying tactic to advance narrow US commercial interests, inconsistent with the US brand image as a defender of a rules-based world economic order, the tariffs can backfire and reduce their demand for US movies. The viewership of US movies thus provides a concrete channel for understanding how an exercise of US economic power can affect its ability to project its soft power. Yet, we are not aware of any systematic study on such an impact.

The same setting also affords us an opportunity to study a connection between service and goods trade not previously documented in the literature. The United States always runs a surplus in movie trade with China (and indeed likely with most other trading

[1] When the United States imposed an economic and visa sanction on the officials of the Venezuela's Maduro government in 2014 for human rights violation, the share of US movies in Venezuela's total movie revenue increased from 87% in 2013 to 98% in 2015 (Authors' calculation based on IMDb data).

partners). The surplus is lopsided as the US rarely imports movies from China. As movie trade is a part of service trade, it is not counted in standard merchandized trade statistics. There is no study that we are aware of on a possible spillover effect from a country's tariff increases on imported goods to its service exports.

This paper aims to fill these voids by examining how the viewership of US movies in China has been affected by the trade war. The identification of the effect exploits regional variations in the exposure to the Trump tariff increases. As some regions in China may rely more on industries that are heavily exposed to US tariff increases, the same Trump tariffs translate into different impacts in different Chinese regions. We will study whether greater exposure to the Trump tariffs translates into a greater awareness of the trade war and whether the viewership of US movies in the more exposed regions has gone up or down relative to other regions in response to the trade war. We will control the trade war's possible income effect on general movie consumption. We work with a data set that records revenue at the movie-theater-time of the day level and use revenue accrued to US movies as a proxy for viewership of US movies. The evidence shows a steeper fall in US movie revenue in the more tariff-exposed regions relative to the pre-trade war level. As it turns out, a World Trade Organization panel consisting of legal experts from countries other than China and the United States ruled in 2019 that the Trump tariff increases on Chinese goods are illegal.

Omitted variables are a possible threat to our identification. One possible omitted variable is an income effect. The Chinese regions more exposed to the Trump tariffs may also experience a relative decline in local income. However, the income effect on general movie consumption appears weak, and the decrease in US movie revenue goes beyond the income effect. Indeed, the overall movie revenue and revenue for non-US foreign movies do not go down in regions with a greater exposure to US tariffs.

Another possible omitted variable is the role of the Chinese government. The government could in theory limit the number of US movies imported during the trade war or direct state-owned theaters to reduce the showing times of US movies. We find that the government role is relatively limited in this context. First, the Chinese imports of US movies in 2018 were in fact higher than any of the Obama years. Second, we examine if there are more government media commentaries on the trade war in more tariff exposed regions and find that is not to be the case. Third, we check whether the fall in revenue is more concentrated in revenue-sharing US movies (for which the US movie studios would share the revenue loss) than in flat-fee US movies (for which Chinese importers bear all the loss) and find that it is not the case. These patterns suggest that the fall in US movie revenue during the trade war is driven primarily by private individuals' choices.

We perform several checks on the validity of our key regressor—local exposure to the Trump tariffs—which is essentially a shift-share or Bartik instrument. For example, we confirm that the regional variations in the exposure to the Trump tariffs are not dominated by one or two industries. In fact, our results are robust if we exclude the top three industries with the highest Rotemberg weights from the construction of our tariff exposure mesure. We also consider three other ways to construct the local exposure to the Trump tariffs, control for the Chinese retaliatory tariffs, and find our results to be robust.

We conduct several extensions. For example, movies come in different genres (actions, animation, and others). The international audience may associate Hollywood action movies with the United States more than they do animations (Lichtenfeld, 2007). If the change in the viewership of US movies is driven by a change in the attitude towards the United States, one may expect the effect of the trade war to be stronger for action movies than for animation. This provides another opportunity to check the validity of the inference. We find this indeed to be the case.

We complement the movie revenue results with an investigation of private citizens' search activity on Baidu, the dominant Chinese-language search engine similar to Google in the United States. We find that the Baidu search intensity for the trade war or its synonyms is higher in more tariff-exposed regions, suggesting that the residents in these regions are more aware of or concerned with the trade war. The Baidu search intensities for US movies and US tourism destinations are also systematically lower in these regions. These results corroborate the interpretation that the fallen US movie revenue is driven by a reduced appetite by private Chinese citizens.

Some indirect evidence indicates that the trade war has a somewhat smaller negative effect on the attitude of more affluent Chinese. First, when we disaggregate movie theaters by the average ticket prices they charge (which presumably reflect the quality of seats, screens, and other amenities), we find a somewhat smaller reduction in US movie revenue in fancier theaters than in ones charging a lower average price. Second, the Internet search for US colleges in more tariff exposed regions, while still negatively affected by the trade war, exhibits a somewhat milder reduction than those for either US movies or US branded sports shoes. To the extent that more affluent households are more likely to use fancier theaters or to send their children for education in the United States, these patterns suggest a smaller downturn in such households' attitudes to the United States than their compatriots during the trade war.

The paper makes five main contributions. First, it provides the first systematic evidence on how the US ability to project its soft power is affected by an exercise of its economic hard power (tariffs). While Nye (1990, 2004) pioneered the influential concept of soft power and provided a qualitative reasoning for US movies as a key element of the US soft power, our paper helps to elevate the discussion to a rigorous data-based assessment. With a gravity model of trade, Rose (2015 and 2019) show that the soft power affects trade. In particular, the popularity of Country 1 in Country 2 (such as the percent of the survey respondents in Country 2 holding a favorable view of Country 1) affects how much Country 1 is able to export to Country 2. In comparison, we examine a reverse question: how a change in the tariffs on manufacturing imports by the United States affects its popularity in the target country. There does not seem to be an answer in the existing literature. A related recent literature has studied the political implications of international trade. Notable contributions include Blanchard, Bown and Chor (2019), Fetzer and Schwarz (2021) and Brutger, Chaudoin and Kagan (2021) on the impact of the trade war on the US electoral politics, and Kleinman, Liu and Redding (2020) on the impact of international trade on political alignment across countries. There is a relative paucity of studies on the impact of the trade war on the Chinese domestic political economy. A notable exception is Steinberg and Tan (2019) who, through surveys of Chinese respondents, find that the Trump tariffs have led many Chinese to become less supportive of trade liberalization. Our novelties are to use Chinese viewership of US movies as an objective gauge of the attitude towards the United States (in comparison to subjective self-reported attitude in surveys) and to use exogenous regional variations in the exposure to the Trump tariffs to estimate a causal effect of the trade war on people's attitudes towards the US soft power.

Second, we add a fresh new angle—the effect of a country's import tariffs on its service exports—to a literature on the consequences of the trade war. This literature so far tends to focus on the effects on local prices, jobs, and income, as an excellent survey by Fajgelbaum and Khandelwal (2021) reveals. Notable contributions include Amiti, Redding and Weinstein (2019), Amiti, Redding and Weinstein (2020), Fajgelbaum, Goldberg, Kennedy and Khandelwal (2020), Waugh (2019), Flaaen, Hortaçsu and Tintelnot (2020), and Cavallo, Gopinath, Neiman and Tang (2021). They generally find that the trade war has led to an increase in US prices and a decline in US real income. Huang, Lin, Liu and Tang (2019) document that those publicly listed US firms that depend relatively more on either exports to or imports from China experience a greater relative decline in their stock prices. There are fewer studies on the effect of the trade war on China. A notable exception is Chor and Li (2021), who find a significantly negative effect on Chinese local incomes using nighttime light data. Interestingly, they find no significant effect of the Chinese retaliatory tariffs on local incomes (which we will also confirm). Other papers find a relative decline in new firm entry (Cui and Li, 2021) or a decline in firm hiring (He, Mau and Xu, 2021) in industries more exposed to the Trump tariffs. While Chinese exports to the United States fell during the trade war, its exports to the European Union and other markets did not increase much (Jiao, Liu, Tian and Wang, 2021). No study has examined a possible spillover effect from tariffs on manufacturing imports to service exports. Our paper appears to be the first to document such a spillover effect. While we focus on US movie exports in the main analysis, the logic may apply to other export items whose country of origin is salient. Indeed, we will report some results based on online search activities that the Chinese citizens in the more tariff-exposed regions also exhibit a greater relative decline in their interest in US tourist destinations, US college education, and US branded sports goods.

Third, we complement a literature on the effects of media exposure on people's behavior by examining a reverse question—how people's voluntary choice on exposure to the values espoused in foreign movies is affected by foreign economic policies. Gentzkow (2006), DellaVigna and Kaplan (2007), and Enikolopov, Petrova and Zhuravskaya (2011) show media exposure affects electoral outcomes, and DellaVigna, Enikolopov, Mironova, Petrova and Zhuravskaya (2014) show how nationalistic media can fan animosity towards another country. Gentzkow and Shapiro (2010) and Qian and Yanagizawa-Drott (2017) show a bias is present in local media and the US newspapers' coverage of human rights abuses by foreign governments, respectively. In comparison, while US movies could affect foreign viewers' attitude towards the US, we examine how their movie choices themselves are affected by US trade policy.

Fourth, we enrich a literature on consumer boycott of foreign products in a number of interesting and important ways. Heilmann (2016) studies the American boycott of French products in 2003 and the Chinese boycott of Japanese goods in 2012. Michaels and Zhi (2010) and Chavis and Leslie (2009) find that the US-France dispute over the Iraq war in 2003 reduced American firms' and consumers' purchases of French products. Fisman, Hamao and Wang (2014) and Barwick, Li, Wallace and Weiss (2019) find political tensions between China and Japan in 2012 depressed stock prices in both countries and reduced the sale of Japanese cars in China, respectively. Hong, Hu, Prieger and Zhu (2011) and Fuchs and Klann (2013) find political tensions with China are associated with lower subsequent sales of a country's products to China. It is generally challenging to identify a causal effect and rule out cleanly all confounding factors in cross-country or cross-firm comparisons. For example, while a lower sale of foreign products may reflect buyers' boycott, it could in principle also reflect either a supplier response or a selective government intervention in the buyer country. Our paper makes several advances in these regards. In terms of the outcome variable under study, US movie viewership is the only one in the literature that is linked to soft power, which can have an additional spillover effect through a change in foreign exposure to US values and US ways of life. In terms of methodology, our use of a shift-share instrument provides a better identification of a causal effect than the cross-country or cross-firm comparisons.[2] In addition, our disaggregated data on movie viewership also allows us to more directly investigate and control for possible confounding factors. For example, we rule out a government effect in driving our result as we find no regional variations in either local government media reports or local policies on theater operation. We can also rule out the movie supplier effect and focus on consumer choices in our context. The disaggregated data in both search results and movie theater types help us to examine possibly different behaviors by more and less affluent households in the boycott context. Finally, we find the effect to be longer lasting than the episodes of the goods boycott reported in the literature.

Fifth, we offer insight on measuring attitude toward a foreign country which is relevant for the literature on trust and economic exchanges. Guiso, Sapienza and Zingales (2009) find that bilateral trust (based on surveys) promotes bilateral trade. Nunn and Wantchekon (2011) find that historical slave trade is a determinant of the current level of mistrust (also based on surveys) in Africa. Lan and Li (2015) study the effects of trade openness on nationalism in China. Other interesting papers include Disdier and Mayer (2007), Felbermayr and Toubal (2010), and Bao, Liu, Qiu and Zhu (2020). Many papers in this literature rely on survey responses to gauge people's trust in strangers or attitude towards other countries, which could be subject to a "demand characteristics bias" (guessing what the survey designer wants) or a "social desirability bias" (giving a response that conforms to social expectations), influenced by the framing of the survey questions, and available only infrequently. To the extent that an appreciation of foreign movies is associated with a trust in the country that produces them,[3] we study how a foreign country's commercial policy (e.g., tariff increase) affects the attitude toward that country. The measure based on movie viewership is objectively observed, potentially free of problems in survey responses, and is available in multiple regions at a high frequency. By comparing audience responses to US movies across theaters charging different average prices, we also obtain clues to possible differences among households of varying affluence levels. As such, it provides a useful complement to typical survey-based measures. The new measure can in principle be used in other research context.

[2] Rather than a cross-country comparison, Pandya and Venkatesan (2016) compare the US supermarket sale of French-sounding brands that are not actually from France (e.g., TRESemmé shampoo and Raison d'Être beer) to that of other US branded products.

[3] Using Pew Reserarh survey data on a country's attitude towards the US for 26 countries from 2017 to 2019, we find that changes in US movies as a share of local movie revenue and changes in a country's favorable views towards the United States are positively correlated.

The paper is organized as follows. In Section 2, we provide background information on both US movies in China and the trade war. In Section 3, after describing the basic specification and the data, we present both baseline results and robustness checks. In Section 4, we provide complementary evidence from Baidu search results. In Section 5, we report a number of extensions. In Section 6, we conclude.

2 Background Information

2.1 US Movies: Double Features

US movies have double properties: they are produced as a form of for-profit entertainment, but they are also recognized as a powerful tool to advertise American values, goods, and foreign policies (Bennett, 2012). Nye (2004), who has pioneered the concept of "soft power," considers the movies an essential element of the US soft power. While the incorporation of US values and interest in a movie plot is often an incidental choice by movie directors and screenwriters, the US government also actively seeks to influence the production and editing of films. Robb (2011) and Jenkins (2016) document extensive involvement of the Pentagon and the CIA in movies production and content edition, with a free supply of otherwise costly or unattainable equipment (e.g., tanks, battleships, and fighter planes), filming locations (e.g., military bases), and personnel as an inducement.[4]

The US State Department also seeks to use Hollywood to support American foreign policy aims. For example, the US embassy and consulates in China regularly host movie nights and invite college students to attend. Past screenings at movie nights at the US Consulate General in Shanghai included Selma (about the civil rights movement in the United States), Swing Vote (a comedy-drama about US presidential elections), and Pursuit of Happiness (a biographical drama about African American entrepreneur, stockbroker, and motivational speaker Chris Gardner).[5]

[4]Under the US Department of Defense, the Entertainment Liaison Office can coordinate the supply of tanks, ships, fighter jets, other military equipment, bases, and sometimes troops for use in movies that they approve. These subsidies are used to induce the production of certain content. The Central Intelligence Agency also has a program to work with movie studios. A well-documented example is the making of Zero Dark Thirty, an Oscar and Golden Globe winner, which has received advice and cooperation from the CIA and has been criticized for portraying the use of torture as yielding useful intelligence. Both have succeeded in getting movie directors to change their scripts to avoid painting the armed forces or US government in a negative light. The incentives constitute a form of "industrial policy" for certain movie content.

[5]Linda Jewell, the US Ambassador to Ecuador, explained the importance of film screenings for cultural diplomacy by noting that "a well-selected series of independent and less-commercialized US films would be a powerful way to refute misconceptions and stereotypes about the US" (WikiLeaks, 22 December 2005, as cited by Moody (2017)).

The US movie industry gained its global dominance after World War I (Giannetti and Eyman, 2010). According to Figure 1, US movies account for between 64% and 73% of global movie revenue from 2012 to 2019. As Figure 1 shows, this share is much higher than the US shares in the global totals of outward FDI, GDP, exports, imports, or military expenditure. In particular, the percentage in global movie revenue is more than twice higher than the US share in global military spending. This comparison is striking given that the US defense budget in 2019 is greater than the next ten countries combined that year. The US movie share in the world peaked in 2016, the last year before Trump assumed the US presidency.

The United States has a comparative advantage in modern services and has always run a surplus in service trade against the world in general and against China in particular. Modern services include movies in addition to finance, accounting, auditing, business consulting, and others. According to data from US Census Bureau, US service exports to China nearly doubled from 33.0 billion dollars in 2012 to 59.4 billion dollars in 2019. While US service imports from China also increased, they were comparatively low at 19.8 billion dollars in 2019. The US surplus in service trade, at 39.6 billion dollars in 2019, contrasts sharply with its deficit in goods trade.

Exports of movies account for a substantial share of total service exports for the US. The movie exports as a share of total US service exports to China rose from 3.8% in 2012 to 5.8% in 2017 but fell during the trade war period to 4.9% in 2019. Since the United States seldom imports movies from China, movie trade is a considerable contributor to the US surplus in the overall services trade with China. Thus our paper also helps us understand how the Trump trade war in the manufacturing space affects US service exports.

2.2 US Movies: Entering China

Foreign movies can be imported into China on either a revenue-sharing or a flat-fee basis.[6] The revenue sharing arrangement usually specifies that 25% of the box office revenue goes to foreign movie studios. The revenue-sharing movies are typically major foreign titles and subject to an annual import quota. Since 2012, this quota has been set at 34 titles, used overwhelmingly to import Hollywood movies. In comparison, with a flat-fee import, the Chinese importer acquires an exclusive right to distribute the movie in the country and does not share any profit (or loss) with the original movie studio. There is no binding quota for flat-fee movies.[7] On the other hand, as the flat fee tends to be

[6] See Ho, Rysman and Wang (2020) for an analysis on the effects of the quota system on welfare.

[7] See `http://media.people.com.cn/n1/2018/0620/c40606-30068986.html` (in Chinese).

low, generally below a half million dollars, the flat-fee arrangement is almost never used to import major Hollywood movies. The imports of flat-fee movie have increased over time, including during the Trump years.[8] Under the Chinese regulation, only two state-owned firms, the China Film Group and Huaxia Film Group, can import and distribute foreign movies. Importantly for this paper, there was no change in either the quotas or the distribution system during the trade war.

Figure 2 presents the time series plots of the number of imported US and other foreign movies over 2012–2019, for the revenue-sharing and flat-fee titles, respectively. Notably, as Panel (a) shows, the Chinese imports of the revenue-sharing US movies were above the quota (for all foreign movies) in 2017 and 2018 and were higher than any of the Obama years, but fell back to the quota in 2019. Panel (b) shows no systematic change in the imports of flat-fee US movies during the trade war (though there is a noticeable increase in flat-fee non-US foreign movies).

We check if there is any decline in the quality of imported US movies during the trade war which might contribute to a decline in the Chinese viewership. We use the box office revenue in North America and the average user rating from IMDb.com as two measures of a movie's attractiveness to moviegoers. For comparison, we also calculate the averages of the two measures for the top 100 US movies in North America box office.

As Figure 3 shows, there is no evidence of a quality decline in the US movies imported to China in 2018 and 2019 relative to earlier years. If anything, the movies during the trade war happen to be more popular than before. We also note that the revenue-sharing US movies have a higher North America box office sale and a better IMDb user rating than the flat-fee imports on average. This confirms the earlier statement that revenue-sharing movies tend to be blockbuster movies by major US studios. In contrast, flat-fee US movies are more likely to be smaller productions by independent producers (Dresden, 2018).

Imported movies are subject to neither tariffs at the border nor any special tax inside the border. Importantly, no new tariffs or taxes on US movies were introduced during the trade war years. Appendix Table A1 reports the average ticket prices for US and Chinese movies. The prices are close to each other. The average price for US movies is slightly higher than their Chinese counterparts, mostly because the US movies are somewhat more heavily advertised and are often a bit longer in viewing times. Importantly, the slightly higher average price of US movies is true in both 2017 (i.e., before the trade war) and 2019 (during the trade war). We separately compute the average price for prime sessions (afternoon and evening sessions on weekends or public holidays and Friday

[8] See https://www.ghjadvisors.com/blog/history-of-china-import-film-quota-and-revenue-sharing-remittance and https://new.qq.com/rain/a/20211126a0afjd00 (in Chinese).

evenings or evenings before major holidays). These patterns confirm an absence of extra tariff at the border or other special taxes on US movies during the trade war.

While foreign movies are imported by two state-owned importers, we investigate the prevalence of state-owned movie theaters. Using the firm registry data of the Chinese State Administration for Market Regulation in 2015, we find that 98.8% of all movie theaters are owned and operated by non-state-owned companies. In other words, state ownership is negligible, and an overwhelming majority of theaters are profit-motivated.

We depict the spatial variation in per capita movie expenditure across Chinese cities in Appendix Figure A1, with US and Chinese movies in 2017 in Panels (a) and (b), respectively, and their corresponding growth from 2017 to 2019 in Panels (c) and (d), respectively. In the lower panel, we use yellow and orange colors to denote big and small positive growth, respectively, and blue to denote a revenue decline. We see a decline in US movie revenue in many cities during 2017-2019 (Panel c), even though the Chinese movie revenue grew in the same cities during the same period. A comparison of the two figures in the lower panel hints that the reduction in US movie revenue is not because people have reduced movie consumption during this period (see Section 3.4).

2.3 The Trade War: Uneven Exposure across Chinese Regions

In March 2018, President Donald J. Trump asked the United States Trade Representative to raise the tariffs on $50–60 billion of Chinese exports, citing Section 301 of the Trade Act of 1974.[9] This initial round of tariffs took effect in July and August of 2018, respectively. In further escalations of the conflict, two additional rounds of tariffs, covering $200 billion and $272 billion of Chinese goods, were announced in July 2018 and August 2019, respectively. These additional rounds of tariffs, except for a subset ($160 billion) from the third round, came into effect in September 2018 and June and September of 2019, respectively.

With the extra tariffs ranging from 10% to 25%, covering $362 billion of the US imports from China (Bown and Kolb, 2021), the average US tariff on Chinese goods had increased sharply from 3.1% in January 2018 to 21.0% by December 2019, which is about the level under the Smoot-Hawley Tariff Act in the 1930s. In comparison, the average US tariff on imports from other countries had increased moderately from 2.2% to 3.0% during the same period (Bown, 2021). As discussed earlier, the Trump tariff increases were

[9] In the baseline analysis, we focus on the multiple tariff increases imposed on China by the US under Section 301 of the Trade Act of 1974. We exclude those that are also applicable to other US trade partners, such as the ones on solar panels and washing machines applied under Section 201 of the Trade Act of 1974 and the steel and aluminum tariffs imposed under Section 232 of the Trade Expansion Act of 1962. However, as a robustness check, we use an alternative measure that includes all these tariffs as well and find the results essentially unchanged. These results are reported in Table 7.

eventually ruled illegal by the World Trade Organization in 2020.

The Trump tariff increases are uneven across industries (see Appendix Table A2). They are most substantial in "furniture" and "general-purpose machinery," and comparatively modest in "medicines" and "smelting and pressing of non-ferrous metals." The tariff increases also came into effect at different times for different industries. In sum, there is substantial variation in the tariff increases across both industries and time periods. Because the new tariffs introduced by President Trump are added to the existing ones, we will use "Trump tariffs" and "Trump tariff increases" interchangeably.

We can convert industry-level variations in the exposure to Trump tariffs to city-level variations, using the industry composition of each city's employment.[10] As an illustration, for Foshan, a coastal city in Guangdong Province, 44.3% of its employment are in industries with the biggest increase US tariffs (by over 20 percentage points). In comparison, for Xinzhou, an inland city in Shanxi Province, the employment share in industries with a high US tariff increases is only 3.5%. Therefore, Foshan is substantially more exposed to the Trump tariff shock than Xinzhou. As it turns out, the decline in the US movie revenue is also much steeper in Foshan than Xinzhou during the trade war.

We construct the city-specific exposure to US tariff$_{ct}$ by

$$\Delta \text{tariff}_{ct} = \sum_k \left(\frac{L_{c0}^k}{L_{c0}} \cdot \Delta \text{tariff}_t^k\right) \tag{1}$$

where Δtariff_t^k is the addition Trump tariff on imports in industry k in month t, L_{c0}^k and L_{c0} are the initial industry k and total employment for city c, respectively. We are effectively aggregating industry-level tariff increase to the city level, using as weights the industry employment composition of each city before the start of the trade war. According to Equation 1, a city has higher exposure to the trade war, if a greater share of its workers are employed in industries that are subject to newly-introduced tariffs by the US.

Figure 4 presents the summary statistics of our measure of exposure to the Trump tariff increase over time and across cities. As indicated by Equation 1, a city would have a value of 0.1 for the exposure measure if all of its industries are subject to US tariff increase of 10 percentage points. Because the US implemented the first round of US tariffs only starting from July 2018, the exposure measure was zero for all cities before then. Figure 4 shows that the average exposure rose steadily with the escalation of the trade war, from

[10] Throughout the paper, a city refers to either a prefecture city or one with a higher administrative status (such as Beijing, Shanghai, Chongqing, and Tianjin). Rural counties under the administration of a city are included in the statistics for that city. There are a total of 333 cities at the level of a prefecture or above in Mainland China. Our movie database covers 325 cities.

a value of 0.005 in July 2018 to 0.054 in December 2019. There is a considerable variation across the 325 cities in a given month. In December 2019, the standard deviation for the exposure to the Trump tariff increase is 0.0269, with the 25th, 50th, and 75th percentiles being 0.0345, 0.0535, and 0.0695, respectively. These variations will help us identify if the movie viewership is affected by the exposure to the trade war.

3 Baseline Results

3.1 Specification

To study how the trade war affects US movies' box office performance across Chinese cities, we start by a long-difference specification:

$$\Delta \log y_c = \beta_0 + \beta_1 \Delta \text{tariff}_c + \beta_2 \Delta X_c + u_c, \tag{2}$$

where $\Delta \log y_c$ denotes difference in log y in city c over a period of interest (e.g., from second half 2017 to second half of 2018) and y_c is a measure of box office performance. We aggregate the data at the semi-annual level.

In the second exercise, we use year-over-year variations and perform a panel estimation at both the city and theater levels. We focus on year-over-year variations to account for seasonality in the motion picture industry (For example, summer months could have a different viewership pattern from other months).[11] For the city level, we use

$$\tilde{\Delta} \log y_{ct} = \beta_0 + \beta_1 \tilde{\Delta} \text{tariff}_{ct} + \beta_2 \tilde{\Delta} X_{ct} + \phi_t + \phi_c + u_{ct}. \tag{3}$$

where $\tilde{\Delta} z$ denote the change in variable z compared to a year earlier, y_{ct} could be local export, local income, or a measure of box office performance and ϕ_c and ϕ_t are city and time fixed effects, respectively. We implement this specification at the annual frequency for the local economy outcomes, and at both the semi-annual or monthly frequency for the measures of box office performance. For analysis at the theater level, we replace the city-specific outcome y_{ct} in Equation 3 with a theater-specific outcome y_{ict} and could additionally introduce theater fixed effects ϕ_i.

In the panel regressions, we cluster the standard errors at the city level to account for possible serial correlation within a city. For regressions at the monthly frequency, we use two-way clustered standard errors (at both the city dimension and the region-month

[11] While we report the results from a year-on-year specification similar to Amiti et al. (2019), we obtain similar results when we use a monthly first-difference specification as Fajgelbaum et al. (2020).

level). The cities are placed in four regions—east, central, west and northeast—using the definition of the National Bureau of Statistics. As clustering at the individual month level would result in too few clusters, we use region-month level clustering as a compromise.

A key assumption here is that the local exposure to the Trump tariff increases is exogenous. We will examine the validity of the assumption by following the diagostic and sensitivity checks proposed by Adao, Kolesár and Morales (2019), Goldsmith-Pinkham, Sorkin and Swift (2020), and Borusyak, Hull and Jaravel (2022). The results together with other robustness checks will be reported after the baseline results.

3.2 Data

We collect five sets of data. First, for disaggregated data on movie revenue information, we acquire a proprietary data set from Entgroup, a consulting firm specializing in the entertainment industry, which in turn obtains the underlying data from the National Film Ticketing Integrated Information Management System (NFTIIMS). The NFTIIMS is a digital data collection system implemented by the State Administration of Radio, Film, and Television (SARFT) in 2012 to replace the previous more haphazard data collection methods. Our data contain rich information on the box office performance—including total revenue and fill rate—of each movie in each theater. These data are available at the time-of-the-day frequency (mornings, afternoons, evenings, and late nights of each day) for 2017–2019 and 2021 and at the monthly level for 2012–2016. We focus our analysis on data from 2017 to 2019 but use the 2012–2016 and 2021 data to conduct placebo tests and the 2021 data to analyze longer-run effects, respectively. Our movie data include movie-level variables such as genre, the premiere date in China, and country of origin.

Second, the average viewer ratings and box office revenue in North America, as measures of movie quality, are from either IMDb.com for US movies or douban.com for Chinese movies. These data will help us to account for any possible change in revenue due to differences in movie quality.

Third, to construct time-varying city-level exposure to the Trump tariffs, we start with the product lists of US tariffs, which contain the ad-valorem tariff rates and effective implementation dates. We convert the HS products to 4-digit China Industry Classification (CIC) codes, and scale the tariffs by the number of days they were in effect when computing the effective tariffs at the monthly, semi-annual, or annual frequency. We use the 2008 Economic Census of China to compute employment share in each industry for each city.

Fourth, we supplement the data on tariff exposure and box office performance with additional city-level variables. Data on city-level socioeconomic indicators at annual frequency, such as population, local GDP, exports, and imports, are obtained from the CEIC database. The weather data are from the National Climatic Data Center of the US National Oceanic and Atmospheric Administration (NOAA), which provides rich daily weather information at the monitor station level. Finally, data on air pollution come from the Ministry of Environmental Protection in China.

Fifth, we use Baidu Index, the Chinese equivalent of Google Trends, to gauge residents' interests in a given subject. This is discussed in more detail in Section 4. Finally, we use WiseNews to examine the role of local newspaper coverage. Appendix A provides further details on data sources and processing.

Our baseline sample includes 323,865 observations at the theater-month level, covering 10,057 movie theaters in 325 cities. Table 1 provides a summary of key variables in our analysis. As the table shows, the monthly revenue for US movies, Chinese movies, and all movies at the average theater is 188.5, 313.3, and 534.5 thousand yuan (US$ 27.7 thousand, 46.1 thousand, and 78.6 thousand), respectively. On average, US and Chinese films account for about 35% and 59% of total monthly revenue, respectively, while movies by other countries make up the rest. Finally, the lower panel of Table 1 reports the summary statistics of city-level tariff exposure by year, by half-year, and for selected months. In December 2019, the average exposure to the Trump tariff is 5.35%, while the standard deviation is 2.69%.

3.3 US Movie Revenue in China

3.3.1 Results across Chinese cities

We start by examining the effects the Trump tariffs on local economies in China. Using the local exports, the export/GDP ratio, log GDP, and log GDP per capita, respectively, as the outcome variables, we conduct the regression in Equation 3 at the annual frequency. The results are reported in Table 2. The coefficient on local exposure to Trump tariffs is negative and statistically significant in all regressions. For example, an increase in the exposure to the Trump tariffs by one standard deviation in the sample (which is 0.0194 in 2019 according to Table 1) would be associated with a reduction in local exports by 11.2 percent according to Column 1, and a reduction in per capita income by 4.0 percent according to Column 4. It is not surprising to see a smaller effect on local income than on local exports since many people work in jobs not directly related to exports. Our estimated effect on the local income is somewhat larger than the one by Chor and Li (2021). They subdivide the Chinese population into many 11km-by-11km grid cells and use changes in the nighttime lights from satellite images as a gauge for changes in local real output. By that method, they conclude that the grid cells most exposed to the Trump tariffs may have experienced a decline in real output per capita by 2.5%, relative to those unaffected grids.[12] Since the trade war affects local income, it is important to account for any changes in local movie revenue due to changes in local income. In other words, we will estimate the effect of the trade war on US movie revenue beyond an income effect.

We proceed to study changes in local US movie revenue across Chinese cities. Since movie revenue is available at a higher frequency than either exports or income, we are able to use more data to account for possible seasonality in movie viewership by comparing US movie revenue in the second half of 2018 to that in the second half of 2017, and that in the first half of 2019 to that in the first half of 2018. Such a specification accounts for possible differences in the movie viewership between Christmas season (always in the second half of a year) and the Chinese Spring Festival (always in the first half of the year).

Following Equation 2, the results for year-over-year changes from the second half of 2017 (denoted by 2017h2) to the same period in 2018 (2018h2), from 2018h1 to 2019h1, and from 2018h2 to 2019h2, respectively, are reported in Columns 1–3 in Panel A of Table 3. We control for an income effect on movie revenue in all regressions. The coefficients on the local tariff exposure are negative in all cases, and significantly so at the 1% level in Columns 2 and 3. The magnitude of the estimate increases from Column 1 to Column 3, partly because the Trump administration has escalated tariffs over time (as the last panel of Table 1 shows). Perhaps the local moviegoers' awareness of the trade war has also increased over time. Based on these estimates, an increase in the exposure to the Trump tariffs by one standard deviation leads to a decline in US movie revenue by 0.46%, 1.3%, and 2.5%, respectively, in the three successive time periods (Note that the standard deviations of the Trump tariff exposure are 0.96%, 1.37%, and 2.53% in the three periods, respectively, as reported in Table 1.) The difference over two years (from 2017h2 to 2019h2) is reported in Column 4. We continue to find a negative and significant coefficient on tariff exposure. These patterns offer *prima facie* evidence that greater exposure to the Trump tariffs translates into a greater reduction in local demand for US movies.

We also conduct panel regressions using the specification in Equation 3. The panel specification allows us to control for city fixed effects, which could be correlated with local exposure to the Trump tariffs. In Columns 1 and 2 of Panel B of Table 3, we pool the observations over four time periods (2017h1 to 2018h1, 2017h2 to 2018h2, 2018h1 to 2019h1, and 2018h2 to 2019h2, respectively), add period fixed effects, and cluster the stan-

[12]For comparison, using changes in automobile sale in US counties as a gauge for changes in local consumption, Waugh (2019) finds that US counties in the upper quartile of exposure to Chinese tariffs experienced a 3.8% decline in consumption growth relative to counties in the bottom quartile.

dard errors at the city level. While our way of organizing the data helps us filter out possible seasonality in movie viewership, it may also introduce serial correlation in the error terms. It is therefore important to cluster the standard errors at the city level to account for possible serial correlation by city. For comparison, we include city fixed effects in Column 2 but not in Column 1. We see that the coefficient on the local exposure to the Trump tariffs is larger in absolute value with the city fixed effects (-1.723 in Column 2 relative to -0.915 in Column 1). As the standard deviation in tariff exposure is 0.0253 in 2019h2 (see Table 1), an increase in the tariff exposure by one standard deviation in 2019h2 would lead to a reduction in local US movie revenue by 4.4% according to Column 2.

Taking advantage of the high-frequency nature of our movie data, we also conduct panel regressions on monthly year-over-year changes. This provides an even more powerful way to control for seasonality in the data. The results are reported in Column 3 (with monthly fixed effects) and Column 4 (with both city fixed effects and monthly fixed effects). As the results show, controlling for monthly seasonality and adding these fixed effects are important. The point estimates on the local tariff exposure at the monthly frequency are about 20–40% greater than their semi-annual counterparts from Columns 1 and 2. They remain statistically significant at the 1% level in both cases, in spite of the more demanding two-way clustering of the standard errors at both the city and region-month levels. According to the last column, an increase in the local exposure to the Trump tariffs by one standard deviation (2.69% in December 2019) would reduce the local US movie revenue by 5.6%.

To check for possible influence of outliers, we present a bin-scatter plot, in the left panel of Figure 5, of the residualized local US movie revenue against the residualized local exposure to the Trump tariffs, based on the last column of Panel B in Table 3. We can see that the negative relationship between the two is unlikely to be driven by outliers.

3.3.2 Results across Theaters

Not all movie theaters are the same. Some may be fancier and more luxurious than others, and the different theater types may cater to different income and demographic groups. Suppose US movies are more likely to be shown in more luxurious theaters, and those cities more exposed to the Trump tariffs are likely to close down the more luxurious theaters, the estimates in the previous city-level regression could then be upwardly biased. Conversely, if those cities more exposed to the Trump tariffs disproportionately reduce the movie showing times in less luxurious theaters, the previous estimates could be downwardly biased. To ensure that the theater composition effect does not contaminate our results, we conduct theater-level panel regressions using year-over-year changes at the monthly frequency. There are 9983 theaters in the sample.

Table 4 reports the results, with successively more fixed effects from Column 1 to 3. With only month fixed effects in Column 1, the estimated coefficient on the tariff exposure is -1.12 and statistically significant at the 5% level. With additional city fixed effects in Column 2, the point estimate becomes bigger (-2.39). If we also include theater fixed effects in Column 3 (which supersede the city fixed effects), the estimated effect of the exposure to the Trump tariffs becomes -2.62. That is, holding theater attributes constant, we see that an increase in the exposure to the Trump tariffs by one standard deviation now translates into a greater reduction in US movie revenues by 7.1 percent.

In Column 4, we expand the list of control variables to include local population growth, air pollution, and additional variables for weather conditions such as the shares of rainy days, hot days, cold days, and days with severe air pollution in a month to account for possible changes in the propensity of local residents to consume movies that are not related to the trade war.[13] The point estimate for the local exposure to the Trump tariffs is -2.68 and is significant at the 1% level. A conditional bin-scatter plot of the residualized US movie sale against the residualized local exposure to the Trump tariffs is presented in the right panel of Figure 5. The scatter plot confirms a negative correlation between the two and suggests that it is unlikely to be driven by any outliers. If anything, removing the most likely candidates for outliers on the far left and far right of the graph could make the point estimate larger in absolute value.

Using the result in Column 4 as our preferred estimate, an increase in the local tariff exposure in December 2019 by one standard deviation would reduce the US movie revenue by 7.2%. As another way to gauge the economic magnitude of the estimate, an increase in the exposure to the Trump tariffs from zero in June 2018 to 5.35% in December 2019 (which is the observed increase for all cities on average according to Table 1) leads to a reduction in the US movie revenue by 14.4%.

To illustrate a possible economy-wide effect, we do a back-of-envelop calculation by assuming that the intercept in the regression is not affected by the trade war.[14] Relative to a counterfactual in which the US-China trade war did not take place, the loss in box office revenue for US movies in theater i and month t is given by

$$\text{loss}_{it} = \text{revenue}_{ict} \cdot (e^{-\hat{\beta}_1 \cdot \text{ tariff}_{ct}} - 1)$$

[13]We classify a day as a rainy day, a hot day, or a cold day if its total rainfall depth exceeds 10 millimeters, its average temperature exceeds 30 degrees Celsius, or falls below 0 degrees Celsius, respectively. Similarly, we classify a heavy polluting day if the Air Quality Index (AQI) exceeds 150 on that day.

[14]This assumption may not hold in general but a similar assumption is used by Autor, Dorn and Hanson (2013) in their illustration of an economy-wide effect of trading with China on US jobs.

where $\hat{\beta}_1$ is a coefficient estimate from Table 4, tariff_{ct} denotes the month t tariff exposure for city c, and revenue_{ict} denotes the month t revenue of theater i. We then sum up these effects across theaters and over time. The cumulative loss in the US movie revenue reaches 2.7 billion RMBs (or US$400 million) by December 2019. This would account for about 40% of the observed shortfall in aggregate US movie revenue in China relative to a linear trend extrapolation using 2012-2017 data (reported in Appendix Figure A2).

3.4 Compared to Non-US Movies

While we have controlled for changes in log GDP per capita in all regressions, one may still be concerned that the changes in GDP per capita may not fully capture the changes in household income. It is certainly possible that changes in movie viewership depend on changes in the distribution of the local income beyond the changes in the average income. To see if this is a quantitatively important concern, we can examine the impact of the Trump tariff exposure on non-US foreign films. If our baseline findings are an artifact of an insufficient control for an income effect, we would expect to find similar adverse effects of the tariff exposure on non-US foreign films.

We follow the baseline specification (but with the changes in non-US movie revenue as the dependent variable) and report the regression results in Panel A of Table 5. From Column 1, we see that the differential exposure to the Trump tariffs has no statistically significant effect on non-US foreign movies. From Column 2, where the dependent variable is the change in the revenue of Chinese movies, we see that the coefficient on Trump tariff exposure is positive at 2.30 and statistically significant at the 5% level. This suggests that the consumers substitute in-theater consumption of US movies with Chinese ones due to a trade-war-induced aversion towards the US movies. Finally, in Column 3, we examine the effects of the tariff exposure on the total local movie revenue. We find the effect to be indifferent from zero. These results suggest that, if the income effect is not fully captured by the change in local GDP per capita, it is not strong enough to reduce moviegoers' overall demand for in-theater movie entertainment.

Rather than looking at changes in the monetary value of movies, we can also examine changes in the shares of US movies, non-US foreign movies, and Chinese movies, respectively. This specification can accommodate possible zero values in some movie categories in some theaters and time periods. We report the results in Panel B of Table 5. We can see that the number of observations indeed goes up due to fewer missing observations (relative to the regressions Panel A). The results from the new regressions are broadly in line with those from Panel A. In particular, greater exposure to the Trump tariffs translates into a lower market share for US movies, but no significant change in the market share for non-US foreign movies, and an increase for Chinese movies.

Note that the regional variations in changes in the market shares are unlikely driven by supply factors. As movies are sent digitally to local theaters, there is no regional variations in the shipping costs. Nonetheless, as a complementary exercise, we gauge the relative importance of local demand and supply factors by examining the theater fill rates—the sold seats as a share of the total seats—by movie types. If the decline in US movie revenue in a location is driven by a reduced supply of showing times, we would expect the fill rate to go up for US movies. Conversely, if the decline in US movie revenue is driven by a lower local demand, we would expect the fill rate to go down.

Using the monthly year-on-year changes in the fill rate as the dependent variable, we report the results in Table 6. In Column 1, where we examine changes in the fill rates for US movies, the coefficient on the Trump tariff exposure is -3.15 and statistically significant at the 1% level. That is, in regions with greater exposure to the Trump tariffs, the theaters showing US movies tend to be emptier. This means the local demand factor is the key for our result. In fact, the estimated elasticity (-3.15) is slightly higher than the elasticity of US movie revenue (-2.68). Therefore, the negative impact of the tariff exposure on US movie revenue can be entirely explained by a decline in the fill rate.

In comparison, we find no effect of the tariff exposure on the fill rate of either non-US foreign movies (as reported in Column 2) or Chinese movies (Column 3). Overall, these results support our interpretation that a reduction in US movie revenue is driven by shifts in consumer tastes rather than changes in supply factors.

3.5 Robustness

3.5.1 Controlling for Chinese retaliatory tariffs

We examine the robustness of our theater-level baseline results to including China's retaliatory tariffs on US goods. These tariffs can potentially impact the income, employment, or preferences towards US movies of residents. To address the concern that retaliatory tariffs may drive our results, we construct a city-specific retaliation measure by replacing the US tariffs (tariff_t^k) in Equation 1 with retaliatory tariffs. We then add the resulting variable as a new control variable in the regression.

As reported in Column 1 of Table 7, the coefficient on retaliatory tariffs is statistically indifferent from zero. It is possible that the moviegoers in different regions do not have a differential awareness of these retaliatory tariffs, or do not regard them as relevant in their movie choices once the income effect is controlled. In any case, accounting for retaliatory tariffs has no material impact on the coefficient on exposure to US tariffs.

3.5.2 Alternative constructions of the tariff exposure

We consider three alternative ways to construct the regional exposure to the US tariffs. The first alternative is to use the announcement dates of the new US tariffs rather than the implementation dates to construct the variable. The conceptual difference between the two is whether the moviegoers respond to the trade war when they first learn about the tariff announcements from the news or when they feel the effects of the trade war through actual changes in the jobs or incomes of themselves, their family members, friends, or neighbors. We include the exposures to both the announced tariffs and the implemented tariffs in the regression in Column 2 of Table 7. The coefficient on the exposure to announced tariffs is statistically indifferent from zero, while the coefficient on the exposure of the implemented tariffs is unchanged from the baseline. This suggests that the moviegoers respond to the Trump tariffs when they start to feel them through their jobs or income changes rather than when they first hear the announcement.

Our second alternative measure considers not only the Trump tariffs that are specific to Chinese goods (under Section 301 of the Trade Act of 1974) but also other tariffs on goods from other countries in addition to China (such as those on solar panels and washing machines under Section 201 of the Trade Act of 1974 and the steel and aluminum tariffs under Section 232 of the Trade Expansion Act of 1962). This adjustment turns out to have negligible effects on our baseline results (reported in Column 3 of Table 7).

Our third alternative measure aims to take into account the varying importance of the US market relative to other markets for different industries. It is constructed according to

$$\Delta \text{tariff}_{ct}^{\text{USshare}} = \sum_{k} \left(\frac{L_{c0}^{k}}{L_{c0}} \cdot \Delta \text{ tariff}_{t}^{k} \cdot \text{USshare}^{k} \right)$$

where USshare^k denotes the Chinese exports to the US in industry k as a share of the total exports of that industry in 2017. As reported in Column 4 of Table 7, the point estimates of the new regressor are different from the baseline estimates since modifying the exposure measure implies different mean and variance of the variable. Nevertheless, we continue to find a significantly negative impact of the exposure to US tariffs on the local US movie revenue. Using the same aggregation approach in Section 3.3, the point estimate corresponds to a loss of 2.4 billion yuan in the US movie revenue, which is comparable to the value of 2.7 billion yuan based on our baseline estimates. In summary, our results are robust to these alternative measures of the exposure to US tariffs.

3.5.3 Movie quality during versus before the trade war

If, by coincidence, there is a relative decline in the quality of US movies during the trade war, there could be a decline in US movie viewership even without the trade war. The decline in movie quality would not matter for our results unless there is a correlation between the quality composition of the US movies shown in different regions and the local exposure to the Trump tariffs.

Rather than assuming an absence of the correlation, we now measure and control for the average movie quality. We measure local movie quality by a session-weighted average of IMDb ratings (for US movies) and *douban* ratings (for Chinese movies) of all the films shown by a theater in a month. We incorporate this as a new control variable and report the results in Column 5 of Table 7. We find that a higher IMDb rating of US movies increases their revenue, while a higher *douban* rating of Chinese movies decreases it (as Chinese movies are a substitute for US movies). Nevertheless, the coefficient on tariff exposure is barely changed from the baseline.

3.5.4 Validity of the shift-share instrumental variable

Our key regressor—the local exposure to the Trump tariffs—is essentially a shift-share instrument, also known as a Bartik instrument. We probe the validity of this variable and the robustness of our results following a recent literature. Goldsmith-Pinkham, Sorkin and Swift (2020) demonstrate an equivalence between a shift-share IV and a generalized method of moments (GMM) estimator with the local industry shares as the instruments and a weighting matrix based on national industry growth rates (the "Rotemberg" weights). These Rotemberg weights characterize each industry's influence in a given shift-share application. One concern is that the coefficient estimates are driven by a small number of high-Rotemberg-weight industries. We compute the Rotemberg weights following Goldsmith-Pinkham et al. (2020) and report the top 20 industries by the Rotemberg weights in Appendix Table A3. We find that the industry-level Rotemberg weights in our case are more dispersed than those from Autor et al. (2013). Specifically, the three highest Rotemberg weights in our case are 0.070, 0.044, and 0.042, which are noticeably smaller than the values of 0.18, 0.14, and 0.09 from Autor et al. (2013) (as reported by Goldsmith-Pinkham et al. (2020)). In any case, as a robustness check, we compute two new measures of local tariffs by removing the top three and top ten industries by Rotemberg weights, respectively. We then repeat the city-level regressions and find that the results are essentially unchanged (see Appendix Table A4). As also recommended by Goldsmith-Pinkham et al. (2020), we check for pretrends in Section 3.6.

Borusyak, Hull and Jaravel (2022) study a shift-share research design in which the identification follows from a quasi-random assignment of shocks at the industry level. They demonstrate that the shift-share coefficients estimated in a sample of locations are identical to those from a weighted instrumental-variable (IV) regression in the industry dimension. A balance test can be conducted using industry-level regressions to examine the validity of the identifying assumption. Following Borusyak et al. (2022), we convert the location-specific variables to industry-level weighted averages and examine the correlation between observable local shocks and the Trump tariffs. We find that the industry-level US tariffs introduced in the trade war are not correlated with year-over-year growth of US movie revenue before the trade war, changes in log population, the manufacturing share of total employment, or the share of households with broadband Internet access (Appendix Table A7). These results support the identifying assumption that industry-level tariff shocks are uncorrelated with other local shocks. Appendix B provides other diagnostic tests, including shock intra-class correlations proposed by Borusyak et al. (2022).

Adao, Kolesár and Morales (2019) show that the conventional standard errors could be biased if regions with similar industry shares have a high correlation in the regression residuals. In Appendix C, we compute six different sets of standard errors, including those from the methods proposed by Adao et al. (2019) and Borusyak et al. (2022). In all cases, the coefficients on the local exposure to the Trump tariffs are negative and statistically significant. Therefore, our results are robust to the alternative inference methods.

3.6 Placebo

A potential threat to our empirical strategy is that the local tariff exposure may be correlated with a preexisting trend of US movies' box office performance. We address this concern using two different approaches.

First, we use data on movie revenue from 2012 to 2016 (i.e., before the trade war) to conduct a placebo test. Imitating the theater-level baseline specification, we use the monthly year-over-year change in US movie revenue computed for the 2012–2014, 2013–2015, and 2014–2016 periods, respectively, as the dependent variable. We use the corresponding monthly year-over-year changes in tariff exposure over 2017–2019 as the key regressor. In other words, we examine whether changes in current tariff exposure in a location are correlated with the growth of local US movie revenue from five years, four years, or three years earlier, respectively.

We report the results without and with theater fixed effects in odd-numbered and even-numbered columns in Table 8, respectively. In all the placebo cases, we do not find any significant relationship between tariff exposure changes during 2018-19 and the US movie revenue growth in earlier years. Thus, there is no evidence of a preexisting trend.

Second, we estimate a series of month-by-month coefficients on the response of the local US movie revenue to local exposure to Trump tariffs, with an aim to see when a negative relationship between the two first begins to emerge and whether it persists. The time series profile should help us see if a pre-trend exists. Specifically, we conduct the following regression:

$$\tilde{\Delta} \log \text{Revenue}_{ict} = \gamma_0 + \sum_{\tau \neq \tau_0} \gamma_\tau \cdot \mathbb{1}\{t = \tau\} \cdot \text{tariff}_c^{\text{Dec2019}} + \gamma_1 \tilde{\Delta} X_{ct} + \phi_t + \phi_i + u_{ict} \tag{4}$$

where $\text{tariff}_c^{\text{Dec2019}}$ is the cumulative Trump tariff increases by December 2019 in city c, and $\mathbb{1}\{t = \tau\}$ is an indicator variable for month τ. As before, X_{ct}, ϕ_i, and ϕ_t denote time-varying city-level controls, theater fixed effects, and time fixed effects, respectively. In Equation 4, γ_τ captures the correlation in month τ between box office performance and the final (December 2019) exposure intensity, relative to a baseline month τ_0 (set to be June 2018, which is the last month before the first Trump tariffs were implemented).

We cluster the standard errors at both the city and region-month levels. Figure 6 plots the estimated γ_τ's from this exercise. As the upper panel of the figure shows, all of the estimated γ_τ's before June 2018 are statistically indistinguishable from zero. Put differently, we do not detect any significant correlation between changes in the local US movie revenue and the final tariff exposure before the trade war started. This means assuming parallel trends is reasonable in our differences-in-difference (DID) research design.

Consistent with the interpretation of a trade war effect, the estimated γ_τ starts to decline in July 2018 when the China-specific Trump tariffs took effect, and a majority of the estimates are negative and statistically significant. Interestingly, the time series graph of γ_τ estimates is approximately a mirror image of the average 12-month changes in tariff exposure, depicted in the lower panel of Figure 6, with a correlation coefficient of -0.75. In sum, the patterns in Figure 6 suggest that the estimated negative effect of the trade war on US movie revenue is unlikely to be an artifact of a pre-trend.

3.7 Revenue Sharing versus Flat Fee

A key question is to what extent our results reflect the local governments' actions rather than private citizens' choices. In particular, the local government in cities more adversely affected by US tariffs may develop a distaste for US products in general. One way for the local government to express their displeasure is to reduce the profits going to US movie producers, for example, by requiring local theaters to reduce the showing of US movies.

Note that the incentive to reduce the showing of US movies as a punishment to the Americans differs between the revenue-sharing and flat-fee movies. For flat-fee movies, the payments to US movie studios are fixed at the time of importation and are insensitive to actual local movie revenue. As the Chinese importers are the residual claimant, any reduction in the showing of the flat-fee movies would not penalize US movie studios in any way. This means to penalize US movie studios, it would only make sense to reduce the showing of the revenue-sharing movies.

If the goal of the local government is to retaliate against the United States by reducing US companies' income, any official or unofficial policy intended to achieve this goal would be best directed at revenue-sharing movies. Therefore, if our baseline results reflect the actions of the local governments, we should expect to find a greater negative effect on the revenue-sharing US movies than on the flat-fee ones.

We run separate regressions for the two types of US movies. In Columns 1 and 3 of Table 9, we see no evidence of a steeper drop in revenue for revenue-sharing US movies than for flat-fee movies in more tariff-exposed regions. In fact, we see an opposite pattern: a much larger negative effect on flat-fee movies than revenue-sharing ones. These results are robust to controlling for the quality of the movies (the session-weighted average of the IMDb rating for the respective movies), as reported in Columns 2 and 4.

These patterns are consistent with an absence of local government actions in the more tariff-exposed regions to deliberately suppress viewership of the revenue-sharing US movies. The comparatively smaller effect of the trade war on the revenue-sharing movies likely results from the more heavy advertisement of such movies (which are more likely to be big-budget movies from major Hollywood studios) than of the flat-fee movies (Dresden, 2018). In sum, the reduction in the viewership of US movies likely results from local citizens' choices rather than local government actions.

4 Complementary Evidence from Baidu Searches

We gather data on Baidu search intensity on various phrases to validate the previous findings and interpretations. Baidu is the dominant Chinese-language search engine in China, much like Google in the United States. According to statistica.com, searches using Baidu accounted for 77% of all online page views in China in 2021.[15] Baidu Index, similar to Google Trends in the United States, provides a measure of search intensity for a given

[15] \https://www.statista.com/statistics/253340/market-share-of-search-engines-in-china-pageviews/.

keyword and can be computed separately by region and time. While Baidu does not report the raw search volume, the value of the Baidu index is a linear scaling of the underlying search volume (Qin and Zhu, 2018). As such, it is informative about the relative search intensity for a given keyword across cities and over time.

We assume that the relative search intensity is not manipulated by the central and local governments as there is no obvious benefit from doing so. To be sure, some of the politically sensitive words are blocked on the search engine. But such censorship policy is almost always nationally uniform and does not have a regional component. In other words, conditional on the availability of the Baidu index for a keyword, different values of the Baidu Index across regions should reflect different search intensities in these places. A number of other studies in the literature have used the Baidu search index and find it informative. For example, Qin and Zhu (2018) use the Baidu index for "emigration (*yimin*)" to measure residents' intention to emigrate. Campante, Chor and Li (2019) use the Baidu index for "maintaining social stability (*weiwen*)" to gauge local public concerns for labor unrests. We use the Baidu index to measure the relative strength of local residents' interests in the trade war, US movies, and other US products.

We use both long differences in search intensity and panel regression specifications, similar to those used earlier in the city-month regressions of US movies, to study the effect of the Trump tariff exposure on the Baidu search intensity for various keywords of interest. In the long difference specification, we use log (1+Baidu Index in 2019) - log (1+Baidu Index in 2017) as the dependent variable. This ensures that the dependent variable is defined even if the Baidu index takes on a value of zero for some cities in some years. The key regressor of interest is the cumulative change in the local exposure to Trump tariff increases by December 2019. In line with our movie-revenue specifications, we control for the change in log GDP per capita in a city over these two years.

Separately, to take advantage of the high frequency nature of the search intensity data, we also conduct a panel regression across the cities and the 24 months in 2018–2019. In this case, log(1+Baidu Index) in a city and a month is the dependent variable, and the local exposure to the Trump tariffs in that month is the main regressor (along with log income). We include both city fixed effects and month fixed effects.

4.1 Awareness of the Trade War

We start with words reflecting interest in the trade war, including "Sino-US trade war (*zhongmei maoyi zhan*)," "trade war (*maoyi zhan*)," and "Sino-US trade friction (*zhongmei* *maoyi moca*).”[16] We construct a composite index by aggregating the Baidu Index for these keywords. Appendix Figure A3 plots a time series of the value of the Baidu search index for these keywords from 2017 to 2019. Unsurprisingly, the composite index takes on a value of zero before March 2018 (when President Trump first launched the trade war) and goes up with each new wave of the Trump tariff increases.

We then check if the local search intensity for these words corresponds to the local exposure to the Trump tariff increases. In Appendix Figure A4, we plot the composite trade-war index, normalized by the local population, against the local exposure to the Trump tariff increases. We see a clear positive association: those cities with greater exposure to the Trump tariffs exhibit more Baidu queries per capita about the trade war. In other words, where local communities are more affected by the trade war, there is an increase in the local search intensity for trade war or trade frictions.

This relationship is confirmed in Table 10. Columns 1–3 report the results for keywords “Sino-US trade war,” “trade war,” and “Sino-US trade friction,” respectively. In the long difference regressions reported in Panel A, the point estimates of the coefficients on the tariff exposure are all significantly positive and range from 24.3 to 41.4. Column 4 reports the results for the composite index, constructed from aggregating the previous three indices. The coefficient on tariff exposure remains significantly positive.

Panel B presents the results from the panel regressions. We continue to find a highly significant effect of the Trump tariff exposure on searches for trade-war-related keywords. The smaller magnitudes of the estimates in Panel B can be understood by an anticipatory effect. Specifically, from March to June 2018, with the tariff announcement (but not yet implemented) by the Trump administration, there were elevated interests in the trade war (Appendix Figure A3) even though tariff exposure was zero for all regions (recall that the first wave of US tariff came into effect in July 2018). By contrast, the Baidu index for trade-war-related keywords was zero throughout 2017. Consequently, the panel regression, which exploits within-city variations across the months in 2018 and 2019, produces smaller coefficients than the long-difference specification, which is based on the within-city comparison between 2017 and 2019. Taken together, these results suggest that the residents in regions more exposed to the Trump tariffs are more aware of and concerned with the trade war, as reflected in their search activities.

[16] Baidu Index is available only when the search volume exceeds a threshold. While “trade friction (*maoyi moca*)” is the preferred phrase by the Chinese government, it is not as popular with the public, and its search volume appears to be below this threshold.

4.2 Search Intensity for US and Other Movies

Baidu search information can also help us gauge any change in local residents' appetite for US movies (relative to movies in general). For ease of comparison, we construct local Baidu search indices for "US movies (*meiguo dianying*)," "foreign movies (*guowai dianying*)," and "movie tickets (*dianying piao*)." Furthermore, for each of the top 5 US movie titles in a given month in terms of the national box office revenue, we gather their search intensity individually by city and month and then aggregate them into a composite Baidu index for top 5 US movies by city and month. This composite index reflects local residents' interest in the top US movies currently shown in theaters, and provides a useful complement to the Baidu index for "US movies" in general.

From Panels A and B in Column 1 of Table 11, we see a clear negative and statistically significant effect of the local exposure to the Trump tariffs on the local interests in US movies, both in the long difference specification (Panel A) and panel regressions (Panel B). In other words, people in cities more exposed to the Trump tariffs demonstrate a reduced interest in US movies through their lower search activities. In Column 2, we see a negative effect in the search interest in the top five US movies currently in theaters in both the long-difference and panel specifications, although only the coefficient based on long-difference (Panel A) is statistically significant.

For comparison, we use Baidu Index for "foreign movies" as the dependent variable in Column 3. We see no significant effect of the exposure to the Trump tariffs on local search interest for foreign movies in general. In Column 4, we see no effect of the Trump tariffs on local search interest for "movie tickets." In other words, the residents in regions more exposed to the Trump tariffs watch fewer US movies not because they watch fewer movies in general (which could result from an income effect of the trade war) but because they choose to substitute non-US movies for US movies. These search patterns reinforce our conclusions from the movie revenue analysis in the previous section.

4.3 Search Interests beyond Movies

With the Baidu search information, we can also check if the exposure to the trade war affects the local interest for the United States as a tourist destination, US colleges and graduate schools, and famous US brands. We start with US-bound tourism and report the results in Table 12. We find that in regions with greater exposure to Trump tariff, there is a statistically significant reduction in the Baidu search intensity for "US tourism (*meiguo lvyou*)" or "US visa for tourists (*meiguo lvyou qianzheng*)." For comparison, we use Baidu Index for "Japanese tourism (*riben lvyou*)" and "tourism (*lvyou*)" as the dependent variable in Columns 3 and 4. We find that tariff exposure does not significantly affect the search intensity for "Japanese tourism." Finally, for "tourism," while the coefficient on tariff exposure is marginally significant in the long-difference specification, the coefficient turns positive and insignificant in the panel regression. These results suggest that the Trump tariffs have reduced Chinese citizens' interests in vacationing in the United States but do not affect the interests in traveling to other countries such as Japan.

Next, we investigate whether the trade war would alter citizens' willingness to consume US-branded goods. We obtain the Baidu Index of "Nike (*naike*, a US brand)," "Anta (*anta*, a Chinese brand)," "ASICS (*yaseshi*, a Japanese brand)," and "sports shoes (*yundong xie*)." We report the results in Table 13. In the long-difference specification, we find a reduced search intensity for "Nike," but a greater search intensity for "Anta," "ASICS," or "sports shoes" in regions with greater exposure to the Trump tariffs. These results suggest that consumers substitute consumption of a US-branded goods with those from other countries. While we find smaller and statistically insignificant estimates from the panel regressions in Panel B, the coefficient in the "Nike" regression remains negative. Furthermore, a t-test reveals that the coefficient from the "Nike" regression is significantly different from those in the "Anta" or the "ASICS" regression.

Finally, we investigate whether the trade war has altered Chinese citizens' interests in educational pursuits in the United States. We obtain the Baidu Index for "US college (*meiguo daxue*)," "UK college (*yingguo daxue*)," "Japanese college (*riben daxue*)," and "study abroad (*chuguo liuxue*)." The results, reported in Table 14, suggest that the exposure to the Trump tariffs reduces Chinese citizens' interests in US colleges, but not in other countries' colleges. Compared to the results on US tourism or US sports goods, the results on US colleges are weaker in terms of statistical significance (the coefficient from the panel regression is significant at the 10% level). In other words, for those Chinese families interested in sending their children to study abroad, the US tariff increases do not have the same negative effect on their attitudes towards the United States. Such families could be wealthier and better educated and may not share the same world views as the general Chinese public. We note, however, that the Trump tariffs have not made them more eager to study in the United States, either.

5 Extensions

5.1 Action Speaks Louder?

Movies come in different genres. The salience of the country of origin is more prominent in action movies and perhaps in dramas than in other genres. Lichtenfeld (2007) makes this point and suggests that the use of internationally-recognizable movies stars such as Tom Cruise, Will Smith, Angelina Jolie, and Arnold Schwarzenegger in action movies makes it easier for the audience to associate them with the United States. In addition, their tendency to glorify individualism, heroism, vigilantism, and masculinity may further strengthen their association with the US in the minds of international audience. In contrast, for animation, such as Toy Story or How to Train a Dragon, the audience may be entirely unaware of the country of origin unless they specifically look up the information. If the trade war generates an aversion to movies with a salient "Americanness," the effect may show up more strongly on action and drama movies than on animation.

Entgroup, the data provider, places a movie into one of twelve genres: action, drama, sci-fi, fantasy, animation, documentary, horror, suspense, disaster, war, romance, and comedy.[17] We combine sci-fi and fantasy as a single category as they are often indistinguishable to casual movie viewers. As the number of movies is sparse in small categories, including documentary, horror, suspense, disaster, war, romance, and comedy, we consolidate them into an "others" category. They collectively account for only 4.8% of the total US movie revenue in China.

We apply our baseline specification for each genre of the US movies separately, and report the results in Table 15. According to Column 1, for US action movies, the largest movie category which typically accounts for 65% of total US movie revenue, the point estimate of the coefficient on the tariff exposure is -2.62 and statistically significant, indicating a greater reduction in the viewership of US action movies in more tariff-exposed regions. The size of the estimated effect is close to that in Column 4 of Table 4.

For dramas, reported in Column 2, we also see a negative coefficient whose point estimate is larger in absolute value than the corresponding one for action movies. But the estimate is not statistically significant because the standard error is much larger than for action movies. In Columns 3–5, we examine the effects on sci-fi/fantasy, animation, and other categories, and find no significantly negative effect. The coefficient on the tariff exposure is in fact positive and significant for animation movies, though we do not have a straightforward explanation for this pattern.

Note that there are many zero values at the theater-month level. This is a more se-

[17] While Entgroup places a given movie in a unique genre, IMDb can assign the same movie to multiple genres. In a robustness check, we also adopt the IMDb classification, assigning a movie with multiple labels into several genres and re-do our regressions. We find our results to be qualitatively the same.

rious problem for a relatively small genre such as animation or drama than for a large genre such as action. To avoid this problem and as a robustness check, we re-do these regressions with the revenue of a given US movie genre as a share of the total local movie revenue (including that of non-US movies) as the dependent variable. With such a specification, there is no missing value for any theater-month. From the results reported in Panel B of Table 15, we find a significantly negative effect of the local tariff exposure on both US action and US drama movies. At the same time, we find no statistically significant effect on animation or other genres.

To summarize, the negative effect of the trade war on the Chinese viewership is much stronger for US action and drama movies than for other genres. Since the association with the United States tends to be stronger for actions and dramas, the differential patterns across the movie genres also support the interpretation that the Trump trade war has diminished the US brand image in the minds of Chinese citizens.

5.2 Show Time and Theater Type Effects

Different show times can attract different movie audience. For example, daytime shows during work weeks tend to attract retirees, whereas shows on weekends and holidays tend to attract professionals and families with children. By utilizing our disaggregated data on movie revenue by show times, we could obtain insight on whether the trade war effect is concentrated in certain demographic groups or not.

Different show times also affect the volume of viewership. If the theater owners or managers in tariff-exposed regions choose to reshuffle the schedule and place US movies in less favorable slots during the trade war, this could lead to a greater reduction in US movie viewership in these regions. Since 98% of the movie theaters are not owned by the state, movie theaters are profit-driven. Nonetheless, one wonders if the theaters could be ordered to do so by the local governments. In that case, the spatial pattern would not necessarily reflect an increased aversion to US brands by the general public.

We search for anecdotes to these effects among both media reports and private WeChat discussions among circles of friends and find no hint of either a special retiree effect or a special government action effect. In principle, an effect may be there but ordinary people may not realize it. We therefore investigate the show time effects more formally by using our disaggregated data. First, for any given theater and month, we sort the US movie revenue by show times (mornings, afternoons, evenings, and late nights) and run separate baseline regressions. The results, reported in Panel A of Appendix Table A9, shows that the negative effect of the trade war is present in every time slot, and no statistically significant difference can be detected between any two time slots. Second, we sort the US movie revenue into those on weekends and holidays versus those on other days, and report their results in the first two columns in Panel B of Appendix Table A9, respectively. We see again that the negative effect of the trade war is the same in the two cases. Third, we sort the US movie revenue between those in prime sessions (i.e., the afternoon and evening sessions on weekends and holidays plus the evenings before these days) and those in other sessions, and report the results in the last two columns in Panel B of Appendix Table A9. Once again, for both prime and non-prime sessions, there is a significant reduction in US movie viewership in regions with greater exposure to the Trump tariffs.

We check directly whether the shares of US movies in evenings, weekends/holidays, or prime sessions in general, have declined in more tariff-exposed regions during 2018-2019, and report the results in Appendix Table A10. We see no negative relationship between the US movie share in favorable time slots in a region and the exposure to the Trump tariffs of that region. In fact, there appears to be an increase in the share of US movies in prime sessions in the more tariff-exposed regions.

In sum, the negative effect of the trade war on US movie viewership is unlikely to be driven by a show time effect. Moreover, to the extent different show times tend to attract different mixes of demographics (e.g., more retirees on weekdays and more students on holidays and weekends), the results show that the negative effect of the trade war holds across most demographic groups.

Finally, we explore the heterogeneity in movie theater types as measured by the average ticket price. A higher average ticket price associated with a theater likely reflects better amenities such as better seats, a bigger screen, and better overall ambience. Presumably relatively more affluent audience would self-select to use such movie theaters on average. In Appendix D, we sort movie theaters by the average ticket price and find that the negative impact of the exposure to Trump tariffs on US movie revenue is greater in theaters with a lower average ticket price. This suggests that the aversion to US movies triggered by the trade war is more pronounced among the less affluent Chinese citizens. Recall from the previous section on the Baidu search results that the attitudes of the more affluent families who are able to send their children abroad appear to less affected by the trade war. Therefore, the results on theater types and Baidu searches are consistent with each other. Since we do not observe directly the income or other demographic characteristics of the audience, these results are suggestive rather than conclusive.

5.3 Supply Chain Linkages and US Sanctions

The effect of the trade war can be felt indirectly through the supply chain channels. Even if a city does not have many industries that directly experience an increase in US tariffs, it can still be affected indirectly by the Trump tariffs if many of the city's firms produce products that are primarily used as inputs to other firms located elsewhere that are negatively affected by the Trump tariffs. This indirect supply chain channel has been emphasized by Wang, Wei, Yu and Zhu (2018) in the context of the effects of the US imports from China on the US local employment.

Formally, a given Chinese region may be exposed to the Trump tariffs in three ways which can be measured separately. The first is a direct exposure—as measured in our baseline estimation. The second is an indirect exposure through an upstream channel. It measures the extent to which the local employment is concentrated in industries that are upstream suppliers to other industries heavily impacted by the Trump trade war. The third channel is another indirect exposure through a downstream channel. It measures the extent to which the local employment is concentrated in industries that are downstream buyers from other industries heavily impacted by the Trump tariffs. We construct the three indicators of the exposure to the Trump tariffs following Wang et al. (2018). The details are reported in Appendix E.

In principle, a higher upstream tariff exposure may also reduce US movie revenue, but a higher downstream tariff exposure may have an ambiguous effect. Intuitively, a city with a sizable upstream exposure would be more adversely impacted by the trade war because more local outputs are sold to industries directly targeted by US tariffs. In comparison, a city with a substantial downstream exposure might benefit from the Trump tariffs as the local industries may be able to buy inputs more cheaply if their suppliers cannot sell as much to the US market as they did before the trade war.

The estimation results incorporating the supply chain channels are reported in Column 1 of Table 16. We find that indeed both greater exposures to the Trump tariffs through either the direct channel or the upstream channel lead to a reduction in US movie viewership. Both effects are statistically significant at the 5% level. The exposure to the Trump tariffs through the downstream channel has a statistically insignificant coefficient, consistent with the intuition discussed above.

A broader interpretation of the Trump trade war against China may also include the non-tariff sanctions on individual Chinese firms or research institutions on (US) national security grounds. Specifically, when an entity is placed on a black list by the US Department of Commerce's Bureau of Industry and Security (BIS), it is subject to additional US license requirements to buy parts and components from the US or sign a technology agreement with a US firm or research institute. A notable example is Huawei, which was placed on the black list in May 2019. There are altogether 48 Chinese entities, located in 16 Chinese cities, that were placed on the black list in 2018 and 2019.

The effect of the black list on the US soft power, similar to the Trump tariffs, is ambiguous ex-ante. If the entities on the black list are regarded "guilty as charged" by Chinese citizens for having conducted immoral or illegal actions, the US actions may raise the Chinese citizens' appreciation of US movies and other US brands. On the other hand, if the US blacklist actions are regarded as unjustified by the Chinese citizens, they may reduce their appreciation for US movies (and other US brands).

Because the entity list is relatively sparse (i.e., no entities are on the US black list for most Chinese cities), we construct a province-level BIS sanction variable. It takes the value of one if any entity from the province has been placed on the BIS black list in 2018 or 2019, and zero otherwise. In Column 2 of Table 16, we examine whether the presence of local entities on the US blacklist affects the local US movie revenue. The coefficient on the BIS sanction dummy is negative and significant at the 5% level. There is a statistically significant but economically modest reduction in the local US movie viewership in the regions with US sanctioned entities. In Column 3, we include both the tariff exposure measures and the US sanction dummy. We find all three effects—the direct channel, the upstream channel, and the BIS sanction effect—remain negative and statistically significant.

5.4 Local Media Coverage

One might wonder if our baseline results reflect the variation across regions in the intensity of media coverage of the trade war rather than the variation in actual tariff exposure. Since the media market is highly regulated in China, we do not expect to see much variation in the way the trade war is discussed across regions. It is reported by Wang and Yu (2018) that the authorities have directed the official media to play down rather than play up the trade war. In addition, local media and internet news portals are instructed to simply carry the contents produced by a subset of approved state media. These reports point to possibly minimal variation across cities in the coverage of the trade war.

We examine this issue more formally with data on newspaper coverage. We use the WiseNews database, which covers Chinese-language newspapers in 187 Chinese cities.[18] We focus on local daily newspapers and exclude ones with national circulation. For each city in each month over 2017–2019, we search and count the number of articles containing

[18]The WiseNews database is similar to Factiva for English-language news media. Qin, Strömberg and Wu (2018) use the WiseNews database to conduct an in-depth analysis of the newspaper markets in China.

either "Sino-US trade war (*zhongmei maoyi zhan*)" or "Sino-US trade frictions (*zhongmei maoyi mocha*)" in the article titles. Adding additional variations of the keywords do not appear to noticeably increase the article count. We use the information to examine two questions: (a) Does local media coverage of the trade war vary by the local exposure to the Trump tariffs? (b) To the extent there is local variation, does the local variation in coverage matter for local US movie revenue?

We first study the effects of tariff exposure on the newspaper coverage among cities with at least one daily newspaper in the WiseNews database. The dependent variable is the 12-month log difference in the count of news stories about the trade war in local media. Columns 1 and 2 of Table 17 report the results from regressions without and with city fixed effects, respectively. We find no association between the local exposure to the Trump tariffs and the intensity of local newspaper coverage.

We then augment the baseline regression on the US movie revenue by adding local newspaper coverage of the trade war as an additional control variable. Besides the restricted sample of cities with at least one daily newspaper in the database, we extend the analysis to the full sample in which the local article count is assumed to be zero for those cities with no newspaper. We report the results for the restricted and full samples in Columns 3 and 4, respectively. While the local exposure to the Trump tariffs continues to depress the local viewership of US movies, there is no statistically significant effect from the count of trade-war-related articles in the local newspaper. This means that the reduction in the viewership of US movies in the more tariff-exposed regions is not driven by more local media coverage of the trade war.

In Columns 5 and 6, we add an interaction term between the local exposure to the Trump tariffs and the count of trade-war-related articles in the local newspapers. We find that the coefficients on the interaction terms are negative in both samples and statistically significant at the 10% level in the full sample. In other words, greater local exposure to the Trump tariffs tends to depress the viewership of US movies. This depressing effect becomes even larger in regions with more local newspaper articles about the trade war.

5.5 Long-term Effects

In November 2020, Joe Biden was elected the president of the United States. While he has not formally rescinded the trade war, his government has used a tariff exclusion program to grant exemption to the Trump tariffs to approved US importers. As these exemptions are somewhat ad hoc, they are not equivalent to revoking the Trump tariffs.

We purchased the disaggregated movie revenue data in China for 2021 and study whether the aversion to US movies persisted to 2021. (We skip 2020 out of a concern for noises due to Covid related closure of movie theaters.) We define the dependent variable as the difference in log US movie revenue in a given city (or theater) between a given month in 2021 and the same month in 2017. The month-to-month match is to filter out possible seasonal patterns in movie viewership. The key regressor is the cumulative exposure to the Trump tariffs. This gives us a panel data across either cities or theaters over 12 calendar months. We include month fixed effects, log GDP per capita changes, and initial GDP per capita as control variables. As in our baseline in Table 4, we cluster the standard errors two-way by city and region-month to correct for possibly correlated errors.

We report the result in Column 1 of Table 18. Panels A and B present the results from the city and the theater samples, respectively. We find that greater local exposure to the trade war continues to depress the viewership of US movies in 2021 (relative to 2017), in both the city and theater samples, with the coefficient from the latter significant at the 5% level. Moreover, a comparison of the point estimates with Column 4 of Table 4 suggests no significant decline in the size of the depressing effect on US movie viewership.

Given China's zero tolerance for the Covid-19 virus during the pandemic, any emergence of a cluster of infections in a city can cause theater closure or other restrictions on movement in that city. From a count of Covid-19 infection cases by city and month in 2021 that we collect from the website of the National Health Commission, we sort city-months into three bins: no reported Covid-19 case, mild outbreak (1-10 cases), and relatively severe outbreak (more than 10 cases). The mean, median, and standard deviation for the number of Covid-19 cases across cities in 2021 are 21.4, 0, and 113.5, respectively. The fraction of city-month observations with mild outbreaks and relatively severe outbreaks is 2.6% and 1.5%, respectively. As expected, Covid-19 cases tend to reduce US movie viewership (Column 2). But holding that constant, greater exposure to the Trump tariff increases continues to translate into a lower viewership of US movies even in 2021.

To put the viewership of US movies in context, we also examine the overall movie revenue in a city, regardless of the country of origin. From Columns 3 and 4 of Table 18, we see that Covid-19 cases (and the associated lockdowns) tend to depress the viewership of all movies. At the same time, the local exposure to the Trump tariffs does not significantly depress the overall movie viewership. This means that the reduction in US movie viewership in 2021 is not because people consume fewer movies in general. Importantly, the evidence also confirms that the effect of the Trump tariffs is persistent at least to 2021.

6 Conclusion

While the American soft power embedded in its movies helps to project its global influence, whether the US trade war, an exercise of its economic might, enhances or diminishes the US ability to project its soft power is unknown in the literature. We find in this paper a significantly negative effect of the trade war on the Chinese viewership of US movies. This effect is not explained by either an income effect or a Chinese government action. The reduction in US movie revenue in China is estimated to be 2.7 billion yuan (or US$ 400 million) during 2018-2019, which is economically large.

This finding is corroborated by the evidence on online search intensity for US movies and US tourism destinations. This further confirms that the fallen US movie viewership is driven mostly by Chinese citizens' choices as opposed to government actions. As there is no corresponding fall in non-US foreign movie revenue, or online searches for non-US foreign movies or non-US tourist destinations, the empirical pattern results from changing attitude towards the US rather than a lower income effect or a general reduction in movie consumption. In other words, the Trump tariffs on goods exports negatively affect US service exports. They also negatively affect the US ability to project its soft power.

We find the effect to be persistent. The aversion to US movies appears as strong in 2021 under President Biden as in 2018–2019 under President Trump. It will be interesting to re-examine the question in 2025 to see whether the effect disappears. By the IMDb data, US movie revenue declined in 47 out of 81 countries from 2017 to 2019. As the decline in US movie viewership is not unique to China, future research could examine if the decline in US soft power is also present in multiple countries.

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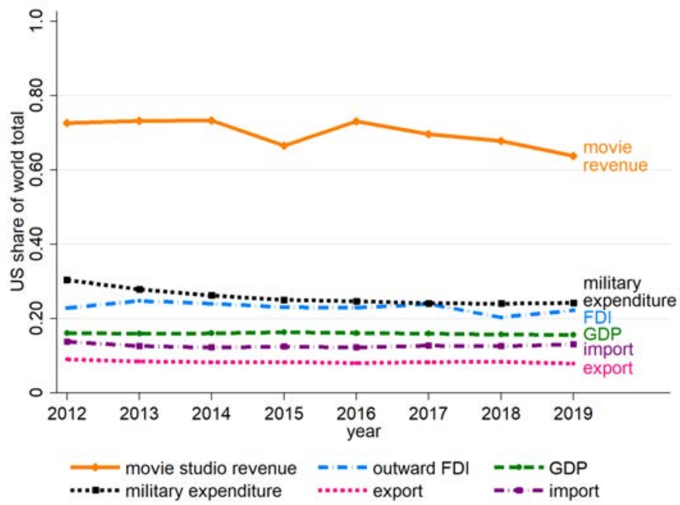
Tables and graphs

Figure 1: US and the motion picture industry

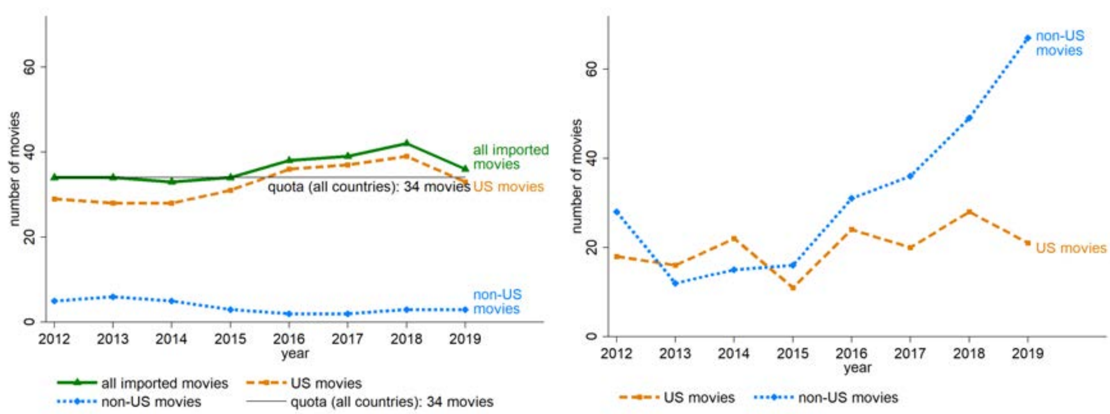


Notes: Calculations by the authors based on data from IMDb, MPAA and various other sources.

Figure 2: number of imported movies over 2012-2019

(a) revenue-sharing imports

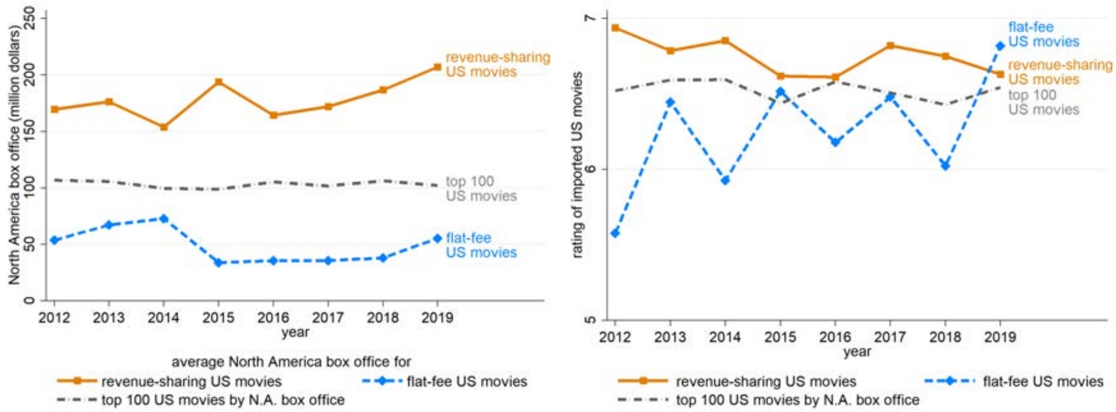
(b) flat-fee imports



Notes: Calculations by the authors based on data from Entgroup.

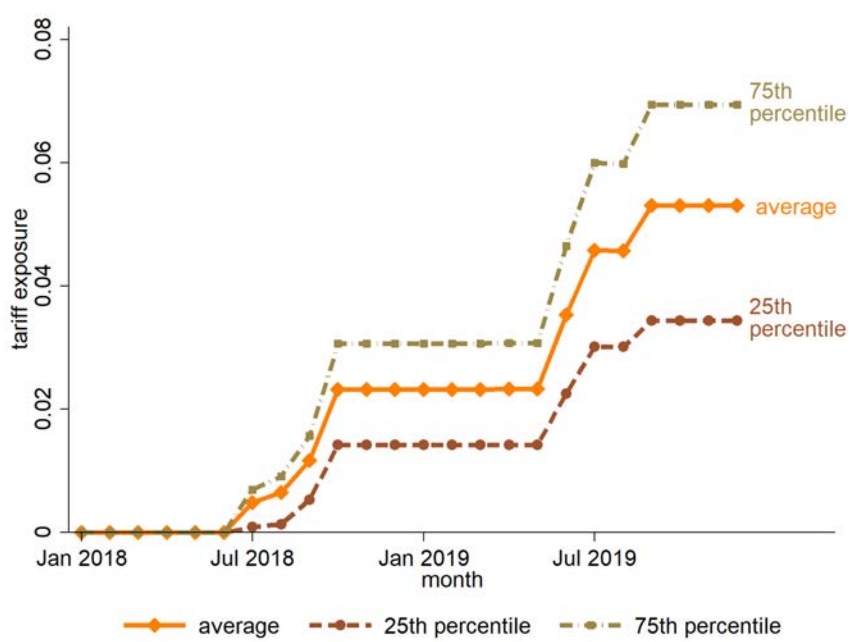
Figure 3: quality of imported US movies over 2012-2019

(a) average movie revenues in North America (b) average IMDb rating



Notes: This figure plots the average quality of imported US movies over 2012-2019. Panel (a) uses the box office revenue in North America as measure of quality, while Panel (b) uses average rating from IMDb.com. For comparison, the figure also plots the quality averages for the top 100 US movies in the North America box office. Calculations by the authors.

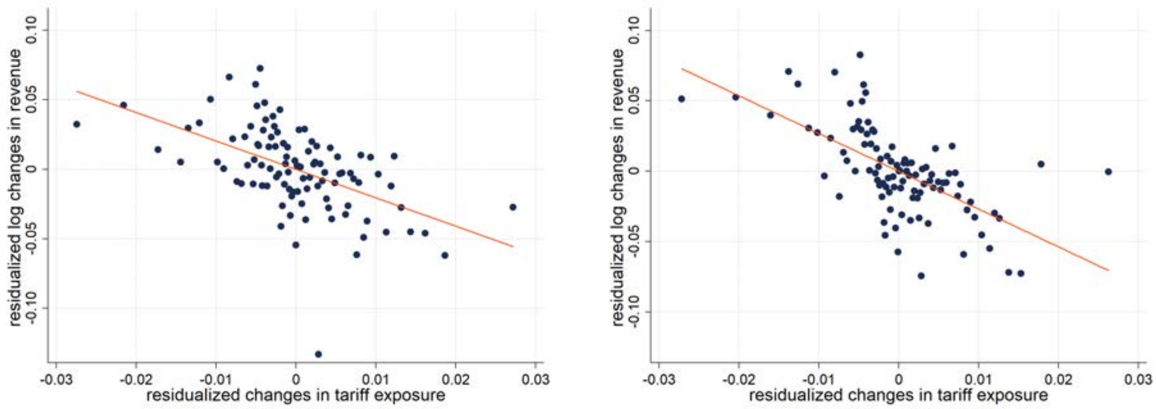
Figure 4: exposure to Trump tariffs over time



Notes: Calculations by the authors.

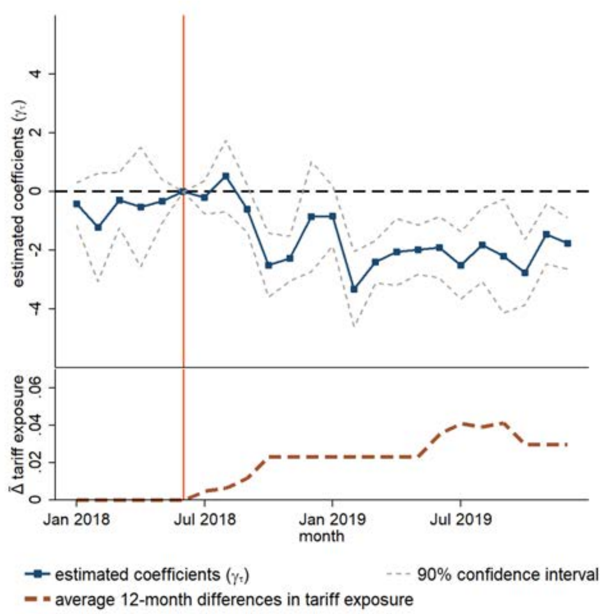
Figure 5: bin-scatter plots for baseline regressions

(a) city-level (b) theater-level



Notes: Panels (a) and (b) presents the binscatter plots of residualized variables corresponding to the city-level regression in Column 4, Panel B of Table 3, and the theater-level regression in Column 4 of Table 4, respectively. For each of the plots, we group the observations into 100 equal-sized bins according to residualized tariff exposure. For the city-level plot, the residualized variables are obtained from regressions of the relevant variables on month and city fixed effects, and changes in log GDP per capita. For the theater-level plot, the residualized variables are obtained from regressions of the relevant variables on month and theater fixed effects and a vector of baseline city controls.

Figure 6: estimated coefficients on month dummies from a differences-in-difference framework



Notes: The upper panel reports estimated γ_τ using Equation 4. See text for further explanations. The lower panel reports average 12-month changes in tariff exposure across all cities in our sample. The correlation between the point estimates of γ_τ and average changes in tariff exposure is -0.747.

Table 1: Summary Statistics of Key Variables

Variables	mean	median	std. dev.	N
movie revenues				
US revenue ('000 yuan)	188.5	66.7	341.8	323865
CN revenue ('000 yuan)	313.3	124.4	488.0	323865
total revenue ('000 yuan)	534.5	298.1	673.1	323865
$\tilde{\Delta}$ log US revenue	-0.230	-0.313	1.154	197715
$\tilde{\Delta}$ log CN revenue	0.011	0.053	1.160	197715
$\tilde{\Delta}$ log total revenue	-0.070	-0.088	0.701	197715
cumulative tariff exposure				
By year				
2018	0.0078	0.0075	0.0048	325
2019	0.0382	0.0372	0.0194	325
By half year				
2018h2	0.0156	0.0149	0.0096	325
2019h1	0.0254	0.0246	0.0137	325
2019h2	0.0511	0.0502	0.0253	325
For selected month				
2018 December	0.0234	0.0226	0.0129	325
2019 June	0.0355	0.0352	0.0180	325
2019 December	0.0535	0.0533	0.0269	325

Notes: Panel A reports the summary statistics of monthly movie revenues in the theater-level regression sample. Movie revenues are measured in thousand yuan and covers 10057 theaters. The variables denoted by $\tilde{\Delta} \log y$ refer to the 12-month differences of $\log y$ and covers 9983 theaters. Missing values of $\tilde{\Delta} \log y$ can result from zeros in y, or if a theater is less than 12 months old. Panel B reports the summary statitics of city-level tariff exposure by year, by half-year, and for selected months.

Table 2: Effects of Tariff Exposure on the Local Economy

	(1)	(2)	(3)	(4)
yearly changes in	log export	export/GDP ratio	log GDP	log GDP per capita
$\tilde{\Delta}$ tariff exposure	-5.763***	-0.622**	-2.054***	-2.076***
	(1.877)	(0.265)	(0.658)	(0.662)
R-square	0.352	0.594	0.572	0.569
city FE	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes
N cities	321	321	325	325
N obs	642	642	650	650

Notes: All regressions include city and year fixed effects. Standard errors in parentheses are clustered by city. ***p < 0.01 **p < 0.05 *p < 0.1.

Table 3: Effects of Tariff Exposure on US Movie Revenue: City-level Regressions

	(1)	(2)	(3)	(4)
Panel A: Single Differencing				
	2017h2-2018h2	2018h1-2019h1	2018h2-2019h2	2017h2-2019h2
Δ tariff exposure	-0.479	-0.951***	-1.006***	-0.507*
	(0.433)	(0.321)	(0.333)	(0.301)
Δ log GDP pc	0.033	0.250***	0.056	0.091**
	(0.040)	(0.070)	(0.043)	(0.045)
R-square	0.046	0.107	0.026	0.027
N obs	325	325	325	325
Panel B: Panel Regressions				
	semi-annual		monthly	
$\tilde{\Delta}$ tariff exposure	-0.915***	-1.723***	-1.291***	-2.077***
	(0.254)	(0.331)	(0.353)	(0.589)
$\tilde{\Delta}$ log GDP pc	0.071**	0.069**	0.045	0.026
	(0.027)	(0.027)	(0.049)	(0.049)
R-square	0.871	0.922	0.962	0.968
time FE	Yes	Yes	Yes	Yes
city FE	No	Yes	No	Yes
N cities	325	325	325	325
N obs	1300	1300	7794	7794

Notes: All regressions control for changes in log number of theaters. Each city-level observation is weighted by the number of theaters of the city in 2017. Panel A reports robust standard errors in parentheses. Standard errors in Columns 1 and 2 of Panel B are clustered by city, while those in Columns 3 and 4 are two-way clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 4: Effects of Tariff Exposure on US Movie Revenue: Theater-level Regressions

	(1)	(2)	(3)	(4)
$\tilde{\Delta}$ tariff exposure	-1.119**	-2.394***	-2.622***	-2.684***
	(0.438)	(0.712)	(0.724)	(0.726)
$\tilde{\Delta}$ log GDP pc	-0.017	0.036	0.059	0.060
	(0.061)	(0.057)	(0.056)	(0.055)
$\tilde{\Delta}$ log population				0.402
				(0.391)
$\tilde{\Delta}$ share rainy days				-0.178**
				(0.080)
$\tilde{\Delta}$ share hot days				-0.140*
				(0.083)
$\tilde{\Delta}$ share cold days				-0.148**
				(0.074)
$\tilde{\Delta}$ share polluted days				-0.153*
				(0.079)
R-square	0.622	0.627	0.698	0.698
city FE	No	Yes	No	No
theater FE	No	No	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N theaters	9983	9983	9983	9983
N obs	197715	197715	197715	197715

Notes: Additional city controls include population and measures of weather and air pollution. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 5: Effects on non-US Movies

	(1)	(2)	(3)
Panel A: $\tilde{\Delta}$ log revenue			
	other foreign	CN	total
$\tilde{\Delta}$ tariff exposure	-0.754	2.299**	0.875
	(2.252)	(1.079)	(0.648)
$\tilde{\Delta}$ log GDP pc	0.515***	0.052	0.079*
	(0.153)	(0.075)	(0.043)
R-square	0.713	0.672	0.448
theater FE	Yes	Yes	Yes
month FE	Yes	Yes	Yes
N theaters	9825	9983	9983
N obs	186376	197715	197715
Panel B: $\tilde{\Delta}$ share of total theater revenue			
	other foreign	CN	US
$\tilde{\Delta}$ tariff exposure	-0.129	0.947***	-0.818***
	(0.117)	(0.291)	(0.248)
$\tilde{\Delta}$ log GDP pc	0.019**	-0.019	0.000
	(0.008)	(0.021)	(0.018)
R-square	0.712	0.779	0.812
theater FE	Yes	Yes	Yes
month FE	Yes	Yes	Yes
N theaters	10057	10057	10057
N obs	202046	202046	202046

Notes: All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 6: Effects on Theater Fill Rates

	(1) US	(2) other foreign	(3) CN
$\tilde{\Delta}$ tariff exposure	-3.148***	-1.263	-0.608
	(1.046)	(1.416)	(1.072)
$\tilde{\Delta}$ log GDP pc	0.142**	0.269***	0.119*
	(0.069)	(0.090)	(0.071)
R-square	0.396	0.392	0.421
theater FE	Yes	Yes	Yes
month FE	Yes	Yes	Yes
N theaters	9983	9824	9983
N obs	197715	186322	197715

Notes: The left-hand-side variable is the 12-month differences in log fill rate, defined as the number of filled seat as a share of all available seats among scheduled sessions, for movies from the relevant country or region. All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 7: Robustness Checks (Theater-level Regressions)

	(1)	(2)	(3)	(4)	(5)
$\tilde{\Delta}$ tariff exposure	-2.799***	-2.799***	-2.709***	-18.393***	-2.576***
	(0.875)	(0.763)	(0.685)	(5.122)	(0.727)
$\tilde{\Delta}$ log GDP pc	0.059	0.059	0.062	0.061	0.060
	(0.055)	(0.054)	(0.055)	(0.055)	(0.053)
$\tilde{\Delta}$ exposure to retaliatory tariffs	0.133				
	(0.527)				
$\tilde{\Delta}$ exposure to announced tariffs		0.294			
		(0.572)			
$\tilde{\Delta}$ log population	0.395	0.409	0.410	0.449	0.388
	(0.392)	(0.396)	(0.393)	(0.395)	(0.337)
$\tilde{\Delta}$ share rainy days	-0.177**	-0.179**	-0.175**	-0.185**	-0.194**
	(0.079)	(0.079)	(0.080)	(0.080)	(0.083)
$\tilde{\Delta}$ share hot days	-0.143*	-0.138*	-0.138*	-0.118	-0.137
	(0.081)	(0.082)	(0.083)	(0.079)	(0.087)
$\tilde{\Delta}$ share cold days	-0.149**	-0.149**	-0.152**	-0.142*	-0.127*
	(0.074)	(0.074)	(0.074)	(0.074)	(0.076)
$\tilde{\Delta}$ share polluted days	-0.154*	-0.153*	-0.153*	-0.152*	-0.165**
	(0.079)	(0.079)	(0.079)	(0.080)	(0.077)
$\tilde{\Delta}$ rating of US films (IMDb)					0.285***
					(0.039)
$\tilde{\Delta}$ rating of CN films (douban)					-0.034***
					(0.011)
R-square	0.698	0.698	0.698	0.698	0.701
theater FE	Yes	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes	Yes
N cities	325	325	325	325	325
N theaters	9983	9983	9983	9983	9983
N obs	197715	197715	197715	197715	197715

Notes: Column 1 controls for exposure to Chinese retaliatory tariffs. Column 2 controls for exposure to announced tariffs. Columns 3 and 4 use alternative measures of exposure to Trump tariffs. Column 3 includes 201/232 tariffs when computing tariff exposure. Column 4 accounts for the share of the US in total exports for each sector when computing tariff exposure. Column 5 controls for the average rating of US and Chinese movies. All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 8: Placebo Tests

	(1)	(2)	(3)	(4)	(5)	(6)
	$\tilde{\Delta}$ log US movie revenue					
placebo years:	2012-2014		2013-2015		2014-2016	
$\tilde{\Delta}$ tariff exposure	0.422	1.542	0.476	-0.811	0.060	-0.102
(2017-2019)	(0.796)	(1.350)	(0.716)	(1.239)	(0.692)	(1.217)
$\tilde{\Delta}$ log GDP pc	-0.037	0.237	0.185*	0.057	0.168**	0.024
	(0.217)	(0.350)	(0.094)	(0.175)	(0.065)	(0.135)
R-square	0.378	0.557	0.591	0.700	0.683	0.757
theater FE	No	Yes	No	Yes	No	Yes
N cities	316	316	321	321	322	322
N theaters	3521	3521	4526	4526	5762	5762
N obs	62725	62725	82193	82193	105010	105010

Notes: Notes: The left-hand-side variable is 12-month changes in log US movie revenue computed from earlier years indicated in the respective column title, while 12-month changes in tariff exposure on the right hand side are computed from 2017-2019 data. All regressions include monthly fixed effects and control for changes in log GDP per capita. Each city-level observation is weighted by the initial number of theaters. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 9: Revenue-Sharing versus Flat-Fee US Movies

	(1)	(2)	(3)	(4)
import arrangement:	revenue sharing		flat fee	
$\tilde{\Delta}$ tariff exposure	-2.186***	-2.267***	-7.094***	-6.790***
	(0.782)	(0.785)	(1.687)	(1.649)
$\tilde{\Delta}$ log GDP pc	0.075	0.073	0.076	0.080
	(0.055)	(0.054)	(0.134)	(0.133)
$\tilde{\Delta}$ IMDb rating		0.310***		0.216***
		(0.030)		(0.066)
R-square	0.695	0.698	0.795	0.796
theater FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N cities	325	325	325	325
N theaters	9967	9967	9516	9516
N obs	196768	196768	108979	108979

Notes: Columns 1 and 2 report results for revenue-sharing US movies, while Columns 3 and 4 report results for flat-fee US movies. All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 10: Effects on the Baidu Index for Trade War or Trade Frictions

keyword	(1) Sino-US trade war	(2) trade war	(3) Sino-US trade friction	(4) composite index
Panel A: Long Difference				
		Δlog (1+Baidu Index)		
Δ tariff exposure	41.378***	38.343***	24.340***	30.321***
	(4.886)	(4.831)	(4.079)	(4.775)
Δ log GDP pc	1.271	0.452	0.897	1.064
	(0.940)	(0.919)	(0.630)	(0.858)
R-square	0.171	0.170	0.102	0.124
N obs	325	325	325	325
Panel B: Panel Regressions				
		log (1+Baidu Index)		
tariff exposure	8.478***	8.580***	5.338**	9.346***
	(2.664)	(2.685)	(2.203)	(2.697)
log GDP pc	0.151	0.138	0.181	0.164
	(0.233)	(0.230)	(0.199)	(0.270)
R-square	0.900	0.904	0.818	0.910
city FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N obs	7800	7800	7800	7800

Notes: Baidu Index measures the number of searches on Baidu.com using relevant terms. The composite index for trade war is constructed by aggregating the Baidu Index for the three trade-war-related keywords. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Robust standard errors are presented in Panel A. Standard errors in Panel B are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 11: Baidu Index for Movies

keyword	(1) US movies	(2) top 5 US movie titles	(3) foreign movies	(4) movie tickets
Panel A: Long Difference				
		Δlog (1+Baidu Index)		
Δ tariff exposure	-6.114***	-3.371***	2.060	-1.259
	(1.389)	(0.995)	(2.129)	(1.916)
Δ log GDP pc	0.261	0.468***	1.106***	0.611***
	(0.177)	(0.162)	(0.288)	(0.219)
R-square	0.104	0.109	0.048	0.022
N obs	325	325	325	325
Panel B: Panel Regressions				
		log (1+Baidu Index)		
tariff exposure	-2.646***	-3.172	-0.414	0.387
	(0.587)	(2.041)	(0.622)	(0.701)
log GDP pc	0.161	0.109	0.153*	0.066
	(0.117)	(0.102)	(0.090)	(0.115)
R-square	0.924	0.969	0.788	0.941
city FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N obs	7800	7800	7800	7800

Notes: Baidu Index measures the number of searches on Baidu.com using relevant terms. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Robust standard errors are presented in Panel A. Standard errors in Panel B are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 12: Baidu Index for Tourism

keyword	(1) US tourism	(2) US visa for tourists	(3) Japanese tourism	(4) tourism
Panel A: Long Difference				
		Δlog (1+Baidu Index)		
Δ tariff exposure	-4.653*	-6.395***	-2.082	-2.107*
	(2.456)	(1.972)	(1.402)	(1.189)
Δ log GDP pc	-0.188	0.347	0.230	0.523***
	(0.308)	(0.271)	(0.180)	(0.160)
R-square	0.011	0.042	0.016	0.071
N obs	325	325	325	325
Panel B: Panel Regressions				
		log (1+Baidu Index)		
tariff exposure	-5.866***	-5.685***	-1.580	0.582
	(0.735)	(0.981)	(0.980)	(0.510)
log GDP pc	-0.300**	0.032	-0.112	0.126
	(0.127)	(0.161)	(0.125)	(0.077)
R-square	0.835	0.874	0.913	0.965
city FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N obs	7800	7800	7800	7800

Notes: Baidu Index measures the number of searches on Baidu.com using relevant terms. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Robust standard errors are presented in Panel A. Standard errors in Panel B are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 13: Baidu Index for Sports Shoes

keyword	(1) Nike (US brand)	(2) Anta (Chinese brand)	(3) ASICS (Japanese brand)	(4) sports shoes
Panel A: Long Difference				
	Δlog (1+Baidu Index)			
Δ tariff exposure	-2.854***	1.211	2.717**	4.925*
	(0.997)	(0.947)	(1.364)	(2.560)
Δ log GDP pc	0.531***	0.605***	0.721***	0.509**
	(0.141)	(0.145)	(0.209)	(0.244)
R-square	0.116	0.070	0.052	0.024
N obs	325	325	325	325
Panel B: Panel Regressions				
	log (1+Baidu Index)			
tariff exposure	-0.635	0.929*	0.443	0.336
	(0.409)	(0.527)	(0.665)	(0.723)
log GDP pc	0.171***	0.189**	0.244**	0.017
	(0.061)	(0.081)	(0.103)	(0.147)
R-square	0.981	0.959	0.942	0.901
city FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N obs	7800	7800	7800	7800

Notes: Baidu Index measures the number of searches on Baidu.com using relevant terms. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of the months of the year. Robust standard errors are presented in Panel A. Standard errors in Panel B are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 14: Baidu Index for Studying Abroad

keyword	(1) US college	(2) UK college	(3) Japanese college	(4) study abroad
Panel A: Long Difference				
	Δlog (1+Baidu Index)			
Δ tariff exposure	-2.677	0.213	-0.177	1.459
	(2.249)	(1.710)	(1.695)	(1.973)
Δ log GDP pc	0.676**	0.309	0.703***	-0.011
	(0.272)	(0.259)	(0.222)	(0.288)
R-square	0.031	0.005	0.028	0.002
N obs	325	325	325	325
Panel B: Panel Regressions				
	log (1+Baidu Index)			
tariff exposure	-1.464*	-0.401	0.103	0.827
	(0.824)	(0.687)	(0.642)	(1.247)
log GDP pc	0.341**	-0.021	0.120	0.136
	(0.152)	(0.127)	(0.136)	(0.192)
R-square	0.866	0.880	0.872	0.856
city FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N obs	7800	7800	7800	7800

Notes: Baidu Index measures the number of searches on Baidu.com using relevant terms. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Robust standard errors are presented in Panel A. Standard errors in Panel B are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 15: Different Movie Genres

	(1)	(2)	(3)	(4)	(5)
genre	action	drama	sci-fi/ fantasy	animation	others
% of total revenue	65.5%	5.3%	13.7%	10.7%	4.8%
Panel A: $\tilde{\Delta}$ log movie revenue					
$\tilde{\Delta}$ tariff exposure	-2.624***	-5.051	0.103	2.557*	2.967
	(0.977)	(4.502)	(2.279)	(1.395)	(2.133)
R-square	0.819	0.709	0.865	0.595	0.763
theater FE	Yes	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes	Yes
N cities	325	324	325	325	325
N theaters	9968	8248	9448	9673	9034
N obs	176964	45795	71239	126971	62422
Panel B: $\tilde{\Delta}$ share of total theater revenue					
$\tilde{\Delta}$ tariff exposure	-0.464*	-0.094**	-0.177	-0.039	-0.043
	(0.255)	(0.047)	(0.125)	(0.038)	(0.056)
R-square	0.855	0.642	0.856	0.643	0.694
theater FE	Yes	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes	Yes
N cities	325	325	325	325	325
N theaters	10057	10057	10057	10057	10057
N obs	202046	202046	202046	202046	202046

Notes: All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of the months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 16: Supply Chains and Sanctions

	(1)	(2)	(3)
$\tilde{\Delta}$ tariff exposure	-2.685***		-2.480***
	(0.709)		(0.746)
$\tilde{\Delta}$ upstream tariff exposure	-4.205**		-4.439***
	(1.632)		(1.596)
$\tilde{\Delta}$ downstream tariff exposure	-2.481		-2.243
	(1.777)		(1.811)
BIS sanction		-0.041**	-0.031*
		(0.018)	(0.016)
R-square	0.698	0.698	0.698
theater FE	Yes	Yes	Yes
month FE	Yes	Yes	Yes
N cities	325	325	325
N theaters	9983	9983	9983
N obs	197715	197715	197715

Notes: The downstream and upstream measures of tariff measure are constructed using 2017 provincial-level input-output linkage. The variable BIS sanction takes the value of one if any entity from the province has been newly (since January 2017) added to the BIS list, and zero otherwise. All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of the months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 17: Local Newspaper Coverage

	(1)	(2)	(3)	(4)	(5)	(6)
dep. var.	$\tilde{\Delta}$ log (1+ N articles)		$\tilde{\Delta}$ log US movie revenue			
$\tilde{\Delta}$ tariff exposure	-0.202	-1.123	-1.702**	-2.679***	-1.830**	-2.807***
	(0.447)	(1.088)	(0.710)	(0.724)	(0.719)	(0.734)
$\tilde{\Delta}$ log (1+N articles)			0.001	0.007	0.022	0.035
			(0.012)	(0.014)	(0.021)	(0.023)
$\tilde{\Delta}$ tariff exposure $\times$					-0.915	-1.259*
... $\tilde{\Delta}$ log (1+N articles)					(0.630)	(0.697)
R-square	0.240	0.247	0.710	0.698	0.710	0.698
city FE	No	Yes	No	No	No	No
theater FE	No	No	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes	Yes	Yes
N cities	187	187	187	325	187	325
N theaters			7879	9983	7879	9983
sample	restricted	restricted	restricted	full	restricted	full
N obs	4488	4488	156248	197715	156248	197715

Notes: "N articles" refers to the number of articles that contains "Sino-US trade war" and "Sino-US trade frictions" in titles published by the local newspapers. The restricted sample includes only cities with at least one daily newspaper appeared in the WiseNews database while the full sample includes all cities from the baseline. All regressions include month FE and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 18: Long-term Effects of the Trade War, 2017-2021

	(1)	(2)	(3)	(4)
	US movies		all movies	
Panel A: City Level				
change in tariff exposure	-1.247	-1.787*	-0.590	-1.035
	(1.041)	(1.010)	(0.888)	(0.868)
Covid-19: 1 to 10 cases		-0.289**		-0.445***
		(0.138)		(0.134)
Covid-19: more than 10		-1.333***		-0.949***
		(0.409)		(0.178)
R-square	0.766	0.780	0.562	0.591
month FE	Yes	Yes	Yes	Yes
N cities	324	324	324	324
N obs	3829	3829	3874	3874
Panel B: Theater Level				
change in tariff exposure	-2.262**	-2.625***	0.035	-0.653
	(1.003)	(0.936)	(0.606)	(0.596)
Covid-19: 1 to 10 cases		-0.073		-0.162**
		(0.106)		(0.080)
Covid-19: more than 10		-0.412**		-0.714***
		(0.160)		(0.160)
R-square	0.556	0.558	0.311	0.323
month FE	Yes	Yes	Yes	Yes
N cities	324	324	324	324
N theaters	7235	7235	7433	7433
N obs	69950	69950	77274	77274

Notes: This table presents regression results based on long differences between 2017 and 2021. The LHS variable is change in log monthly movie revenue from 2021, relative to the same month in 2017. All regressions include changes in log GDP per capita, initial GDP per capita and month fixed effects. The omitted category of covid-19 severity is "0 case." Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Online Appendices for
"Is the American Soft Power a Casualty of the Trade War? Evidence from the Chinese Viewership of US Movies"
(Not for Publication)

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Contents

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Appendix A Data Appendix

Appendix A.1 Movie-related data

Entgroup data: The Entgroup (english.entgroup.cn) data contain rich information on the box office performance, including total revenue, number of sold tickets, number of sessions, and fill rate (the number of sold tickets as a share of available seats) for each movie in each theater. These data are available at the time-of-the-day frequency (mornings, afternoons, evenings, and late nights of each day) for 2017–2019 and 2021 and at the monthly level for 2012–2016. The movie data also include movie-level variables such as movie title (in Chinese), genre, the premiere date in China, and country of origin. For a given film, multiple countries may be listed as the countries of origin. We classify a movie as a US movie if the countries of origin include the US but not China. Similarly, we exclude joint production between the US and China from the classification of Chinese films. We verify that alternative classification of joint-production movies between the US and China does not affect our results.

IMDb: The Internet Movie Database (IMDb, www.imdb.com) provides rich information on movie titles from different countries. We obtain the movie titles, the average user ratings, box office revenue from North America (the US and Canada), and global box office revenue for all US movies released over 2012–2019. We aggregate the box office information to obtain total revenues for all US movies for each year, which are then used to compute the statistics reported by Figure 1. Finally, we merge the IMDb user ratings of each US movie title into the Entgroup database.

Douban: Douban (www.douban.com) is a Chinese social networking service website where registered users can provide reviews and ratings for movies, books, and various events. We obtain the average user ratings for all movie titles in the 2017-2019 Entgroup data. In addition, we retrieve both the Chinese titles and the original English titles for the US movies to facilitate the merge between the IMDb data and the Entgroup data.

MPAA THEME Reports: The Motion Picture Association of America (MPAA) publishes an annual Theatrical and Home Entertainment Market Environment (THEME) report. We obtain the global box office for all films released (regardless of country of origin) from these reports. The data are used in the calculation for US movies' share in global movie revenue, as reported by Figure 1.

Appendix A.2 Tariffs and related data

Tariffs: We compile U.S. tariffs panel data according to public schedules from the Federal Register (https://www.federalregister.gov), following press releases by the Office of the US Trade Representative. The data contain the targeted HS 8-digit products, detailed description of the goods, sourcing countries, and the effective dates. We convert the HS products to 4-digit China Industry Classification (CIC) codes (so as to be matched with the employment shares by industry). Similarly, we construct a monthly panel of retaliatory tariffs based on official documents released by the Chinese Ministry of Finance (http://gss.mof.gov.cn). We scale the tariffs by the number of days they were in effect when computing the effective rates at the monthly, semi-annual, or annual frequency.

2008 Economic Census of China: The 2008 Economic Census of China contains detailed firm-level information for all registered firms in China, such as employment, capital stock, and gross output. We use the data to calculate each 4-digit CIC industry's employment share in each city. The resulting variables are used to construct the local exposure to Trump tariffs.

US sanctions on Chinese entities: The US Department of Commerce's Bureau of Industry and Security (BIS) maintains a list of entities ("entity list" or black list for short) subject to additional US license requirements to buy parts and components from US firms or sign a technology agreement with a US firm or research institute. The Entity List is published by the BIS as Supplement No. 4 to Part 744 of the Export Administration Regulations (EAR). We obtain the names and addresses of all Chinese entities added to the list in 2018 or 2019 from US Commerce Department's website.

Input-Output Table: To construct the city-level upstream and downstream measures of the tariff exposure, we use the 2017 Input-Output Table from the Chinese National Bureau of Statistics. The Chinese IO table is based on the IO industry classification (which is distinct from the CIC classification). To accommodate the different industry classifications, we convert the US tariffs to the IO industry level. Additionally, we calculate the 3-digit IO industry's employment share in each city, using the 2008 Economic Census of China (based on 4-digit CIC classification) and a concordance between 4-digit CIC level and the IO level. Finally, we compute the city-level upstream and downstream tariff exposures according to the equations given in Appendix E.

Appendix A.3 Baidu Indices

Baidu Index: Baidu is the largest Chinese-language search engine in China with about 3/4 of the search market share. Baidu Index is the Chinese counterpart to Google Trends that reflects the search intensity for a given keyword in a specific region and time. We collect data on Baidu Index for a number of keywords related to the US-China trade war, US movies, and other US products and services. We gather each keyword's daily Baidu Index for each city over 2017–2019. The raw daily data of the Baidu Index exhibit discrete jumps: for a given keyword, the smallest values for the daily index are 0, 57, 58, etc., with large density spikes at values that are multiples of 57. We normalize the Baidu Index so that the smallest positive value is one, and then aggregate the daily index to either the monthly or the annual level for each city.

Appendix A.4 Other data

News Counts from WiseNews database: WiseNews is a database that provides full-text content of Chinese-language newspapers and magazines. It covers newspapers located in 187 Chinese cities. We use the database to construct a measure of newspaper coverage on the trade war at the city-month level. We search the WiseNews database for articles with titles containing either "sino-US trade war (zhongmei maoyi zhan)" or "Sino-US trade frictions (zhongmei maoyi mocha)" from 2017 to 2019. To measure relative regional variations, we limit the search among local daily newspapers and exclude the ones with a national circulation. We experiment with additional variations of the search keywords but find no noticeable increase in the article count.

City-level Variables from CEIC: City-level population, GDP, exports, imports, and other socioeconomic indicators at the annual frequency are from the CEIC database.

Weather: The National Climatic Data Center of the US National Oceanic and Atmospheric Administration (NOAA) reports hourly weather information at the monitor station level throughout the world (https://www.ncei.noaa.gov/data/global-hourly). We aggregate the hourly data by monitor station to daily city level by averaging over all monitor stations in a given city. We define a day as rainy, hot, or cold if its total rainfall exceeds 10 millimeters, its average temperature exceeds 30 degrees Celsius, or if it falls below 0 degrees Celsius, respectively. For each month in a city, we compute the shares of rainy days, hot days, and cold days, respectively.

Air pollution: The Ministry of Ecology and Environment (formerly known as the Ministry of Environmental Protection) publishes an air quality index (AQI) for each city at a daily frequency (http://www.cnemc.cn/). We classify a heavy polluting day if the AQI exceeds 150, and compute the share of heavy polluting days in a month for each city-month.

Country-level data: Country-level data on export, import, GDP, and military expenditure are from the World Bank. Data on the stock of outward FDI are from United Nations Conference on Trade and Development (UNCTAD) (UNCTAD, https://unctadstat.unctad.org/wds). We compute the US shares in the global total for exports, imports, GDP, and military expenditure during 2012–2019 and report them in Figure 1.

Trade in goods, services, and movies: US bilateral data on goods and service trade are obtained from US Census Bureau. The share of US movie exports in total US service exports to China is obtained by dividing total revenue accrued to US movies in China, taken from the Entgroup data, by the total US service exports to China. These statistics are discussed in Section 2.1.

Covid-19 cases: From the daily briefing by the National Health Commission of China (NHCC, `http://www.nhc.gov.cn`), we extract the daily number of locally-transmitted Covid-19 cases by city. If the NHCC does not break down the provincial total by city, we go to the press releases by the province and city health commissions to assign the cases to individual cities. We aggregate the case count to the monthly level by city.

Appendix B Diagnostic Tests on Industry Shocks

Appendix B.1 Shock Summary Statistics

Appendix Table A5 (which closely follows Table 1 of Borusyak et al. (2022)) presents the summary statistics of the industry-level Trump tariff shocks. Columns 1–3 present the year-over-year shocks computed at the semi-annual frequency (e.g., from 2018h2 to 2019h2). Column 1 includes the service industry—more precisely an industry aggregated from non-manufacturing industries not subject to the Trump tariffs—which has a shock of zero in each period. When the service-industry shocks are included, the distribution of $\tilde{\Delta}\text{tariff}_{kt}$ is atypical. In particular, the interquartile range is zero. The atypical pattern arises from the service industry's large fraction of total employment (61.9%) and is the same as that in the application of Autor et al. (2013) (see Table 1 of Borusyak et al. (2022)).

The distribution of shocks becomes more regular after excluding the service industry, with a mean of 6.4%, a standard deviation of 7.0%, and an interquartile range of 10.8%. As Column 2 shows, in terms of providing identification variation, the shocks in our context are slightly more favorable to those in Autor et al. (2013) (as reported by Borusyak et al. (2022)). For example, the inverse HHI of the s_{kt} across industry-periods is 341, comparable to the value of 192 in Autor et al. (2013). The largest shock weights are 1.4% across industry-periods, compared to a value of 3.5% in Autor et al. (2013). Therefore, there is substantial variation across industries, which are crucial to the econometric framework of Borusyak et al. (2022). Column 3 of Appendix Table A5 shows that there is still substantial variation in the Trump tariff shocks ($\tilde{\Delta}\text{tariff}_{kt}$) after residualizing on period fixed effects. Columns 4–6 present the year-over-year shocks at the monthly frequency (e.g., from July 2018 to July 2019). The results are essentially the same as the first three columns.

Appendix B.2 Shock Intra-Class Correlations

To examine whether industry-level tariffs shocks are sufficiently uncorrelated, we compute the intra-class correlation coefficients (ICC) of tariff shocks within different industry aggregation levels. This exercise closely follows Table 2 of Borusyak et al. (2022). Specifically, a random effects model is proposed to provide a hierarchical decomposition of residual within-period shock variation:

$$\tilde{\Delta}\text{tariff}_{kt} = \mu_t + a_{\text{sector}(k),t} + b_{\text{cic-3d}(k),t} + c_{\text{cic-2d}(k),t} + d_k + e_{kt}$$

where μ_t are period fixed effects; $a_{\text{sector}(k),t}$, $b_{\text{cic-3d}(k),t}$, and $c_{\text{cic-2d}(k),t}$ are time-varying random effects by sectors, 3-digit CIC groups, and 2-digit CIC groups, respectively; and d_k is a time-invariant industry random effects. The above equation is then estimated as a hierarchical linear model using maximum likelihood and assuming Gaussian residual components. The ICCs can be obtained by computing the share of the overall shock residual variance due to each random effect.

Appendix Table A6 reports the results. The intra-class correlations (ICC) at 4-digit CIC industry and 3-digit CIC group levels are comparable to those from the corresponding levels (SIC-4 and SIC-3) in Autor et al. (2013) (as computed and reported by Borusyak et al. (2022)). The ICCs at the more aggregated 2-digit CIC group and sector levels are somewhat higher than those from the corresponding levels (SIC-2 and industry sector) in Autor et al. (2013). Consequently, for all equivalent industry-level regressions reported in this paper, besides the standard errors at the 3-digit CIC group and sector-month levels, we also compute the standard errors clustered at the 2-digit CIC group and sector-month levels. (A caveat is that although there are 42 2-digit CIC groups in our sample, the effective sample size at this level of aggregation is only 23, as indicated by Appendix Table A5.) We find the resulting standard errors to be essentially the same compared to those clustered at the 3-digit CIC group and sector-month levels.

Appendix C Alternative Methods of Computing Standard Errors

Table A8 reproduce our city-level regressions from Panel B of Table 3 and include six different sets of standard errors. The first set is the Eicker-Hubert-White—or heteroskedasticity-robust—standard errors ("Robust"). The second set is the standard errors clustered by city ("Cluster"), which are the ones provided for the semi-annual regressions in Table 3. The next two sets are the standard errors ("AKM0" and "AKM1") proposed by Adao, Kolesár and Morales (2019), who demonstrate that the conventional standard errors in shift-share regressions could be biased as different regions with similar industry shares may have high correlations in the regression residuals. Since Adao et al. (2019) procedures require the number of industries to be fewer than the number of the locations, we implement their method at the 3-digit CIC level (There are 187 3-digit CIC groups). Throughout Columns 1–4 of Table A8, the AKM0 and AKM1 standard errors are larger than the robust or city-clustered standard errors, consistent with the insight of Adao et al. (2019). Nevertheless, the coefficient on exposure to Trump tariffs remains statistically significant at the conventional level for the different specifications in Columns 1–4.

We provide two additional sets of standard errors for the regressions at the monthly frequency (Columns 3–4). For the fifth set of standard errors, we cluster two-way at both the city and region-month levels (which are the ones provided for the monthly regressions in Table 3). These standard errors are motivated by the concern that US movie revenues could be correlated across cities within the same month. As clustering at the individual month level would result in too few clusters, we use region-month level clustering—where cities are placed into China's east, central, west, or northeast regions—as a compromise. These two-way clustered standard errors ("City Two-way") turn out to be more conservative than the AKMO or AKM1 standard errors, as Table A8 shows.

The last set of standard errors are obtained from equivalent industry regressions, which are noted by Borusyak et al. (2022) to provide convenient alternatives (but not equivalent) to the Adao et al. (2019) standard errors. We cluster this new set of standard errors two-way by 3-digit CIC group and sector-month ("Industry Two-way"). While the industry two-way standard errors are numerically close to the city two-way ones (Column 3), there is a moderate increase from city two-way standard errors to industry two-way ones for the regressions with city fixed effects (Column 4). In any case, the coefficient on exposure to Trump tariffs remains highly statistically significant for both Columns 3 and 4. In sum, our conclusions are unaltered by the above alternative inference methods.

Appendix D Heterogeneity by Theater Attributes

We investigate possible heterogeneity across three theater attributes: average ticket price, theater size (as measured by the number of screens), and age. First, for each theater, we compute the average price of all ticket sales (= sale revenue/ticket number) in 2017. To focus on within-city variation, we normalize the average ticket price by the corresponding city average in that year. Second, we define small, medium, and large theaters as the ones with 1–4 screens, 5–6 screens, and 7 or more screens, respectively. We take small theaters as the omitted category in the regressions. Third, we distinguish between new and older theaters, with those established in 2014 or later as the new ones. Approximately half of the theaters (52% to be exact) are new.

We introduce the interaction terms between each of the theater attributes and the local exposure to the Trump tariffs. Appendix Table A11 presents the new regression results. From Column 1, where only the interaction term between log average ticket price of a theater and the tariff exposure is added, we find this new regressor has a positive and significant coefficient. To the extent that relatively more affluent Chinese visit more expensive theaters, this suggests that the same Trump tariffs may generate a smaller negative effect on these households' attitudes towards the United States. In concrete terms, for theaters charging a price at the 25th percentile, an increase in the tariff exposure by one standard deviation (which is 0.0269, see Table 1), the US movie revenue would decline by 9.5 percent. In comparison, for theaters charging a price at the 75th percentile, the same increase in the tariff exposure would reduce the US movie revenue by 5.2 percent.

In Column 2 of the same table, we add the interaction terms between the tariff exposure, and both the theater size dummies and the dummy for new theaters. The coefficients on the newly added regressors are all negative and statistically significant. In particular, moviegoers to larger or newer theaters display a significantly greater aversion to US movies during the trade war. On the other hand, the coefficient for theaters with a higher average price continues to be positive and statistically significant. So the more moderate reaction to the trade war by moviegoers to fancier theaters appears to be a robust result.

Appendix E Constructing Upstream and Downstream Tariff Exposures

Upstream Exposure: The upstream exposure to the Trump tariffs measures a city's indirect exposure to the Trump tariffs due to a decline of the sales of the intermediate goods by the firms in the city to those Chinese firms that export to the US directly. Even without selling to the US directly, some firms in a city can be hurt indirectly by the Trump tariffs if their buyers who are direct exporters to the US are reducing their production scale.

We use the 2017 provicial-level input-output table to compute the weighted upstream exposure for each sector and province. For a given sector k in province i, the upstream exposure is the sales-weighted average of the exposure to those sectors exporting to the US:

$$\Delta\tau^{\text{up}}_{i,k,t} = \sum_{g} w^{\text{up}}_{i,k,g} \times \Delta\,\text{tariff}_{gt}$$

where the weight, $w^{\text{up}}_{i,k,g'}$, are computed as $w^{\text{up}}_{i,k,g} = \frac{Z_{i,k,g}}{\sum_{g'} Z_{i,k,g'}}$. The numerator in the weight represents the sales of intermediate input by industry k in province i to industry g, whereas the denominator is total sale by industry k in province i to all industries.

Finally, we compute the city-level upstream exposure with the weights proportional to each industry's share in local employment. More precisely, it is calculated as:

$$\Delta \text{tariff}_{ct}^{\text{up}} = \sum_{k} \frac{L_{co}^{k}}{L_{c0}} \times \Delta \tau_{i,k,t}^{\text{up}}$$

Downstream Exposure: The downstream exposure measures a city's indirect exposure to the Trump tariffs due to a potential cost decline as the firms in the city buy the inputs from those Chinese firms that export directly in the United States. In other words, even if some firms do not participate directly in the trade with the US, they could potentially benefit indirectly from the Trump tariffs if other Chinese firms that normally sell their intermediate goods to the United States may have to increase sales at home due to the trade war. The city-level downstream exposure is also calculated based on the 2017 provicial-level input-output table.

For a given industry k in province i, the downstream exposure is a weighted average of all of its input exposure to the Trump tariff:

$$\Delta \tau_{i,k,t}^{\text{down}} = \sum_{g} w_{i,g,k}^{\text{down}} \times \Delta \text{ tariff}_{gt}$$

where the industry weights $w_{i,g,k}^{\text{down}}$ are computed as $w_{i,g,k}^{\text{down}} = \frac{Z_{i,g,k}}{\sum_{g'} Z_{i,g',k}}$. The numerator in the weight represents industry g's sales to industry k in province i as the latter's intermediate input, whereas the denominator is all intermediate inputs used by industry k in province i.

Finally, we compute the city-level downstream exposure with the weights proportional to each industry's share in local employment. More precisely, it is calculated as:

$$\Delta \text{tariff}_{ct}^{\text{down}} = \sum_{k} \frac{L_{co}^{k}}{L_{c0}} \times \Delta \tau_{i,k,t}^{\text{down}}$$

References

Adao, Rodrigo, Michal Kolesár, and Eduardo Morales, "Shift-share designs: Theory and inference," *The Quarterly Journal of Economics*, 2019, *134* (4), 1949–2010.

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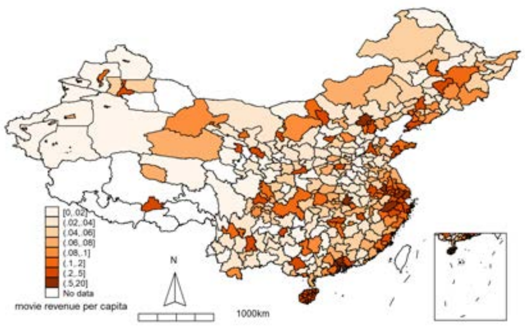
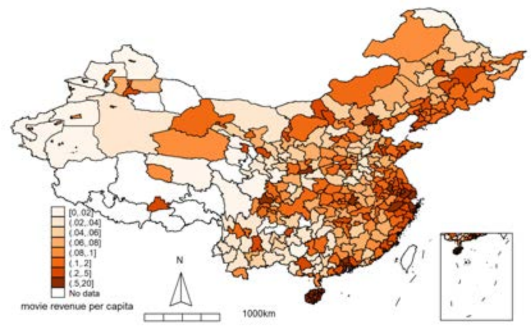
Appendix tables and graphs

Appendix Figure A1: Movie Revenues across Chinese Cities

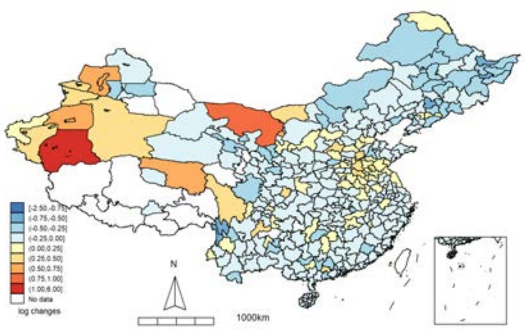
(a) US revenue per capita in 2017



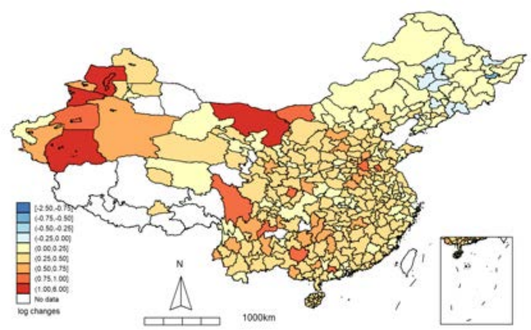
(b) CN revenue per capita in 2017



(c) US revenue growth (2017-2019)

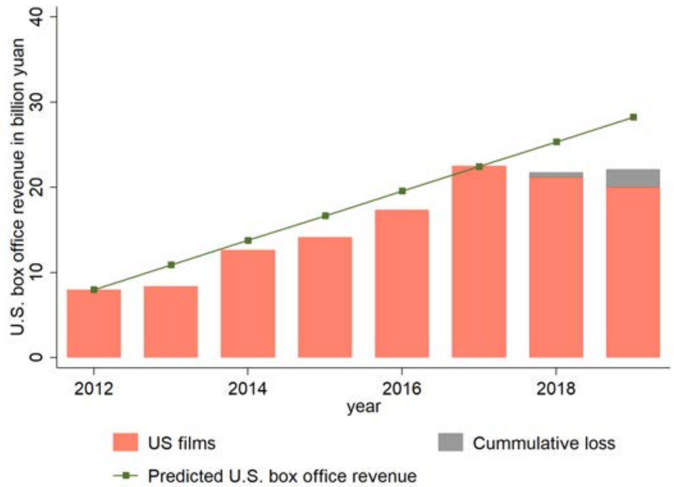


(d) CN revenue growth (2017-2019)



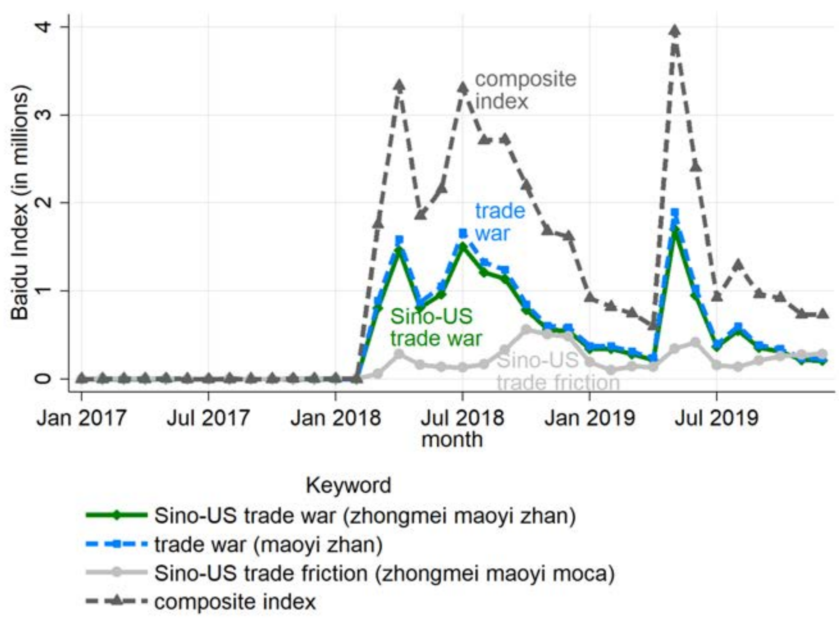
Notes: Movie revenue per capita is measured in yuan. Calculations by the authors.

Appendix Figure A2: Revenue Growth of US Movies and Inferred Loss from the Trade War



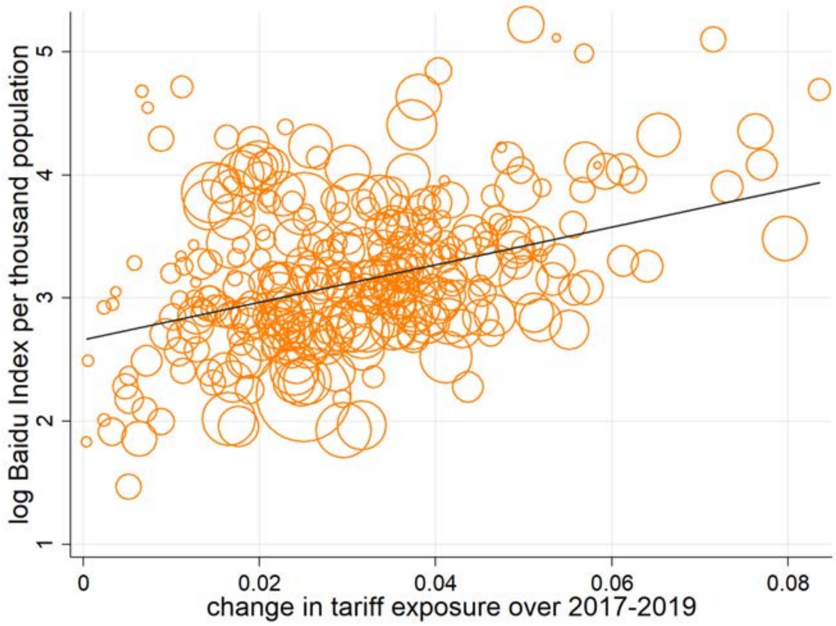
Notes: calculations by authors.

Appendix Figure A3: Baidu Index for Trade War and Trade Frictions over Time



Notes: We construct the composite index for trade war by aggregating the Baidu Index for the three trade-war-related keywords.

Appendix Figure A4: Baidu Index for Trade War and Trade Frictions across Cities.



Notes: The figure presents a scatter plot between the cumulative composite index over 2017-2019 per thousand population and change in tariff exposure over the same period. The composite index is constructed from aggregating the Baidu Index for the three trade-war-related keywords. The size of each bubble corresponds to city population in 2017.

Appendix Table A1: Average Ticket Price of Domestic and US Movies

session	all			prime sessions		
country	CN	US	difference	CN	US	difference
2017						
average price of top 20 movies	34.8	35.1	0.3	34.9	35.4	0.5
	(0.4)	(0.4)	(0.6)	(0.5)	(0.4)	(0.6)
average price of all movies	34.6	34.9	0.4	34.8	35.2	0.4
	(0.4)	(0.3)	(0.5)	(0.5)	(0.3)	(0.6)
N movie titles	313	57	370	313	57	370
2019						
average price of top 20 movies	36.8	36.2	-0.7	37.3	36.6	-0.7
	(0.8)	(0.8)	(1.1)	(0.9)	(0.8)	(1.2)
average price of all movies	37.3	37.7	0.4	38.0	37.9	-0.0
	(0.8)	(2.0)	(2.1)	(0.9)	(1.7)	(2.0)
N movie titles	303	53	356	303	53	356

Notes: Robust standard errors are reported in parentheses. Results for top 20 movies are unweighted, while those for all movies are weighted by total revenue. Prime sessions refer to afternoon and evening sessions on weekends or public holidays and Friday evenings or evening before public holidays.

Appendix Table A2: Trade-war Tariffs by 2-digit CIC Industries

Chinese Industrial Classification (2-digit code):	tariff rate	employment share
Processing of Food from Agricultural Products (13)	14.21	1.26
Manufacture of Foods (14)	16.02	0.62
Manufacture of Beverages (15)	15.01	0.45
Manufacture of Tobacco (16)	15.97	0.08
Manufacture of Textile (17)	20.06	2.61
Manufacture of Textile Wearing Apparel, Footware, and Caps (18)	15.50	1.81
Manufacture of Leather, Fur, Feather and Related Products (19)	21.11	1.09
Processing of Timber, Manufacture of Wood, Bamboo, Rattan, Palm, and Straw Products (20)	19.81	0.53
Manufacture of Furniture (21)	24.76	0.41
Manufacture of Paper and Paper Products (22)	22.02	0.61
Printing, Reproduction of Recording Media (23)	17.80	0.33
Manufacture of Articles For Culture, Education and Sport Activity (24)	11.64	0.53
Processing of Petroleum, Coking, Processing of Nuclear Fuel (25)	16.03	0.34
Manufacture of Raw Chemical Materials and Chemical Products (26)	19.94	1.57
Manufacture of Medicines (27)	1.36	0.61
Manufacture of Chemical Fibers(28)	23.08	0.18
Manufacture of Rubber (29)	20.89	0.39
Manufacture of Plastics (30)	20.25	1.02
Manufacture of Non-metallic Mineral Products (31)	21.89	1.99
Smelting and Pressing of Ferrous Metals (32)	14.44	1.23
Smelting and Pressing of Non-ferrous Metals (33)	9.22	0.61
Manufacture of Metal Products (34)	20.36	1.30
Manufacture of General Purpose Machinery (35)	23.14	1.96
Manufacture of Special Purpose Machinery (36)	19.95	1.15
Manufacture of Transport Equipment (37)	18.47	1.76
Manufacture of Electrical Machinery and Equipment (39)	21.05	2.11
Manufacture of Communication Equipment, Computers and Other Electronic Equipment (40)	19.01	2.57
Manufacture of Measuring Instruments and Machinery for Cultural Activity and Office Work (41)	22.20	0.47
Manufacture of Artwork (42)	17.32	0.53
Recycling and disposal of waste (43)	16.48	0.04

Notes: Industry averages of tariff rates are computed from tariff rates at HS-10 level using export values as weights. Employment share are computed from 2008 Economic Census of China.

Appendix Table A3: List of Top 20 4-digit Industries by Rotemberg Weight

rank	CIC 4d code	industry description	Rotemberg weight
1	4061	electronic parts and components manufacturing	0.0696
2	1810	textile and garment manufacturing	0.0443
3	1711	cotton and chemical fiber textile processing	0.0415
4	1921	leather shoe manufacturing	0.0238
5	4043	computer peripheral equipment manufacturing	0.0223
6	3132	construction ceramic products manufacturing	0.0203
7	3931	wire and cable manufacturing	0.0192
8	4059	manufacturing of optoelectronic devices and other electronic devices	0.0186
9	1923	luggages and bags manufacturing	0.0178
10	4062	printed circuit board manufacturing	0.0176
11	1712	cotton and chemical fiber printing and dyeing finishing	0.0171
12	2110	wooden furniture manufacturing	0.0160
13	3972	lighting fixture manufacturing	0.0146
14	3625	mold making	0.0136
15	4053	manufacturing of integrated circuits	0.0133
16	3924	manufacturing of power electronic components	0.0128
17	3351	commonly used non-ferrous metal rolling processing	0.0128
18	3921	Transformer, Rectifier and Inductor Manufacturing	0.0122
19	3940	battery manufacturing	0.0118
20	3923	Distribution switch control equipment manufacturing	0.0117

Notes: Rotemberg weights are computed from our city-level panel data following Goldsmith-Pinkham et al. (2020). We report the top 20 4-digit industries by Rotemberg weight in this table.

Appendix Table A4: Robustness to Excluding High-Rotemberg-weight Industries

	(1)	(2)	(3)	(4)	(5)	(6)
		Semi-annual			Monthly	
	benchmark	excl. top 3	excl. top 10	benchmark	excl. top 3	excl. top 10
$\tilde{\Delta}$ tariff exposure	-1.723***	-1.912***	-1.972***	-2.077***	-2.170***	-2.280***
	(0.331)	(0.335)	(0.355)	(0.589)	(0.619)	(0.644)
$\tilde{\Delta}$ log GDP pc	0.069**	0.065**	0.066**	0.026	0.023	0.024
	(0.027)	(0.027)	(0.027)	(0.049)	(0.049)	(0.049)
R-square	0.922	0.922	0.922	0.968	0.968	0.968
city FE	Yes	Yes	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes	Yes	Yes
N cities	325	325	325	325	325	325
N obs	1300	1300	1300	7794	7794	7794

Notes: Columns 1 and 4 reproduce the city-month regressions from Table 3. Columns 2 and 5 exclude the three industries with largest Rotemberg weights. Columns 3 and 6 exclude the ten industries with largest Rotemberg weights. All regressions control for changes in log number of theaters and log GDP per capita. Each city-level observation is weighted by the number of theaters of the city in 2017. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in Columns 1–3 are clustered by city, while those in Columns 4–6 are two-way clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Appendix Table A5: Shock Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
		Semi-annual			Monthly	
Mean	0.0243	0.0636	0.0000	0.0243	0.0636	0.0000
Standard deviation	0.0531	0.0699	0.0565	0.0557	0.0750	0.0606
Interquartile range	0.0000	0.1078	0.0484	0.0000	0.1071	0.0482
Specification						
Excluding service industries		✓	✓		✓	✓
Residualizing on period FE			✓			✓
Effective sample size (1/HHI of s_{kt} weights)						
Across industries and periods	10.4	341.4	341.4	62.4	2048.5	2048.5
Across 3-digit CIC groups	2.6	62.3	62.3	2.6	62.3	62.3
Across 2-digit CIC groups	2.6	22.7	22.7	2.6	22.7	22.7
Largest s_{kt} weights						
Across industries and periods	0.155	0.014	0.014	0.026	0.002	0.002
Across 3-digit CIC groups	0.619	0.057	0.057	0.619	0.057	0.057
Across 2-digit CIC groups	0.619	0.076	0.076	0.619	0.076	0.076
Observations counts						
N industry-period shocks	2012	2008	2008	12072	12048	12048
N 4-digit CIC industries	503	502	502	503	502	502
N 3-digit CIC groups	187	186	186	187	186	186
N 2-digit CIC groups	42	41	41	42	41	41

Notes: This table summarizes the distribution of Trump tariff shocks across industries k and periods t. Columns 1–3 report statistics for shocks at the semi-annual frequency while Columns 4–6 are at the monthly frequency. All statistics and weighted by the average industry employment share s_{kt}, which are normalized to add up one in the entire sample. Columns 1 and 4 include an aggregated service industry, while all other columns exclude the aggregated service industry. Columns 3 and 6 residualize the tariff shocks on period indicators.

Appendix Table A6: Shock Intra-Class Correlations

	Estimate	SE
Shock ICCs		
CIC sector	0.195	0.066***
2-digit CIC group	0.270	0.014***
3-digit CIC group	0.221	0.025***
4-digit CIC Industry	0.070	0.010***
Period means		
2018 h1	-0.012	0.004***
2018 h2	0.023	0.014
2019 h1	0.047	0.021**
2019 h2	0.074	0.027***
N industry-periods	2008	

Notes: This table reports intra-class correlation coefficients, estimated from a hierarchical model described in Borusyak et al. (2022). Tariff shocks are computed at the semi-annual frequency. Results are obtained from a maximum likelihood estimator with period fixed effects and an exchangeable covariance structure for each industry and sector random effect. The aggregated service industry is excluded from the estimation. Robust standard errors are reported in parentheses.

Appendix Table A7: Balance Tests

	Coefficient	SE	N cities
US revenue growth 2012-2014	0.03242	0.03049	318
US revenue growth 2013-2015	-0.00253	0.02900	322
US revenue growth 2014-2016	-0.00409	0.03516	322
change in log population	0.00032	0.00097	325
change in manufacturing share	-0.00185	0.00161	285
change in Internet ratio	0.00011	0.01242	295

Notes: This table reports coefficients from regressions of city-level covariates on the shift-share instrument (the tariff exposure measure), controlling for month indicators. Standard errors in parentheses are obtained from equivalent 4-digit CIC industry-level regressions following Borusyak et al. (2022) and two-way clustered by 3-digit CIC group and sector-month. Independent variables are normalized to have a variance of one in the sample. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Appendix Table A8: Alternative Standard Errors for City Panel Regressions

	(1)	(2)	(3)	(4)
	semi-annual		monthly	
$\tilde{\Delta}$ tariff exposure	-0.915	-1.723	-1.291	-2.077
standard errors:				
robust	0.211***	0.342***	0.187***	0.285***
cluster	**(0.254***)**	**(0.331***)**	0.256***	0.342***
AKM0	0.355**	0.394***	0.311***	0.451***
AKM1	0.304***	0.350***	0.305***	0.444***
City Two-way	—	—	**(0.353***)**	**(0.589***)**
Industry Two-way	—	—	0.344***	0.801***
city FE	No	Yes	No	Yes
month FE	Yes	Yes	Yes	Yes
N cities	325	325	325	325
N obs	1300	1300	7794	7794

Notes: We report six sets of standard errors in this table. Robust: heteroscedasticity-robust standard errors. Cluster: standard errors clustered by city. AKM0 and AKM1: standard errors computed using the AKM0 and AKM1 procedures in Adao et al. (2019), implemented at the CIC 3-digit level (The Adao et al. (2019) procedures require the number of industries to be fewer than the number of the locations); Location two-way: standard errors two-way clustered by city and region-month; Industry two-way: standard errors obtained from equivalent CIC 4-digit industry regressions following Borusyak et al. (2022) and two-way clustered by CIC 3-digit group and sector-month. Standard errors that are reported in Table 3 are highlighted with parentheses. All regressions control for changes in log number of theaters. Each city-level observation is weighted by the number of theaters of the city in 2017. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. ***p < 0.01, **p < 0.05, *p < 0.1.

Appendix Table A9: Different Show Times

	(1)	(2)	(3)	(4)
Panel A.	times of the day			
	morning	afternoon	evening	late night
$\tilde{\Delta}$ tariff exposure	-2.017**	-2.234***	-2.517***	-2.483***
	(0.808)	(0.729)	(0.651)	(0.804)
R-square	0.590	0.653	0.710	0.688
revenue share	6.9	40.9	37.4	15.0
theater FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N theaters	8562	9684	9849	9028
N obs	159376	190070	192731	169624
Panel B.	day and prime sessions of the week			
	weekends & holidays	workdays	prime sessions	non-prime sessions
$\tilde{\Delta}$ tariff exposure	-2.302***	-2.589***	-2.113***	-2.622***
	(0.769)	(0.695)	(0.681)	(0.760)
R-square	0.698	0.722	0.698	0.698
revenue share	53.1	46.9	51.4	48.5
theater FE	Yes	Yes	Yes	Yes
month FE	Yes	Yes	Yes	Yes
N theaters	9838	9870	9853	9847
N obs	193880	194735	192994	193282

Notes: Prime sessions refer to afternoon and evening sessions on weekends or public holidays and Friday evenings or evening before major holidays. All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Appendix Table A10: Revenue and Sessions Share by Different Show Times

	(1) evening	(2) weekends/ holidays	(3) prime
A. share of total revenue			
$\tilde{\Delta}$ tariff exposure	0.241	0.024	0.330**
	(0.166)	(0.091)	(0.141)
R-square	0.162	0.318	0.253
B. share of total number of sessions			
$\tilde{\Delta}$ tariff exposure	0.159	-0.031	0.181**
	(0.097)	(0.031)	(0.082)
R-square	0.178	0.403	0.275
theater FE	Yes	Yes	Yes
month FE	Yes	Yes	Yes
N cities	325	325	325
N theaters	9983	9983	9983
N obs	197715	197715	197715

Notes:Prime sessions refer to all afternoon and evening sessions on weekends and public holidays, and the evening sessions before such days. All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.

Appendix Table A11: Heteorogeneity of Effects across Theaters

	(1)	(2)
$\tilde{\Delta}$ tariff exposure	-2.243***	2.698**
	(0.712)	(1.090)
$\tilde{\Delta}$ tariff exposure $\times$ log theater price	10.437***	11.481***
	(2.475)	(2.560)
$\tilde{\Delta}$ tariff exposure $\times$ if medium theater		-2.520***
		(0.724)
$\tilde{\Delta}$ tariff exposure $\times$ if large theater		-4.027***
		(1.141)
$\tilde{\Delta}$ tariff exposure $\times$ if new theater		-4.504***
		(0.760)
R-square	0.703	0.705
theater FE	Yes	Yes
month FE	Yes	Yes
N cities	325	325
N theaters	8800	8800
N obs	188980	188980

Notes: The table reports regressions in which "$\tilde{\Delta}$ tariff exposure" is interacted with theater characteristics. Average price is computed from the 2017 ticket sales of all movies of the theater and is normalized by the average ticket price of the city. We classify the theaters into three size categories according to the number of screens: small (1-4 screens), medium (5-6 screens) and large (7 or more screens). We take small theaters as the omitted category in the regressions. New theaters refer to those were established in 2015 or later. All regressions include month and theater fixed effects, and baseline city controls. Month FE refers to fixed effects for specific months (e.g., 2018 July) instead of months of the year. Standard errors in parentheses are clustered by city and region-month. ***p < 0.01, **p < 0.05, *p < 0.1.