Journal of International Economics 150 (2024) 103926

Contents lists available at ScienceDirect

Journal of International Economics

journal homepage: www.elsevier.com/locate/jie

Full length articles

Illuminating the effects of the US-China tariff war on China's economy[☆]

Davin Chor [a,b,*], Bingjing Li [c]

[a] Dartmouth, United States of America
[b] NBER, United States of America
[c] HKU, Hong Kong

ARTICLE INFO

JEL classification:
E01
F10
F13
F14
F16

Keywords:
US-china tariff war
Night lights

ABSTRACT

This paper studies the impact of the US-China tariff war on China, using high-frequency night lights data and grid-level measures of tariff exposure. Exploiting within-grid variation over time and controlling extensively for grid-specific contemporaneous trends, we find that each one-percentage-point increase in exposure to the US tariffs was associated with a 0.59% reduction in night-time luminosity. This impact was highly skewed across locations: Grids with negligible direct exposure to the US tariffs accounted for 70% of China's population. But the tail 2.5% of China's population with the highest exposure saw an implied 2.52% (1.62%) decrease in income per capita (employment) relative to unaffected grids. These effects were moreover concentrated in locations with a high commuting openness. By contrast, we do not find significant effects from China's retaliatory tariffs, and offer evidence of several channels through which the impact on imported inputs was mitigated. In a parallel analysis at the prefecture level, we confirm that the US tariffs had discernible negative aggregate consequences.

1. Introduction

In early 2018, the US and China started engaging in a series of high-profile tariff actions that escalated over the next two years. This steadily unwound the progress on trade liberalization and economic cooperation achieved since the mid-1990s between the two countries. At the height of this "tariff war" in September 2019, US tariffs on China had surged by 20.7 percentage points on average; these increases covered 93.0% of all Harmonized System (HS) 6-digit products, that made up 14.2% of the value of China's total exports (or 74.7% of China's exports to the US) in 2017. In response, China enacted retaliatory tariffs on goods from the US

☆ We thank Costas Arkolakis (editor) and two expert referees for their valuable comments; we are grateful also to Mary Amiti, Andy Bernard, Emily Blanchard, Matilde Bombardini, Chad Bown, Lorenzo Caliendo, Filipe Campante, Mi Dai, Teresa Fort, Gordon Hanson, Chang-Tai Hsieh, James Lake, Paul Raschky, Steve Redding, Zheng (Michael) Song, Robert Staiger, Xiaodong Zhu, as well as audiences at Yale, Harvard/MIT, Groningen, Singapore Management University, Academia Sinica, the NBER Trade and Trade Policy in the 21st Century Conference, NBER Chinese Economy Working Group Meeting, China Economics Summer Institute, Melbourne Trade Workshop, Asia Meeting of the Econometric Society, the 1st Annual Conference on China and the Global Economy, the 17th APTS meeting, Workshop on International Economic Networks, the 15th Australasian Trade Workshop, Joint PRN, ADBI and INSEAD EMI, University of Tokyo and Hitotsubashi University Conference, and the Trade, Firms, and Economic Development Workshop (Bad Homburg). Bashudha Dhamala, Sirig Gurung, Kshitij Jain, Jinci Liu, Wei Lu, Hanchen Qiu, and Yang Xu delivered excellent research assistance.

* Corresponding author at: Dartmouth, United States of America.
E-mail addresses: davin.chor@dartmouth.edu (D. Chor), bingjing@hku.hk (B. Li).

https://doi.org/10.1016/j.jinteco.2024.103926
Received 18 October 2023; Received in revised form 2 April 2024; Accepted 5 April 2024
Available online 16 April 2024

averaging 16.6 percentage points; these affected 84.3% of HS 6-digit products, that accounted for 5.6% of the value of China's total imports (or 66.0% of imports from the US) in 2017.[1]

This paper studies the impact of these tariffs on China. Did higher US tariffs exert downward pressure on economic activity in China, and if so, by how much? Did China's retaliatory tariffs have a similar effect, by limiting access to inputs from the US? There have to date been relatively few studies assessing this impact on China, due in no small part to reporting lags and limited access to Chinese data especially at the sub-national level. To the best of our knowledge, firm-level customs data for 2018 and after are not yet publicly available to researchers, nor do the Chinese authorities have a consistent practice of releasing statistics on output and employment at the local level for detailed industries.[2] There is moreover no prevailing consensus on how severely the tariff war affected China's economy, given that China was already experiencing a slowdown prior to the trade disputes, which could reflect other macroeconomic shocks with a timing coincident with the tariffs.[3] A key aim of this paper is therefore to identify and quantify the effects of the US-China tariffs on economic outcomes across locations in China.

To overcome data constraints, we use satellite readings on night-time luminosity. Total visible light emitted from Earth's surface at night has become a common proxy for local economic performance, given its strong correlation with conventional measures such as GDP per capita. The night lights data have several advantages. First, it has a high spatial resolution; our analysis works with nearly one hundred thousand 11-km-by-11-km grid cells that cover mainland China. Second, the data is less subject to manipulation and censoring compared to official statistics, a relevant concern in the context of China (Nakamura et al., 2016; Chen et al., 2019).

Fig. 1 provides an illustration of the detailed variation in the night lights data. The area shown is Suzhou, a prefecture-level city in the coastal province of Jiangsu. In it, we have outlined two regions – the New & Hi-tech Zone (Huqiu District) in red, and the Suzhou Industrial Park in green – which both have a high concentration of manufacturing firms; for example, the two largest categories of exports by value in the Industrial Park were electronics products and machinery, equipment, and components.[4] Night lights dimmed between Q1/2018 to Q1/2019 across Suzhou, but there was substantial within-city variation. The year-on-year change in mean log night lights was −0.105, −0.085, and −0.067 for the New & Hi-tech Zone, the Industrial Park, and the rest of Suzhou respectively. The larger decline in night lights in industrial districts with a high export exposure hints at a link from the tariffs to an adverse impact on economic activity. If US tariffs shrank export orders for Chinese firms, the resulting contraction in production and in their labor demand would in principle reduce lights emitted from factory night-time operations and from the worker dormitories often situated adjacent to these factories.

To establish the causal nature of this link, we adopt a Bartik (or shift-share) design. We construct a measure of exposure to the US tariffs for each micro-geographic location in China that is based on the initial product composition of its exports. Intuitively, grids that specialized in selling products to the US that were then hit by tariffs were more directly exposed to the resulting decline in export demand. At the same time, China's retaliatory tariffs could have disrupted production for firms that source inputs from the US. To explore this, we construct a second Bartik measure that combines the initial composition of a location's intermediate and capital goods imports with product-level retaliatory tariffs. As described, this empirical strategy requires information on the structure of trade flows at the detailed grid level. We assemble this using web-mapping services (Google Maps and Amap) to geo-locate firms in the 2016 China customs dataset; we will use the Bartik variables constructed from geo-coordinates from one mapping service to instrument for the corresponding variables built from the second, to address possible coefficient attenuation due to measurement error in any individual source.

We perform our main analysis on a grid-level panel constructed at the quarterly frequency. Specifically, we examine the impact of the tariff shocks that unfolded over the period Q1/2018-Q3/2019 on the year-on-year growth in night lights intensity over Q2/2018-Q4/2019. We find that each one-percentage-point increase in exposure to the US tariffs lowered night lights intensity by 0.59 log points, this being a point estimate on the conservative end of the magnitudes across different specifications. By contrast, we do not find statistically significant effects for the retaliatory tariffs on inputs, and discuss several potential explanations. In particular, we document a striking increase in the share of imports from the US conducted under the processing trade regime, suggesting that this was a key channel through which Chinese producers mitigated (at least partially) the additional burden from the retaliatory tariffs.

We provide a discussion of the orthogonality conditions required for our Bartik strategy to be valid (c.f., Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). In this empirical setting, we view the case for causal identification as stemming from the plausible exogeneity of the initial product-level trade shares for the granular grid cells. Related to this, we perform extensive checks to show that the results are not confounded by pre-trends, including trends associated with other initial grid characteristics that could be correlated with trade shares. Our results likewise hold when we control for contemporaneous policy shocks, including changes in China's MFN tariff rates and exchange rate movements, that could impact economic activity through grid-level trade exposure. We also perform a Monte Carlo exercise to allay concerns related to possible over-rejection in statistical inference with Bartik measures (Adão et al., 2019).

[1] The average tariff rates reported are simple averages across all HS 6-digit codes. Imports by China from the US amounted to US$154.4 billion in 2017, of which China subjected US$110 billion (around 71.2%) to retaliatory tariffs by the end of 2018, in response to Lists 1–3 of the Section 301 US tariff actions. We obtain a lower coverage ratio of 66.0%, as China rolled out a wave of exemptions for tariffs on auto part imports in January 2019.

[2] While a number of provincial statistical agencies periodically report industry-level output and employment, this is for relatively aggregate two-digit industries.

[3] Besides weak external demand, China's economic slowdown since 2015 has been partly driven by structural problems including the debt-laden financial system, weakening property markets, and the diminishing return from investment in infrastructure (New York Times, 17 Oct 2019).

[4] Based on the 2017 Annual Industrial Park Statistics, the Industrial Park exported 16.97 billion dollars in electronic products and 13.94 billion dollars in machinery, equipment and components, which accounted for 40.7% and 33.4% respectively of its total exports; around half of this value was exported to the US.

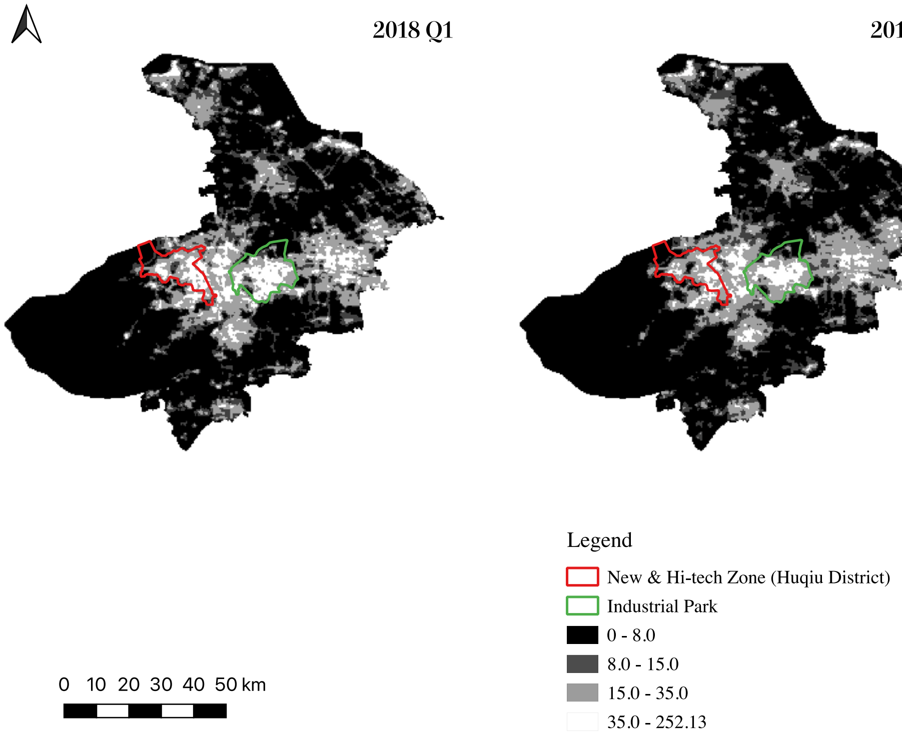

Fig. 1. VIIRS-DNB night lights intensity in Suzhou in Q1/2018 and Q1/2019. *Notes:* From the VIIRS-DNB. The highlighted zones are: the Huqiu district, which lies to the west of the Suzhou Industrial Park.

How do these effects on night lights translate to economic outcomes such as income? Building on the framework in Henderson et al. (2012), we propose an approach to estimate the inverse elasticity of night lights with respect to GDP per capita, that leverages on the time series dimension of the night lights and income data available at the more aggregate prefecture level. From this, we infer the change in GDP per capita that matches the change in night-time luminosity induced by the tariff shocks; we use the same methodology to compute implied effects on employment.

A key message that emerges is that the impact of the US tariffs was very skewed across locations. Grids that saw zero direct export exposure to the US tariffs accounted for close to 70% of China's population. At the other end of the spectrum, the cumulative US tariff shock over Q4/2017-Q4/2019 at the 97.5th population-weighted percentile bin was 9.1 percentage points; for this tail bin, we infer that GDP per capita was 2.52% lower and employment was 1.62% lower relative to unaffected grids. This impact was concentrated especially in grids with a high commuting intensity, suggesting that the negative demand shock prompted a reallocation of labor in line with the spatial general equilibrium framework of Monte et al. (2018). Due to the difference-in-differences nature of our regressions, our estimates net out any impact of the tariff war that was common across all grid cells. In particular, even grids with no direct tariff exposure could have been impacted if there were spillovers through interactions with firms in the rest of China. We show that such effects – to the extent that these are captured by weighted averages of nonlocal tariff shocks, as in the framework of Adão et al. (2020) – likely reinforced the negative direct impact of the tariffs. In this sense, we view our quantitative estimates as constituting lower bounds to the full adverse impact of the US tariffs on China's economy. Last but not least, we uncover similar results – both directionally and in magnitude – in a parallel analysis conducted at the prefecture level, indicating that the US tariffs were consequential at the level of these key administrative units.

This paper contributes to a body of research into the effects of the US-China tariff war on economic activity (see Fajgelbaum and Khandelwal, 2022, for a recent survey). Most of the work to date has been on the impact in the US; this has shed light on the far-reaching repercussions for: pass-through to prices (Amiti et al., 2019; Fajgelbaum et al., 2020; Flaaen et al., 2019; Cavallo et al., 2021), consumption (Waugh, 2019), employment (Flaaen and Pierce, 2019; Benguria and Saffie, 2020; Goswami, 2020), investment (Amiti et al., 2020), supply chains (Charoenwong et al., 2020; Handley et al., 2020), and electoral support (Fetzer and Schwarz, 2021; Kong, 2020; Li et al., 2022a; Lake and Nie, 2023; Autor et al., 2024; Blanchard et al., 2024).

Less is known in contrast about how China's economy has weathered this trade tension. While access to firm-identified customs data for this period has been limited, several studies have documented the fall in China's exports to the US, and the corresponding expansion of its exports to third-country markets, using respectively product-level trade data (Jiang et al., 2023; Sheng et al., 2023) and proprietary micro-level trade and sales data from a single large city (Jiao et al., 2022).[5] Other studies have assessed the tariff

[5] These and related papers (Tian et al., 2022; Feng et al., 2023) have examined the price pass-through implications for China.

war's impact on listed Chinese firms, for which more data on firm-level outcomes is publicly reported (Benguria et al., 2022; Huang et al., 2022). Instead of examining just a particular subset of firms or locations, our paper exploits the satellite coverage to observe the impact on granular grid cells that span mainland China.

Absent detailed data on industry and employment outcomes, researchers have looked beyond traditional sources of information to gauge the consequences of the US-China tariffs. For example, Cui and Li (2021) find that the US tariffs curtailed new firm registrations in China, while He et al. (2021) detect a drop in online job postings and the wages offered by Chinese firms with greater exposure to these tariffs. Another approach has been to use available pre-tariff war data to perform model-based assessments of the impact on China, through quantitative trade models (e.g., in the mould of Caliendo and Parro, 2015) that build in global value chain linkages (Ferraro and Leemput, 2019; Ju et al., 2020; Zhou, 2020), or by gauging the size of model-implied non-tariff trade distortions (Chen et al., 2022). We also seek to assess how much the tariffs affected China's economy, but our approach will instead exploit quasi-experimental variation in grid locations' exposure to the tariffs. While our current focus is on economic outcomes, it bears noting that negative economic shocks have been shown to have repercussions too for social stability and political economy outcomes in China (Campante et al., 2023).

Our paper is related to the wider literature on the use of night lights in empirical research (surveyed in Donaldson and Storeygard, 2016). This has established that night lights are strongly correlated with standard economic measures, including: GDP (Chen and Nordhaus, 2011; Henderson et al., 2012; Roland and Raschky, 2014; Storeygard, 2016), average income (Pinkovskiy and Sala-i Martin, 2016), geographic features (Henderson et al., 2017), population (Bleakley and Lin, 2012; Asher et al., 2021), and development indicators (Michalopoulos and Papaioannou, 2013). While by no means a perfect substitute, there is growing recognition that night lights can be informative of development outcomes at the national as well as sub-national levels. Our analysis adds specifically to a recent body of applications demonstrating the feasibility of using high-frequency changes in night lights to study short-run responses to negative shocks in data-scarce environments (World Bank, 2017; Chodorow-Reich et al., 2019).[6]

In what follows, Section 2 describes the key data sources. Section 3 lays out the empirical strategy for identifying the effects of tariff shocks on night lights, and reports these findings. Section 4 computes the implied effects for GDP per capita and employment, while discussing potential mechanisms. Section 5 concludes. Appendices A–B document more details on the data, while Appendices C–D report additional empirical analysis (available online).

2. Data

2.1. Product-level data on tariffs

Our primary source of information on the US-China tariffs is Bown (2021), which records and dates the tariff actions starting in January 2018. We obtain: (i) tariffs imposed by the US on goods from China, available at the HS 10-digit level; and (ii) retaliatory tariffs enacted by China on US goods, available at the HS 8-digit level. The ebb and flow of these tariff actions is well-documented (e.g., Wong and Koty, 2019; Bown and Kolb, 2021), with each round of increases by the Trump administration prompting China to respond in short order with retaliatory tariffs.

Our analysis includes the Section 201 tariffs on solar panels and washing machines (which came into effect in February 2018), the Section 232 tariffs on aluminum and steel products (March 2018), as well as the four rounds of Section 301 tariffs (July 2018 to September 2019). We focus on these tariff actions up until September 2019, prior to the Phase One Trade Agreement between the US and China, though we have also coded up the tariff changes to end-2020. We incorporate the information from Bown (2021) on tariff exemptions, granted by either the US or Chinese authorities to particular products on a case-by-case basis; these exemptions are netted out from the tariffs. When aggregating the data to the monthly or quarterly level, we scale the tariffs by the number of days that they were in effect. As the China customs data that we will use are at the HS 6-digit level, we take a simple average over all associated 10-digit or 8-digit tariff changes (for the US and China tariffs respectively) to obtain tariff shock measures for HS6 codes. We complement the above with data from Most Favored Nation (MFN) tariff schedules from China's General Administration of Customs, to keep track of discretionary changes to those tariffs (c.f., Bown et al., 2019). By comparison, MFN tariffs for the US were stable during this period.

We provided in the Introduction a sense of how far-reaching the tariff increases were. Appendix A contains a detailed timeline and description of these changes, which are also illustrated in Figure A.1. The biggest ramp-up in tariffs occurred during September 2018, with the third round of Section 301 tariffs and China's retaliation to this (Tables A.1-A.2). Somewhat less well known is the fact that China also undertook cuts to its MFN tariffs – on product codes that accounted for more than a third of its total imports in 2017 – which softened the impact of the US tariffs (Table A.3); we will control for these in our empirical analysis.

We verify in Appendix C that the tariffs substantially affected trade flows at the product level. The lead–lag specifications there show that China's exports to the US declined after the US tariff increases, with statistically significant cumulative effects (Figure C.1, Panel I.A); this corroborates Amiti et al. (2019) and Fajgelbaum et al. (2020) on the swift response of these trade flows to the tariffs. We find some evidence of a rise in exports to other destinations, starting around four months after a product was first hit with US tariffs (Panel I.B). We separately confirm that China's imports from the US fell following retaliatory tariff actions (Panel II.A).

[6] Khachiyan et al. (2022) showcase the further potential in exploiting daytime satellite images to assess income and population for fine grid locations, although the training algorithms involved can be computationally intensive.

2.2. Firm geographic coordinates and grid-level trade flows

We require information on initial exports and imports at the grid level, in order to capture a location's exposure to the tariffs. For this, we draw on 2016 Chinese customs data, which covers the universe of China's exporters and importers; the customs data assigns (in principle) a unique identifier with each plant that undertakes international trade activities, although we will refer to each such entity as a "firm" for simplicity. For each trading firm, the customs records provide a breakdown of its trade flows by destination/origin countries for HS 6-digit products. To pin down location, we use Google Maps API to recover each firm's geo-coordinates (longitude and latitude) on the basis of a search of firm names and (where necessary) addresses.[7]

With this geo-information, we then map firms to 0.1 arc-degree grid cells (approximately 11-km-by-11-km) and compute grid-level exports and imports in the base year (2016, indexed by 0).[8] We define the value of exports of product k to the US from grid i as: $X^{US}_{ik0} = \sum_{f\in i} X^{US}_{fk0}$, where the sum is taken over the corresponding exports of all firms f located in the grid cell (X^{US}_{fk0}). Similarly, we define grid-level imports of product k from the US to be: $M^{US}_{ik0} = \sum_{f\in i} M^{US}_{fk0}$, where M^{US}_{fk0} is the corresponding imports by firm f in grid i. We exclude all trade flows by intermediary firms, as these transactions may not reflect actual production in a grid location.[9,10]

The geo-coordinates obtained from a single mapping service could be subject to some inaccuracy – for example, there could be different thresholds for accepting a search outcome that is a potential false positive – and this constitutes a source of measurement error in the inferred export and import structure at the grid level. We therefore repeat the above procedure using the Amap API – a web-based mapping, navigation and location service maintained by the Alibaba Group – to construct a second measure of grid-level trade exposure. There are about 283,000 non-intermediary firms in the 2016 Chinese customs data; Google Maps geo-located 95.9% of these firms, while the corresponding share was 79.9% for Amap. Importantly, the two sets of geo-coordinates are highly correlated, with a median distance of 5.2 km between Google Maps and Amap locations, as we report in detail in Appendix B. We employ alternative APIs not only to cross-validate the firm geo-data, but also to design an IV strategy to address the attenuation bias introduced by measurement error that is particular to each individual source.

2.3. Night lights intensity

Given the paucity of detailed statistics on Chinese economic activity at the sub-national level, we turn to a common proxy measure of local economic performance, namely human-generated night lights. In the context of China, night lights emanating from industrial areas (such as that in Fig. 1) in principle reflect the intensity of factory operations during night shifts, as well as occupancy in adjacent dormitories (where workers employed in the factories are often housed).

We use the VIIRS-DNB dataset, which provides a monthly average of night lights intensity in 15 arc-second geographic grids commencing in April 2012.[11] These readings are filtered by the data provider to exclude observations impacted by lightning, lunar illumination, and cloud-cover. Two night lights series are available. The first excludes any data impacted by stray light during the summer months. The second adopts a stray-light correction procedure (Mills et al., 2013); this raises the coverage for Northern China during the summer, but relies on the quality of the applied corrections. We use the first series as our baseline measure, and the second for a robustness check. Compared to prior vintages of night lights data, the VIIRS-DNB has the key advantage that it is not top-coded.[12] The satellite overpass however occurs later at night (around 1:30am). Improvements in satellite technology notwithstanding, the accuracy of the readings can still be affected by local weather and atmospheric conditions, and so the night lights data on any given day should be viewed as a proxy for economic activity that is observed with noise.[13]

We aggregate this data to 0.1 arc-degree grids. There are 97,313 grid cells of this size covering mainland China, and these serve as the geographic units for the analysis in Section 3. While most of our regressions will be run at the grid level, we will also present some findings based on more aggregate administrative units. Within China's region-based, multi-level hierarchy, there are

7 The prefecture where each firm is located can be inferred from its unique identifier, but this is too coarse for mapping to a grid cell. Note also that it is not feasible to use the firm address as the primary search criterion, since addresses are missing for around 57% of the firms in the customs dataset.

8 We use the ArcGIS tool "Fishnet" to create a net of rectangular cells with width 0.1 arc-degree for mainland China, based on a map from the China Data Center at the University of Michigan.

9 Intermediary firms are identified from their firm names. Following Ahn et al. (2011), firms with names containing Chinese characters for "importer", "exporter", and/or "trading" are excluded.

10 To address the possibility that the exports and imports of multi-establishment firms could be pooled under the firm headquarters for reporting purposes, we have verified that our results are robust to excluding the top five (or even top ten) prefectures by 2017 GDP (or export value), where firm headquarters are more likely to be based (see Table D.4 in the appendix).

11 The night-time imaging capacity in the VIIRS-DNB is an advance over its predecessor – the Defense Meteorological Program Operational Line-Scan System (DMSP-OLS) – in radiometric accuracy, spatial resolution and geometric quality (Hu and Yao, 2022); the DMSP-OLS data was discontinued after 2013.

12 Figure 4 in the IMF working paper version of Hu and Yao (2022) shows that while the DMSP-OLS night lights intensity is increasing in country real GDP per capita, the slope of this relationship tapers off in the top range of the latter variable. In contrast, the relationship between the VIIRS-DNB night lights and real GDP per capita is distinctly linear. Relatedly, Bluhm and Krause (2022) show that the top-coded DMSP-OLS night lights understate the extent of economic activity in the biggest urban areas.

13 When faced with negative demand shocks, manufacturing plants in China have been known to cut back on the use of costly pollution-reducing technologies, while emitting the pollutants at night to minimize public scrutiny. Li et al. (2022b) and Du and Li (2023) uncover evidence of this at the height of the US-China tariff war, particularly in prefectures where local leaders were keen to maintain economic output to bolster their promotion prospects. The extensive controls we will use – initial grid characteristic-specific time trends and prefecture-time fixed effects – help to account for such forces, to the extent that these were concentrated in locations with a high initial level of industrial activity and where local politicians were strongly motivated by career concerns.

in decreasing rank order 31 provincial-level units, 334 prefectures, and 2580 counties in mainland China as of 2015. The mean number of grid cells per prefecture (whose centroids lie within the boundaries of the prefecture) is 1316, with a median of 750.

Appendix C provides a basic proof of concept that product-level tariffs affected economic activity as captured by night-time luminosity. Using a lead–lag specification, we show that US tariffs levied at the product level were accompanied by a decline in the night lights intensity of grids that prior to the tariff war were more engaged in exports of that product.

3. Tariff shocks and night lights: Grid-level analysis

We turn now to investigate the effects of the US-China tariff war on grid-level night lights. We describe the empirical strategy in Section 3.1, before reporting the findings in Section 3.2.

3.1. Empirical strategy

Our primary interest is in studying the economic impact on China of the following trade barriers: (i) the US tariffs imposed on goods from China; and (ii) the retaliatory tariffs imposed by China on imported inputs from the US. We therefore relate grid-level changes in night lights intensity to local exposure to the tariff shocks via the following empirical model:

$$\Delta \ln(Light_{it}) = \pi_1 \Delta USTariff_{i,t-1} + \pi_2 \Delta CHNInputTariff_{i,t-1} + D_{pt} + D_i + W_{i0} \times D_t + u_{it}. \tag{1}$$

Here, $Light_{it}$ is the mean night-time luminosity of grid i during time t, and the Δ operator denotes the difference between the time t and $t-4$ values for the variable. (We refer to the unit of time, e.g., Q2/2019, as the year-quarter.) The variable $\Delta USTariff_{i,t-1}$ captures exposure to US tariff shocks during time $t-1$ faced by Chinese exporters geo-located in grid i, while $\Delta CHNInputTariff_{i,t-1}$ measures the exposure of importers in that location to changes in the retaliatory tariffs levied on inputs from the US; we focus on tariffs on inputs, since these would in principle have raised production costs for Chinese manufacturers (though we will explore broader import tariff shocks in a later check). We lag the tariff shocks on the right-hand side by one period to accommodate a lagged response of local economic activity to the tariff increases.

The fixed effects structure in (1) comprises a full set of prefecture-by-year-quarter (D_{pt}) and grid dummies (D_i). The D_{pt}'s account in a flexible manner for unobserved shocks common to all grids within a prefecture p; in particular, these absorb the effects of policy moves or directives enacted at the prefecture level that could have affected local economic activity. The use of the D_i dummies further means that we are exploiting within-grid variation over time in night lights intensity. With these fixed effects, the π_1 and π_2 coefficients are difference-in-differences estimates of the effects of exposure to the tariffs. We control in addition for a vector of initial grid-level characteristics interacted with year-quarter fixed effects, $W_{i0} \times D_t$; as we elaborate on shortly, this will help to bolster the causal interpretation of the tariff shock coefficients.

In our estimation, we regress stacked year-on-year changes in log night lights intensity over the period Q2/2018 to Q4/2019 on stacked year-on-year changes in grid-level tariffs over Q1/2018 to Q3/2019, which covers all the major tariff increases by the Trump administration as well as China's responses. To guard against outliers, we winsorize $\Delta \ln(Light_{it})$ observations that are above the 99th percentile or below the 1st percentile within each period t. Standard errors are clustered at the province level to account for serial correlation over time as well as spatial correlation across grids within a province. To generate population-relevant estimates, we weight the regression observations by grid population in 2015.[14]

We adopt a Bartik (or shift-share) construction for the key tariff shock explanatory variables. For the exposure of location i within China to the US tariffs, we compute:

$$\Delta USTariff_{it} = \sum_k \frac{X_{ik0}^{US}}{X_{i0}} \Delta USTariff_{kt}, \tag{2}$$

where X_{ik0}^{US}/X_{i0} is the value of product-k exports from grid i to the US, expressed as a share of total grid-level exports, in 2016 prior to the tariff war. The variation in $\Delta USTariff_{it}$ stems from: (i) cross-grid differences in initial export product mix and in the importance of the US as a destination for those exports; and (ii) cross-product differences in the US tariff changes over time, $\Delta USTariff_{kt}$. A location is thus exposed to a bigger decline in external demand if it specializes in exporting HS 6-digit products to the US that then face larger US tariff hikes.

Analogously, our measure of location i's exposure to retaliatory tariffs on inputs is:

$$\Delta CHNInputTariff_{it} = \sum_{k \in \mathcal{K}} \frac{M_{ik0}^{US}}{M_{i\mathcal{K}0}} \Delta CHNTariff_{kt}, \tag{3}$$

where $\mathcal{K}$ denotes the set of products k classified as either an intermediate input or capital good under the UN Broad Economic Categories (BEC, Revision 5) coding system. $M_{ik0}^{US}/M_{i\mathcal{K}0}$ is the base-year value of product-k imports from the US, expressed as a share of total grid-level imports of inputs. As constructed, $\Delta CHNInputTariff_{it}$ leverages: (i) cross-location differences in initial

[14] We impute a population of 1 for cells with a reported zero population, though our results are virtually unchanged even if we were to entirely drop these grids from the sample (see Table D.4 in the appendix). A description of data sources for all auxiliary variables, including grid-level population, is in Appendix B.

import composition and in the importance of the US as a source country for these inputs; and (ii) variation across products and over time in China's retaliatory tariffs, $\Delta CHNTariff_{kt}$.

The tariff shock measures defined in (2) and (3) exhibit substantial variation across grid cells. The cumulative population-weighted mean exposure between Q1/2018 and Q3/2019 to the US tariffs is 1.15 percentage points, with a standard deviation of 2.26. The corresponding mean for the retaliatory tariffs on inputs is 0.55 percentage points, with a standard deviation of 2.23.[15] These baseline measures focus on a location's direct exposure to the tariffs, through trade with the US that is geo-located to the grid cell; we will later consider the role of indirect exposure through the tariff shocks experienced in neighboring locations (see Section 4.4.2).

The validity of the regression model in (1) for identifying the causal effect of the tariff shocks rests on the extent to which the measures in (2) and (3) capture sources of variation in local exposure to the tariffs that are exogenous with respect to the night lights outcome variable. Drawing on the recent literature that has clarified the conditions required for the implementation of a Bartik empirical strategy, one would need to be reassured that, conditional on the set of included controls, the u_{it}'s are uncorrelated with either: (i) the initial export/import structure of the grid cell (Goldsmith-Pinkham et al., 2020); or (ii) the product-specific tariff shocks experienced at the national level (Borusyak et al., 2022). The latter condition is more challenging to defend in the current context, given the targeted nature of the tariff actions. For example, the Section 201 and Section 232 tariffs were directed at narrow sets of products, such as solar panels and washing machines. The early Section 301 rounds imposed tariffs predominantly on intermediate inputs, out of the Trump administration's apparent desire to avoid tariffs on consumption goods that would fall directly on American households (Bown and Kolb, 2021; Grossman et al., 2023). The Section 301 tariffs were moreover ostensibly directed at products from industries deemed to be beneficiaries of China's "Made in China 2025" industrial policy plan (Ju et al., 2020).[16]

We thus view identification in our context as stemming more plausibly from the conditional exogeneity of the initial grid-level trade shares that capture a location's exposure to subsequent tariffs. More formally, this requires: $E[(X^{US}_{ik0}/X_{i0})u_{it} \mid \mathcal{W}] = 0$ and $E[(M^{US}_{ik0}/M_{i\mathcal{K}0})u_{it} \mid \mathcal{W}] = 0$, where $\mathcal{W} = \{D_{pt}, D_i, W_{i0} \times D_t\}$ is a shorthand for this set of controls on the right hand-side of the regression. At a basic level, it is worth pointing out that the granular 11 km-by-11 km grids that are our unit of analysis do not systematically coincide with the geographic boundaries of administrative districts or economic zones within China. The orthogonality conditions may still be called into question if: (i) there are grid-specific trends in night lights intensity driven by forces other than the tariff shocks, that are nevertheless correlated with the initial grid-level trade shares (i.e., X^{US}_{ik0}/X_{i0} and $M^{US}_{ik0}/M_{i\mathcal{K}0}$); or if (ii) there are other contemporaneous product-level shocks that affect local outcomes through the same profile of exposure shares.

We take several steps to address concern (i). First, we will confirm that the tariff shocks in (2) and (3) are uncorrelated with pre-trends in night lights growth. Second, it bears repeating that the specification in (1) includes grid fixed effects (D_i), which account for differences in average year-on-year growth in night lights across grids. As we will see, our findings are further robust to controlling for a $D_i \times t$ term – i.e., grid-specific linear time trends in $\Delta \ln(Light_{it})$ – which accounts for a further dimension of flexibility at the grid level in how night lights might evolve. Third, the initial export or import structure could be correlated with other location features, that in turn might be the basis for systematic shifts over time in night-time luminosity. We therefore construct additional grid-level base-year variables W_{i0}, namely: log exports per capita and log intermediate imports per capita (to capture overall openness), the shares of exports to and intermediate imports from the US (to capture the importance of the US as a trade partner), and log mean night lights intensity (to capture the overall level of local economic development). In further checks, we also consider the possible role of initial grid-level trade shares in particular subsets of products, as well as of the importance of state-owned enterprises in grid-level exports. These are each interacted with a full set of year-quarter dummies ($W_{i0} \times D_t$), to control for trends in local economic activity that might stem from these initial grid features.

To address the latter concern in (ii), we will verify that our findings hold even when controlling for Bartik-style variables that seek to directly pick up grid-level exposure to other candidate shocks. These shocks include: product-level adjustments in China's MFN tariffs, in China's value added tax (VAT) rates (Gourdon et al., 2022), and movements in the bilateral exchange rate.

When estimating (1), we use the US tariff shock (respectively, the retaliatory tariff shock) constructed on the basis of Amap geo-coordinates as an instrumental variable (IV) for the corresponding measure constructed using geo-coordinates retrieved from Google Maps. As discussed earlier, the firm geo-location from each source could be subject to inaccuracy, which potentially introduces measurement error in the X^{US}_{ik0}/X_{i0} and $M^{US}_{ik0}/M_{i\mathcal{K}0}$ trade shares that we infer for each cell. The IV approach helps to address attenuation bias in the coefficients of interest (π_1 and π_2), insofar as the measurement error associated with the two web-mapping services is uncorrelated.

3.2. Empirical findings

Baseline Results: Table 1 reports our findings on the impact of the tariffs, following the specification in (1); all columns presented control for prefecture-time and grid dummies. When entered into the regression model on its own, the US tariff shock ($\Delta USTariff_{i,t-1}$) has a significant negative effect (Column 1). Relative to other locations, a one-percentage-point increase in a grid cell's exposure to these tariffs is associated with night lights growth that is 0.77 log points lower, indicating that the US tariffs dampened the intensity of local economic activity. Note that the first-stage F-statistics confirm the relevance of the Amap-based tariff

[15] The correlation between $\Delta USTariff_{it}$ and $\Delta CHNInputTariff_{it}$ is 0.208, so the two tariff shock measures are not particularly strongly correlated.

[16] This is expressly stated in the "Section 301 Investigation Fact Sheet" released by the office of the United States Trade Representative.

Table 1
Tariff shocks and night lights intensity.

Dep. Var.: $\Delta \ln(Light_{it})$	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) OLS-RF	(6) 2SLS
$\Delta USTariff_{i,t-1}$	−0.7702***		−0.7851***	−0.5903**	−0.2523**	−1.1555**
	(0.2166)		(0.2099)	(0.2673)	(0.1115)	(0.4839)
$\Delta CHNInputTariff_{i,t-1}$		−0.0619	0.1051	0.5509	0.1631	0.3897
		(0.3202)	(0.3299)	(0.5555)	(0.1766)	(0.9943)
Grid FE	Y	Y	Y	Y	Y	Y
Prefecture × Year-Quarter FE	Y	Y	Y	Y	Y	Y
Grid W_{i0} × Year-Quarter FE	N	N	N	Y	Y	Y
Grid FE × Linear Time Trend	N	N	N	N	N	Y
Observations	669,845	669,845	669,845	669,845	669,845	669,845
F-stat	183.8	72.31	37.33	21.22	–	10.92

Notes: All columns use each respective Amap grid-level tariff shock as an instrumental variable for the corresponding Google Maps grid-level tariff shock. The exception is Column 5, which reports the OLS reduced-form estimates from using the Amap-based tariff shocks directly as explanatory variables. The initial grid characteristics, W_{i0}, whose time-varying effects are included in Columns 4–6 are: the US share in exports, the US share in imports of intermediates, log exports per capita, log intermediate imports per capita, and log 2016 mean night lights intensity; grid-level exports and imports geolocated via Amap are used to construct the first four of these W_{i0} variables. All regressions are weighted by grid population in 2015, with a minimum population of 1 imputed for cells with zero population in the raw data. Standard errors are clustered at the province level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

shock for explaining variation in the corresponding Google Maps-based variable, a reflection of the positive correlation between the two sets of geo-coordinates. In the analogous regression for $\Delta CHNInputTariff_{i,t-1}$, the estimated coefficient for the retaliatory tariffs on inputs from the US shows a negative effect on night lights growth, but this is not statistically distinguishable from zero (Column 2).

We include in Column 3 both the US tariff and the China input tariff shocks. The coefficient for the US tariff shock resembles that in Column 1, while that for the tariffs on imported inputs remains insignificant. Column 4 further controls for grid-level initial characteristics W_{i0} – log exports per capita, log intermediate imports per capita, the US share in total exports, the US share in imported intermediates, and log 2016 mean night lights intensity – each interacted with year-quarter dummies. Our identifying assumption is therefore that, conditional on the fixed effects and these grid-specific trends, the profile of grid-level HS 6-digit trade shares is uncorrelated with unobserved factors that influence night lights growth. The results are reassuringly robust, which helps to assuage the concern that the tariff shocks could instead have been picking up underlying trends in night lights intensity related to initial levels of trade openness or local development. Even with these additional controls, the magnitude of the US tariff shock coefficient is only slightly smaller, with a one-percentage-point increase in tariff exposure lowering night lights growth by 0.59 log points (holding all else constant). We illustrate the tariff shock effects implied by this Column 4 specification in Figure D.1 in the appendix. The binned scatterplots of residualized changes in night lights against the residualized tariff shock variables confirm how greater exposure to the US tariffs was associated with slower growth in night lights; on the other hand, the China input tariffs did not affect night lights in a distinct way. These relationships do not appear to be driven by any particularly influential bins.

For a side-by-side comparison, we report in Column 5 an OLS regression that is the reduced-form analogue of the 2SLS specification in Column 4, where the Amap-based tariff shock measures (the instruments in the prior columns) are used directly as explanatory variables. The US tariff shock effect remains significant, though smaller in magnitude relative to Column 4, consistent with the IV approach helping to address attenuation bias in this coefficient of interest. Lastly, Column 6 reports a particularly stringent specification in which we add grid-specific linear time trends to the right-hand side. This does not affect our conclusions on the negative effect of the US tariffs on local economic activity as proxied by night lights, and in fact yields a much larger coefficient (−1.16). In what follows though, we will use the point estimates from Column 4 for what we view as a more conservative benchmark for the impact of the US-China tariff war on China's economy.

A natural question that arises at this juncture is how uniform and persistent the impact of the tariffs was. To explore this, we estimate a modified version of the Column 4 specification that: (i) extends the sample period to encompass all 12 quarters from Q1/2018 to Q4/2020, while (ii) allowing for heterogeneous tariff shock effects over time, specifically for each half-year period.[17] We report these results in Table D.1 in the appendix: We obtain negative and significant US tariff effects on night lights growth in the second half of 2018 and the first half of 2019, coinciding with the large expansion in scope of the Section 301 tariffs in September 2018. The US tariffs continued to exert a negative impact on night lights growth in the first half of 2020, suggesting that this could have been a contributing factor in the overall economic contraction in the early months of the Covid-19 pandemic.[18] By contrast, we do not uncover any significant effects for the China input tariff shock over the three years. Although these tariffs would

[17] To be clear, we continue to estimate how changes in night light intensity observed in year-quarter t are affected by year-on-year changes in grid-level tariffs with a one-period lag, $\Delta USTariff_{i,t-1}$ and $\Delta CHNInputTariff_{i,t-1}$. However, the sample period is now t = Q1/2018, … , Q4/2020, with separate tariff shock coefficients estimated for t = Q1&2/2018, Q3&4/2018, etc.

[18] This negative US tariff shock effect in Q1&2/2020 is robust to controlling for the proximity of a grid location to Wuhan, the epicenter of the pandemic, interacted with time fixed effects (see Table D.1).

in principle have raised the cost of inputs from the US, this does not appear to have been a significant drag on economic activity. We will explore several potential reasons for this later below.

Additional Results and Checks: We have performed a series of checks to establish the robustness of our findings and to validate our Bartik empirical strategy. These are reported in full in Appendix D, but we provide key highlights below.

Table D.2 addresses concerns related to pre-existing trends. We adapt the Column 4 specification to verify that the time-$(t-1)$ US and China tariff shocks are uncorrelated with night lights growth in earlier periods; there thus do not appear to be pre-trends or anticipation effects in observed night lights. We next show in Table D.3 that our estimates are stable, even when we expand the set of initial grid-level characteristics for which we control for associated time trends. We consider additional $W_{i0} \times D_t$ terms for log grid population, the state-owned enterprise share of grid-level exports, as well as grid-level trade shares for each of 15 HS sections.[19] This helps to account in particular for the potential effects of any explicit or implicit policies, such as over the allocation of bank credit, that might have benefited state-owned over private enterprises. This check moreover confirms that the negative US tariff shock effect stems from variation across grids in the detailed composition of exported products (e.g., Men's or boy's shirts, of cotton), rather than from forces affecting broad HS sections (e.g., Textiles and Textile Articles).

Table D.4 performs checks on our preferred specification. Among other tests, we estimate the regression model in levels as in equation (D.1), rather than in time-differenced changes. We also explore: (i) interchanging the Google Maps and Amap-based measures in the 2SLS estimation; (ii) using the VIIRS-DNB measure with stray light correction; (iii) dropping all zero-population grid cells (rather than imputing a population value of 1); (iv) replacing prefecture-by-year-quarter fixed effects with province-by-year-quarter fixed effects; and (v) excluding grid cells in the five largest prefectures by 2017 GDP, where headquarters – rather than production facilities – tend to be located. Our headline result of a negative and significant US tariff shock effect, together with an insignificant China input tariff effect, holds throughout.

Table D.5 controls for other contemporaneous shocks that could affect local economic activity through the initial grid composition of exports or imports. We do so through the use of additional Bartik variables to capture a grid location's exposure to: (i) adjustments in China's MFN tariffs; (ii) cuts in VAT rates; and (iii) movements in the USD to RMB exchange rate. The grid-level tariff effects estimated in Table D.5 are virtually unchanged, and so our baseline results are unlikely to be picking up the influence of these other forces.[20]

We explore alternative tariff shock measures in Table D.6. As a falsification test, we construct the tariff shocks using initial product-level shares for a grid location's trade with a set of other high-income countries, instead of with the US.[21] These placebo shocks yield insignificant effects, confirming that our results are driven by trade exposure to the US rather than to high-income countries more broadly. While our focus is on the retaliatory tariffs on inputs, we verify that the tariffs on US consumption goods did not affect night lights growth either. We also consider an alternative retaliatory tariff shock variable, that draws on China's 2017 Input-Output Tables to construct a weighted-average measure of each product's exposure to tariffs on inputs used (following Amiti and Konings, 2007), and then projects these to grid locations; we likewise obtain an insignificant effect with this formulation of exposure to the tariffs on inputs. Separately, we find that the negative effect of the US tariff shock on night lights growth is driven primarily by: (i) tariffs levied on differentiated (rather than homogeneous) products (c.f., Rauch, 1999); and (ii) tariffs that fell on the exports of foreign-owned (rather than state-owned or private domestic) enterprises. The former pattern suggests that firms could have faced greater difficulties finding other buyers for products customized for the US market. The latter hints at the possibility that China-owned firms may have been shielded (perhaps by policy measures) from the most severe impact of the US tariffs.

Two additional checks are of note. In Figure D.2, we conduct a permutation test, in which we randomly reshuffle product-level tariff shocks across HS 6-digit codes, and then re-build shift-share regressors by combining actual trade shares in 2016 with the placebo shifters. The standard deviation of the estimates from 300 placebo samples is smaller than our baseline standard error (for both the US and retaliatory tariff shocks). This relieves the concern that we may be under-stating the standard errors due to an underlying spatial correlation in the trade shares (Adão et al., 2019).[22] Lastly, in Table D.7, we adapt our specification to run the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020); this guards against concerns that the average treatment effect could be mis-estimated in two-way fixed effects regressions, when the treatment timing differs across grids and treatment effects vary across grids and time.

Understanding the China Input Tariff Shock Effect: That China's tariffs on inputs appear not to have had a significant effect on night lights warrants more discussion, since one might have expected these tariffs to weigh down on firms that were sourcing intermediates from the US. Indeed, apart from agricultural inputs such as unprocessed soybeans, the retaliatory tariffs also affected a wide range of manufacturing inputs which China was importing in significant volumes from the US, such as pulp products, electronics components, and auto parts. The absence of a systematic negative effect from these tariffs suggests that there were forces that mitigated the rise in the cost of inputs. We present evidence on several such channels below.

---

19 See Appendix D.1 for a full list of the 15 HS sections.

20 See Appendix D.1 for details on the construction of these additional shock variables. While several of these variables – for exposure to shifts in MFN tariffs, VAT rates, and the bilateral exchange rate – yield coefficients with signs consistent with economic intuition, these are imprecisely estimated when all the exposure variables are jointly included; see Table D.5 for more details.

21 This comprises the eight countries used by Autor et al. (2013) to construct their "China shock" instrumental variable, namely: Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

22 Figure D.3 provides further reassurance on this issue, by instead reshuffling the export and import trade shares across grids within each prefecture.

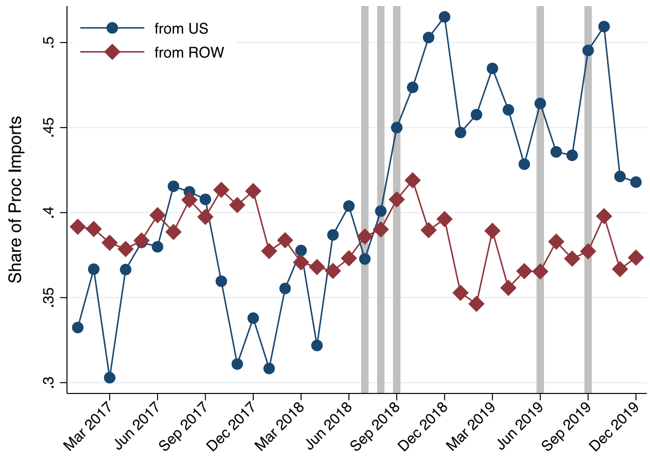

Fig. 2. Changes over time in the share of processing imports. *Notes:* The figure plots the shares of imports from the US and the ROW respectively that are organized through processing trade. The shaded areas correspond to the different phases of China's retaliatory tariff actions, as listed in Table A.2.

At a basic level, one might reasonably expect that the Chinese authorities would have chosen strategically to levy tariffs on US products for which the adverse impact on Chinese firms could be softened, due for example to the availability of other country sources (such as soybeans from Brazil). We uncover supporting evidence of this using product-level data from Trade Map. In a series of lead–lag event study regressions in Appendix C, we find that even as imports of intermediates from the US fell (Panel II.A, Figure C.1), there was a slight pickup in imports of these products from the rest of the world (ROW) within a few months in the aftermath of retaliatory tariffs (Panel II.B). China also lowered its MFN tariffs on selected products, and these appear to have helped dampen the fall in imported inputs from the US to some degree (Panel II.C).[23] It is plausible too that some substitution to domestic inputs may have occurred, though we lack direct data on such domestic purchases to verify the extent of this.[24]

We uncover one additional channel that has received less public attention, namely: the use of China's processing trade regime to facilitate imports of inputs. For affected firms, the retaliatory tariffs would have raised incentives to switch from importing under ordinary trade to importing under the processing trade regime, as this would exempt firms from those duties on inputs they would have paid prior to the US-China tariff war. While firms are required to sell goods produced with processing inputs in overseas markets, this may not have been a major restriction for firms that were already oriented toward exporting. Of note, there were reforms during this period – including the removal of a certificate application to China's Ministry of Commerce, implemented on 1 January 2019 – that lowered the administrative barriers for enterprises to engage in processing trade (see Appendix C.3 for more institutional details).

In line with this discussion, Fig. 2 reveals a sharp rise in the share of imported inputs from the US brought in under the processing trade regime following China's tariff retaliation.[25] This rise was conspicuously specific to imports from the US, with the corresponding processing import share for the ROW remaining stable. We further show in Appendix C.3 that this shift for US-sourced inputs was concentrated in products that can be more readily designated as processing inputs, due to there being ample pre-tariff war precedent and experience with importing these under processing trade (c.f., Brandt et al., 2021). More broadly, it is well-known that the processing trade share in China's imports had been on a decade-long decline prior to the tariff war (see for example, Deb et al., 2019), and this reduction in dependence on the processing trade regime had been motivated in part by the lower productivity and weaker performance of processing relative to ordinary trade firms (Dai et al., 2016). With the onset of the US-China tariffs, the rise in the processing share that we are now seeing in Fig. 2 is therefore a notable reversal to this prior trend.[26]

4. Implications for economic outcomes

We have thus far shown that the higher US tariffs faced by Chinese exporters resulted in a dimming in night-time luminosity. In this section, we examine the implications for more conventional measures of China's economic performance. First, drawing on the

[23] As described in full in Appendix C, these results are based on event study regressions run on monthly trade flow data at the HS 6-digit product level, while controlling for HS 6-digit and HS 2-digit-by-month fixed effects.

[24] Table D.6 in the appendix explores an indirect test for such domestic substitution, by exploring whether there was an intensification of night lights in grid locations within China that were initially specialized in exporting intermediates, on which China later placed retaliatory tariffs on imports originating from the US; the estimated effect though is not statistically significant (see Column 4).

[25] The bilateral trade data by product and by trade regimes is obtained from China's General Administration of Customs: http://stats.customs.gov.cn.

[26] As further evidence of trade regime switching, and bearing in mind that goods produced with processing inputs need to exported, we show in Figure C.4 that there was an increase in China's processing exports (but not ordinary trade exports) for products whose inputs were more severely hit by the retaliatory tariffs. These findings mirror Brandt and Morrow (2017) who show that following China's accession to the WTO, the reduction in import tariffs induced firms to make the opposite switch, from processing to ordinary trade.

literature that has exploited night lights to study economic development, we estimate structural elasticities between observed night lights intensity and local economic outcomes for China. Second, we combine these with the estimates from Section 3.2, and compute implied effects for GDP per capita and urban employment. Building on this, we present evidence of a role for labor reallocation as a mechanism through which the effects of the tariff shocks have been transmitted across locations within China. Last but not least, we briefly discuss aggregate implications at the prefecture level.

4.1. Statistical framework

We adopt a statistical framework following Henderson et al. (2012) that relates observed night lights to measured economic outcomes. This will discipline the manner in which we back out the structural elasticities of interest. We focus on GDP per capita as the economic outcome in the exposition below, but the framework is entirely analogous for employment.

In this setting, the true value of log GDP per capita y_{js} for Chinese location j in year s is reported with measurement error:

$$z_{js} = y_{js} + \varepsilon_{z,js}. \tag{4}$$

Here, z_{js} denotes the measured value of log GDP per capita; the error term $\varepsilon_{z,js}$ is assumed to be independent and identically distributed (iid) with variance σ_z^2. The intensity of observed night lights, x_{js}, is in turn a function of the true underlying level of GDP per capita, given by:

$$x_{js} = \beta y_{js} + \varepsilon_{x,js}. \tag{5}$$

The $\varepsilon_{x,js}$ is a measurement error term, which arises in particular from idiosyncratic climatic and atmospheric conditions as the satellite readings are taken. We assume these are iid with variance σ_x^2, and that these are uncorrelated with the measurement error in the income data, i.e., that $cov(\varepsilon_{x,js}, \varepsilon_{z,js'}) = 0$ for any s and s'. Our goal is to recover an estimate of $1/\beta$, which will then enable us to map changes in night-time luminosity to GDP per capita outcomes.

With the above structure, Henderson et al. (2012) show that the implied estimating equation is: $z_{js} = (1/\beta)x_{js} + u_{js}$, where $u_{js} = -(1/\beta)\varepsilon_{x,js} + \varepsilon_{z,js}$. The OLS estimator for $1/\beta$ is then:

$$\widehat{\left(\frac{1}{\beta}\right)} = \frac{1}{\beta}\frac{\beta^2\sigma_y^2}{\beta^2\sigma_y^2 + \sigma_x^2}, \tag{6}$$

where σ_y^2 denotes the variance of the true income variable y_{js}. The naive OLS estimate is thus attenuated with respect to the true value of $1/\beta$ whenever $\sigma_x^2 > 0$, due to measurement error in the night light readings. Henderson et al. (2012) work around this, by proposing a methodology to bound the signal-to-noise ratio $\sigma_y^2/(\sigma_y^2 + \sigma_z^2)$ in the country income data.

We instead adopt an approach that exploits the panel dimension of the night lights and GDP per capita data. We make one additional orthogonality assumption, namely: $cov(\varepsilon_{x,js}, \varepsilon_{x,js-1}) = 0$, so that the measurement error in the night lights data is viewed as serially uncorrelated at the annual frequency. In other words, while there may be measurement error in the satellite readings due to climatic conditions on any given day, the scientific instruments do not make systematic mistakes for a given location j that are correlated over annual averages. We can then obtain a consistent estimate of $1/\beta$ by running a regression of log GDP per capita, z_{js}, on observed night lights, x_{js}, while instrumenting for the latter with its lagged value, x_{js-1}. Specifically, the IV estimation yields an estimator of $1/\beta$ as follows:

$$\frac{cov(z_{js}, x_{js-1})}{cov(x_{js}, x_{js-1})} = \frac{cov(\frac{1}{\beta}x_{js} + u_{js}, x_{js-1})}{cov(x_{js}, x_{js-1})} = \frac{1}{\beta} + \frac{cov(-\frac{1}{\beta}\varepsilon_{x,js} + \varepsilon_{z,js}, \beta y_{js-1} + \varepsilon_{x,js-1})}{cov(x_{js}, x_{js-1})} = \frac{1}{\beta},$$

since $cov(\varepsilon_{x,js}, \varepsilon_{x,js-1}) = cov(\varepsilon_{z,js}, \varepsilon_{x,js-1}) = 0$. Notice that this procedure remains valid even if observed GDP per capita z_{js} exhibits serial correlation, which is a more distinct possibility should there be persistent errors or reporting biases in this data series.

4.2. Estimating the inverse elasticity of night lights intensity

We implement the above IV approach for two economic outcomes, GDP per capita and urban employment. These latter variables are drawn from the China City Statistical Yearbooks, where they are reported at a lower frequency (annual) and for larger geographic units (prefectures) than the night lights data; when we later use the estimated inverse elasticity to assess grid-level outcomes, this inference will be valid to the extent that the parameter $1/\beta$ is stable across different levels of geographic aggregation and observation frequency.[27]

Using the available data from 2013–2016, we estimate the following:

$$\ln(Y_{js}) = (1/\beta)\ln(Light_{js}) + D_j + D_{vs} + v_{js}, \tag{7}$$

where Y_{js} is GDP per capita or employment in prefecture j in year s, and $Light_{js}$ is the annual mean VIIRS-DNB night lights reading over all grid cells with centroids in the prefecture. The D_j and D_{vs} denote prefecture and province-year fixed effects respectively;

[27] In Table D.12 in the appendix, we obtain comparable estimates for the inverse elasticity with respect to GDP per capita using a panel of counties; note though that the requisite data on GDP are available for only about two-thirds of China's counties.

Table 2
GDP per capita, employment and night lights intensity.

Panel A.	Dep. Var.: $\ln(GDPpc_{js})$						
	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS
$\ln(Light_{js})$	0.2238***	0.1955***	0.1563***	0.4104*	0.4667**	0.4698***	0.5534***
	(0.0601)	(0.0518)	(0.0343)	(0.2273)	(0.1821)	(0.1536)	(0.1935)
Observations	1133	1115	1018	1133	1115	1018	1018
Partial R-squared	0.0309	0.0339	0.0399	–	–	–	–
F-stat	–	–	–	54.43	41.18	29.80	28.48
Panel B.	Dep. Var.: $\ln(Emp_{js})$						
	(8) OLS	(9) OLS	(10) OLS	(11) 2SLS	(12) 2SLS	(13) 2SLS	(14) 2SLS
$\ln(Light_{js})$	0.0895*	0.0748**	0.0820**	0.4704**	0.2708***	0.3021**	0.3014**
	(0.0523)	(0.0281)	(0.0355)	(0.1922)	(0.0918)	(0.1242)	(0.1182)
Observations	1133	1116	1013	1133	1116	1013	1013
Partial R-squared	0.0041	0.0056	0.0093	–	–	–	–
F-stat	–	–	–	54.43	48.44	47.58	67.37
Province × Year FE	Y	Y	Y	Y	Y	Y	Y
Prefecture FE	Y	Y	Y	Y	Y	Y	Y
Trimmed	N	Tail 1%	Tail 5%	N	Tail 1%	Tail 5%	Tail 5%
Weighted by population	N	N	N	N	N	N	Y

Notes: The dependent variables are log prefecture GDP per capita (Panel A) and log employment (Panel B) respectively. In each panel, Columns 1–3 report OLS regressions, while Columns 4–7 perform 2SLS regressions in which the lagged night lights variable, $\ln(Light_{j,s-1})$, is used as an IV. Columns 2 and 5 drop observations where the annual change in log prefecture GDP per capita (respectively, log employment) between years $s-1$ and s is below the 1st percentile or above the 99th percentile values of its distribution in that year. Columns 3, 6 and 7 further drop observations where the annual change in the dependent variable is below the 5th percentile or above the 95th percentile of its distribution in each given year. Column 7 weights the observations by initial prefecture population in 2015. The partial R-squared reports the share of variation explained after partialling out the prefecture and province-year fixed effects. Standard errors are clustered at the province level. *** p < 0.01, ** p < 0.05, * p < 0.1.

these help to absorb lingering forces that might be a source of serial correlation in measured night lights that is not attributable to GDP per capita (i.e, serial correlation in the $\varepsilon_{x,js}$'s that would otherwise be present in the v_{js} regression residuals).

Table 2 presents the results from our estimation of (7). Starting in Panel A with GDP per capita as the outcome variable, Column 1 presents an OLS specification where we use all available data, covering about 280 of China's 333 prefectures. We successively trim out data points in Column 2 (respectively, Column 3) for which the change in log GDP per capita over the prior year lies in the tail 1% (respectively, 5%) of values for that given year, since there are outliers with large year-to-year swings in this variable. We obtain positive and significant estimates for the inverse elasticity of night lights ranging from 0.16 to 0.22. As argued above, these OLS estimates would be downward-biased relative to the true $1/\beta$ if there is measurement error in the night lights data. Columns 4–7 turn then to 2SLS estimation, where we use the one-year lag, $\ln(Light_{j,s-1})$, as the IV.[28] Columns 4–6 report the 2SLS analogues of Columns 1–3; the estimates here associate a 1% increase in night lights intensity with a 0.41–0.47% rise in GDP per capita. Lastly, Column 7 re-runs Column 6 using prefecture population as observation weights; this once again yields a significant positively-sloped relationship. Moving forward, we will use the elasticity recovered from the unweighted specification in Column 6, which implies a slightly more conservative range of GDP per capita effects. In the residualized scatterplots in Figure D.4 in the appendix, we further verify that this estimated relationship is not driven by outliers, nor are there indications of non-linearities.[29]

It is helpful to benchmark these elasticities from Table 2 against what has been found elsewhere in the night lights literature. These comparisons come with caveats attached, as there are differences in datasets and methodologies across studies: Most existing papers focus on countries as the unit of analysis, and are based on the DMSP-OLS satellite data (the VIIRS-DNB's precursor); also, studies often estimate the relationship between night lights and GDP, rather than GDP per capita. Be that as it may, our estimate of $1/\beta = 0.47$ for China is generally smaller than what has been found in cross-country studies. For example, Table 5 in Henderson et al. (2012) implies a range of values for the inverse elasticity with respect to GDP that is between 0.58 and 0.97, while the more recent work of Hu and Yao (2022) has a central estimate of $1/1.317 = 0.76$ (see Appendix D.2 for more details on the methodologies in these papers). On the other hand, Storeygard (2016) reports a night lights coefficient of 0.25 when running a long-difference regression using Chinese prefecture-level GDP data over 1992–2005 (see his Table 1), which is similar in magnitude to our OLS estimates in Panel A of Table 2.

[28] As the VIIRS-DNB commences in April 2012, the mean night lights values for 2012 are computed using fewer months of data. This could introduce noise to these observations, but as these are only used in the first stage as part of the IV, the 2SLS coefficient in principle continues to yield an unbiased estimate of $1/\beta$.

[29] While we report standard errors clustered at the province level as a baseline, we have obtained very comparable results via a province-block bootstrap procedure. For the Column 6 specification, the standard error implied from 500 bootstrap samples is 0.1683 (versus 0.1536 in Table 2); the corresponding block bootstrap standard error in the Column 13 specification for employment is 0.1146 (versus 0.1242 in the table).

Given that we propose a novel approach for uncovering $1/\beta$, it is useful too to benchmark our estimates against what we obtain when we apply this panel IV procedure – based on the regression model in (7) – on a standard cross-country dataset. Table D.8 in the appendix provides this point of reference, using the dataset on DSMP-OLS night lights and GDP per capita for 1993–2010 from Pinkovskiy and Sala-i Martin (2016). The 2SLS estimates of the inverse elasticity of night lights to GDP per capita range from 0.37 to 0.41, which is comparable with the values we have just reported for the Chinese prefecture-level data.[30]

Turning to Panel B of Table 2, we run a parallel set of regressions using prefecture urban employment as the outcome variable.[31] We obtain OLS estimates of the inverse elasticity of night lights to employment of around 0.08 (Columns 8–10). These once again are biased downward relative to the 2SLS estimates in Columns 11–14, which range from 0.27 to 0.47. We will use the Column 13 estimate of 0.30 in what follows, particularly since the trimming of tail observations associated with large changes in employment moderates the influence of outliers on the estimated magnitude of this elasticity.[32]

Two remarks are in order. First, we have given consideration to the possibility that the relationship with night-time luminosity might vary across prefectures that differ along key dimensions. On this, Table D.9 in the appendix finds no significant evidence of such heterogeneity in $1/\beta$ – for either GDP per capita or employment – across prefectures with different initial rural population share, manufacturing employment share, or capital intensity in local production (proxied by fixed assets per worker). Second, it is interesting that the value of $1/\beta$ estimated for GDP per capita is larger than that for urban employment across all specifications (particularly once outliers are trimmed). Note that GDP per capita, Y/L, and urban employment, L_u, are linked via the identity: $w_u L_u / L = \alpha_u s_u (Y/L)$, where w_u and α_u are respectively the wage rate and the labor share in the urban sector, and s_u is the share of the urban sector in prefecture GDP. Suppose that the labor share α_u is pinned down by technology and thus stable. In response to a decrease in night lights, the more negative response of GDP per capita compared to urban employment would then have to be accompanied by some combination at least in relative terms of a fall in urban wages (w_u), a rise in the urban sector's share of output (s_u), or a rise in population (L).[33] Although we are unable to make sharper quantitative statements due to the lack of data on urban wages and the urban output share, this discussion nevertheless highlights how other key economic variables would need to adjust to be consistent with the relative sizes of the estimated $1/\beta$'s for GDP per capita and for employment.

4.3. Effects on GDP per capita and employment across locations

We explore now the implications of the US tariffs for local economic outcomes within China. To fix ideas, consider first a one-percentage-point increase in a grid location's exposure to these tariffs on Chinese exports to the US. Our preferred estimate in Column 4 of Table 1 indicates that this reduces night lights intensity by 0.59 percent relative to other locations. Combining these with the inverse night lights elasticities obtained in the previous subsection, this translates to a decrease in income per capita of $0.59 \times 0.47 = 0.28\%$ and a fall in employment of $0.59 \times 0.30 = 0.18\%$.

In practice, there was substantial skew across grids in the severity of exposure to the US tariffs, and the most negatively affected locations saw a cumulative tariff shock well in excess of one percentage point. To illustrate this, we sort mainland China's population into percentile bins in ascending order of the increase in $USTariff_i$ between Q4/2017–Q4/2019. The direct exposure of the median individual to the US tariffs is zero, and this remains small even up to the 70th population-weighted percentile bin (which had a cumulative US tariff shock over the two-year period of 0.6 percentage points). On the other hand, the tail 2.5% of China's population in the most exposed grids experienced a US tariff shock of 9.1 percentage points. These latter grids are geographically spread out across China, with 203 prefectures having at least one grid cell in this right tail bin.[34]

This dispersion in tariff exposure translates into highly heterogeneous impacts on night lights, income per capita, and employment. We compute these implied effects by percentile bin using the tariff shock and night lights coefficients (0.59, 0.47, and 0.30) that were applied above; Fig. 3 presents these together with 90% confidence intervals (based on Monte Carlo draws from the underlying distributions of the estimated coefficients). To be clear, the calculations here use a uniform inverse night lights elasticity for GDP per capita (respectively, employment), and so the heterogeneity in implied effects that is illustrated is driven by the different-sized tariff shocks experienced across the grid cells. The implied effects at the 70th population-weighted percentile bin are −0.16% for GDP per capita and −0.10% for employment. For the 97.5th percentile bin, the negative hit is much more sizeable at −2.52% for income per capita and −1.62% for employment, holding all else constant. These latter effects are economically meaningful, even bearing in mind that they are spread over 24 months; as a point of reference, the annual growth rate of GDP per capita for China as a whole was 11.7% in 2017–2018 and 2.4% in 2018–2019 (based on World Bank data).

---

30 For completeness, we present estimates for our sample of the inverse night lights elasticity with respect to GDP and population separately in Tables D.10-D.11 in the appendix. As the discussion there makes clear, we prefer to work with GDP per capita as our main outcome variable, as the GDP and population data series can exhibit large jumps for prefectures that underwent administrative boundary changes during the sample period.

31 We use the measure of "Persons employed in urban units at year-end" from the China City Statistical Yearbooks.

32 Note that GDP per capita is equal to GDP per urban worker multiplied by the urban-employment-to-population ratio. Under the assumption that prefecture population has been relatively steady during this period, and bearing in mind that we have run log specifications, the estimates in Columns 6 and 13 would then jointly imply an inverse elasticity of night lights to GDP per urban worker of $0.47 - 0.30 = 0.17$.

33 A further implication is that the response of GDP per capita can be smaller than that of the urban wage, as long as the elasticity of the urban employment share, L_u/L, to night lights is smaller than the elasticity of the urban output share, s_u.

34 The grid cells in the top 2.5th population-weighted percentile bin tend to be more developed. The average night light intensity in these grid cells is 1.97, compared to 0.41 in the other grids.

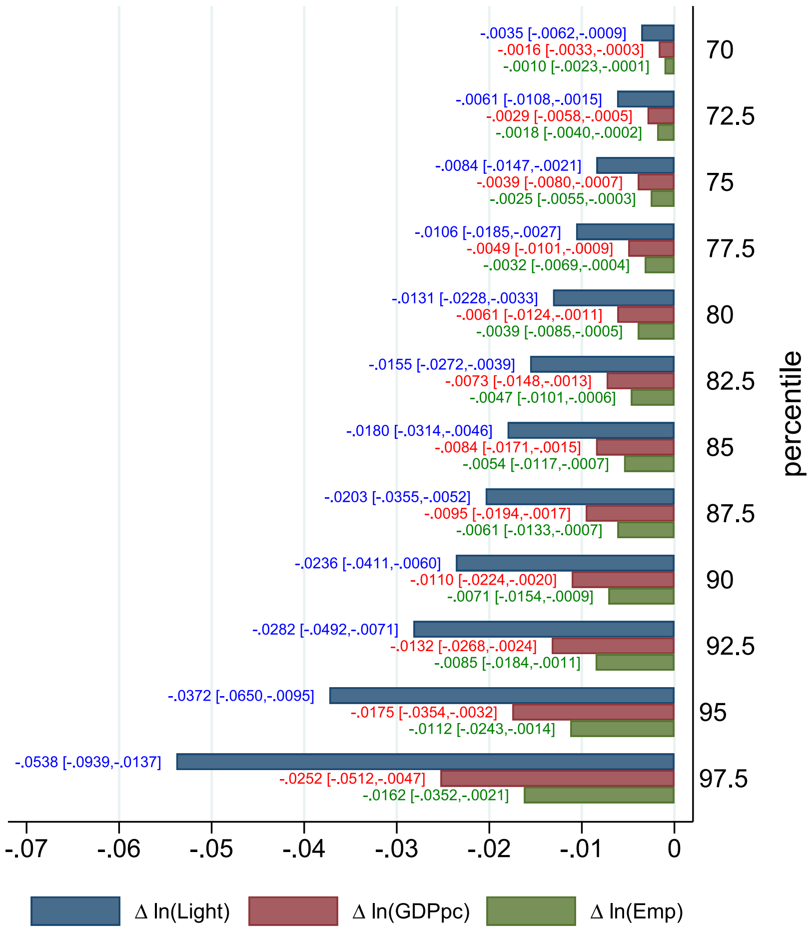

Fig. 3. Relative effects of the US tariff shock: Heterogeneity across locations. *Notes:* The vertical axis indicates grid-level percentiles of cumulative exposure to the US tariffs between Q4/2017 and Q4/2019; the percentiles are weighted by grid population, so that each successive percentile contains an equal share of total population. The figure illustrates the implied grid-level impact of the US tariffs on night lights, GDP per capita, and employment respectively. Point estimates of each implied effect are reported. For night lights, the 90% confidence intervals reported are based on the distribution of the regression coefficient on the US grid-level tariff shock variable in Column 4 of Table 1; for GDP per capita (respectively, employment), the 90% confidence intervals are computed based on 10,000 sets of Monte Carlo draws from the distribution of the aforementioned US tariff shock coefficient and the distribution of the estimated elasticity between GDP per capita (respectively, employment) and night lights from Column 6 (respectively, Column 13) of Table 2.

4.4. Cross-location spillovers

The difference-in-differences nature of our regression models means that the impacts we have just computed should be strictly interpreted as partial equilibrium effects. The fixed effects in (1) absorb the average change across all grids in night lights that might be induced by the US-China tariff war, leaving us with variation that speaks to the differential impact when one location suffers a more severe shock relative to another. This absorbs in particular the effect of cross-grid spillovers – arising from labor mobility or from spatial input–output linkages – that are part of the full general equilibrium impact of the tariffs. The net sign of these spillovers is *a priori* ambiguous. Take the example of spatial input–output linkages: A negative US tariff shock in neighboring locations could depress demand for firms in a given grid cell, if the neighboring firms tend to be downstream buyers of inputs produced by local firms. On the other hand, if the neighboring firms tend instead to be upstream suppliers, then the US tariffs on their exports could end up lowering input prices to the benefit of domestic firms.

To what extent can we make further inferences about the direction and magnitude of the full impact of the US tariffs, that take into account spatial spillovers? We address this via three approaches. First, following Monte et al. (2018), we explore whether the commuting intensity of grid locations might mediate such spillovers that occur through the cross-grid reallocation of workers. Second, with the insights from Adão et al. (2020), we capture the spatial equilibrium linkages by including measures of nonlocal tariff shocks into the regression model. Third, we aggregate the data and conduct the analysis at the prefecture level (c.f., Campante et al., 2023), to obtain estimates of the US and retaliatory tariff shock effects that account for the role of cross-grid spillovers within prefectures as viewed as local labor markets.

Table 3
Spillovers aross grid cells by commuting openness, 2SLS.

Dep. Var.: $\Delta \ln(Light_{it})$ Measure of commuting openness:	(1)	(2) $1-\lambda^R_{cc\|c}$	(3) $1-\lambda^L_{cc\|c}$	(4) $1-\lambda^R_{cc\|c}$	(5) $1-\lambda^L_{cc\|c}$
$\Delta USTariff_{i,t-1}$	−0.6320**				
	(0.2709)				
$\Delta USTariff_{i,t-1}\times$:					
Commuting openness$_c$: above median		−0.7580***	−0.7606**	−0.7632*	−0.7723*
		(0.2698)	(0.2801)	(0.3814)	(0.4073)
Commuting openness$_c$: below median		−0.1094	−0.2016	−0.1007	−0.1392
		(0.3685)	(0.3033)	(0.3994)	(0.3116)
$\Delta CHNInputTariff_{i,t-1}$	0.4071				
	(0.5401)				
$\Delta CHNInputTariff_{i,t-1}\times$:					
Commuting openness$_c$: above median		0.5111	0.7876	0.4616	0.8543
		(0.4846)	(0.4866)	(0.5736)	(0.6077)
Commuting openness$_c$: below median		−0.1491	−0.9812	−0.5975	−1.4617
		(1.6032)	(1.0468)	(1.7425)	(1.1516)
Grid FE	Y	Y	Y	Y	Y
Prefecture × Year-Quarter FE	Y	Y	Y	Y	Y
Grid W_{i0} × Year-Quarter FE	Y	Y	Y	Y	Y
County Characteristics × Tariff Shocks	N	N	N	Y	Y
Observations	343,222	343,222	343,222	343,222	343,222
F-stat	27.18	16.80	14.05	3.697	2.288

Notes: All columns report 2SLS estimates, using the respective Amap grid-level tariff shocks as instrumental variables for the corresponding Google Maps grid-level tariff shocks. The initial grid characteristics, W_{i0}, whose time-varying effects are included in all columns are: the US share in exports, the US share in imports of intermediates, log exports per capita, log intermediate imports per capita, and log 2016 mean night lights intensity; grid-level exports and imports geolocated via Amap are used to construct the first four of these W_{i0} variables. Columns 4 and 5 further control for (demeaned) characteristics of the local county as well as neighboring counties (within 100 km) interacted respectively with $\Delta USTariff_{i,t-1}$ and $\Delta CHNInputTariff_{i,t-1}$; characteristics considered are: log county employment, log land area, and the population share with high school education or above. All regressions are weighted by grid population in 2015, with a minimum population of 1 imputed for cells with zero population in the raw data. Standard errors are clustered at the province level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

4.4.1. Spillovers across grid cells: Commuting flows

As shown in Monte et al. (2018), the general equilibrium elasticity of local employment to a labor demand shock is heterogeneous depending on the nature of commuting flows across locations. Since our analysis focuses on outcomes for granular 11 km-by-11 km units, trade shocks that shift labor demand may easily propagate across grid boundaries through commuting flows.[35] To account for such potential spillovers, we draw on the approach in Monte et al. (2018); specifically, their quantitative spatial model highlights that the initial commuting openness of locations can be incorporated in reduced-form regressions to infer heterogeneous general equilibrium elasticities that embed the role of such cross-location spillovers.

We draw on the 2015 *One-Percent Population Sampling Survey of China*, specifically the 10% subsample; this mini census contains information on residence-workplace commuting patterns within or across county boundaries for workers residing in prefectures with urban districts.[36] Following Monte et al. (2018), we calculate for each county the share of residents who work locally, λ^R_{cc}, and define commuting openness accordingly as $1-\lambda^R_{cc}$. As a robustness check, we also proxy commuting openness with $1-\lambda^L_{cc}$, where λ^L_{cc} is the share of workers who reside locally. We assign to each grid cell the commuting openness of the county that it is located in.

Table 3 incorporates these measures of commuting openness into the regression analysis. Column 1 re-estimates our preferred specification (from Table 1, Column 4), but restricts the sample to grid cells for which there is information on commuting openness (i.e., grids in prefectures with urban districts). This yields once again a negative and significant US tariff shock coefficient that is similar in size to our earlier baseline findings, together with an imprecisely estimated China input tariff shock coefficient. We then introduce interaction terms to explore whether there were heterogeneous responses to the tariff shocks, for grid cells with an above-median (respectively, below-median) commuting openness. This reveals that the negative response of night lights to the US tariff shock was indeed larger in grid cells more open to commuting flows, as captured by $1-\lambda^R_{cc}$ (Column 2) or $1-\lambda^L_{cc}$ (Column 3). The effect of the US tariff shock in cells with low commuting openness is on the other hand statistically insignificant, nor is there evidence of heterogeneous responses by commuting intensity to China's retaliatory tariffs on US inputs. These patterns are consistent with the intuition that the impact of the US tariffs would be more severe where the employment response to a labor demand shock is more elastic due to commuting, indicating that cross-grid spillovers mediated via commuting flows are relevant for fully understanding the impact of the US tariffs on local economic activity.

[35] The median and average one-way residence-workplace commuting distance in China was 3.3 km and 7.1 km respectively, based on information on commuting in the 2015 *One-Percent Population Sampling Survey of China*.

[36] 92% of the observations in the 2015 mini census reside in prefectures with urban districts.

A potential concern is that these results could be confounded by other location characteristics which are correlated with commuting openness. In Columns 4–5, we therefore further control for interactions of $\Delta USTariff_{i,t-1}$ (respectively, $\Delta CHNInputTariff_{i,t-1}$) with characteristics of the county in which the grid cell is located, as well as with the average characteristics of neighboring counties within 100 km. The location characteristics we consider are log employment, log land area, and the population share with high school education or above, following Monte et al. (2018). The negative US tariff shock effect continues to be driven by the response of night lights in locations with high commuting openness, even with these additional controls.

4.4.2. Effects of nonlocal tariff shocks

To account more comprehensively for cross-location spillovers, we take guidance from the quantitative spatial model developed by Adão et al. (2020). In their framework, locations are linked through both trade and worker flows. Under fairly minimal parametric assumptions, they derive a model-implied expression that traces out how external trade shocks affect local economic outcomes; this comprises a local effect – capturing the direct impact of a location's own exposure to the trade shock – and a nonlocal effect – that then jointly reflects the full impact of the shock. The empirical implementation in Adão et al. (2020) proceeds to capture the full effect of the trade shock on local economic outcomes through a specification that includes: (i) a Bartik variable for a location's own-exposure, arising from the initial sectoral composition of its trade-exposed activities (as in Autor et al., 2013), and (ii) a novel nonlocal exposure term, that is a weighted average of the Bartik variables across other locations.

We run regressions of this form in Table 4, by augmenting Eq. (1) as follows:

$$
\begin{aligned}
\Delta \ln(Light_{it}) = {} & \pi_1 \Delta USTariff_{i,t-1} + \pi_2 \Delta CHNInputTariff_{i,t-1} \\
& + \rho_1 \Delta NonLocalUSTariff_{i,t-1} + \rho_2 \Delta NonLocalCHNInputTariff_{i,t-1} \\
& + D_{pt} + D_i + W_{i0} \times D_t + u_{it}, \qquad (8)
\end{aligned}
$$

where the nonlocal US and China input tariff shock variables seek to account for spatial spillovers. As discussed at the start of Section 4.4, the nonlocal exposure effects in (ii) could run in either direction; whether these tend to dampen or amplify the local effect in (i) ultimately needs to be pinned down empirically. The signs of ρ_1 and ρ_2 are thus informative of whether nonlocal forces reinforce (or not) the direct effect of a location's own exposure to the tariffs. From an econometric standpoint, if cross-grid spillovers are relevant, this would call into question the stable unit treatment value assumption (SUTVA); the difference-in-differences coefficients π_1 and π_2 would not then capture the true average treatment effects of direct exposure to these tariffs, unless the nonlocal exposure terms are also explicitly controlled for.[37,38]

We have worked with several constructions of the nonlocal shock terms (see Appendix D.3 for more details). In Columns 1–2 of Table 4, we control for nonlocal tariff shocks from concentric rings of increasing radius around a grid location (<15 km, 15–30 km, 30–50 km, and 50–100 km); the tariff shocks associated with each ring are population-weighted averages of $\Delta USTariff_{i,t-1}$ (respectively, $\Delta CHNInputTariff_{i,t-1}$) across the grids i located within the ring.[39] In Columns 3–4, we collapse the four neighboring-ring US tariff shocks (respectively, China input tariff shocks) into a single measure via a weighted average, where the weights used are equal to the "market potential" of each ring – increasing in population size and decreasing in the radial distance – as implied by a standard gravity model.[40] A potential drawback of these previous approaches is that only spillovers within a 100 km radius are considered. In Columns 5–6, we therefore pursue an alternative approach in which we first compute tariff shocks at the prefecture level, and then take a market-potential-weighted average for each grid location i of these prefecture tariff shocks.[41]

Across all columns of Table 4, we find that accounting for nonlocal tariff shocks does not detract from the negative impact of a location's own exposure to the US tariffs; the magnitude of this direct effect (i.e., the $\Delta USTariff_{i,t-1}$ coefficient) is moreover remarkably stable. This holds for each of the three approaches we implement for constructing the nonlocal shocks, and applies regardless of whether or not we control for the time-varying effects of the auxiliary grid characteristics (the $W_{i0} \times D_t$ terms, included in Columns 2, 4 and 6). The coefficients of the nonlocal tariff exposure terms are themselves of interest: The implied spatial spillovers from either the US tariffs or the China input tariffs take on a negative sign whenever these are precisely estimated. Note though that these effects are uniformly insignificant in the more thorough even-numbered column specifications, which suggests that the strength of spillovers absorbed by a location is closely linked to underlying grid-specific time trends.

Taking stock, the above evidence on nonlocal tariff shocks points us toward the conclusion that spatial spillovers are likely reinforcing the direct negative effect of the US tariffs on observed night lights growth. This implies that the effects we have reported

---

37 Autor et al. (2013) and Adão et al. (2020) use labor-share weights to construct measures of local exposure to import shocks. Information on employment by detailed industries within China is however not available at the granular grid level, hence our use of initial trade share weights; such trade-share weights can be rationalized by log-linearizing the relationship between trade flows and demand shifters (see for example, Campante et al., 2023).

38 The finding of an insignificant π_2 coefficient (from direct exposure to China's retaliatory tariffs) is unchanged when controlling for exposure to tariffs on consumption goods (as classified by the UN BEC), that speaks to exposure via what Adão et al. (2020) refer to as final import expenditure; see Table D.6 in the appendix.

39 Recent examples adopting a similar "donut" ring research design to explore spatial spillovers include: Feyrer et al. (2017), James and Smith (2020), Keller and Shiue (2021), and Myers and Lanahan (2022).

40 The market-potential weight used for each ring r around grid cell i is equal to $L_{r,i} D_{ir}^{-\delta}$, where $L_{r,i}$ is the population located in ring r, D_{ir} is the bilateral distance (set at 7.5 km, 22.5 km, 40 km, and 75 km respectively for $r = 1, \ldots, 4$), and the distance elasticity δ is set to 5 (following Adão et al., 2020).

41 To be more specific, we first compute the tariff shock for each prefecture as a population-weighted average of the US tariff shock (respectively, China input tariff shock) over all cells in the prefecture; we take out grid i itself before computing this for the prefecture in which i is located. In a second step, we average the prefecture US tariff shocks (or China input tariff shocks) using weights $L_{j,i} D_{ij}^{-\delta}$, where $L_{j,i}$ is the prefecture j population, D_{ij} is the distance between grid i and prefecture j, and δ is set to 5 (see Appendix D.3 for more details).

Table 4
Local and nonlocal tariff shocks, 2SLS.

Dep. Var.: $\Delta \ln(Light_{it})$	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta USTariff_{i,t-1}$	−0.6609***	−0.5426**	−0.6802***	−0.5799**	−0.8050***	−0.5639*
	(0.1902)	(0.2614)	(0.1972)	(0.2700)	(0.2342)	(0.2844)
$\Delta USTariff_{i,t-1}$, <15 km ring	−0.8538	−0.1753				
	(0.5424)	(0.5162)				
$\Delta USTariff_{i,t-1}$, 15–30 km ring	−0.8094*	−0.1592				
	(0.4335)	(0.4344)				
$\Delta USTariff_{i,t-1}$, 30–50 km ring	0.4288	0.8396				
	(0.8183)	(0.7730)				
$\Delta USTariff_{i,t-1}$, 50–100 km ring	0.6876	0.6269				
	(1.3275)	(1.2528)				
Nonlocal $\Delta USTariff_{i,t-1}$, in 100 km ring			−1.6113**	−0.4112		
			(0.6961)	(0.6529)		
Nonlocal $\Delta USTariff_{i,t-1}$, all prefectures					−0.7375	0.8142
					(1.6426)	(1.5034)
$\Delta CHNInputTariff_{i,t-1}$	0.1728	0.4127	0.2930	0.4915	0.0978	0.5876
	(0.3342)	(0.6082)	(0.3450)	(0.5755)	(0.3108)	(0.5437)
$\Delta CHNInputTariff_{i,t-1}$, <15 km ring	−1.3886**	0.1414				
	(0.6421)	(0.5975)				
$\Delta CHNInputTariff_{i,t-1}$, 15–30 km ring	−1.4659	−0.3996				
	(1.0011)	(0.9093)				
$\Delta CHNInputTariff_{i,t-1}$, 30–50 km ring	−2.7819**	−1.8007				
	(1.2457)	(1.1936)				
$\Delta CHNInputTariff_{i,t-1}$, 50–100 km ring	−1.9600	−1.3087				
	(2.5769)	(2.4970)				
Nonlocal $\Delta CHNInputTariff_{i,t-1}$, in 100 km ring			−1.4408	0.6153		
			(1.0455)	(0.8451)		
Nonlocal $\Delta CHNInputTariff_{i,t-1}$, all prefectures					−0.4393	1.0919
					(1.7915)	(1.8993)
Grid FE	Y	Y	Y	Y	Y	Y
Prefecture × Year-Quarter FE	Y	Y	Y	Y	Y	Y
Grid W_{i0} × Year-Quarter FE	N	Y	N	Y	N	Y
Observations	669,845	669,845	669,845	669,845	669,845	669,845
F-stat	2.242	2.370	12.66	7.265	16.19	10.28

Notes: All columns report 2SLS estimates, using the respective Amap grid-level and nonlocal tariff shocks as instrumental variables for the corresponding Google Maps grid-level and nonlocal tariff shocks. The initial grid characteristics, W_{i0}, whose time-varying effects are included in even-numbered columns are: the US share in exports, the US share in imports of intermediates, log exports per capita, log intermediate imports per capita, and log 2016 mean night lights intensity; grid-level exports and imports geolocated via Amap are used to construct the first four of these W_{i0} variables. All regressions are weighted by grid population in 2015, with a minimum population of 1 imputed for cells with zero population in the raw data. Standard errors are clustered at the province level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

in Table 1 and Fig. 3, based on difference-in-differences coefficients from a location's direct tariff exposure, are likely lower bounds on the full adverse impact of the tariff shocks on the Chinese economy.

4.4.3. Prefecture-level analysis

As a final exercise, we report in Table 5 results from performing a parallel analysis at the prefecture level. This highlights that the grid-level effects we have uncovered are manifest too in more aggregate spatial units that can be viewed as local labor markets.[42]

We run the following analogue of Eq. (1) at the prefecture level (indexed by j):

$$\Delta \ln(Light_{jt}) = \pi_1 \Delta USTariff_{j,t-1} + \pi_2 \Delta CHNInputTariff_{j,t-1} + D_{prov,t} + D_j + W_{j0} \times D_t + u_{jt}. \tag{9}$$

We construct the US and China input tariff shocks in a manner similar to (2) and (3) using prefecture-level trade shares. As we can observe in the China customs data the prefecture where firms are located – this information is encoded in the unique firm identifiers – the tariff shocks to prefecture j, $\Delta USTariff_{j,t-1}$ and $\Delta CHNInputTariff_{j,t-1}$, can be directly constructed without introducing potential measurement error from geo-locating firm coordinates. We therefore run (9) via OLS, while controlling for prefecture and province-by-year-quarter fixed effects; we report standard errors clustered by province. To bolster the case for identification stemming from the conditional exogeneity of initial prefecture export and import shares, we control for flexible time trends associated with initial prefecture characteristics ($W_{j0} \times D_t$), where W_{j0} comprises: the US share in exports, the US share in imports of intermediates, log exports per capita, log intermediate imports per capita, and log 2016 mean night lights intensity.[43]

[42] The share of workers commuting across prefecture boundaries is close to zero in the 2015 China mini census, which implies that the cross-boundary spillovers reported in Table 5 are not likely to stem from commuting flows.

[43] For the dependent variable, we construct $Light_{jt}$ at the prefecture level as the population-weighted average of grid-level night light intensities. We winsorize the year-on-year change $\Delta \ln(Light_{jt})$ of observations above the 97.5th or below the 2.5th percentile within each period t to reduce the influence of outliers.

Table 5
Tariff shocks and night lights intensity: Prefecture-level analysis.

Dep. Var.: $\Delta \ln(Light_{jt})$		Nonlocal shocks:	
		Neighbors	All Prefs.
	(1)	(2)	(3)
$\Delta USTariff_{j,t-1}$	−0.7473*	−0.8773*	−0.7899*
	(0.4274)	(0.4717)	(0.4294)
$\Delta CHNInputTariff_{j,t-1}$	−0.0371	0.0887	0.0500
	(0.2995)	(0.3469)	(0.3340)
Nonlocal $\Delta USTariff_{j,t-1}$		−2.0048*	0.3023
		(0.9754)	(0.4742)
Nonlocal $\Delta CHNInputTariff_{j,t-1}$		1.1408	1.0027
		(0.7720)	(0.9158)
Prefecture FE	Y	Y	Y
Province × Year-Quarter FE	Y	Y	Y
W_{j0} × Year-Quarter FE	Y	Y	Y
Observations	2259	2259	2259
R-squared	0.6365	0.6383	0.6372

Notes: All columns report OLS estimates. The initial prefecture characteristics, W_{j0}, whose time-varying effects are included are: the US share in exports, the US share in imports of intermediates, log exports per capita, log intermediate imports per capita, and log 2016 mean night lights intensity. Prefecture-level exports and imports are from the China customs data; these are used to construct the first four of these W_{j0} variables. All regressions are weighted by prefecture population in 2010 (from Census data). Standard errors are clustered at the province level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

From this prefecture-level analysis, we continue to obtain a negative and significant US tariff shock effect, but no corresponding significant impact from China's retaliatory tariffs on inputs (Column 1). The US tariff shock coefficient is comparable to that in our grid-level findings, with a slightly larger magnitude (−0.75 versus −0.59); as one might expect, the prefecture-level analysis has weaker statistical power due to a smaller sample size, and hence the coefficient estimates are less precise. The similarity across the prefecture- and grid-level findings is reassuring; it alleviates the concern that the difference-in-differences estimates from the grid-level analysis might mis-state the size of the tariffs' impact if there are spillovers across grid locations within prefectures. The remaining columns in Table 5 account for the possibility of spillovers across prefecture boundaries. Column 2 introduces a nonlocal US tariff shock term (respectively, for China's retaliatory tariffs on inputs) that is a population-weighted average over all prefectures with which j shares a border. Column 3 instead uses a market-potential-weighted average of the tariff shocks experienced in all other prefectures. The local US tariff shock remains important for explaining night lights growth in prefecture j itself; interestingly, the results in Column 2 point to pertinent negative spillovers from the US tariff exposure of one's neighboring prefectures that tend to reinforce the direct impact of these tariffs.[44]

5. Conclusion

We present evidence that the US-China tariff war had a negative impact on China's economy. We circumvent constraints on the availability and reliability of official data by using satellite readings on night-time luminosity, drawing on a recent body of work that has used such measures to infer economic performance. The granularity of the night lights data allows us to work with grid locations as our unit of analysis. We combine this with customs-level information on the initial profile of Chinese firms' exports and imports, together with geo-located firm coordinates, to implement a Bartik identification strategy that uncovers the local impact of the tariffs. Our central finding is that night-time luminosity was dimmer in grids that were more exposed to the US tariffs on Chinese exports, implying worse income per capita and employment outcomes in these locations. These effects were concentrated on the 30% of China's population in the most tariff-exposed grids, and were moreover stronger in locations particularly open to commuting flows. By contrast, China's retaliatory tariffs on imported inputs from the US did not significantly affect night lights; we discuss several avenues – including switching into the processing trade regime – that firms likely adopted to cope with these tariffs.

Our findings contribute toward a better understanding and assessment of how China's economy has been impacted by recent tariff actions. The effects we have identified speak to the usefulness of satellite data for uncovering shifts in economic activity, including short-run contractions, in data-scarce polities such as China. We view this as a useful first step, and welcome future efforts to validate these findings using direct regional or even firm-level data, as and when these might become available. There is much scope too for future work to adopt more model-based quantitative approaches to size up the economic and welfare impact of the US-China tariffs on China, in a manner that captures the rich heterogeneity in local impacts that can arise when accounting for general equilibrium interactions across locations (as exemplified in Adão et al., 2020). More broadly, our analysis raises open questions related to the long-term consequences of the tariffs. Will the rise in processing trade exacerbate distortions and misallocation across firms in the Chinese economy? Given that the tariff war has raised business uncertainty and likely led firms to hold back on investment in production capacity (Altig et al., 2018; Benguria et al., 2022), how will this ultimately affect China's longer-term growth prospects?

[44] In Table D.13, we have conducted a further analysis at the county level, these being sub-prefectural spatial units. We obtain similar results, with a negative and significant US tariff shock effect (coefficient = −0.51), that is moreover driven by the impact on counties with above-median commuting openness (c.f., Table 3).

Declaration of competing interest

The authors declare that they have no known competing interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jinteco.2024.103926.

References

Adão, Rodrigo, Arkolakis, Costas, Esposito, Federico, 2020. General Equilibrium Effects in Space: Theory and Measurement. NBER Working Paper.

Adão, Rodrigo, Kolesàr, Michal, Morales, Eduardo, 2019. Shift-share design: Theory and inference. Q. J. Econ. 134 (4), 1949–2010.

Ahn, JaeBin, Khandelwal, Amit K., Wei, Shang-Jin, 2011. The role of intermediaries in facilitating trade. J. Int. Econ. 84 (1), 73–85.

Altig, David, Bloom, Nick, Davis, Steven J., Meyer, Brent, Parker, Nick, 2018. Are Tariff Worries Cutting Into Business Investment? Federal Reserve of Atlanta Blog.

Amiti, Mary, Kong, Sang Hoon, Weinstein, David, 2020. The Effect of the U.S.–China Trade War on U.S. Investment. NBER Working Paper.

Amiti, Mary, Konings, Jozef, 2007. Trade liberalization, intermediate inputs, and productivity: Evidence from Indonesia. Amer. Econ. Rev. 97 (5), 1611–1638.

Amiti, Mary, Redding, Stephen J., Weinstein, David, 2019. The impact of the 2018 trade war on U.S. prices and welfare. J. Econ. Perspect. 33 (4), 187–210.

Asher, Sam, Lunt, Tobias, Matsuura, Ryu, Novosad, Paul, 2021. Development research at high geographic resolution: An analysis of night-lights, firms, and poverty in India using the SHRUG open data platform. World Bank Econ. Rev. 35 (4), 845–871.

Autor, David, Beck, Anne, Dorn, David, Hanson, Gordon, 2024. Help for the Heartland? The Employment and Electoral Effects of the Trump Tariffs in the United States. NBER Working Paper No. 32082.

Autor, David, Dorn, David, Hanson, Gordon, 2013. The China syndrome: Local labor effects of import competition in the United States. Amer. Econ. Rev. 103 (6), 2121–2168.

Benguria, Felipe, Choi, Jaerim, Swenson, Deborah L., Xu, Mingzhi, 2022. Anxiety or pain? The impact of tariffs and uncertainty on Chinese firms in the trade war. J. Int. Econ. 137, 103608.

Benguria, Felipe, Saffie, Felipe, 2020. The Impact of the 2018-2019 Trade War on U.S. Local Labor Markets. Mimeo.

Blanchard, Emily J., Bown, Chad P., Chor, Davin, 2024. Did trump's trade war impact the 2018 election?. J. Int. Econ. 148, 103891.

Bleakley, Hoyt, Lin, Jeffrey, 2012. Portage and path dependence. Q. J. Econ. 127 (2), 587–644.

Bluhm, Richard, Krause, Melanie, 2022. Top lights: Bright cities and their contribution to economic development. J. Dev. Econ. 157, 102880.

Borusyak, Kirill, Hull, Peter, Jaravel, Xavier, 2022. Quasi-experimental shift-share research designs. Rev. Econom. Stud. 89 (1), 181–213.

Bown, Chad P., 2021. The US–China Trade War and Phase One Agreement. Working Paper 21-2, Peterson Institute for International Economics.

Bown, Chad P., Jung, Euijin, Zhang, Eva, 2019. Trump Has Gotten China to Lower Its Tariffs. Just Toward Everyone Else. Peterson Institute for International Economics.

Bown, Chad P., Kolb, Melina, 2021. Trump's Trade War Timeline: An up-to-Date Guide. Peterson Institute for International Economics.

Brandt, Loren, Li, Bingjing, Morrow, Peter M., 2021. Processing trade and costs of incomplete liberalization: The case of China. J. Int. Econ. 131, 103453.

Brandt, Loren, Morrow, Peter M., 2017. Tariffs and the organization of trade in China. J. Int. Econ. 104, 85–103.

Caliendo, Lorenzo, Parro, Fernando, 2015. Estimates of the trade and welfare effects of NAFTA. Rev. Econom. Stud. 82 (1), 1–44.

Campante, Filipe R., Chor, Davin, Li, Bingjing, 2023. The political economy consequences of China's export slowdown. J. Eur. Econom. Assoc. 21 (5), 1721–1771.

Cavallo, Alberto, Gopinath, Gita, Neiman, Brent, Tang, Jenny, 2021. Tariff pass-through at the border and at the store: Evidence from US trade policy. Am. Econ. Rev.: Insights 3 (1), 19–34.

Charoenwong, Ben, Han, Miaozhe, Wu, Jing, 2020. Trade and Foreign Economic Policy Uncertainty in Supply Chain Networks: Who Comes Home? Mimeo.

Chen, Wei, Chen, Xilu, Hsieh, Chang-Tai, Song, Zheng, 2019. A forensic examination of China's national accounts. Brook. Pap. Econ. Act. 2019 (1), 77–141.

Chen, Tuo, Hsieh, Chang-Tai, Song, Zheng (Michael), 2022. Non-Tariff Trade Barriers in the U.S.-China Trade War. NBER Working Paper No. 30318.

Chen, Xi, Nordhaus, William D., 2011. Using luminosity data as a proxy for economic statistics. Proc. Natl. Acad. Sci. 108 (21), 8589–8594.

Chodorow-Reich, Gabriel, Gopinath, Gita, Mishra, Prachi, Narayanan, Abhinav, 2019. Cash and the economy: Evidence from India's demonetization. Q. J. Econ. 135 (1), 57–103.

Cui, Chuantao, Li, Leona Shao-Zhi, 2021. The effect of the US-China trade war on Chinese new firm entry. Econ. Lett. 203, 109846.

Dai, Mi, Maitra, Madhura, Yu, Miaojie, 2016. Unexceptional exporter performance in China? The role of processing trade. J. Dev. Econ. 121, 177–189.

de Chaisemartin, Clément, D'Haultfoeuille, Xavier, 2020. Two-way fixed effects estimators with heterogeneous treatment effects. Am. Econ. Rev. 110 (9), 2964–2996.

Deb, Pragyan, Gjonbalaj, Albe, Hannan, Swarnali Ahmed, 2019. The Drivers, Implications and Outlook for China's Shrinking Current Account Surplus. IMF Working Paper WP/19/244.

Donaldson, Dave, Storeygard, Adam, 2016. The view from above: Applications of satellite data in economics. J. Econ. Perspect. 30 (4), 171–198.

Du, Xinming, Li, Lei, 2023. When Growth Stumbles, Pollute? Trade War, Environmental Enforcement, and Pollution. Mimeo.

Fajgelbaum, Pablo D., Goldberg, Pinelopi K., Kennedy, Patrick J., Khandelwal, Amit K., 2020. The return to protectionism. Q. J. Econ. 135 (1), 1–55.

Fajgelbaum, Pablo D., Khandelwal, Amit K., 2022. The economic impacts of the US-China trade war. Annu. Rev. Econ. 14, 205–228.

Feng, Chaonan, Han, Liyan, Li, Lei, 2023. Who Pays for the Tariffs and Why? A Tale of Two Countries. CESifo Working Paper No. 10497.

Ferraro, John K., Leemput, Eva Van, 2019. Long-Run Effects of Chinese GDP from U.S.-China Tariff Hikes. FEDS Notes, Board of Governors of the Federal Reserve System, Washington, http://dx.doi.org/10.17016/2380-7172.2382, July 15 2019.

Fetzer, Thiemo, Schwarz, Carlo, 2021. Tariffs and politics: Evidence from Trump's trade wars. Econ. J. 131 (636), 1717–1741.

Feyrer, James, Mansur, Erin, Sacerdote, Bruce, 2017. Geographic dispersion of economic shocks: Evidence from the fracking revolution. Amer. Econ. Rev. 107 (4), 1313–1334.

Flaaen, Aaron, Hortaçsu, Ali, Tintelnot, Felix, 2019. The production, relocation, and price effects of US trade policy: The case of washing machines. Amer. Econ. Rev. 110 (7), 2103–2127.

Flaaen, Aaron, Pierce, Justin, 2019. Disentangling the Effects of the 2018-2019 Tariffs on a Globally Connected U.S. Manufacturing Sector. Working Paper.

Goldsmith-Pinkham, Paul, Sorkin, Isaac, Swift, Henry, 2020. Bartik instruments: What, when, why, and how. Amer. Econ. Rev. 110 (8), 2586–2624.

Goswami, Sanjana, 2020. Employment Consequences of the U.S. Trade Wars. Mimeo.

Gourdon, Julien, Hering, Laura, Monjon, Stephanie, Poncet, Sandra, 2022. Estimating the repercussions from China's export VAT rebate policy. Scand. J. Econ. 124 (1), 243–277.

Grossman, Gene M., Helpman, Elhanan, Redding, Stephen, 2023. When Tariffs Disturb Global Supply Chains. NBER Working Paper No. 27722.

Handley, Kyle, Kamal, Fariha, Monarch, Ryan, 2020. Rising Import Tariffs, Falling Export Growth: When Modern Supply Chains Meet Old-Style Protectionism. NBER Working Paper No. 26611.

He, Chuan, Mau, Kasten, Xu, Mingzhi, 2021. Trade shocks and firms hiring decisions: Evidence from vacancy postings of Chinese firms in the trade war. Labour Econ. 71, 102021.

Henderson, J. Vernon, Squires, Tim, Storeygard, Adam, Weil, David N., 2017. The global distribution of economic activity: Nature, history, and the role of trade. Q. J. Econ. 133 (1), 357–406.

Henderson, J. Vernon, Storeygard, Adam, Weil, David N., 2012. Measuring economic growth from outer space. Amer. Econ. Rev. 102 (2), 994–1028.

Hu, Yingyao, Yao, Jiaxiong, 2022. Illuminating economic growth. J. Econometrics 228 (2), 359–378.

Huang, Yi, Lin, Chen, Liu, Sibo, Tang, Heiwai, 2022. Trade Networks and Firm Value: Evidence from the US-China Trade War. Working Paper.

James, Alexander, Smith, Brock, 2020. Geographic dispersion of economic shocks: Evidence from the fracking revolution: Comment. Amer. Econ. Rev. 110 (6), 1905–1913.

Jiang, Lingduo, Lu, Yi, Song, Hong, Zhang, Guofeng, 2023. Responses of exporters to trade protectionism: Inference from the U.S.-China trade war. J. Int. Econ. 140, 103687.

Jiao, Yang, Liu, Zhikuo, Tian, Zhiwei, Wang, Xiaxin, 2022. The impacts of the U.S. trade war on Chinese exporters. Rev. Econ. Stat. forthcoming.

Ju, Jiandong, Ma, Hong, Wang, Zi, Zhu, Xiaodong, 2020. Trade Wars and Industrial Policy Along the Global Value Chains. Working Paper.

Keller, Wolfgang, Shiue, Carol, 2021. The Economic Consequences of the Opium War. NBER Working Paper No. 29404.

Khachiyan, Arman, Thomas, Anthony, Zhou, Huye, Hanson, Gordon, Cloninger, Alex, Rosing, Tajana, Khandelwal, Amit K., 2022. Using neural networks to predict microspatial economic growth. Am. Econ. Rev.: Insights 4 (4), 491–506.

Kong, Sang Hoon, 2020. Rational Voter Responses to the 2018 Trade War: Evidence from the 2018 U.S. House of Representatives Elections. Mimeo.

Lake, James, Nie, Jun, 2023. The 2020 US presidential election and Trump's wars on trade and health insurance. Eur. J. Political Econ. 78, 102338.

Li, Ben G., Lu, Yi, Sgro, Pasquale, Xu, Xing, 2022a. Trump, China, and the Republicans. Mimeo.

Li, Taipeng, Trimachi, Lorenzo, Xie, Rui, Yang, Guohao, 2022b. Environmental Political Cycles. Mimeo.

Michalopoulos, Stelios, Papaioannou, Elias, 2013. National institutions and subnational development in Africa. Q. J. Econ. 129 (1), 151–213.

Mills, Stephen, Weiss, Stephanie, Liang, Calvin, 2013. VIIRS day/night band (DNB) stray light characterization and correction. SPIE Proc. (8866).

Monte, Ferdinando, Redding, Stephen J., Rossi-Hansberg, Esteban, 2018. Commuting, migration, and local employment elasticities. Amer. Econ. Rev. 108 (12), 3855–3890.

Myers, Kyle, Lanahan, Lauren, 2022. Estimating spillovers from publicly funded R & D: Evidence from the US Department of Energy. Amer. Econ. Rev. 112 (7), 2393–2423.

Nakamura, Emi, Steinsson, Jon, Liu, Miao, 2016. Are Chinese growth and inflation too smooth? Evidence from Engel curves. Am. Econ. J.: Macroecon. 8 (3), 113–144.

New York Times, 2019. China's economic growth slows as challenges mount. 17 Oct 2019.

Pinkovskiy, Maxim, Sala-i Martin, Xavier, 2016. Lights, camera. Income! Illuminating the national accounts-household surveys debate. Q. J. Econ. 131 (2), 579–631.

Rauch, James E., 1999. Networks versus markets in international trade. J. Int. Econ. 48 (1), 7–35.

Roland, Hodler, Raschky, Paul A., 2014. Regional favoritism. Q. J. Econ. 129 (2), 995–1033.

Sheng, Liugang, Song, Huasheng, Zheng, Xueqian, 2023. How Did Chinese Exporters Manage the Trade War? Mimeo.

Storeygard, Adam, 2016. Farther on down the road: Transport costs, trade and urban growth in sub-Saharan Africa. Rev. Econom. Stud. 83 (3), 1263–1295.

Tian, Wei, Yu, Miaojie, Zheng, Chunru, 2022. China-US Trade Protectionism: The Role of China's Retaliatory Tariffs. CCER Working Paper.

Waugh, Michael E., 2019. The Consumption Response to Trade Shocks: Evidence from the US-China Trade War. NBER Working Paper.

Wong, Dorcas, Koty, Alexander Chipman, 2019. The US-China trade war: A timeline.

World Bank, 2017. Growth out of the blue. In: South Asia Economic Focus.

Zhou, Yang, 2020. The US-China Trade War and Global Value Chains. Mimeo.