Supply Chain Disruption and Reorganization: Theory and Evidence From Ukraine's War*

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Abstract

How do supply chain disruption and reorganization shape the impact of large-scale shocks, such as wars? Using firm-to-firm Ukrainian railway-shipment data during the 2014 Russia-Ukraine conflict, we document that firms with prior supplier or buyer linkages to the conflict areas substantially decreased their output. Simultaneously, firms with prior supplier linkages increased the number of suppliers in nonconflict areas, those with prior buyer linkages decreased them, and both firm types saw a reduction in the number of buyers in nonconflict areas. Our production-network disruption model accurately explains the observed firm-level output decline once we account for this network reorganization. The model predicts a 10% reduction in aggregate welfare in nonconflict areas through production-network disruption and reorganization, underscoring that localized conflicts have detrimental, far-reaching economic costs.

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1 Introduction

The modern economy relies on intricate supply chains. These production networks are crucial to the economic activity of firms, regions, and countries. For example, researchers have found that access to cheaper and more diverse intermediate inputs leads to significant productivity gains, both at the firm and aggregate levels (Amiti and Konings, 2007; Goldberg, Khandelwal, Pavcnik, and Topalova, 2010; Halpern, Koren, and Szeidl, 2015). At the same time, these networks also make the economy vulnerable to adverse external shocks. In particular, studies have demonstrated that localized transient shocks from natural disasters can cause extensive economic disruptions by propagating through existing production networks (Barrot and Sauvagnat, 2016; Boehm, Flaaen, and Pandalai-Nayar, 2019; Carvalho, Nirei, Saito, and Tahbaz-Salehi, 2021).

However, firms may also reorganize their production networks when facing more intense and persistent negative shocks, such as wars or conflicts. How firms reorganize their production networks in response to such shocks is theoretically ambiguous. On one hand, firms may be able to find alternative suppliers and buyers to mitigate the disruption. On the other hand, shocks may induce firms to scale down production and stop sourcing from or selling to existing trade partners, generating cascading negative effects on the economy. How production-network structures respond to supply chain disruptions and what is the resulting impact of such reorganization on firm-level and aggregate production and welfare remain open empirical questions.

This paper investigates these questions in the context of the 2014 Russia-Ukraine conflict. This conflict began immediately following the Ukrainian Revolution in February 2014, when the Russian government annexed Crimea and started promoting separatist movements and militant groups in the Donetsk and Luhansk provinces (the Donbas region). The prolonged conflict devastated the Donbas region through bombing, infrastructure destruction, and death; however, the rest of the country was not directly exposed to the violence.[1] This feature of the context allows us to examine the effects of supply chain disruption and reorganization throughout the rest of Ukraine.

We start our analysis by providing reduced-form evidence for the impact of the 2014 Russia-Ukraine conflict on the disruption and reorganization of production networks. To accomplish this,

[1]This situation rapidly changed on February 24, 2022, when Russia launched its full-scale invasion of Ukraine.

we utilize a unique data set of the universe of firm-to-firm railway shipments within Ukraine from 2012 to 2016, capturing around 100 million transactions between approximately 8,500 firms, with information on sender and receiver firm IDs, shipment dates, weights, and origin and destination station codes. These data enable us to map the evolution of Ukraine's production networks before and after the conflict's onset. We augment this data set with firm-level accounting data to gain insights into firm-level production and sales.

We first demonstrate that the conflict resulted in a major disruption of firms' production outside the conflict areas. Using railway-shipment data, we construct proxies for firms' exposure to conflict (hereafter, simply *exposure*) through their suppliers and buyers—measured by the share of transactions with firms in the conflict areas before the conflict. Using a difference-in-differences strategy, we document that firms with positive supplier or buyer exposure experienced a sudden 16% decline in sales compared to firms without any prior trade connections to the conflict areas. These effects hold for both supplier exposure and buyer exposure separately. They are also robust to various checks, such as controlling for the province-year or industry-year fixed effects as well as firms' prior trade with Russia. Dynamic difference-in-differences estimates confirm the absence of pretrends and show that the negative impact persists through the end of our sales data in 2018.

We next show that the conflict shock has led to a systematic reorganization of the production-network structure even outside the conflict areas. To do so, we use our railway-shipment data to define the changes in supplier and buyer linkages before and after the onset of the conflict. We then implement the same difference-in-differences strategy to study how these linkages change depending on firms' supplier and buyer exposure.

Here, we summarize our three main findings from this analysis. First, firms with higher supplier exposure *increased* supplier linkages strictly outside conflict areas. This evidence suggests that the loss of suppliers in conflict areas is partially substituted by the gain in suppliers in nonconflict areas. Second, firms with higher buyer exposure *decreased* supplier linkages strictly outside conflict areas. This evidence is consistent with an interpretation that firms scaled down production in response to reduced demand. Third, firms with higher exposure, for both the supplier and the buyer sides, *decreased* buyer linkages strictly outside the conflict areas. This evidence is consistent with an interpretation that the conflict shocks resulted in production disruption, which led to the loss of buyer linkages even outside the conflict areas. Overall, our evidence suggests that the localized conflict shocks have caused a mix of positive and negative responses in production linkages even outside the conflict areas, depending on whether firms were exposed to the conflict through their suppliers or through their buyers.

Our results so far indicate that the conflict led to disruption and reorganization of production networks in the rest of Ukraine. However, there are at least two limitations in translating these reduced-form estimates into the economy-wide effect. First, our reduced-form evidence is based on a differences-in-differences strategy, comparing firms with different levels of direct supplier and buyer exposure. However, firms without direct production linkages with the conflict areas may also be affected by the shock, e.g., through their higher-order connections in production networks (their suppliers' suppliers, their buyers' buyers, and so on). This leads to a violation of the Stable Unit Treatment Value Assumption (SUTVA), and we cannot interpret the difference-in-differences results as the overall impact of the localized conflict on nonconflict areas. Second, the reduced-form evidence alone does not inform us about how the pattern of production-networks reorganization is related to the changes in firm-level output and aggregate welfare.

To overcome these limitations, we develop a multi-sector and -location general equilibrium trade model to analyze the effects of the disruption and reorganization of production networks. Firms produce differentiated varieties of intermediate inputs. Production requires labor and intermediate inputs sourced from other firms connected through production networks in various locations and sectors. Having a larger number of suppliers benefits production through a love-of-variety effect in intermediate inputs. We also allow for the possibility that supplier and buyer connections change in response to shocks. Productivity and trade-cost shocks to a particular segment of the economy affect firms' output not only through their direct supplier and buyer connections but also through their indirect production linkages and their reorganization in response to shocks.

Our model illustrates how and why disruption and reorganization of production networks affect firm-level output and aggregate welfare. In particular, we show that "supplier access" and "buyer access" serve as sufficient statistics for a firm's output under general equilibrium. Supplier access summarizes the cost linkages of the firm, capturing direct and indirect supplier linkages as well as how these linkages change in response to the shock. Buyer access summarizes the demand linkages of the firm, capturing direct and indirect buyer linkages as well as how these linkages change in response to the shock. Our supplier access and buyer access expressions extend the ones in the gravity trade literature (Redding and Venables, 2004; Donaldson and Hornbeck, 2016) to accommodate the changes in production linkages. Importantly, we derive these expressions as direct functions of observed linkage changes. Hence, our sufficient-statistics expression holds across a broad class of models that take alternative microfoundations for production network formation.

A key benefit of these sufficient statistics is that they allow researchers to directly test our model predictions using observed changes in firm-level output. To implement this approach, we use our railway-shipment data from Ukraine before and after the conflict's onset to estimate supplier and buyer access. We then use these estimates to construct the model-predicted changes in firm output, and we regress the model-predicted changes on the observed changes in firm output. To address potential endogeneity in firm output, we use supplier and buyer exposure interacted with the preconflict dummy as instrumental variables (IV), following our reduced-form estimation strategy.

Our analysis reveals that the IV regression coefficients closely approximate the value one, indicating a one-for-one movement between model-predicted and observed output changes. Crucially, even with tight standard errors, we fail to reject the null hypothesis that the regression coefficient equals one. In contrast, when excluding changes in supplier and buyer linkages during the estimation of supplier and buyer access, the regression coefficients tend to be significantly below one. These findings suggest that disregarding the reorganization of production networks may lead to an underestimation of the variability in firm-level effects of supply chain disruption.

Having validated our model, we use it to assess the aggregate welfare effects of supply chain disruption and reorganization due to the 2014 Russia-Ukraine conflict. To do so, we calibrate our model using the preconflict period. We then undertake a simulation to shut down trade linkages to and from the conflict areas. To highlight the role of the reorganization of production linkages, we conduct this simulation under several alternate scenarios of production-network reorganization. In our baseline scenario, we change the production linkages consistent with our difference-in-differences estimates depending on the firms' supplier and buyer exposure. We compare this baseline scenario to a version where we fix the production linkages at preconflict levels.

We find that aggregate welfare strictly outside the conflict areas decreases on average by 10.2%

in our baseline scenario. This magnitude is sizable, even compared to the direct economic loss in the conflict areas, which contributed about 17.5% of preconflict Ukrainian GDP. These large welfare losses are consistent with the economic importance of the conflict areas within Ukraine's production network before the conflict erupted. This welfare loss is larger for regions that are geographically close to the conflict areas. However, regions that are geographically remote from the conflict areas (e.g., the western side of Ukraine), particularly those that specialize in the manufacturing sector, also face substantial welfare loss. Thus, the localized conflict triggers far-reaching adverse economic repercussions through the disruption of production networks.

We also find that the reorganization of production linkages plays a quantitatively important role in the aggregate welfare loss. If we shut down the increase in supplier linkages by firms with high supplier exposure, the welfare loss increases to 12.4%. This result indicates that the substitution of supplier linkages toward nonconflict areas tends to mitigate the aggregate welfare loss from supply chain disruption. Alternatively, if we shut down the decrease in supplier linkages by firms with high buyer exposure, the welfare loss decreases to 6.6%. This result indicates that scaling down supplier linkages by these firms amplifies the negative effects of supply chain disruption. Finally, if we shut down both channels, thereby fixing the production networks at the preconflict levels, we find an 8.8% reduction in aggregate welfare, smaller in magnitude than our baseline scenario. Therefore, completely abstracting from network reorganization leads to an underestimation of aggregate welfare loss. In other words, the negative effects of supplier loss by firms with high buyer exposure dominate the positive effects of supplier recovery by firms with high supplier exposure on aggregate welfare.

Related literature. First, we contribute to the existing literature on supply chain disruptions, augmenting it with a more careful approach to network reorganization. Existing literature has documented that negative transient shocks, particularly natural disasters, transmit through production networks. Our paper is most closely related to Carvalho et al. (2021), who present evidence indicating that a localized transient earthquake shock negatively affects the outputs of firms with suppliers and buyers in affected areas, using the 2011 Tohoku earthquake and tsunami in Japan as a case study. They also quantify the aggregate effects using a model with fixed production networks. While the assumption of fixed production networks is plausible in the context of transient shocks, it is unlikely to hold for a larger, more prolonged, and persistent shock. We provide evidence that firms reorganize production networks in response to such shocks and that our model fails to explain the large drop in firm output if one abstracts from such reorganization.

Our paper is also related to Khanna, Morales, and Pandalai-Nayar (2022), who provide empirical evidence on how firms' production, supplier retention, and acquisition patterns are affected if their existing suppliers were exposed to lockdowns during the COVID-19 pandemic in India. Besides an obvious difference in the contexts, we show how firms reorganize *both* supplier and buyer linkages in response to *both* supplier and buyer exposure to conflict areas, thereby capturing comprehensive patterns of the reorganization of production networks. In particular, we show that firms with prior supplier linkages to conflict areas *increase* their supplier linkages in nonconflict areas, while those with prior buyer linkages *decrease* them, and these two opposite responses have offsetting effects on aggregate welfare.[2]

We also contribute to the theoretical literature on modeling the endogenous formation of production networks. This literature has sought various microfoundations for the formation of supply chain linkages and production networks, such as market- or relationship-specific fixed costs, search and matching, and optimal supplier choice.[3] Our theoretical framework is distinct from this literature in its scope. Instead of taking a specific microfoundation of production-network formation, we develop a sufficient-statistics result for firm-level and aggregate welfare changes given *observed* changes in production networks. The benefit of our approach is that, as long as we observe the changes in production networks, our results apply to a general class of models with common production-function assumptions. This approach comes at the cost of not allowing for counterfactual simulation where changes in production networks are not observed; in such a case, researchers

[2]Other recent papers documenting the impacts of firm-level shocks on reorganization of production networks include Huneeus (2018) and Demir, Fieler, Xu, and Yang (2024), who study international supply and demand shocks, Alfaro-Urena, Manelici, and Vasquez (2022), who study the effects of the entry of multinational corporations, and Miyauchi (2023), who studies unanticipated supplier bankruptcy shocks.

[3]For example, Melitz and Redding (2014) and Antras, Fort, and Tintelnot (2017) consider buyer-market and supplier-market entry costs; Lim (2018), Bernard, Dhyne, Magerman, Manova, and Moxnes (2022), and Dhyne, Kikkawa, Kong, Mogstad, and Tintelnot (2023) consider relationship-specific fixed costs; Eaton, Kortum, and Kramarz (2023), Arkolakis, Huneeus, and Miyauchi (2023), Miyauchi (2023), and Demir et al. (2024) consider bilateral search and matching; and Oberfield (2018), Acemoglu and Azar (2020), and Taschereau-Dumouchel (2020) consider optimal supplier choice.

need to impose more structure to predict the counterfactual changes in production networks.[4]

Finally, we build on the literature on the economic effects of conflict. Existing papers mostly focus on the economic consequences of conflict for directly affected firms and regions.[5] Instead, our focus is on the economic spillovers to firms and localities outside the conflict areas through supply chain disruption and reorganization. We provide direct evidence of how firms respond to shocks using transaction-level data on actual firm-to-firm linkages, rarely available in a conflict setting. Furthermore, we show that these spillover effects have large negative effects on the country's economic welfare, thereby providing one mechanism in which localized conflicts within a country tend to have large aggregate consequences (Rohner and Thoenig, 2021). Our empirical evidence resonates with recent findings by Couttenier, Monnet, and Piemontese (2022) that the Maoist insurgency in India negatively affects firm production depending on how their input and output bundles are related to the insurgent areas. Beyond confirming a similar negative effect on firm sales by directly utilizing data on actual trade between firms, we also provide evidence for the reorganization of firm-level production networks and how this reorganization affects firm production and aggregate welfare.

The rest of this paper is organized as follows. Section 2 describes the background of the 2014 Ukraine-Russia conflict and discusses our main data. Section 3 presents our reduced-form results on the conflict's effects on the disruption and reorganization of production networks. Section 4 develops our theoretical framework. Section 5 provides the results of our model-based quantitative analysis. Section 6 concludes.

[4]Our approach is related to Baqaee, Burstein, Duprez, and Farhi (2024), who analyze the role of observed supplier churning on firm production and aggregate GDP. In contrast to their nonparametric approach, we focus on the parametric production function common in the existing literature to derive succinct sufficient statistics and apply them to the context of the 2014 Russia-Ukraine conflict.

[5]See Guidolin and La Ferrara (2007), Amodio and Di Maio (2018), Utar (2018), Ksoll, Macchiavello, and Morjaria (2022), and Del Prete, Di Maio, and Rahman (2023) for the empirical evidence that conflict affects firms in immediate conflict areas using micro data. Hjort (2014) and Korovkin and Makarin (2023) explore alternative channels of spillover effects of conflicts, where conflict-induced intergroup tensions adversely affect both firm productivity and interfirm trade. Finally, researchers have documented the direct effects of violence on the Donbas economy by using nightlight data and other indirect approaches, e.g., see Coupé, Myck, and Najsztub (2016), Mirimanova (2017), and Kochnev (2019). See also Behrens (2024) for the conflict's impact on Russian firms located near Ukraine.

2 Background and Data

2.1 Annexation of Crimea and the Donbas War (2014–2022)

Immediately after the Ukrainian revolution of February 2014, the Russian government annexed Crimea and started promoting separatist movements in the Donetsk and Luhansk provinces (the Donbas region).[6] The annexation was complete by early March 2014; it occurred without direct military conflict. Later, pro-Russian protests ensued in Donbas; groups of protesters captured security service buildings and main administrative buildings. Claiming independence from Ukraine, they formed the Donetsk People's Republic (DPR) on April 7, 2014, and the Luhansk People's Republic (LPR) on April 27, 2014.

In response, the acting Ukrainian president launched an "antiterrorist operation" to suppress these separatist movements. Russia supported the DPR and LPR, among other things, supplying them with military power. A long-lasting conflict ensued, leading to more than 13,000 deaths, 30,000 wounded, and the displacement of hundreds of thousands of civilians.[7] The conflict had been in a rather "frozen" state since the Minsk agreements, especially following the election of President Zelensky. That abruptly changed on February 24, 2022, when Russia launched its full-scale invasion Ukraine.

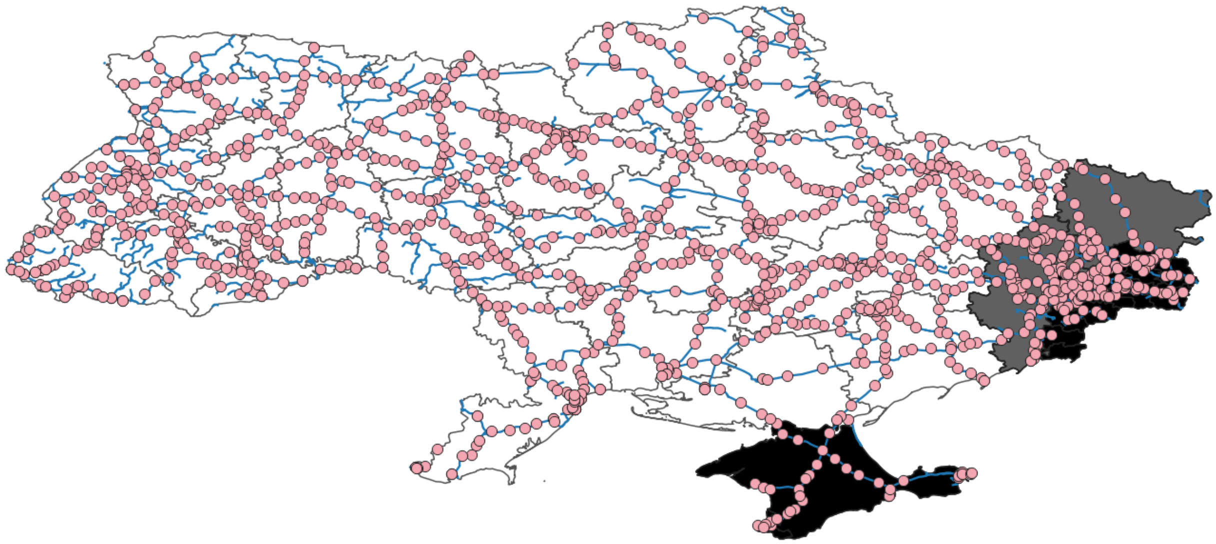
Figure 1 shows the areas directly affected by the 2014 Russia-Ukraine conflict. These areas include Crimea (in black at the bottom) and the DPR and LPR territories (in black with a red rim on the right side of the map). While the conflict was intense in certain DPR and LPR territories, especially at their respective borders, the rest of the country was not exposed to violence directly.

Economic Activity in Donbas and Crimea. Before the conflict, Donbas and Crimea together accounted for a sizeable share of Ukraine's economy—about 17.5% of the country's 2013 GDP. The Donbas region has always been prominent for its extractive industries, especially coal, metallurgy, and manufacturing. Donetsk oblast (province) is the most populous province in Ukraine, with 4.4 million people, or 10% of Ukraine's population; it was responsible for more than 20% of all Ukrainian manufacturing and 20% of all Ukraine's exports in 2013. Luhansk oblast was

[6]The decision to annex Crimea was made secretly by Vladimir Putin and a handful of senior security advisors. It took everyone else by surprise (Treisman, 2018).

[7]See https://neweasterneurope.eu/2019/09/24/the-cost-of-five-years-of-war-in-donbas/.

Figure 1: Conflict Areas (2014–2022) and Railroads in Ukraine



Notes: The map highlights the areas directly affected by the 2014 Russia-Ukraine conflict and displays the geographic location of the railroads and the railway stations. The Crimean Peninsula, in black at the bottom of the map, was annexed by Russia in early 2014. The Donetsk People's Republic (DPR) and Luhansk People's Republic (LPR) are in black at the right of the map. Together with the rest of the Donetsk and Luhansk provinces, in light gray, they form the Donbas region. Blue lines indicate the Ukrainian railroads. Red dots represent the railway stations in our railway-shipments data.

also economically essential; it was the sixth-most-populous Ukrainian province, with 2.16 million people, producing 6% of Ukraine's exports.

In contrast to Donbas, Crimea (population 2.2 million) is known mainly for its agriculture and tourism. However, before the annexation, it was also a vital contributor to Ukraine's economy, at the center of several major industries such as shipbuilding.[8]

The conflict here led to devastating consequences. Crimea was almost entirely cut off from the Ukrainian transportation network, disrupting its supply chain links. The DPR and LPR were overtaken by violence, bombing, destruction of infrastructure and physical capital, and outmigration of the labor force. In just two years, manufacturing production fell by 50% in Donetsk oblast and by more than 80% in Luhansk oblast (Amosha, Buleev, and Zaloznova, 2017, pp.132–133; see also Appendix A.5 for the reduction of firm output in direct conflict areas).

[8]Appendix Figure A.1 shows the distribution of the sales shares of manufacturing, mining, and other sectors across provinces within Ukraine.

Ukrainian Railroad System. Railway transportation is critical for Ukraine's economy. Ukraine has the world's 13th-most-extended railroad network and is the world's seventh-largest railway freight transporter. Railroads are the main vehicle for transporting products in Ukraine: according to UkrStat, as of 2018, railroads were responsible for 80% of ton-kilometers of all freight transport.[9] According to the World Economic Forum 2013–2014 Global Competitiveness Report, the Ukrainian railroad infrastructure was among the best in the world (ranked 25th).[10] In contrast, other modes of transportation were not particularly well maintained. Regular roads and airway transportation ranked poorly in that report (144th and 105th, respectively).

2.2 Data

Firm-to-Firm Railway-Shipment Data. Our main data set is the universe of railway shipments within Ukraine from 2012 through 2016. The data originate from the records of *Ukrainian Railways*, a state-owned railway monopoly company.[11] This data set contains around 100 million transactions between approximately 8,500 firms. It includes shipment dates, weights (in kilograms), freight charges, product codes (ETSNV codes, with around 4,600 unique classifications), and station codes filled out by railway clerks. Importantly, the data set contains unique IDs for the sending and receiving firms, which enables us to merge it with other firm-level data. We use the railway-shipment data both to define firms' preexisting supplier and buyer linkages before the conflict (i.e., supplier and buyer exposure) and as outcome variables for the changes in production linkages before and after the conflict's onset. To focus our analysis on trade between firms, we discard intrafirm trade, which constitutes 6.5% of all transactions in weight shares in 2013.

For some parts of the analysis, we use information about the value of transactions between firm pairs, in addition to the shipment weights and the presence of transaction linkages. Given that the value of transactions is not reported in our data, we impute transaction value using the detailed product codes and shipment weights associated with each transaction. Specifically, we first use separate customs data from Ukraine to obtain the geometric mean of the value per weight of imported and exported product codes at the HS-8-digit code level. We then use the correspondence

[9] http://www.ukrstat.gov.ua/operativ/operativ2018/tr/vtk/xls/vtk_2018_e.xlsx.

[10] https://www.weforum.org/reports/global-competitiveness-report-2013-2014.

[11] These data and our customs data were purchased by CERGE-EI from Statanaliz, LLC, a marketing company that collects and sells data on export and import transactions and domestic shipments for the post-Soviet states.

between the HS-8-digit code and the ETSNV codes (the product code classification in our railway-shipment data) to impute the value of each shipment. Appendix B further describes this procedure.

One limitation of this data set is that we observe the shipment only over railways but not through other transportation modes. We believe our results are not substantially biased by this limitation for two reasons. First, as noted earlier, railroads were responsible for 80% of ton-kilometers of all freight transport due to the relatively high-quality railway network compared to other shipment modes. Second, by focusing on the changes in firm-level trade patterns in our difference-in-differences strategy, any time-invariant factors that affect the coverage rates of railway shipments out of overall shipments are absorbed by the firm-level fixed effects. Therefore, the only identification concern is the presence of systematic time-varying factors in the coverage rates of railway shipments. We argue that assuming away such time-varying factors is plausible, especially when we study the reorganization of production networks *strictly outside conflict areas*, in Section 3.3, as there was no systematic disruption specific to railway networks relative to road networks outside Crimea and Donbas region.[12]

Figure 1 depicts the Ukrainian railway network, as well as the 1,200 railway stations in our data set. The stations cover the entire country, indirectly confirming the universal nature of our data on railway shipments. As one can see, the network is especially dense in the Donbas region, consistent with the Donbas' heavy reliance on railway transportation, given its focus on coal and mineral extraction, metallurgy, and other heavy industries.

Firm Accounting Data. We complement our firm-to-firm railway-shipment data with firm-level accounting data from ORBIS/AMADEUS and SPARK-Interfax. Both of these sources are based on official government statistics, the provision of which is mandatory for all Ukrainian firms except individual entrepreneurs. We combine these two data sets for their complementary coverage of available variables. Hereinafter, for brevity, we refer to the combined data as SPARK-Interfax. The data sets contain information on firm IDs, sales, profits, total costs, capital, and other variables for more than 370,000 Ukrainian firms from 2010 through 2018.

[12]See Appendix C.4 for a more detailed discussion of this identification concern, using a formal model where firms choose shipment modes.

Customs Data. For our value-imputation exercise and for some of the robustness checks, we also use the transaction-level customs data for Ukraine from 2012 through 2016. For each international shipment, we observe its date, weight, value (in Ukrainian hryvnia), product code, direction (export or import), tax ID of the Ukrainian firm, and the counterpart firm's country. We use this data to control for the presence of international transactions in some of our regressions and for imputing transaction values for our railway-shipment data.

Input-Output Tables. We use the official input-output tables produced by the State Statistics Service of Ukraine and published on its website (State Statistics Service of Ukraine, 2021). We use the 2013 version for our model calibration in Section 5.

2.3 Conflict Exposure and Summary Statistics

Our primary reduced-form empirical approach investigates the impact of conflict on firms' output and production linkages by their preexisting trade connections with conflict-affected regions. To do so, we define *conflict areas* as the combination of Crimea and the separatist-controlled parts of Donbas (the DPR and LPR regions). Although Crimea was not directly affected by violence, the trade linkages to both areas were substantially disrupted post-annexation, as we document below.

Table A.1 displays the summary statistics for our data sets, including the pattern of firms' preexisting trade linkages with the conflict areas. Of the firms in our data whose headquarters are strictly outside the conflict areas, 54% traded with the conflict areas in 2012–2013, i.e., before the conflict started. An average firm received 9% of its 2012–2013 incoming shipments from the conflict areas in value (i.e., supplier exposure) and sent 10% of its 2012–2013 outgoing shipments to the conflict areas in value (i.e., buyer exposure).

Besides the disruption of trade linkages within Ukraine, the conflict has also resulted in a disruption of international trade, in particular to and from Russia (Korovkin and Makarin, 2023). In this paper, we primarily focus on the disruption of domestic production networks that reach into the conflict areas in Ukraine. We make this choice because, for Ukrainian firms outside the conflict areas, trade exposure with the conflict areas within Ukrainian borders is substantially larger than that with Russia. According to Table A.1, 54% of the firms in our sample traded with the conflict areas in 2012–2013, but only 23% traded with Russia in that same period. Furthermore, while trade with the conflict areas fell to almost zero (as we show below), trade with Russia as a fraction of GDP declined by only about a half (World Bank, 2016). We also present the robustness of our reduced-form analysis to international trade disruption by controlling for the firms' prewar trade with Russia using our separate customs data.

3 Reduced-Form Evidence

In this section, we provide reduced-form evidence on the impact of the 2014 Russia-Ukraine conflict on firm activity and production networks in Ukraine. Section 3.1 documents a substantial decline in shipments between firms in and outside the direct conflict areas. Section 3.2 shows that firms outside the conflict areas but with prior supplier or buyer linkages to those areas significantly decreased their output. Finally, Section 3.3 reveals that firms with prior supplier and buyer linkages with the conflict areas reorganized their supplier and buyer linkages outside those areas.

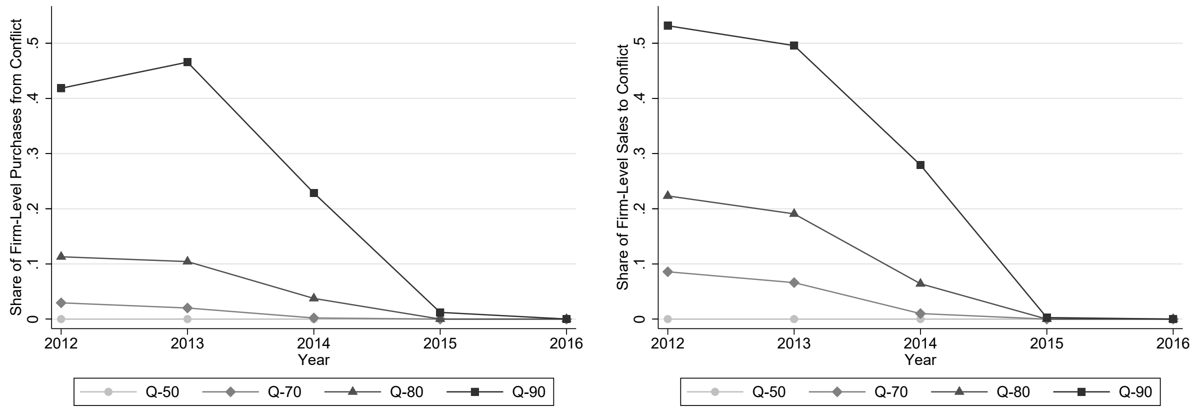
3.1 Impact on Trade With the Conflict Areas

We first examine how the conflict led to the disruption of trade between the conflict-affected areas and the rest of Ukraine. The left panel of Figure 2 illustrates the evolution of input-loading distribution for firms that received any shipments from the conflict areas in 2012–2013. We present the median and upper (70th, 80th, and 90th) percentiles of the distribution of the yearly value of shipments received by a firm from the conflict areas, normalized by the total value of the firm's incoming shipments. The right panel of Figure 2 performs the same analysis, focusing on firms sending their goods to Crimea and occupied Donbas. In both instances, the receiving and sending loading percentiles rapidly plummet, becoming close to zero by 2015 and precisely zero by 2016.

These sharp declining patterns are confirmed in the event-study graphs displayed in Figure A.2, which show that an average firm reduced its share of trade with the conflict areas by approximately 10 percentage points by 2016—the almost entire share of transactions to and from conflict areas—with negligible pretrends prior to the conflict. Figures A.3 and A.4 present results identical to Figures 2 and A.2 but based on shipment weight as opposed to value.

Overall, these estimates suggest that trade between the conflict areas and the rest of Ukraine was severely disrupted as a result of the annexation of Crimea and the war in Donbas. In the DPR and LPR areas, this disruption of transactions is likely driven by the severe disruption of

Figure 2: Distribution of Firm Trade Value Shares With the Conflict Areas



Notes: This figure displays the evolution of the distribution of firm trade share with the DPR, the LPR, and Crimea. Q-50, Q-70, Q-80, and Q-90 refer to the median and upper percentiles of the distribution. The figure on the left (right) describes the distribution for the share of firm sales that went to (purchases that came from) the conflict areas, measured as the value of the shipments sent to (received from) the conflict areas divided by the total value of the shipments sent out (received) by a given firm that year. Value is imputed based on the weight and product type of a given shipment based on the customs data, as described in Appendix B.

firm operation in those areas, coupled with the disruption of transportation and boycotts.[13] In what follows, we analyze the implications of the disruption of trade with the conflict areas for firms' output and reorganization of production linkages strictly outside the conflict areas.

3.2 Impact on Firms Outside the Conflict Areas

Having established that the conflict disrupted trade to and from the conflict areas, we now investigate how firms outside the conflict areas were affected depending on their trade linkages with the direct conflict areas. We combine the data on firms' yearly sales from SPARK-Interfax, data on firms' railway shipments, and measures of preconflict exposure through railway linkages. We start by estimating the following equation:

$$Y_{ft} = \alpha_f + \delta_t + \beta \left(\text{Post}_t \times \mathbb{1}[\text{TradeConflictExposure}]_{f,2012-13}\right) + \varepsilon_{ft} \tag{1}$$

[13]Appendix A.5 examines the impact of the war in Donbas on the sales of firms located directly in the DPR and LPR. We see a large, sharp decline in the reported output of those firms. The official trade blockade of Donbas came into effect only after our study period, in March 2017 (Fisman, Marcolongo, and Wu, 2024), and the official trade blockade of Crimea started only in mid-December 2015 (see, e.g., https://tass.com/world/844510). Therefore, the decline in trade with the conflict areas is not mechanical, with the possible exception of trade with Crimea in 2016.

where f indexes a firm whose headquarters is located strictly outside the conflict areas,[14] t indexes the year, Y_{ft} is an outcome of firm f at year t, α_f and δ_t are the firm and year fixed effects, Post_t is the post-2014 dummy, and $\mathbb{1}[\text{TradeConflictExposure}]_{f,2012-13}$ is an indicator for whether firm f traded with the conflict areas in 2012–2013. We cluster the standard errors at the firm level.

The specification raises two main concerns. First, one may worry about the plausibility of the parallel-trends assumption. Specifically, for β to accurately estimate the causal effect of conflict exposure on firms through production linkages, it is crucial that the outcomes of firms with varying degrees of trade engagement with the conflict areas would have evolved similarly in a counterfactual scenario absent the conflict. Second, the measure of firms' supplier and buyer exposure could be confounded with other conflict-induced shocks that affect either demand (for instance, due to military needs) or supply (such as through an increase in labor supply due to refugee resettlement).

To address the first issue, we present the dynamic difference-in-differences estimates and examine them for potential pretrends. We find no statistically significant pretrends in most outcome variables, consistent with the interpretation that the conflict was unanticipated. To address the second issue, we provide a battery of robustness checks, including controlling for the province-year and industry-year fixed effects, as well as firms' trade with Russia.[15]

Baseline Results. Figure 3 presents our baseline estimates of the conflict's impact on firm sales; here, we have slightly modified Equation (1) by interacting the year fixed effects with the exposure indicator. The results show no pretrends, reinforcing the validity of the parallel-trends assumption introduced above, followed by a sharp, persistent differential drop in firm sales of 10 to 30 log points. This result confirms that the conflict negatively impacts not only firms located near the violence but also those indirectly connected to the conflict areas through production linkages.

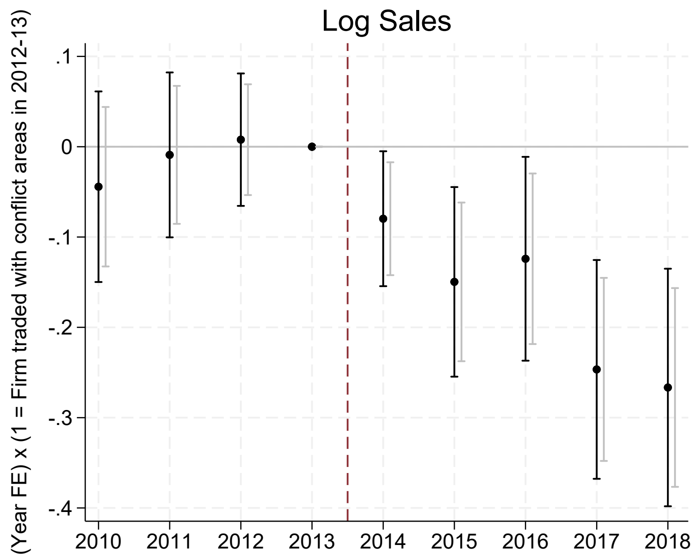
Encouraged by the patterns in Figure 3, we now estimate Equation (1) focusing not only on the annual accounting sales but also on an indicator of whether accounting sales data are missing, which we interpret as an alternative proxy for production disruption.

Columns (1) and (2) of Table 1 present the results. Column (1) shows that firms outside the

[14]Among the robustness checks in Appendix A.2, we show that our results are invariant to using alternative sample restrictions focusing on firms that never used the railway stations located in the conflict areas (Table A.7).

[15]In Appendix A.6, we also provide an analysis of how regions' exposure to the conflict areas through suppliers and buyers relates to changes in population size.

Figure 3: Conflict and Sales of Firms That Traded With the Conflict Areas



Notes: This figure displays the results of estimating Equation (1) and explores the impact of the conflict on firm sales by whether a firm had prior trade ties with the conflict areas. The sample is restricted to firms outside the conflict areas. Black bars represent 95% confidence intervals, gray bars represent 90% confidence intervals. Standard errors are clustered at the firm level.

directly affected conflict areas but with prior trade links to these regions experienced a 15.8% decline in sales compared to firms without such connections on average over five years from the onset of the conflict. Column (2) shows that these firms were also 6.8 percentage points more likely to cease reporting sales data in a given year.

Next, we disaggregate firm connections to the conflict areas into those coming from the supplier side and those coming from the buyer side; we estimate the following specification:

$$Y_{ft} = \alpha_f + \delta_t + \beta \left(\text{Post}_t \times \text{BuyerExposure}_{f,2012-13}\right) + \gamma \left(\text{Post}_t \times \text{SupplierExposure}_{f,2012-13}\right) + \varepsilon_{ft} \tag{2}$$

where $\text{BuyerExposure}_{f,2012-13}$ is measured as the share of firm's out-shipments being to the conflict areas and $\text{SupplierExposure}_{f,2012-13}$ is the share of firm's in-shipments being from the conflict areas, both calculated as value shares. The estimates, presented in columns (3) and (4) of Table 1, demonstrate that connections to the conflict areas, regardless of direction, affect firm performance negatively and with broadly similar magnitudes. Columns (5) and (6) of Table 1 confirm that the

Table 1: Conflict and Sales of Firms Trading With the Conflict Areas

	(1) Log Sales	(2) No Sales Reported	(3) Log Sales	(4) No Sales Reported	(5) Log Sales	(6) No Sales Reported
Post-2014 × 𝟙[Firm traded with conflict areas, 2012–13]	-0.158***	0.068***				
	(0.046)	(0.009)				
Post-2014 × Firm's buyer conflict exposure, 2012–13			-0.212**	0.059***		
			(0.100)	(0.023)		
Post-2014 × Firm's supplier conflict exposure, 2012–13			-0.282***	0.065***		
			(0.099)	(0.021)		
Post-2014 × 𝟙[High firm's buyer conflict exposure, 2012–13]					-0.186***	0.056***
					(0.057)	(0.012)
Post-2014 × 𝟙[High firm's supplier conflict exposure, 2012–13]					-0.138**	0.043***
					(0.054)	(0.011)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean	16.887	0.314	16.887	0.314	16.887	0.314
SD	2.488	0.464	2.488	0.464	2.488	0.464
Observations	35,716	52,317	35,716	52,317	35,716	52,317
Number of Firms	4,816	6,098	4,816	6,098	4,816	6,098

Notes: The table presents the estimates for the conflict's impact on firm sales and an indicator for missing sales data by firms' preexisting trade ties with the conflict areas. High exposure in columns (5) and (6) refers to exposure greater than the 80th percentile in the overall sample. The 80th percentile cutoffs are 0.086 for buyer exposure and 0.083 for supplier exposure. The average buyer and supplier exposure in the high-exposure category are 0.444 and 0.448, respectively, while those in the low-exposure category are 0.004 and 0.006. The sample is restricted to firms outside the conflict areas (DPR, LPR, and Crimea). The firm accounting data from SPARK/Interfax cover the 2010–2018. Standard errors in parentheses are clustered at the firm level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

patterns are robust to defining binary indicators for high supplier or high buyer exposure based on whether they lie above or below 80th percentile in our sample.

These estimates are large compared to existing studies on the effects of supply chain disruptions from transient shocks. For instance, Carvalho et al. (2021) find that firms with at least one supplier or buyer directly exposed to the 2011 Tohoku earthquake and tsunami in Japan saw their sales reduced by 3%–4% the year after. This difference could be driven by the fact that this conflict shock was a larger, more prolonged, and persistent shock, which resulted in the changes in the architecture of production networks. In particular, we show in Section 3.3 that firms with conflict exposure lost buyer linkages even strictly outside the conflict areas. Such reorganization of production linkages is critical to explaining the large effects on firm sales—we revisit this in Section 5.2, with our general equilibrium model of production network reorganization.

Robustness and Heterogeneity. In Appendix A.3, we demonstrate that the findings above are robust to a battery of checks. Tables A.2 and A.3 show that the results are invariant to restricting the sample to firms that reported revenue every year, flexibly controlling for firms' location and distance to the conflict areas interacted with the post indicators, controlling for firms' prewar trade with Russia, including industry-year and province-year fixed effects in the specification, and excluding firms located in non-occupied parts of Donbas or in Kyiv. Table A.4 shows that the results remain similar if we define exposure using shipment weight instead of transaction values. Table A.5 shows that the effects are larger for firms in manufacturing, consistent with the importance of input-output linkages in this sector. The same table also shows that the effects of exposure to Crimea and DPR/LPR regions are similar if we study them separately. Table A.6 shows that our estimates remain robust to controlling for placebo firm exposure as suggested by Borusyak and Hull (2023), thereby dealing with the concern for firms' nonrandom exposure to conflict areas.

3.3 Evidence of the Reorganization of Production Networks

We next show that the conflict shock has led to a systematic reorganization of the production-network structure strictly outside the conflict areas. To do so, we use our railway-shipment data to define the changes in supplier and buyer linkages before and after the onset of the conflict. We then implement our difference-in-differences strategy to study how these linkages change depending on firms' supplier and buyer exposure.

To examine whether firms have reorganized their production linkages strictly outside the conflict areas, we estimate Equation (2) but with the number of trade linkages with nonconflict areas as outcomes. We utilize the data on railway stations to ensure that firms' partners were indeed located outside the conflict areas. To focus on firms for which reorganization of production linkages is well-defined, we restrict our sample to firms that appeared at least once in our dataset before the conflict's onset. To study pretrends and the effect dynamics, we estimate an event-study version of the equation whereby we interact firms' exposure with the year fixed effects.

Baseline Results. Figure 4 presents the resulting estimates for the number of suppliers and buyers in nonconflict areas. In the left figure, we present the results where the dependent variable is the log number of the firms' suppliers strictly outside the conflict areas. In the right figure, we present the results where the dependent variable is the log number of the firms' buyers strictly outside the conflict areas. In both figures, we present the estimated regression coefficients and their 95%

confidence intervals for the interaction between high supplier and buyer exposure (defined by the 80th percentile) and the year fixed effects.

In the left panel of Figure 4, we find that firms with high supplier exposure increased supplier linkages strictly outside the conflict areas. There are no pretrends, and the effects occur immediately after the onset of the conflict in 2014. The magnitudes of the coefficients suggest that if a firm had high supplier exposure to the conflict areas, they increased the measure of supplier linkages from nonconflict areas by around 0.1 log points. Given that the difference in supplier exposure between the high and low exposure is approximately 45 percent, slightly less than a quarter of the loss of suppliers in the conflict areas is substituted by suppliers in nonconflict areas.[16] This evidence is consistent with Khanna et al. (2022), who show that firms whose suppliers were exposed to lockdowns during the COVID-19 pandemic in India acquired new suppliers elsewhere.

We also find that firms with high buyer exposure decreased supplier linkages strictly outside the conflict areas. In contrast to the responses in supplier linkages, this effect occurred relatively gradually over time and became significant in 2015. If a firm had a high buyer exposure to the conflict areas, it decreased the measure of supplier linkages from nonconflict areas by around 0.1 log points in 2015. This evidence is consistent with an interpretation that firms gradually scaled down supplier linkages in response to reduced demand.

In the right panel of Figure 4, we find that firms with both high supplier and buyer exposure decreased buyer linkages strictly outside conflict areas. There are no statistically significant pretrends, and the effects increase gradually as time goes by, reaching 0.15 log points reduction for high supplier exposure firms and 0.2 log points reduction for high buyer exposure firms. This evidence is consistent with an interpretation that both supplier exposure and buyer exposure translated into production disruption, which resulted in the loss of buyer linkages, even in nonconflict areas.

Table 2 displays the nondynamic estimates for the number of linkages. Columns (1) and (2) present the results of the specification using continuous proxies for the supplier and buyer exposure, while columns (3) and (4) use binary indicators based on the 80th-percentile cutoff of the exposure proxies. The results confirm the patterns displayed earlier in Figure 4. Across the board,

[16] Table A.15 displays the estimates for the total number of linkages and shows the negative effects of supplier exposure on the total number of suppliers (column 1).

we find consistent patterns: firms with high supplier exposure increased supplier linkages in non-conflict areas, those with high buyer linkages decreased them, and firms with both high supplier and buyer exposure decreased buyer linkages in nonconflict areas.

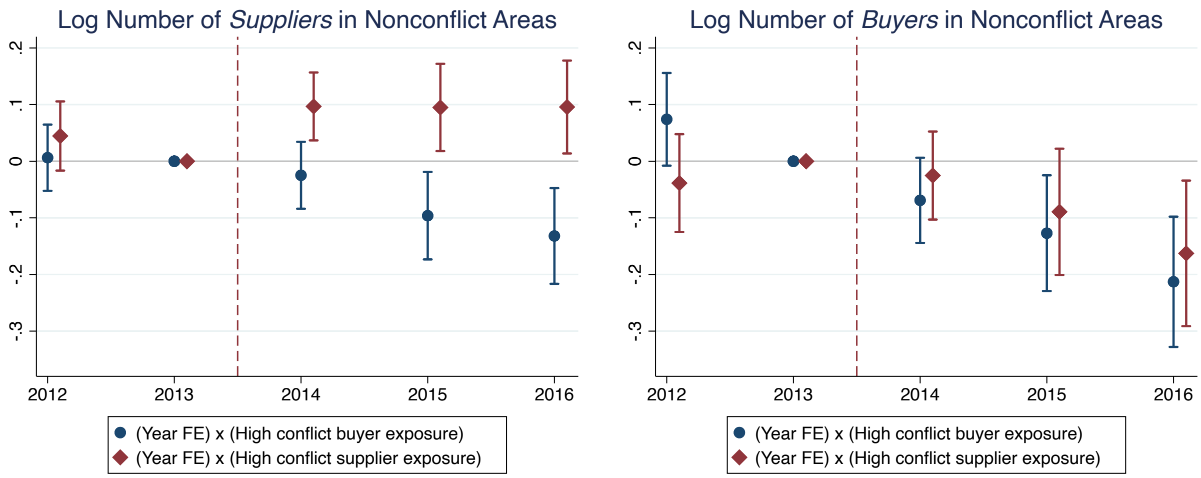
Overall, our evidence suggests that the localized conflict shocks have led to a mix of positive and negative responses in production linkages outside the conflict areas, depending on whether firms were exposed to the conflict through their suppliers or their buyers.

Robustness. In Appendix A.4, we establish robustness of the above results. Tables A.8 and A.9 confirm that the findings withstand a battery of checks introduced in Tables A.2 and A.3, such as only considering firms that sent or received shipments every year or controlling for the industry-year and province-year fixed effects. Table A.10 demonstrates robustness to controlling for placebo exposure following Borusyak and Hull (2023). Table A.11 ensures that our findings are robust to excluding firms that use stations in the conflict areas at least once throughout the data period. Table A.12 show that the effects on shipment weight to and from non-conflict areas mirror those observed for the number of buyers and suppliers. Table A.13 shows that our results are robust if we only count trade partners present in the data before the conflict's onset; therefore, newly registered trading partners after the conflict's onset (e.g., who might have moved from the conflict areas as new entities) do not drive our results. Finally, Table A.14 displays analogous estimates at the firm-region-year level, where 'region' refers to the province of a railway station utilized by the firm.

4 Model

In the previous section, we provide reduced-form evidence for the supply chain disruption and reorganization based on our difference-in-differences method. These estimates, however, do not represent an economy-wide effect because firms without direct production linkages with the conflict areas may also be affected by the shock, for instance, through their higher-order connections in production networks. Nor does the reduced-form evidence inform us about how the pattern of production-network reorganization is related to firm-level sales reduction and aggregate welfare. To overcome these challenges, we build a multi-location and -sector general equilibrium trade model of production-network disruption and reorganization in this section.

Figure 4: Conflict Exposure and Firm's Linkages With Nonconflict Areas



Notes: This figure evaluates whether a firm's number of partners in nonconflict areas changes with the start of the conflict and how it depends on firm-level buyer and supplier exposure. The figure on the left (right) presents the estimates for Equation (2) with the logarithm of the number of suppliers (buyers) as the outcome variable and the indicators for high buyer and high supplier exposure (defined by 80th percentile) as the measures of trade connections with the conflict areas. The 80th percentile cutoffs in our overall sample are 0.086 for buyer exposure and 0.083 for supplier exposure. The average buyer and supplier exposure in the high-exposure category are 0.444 and 0.448, respectively, while those in the low-exposure category are 0.004 and 0.006. Bars represent 95% confidence intervals. Standard errors are clustered at the firm level.

The economy is segmented by a finite number of locations denoted by $u, i, d \in \mathcal{L}$. In each location, there is an L_i measure of households. Each household supplies one unit of labor and earns a competitive wage w_i. There is a fixed mass of firms in each location. Each firm also belongs to a sector denoted by $k, m, l \in K$. Firms produce goods that can be used both for intermediate use and for final use combining labor and intermediate goods. Intermediate goods can be traded across firms in different locations and sectors subject to iceberg trade costs as long as there are production linkages between them. Goods produced for final use are sold to local competitive retailers, and the retailers sell the combined composites to local consumers.

4.1 Production

A continuum of firms produces a distinct variety in each location and sector. To account for a flexible form of firm heterogeneity, we assume that each firm in location i and sector k belongs to a distinct firm type indexed by $\upsilon, \omega, \psi \in \Omega_{i,k}$. These firm types capture the heterogeneity of firm productivity, trade costs, and production linkages. While our model accommodates an arbitrary

Table 2: Conflict Exposure and Firm's Linkages With Nonconflict Areas

	(1) Log # of Suppliers in Nonconflict Areas	(2) Log # of Buyers in Nonconflict Areas	(3) Log # of Suppliers in Nonconflict Areas	(4) Log # of Buyers in Nonconflict Areas
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.074	-0.165*		
	(0.059)	(0.097)		
Post-2014 × Firm's seller conflict exposure, 2012–13	0.297***	-0.196**		
	(0.065)	(0.096)		
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]			-0.086***	-0.170***
			(0.032)	(0.042)
Post-2014 × $\mathbb{1}$[High firm's seller conflict exposure, 2012–13]			0.074**	-0.071
			(0.031)	(0.045)
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean	1.756	1.922	1.756	1.922
SD	1.240	1.489	1.240	1.489
R^2	0.78	0.78	0.78	0.78
Observations	19,839	12,387	19,839	12,387
Number of Firms	4,693	3,198	4,693	3,198

Notes: The table presents the estimates for the conflict's impact on firms' outgoing and incoming trade with nonconflict areas by firms' preexisting trade connections with the conflict areas. The outcomes are the total number of distinct suppliers and buyers that engaged in trade with a given firm during a specific year using a railway station situated outside the conflict areas. High exposure refers to exposure greater than the 80th percentile in the overall sample. The 80th percentile cutoffs are 0.086 for buyer exposure and 0.083 for supplier exposure. The average buyer and supplier exposure in the high-exposure category are 0.444 and 0.448, respectively, while those in the low-exposure category are 0.004 and 0.006. The sample is restricted to firms outside the conflict areas (DPR, LPR, and Crimea) and to firms that existed in our data before the conflict. The firm accounting data from SPARK/Interfax covers 2010–2018. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

dimension of firm heterogeneity, in our quantification in Section 5, we particularly focus on firm heterogeneity with respect to preexisting supplier and buyer linkages to the conflict areas.[17] We denote the measure of type ω firms in location i and sector k by $N_{i,k}(\omega)$.[18]

Production of intermediate goods requires labor and intermediate inputs. Intermediate inputs are sourced from firms that are directly connected by production networks. The production func-

[17]While we assume a discrete number of firm types for expositional purposes, our framework can be extended with a continuum of firm types by replacing summation with integrals.

[18]See Appendix C.3 for an extension that incorporates firms' entry and exit through the changes in $N_{i,k}(\omega)$.

tion of firm type $\omega \in \Omega_{i,m}$ is given by

$$Y_{i,m}(\omega) = Z_{i,m}(\omega) \left(\frac{L_{i,m}(\omega)}{\beta_{m,L}}\right)^{\beta_{m,L}} \prod_{k \in K} \left(\frac{Q_{i,km}(\omega)}{\beta_{km}}\right)^{\beta_{km}} \tag{3}$$

where $Z_{i,m}(\omega)$ is the total factor productivity (TFP), $L_{i,m}(\omega)$ is labor inputs, $Q_{i,km}(\omega)$ is the composite of intermediate inputs, $\beta_{m,L}$ and β_{km} are the parameters proxying sector m's input share for labor and intermediate inputs from sector k, respectively.

The composite of intermediate inputs is a constant elasticity of substitution (CES) aggregator of the input varieties sourced from their connected suppliers. We assume that all firms of type $\omega \in \Omega_{i,m}$ are connected with identical measure of suppliers of type $\upsilon \in \Omega_{u,k}$, denoted by $M_{ui,km}(\upsilon, \omega)$. Therefore, the input composite $Q_{i,km}(\omega)$ is given by

$$Q_{i,km}(\omega) = \left(\sum_{u \in \mathcal{L}} \sum_{\upsilon \in \Omega_{u,k}} M_{ui,km}(\upsilon, \omega) q_{ui,km}(\upsilon, \omega)^{\frac{\sigma_k - 1}{\sigma_k}}\right)^{\frac{\sigma_k}{\sigma_k - 1}} \tag{4}$$

where $q_{ui,km}(\upsilon, \omega)$ is the quantity of input for each variety, and σ_k is the elasticity of substitution of sector k goods. We also assume that, within a firm type, firms are identical in terms of the measure of supplier and buyer connections. Therefore, without risk of confusion, we use firm type $\omega \in \Omega_{i,m}$ to index each firm.

The Cobb-Douglas and CES production-function specification follows and nests many existing models of endogeneous production-network formation.[19] However, unlike these existing approaches, we do not assume specific rules that determine $\{M_{ui,km}(\upsilon, \omega)\}$ in the equilibrium. Instead, we develop sufficient statistics for firm-level and aggregate welfare given *observed* patterns in production linkages without relying on a particular microfoundation for the network reorganization.

[19] For example, our framework nests Melitz and Redding (2014) who model $\{M_{ui,km}(\upsilon, \omega)\}$ by suppliers' decision to enter a buyer market by paying a fixed cost; Antras et al. (2017) who model buyers' decision to enter a supplier market by paying a fixed cost; Lim (2018) and Bernard et al. (2022) who model suppliers' decision to make a transaction with a buyer by paying a relationship-specific fixed cost; and Arkolakis et al. (2023) who model production-network formation under search and matching frictions. Oberfield (2018) and Eaton et al. (2023) instead assume homogeneous inputs with random supplier-buyer-specific idiosyncratic productivity or matching shocks following a Fréchet distribution, which delivers a similar expression for the price index of composite inputs as in Equation (8), where $\sigma_k - 1$ corresponds to the dispersion of idiosyncratic shocks.

Final goods are produced by firms and sold to competitive retailers within the same location. Retailers have access to all firms within the region and produce final goods aggregator using the following technology:

$$Y_i^F = \prod_{k \in K} \left(\frac{Q_{i,k}^F}{\alpha_k} \right)^{\alpha_k}, \qquad Q_{i,k}^F = \left(\sum_{\omega \in \Omega_{i,k}} N_{i,k}(\omega) q_{i,k}^F(\omega)^{\frac{\sigma_k - 1}{\sigma_k}} \right)^{\frac{\sigma_k}{\sigma_k - 1}} \tag{5}$$

where α_k is the final consumption share of sector k, $Q_{i,k}^F$ is the aggregator of goods from sector k, $q_{i,k}^F(\omega)$ is the quantity of final consumption of a variety from firm type $\omega \in \Omega_{i,k}$, and $N_{i,k}(\omega)$ is the measure of type $\omega \in \Omega_{i,k}$ firms.

4.2 Trade Costs, Market Structure, and Prices

The shipment of goods from suppliers of type $\omega \in \Omega_{i,m}$ to buyers of type $\psi \in \Omega_{d,l}$ incurs an iceberg trade cost $\tau_{id,ml}(\omega, \psi)$. From the CES input demand in Equation (4), and the fact that a continuum of suppliers is connected to each buyer, suppliers charge a constant markup $\sigma_m / (\sigma_m - 1)$ on top of their production and shipment costs. The unit price charged by suppliers of type $\omega \in \Omega_{i,m}$ for buyers of type $\psi \in \Omega_{d,l}$ is given by

$$p_{id,ml}(\omega, \psi) = \frac{\sigma_m}{\sigma_m - 1} C_{i,m}(\omega) \tau_{id,ml}(\omega, \psi) \tag{6}$$

where $C_{i,m}(\omega)$ is the marginal cost of production by suppliers in sector m. The marginal cost of production, $C_{i,m}(\omega)$, is in turn derived from production functions (3) and (4) as

$$C_{i,m}(\omega) = \frac{1}{Z_{i,m}(\omega)} w_i^{\beta_{m,L}} \prod_{k \in K} P_{i,km}(\omega)^{\beta_{km}} \tag{7}$$

where $P_{i,km}(\omega)$ is the price index of composite inputs given by

$$P_{i,km}(\omega) = \left(\sum_{u \in \mathcal{L}} \sum_{\upsilon \in \Omega_{u,k}} M_{ui,km}(\upsilon, \omega) p_{ui,km}(\upsilon, \omega)^{1-\sigma_k} \right)^{\frac{1}{1-\sigma_k}} \tag{8}$$

Given the vector of wages $\{w_i\}$ and the production linkages $\{M_{ui,km}(\upsilon,\omega)\}$, Equations (6), (7), and (8) uniquely determine the set of prices $\{p_{id,ml}(\omega,\psi), C_{i,m}(\omega), P_{i,km}(\omega)\}$.

4.3 Trade Flows and Firm Sales

We now derive the trade flows between firm-type pairs. Denote the aggregate input demand by firms of type $\omega \in \Omega_{i,m}$ for input k by $D^*_{i,km}(\omega)$.[20] Then, from the CES input demand (Equation 8), the nominal trade flow of intermediate goods from suppliers of type $\upsilon \in \Omega_{u,k}$ to buyers of type $\omega \in \Omega_{i,m}$ is given by

$$X_{ui,km}(\upsilon,\omega) = \varsigma_k M_{ui,km}(\upsilon,\omega)\tau_{ui,km}(\upsilon,\omega)^{1-\sigma_k} C_{u,k}(\upsilon)^{1-\sigma_k} D_{i,km}(\omega) \tag{10}$$

where $\varsigma_k \equiv \left(\frac{\sigma_k}{\sigma_k-1}\right)^{1-\sigma_k}$, and $D_{i,km}(\omega) \equiv D^*_{i,km}(\omega)/P_{i,km}(\omega)^{1-\sigma_k}$ is the buyers' aggregate demand adjusted by the input price index. This equation is analogous to the gravity equations in trade literature, except that the measure of production linkages $M_{ui,km}(\upsilon,\omega)$ also enters into the expression.

Denote the aggregate intermediate goods sales by firms of type $\omega \in \Omega_{i,m}$ by $R_{i,m}(\omega) = \sum_{l\in K}\sum_{d\in\mathcal{L}}\sum_{\psi\in\Omega_{d,l}} X_{id,ml}(\omega,\psi)$. The following proposition shows a convenient analytical expression for $R_{i,m}(\omega)$.

Proposition 1. *The aggregate intermediate goods sales by firms of type $\omega \in \Omega_{i,m}$ is given by*

$$R_{i,m}(\omega) = \tilde{\varsigma}_m Z_{i,m}(\omega)^{\sigma_m-1} w_i^{\beta_{m,L}(1-\sigma_m)} \mathcal{A}^S_{i,m}(\omega)\mathcal{A}^B_{i,m}(\omega) \tag{11}$$

where $\tilde{\varsigma}_m \equiv \varsigma_m \prod_{k\in K} \varsigma_k^{\beta_{km}(1-\sigma_m)/(1-\sigma_k)}$, and $\mathcal{A}^S_{i,m}(\omega)$ and $\mathcal{A}^B_{i,m}(\omega)$ correspond to supplier and

[20] Specifically, from intermediate goods market clearing,

$$D^*_{i,km}(\omega) = \beta_{km}\frac{\sigma_m-1}{\sigma_m}\left(R_{i,m}(\omega) + R^F_{i,m}(\omega)\right) \tag{9}$$

where $R_{i,m}(\omega)$ and $R^F_{i,m}(\omega)$ are aggregate intermediate goods and final goods sales by firm type $\omega \in \Omega_{i,m}$.

buyer access, defined by

$$\mathcal{A}^S_{i,m}(\omega) \equiv \prod_{k\in K}\left(\sum_{u\in\mathcal{L}}\sum_{\upsilon\in\Omega_{u,k}} M_{ui,km}(\upsilon,\omega)\tau_{ui,km}(\upsilon,\omega)^{1-\sigma_k}C_{u,k}(\upsilon)^{1-\sigma_k}\right)^{\frac{1-\sigma_m}{1-\sigma_k}\beta_{km}} \tag{12}$$

$$\mathcal{A}^B_{i,m}(\omega) \equiv \sum_{l\in K}\sum_{d\in\mathcal{L}}\sum_{\psi\in\Omega_{d,l}} M_{id,ml}(\omega,\psi)\tau_{id,ml}(\omega,\psi)^{1-\sigma_m}D_{d,ml}(\psi) \tag{13}$$

See Appendix C.1 for the derivation. The proposition states that, aside from the constant term $\tilde{\varsigma}_m$, firm sales are exactly decomposed into four terms. First, firm revenue is higher if the firm's productivity $Z_{i,m}(\omega)$ is higher. Second, firm revenue is lower if local wages are higher. The third and fourth terms are supplier and buyer access, which summarize the contribution of upstream and downstream production linkages to firm sales. Supplier access represents the influence of intermediate inputs cost on firm sales, i.e., $\mathcal{A}^S_{i,m}(\omega) \propto \left[\prod_{k\in K} P_{i,km}(\omega)^{\beta_{km}}\right]^{1-\sigma_m}$. It is a CES aggregate of the marginal cost of potential suppliers $C_{u,k}(\upsilon)^{1-\sigma_k}$ weighted by iceberg trade costs $\tau_{ui,km}(\upsilon,\omega)^{1-\sigma_k}$ and the measure of supplier linkages $M_{ui,km}(\upsilon,\omega)$ across all supplier types, locations, and sectors. Buyer access represents the potential of making sales to other firms. It is a sum of demand shifter $D_{d,ml}(\psi)$, weighted by the iceberg trade costs $\tau_{id,ml}(\omega,\psi)^{1-\sigma_m}$ and the measure of buyer linkages $M_{id,ml}(\omega,\psi)$.

The observation that the supplier and buyer access serve as key summary statistics for firm sales under general equilibrium is reminiscent of the observations in the gravity trade literature (Redding and Venables 2004; Donaldson and Hornbeck 2016). We extend their insights by allowing for the effects of the production linkages $\{M_{ui,km}(\upsilon,\omega)\}$.

Proposition 1 provides a useful structural interpretation of the reduced-form results. In Section 3.2, we present evidence that firms outside the conflict areas but with direct supplier and buyer linkages to those areas experience a relative sales decline. However, firms may be indirectly affected through production networks even if they are not directly connected to the conflict areas. Furthermore, changes in production linkages $\{M_{ui,km}(\upsilon,\omega)\}$, as documented in Section 3.3, also affect sales through buyer and supplier access. Proposition 1 provides sufficient statistics that summarize these indirect effects. In the next section, we use these sufficient-statistics results to assess the validity of our model.

4.4 General Equilibrium and Aggregate Welfare

Finally, we close the model under general equilibrium. First, from the assumption of the production function of competitive retailers (Equation 5), the final-goods sales of firm type $\omega \in \Omega_{i,m}$ are given by

$$R^F_{i,m}(\omega) = \frac{\varsigma_m N_{i,m}(\omega) C_{i,m}(\omega)^{1-\sigma_m}}{\left(P^F_{i,m}\right)^{1-\sigma_m}} \alpha_m E_i L_i \tag{14}$$

where E_i is per capita income of residents in location i arising from labor income and firm profit (as discussed below), and $P^F_{i,m}$ is final price index of sector m in location i, given by

$$P^F_{i,m} = \left(\varsigma_m \sum_{\omega \in \Omega_{i,m}} N_{i,m}(\omega) C_{i,m}(\omega)^{1-\sigma_m}\right)^{\frac{1}{1-\sigma_m}} \tag{15}$$

The labor-market clearing at each location is given by

$$w_i L_i = \sum_{m \in K} \sum_{\omega \in \Omega_{i,m}} \beta_{L,m} \frac{\sigma_m - 1}{\sigma_m} \left(R_{i,m}(\omega) + R^F_{i,m}(\omega)\right) \tag{16}$$

where $\beta_{L,m} \frac{\sigma_m - 1}{\sigma_m}$ corresponds to the fraction of labor compensation in firm sales for sector m.

We assume that representative workers in each location own local firms. Therefore, per capita income is given by

$$E_i = w_i + \frac{1}{L_i} \sum_{m \in K} \sum_{\omega \in \Omega_{i,m}} \pi_{i,m}(\omega) \tag{17}$$

where $\pi_{i,m}(\omega)$ is the profit by firm type $\omega \in \Omega_{i,m}$, given by[21]

$$\pi_{i,m}(\omega) = \sum_{m \in K} \frac{1}{\sigma_m} \left(R_{i,m}(\omega) + R^F_{i,m}(\omega)\right) \tag{18}$$

Together, given TFP $\{Z_{i,m}(\omega)\}$, trade costs $\{\tau_{id,ml}(\omega, \psi)\}$, measure of firms $\{N_{i,m}\}$, and

[21] In some existing models of production-network formation, firms use some resources to establish linkages, such as a relationship-specific fixed cost (e.g., Bernard et al., 2022) or search cost (e.g., Arkolakis et al., 2023). Our formulation above is isomorphic as long as these resources are fixed factors owned by local households. Alternatively, our formulation is also isomorphic to models satisfying the macro restriction that the aggregate profit is a constant fraction of aggregate labor compensation (i.e., Macro Restriction 2 in Arkolakis, Costinot, and Rodríguez-Clare, 2012). This assumption is satisfied, for example, in a single-sector version of Arkolakis et al. (2023) using labor and intermediate inputs for search costs.

the rules for production linkages $\{M_{id,ml}(\omega,\psi)\}$, the equilibrium is defined by the set of prices $\{p_{id,ml}(\omega,\psi), C_{i,m}(\omega), P_{i,km}(\omega), P_i^F, w_i\}$, trade flows $\{X_{id,ml}(\omega,\psi)\}$, firm sales $\{R_{i,m}(\omega), R_{i,m}^F(\omega)\}$, profit $\{\pi_{i,m}(\omega)\}$, and residents income $\{E_i\}$ that satisfy Equations (6), (7), (8), (10), (11), (15), (16), (17), and (18).

We also define location i's aggregate welfare by real income, given by

$$\mathcal{W}_i = \frac{E_i}{P_i^F} \tag{19}$$

where $P_i^F = \prod_{m \in K} \left(P_{i,m}^F\right)^{\alpha_m}$.

5 Quantitative Analysis

In this section, we combine our theoretical framework in Section 4 with our production-network data to conduct a quantitative evaluation of the impact of the localized conflict in Ukraine on firm production and aggregate welfare beyond the direct conflict zones.

5.1 Calibration

We start by specifying the location and sector in our model. We set the location $\mathcal{L}$ as oblasts (provinces) within Ukraine. As of 2012, there were 27 oblasts (including two cities of regional significance). In our model, we treat Crimea, Sevastopol, and the occupied parts of Donbas as one single "conflict" location. We treat the parts of Donetsk and Luhansk oblasts under the control of the Ukrainian government as two independent locations. Thus, our location set $\mathcal{L}$ consists of 26 locations, 25 of which are strictly outside the conflict areas. We segment firms into three sectors: Mining, Manufacturing, and Other. We take this definition to reflect the importance of mining and manufacturing sectors in the direct conflict and surrounding areas (see Figure A.1 for the spatial distribution of these industries). We take the unit of "firms" in our model as a combination of firm ID and the province of the railway stations from our railway-shipment data.

In our context, a crucial aspect of firm heterogeneity is the firms' preexisting trade linkages with the conflict areas. We divide the set of firms within a location into four types based on the supplier and buyer exposure with the conflict areas before the onset of the conflict. Specifically, we define high-supplier-exposure firms as those where the value share of in-shipment from the conflict areas in our railway-shipment data is above the 80th percentile of all firms in our sample before 2013, following the definition of high/low exposure in Section 3. Similarly, we define high-buyer-exposure firms as those where the value share of out-shipment to the conflict areas is above the 80th percentile of all firms in our sample before 2013. We then divide firms in each region and sector into four types: (1) high supplier and buyer exposure, (2) high supplier exposure and low buyer exposure, (3) low supplier exposure and high buyer exposure, and (4) low supplier and buyer exposure. These four types of firms correspond to firm types $\Omega_{i,k}$ in our model.

We calibrate structural parameters $\{\beta_{L,m}, \beta_{km}, \alpha_k, \sigma_k\}$ using the aggregate input-output table described in Section 2.2. Specifically, for each sector m, we obtain $\{\beta_{L,m}, \beta_{km}\}$ as the share of labor compensation and the input expenditure from sector k. We obtain $\{\alpha_k\}$ from the household expenditure share for each sector k. Finally, we calibrate the elasticity of substitution $\{\sigma_k\}$ from the ratio between pretax operation surplus and corporate income to nominal output, which corresponds to $1/\sigma_k$ in our model.

Table 3 summarizes these parameter choices. The calibrated parameters follow intuitive patterns. Labor share $\{\beta_{L,m}\}$ is 0.35 for Mining and 0.36 for Other, but just 0.10 for Manufacturing. Final expenditure share $\{\alpha_m\}$ is almost zero for Mining, while 0.6 for Manufacturing and 0.39 for Other. Finally, the elasticity of substitution $\{\sigma_k\}$ ranges from 4.8 (Mining) to 8.1 (Manufacturing). These values are within the range of values found in the existing literature.[22]

For our quantitative analysis below, we also use trade flows across firm types and locations $\{X_{ui,km,t}(\upsilon, \omega)\}$, and production linkages $\{M_{ui,km,t}(\upsilon, \omega)\}$, for each year $t \in$ [2012,2016]. We calibrate these values using our railway-shipment data. To obtain the nominal trade flows $\{X_{ui,km,t}(\upsilon, \omega)\}$, we use the value-imputed transaction volumes of our railway-shipment data, as described in Section 2.2. The measure of production linkages $\{M_{ui,km,t}(\upsilon, \omega)\}$ is simply defined by the unique count of the number of linkages between suppliers and buyers across firm types and locations.

[22]For example, Broda and Weinstein (2006) show that the median estimate of the elasticity of substitution across imported varieties in the United States is 3.1, ranging from 1.2 to 22.1 across sectors.

Table 3: Calibrated Parameters

	Sectors (m)		
	Mining	Manufacturing	Other
(a) β_{km}			
k =Mining	0.11	0.12	0.06
k =Manufacturing	0.18	0.33	0.18
k =Other	0.36	0.45	0.40
(b) $\beta_{m,L}$	0.35	0.10	0.36
(c) α_m	0.01	0.60	0.39
(d) σ_m	4.8	8.1	5.0

Notes: Calibrated parameters based on the aggregate input-output table in 2013 produced by the State Statistics Service of Ukraine.

5.2 Model Validation: Can Production-Network Disruption and Reorganization Explain Observed Changes in Firm Output?

In this section, we show that our model accurately accounts for the changes in firm output in response to conflict shocks. Specifically, we regress our model's prediction for firm output against the observed counterpart, instrumented by the supplier and buyer exposure to the conflict areas. We test the null hypothesis that this regression coefficient equals one, indicating that the model's prediction for firm-output changes moves one for one with the observed firm-output changes.

Empirical Strategy. Reformulating Proposition 1, we have the following relationship for the aggregate intermediate goods sales by firm type ω in sector m, location i, and year t:

$$\log\left[w_{i,t}^{\beta_{m,L}(1-\sigma_m)}\mathcal{A}_{i,m,t}^{S}(\omega)\mathcal{A}_{i,m,t}^{B}(\omega)\right] = \log R_{i,m,t}(\omega) - \log Z_{i,m,t}(\omega)^{\sigma_m-1} \tag{20}$$

The left-hand side of this equation summarizes our model prediction for aggregate intermediate goods sales except for the TFP term. As we discuss below, we can directly estimate supplier and buyer access, $\mathcal{A}_{i,m,t}^{S}(\omega)$ and $\mathcal{A}_{i,m,t}^{B}(\omega)$, using observed trade flows and production networks for each year t. Denoting the corresponding estimates by $\tilde{\mathcal{A}}_{i,m,t}^{S}(\omega)$ and $\tilde{\mathcal{A}}_{i,m,t}^{B}(\omega)$, we test our model prediction by running the following regression:

$$\log\left[w_{i,t}^{\beta_{m,L}(1-\sigma_m)}\tilde{\mathcal{A}}_{i,m,t}^{S}(\omega)\tilde{\mathcal{A}}_{i,m,t}^{B}(\omega)\right] = \gamma \log R_{i,m,t}(\omega) + \eta_{i,m}(\omega) + \nu_{i,t} + \delta_{m,t} + \epsilon_{i,m,t}(\omega) \quad (21)$$

where the unit of observation of the regression is firm-type and year. $\eta_{i,m}(\omega)$ are the firm-type-location-sector fixed effects, $\nu_{i,t}$ are the location-time fixed effects, $\delta_{m,t}$ are the sector-time fixed effects, and $\epsilon_{i,m,t}(\omega)$ is the residual. These last four terms in Equation (21) capture the TFP term ($-\log Z_{i,m,t}(\omega)^{\sigma_m-1}$) in Equation (20), including its time-varying components. $R_{i,m,t}(\omega)$ on the right-hand side are the observed intermediate goods sales obtained by aggregating out-shipment value in our railway-shipment data. $w_{i,t}$ on the left-hand side are the wages in each region and time. Given the lack of reliable data on wages across regions throughout the period we analyze, we construct the proxies for wages using the model's labor-market clearing condition (Equation 16; see Appendix D for further details of the calibration).

Using regression (21), we test for $\gamma = 1$, i.e., whether the changes in our sufficient statistics for TFP-adjusted firm intermediate goods sales move one-for-one with the observed counterpart. Importantly, by controlling for firm-type-location-sector fixed effects, we assess the model performance in terms of time changes beyond the cross-sectional variation.

However, estimating this regression using the ordinary least squares (OLS) estimator is problematic for at least two reasons. First, the unobserved changes in TFP, $\epsilon_{i,m,t}(\omega)$, may be correlated with firm revenue. Second, our measurement of firm revenue, $R_{i,m,t}(\omega)$, may involve measurement error, leading to an attenuation bias for γ.

To deal with these issues, we instead estimate Equation (21) using an instrumental variable (IV) approach leveraging the variation induced by the localized conflict. Specifically, motivated by the difference-in-differences strategy in Section 3, we choose our IVs as the interaction between the preconflict dummy and the dummy for high supplier and buyer exposure to the conflict areas. If the model is correctly specified, and if unobserved changes in the firm's TFP are uncorrelated with the IVs, we expect an estimate of $\gamma = 1$. This assumption implies that the effects of conflict shocks on firms with preexisting supplier and buyer linkages primarily manifest through the disruption and reorganization of production networks rather than through other channels influencing TFP. Since we have only one endogenous variable, while we have multiple candidates for IVs based on either the supplier exposure or the buyer exposure, or both, we execute this model validation using an alternate set of IVs to gauge robustness.[23]

Estimation of Supplier and Buyer Access. We first need to estimate supplier access and buyer access to execute this idea. We do so by using our model prediction of trade flows in Equation (10). By adding the time subscript t and manipulating the equation, the trade flow normalized by the measure of linkages is expressed as

$$\frac{X_{ui,km,t}(\upsilon,\omega)}{M_{ui,km,t}(\upsilon,\omega)} = \xi_{u,km,t}(\upsilon)\zeta_{i,km,t}(\omega)\eta_{ui,km}(\upsilon,\omega)\epsilon_{ui,km,t}(\upsilon,\omega) \tag{22}$$

where $\xi_{u,km,t}(\upsilon) \equiv \varsigma_k C_{u,k,t}(\upsilon)^{1-\sigma_k}$, $\zeta_{i,km,t}(\omega) \equiv D_{i,km,t}(\omega)$, and $\eta_{ui,km}(\upsilon,\omega) \equiv \mathbb{E}_t[\tau_{ui,km,t}(\upsilon,\omega)^{1-\sigma_k}]$, with $\mathbb{E}_t$ indicating expectation over time, and $\epsilon_{ui,km,t}(\upsilon,\omega) \equiv \tau_{ui,km,t}(\upsilon,\omega)^{1-\sigma_k}/\mathbb{E}_t\left[\tau_{ui,km,t}(\upsilon,\omega)^{1-\sigma_k}\right]$ capturing the idiosyncratic changes in trade costs and measurement error. To account for the possibility of zero trade flows on the left-hand side, we estimate Equation (22) using a Pseudo-Poisson Maximum Likelihood (PPML) estimator (see Silva and Tenreyro, 2006) with three-way fixed effects $\tilde{\xi}_{u,km,t}(\upsilon)$, $\tilde{\zeta}_{i,km,t}(\omega)$, and $\tilde{\eta}_{ui,km}(\upsilon,\omega)$, where $\tilde{x}$ denotes the estimates of parameter x. Once we estimate Equation (22), we can use the expressions for supplier and buyer market access up to scale using the empirical analogs of Equations (12) and (13), so that

$$\tilde{\mathcal{A}}^S_{i,m,t}(\omega) = \prod_{k\in K}\left(\sum_{u\in\mathcal{L}}\sum_{\upsilon\in\Omega_{u,k}} M_{ui,km,t}(\upsilon,\omega)\tilde{\eta}_{ui,km}(\upsilon,\omega)\tilde{\xi}_{u,km,t}(\upsilon)\right)^{\frac{1-\sigma_m}{1-\sigma_k}\beta_{km}} \tag{23}$$

$$\tilde{\mathcal{A}}^B_{i,m,t}(\omega) = \sum_{l\in K}\sum_{d\in\mathcal{L}}\sum_{\psi\in\Omega_{d,l}} M_{id,ml,t}(\omega,\psi)\tilde{\eta}_{ui,km}(\omega,\psi)\tilde{\zeta}_{i,km,t}(\psi) \tag{24}$$

To benchmark our results, we also construct the model-predicted change in firm sales abstracting from production-network reorganization. That is, when constructing $\{\tilde{\mathcal{A}}^S_{i,m,t}(\omega), \tilde{\mathcal{A}}^B_{i,m,t}(\omega)\}$ using Equations (23) and (24), we fix the measure of supplier and buyer linkages at the level of

[23] Our idea closely follows Adão, Costinot, and Donaldson (2023), who propose to test a model prediction using orthogonality conditions. See also Donaldson (2018), who uses model-predicted welfare-sufficient statistics to test whether trade mechanism is the main driver of the welfare gains from railway networks in colonial India.

2013 instead of the actual values for each year.[24]

Baseline Results. Table 4 presents our results of the IV regressions (Equation 21). In our baseline estimates, we use two years, 2013 (preperiod) and 2016 (postperiod), while we discuss the robustness of using all years below. In Panel (A), we present our baseline results. In Panel (B), we present the results of the same IV regressions abstracting from the changes in production-linkage reorganization when estimating supplier and buyer access. For each specification, we also report the *p*-value for the Wald test for the null hypothesis that the regression coefficient equals one. We also report the effective first-stage F-statistics in the bottom rows that account for clustered standard errors (Montiel Olea and Pflueger, 2013). Across the board, the F-statistics are large. The strong first stage is consistent with the reduced-form evidence in Section 3.2 that supplier exposure and buyer exposure are both associated with a significant reduction in observed firm-level output.

Columns (1)–(3) of Panel (A) start with the specification where the IV corresponds to the interaction between the preconflict dummy and the dummy variable that takes the value one if the firm type has high supplier exposure *and* high buyer exposure. Column (1) starts with the specification where we control only for firm-type-region-sector fixed effects and year fixed effects. The regression coefficient is 1.01, with a standard error of 0.17. Therefore, while the coefficient is tightly estimated, we cannot reject the null hypothesis that it equals one (with a *p*-value of 0.93). In columns (2) and (3), we show that the patterns are similar by controlling for the sector-year fixed effects and the province-year fixed effects.

In the remaining columns of Panel (A), we execute the same exercise with an alternative set of IVs. In column (4), we use only the high-buyer-exposure (instead of the high-supplier-*and*-buyer-exposure) dummy interacted with the preconflict dummy. We find a coefficient of 1.15, with a standard error of 0.30. Again, we cannot reject the null hypothesis that the coefficient equals one (with a *p*-value of 0.63). In column (5), we use only the high-supplier-exposure dummy for the IV. We still cannot reject the regression coefficient at one (coefficient of 1.11, with a standard error of 0.21).

These patterns are in stark contrast with the specification in Panel (B), where we abstract from

[24] Note that we use the same estimates of gravity equations (22) in this alternative specification. Appendix Table E.1 shows that the model without production-link changes tends to be rejected even when we estimate gravity equations (22) abstracting from linkages, i.e., by eliminating $M_{ui,km,t}(\upsilon, \omega)$ from the denominator of the left-hand side.

Table 4: Model Validation: Model-Predicted and Observed Sales

	$\log w_{i,t}^{\beta_{m,L}(1-\sigma_m)} \tilde{\mathcal{A}}^S_{i,m,t}(\omega) \tilde{\mathcal{A}}^B_{i,m,t}(\omega)$				
	(1)	(2)	(3)	(4)	(5)
Panel A: With Link Adjustment					
$\log R_{i,m,t}(\omega)$	1.01	0.96	1.03	1.15	1.11
	(0.17)	(0.15)	(0.15)	(0.30)	(0.21)
p-value (coefficient = 1)	0.93	0.81	0.82	0.63	0.61
Panel B: Without Link Adjustment					
$\log R_{i,m,t}(\omega)$	0.21	0.19	0.21	0.27	0.31
	(0.10)	(0.09)	(0.07)	(0.13)	(0.13)
p-value (coefficient = 1)	0.00	0.00	0.00	0.00	0.00
IV	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer Exposure	High Supplier Exposure
Effective First-Stage F-Statistics	50.1	54.1	55.6	9.9	23.8
Observations	439	439	439	439	439
Firm-Type-Region-Sector Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Sector $\times$ Year Fixed Effects		X	X	X	X
Region $\times$ Year Fixed Effects			X	X	X

Notes: The table reports the results of estimating Equation (21) regressing model-predicted firm sales on the observed firm sales, with and without allowing for production network reorganization. The level of observation is firm-type and year, for 2013 and 2016. $\log R_{i,m,t}(\omega)$ represents imputed total values of out-shipments in our railway data by firms in region i, sector m, and year t. Standard errors are clustered at the firm-type level. The effective first-stage F-statistics follow Montiel Olea and Pflueger (2013).

the changes in production linkages when estimating supplier and buyer access. In columns (1)–(5), the regression coefficients range from 0.19 to 0.31, with tight standard errors of 0.07 to 0.13. Therefore, we can reject the null hypothesis that the regression coefficient equals one (with *p*-values smaller than 0.01). The fact that the coefficients are significantly below one indicates that the model tends to underpredict the variation of firm-level output changes in response to conflict shocks.

Robustness. To further illustrate why the model without the production-link adjustment fails to capture the observed changes in firm output, in Appendix Tables E.2 and E.3, we report the results where we shut down only changes in buyer linkages and supplier linkages one at a time to compute buyer and supplier access, instead of shutting down both of them simultaneously as in Panel (B) of Table 4. We find that the reduction in buyer linkages, as documented in Table 2, is mostly accountable for the poor performance of the model abstracting from overall production-link changes. When we abstract only from buyer-link changes (Appendix Table E.2), the pattern is broadly similar to Panel (B) of Table 4; and when we abstract only from the changes in supplier linkages (Appendix Table E.3), the pattern is broadly similar to Panel (A) of Table 4.

In Appendix Table E.4, we report the results where we use all five years of data $t \in [2012, 2016]$ to run the regressions in Equation (21) instead of using only 2013 and 2016. We find somewhat larger coefficients [columns (1)–(3) range from 1.28 to 1.40 with standard errors of 0.17-0.19], indicating that the model overpredicts the variation in sales. This pattern potentially suggests that our model performs better in predicting long sales changes rather than yearly sales fluctuations.

5.3 Aggregate Welfare Outside the Conflict Areas

We now use our validated model to analyze how the localized conflict affected aggregate welfare strictly outside the conflict areas. To do so, we first calibrate our model using the trade and production linkages in 2013 using our railway-shipment data. We then run a simulation to make trading with firms in the conflict areas prohibitively costly, i.e., $\tau_{ui,km}(\upsilon, \omega) \to \infty$ if u or i are in the conflict areas. We choose this simulation strategy to reflect the fact that trade with the conflict areas became virtually absent within a few years after the onset of the conflict, as we documented in Section 3.1. We assume that trade costs and firm productivity strictly outside the conflict areas $\{\tau_{ui,km}(\upsilon, \omega)\}$ and firm productivity $\{Z_{i,m}(\omega)\}$ outside the conflict areas are unchanged in this simulation. We also adjust the baseline trade flows to satisfy all equilibrium conditions to enable a well-defined counterfactual simulation.[25]

To reflect the reorganization of production linkages $\{M_{ui,km}(\upsilon, \omega)\}$ as documented in Section 3.3, we also change the production linkages consistent with our difference-in-differences es-

[25]See Appendix C.2 for the system of equations to solve for counterfactual equilibrium and Appendix D for the details of the calibration.

timates depending on the firms' supplier and buyer exposure. In particular, based on column (3) of Table 2, if firm type ω has high supplier exposure, we assume that the firm increases the measure of supplier linkages by 7.4 log points outside the conflict areas, equally across supplier types, locations, and sectors. Similarly, if firm type ω has high buyer exposure, we assume that the firm changes the measure of supplier linkages by -8.6 log points outside the conflict areas. If firms have low supplier and buyer exposure, we assume that the measure of supplier linkages does not change. To benchmark these results, we undertake this simulation with a version where we shut down either or both changes in supplier linkages by firms with high supplier or buyer exposure. We also probe how the results differ by changing the measure of supplier linkages depending on whether the suppliers are directly exposed to shocks.

Before proceeding, we wish to make several remarks on the nature of the simulation. First, we do not introduce any changes in TFP outside the conflict areas. While the results in Section 5.2 are consistent with the interpretation that there are no differential changes in TFPs across firms with different supplier and buyer exposure, the conflict may decrease the TFP of all firms equally, such as through the decline in investment. Second, we do not consider changes in international trade, particularly to and from Russia. Third, we assume that the supplier linkages do not change for firms with low supplier and buyer exposure, thereby abstracting from a potential country-level shift of production-network reorganization. For these reasons, the simulation and the resulting welfare effects should be interpreted solely as the quantification of the production-network disruption and subsequent reorganization, rather than the overall economic cost of the conflict.

Baseline Results. Table 5 reports our results. For each model specification, we report the percentage changes in population-weighted welfare (real income) across provinces outside the conflict areas. We also report the 25th, 50th, and 75th percentiles of the welfare changes across provinces.

Row (1) shows that, in our baseline specification, we observe a 10.2% decline in aggregate welfare strictly outside the conflict areas. This magnitude is sizable, even compared to the direct economic loss in the conflict areas, which contributed about 17.5% of preconflict Ukrainian GDP. This large magnitude of the propagation effects illustrates the intensity of the localized conflict in this context, in contrast to the existing literature focusing on smaller, more transient shocks. For example, Carvalho et al. (2021) quantifies that the 2011 Tohoku earthquake and tsunami in

Table 5: Aggregate Welfare Changes Outside Conflict Areas

	Mean	25%-ile	50%-ile	75%-ile
(1) Baseline (With Supplier Link Adjustment)	-10.2	-13.6	-10.8	-5.5
(2) Shut Down Supplier Link Adjustment by High-Supplier-Exposure Firms	-12.4	-17.2	-13.5	-8.0
(3) Shut Down Supplier Link Adjustment by High-Buyer-Exposure Firms	-6.6	-8.7	-7.5	-2.1
(4) No Link Adjustment	-8.8	-12.3	-8.9	-5.1

Notes: The table presents the results of a counterfactual simulation, specified in Section 5.3, quantifying the role of production network disruption and reorganization in amplifying or mitigating the impact of conflict on the welfare of provinces outside of the conflict areas. For each scenario of the counterfactual simulation, we report the percentage change in population-weighted welfare (real income) across provinces strictly outside the conflict areas. We also report the 25th, 50th, and 75th percentiles of the welfare changes across provinces.

Japan resulted in a 0.47% decline in Japan's real GDP growth in the following year (using a model without changes in production networks). We also find a large regional disparity in the welfare loss: 13.6% at the 25th percentile and 5.5% at the 75th percentile. Below, we further examine the pattern of spatial disparity in the welfare changes.

We also find that the reorganization of production networks has a quantitatively large implication for the welfare effects. In row (2), where we shut down the increase in supplier linkages by firms with high supplier exposure, we find an 12.4% decline in aggregate welfare, which is substantially larger than our baseline specification. This result indicates that the substitution of supplier linkages toward nonconflict areas, as documented in Section 3.3, mitigates the aggregate welfare loss from supply chain disruption.

In row (3), where we shut down the decrease in supplier linkages by firms with high buyer exposure, welfare decreases by a smaller 6.6%. This finding suggests that the trend of firms reducing supplier linkages after losing buyers in the conflict areas, as documented in Section 3.3, significantly magnifies the overall welfare loss at the aggregate level.

Finally, in row (4), if we completely shut down supplier-linkage changes, thereby fixing the production networks at the preconflict levels, we find a 8.8% reduction in aggregate welfare, similar in magnitude to our baseline scenario. Therefore, completely abstracting from network reorganization leads to an underestimation of aggregate welfare loss. In other words, the negative effects of supplier loss by firms with high buyer exposure dominate the positive effects of supplier recovery by firms with high supplier exposure on aggregate welfare.

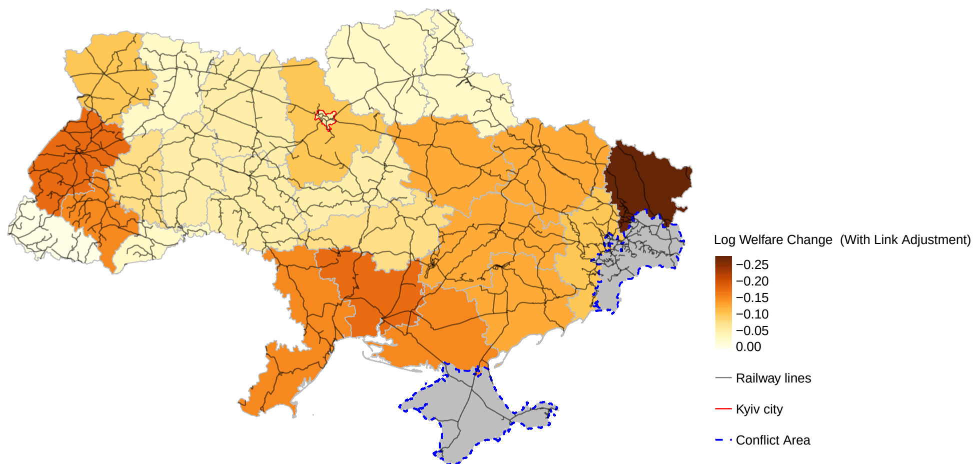
In Figure 5, we show the geographic patterns of these welfare losses. In Panel (A), we plot the simulated welfare loss of each region on a map. We find a large variation of welfare loss across regions in Ukraine, ranging from 0% to 25%. Overall, welfare loss tends to be greater in regions that are geographically closer to the conflict areas. In particular, the region with the largest welfare loss is the Luhansk province, right next to the conflict area in the east.

To further emphasize this heterogeneity, in Panel (B), we project the welfare changes as a function of the distance to the conflict areas. We find a strong upward-sloping relationship in Panel (B), confirming that regions closer to the conflict areas tended to suffer larger welfare loss.

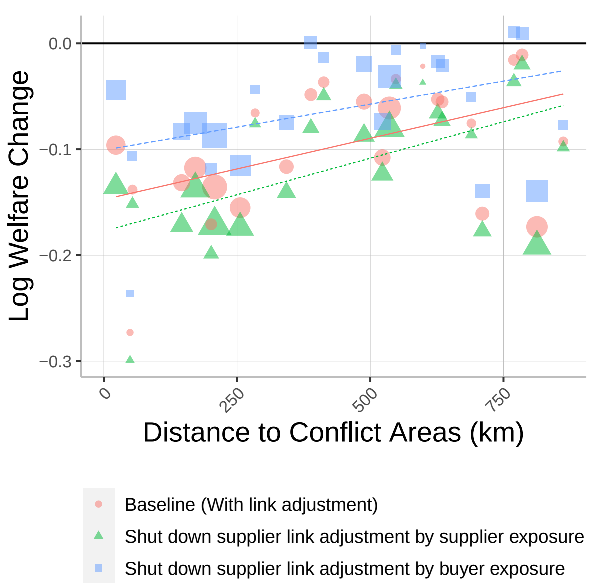
Even so, some regions far from the conflict areas, such as Lviv province (in the west) and Mykolaiv and Odessa provinces (in the southwest), face large welfare losses. These estimates indicate that localized conflicts can have far-reaching, detrimental economic consequences through production networks. One reason why far-away regions could be affected is their higher reliance on manufacturing. The manufacturing sector is more severely affected by the production network disruption due to its higher reliance on intermediate input trade (Table 3, Appendix Table A.5). Panel (C) confirms that regions with a higher sales share of manufacturing firms tend to face a larger welfare loss. Therefore, regions with high reliance on the manufacturing sector, such as Lviv, Mykolaiv, and Odessa provinces (see Figure A.1 for the industrial composition across provinces), face a large welfare loss even though they are geographically far from the conflict areas.

Robustness. In Appendix Table F.1, we report the robustness of our results to alternative specifications. First, we show that our results remain similar even if we change the measure of supplier linkages depending on suppliers' exposure. Specifically, instead of assuming a uniform change of supplier linkages conditional on the buyers' exposure, we assume that this change also differs by whether the suppliers are exposed to shocks through their own suppliers and buyers. In doing so, we calibrate the implied changes in production linkages to rationalize both the patterns of supplier *and buyer* linkages change as a function of supplier and buyer exposure, as we find in columns (3) and (4) of Table 2 in Section 3.3. See Appendix F.2 for the formal procedure. We find that this specification yields a 9.1% welfare loss (row B), similar to but slightly smaller than our baseline specification (10.2%). The slightly smaller welfare loss comes from the welfare benefit through reorganizing buyer linkages away from suppliers negatively hit by the shock. However, this effect

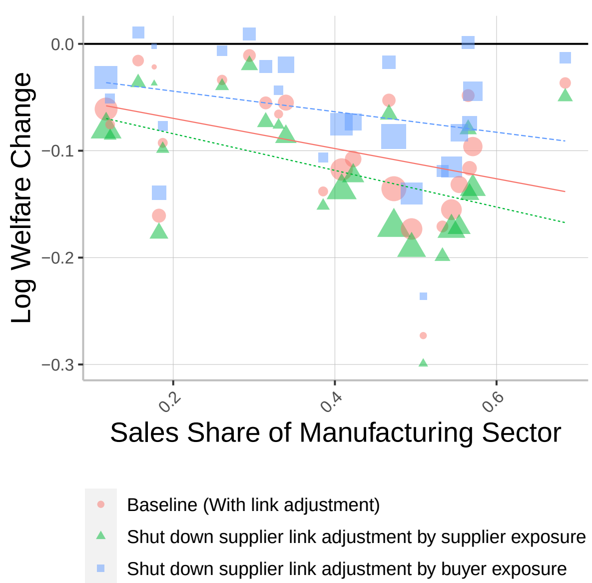
Figure 5: Welfare Changes Outside Conflict Areas



(a) Welfare Changes Across Provinces of Ukraine (with link adjustment)



(b) Province-Level Changes in Welfare by Distance to the Conflict Areas



(c) Province-Level Changes in Welfare by Share of Manufacturing Firms

Notes: These figures present the predicted percentage change in welfare (real income) for regions strictly outside the conflict areas. In Panel (B), distance to the conflict areas is defined as the straight-line distance between the centroid of each province and the closest point of the border to the conflict areas in the Donbas region or Crimea. In Panel (C), sales share of the manufacturing sector is defined using SPARK-Interfax data in 2013. The size of the dot represents the population size of each province in 2013.

is quantitatively negligible compared to the average shift of supplier linkages and the resulting love-of-variety effects in intermediate inputs.

Second, our results remain similar even if we account for the effects of firms' entry and exit in response to the shock. In Appendix C.3, we extend our model to incorporate these effects as exogenous changes in $\{N_{i,k}(\omega)\}$. There, the only additional sources of welfare changes are changes in final consumer prices through love-of-variety effects. To gauge the quantitative magnitude of these effects, we assume that $\{N_{i,k}(\omega)\}$ shifts in a way consistent with our difference-in-differences estimates in column (6) of Table 1, interpreting "no sales reported" as firm exit, and assuming that $\{N_{i,k}(\omega)\}$ does not change if firms have low supplier or buyer exposure. We find that this model predicts an 11.2% welfare loss (row C), somewhat larger than but similar to our baseline results.

In rows (D)–(F) of Appendix Table F.1, we report the results where we use alternative methods for the value imputation in our shipment data (see Section 2.2 and Appendix B for further details). In rows (G) and (H), we report the robustness where we define firm types using the exposure defined by the shares of links and shipment weights instead of using value shares. Across all these checks, we find similar patterns.

6 Conclusion

Does an intense, prolonged localized conflict lead to disruption and reorganization of production networks? What are the consequences for firm production and aggregate welfare? This paper answers these questions in the context of the 2014 Russia-Ukraine conflict. We document that firms with prior supplier linkages to the conflict areas and firms with prior buyer linkages to the conflict areas both experienced significantly decreased output. Simultaneously, firms with prior supplier linkages increased supplier linkages in nonconflict areas, those with prior buyer linkages decreased them, and both types of firms decreased buyer linkages in nonconflict areas.

Based on this evidence, we develop a model of how disruption and reorganization of production networks affect production and welfare. We show that this model *with* production-network reorganization can accurately account for the observed output changes, while the model *abstracting from* the reorganization fails to do so. Our model predicts about a 10% reduction of aggregate welfare strictly outside conflict areas through the disruption and reorganization of production networks.

The reorganization of production linkages contributes to increasing the aggregate welfare loss. Overall, our analysis shows that localized conflicts can have far-reaching, detrimental economic consequences.

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Online Appendix for "Supply Chain Disruption and Reorganization: Theory and Evidence from Ukraine's War" (not for publication)

Vasily Korovkin, Alexey Makarin, Yuhei Miyauchi

A Appendix for Reduced-Form Evidence

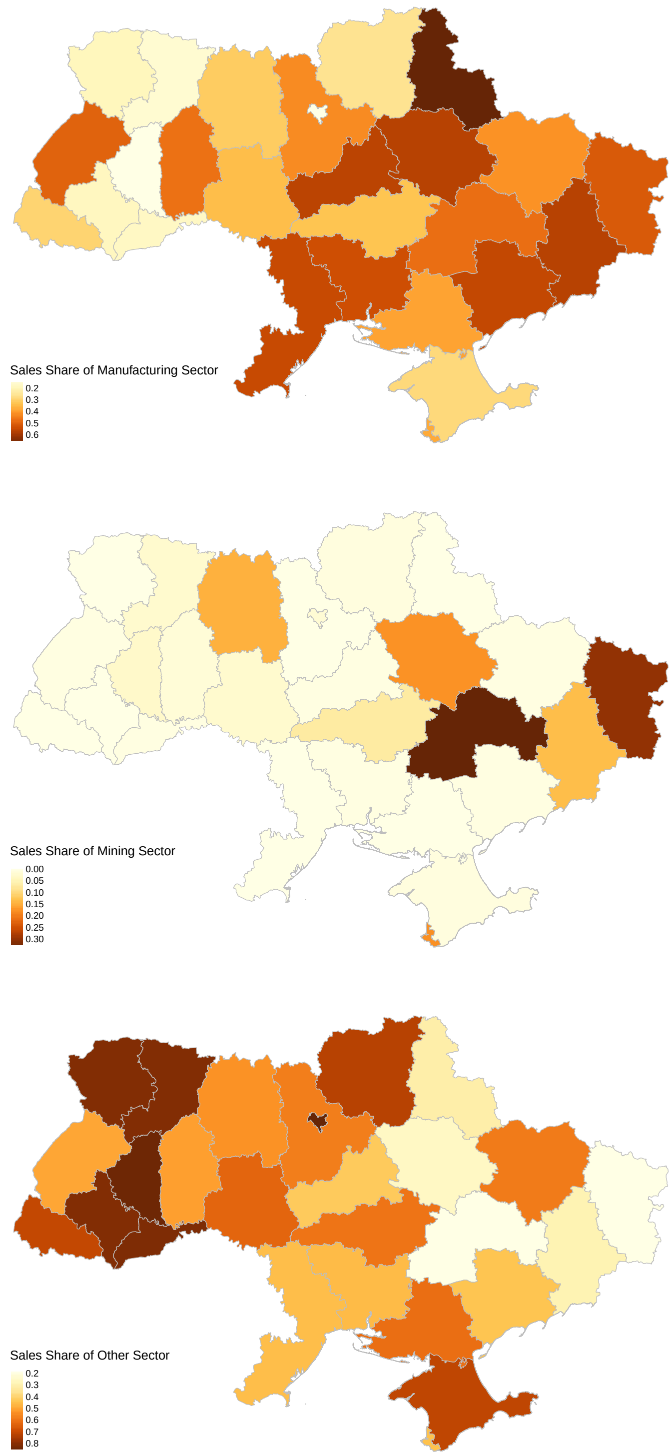
A.1 Summary Statistics

Table A.1: Summary Statistics

	Observations	Mean	SD	Min	Max
	Panel A: Conflict Exposure				
$\mathbb{1}$[Firm traded with conflict areas, 2012–13]	52,346	0.54	0.50	0	1
Firm's buyer conflict exposure, 2012–2013	52,346	0.09	0.22	0	1
Firm's supplier conflict exposure, 2012–2013	52,346	0.10	0.23	0	1
$\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]	52,346	0.19	0.39	0	1
$\mathbb{1}$[High firm's supplier conflict exposure, 2012–13]	52,346	0.19	0.39	0	1
$\mathbb{1}$[Firm traded with Russia in 2012–2013]	52,346	0.23	0.42	0	1
	Panel B: Sales and Trade				
Log of firm sales, 2010–2018	35,879	16.88	2.49	4.61	25.13
$\mathbb{1}$[No sales reported], 2010–2018	52,346	0.31	0.46	0	1
Log weight sent total, 2012–2016	13,376	15.44	3.06	1.61	24.86
Log weight sent to nonconflict areas, 2012–2016	13,034	15.40	3.04	1.61	24.72
Log weight received total, 2012–2016	20,633	15.69	2.37	3.00	24.57
Log weight received from nonconflict areas, 2012–2016	20,236	15.62	2.36	3.00	24.56
Log number of buyers total, 2012–2016	13,376	1.89	1.51	0	7.64
Log number of buyers in nonconflict areas, 2012–2016	13,034	1.85	1.49	0	7.64
Log number of suppliers total, 2012–2016	20,633	1.79	1.24	0	7.80
Log number of suppliers from nonconflict areas, 2012–2016	20,236	1.73	1.26	0	7.79
	Panel C: Industry				
$\mathbb{1}$[Firm is in mining]	52,346	0.04	0.20	0	1
$\mathbb{1}$[Firm is in manufacturing]	52,346	0.20	0.40	0	1
$\mathbb{1}$[Firm is in another industry]	52,346	0.75	0.43	0	1

Notes: This table presents the summary statistics for the firm-year trade and accounting data. The (natural) logarithms do not adjust for zero trade volume and, as such, are only defined for firm-year observations with positive trade volume. The industry indicators are based on the firms' SIC codes from SPARK & Interfax.

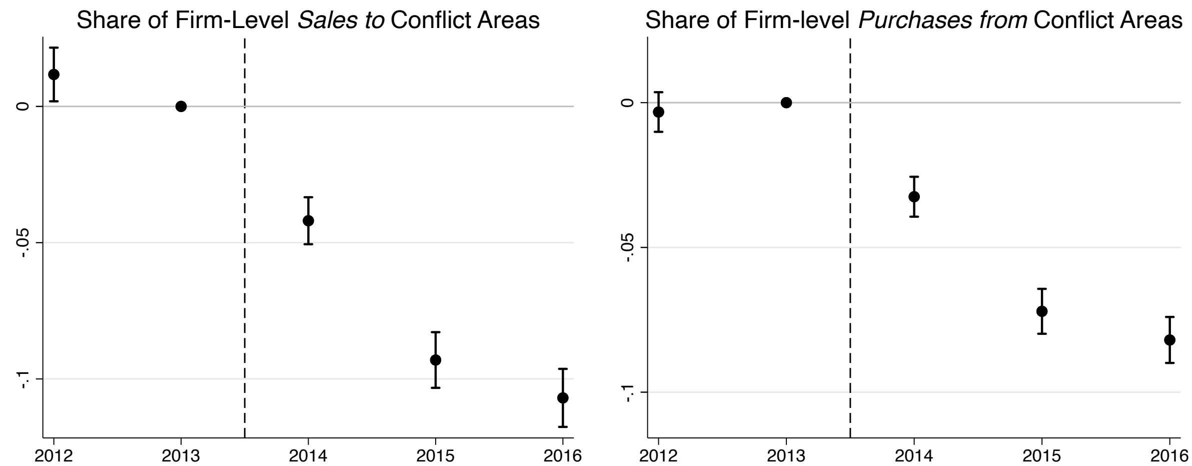
Figure A.1: Industry Composition of Regions in 2013 in Ukraine



Notes: These maps represent the share of sales for each of the three industry classifications (Manufacturing, Mining, and Other) within each province of Ukraine in 2013 using SPARK-Interfax data.

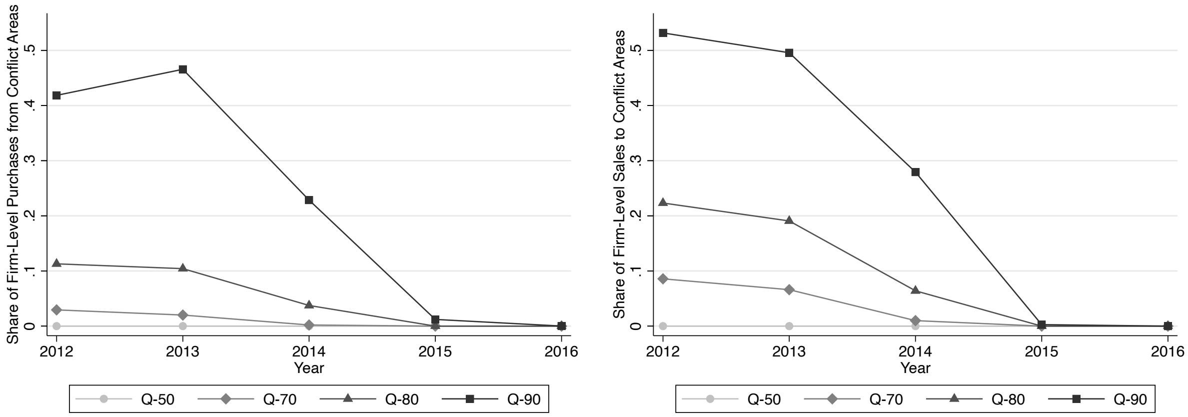
A.2 Additional Evidence for the Reduction In Trade With the Conflict Areas

Figure A.2: Evolution of Firm Trade Value Share With the Conflict Areas



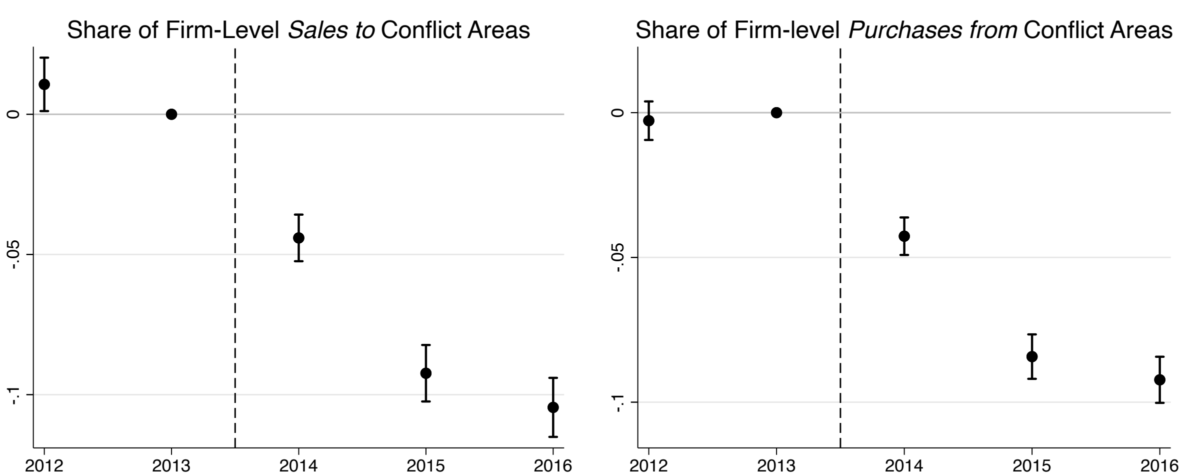
Notes: This figure represents how the firm-level buyer and supplier exposure to the conflict areas changed over time. Specifically, the figure presents the estimates of the year fixed-effects from the following specification: $Y_{it} = \alpha_i + \beta_t + \varepsilon_{it}$, where Y_{it} is the share of firm i's sales to or purchases from the conflict areas (in value) in year t and α_i and β_t are firm and year fixed effects, respectively. We take 2013 as the baseline year. Bars represent 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A.3: Distribution of Firm Trade Weight Shares With the Conflict Areas



Notes: This figure displays the evolution of the distribution of firm trade share with the DPR, the LPR, and Crimea. Q-50, Q-70, Q-80, and Q-90 refer to the median and upper percentiles of the distribution. The figure on the left (right) describes the distribution for the share of firm sales that went to (purchases that came from) conflict areas, measured as the weight of the shipments sent to (received from) the conflict areas divided by the total weight of the shipments sent out (received) by a given firm that year.

Figure A.4: Evolution of Firm Trade Weight Share With the Conflict Areas



Notes: This figure represents how the aggregate firm-level buyer and supplier exposure to the conflict areas changed over time. Specifically, the figure presents the estimates of the year fixed-effects from the following specification: $Y_{it} = \alpha_i + \beta_t + \varepsilon_{it}$, where Y_{it} is the share of firm i's sales to or purchases from the conflict areas (in weight) in year t and α_i and β_t are firm and year fixed effects, respectively. We take 2013 as the baseline year. Bars represent 95% confidence intervals. Standard errors are clustered at the firm level.

A.3 Robustness for the Effects on Sales

This appendix section probes the robustness of the estimates in Table 1.

Tables A.2 and A.3 show that the results for sales volume and the indicator of nonreported sales, respectively, are robust to a battery of checks. First, we show that our estimates remain similar when we focus on a strictly balanced sample of firms (column 2 in each table). This restriction addresses the possible changes in sample composition, which may be especially salient given that our results on nonreported sales suggest increased firm exit.

Second, the results remain unchanged after flexibly controlling for firms' geolocation (columns 3–4) and their distance to the conflict areas (columns 5–6). These checks assuage the possible concerns that conflict could induce concurrent spatially correlated common shocks, such as those related to the threat of future armed conflict expansion or migration.

Third, we control for the firm's 2-digit industry fixed effects interacted with the year indicators (column 7), absorbing any industry-specific time-varying shocks. This addresses possible issues, for instance, related to increased demand for military- or conflict-related products.

Fourth, we control for the province-year fixed effects (column 8), which absorb the impact of any province-year shocks, such as province-specific refugee inflows. In Appendix A.6, we further confirm that province-level population and refugee movements are not related to our conflict exposure measures calculated at the province level.

Fifth, we show that our results are not driven by firms' prewar trade ties with Russia (column 9), which accounts for the disruption of trade between nonconflict areas of Ukraine and Russia following the start of the conflict (Korovkin and Makarin, 2023). Figure A.5 shows that firms that traded with Russia before the conflict also saw sharp, substantial declines in their sales relative to firms that did not trade with Russia; still, the differential impact of conflict on sales by firms' connections to the conflict areas stays negative and of similar magnitude to Figure 3.

Sixth, we control for the total number of trade partners before the conflict interacted with a post-2014 indicator (column 10), thus assuaging the concern that firms with fewer trading partners are mechanically more likely to have lower conflict exposure.

Seventh, our results are not driven by outlier regions, as they survive our omitting firms near the conflict areas, i.e., in the nonoccupied parts of Donetsk and Luhansk oblasts (columns 11 and 12, respectively, in Tables A.2 and A.3), and removing firms in the capital city of Kyiv (column 13).

Table A.4 shows that the results remain similar when exposure is defined by shipment weight rather than transaction values, ensuring that our findings are not influenced by value imputation.

In terms of results' heterogeneity, Table A.5 indicates that the effects are more pronounced for firms within the manufacturing sector, consistent with the importance of input-output linkages in this industry. The same table also shows that the effects of exposure to Crimea or the DPR-LPR region are comparable when analyzed separately.

In Table A.6, we address the concern of nonrandom exposure in Borusyak and Hull (2023) by calculating a placebo firm exposure and controlling for it in our baseline specification. Specifically, we take a hundred random draws, selecting four placebo "conflict" provinces (imitating Crimea, Donetsk, Luhansk, and Sevastopol) out of all Ukrainian provinces, including those actually affected by conflict. We then compute a firm's average placebo conflict exposure across these draws based on the firm's actual trade connections with the placebo "conflict" provinces. Subsequently, we reestimate Table 1 controlling for the corresponding placebo exposure measures.[1] The results in Table A.6 show that while the estimates for missing revenue decrease in magnitude, the estimates for the reduction in sales stay similar, and both sets of estimates remain statistically significant.

Finally, one might also worry that our findings are influenced by firms that have some operations in the conflict areas, which our headquarter-based sample definition does not exclude. Table A.7 demonstrates that our results remain unchanged when we use a stricter sample definition, where we only include firms that never used a railway station located in the conflict area, neither for incoming nor for outgoing shipments.

[1]This approach is equivalent to recentering the exposure variables but allows the coefficients on actual and placebo exposure to differ in magnitude (Borusyak and Hull, 2023, p. 2166). Our results are similar with recentered exposure.

Table A.2: Robustness Checks: Conflict and Sales of Firms That Traded With the Conflict Areas

	(1) Baseline	(2) Strictly Balanced Panel	(3) Latitude & Longitude	(4)	(5) Distance to Conflict Areas	(6)	(7) 2-Digit Industry × Year FE	(8) Province × Year FE	(9) Preconflict Trade With Russia	(10) Preconflict Trade Partners	(11) Omitting Donetsk Oblast	(12) Omitting Luhansk Oblast	(13) Omitting Kyiv
Post-2014 ×	-0.158***	-0.102**	-0.134***	-0.125***	-0.136***	-0.141***	-0.115**	-0.103**	-0.122***	-0.145***	-0.129***	-0.155***	-0.123***
$\mathbb{1}$[Firm traded with conflict areas, 2012–13]	(0.046)	(0.045)	(0.046)	(0.046)	(0.046)	(0.046)	(0.047)	(0.045)	(0.046)	(0.046)	(0.046)	(0.046)	(0.047)
Post-2014 ×			0.063***	-1.164									
Latitude			(0.016)	(0.927)									
Post-2014 ×			-0.021***	-1.032***									
Longitude			(0.005)	(0.290)									
Post-2014 ×				0.005									
Latitude^2				(0.010)									
Post-2014 ×				-0.001									
Longitude^2				(0.001)									
Post-2014 ×				0.022***									
Latitude × longitude				(0.006)									
Post-2014 ×					0.527***								
Distance to conflict area					(0.098)								
Post-2014 ×						0.402***							
Distance to LPR or DPR						(0.079)							
Post-2014 ×									-0.216***				
$\mathbb{1}$[Firm imported from Russia, 2012–13]									(0.060)				
Post-2014 ×									-0.223***				
$\mathbb{1}$[Firm exported to Russia, 2012–13]									(0.061)				
Post-2014 ×										-0.000*			
# of preconflict trade partners										(0.000)			
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	16.887	17.228	16.888	16.888	16.888	16.888	16.916	16.887	16.887	16.887	16.845	16.888	16.834
SD	2.488	2.294	2.486	2.486	2.486	2.486	2.477	2.488	2.488	2.488	2.461	2.485	2.441
Observations	35,716	24,426	35,609	35,609	35,609	35,609	34,156	35,713	35,716	35,716	33,917	35,165	30,608
Number of Firms	4,816	2,714	4,793	4,793	4,793	4,793	4,606	4,815	4,816	4,816	4,571	4,741	4,040

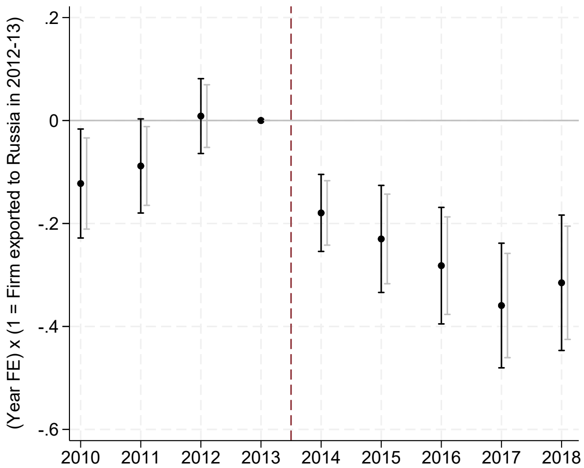
Notes: The table presents the robustness checks of the estimates for the conflict's impact on yearly sales of firms located outside the conflict areas but that traded with the conflict areas before the start of the conflict. The baseline results (column 1) are robust to focusing on a strictly balanced sample of firms (column 2), controlling for firm's latitude and longitude and their powers interacted with Post_{it} (columns 3 and 4), controlling for firm's distance (in 1,000 km) to the conflict areas (DPR, LPR, and Crimea) and distance to the DPR and the LPR interacted with Post_{it} (columns 5 and 6), controlling for firm's 2-digit industry SIC code interacted with the year fixed effects (column 7), controlling for firm's province indicators interacted with the year fixed effects (column 8), controlling for whether a firm has been exporting or importing with Russia before the conflict (2012 or 2013) interacted with Post_{it} (column 9), controlling for the total number of trade partners before the conflict (2012 or 2013) interacted with Post_{it} (column 10), omitting firms near the conflict areas, i.e., the nonoccupied parts of Donetsk and Luhansk oblasts (columns 11 and 12, respectively), and omitting firms in Kyiv (column 13). The outcome variable is the logarithm of the firm's yearly sales. Standard errors in parentheses are clustered at the firm level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.3: Robustness Checks: Conflict and Nonreporting of Sales by Firms That Traded With the Conflict Areas

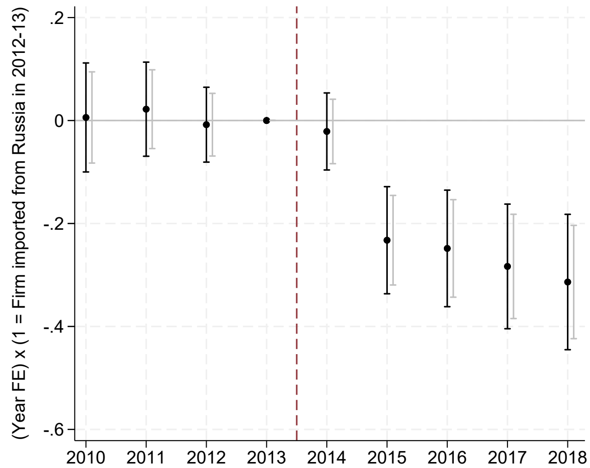
	(1) Baseline	(2) Strictly Balanced Panel	(3) Latitude & Longitude	(4)	(5) Distance to Conflict Areas	(6)	(7) 2-Digit Industry × Year FE	(8) Province × Year FE	(9) Preconflict Trade With Russia	(10) Preconflict Trade Partners	(11) Removing Donetsk Oblast	(12) Removing Luhansk Oblast	(13) Removing Kyiv
Post-2014 ×	0.068***	0.069***	0.062***	0.060***	0.062***	0.062***	0.063***	0.060***	0.063***	0.069***	0.062***	0.066***	0.053***
$\mathbb{1}$[Firm traded with conflict areas, 2012–13]	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)
Post-2014 ×			-0.003	0.154									
Latitude			(0.004)	(0.211)									
Post-2014 ×			0.006***	0.153**									
Longitude			(0.001)	(0.063)									
Post-2014 ×				-0.000									
Latitude^2				(0.002)									
Post-2014 ×				0.000									
Longitude^2				(0.000)									
Post-2014 ×				-0.003***									
Latitude × longitude				(0.001)									
Post-2014 ×					-0.106***								
Distance to conflict area					(0.021)								
Post-2014 ×						-0.093***							
Distance to LPR or DPR						(0.017)							
Post-2014 ×									0.040***				
$\mathbb{1}$[Firm imported from Russia, 2012–13]									(0.013)				
Post-2014 ×									0.022*				
$\mathbb{1}$[Firm exported from Russia, 2012–13]									(0.012)				
Post-2014 ×										-0.000			
# of preconflict trade partners										(0.000)			
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.314	0.291	0.290	0.290	0.290	0.290	0.293	0.291	0.314	0.314	0.317	0.316	0.306
SD	0.464	0.454	0.454	0.454	0.454	0.454	0.455	0.454	0.464	0.464	0.465	0.465	0.461
Observations	52,317	50,625	50,364	50,364	50,364	50,364	48,591	50,616	52,317	52,317	49,869	51,615	44,307
Number of Firms	6,098	5,625	5,596	5,596	5,596	5,596	5,399	5,624	6,098	6,098	5,826	6,020	5,208

Notes: The table presents the robustness checks for the estimates of the conflict's indirect impact on a dummy variable that takes the value one if the firm has no positive reported sales. The baseline results (column 1) are robust to focusing on a strictly balanced sample of firms (column 2), controlling for firm's latitude and longitude and their powers interacted with Post_{it} (columns 3 and 4), controlling for firm's distance (in 1,000 km) to the conflict areas (DPR, LPR, and Crimea) and distance to the DPR and the LPR interacted with Post_{it} (columns 5 and 6), controlling for firm's 2-digit industry SIC code interacted with the year fixed effects (column 7), controlling for firm's province indicators interacted with the year fixed effects (column 8), controlling for whether a firm has been trading with Russia before the conflict (2012 or 2013) interacted with Post_{it} (column 8), controlling for whether a firm has been exporting or importing with Russia before the conflict (2012 or 2013) interacted with Post_{it} (column 9), controlling for the total number of trade partners before the conflict (2012 or 2013) interacted with Post_{it} (column 10), omitting firms near the conflict areas, i.e., the nonoccupied parts of Donetsk and Luhansk oblasts (columns 11 and 12, respectively), and omitting firms in Kyiv (column 13). The outcome variable is the indicator for whether a firm did not report sales in a given year. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

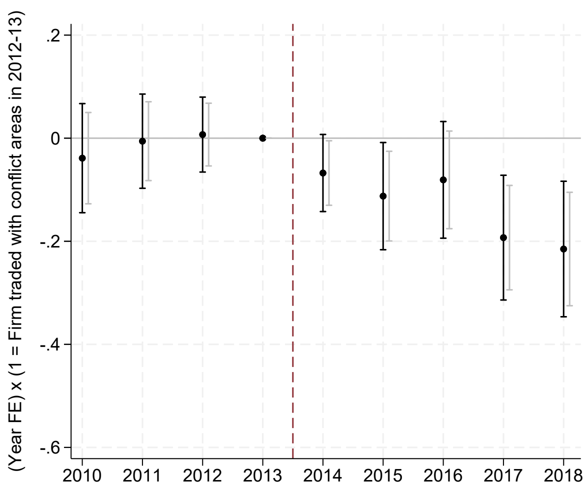
Figure A.5: The Impact of Conflict on Sales of Firms in Nonconflict Areas by Their Prior Trade With the Conflict Areas and Russia



(a) Firm Exported to Russia Preconflict



(b) Firm Imported From Russia Preconflict



(c) Firm Traded With the Conflict Areas Preconflict

Notes: This figure displays the impact of conflict on firm sales by whether a firm had prior trade ties with the conflict areas and by whether a firm exported or imported to Russia before the conflict. All coefficients are estimated within one equation. Black bars represent 95% confidence intervals, and gray bars represent 90% confidence intervals. Standard errors are clustered at the firm level.

Table A.4: Conflict and Sales of Firms That Traded With the Conflict Areas—Weight-Based Exposure

	(1) Log Sales	(2) No Sales Reported	(3) Log Sales	(4) No Sales Reported
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.155	0.051**		
	(0.097)	(0.022)		
Post-2014 × Firm's supplier conflict exposure, 2012–13	-0.326***	0.082***		
	(0.099)	(0.020)		
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]			-0.164***	0.055***
			(0.058)	(0.012)
Post-2014 × $\mathbb{1}$[High firm's supplier conflict exposure, 2012–13]			-0.203***	0.040***
			(0.056)	(0.011)
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean	16.887	0.314	16.887	0.314
SD	2.488	0.464	2.488	0.464
Observations	35,716	52,317	35,716	52,317
Number of Firms	4,816	6,098	4,816	6,098

Notes: The table presents the estimates for the conflict's impact on firm sales and an indicator for sales data missing by firms' preexisting trade connections with the conflict areas. Exposure is calculated as weight share. High exposure refers to exposure greater than the 80th percentile in the overall sample. The sample is restricted to firms outside the conflict areas (the DPR, the LPR, and Crimea). The firm accounting data, from SPARK/Interfax, cover the 2010–2018 period. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.5: Conflict and Sales of Firms That Traded With Conflict Areas, Heterogeneity By Industry and Conflict Location

	By Industry				By Conflict Location		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Manufacturing	Mining	Other Industries	Industry Indicators	Traded with DPR/LPR	Traded With Crimea	Traded With DPR/LPR and Crimea
Post-2014 × 𝟙[Firm traded with conflict areas, 2012–13]	-0.258***	-0.083	-0.120**	-0.117**			
	(0.082)	(0.264)	(0.056)	(0.050)			
Post-2014 × 𝟙[Firm traded with conflict areas, 2012–13] × 𝟙[Firm is in manufacturing]				-0.130**			
				(0.064)			
Post-2014 × 𝟙[Firm traded with conflict areas, 2012–13] × 𝟙[Firm is in mining]				-0.109			
				(0.122)			
Post-2014 × 𝟙[Firm traded with DPR/LPR, 2012–13]					-0.146***		
					(0.045)		
Post-2014 × 𝟙[Firm traded with Crimea, 2012–13]						-0.172***	
						(0.056)	
Post-2014 × 𝟙[Firm traded with DPR/LPR and Crimea, 2012–13]							-0.183***
							(0.060)
Firm FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Mean	17.507	17.295	16.640	16.887	16.887	16.887	16.887
SD	2.518	2.657	2.422	2.488	2.488	2.488	2.488
Observations	8,887	1,732	25,097	35,716	35,716	35,716	35,716
Number of Firms	1,111	224	3,481	4,816	4,816	4,816	4,816

Notes: The table presents the heterogeneous estimates for the conflict's impact on the sales of firms with preexisting trade connections with conflict areas, by industry and by conflict location. Columns (1), (2), and (3) present the baseline results restricting the sample, respectively, to manufacturing firms, mining firms, and firms of other industries. Column (4) contains the regression results with industry indicators interacted with the conflict trade exposure indicator, where the "other" industry is used as a base group. Columns (5), (6), and (7) are the baseline estimates looking at firms' prior trade ties with the occupied Donbas (the DPR or the LPR) areas, Crimea, or both. * p<0.1, ** p<0.05, *** p<0.01.

Table A.6: Conflict and Sales of Firms That Traded With the Conflict Areas—Borusyak and Hull (2023) Method

	(1) Log Sales	(2) No Sales Reported	(3) Log Sales	(4) No Sales Reported	(5) Log Sales	(6) No Sales Reported
Post-2014 × 𝟙[Firm traded with conflict areas, 2012–13]	-0.149***	0.023**				
	(0.054)	(0.011)				
Post-2014 × Firm's buyer conflict exposure, 2012–13			-0.148	0.035		
			(0.104)	(0.023)		
Post-2014 × Firm's supplier conflict exposure, 2012–13			-0.264***	0.044**		
			(0.099)	(0.021)		
Post-2014 × 𝟙[High firm's buyer conflict exposure, 2012–13]					-0.148**	0.044***
					(0.060)	(0.012)
Post-2014 × 𝟙[High firm's supplier conflict exposure, 2012–13]					-0.129**	0.032***
					(0.054)	(0.011)
Placebo exposure means	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean	16.887	0.314	16.887	0.314	16.887	0.314
SD	2.488	0.464	2.488	0.464	2.488	0.464
Observations	35,716	52,317	35,716	52,317	35,716	52,317
Number of Firms	4,816	6,098	4,816	6,098	4,816	6,098

Notes: The table presents the estimates for the conflict's impact on firm sales and an indicator for missing sales data by firms' preexisting trade connections with the conflict areas after applying the Borusyak and Hull (2023) adjustment. Specifically, we amend the estimates in Table 1 by controlling for the mean firm-level placebo conflict exposure, where placebo exposures are estimated using a sample of 100 random draws of four placebo "conflict" provinces (imitating Crimea, Donetsk, Luhansk, and Sevastopol) out of all Ukrainian provinces, including those actually affected by conflict. Columns (1)–(2) control for the placebo exposure defined as the share of simulated province draws during which a firm was connected to at least one placebo "conflict" province. Columns (3)–(4) control for a firm's average placebo conflict exposure calculated across the random draws based on the firm's actual trade connections with the placebo "conflict" provinces. Columns (5)–(6) control for the placebo exposure defined as the share of simulated province draws during which firm's placebo conflict exposure was greater than the 80th percentile in the sample. The sample is restricted to firms outside the conflict areas (the DPR, the LPR, and Crimea). The firm accounting data from SPARK/Interfax cover the 2010–2018. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.7: Conflict and Sales of Firms That Traded With Conflict Areas—Stricter Definition of Nonconflict Firms

	(1) Log Sales	(2) No Sales Reported	(3) Log Sales	(4) No Sales Reported	(5) Log Sales	(6) No Sales Reported
Post-2014 × 𝟙[Firm traded with conflict areas, 2012–13]	-0.175***	0.062***				
	(0.050)	(0.010)				
Post-2014 × Firm's buyer conflict exposure, 2012–13			-0.356***	0.049*		
			(0.124)	(0.029)		
Post-2014 × Firm's supplier conflict exposure, 2012–13			-0.234**	0.071***		
			(0.113)	(0.023)		
Post-2014 × 𝟙[High firm's buyer conflict exposure, 2012–13]					-0.237***	0.047***
					(0.069)	(0.015)
Post-2014 × 𝟙[High firm's supplier conflict exposure, 2012–13]					-0.134**	0.047***
					(0.064)	(0.013)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean	16.733	0.319	16.733	0.319	16.733	0.319
SD	2.423	0.466	2.423	0.466	2.423	0.466
Observations	28,394	41,938	28,394	41,938	28,394	41,938
Number of Firms	3,865	4,871	3,865	4,871	3,865	4,871

Notes: This is a version of Table 1 restricted to firms that have never used railway stations in conflict areas throughout the data period. High exposure refers to exposure greater than the 80th percentile in the overall sample. The sample is restricted to firms outside the conflict areas (i.e., DPR, LPR, and Crimea). The firm accounting data comes from SPARK/Interfax in 2010–2018. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

A.4 Robustness for the Effects on the Reorganization of Production Linkages

This appendix section probes the robustness of the estimates in Table 2.

Tables A.8 and A.9 show that the estimates changes in supplier and buyer linkages, respectively, are robust to the checks introduced in Tables A.2 and A.3 above. Specifically, they remain similar when using a strictly balanced sample of firms that sent or received shipments from nonconflict areas every year (column 2 in each table), flexibly controlling for time-varying importance of firms' location and distance to the conflict areas (columns 3–4 and 5–6, respectively), controlling for the industry-year (column 7) and province-year (column 8) fixed effects, controlling for firms' preconflict trade with Russia (column 9) and firms' total number of preconflict trade partners (column 10) interacted with the post-2014 indicator, and excluding firms located in the non-occupied parts of Donbas (columns 11–12) or in Kyiv (column 13). Table A.10 demonstrates robustness to controlling for firms' placebo "conflict" exposure, following recommendations in Borusyak and Hull (2023). Table A.11 confirms that our results are unlikely to be driven by firms with prewar operations in the conflict areas, as the estimates remain robust to focusing on firms that never sent or received shipments using railway stations located in the conflict areas.

Further, we explore three additional robustness checks that are especially relevant for the results on production-network reorganization. First, Table A.12 indicates that changes in the weight of shipments to and from nonconflict areas (as opposed to the number of linkages) align closely with the patterns observed in Table 2. This suggests that the changes in the number of buyers and suppliers are crucial drivers of the overall trade pattern. Second, Table A.13 shows that our results are robust if we only count trade partners present in the data before the conflict; therefore, newly registered trading partners (e.g., who might have moved from the conflict areas as new entities) do not drive our results. Third, Table A.14 shows that the estimates remain consistent at the firm-region-year level, where 'region' refers to the province of a railway station utilized by the firm.

Finally, Table A.15 presents the estimates for the total number of linkages and total weight of all shipments, including those involving conflict areas. The effects are negative across all outcomes.

Table A.8: Robustness Checks: Number of Suppliers in Nonconflict Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Baseline	Strictly Balanced Panel	Latitude & Longitude		Distance to Conflict Areas		2-Digit Industry × Year FE	Province × Year FE	Preconflict Trade With Russia	Preconflict Trade Partners	Removing Donetsk Oblast	Removing Luhansk Oblast	Removing Kyiv
Post-2014 ×	-0.086***	-0.091**	-0.088***	-0.076**	-0.088***	-0.088***	-0.076**	-0.073**	-0.081**	-0.064**	-0.063*	-0.079**	-0.109***
$\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]	(0.032)	(0.036)	(0.033)	(0.033)	(0.033)	(0.033)	(0.034)	(0.033)	(0.032)	(0.032)	(0.033)	(0.032)	(0.033)
Post-2014 ×	0.074**	0.096***	0.062*	0.069**	0.062*	0.062*	0.066**	0.071**	0.084***	0.089***	0.090***	0.073**	0.062*
$\mathbb{1}$[High firm's seller conflict exposure, 2012–13]	(0.031)	(0.036)	(0.032)	(0.032)	(0.032)	(0.032)	(0.033)	(0.032)	(0.031)	(0.031)	(0.032)	(0.032)	(0.032)
Post-2014 ×			0.008	0.886									
Latitude			(0.009)	(0.592)									
Post-2014 ×			0.002	-0.313*									
Longitude			(0.003)	(0.177)									
Post-2014 ×				-0.012**									
Latitude^2				(0.006)									
Post-2014 ×				-0.002***									
Longitude^2				(0.001)									
Post-2014 ×				0.009***									
Latitude × longitude				(0.003)									
Post-2014 ×					-0.010								
Distance to conflict area					(0.057)								
Post-2014 ×						-0.009							
Distance to LPR or DPR						(0.047)							
Post-2014 ×									-0.116***				
$\mathbb{1}$[Firm imported from Russia, 2012–13]									(0.036)				
Post-2014 ×									-0.017				
$\mathbb{1}$[Firm exported to Russia, 2012–13]									(0.040)				
Post-2014 ×										-0.000***			
# of preconflict trade partners										(0.000)			
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	1.756	2.067	1.789	1.789	1.789	1.789	1.800	1.790	1.756	1.756	1.734	1.756	1.751
SD	1.240	1.197	1.243	1.243	1.243	1.243	1.247	1.243	1.240	1.240	1.227	1.240	1.226
Observations	19,839	13,455	18,328	18,328	18,328	18,328	17,782	18,390	19,839	19,839	18,771	19,557	17,432
Number of Firms	4,693	2,691	4,266	4,266	4,266	4,266	4,138	4,282	4,693	4,693	4,451	4,629	4,105

Notes: The table presents the robustness checks for the estimates of the conflict's impact on firms' supplier linkages in nonconflict areas by firms' preexisting trade connections with the conflict areas. The outcome is the total number of distinct suppliers that engaged in trade with a given firm during a specific year using a railway station situated outside the conflict areas. High exposure refers to exposure greater than the 80th percentile in the overall sample. The baseline results (column 1) are robust to focusing on a strictly balanced sample of firms (column 2), controlling for firm's latitude and longitude and their powers interacted with Post$_{it}$ (columns 3 and 4), controlling for firm's distance (in 1,000 km) to the conflict areas (DPR, LPR, and Crimea) and distance to the DPR and the LPR interacted with Post$_{it}$ (columns 5 and 6), controlling for firm's 2-digit industry SIC code interacted with the year fixed effects (column 7), controlling for firm's province indicators interacted with the year fixed effects (column 8), controlling for whether a firm has been trading with Russia before the conflict (2012 or 2013) interacted with Post$_{it}$ (column 9), controlling for whether a firm has been exporting or importing with Russia before the conflict (2012 or 2013) interacted with Post$_{it}$ (column 10), omitting firms near the conflict areas, i.e., the nonoccupied parts of Donetsk and Luhansk oblasts (columns 11 and 12, respectively), and omitting firms in Kyiv (column 13). Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.9: Robustness Checks: Number of Buyers in Nonconflict Areas

	(1) Baseline	(2) Strictly Balanced Panel	(3) Latitude & Longitude	(4)	(5) Distance to Conflict Areas	(6)	(7) 2-Digit Industry × Year FE	(8) Province × Year FE	(9) Preconflict Trade With Russia	(10) Preconflict Trade Partners	(11) Removing Donetsk Oblast	(12) Removing Luhansk Oblast	(13) Removing Kyiv
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]	-0.170***	-0.097**	-0.170***	-0.167***	-0.169***	-0.170***	-0.123***	-0.162***	-0.171***	-0.152***	-0.146***	-0.173***	-0.162***
	(0.042)	(0.049)	(0.043)	(0.043)	(0.043)	(0.043)	(0.044)	(0.044)	(0.042)	(0.042)	(0.044)	(0.042)	(0.043)
Post-2014 × $\mathbb{1}$[High firm's seller conflict exposure, 2012–13]	-0.071	-0.041	-0.080*	-0.077	-0.080*	-0.079*	-0.043	-0.074	-0.060	-0.043	-0.057	-0.067	-0.053
	(0.045)	(0.055)	(0.047)	(0.047)	(0.047)	(0.047)	(0.047)	(0.048)	(0.046)	(0.046)	(0.048)	(0.046)	(0.048)
Post-2014 × Latitude			-0.005	0.332									
			(0.014)	(0.897)									
Post-2014 × Longitude			0.006	0.171									
			(0.005)	(0.263)									
Post-2014 × Latitude^2				-0.003									
				(0.009)									
Post-2014 × Longitude^2				-0.001									
				(0.001)									
Post-2014 × Latitude × longitude				-0.002									
				(0.005)									
Post-2014 × Distance to conflict area					-0.113								
					(0.085)								
Post-2014 × Distance to LPR or DPR						-0.094							
						(0.070)							
Post-2014 × $\mathbb{1}$[Firm imported from Russia, 2012–13]									-0.140***				
									(0.053)				
Post-2014 × $\mathbb{1}$[Firm exported to Russia, 2012–13]									0.036				
									(0.050)				
Post-2014 × # of preconflict trade partners										-0.000***			
										(0.000)			
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	1.922	2.476	1.946	1.946	1.946	1.946	1.932	1.945	1.922	1.922	1.900	1.927	1.916
SD	1.489	1.433	1.494	1.494	1.494	1.494	1.491	1.495	1.489	1.489	1.486	1.490	1.468
Observations	12,387	7,100	11,843	11,843	11,843	11,843	11,242	11,879	12,387	12,387	11,533	12,164	10,602
Number of Firms	3,198	1,420	3,021	3,021	3,021	3,021	2,880	3,031	3,198	3,198	2,993	3,138	2,733

Notes: The table presents the robustness checks for the estimates of the conflict's impact on firms' buyer linkages in nonconflict areas by firms' preexisting trade connections with the conflict areas. The outcome is the total number of distinct buyers that engaged in trade with a given firm during a specific year using a railway station situated outside the conflict areas. High exposure refers to exposure greater than the 80th percentile in the overall sample. The baseline results (column 1) are robust to focusing on a strictly balanced sample of firms (column 2), controlling for firm's latitude and longitude and their powers interacted with Post$_{it}$ (columns 3 and 4), controlling for firm's distance (in 1,000 km) to the conflict areas (DPR, LPR, and Crimea) and distance to the DPR and the LPR interacted with Post$_{it}$ (columns 5 and 6), controlling for firm's 2-digit industry SIC code interacted with the year fixed effects (column 7), controlling for firm's province indicators interacted with the year fixed effects (column 8), controlling for whether a firm has been trading with Russia before the conflict (2012 or 2013) interacted with Post$_{it}$ (column 9), controlling for whether a firm has been exporting or importing with Russia before the conflict (2012 or 2013) interacted with Post$_{it}$ (column 10), omitting firms near the conflict areas, i.e., the nonoccupied parts of Donetsk and Luhansk oblasts (columns 11 and 12, respectively), and omitting firms in Kyiv (column 13). Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.10: Conflict Exposure and Firm's Linkages With Nonconflict Areas—Borusyak and Hull (2023) Method

	(1) Log # of Suppliers in Nonconflict Areas	(2) Log # of Buyers in Nonconflict Areas	(3) Log # of Suppliers in Nonconflict Areas	(4) Log # of Buyers in Nonconflict Areas
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.038	-0.166*		
	(0.061)	(0.098)		
Post-2014 × Firm's supplier conflict exposure, 2012–13	0.293***	-0.172*		
	(0.065)	(0.096)		
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]			-0.071**	-0.167***
			(0.033)	(0.042)
Post-2014 × $\mathbb{1}$[High firm's supplier conflict exposure, 2012–13]			0.070**	-0.058
			(0.031)	(0.046)
Placebo exposure means	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean	1.756	1.922	1.756	1.922
SD	1.240	1.489	1.240	1.489
Observations	19,839	12,387	19,839	12,387
Number of Firms	4,693	3,198	4,693	3,198

Notes: The table presents the estimates for the conflict's impact on firms' outgoing and incoming trade with nonconflict areas by firms' preexisting trade connections with the conflict areas after applying the Borusyak and Hull (2023) adjustment. Specifically, we amend the estimates in Table 2 by controlling for the mean firm-level placebo conflict exposure, where placebo exposures are estimated using a sample of 100 random draws of four placebo "conflict" provinces (imitating Crimea, Donetsk, Luhansk, and Sevastopol) out of all Ukrainian provinces, including those actually affected by conflict. Columns (1)–(2) control for a firm's average placebo conflict exposure calculated across the random draws based on the firm's actual trade connections with the placebo "conflict" provinces. Columns (3)–(4) control for the placebo exposure defined as the share of simulated province draws during which firm's placebo conflict exposure was greater than the 80th percentile in the sample. The sample is restricted to firms outside the conflict areas (DPR, LPR, and Crimea) and to firms that existed in our data before the conflict. The firm accounting data from SPARK/Interfax covers 2010–2018. The outcomes are the total number of distinct suppliers and buyers that engaged in trade with a given firm during a specific year using a railway station situated outside the conflict areas. Standard errors in parentheses are clustered at the firm level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.11: Conflict Exposures and Firm's Linkages With Nonconflict Areas—Stricter Definition of Nonconflict Firms

	(1) Log # of Suppliers in Nonconflict Areas	(2) Log # of Buyers in Nonconflict Areas	(3) Log # of Suppliers in Nonconflict Areas	(4) Log # of Buyers in Nonconflict Areas
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.110	-0.158		
	(0.075)	(0.130)		
Post-2014 × Firm's supplier conflict exposure, 2012–13	0.363***	-0.063		
	(0.077)	(0.120)		
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]			-0.085**	-0.144***
			(0.042)	(0.051)
Post-2014 × $\mathbb{1}$[High firm's supplier conflict exposure, 2012–13]			0.100***	-0.005
			(0.038)	(0.059)
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean	1.615	1.730	1.615	1.730
SD	1.160	1.392	1.160	1.392
Observations	14,910	8,580	14,910	8,580
Number of Firms	3,564	2,269	3,564	2,269

Notes: This is a version of Table 2 restricted to firms that have never used railway stations in conflict areas throughout the data period. The outcomes are the total number of distinct suppliers and buyers that engaged in trade with a given firm during a specific year using a railway station situated outside the conflict areas. High exposure refers to exposure greater than the 80th percentile in the overall sample. The sample is restricted to firms outside the conflict areas (DPR, LPR, and Crimea) and to firms that existed in our data before the conflict. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.12: Conflict Exposure and Firm's Trade (Shipment Weight) With Nonconflict Areas

	(1) Log Weight Received From Nonconflict Areas	(2) Log Weight Sent to Nonconflict Areas	(3) Log Weight Received From Nonconflict Areas	(4) Log Weight Sent to Nonconflict Areas
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.244**	0.036		
	(0.111)	(0.216)		
Post-2014 × Firm's supplier conflict exposure, 2012–13	0.759***	-0.424**		
	(0.135)	(0.199)		
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]			-0.165***	-0.256***
			(0.059)	(0.081)
Post-2014 × $\mathbb{1}$[High firm's supplier conflict exposure, 2012–13]			0.179***	-0.201**
			(0.058)	(0.093)
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean	15.674	15.542	15.674	15.542
SD	2.330	2.993	2.330	2.993
Observations	19,839	12,387	19,839	12,387
Number of Firms	4,693	3,198	4,693	3,198

Notes: This is a version of Table 2 that uses shipment weight as the outcome variable instead of the number of linkages. High exposure refers to exposure greater than the 80th percentile in the overall sample. The sample is restricted to firms outside the conflict areas (DPR, LPR, and Crimea) and to firms that existed in our data before the conflict. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.13: Conflict Exposure and Firm's Linkages With Nonconflict Areas—Trading Partners Present in Data Set Before the Conflict

	(1) Log # of Suppliers in Nonconflict Areas	(2) Log # of Buyers in Nonconflict Areas	(3) Log # of Suppliers in Nonconflict Areas	(4) Log # of Buyers in Nonconflict Areas
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.073	-0.163*		
	(0.058)	(0.097)		
Post-2014 × Firm's supplier conflict exposure, 2012–13	0.277***	-0.184*		
	(0.064)	(0.096)		
Post-2014 × 𝟙[High firm's buyer conflict exposure, 2012–13]			-0.085***	-0.168***
			(0.032)	(0.041)
Post-2014 × 𝟙[High firm's supplier conflict exposure, 2012–13]			0.062**	-0.064
			(0.031)	(0.045)
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean	1.731	1.914	1.731	1.914
SD	1.229	1.484	1.229	1.484
Observations	19,739	12,334	19,739	12,334
Number of Firms	4,678	3,183	4,678	3,183

Notes: The table is a version of Table 2 that presents the estimates for the conflict's impact on firms' outgoing and incoming trade with nonconflict areas by firms' preexisting trade connections with the conflict areas for firms where both of the partners had positive trade before 2014. High exposure refers to exposure greater than the 80th percentile in the overall sample. The sample is restricted to firms outside the conflict areas (DPR, LPR, and Crimea). The firm accounting data from SPARK/Interfax covers 2010–2018. Standard errors in parentheses are clustered at the firm level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.14: Conflict Exposure and Firm-Region Linkages With Nonconflict Areas

	(1) Log # of Suppliers in Nonconflict Areas	(2) Log # of Buyers in Nonconflict Areas	(3) Log # of Suppliers in Nonconflict Areas	(4) Log # of Buyers in Nonconflict Areas
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.392***	0.048		
	(0.064)	(0.086)		
Post-2014 × Firm's supplier conflict exposure, 2012–13	0.412***	-0.271***		
	(0.049)	(0.069)		
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]			-0.180***	-0.193***
			(0.027)	(0.028)
Post-2014 × $\mathbb{1}$[High firm's supplier conflict exposure, 2012–13]			0.034*	-0.076**
			(0.020)	(0.032)
Firm-Region FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean	1.298	1.654	1.298	1.654
SD	1.127	1.332	1.127	1.332
R^2	0.81	0.81	0.81	0.81
Observations	31,195	18,611	31,195	18,611
Number of Firm-Regions	8,319	5,177	8,319	5,177

Notes: The table is a version of Table 2 that presents the estimates for the conflict's impact on firms' total outgoing and incoming trade with nonconflict areas by their preexisting connectedness with the conflict areas, where firm is defined as firm-region combination. High exposure refers to exposure greater than the 80th percentile in the overall sample. The sample is restricted to firms outside the conflict areas (DPR, LPR, and Crimea) and to firms that existed in our data before the conflict. The firm accounting data, from SPARK/Interfax, cover the 2010–2018 period. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.15: Firms' Total Trade (Linkages and Weight) With Both Conflict and Nonconflict Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log # of Suppliers Total	Log Weight Received Total	Log # of Buyers Total	Log Weight Sent Total	Log # of Suppliers Total	Log Weight Received Total	Log # of Buyers Total	Log Weight Sent Total
Post-2014 × Firm's buyer conflict exposure, 2012–13	-0.075	-0.210*	-0.183**	-0.535***				
	(0.057)	(0.113)	(0.085)	(0.187)				
Post-2014 × Firm's supplier conflict exposure, 2012–13	-0.091*	-0.314***	-0.200**	-0.364*				
	(0.055)	(0.113)	(0.095)	(0.199)				
Post-2014 × $\mathbb{1}$[High firm's buyer conflict exposure, 2012–13]					-0.088***	-0.156***	-0.285***	-0.505***
					(0.031)	(0.058)	(0.041)	(0.080)
Post-2014 × $\mathbb{1}$[High firm's supplier conflict exposure, 2012–13]					-0.129***	-0.224***	-0.080*	-0.185**
					(0.029)	(0.055)	(0.045)	(0.093)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	1.812	15.740	1.954	15.577	1.812	15.740	1.954	15.577
SD	1.251	2.345	1.507	3.015	1.251	2.345	1.507	3.015
Observations	20,264	20,264	12,760	12,760	20,264	20,264	12,760	12,760
Number of Firms	4,776	4,776	3,295	3,295	4,776	4,776	3,295	3,295

Notes: The table presents the estimates for the conflict's impact on firm total outgoing and incoming trade (in both linkages and weight) with both conflict and nonconflict areas by firms' preexisting connectedness with the conflict areas. High exposure refers to exposure greater than the 80th percentile in the overall sample. The sample is restricted to firms outside the conflict areas (the DPR, the LPR, and Crimea). The firm accounting data from SPARK/Interfax cover the 2010–2018. Standard errors in parentheses are clustered at the firm level. * p<0.1, ** p<0.05, *** p<0.01.

A.5 Effects on Firm Sales in the Conflict Areas

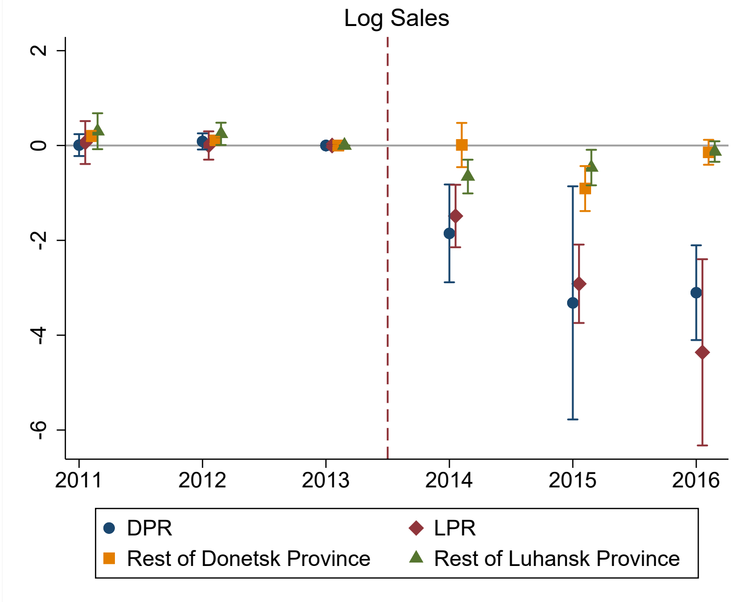
In this appendix, we show that the conflict had a profound negative effect on the economic activity of directly affected territories of the self-proclaimed Donetsk and Luhansk People's Republics (DPR and LPR). To demonstrate this quantitatively, we utilize data for the near-universe of Ukrainian firms from the ORBIS/AMADEUS database for the 2011–2016 period, aggregate it at the rayon (region) level, and estimate a fully dynamic difference-in-differences specification comparing sales of firms inside the conflict-affected areas relative to firms outside, before and after the start of the conflict. Specifically, we estimate:

$$\begin{aligned} Y_{rt} = \alpha_r + \kappa_t + &\beta_t^{LPR} \times \text{LPR}_r + \beta_t^{DPR} \times \text{DPR}_r + \\ &\beta_t^{DON} \times \text{Donetsk}_r + \beta_t^{LUH} \times \text{Luhansk}_r + \varepsilon_{rt} \end{aligned} \tag{A.1}$$

where Y_{rt} represents the aggregate firm sales in rayon r at year t, LPR_r is an indicator for whether rayon r is in the LPR; DPR_r is an indicator for whether rayon r is in the DPR; Donetsk_r is an indicator for whether rayon r is in Donetsk province; and Luhansk_r is an indicator for whether rayon r is in Luhansk province. We cluster standard errors at the rayon level. We leave out Crimea due to reporting inconsistencies in firm accounting data following the annexation.

Figure A.6 presents the results. It reveals that the aggregate sales of Ukrainian firms located in the self-proclaimed DPR and LPR—i.e., the direct conflict territories—decreased by two to four log points after the conflict began, with no pretrends preceding the conflict. While these estimates could partly be due to data-reporting issues caused by conflict, they are in line with the previous findings in Kochnev (2019), documenting a sharp 0.8–1.1 log-point decline in nighttime luminosity in the DPR and the LPR post-2014. Figure A.6 also reports a reduction in sales of firms situated in the rest of Donbas region, potentially driven by the spillover violence and possibly by the reorganization of production linkages.

Figure A.6: Impact of Conflict on Sales of Firms Located in the Conflict Areas, Rayon-Level



Notes: This figure displays the impact of conflict on the immediately affected areas in terms of their aggregate firm sales. The outcome is the sales of all Ukrainian firms in the ORBIS/AMADEUS data set outside Crimea, aggregated to the rayon level. Firms located in Crimea are removed from the sample due to inconsistencies in reporting after the annexation. Blue dot estimates are for rayons in the so-called Donetsk People's Republic, red diamonds are for rayons in the so-called Luhansk People's Republic, orange squares are for rayons in the rest of Donetsk province, and green triangles are for rayons in the rest of Luhansk province. Bars represent 95% confidence intervals. Standard errors are clustered at the rayon level.

A.6 Effects of Supplier and Buyer Conflict Exposure on Local Population Size

One may wonder whether our reduced-form estimates could be confounded by refugee movements correlated with our measures of production-network conflict exposure. As shown in Appendix Tables A.2–A.3, our results of the effects of conflict supplier and buyer exposure are robust to controlling for province-year fixed effects, alleviating such concern to the extent that refugee-flow data is only available at the province level. In this Appendix, we also investigate whether population movements during 2012–2016 within Ukraine show any differential changes in areas with greater buyer or supplier exposure.

Each province (oblast) provides annual reports on population and refugee statistics to the National Statistical Bureau.[2] From this source, we construct a panel data set for the provinces over 2012–2016. Our analysis focuses on 25 provinces that were neither occupied nor directly affected by the war. We then run the analogous difference-in-differences regression as Equation (2) at the province-year level, with the province's total population as an outcome variable.

Before proceeding with the regression analysis, we must address a well-documented issue with refugee registration in government-controlled areas. Anecdotal evidence and journalists suggest that some retirees from areas under occupation might have been falsely listed as refugees in the adjacent non-occupied regions and may have received pension payments from both sides of the conflict.[3] This can lead to errors in our population estimates for refugee groups.

We address this phenomenon in two steps. First, we calculate the ratio of retirees and disabled individuals to all other refugees in eight western Ukrainian provinces.[4] We chose these provinces as they are far from the conflict zone and, consequently, are highly unlikely to be affected by this phenomenon. Second, we use this ratio to adjust (reduce) the number of retirees among the refugee

[2] https://ukrstat.gov.ua/druk/publicat/Arhiv_u/13/Arch_nnas_zb.htm.

[3] https://voxukraine.org/velyke-pereselennya-skilky-naspravdi-v-ukraini-vpo-ua.

[4] These are the Chernivtsi, Ivano-Frankivsk, Lviv, Ternopil, Zakarpattia, Volyn, Rivne, and Khmelnytskyi oblasts.

population in the rest of the country, keeping the rest of the refugee population unchanged.[5] By doing so, we assume that younger refugees were registered correctly and that the proportion of retirees relative to the rest of the refugee population is similar across the country. We also report the estimates with unadjusted data for completeness.

Table A.16 presents our results. The outcome variable is the logarithm of the total population of a region, which combines refugee flows and general population dynamics. Columns (1)–(3) of Table A.16 focus on adjusted refugee numbers, while columns (4)–(6) report the unadjusted numbers for completeness. Columns (1) and (4) of Table A.16 report the results for weight exposure, columns (2) and (5) for value exposure, and columns (3) and (6) for link-based exposure. Given that our analysis is restricted to 25 provinces, the asymptotic standard errors may not give the right coverage, prompting us to present wild-bootstrap p-values from 999 bootstrap samples.

Our analysis does not reveal a statistically significant link between exposure levels and province population for most specifications. An exception is observed with value exposure in the unadjusted data set, yet this is only marginally significant at the 10% level.

[5] Specifically, for western regions, we calculate the ratio: $R = \sum_{r=1}^{r=8} \text{retirees\&disabled}_r / \sum_{r=1}^{r=8} \text{other\_refugees}_r$. For all other non-conflict regions, we calculate $\text{retirees\&disabled}_{r'}^{adj} = R \times \text{other\_refugees}_{r'}$ and then sum it up with $\text{other\_refugees}_{r'}$ for each region r' to get the total adjusted number of refugees.

Table A.16: Robustness Check: Effect on Region-Level Population

Dependent Variable: Log Total Population						
	(1)	(2)	(3)	(4)	(5)	(6)
	Adjusted Refugees			Unadjusted Refugees		
Exposure Type:	Weight	Value	Links	Weight	Value	Links
Post-2014 × Region's buyer conflict exposure, 2012-13	0.058	0.045	0.111	0.134	0.112*	0.203
	(0.052)	(0.036)	(0.065)	(0.079)	(0.065)	(0.127)
Post-2014 × Region's seller conflict exposure, 2012-13	0.032	0.072	0.013	0.080	0.182*	0.209
	(0.043)	(0.042)	(0.062)	(0.053)	(0.098)	(0.150)
Wild bootstrap p-value, buyer	[0.342]	[0.260]	[0.137]	[0.150]	[0.137]	[0.214]
Wild bootstrap p-value, seller	[0.624]	[0.131]	[0.854]	[0.232]	[0.099]	[0.378]
Provinces	25	25	25	25	25	25
Observations	125	125	125	125	125	125

Notes: This table tests whether refugee flows after the onset of the conflict resettled in ways correlated with the region-level buyer and supplier exposure. Regressions are run on the panel of non-occupied provinces and provinces not directly affected by violence. Columns (1)–(3) and then (4)–(6) report the coefficients for three exposure types: weight, value, and links. For columns (1)–(3), we adjust the number of refugees by population share of retirees to avoid including people eligible for pensions on two sides of the border and thus traveling outside conflict zones solely to receive pensions. A region's buyer (seller) exposures are calculated as the total weight, value, or linkages to (from) the conflict areas normalized by the total amount of weight, value, or linkages to (from) a given region. Standard errors clustered at the region level are in parentheses. Wild bootstrap p-values are reported in brackets. * p<0.1, ** p<0.05, *** p<0.01.

B Imputation of Railway Shipment Value

As discussed in Section 2.2, our railway-shipment data reports detailed product classifications (ETSNV codes) and shipment weights but not the value of each transaction. This appendix describes our procedure for imputing transaction values in our railway shipment data using separate customs data. We do so in three steps. First, we define the mapping between the product code classification in our railway-shipment data (ETSNV code) and separate customs data (HS code). Second, we estimate the value per shipment weight for each ETSNV code using the customs data. Third, we use the estimated value per shipment weight to impute transaction values from the weight of each shipment in our railway shipment data.

Step 1: Create product code correspondence between railway-shipment data (ETSNV code) and customs data (HS code). We start this merge using the crosswalks for different periods, available from the National Railways website. The links are provided below. A relatively major change in the coding correspondence occurred on October 10, 2012, that affected approximately 3% of the codes. Therefore, we merge separately before and after this date. There are 9,296 (9,360 after the major classification change) unique HS8 codes and 4,673 (4,669 after the major classification change) unique ETSNV codes.

We first establish a many-to-one match of ETSNV codes to a unique HS code. We assign a unique HS-8-digit code to the ETSNV code whenever the match is unique within our crosswalk. This first step covers 71.9% of ETSNV codes before the major classification change and 66.7% afterward. In the remaining cases, an ETSNV code corresponds to multiple HS8 codes. In this case, we find the finest aggregation of HS codes above HS8 where we can create a correspondence (e.g., HS6, HS5, or HS4). This procedure assigns 97.9% (94.8% after the major classification change) of ETSNV codes to some HS codes.

- Codebooks from 01.07.2011 to 01.07.2012: http://uz.gov.ua/files/file/cargo_transportation/smgs/G_142_izm_2011.rar

- Codebooks from 01.07.2012 to 10.10.2012: http://uz.gov.ua/files/file/cargo_transportation/smgs/G_142_03_07_2012.xls

- Codebooks from 10.10.2012 to 01.07.2013 : http://uz.gov.ua/files/file/cargo_transportation/smgs/G_142_2012.xls

- Codebooks from 01.07.2013 onward: http://uz.gov.ua/files/file/cargo_transportation/smgs/G_142_01.07.2013.xls

- Links to the website archive: https://web.archive.org/web/20121014063056/http://uz.gov.ua/cargo_transportation/legal_documents/nomenklatura/table_gnv_snd/ and https://web.archive.org/web/20130816101734/http://uz.gov.ua/cargo_transportation/legal_documents/nomenklatura/table_gnv_snd/. Links within the archives are nonclickable, but if one copies and pastes them, the download process will start.

Step 2: Construct value-per-shipment-weight for each ETSNV code using customs data. Next, we construct the value-per-shipment-weight for each ETSNV code. To do so, we compute the corresponding information in our customs data at the HS8-code level, where we observe both the shipment weight and the value for each transaction. We then use the crosswalk from Step 1 to impute the value-per-shipment-weight for each ETSNV code.

To probe the robustness, we execute this imputation in four alternate ways. First, we use either (i) all of the custom transactions (both import and export) or (ii) only the export transactions; (i) provides higher precision using a larger sample, while (ii) potentially addresses a concern that import transactions have a higher chance of being misreported than export transactions (e.g., Chalendard, Fernandes, Raballand, and Rijkers, 2023). Second, we use either (a) geometric mean or (b) simple mean to compute the product-level value-per-shipment-weight.[6]

The combinations of these approaches constitute our four alternative ways, where the combination of (i) and (a) constitutes our main specification. The values obtained by the four approaches are highly correlated; the correlation coefficients range from 0.85 to 0.98 (see Table B.1 below).

Step 3: Use the constructed value-per-shipment-weight to impute transaction value for railway shipment transaction. Finally, we return to our railway-shipment data and obtain the value

[6]Specifically, for the geometric mean, for transaction i in good category j we use $\widehat{\text{Unit Value}}_j = \exp\{(1/N_j)\sum_i \log(\text{Value}_{ij}/\text{Weight}_{ij})\}$, where N_j is the number of observations in the j-th HS code. For the simple mean, we use $\sum_i \text{Value}_{ij}/\sum_i \text{Weight}_{ij}$.

Table B.1: Product-Level Correlations Between Values Predicted Using Four Imputation Methods

	(1)	(2)	(3)	(4)
Method:	Raw Correlations:			
(1) average $\log$(Value / Weight), All	1.00			
(2) average $\log$(Value / Weight), Export	0.92	1.00		
(3) $\log(\sum$ Value / $\sum$ Weight), All	0.91	0.85	1.00	
(4) $\log(\sum$ Value / $\sum$ Weight), Export	0.90	0.98	0.86	1.00

Notes: The table reports correlation coefficients between the four measures at the ETSNV code level: export-based and based on all transactions and geometric mean vs simple mean.

for each transaction by multiplying the reported shipment weight and the estimated value-per-shipment weight for the corresponding ETSNV code.

Validity of Value Imputation. We now validate our imputation method. Since transaction value is not directly reported in our railway-shipment data, we cannot directly assess the validity of imputation in our railway-shipment data. However, we can assess the performance of our approach strictly within our customs data. Specifically, for a random 80% subsample of observations in the customs data—the "training data set,"—we run the procedure described above to construct the value-per-shipment-weight for each product category. We then use the remaining 20% of the sample—the "test data set"—to predict their transaction values and assess their accuracy.

The results are reported in Table B.2. The table presents regressions of the actual log values of the transactions on the predicted ones in the test data set without including intercepts. The four columns correspond to four alternative approaches for our prediction. Columns (1) and (3) use all transactions, and columns (2) and (4) use export transactions. Columns (1) and (2) use geometric means, and columns (3) and (4) use simple means. Panels A and B correspond to the periods before and after the major classification change took place.

Across the board, the regression coefficients in Table B.2 are close to one, suggesting a tight, one-to-one relationship between the actual and predicted transaction values. We also find relatively small root-mean-square errors in comparison with the standard deviation of the log values. These results indicate that our value imputation has strong internal validity. Given the best performance of column (1), we use this specification for our baseline analysis and use other measures for robustness.

Table B.2: Predictive Performance for Value Imputation Within Customs Data

	(1)	(2)	(3)	(4)
	All	Exports Only	All	Exports Only
	exp-log	exp-log		
Panel A: January 2012 – October 2012				
$\log(\widehat{\text{Value/Weight}})$	0.992***	0.994***	1.015***	1.014***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	672,430	671,766	672,430	671,766
St. Dev. Raw Data	1.96	1.96	1.96	1.96
RMSE Test Data	1.02	1.06	1.15	1.19
RMSE Training Data	1.02	1.05	1.15	1.19
Panel B: November 2012 – December 2013				
$\log(\widehat{\text{Value/Weight}})$	0.990***	0.971***	1.052***	0.983***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	882,584	795,052	882,584	795,052
St. Dev. Raw Data	2.06	2.06	2.06	2.06
RMSE Test Data	1.43	1.52	1.90	1.60
RMSE Training Data	1.53	1.68	1.92	1.74

Notes: The table presents regressions of the actual log values of the transactions on the predicted ones in the test data set (20% of customs data), without including intercepts. The four columns correspond to four alternative approaches for our prediction. Columns (1) and (3) use all transactions, and columns (2) and (4) use export transactions. Columns (1) and (2) use geometric means, and columns (3) and (4) use simple means to compute value-per-shipment-weight. Panels A and B correspond to the periods before and after the major classification change took place, respectively.

C Appendix for the Model

C.1 Proof of Proposition 1

From Equation (10),

$$
\begin{aligned}
R_{i,m}(\omega) &= \sum_{l\in K}\sum_{d\in\mathcal{L}}\sum_{\psi\in\Omega_{d,l}} X_{id,ml}(\omega,\psi) \\
&= \sum_{l\in K}\sum_{d\in\mathcal{L}}\sum_{\psi\in\Omega_{d,l}} \varsigma_m M_{id,ml}(\omega,\psi)\tau_{id,ml}(\omega,\psi)^{1-\sigma_m} C_{i,m}(\omega)^{1-\sigma_m} D_{d,ml}(\psi) \\
&= \varsigma_m C_{i,m}(\omega)^{1-\sigma_m}\mathcal{A}^B_{i,m}(\omega) \qquad \text{(C.1)}
\end{aligned}
$$

Furthermore, from Equations (6), (7), and (8),

$$
\begin{aligned}
C_{i,m}(\omega)^{1-\sigma_m} &= Z_{i,m}(\omega)^{\sigma_m-1} w_i^{\beta_{m,L}(1-\sigma_m)} \prod_{k\in K} P_{i,km}(\omega)^{\beta_{km}(1-\sigma_m)} \\
&= Z_{i,m}(\omega)^{\sigma_m-1} w_i^{\beta_{m,L}(1-\sigma_m)} \prod_{k\in K}\left[\left(\sum_{u\in\mathcal{L}}\sum_{\upsilon\in\Omega_{u,k}} M_{ui,km}(\upsilon,\omega) p_{ui,km}(\upsilon,\omega)^{1-\sigma_k}\right)^{\frac{1}{1-\sigma_k}}\right]^{\beta_{km}(1-\sigma_m)} \\
&= Z_{i,m}(\omega)^{\sigma_m-1} w_i^{\beta_{m,L}(1-\sigma_m)} \mathcal{A}^S_{i,m}(\omega)\prod_{k\in K}\varsigma_k^{\beta_{km}\frac{1-\sigma_m}{1-\sigma_k}} \qquad \text{(C.2)}
\end{aligned}
$$

By combining, we obtain the desired results.

C.2 Equilibrium Conditions for Counterfactual Simulation

In this appendix, we derive the system of equations for counterfactual simulation.

We first reproduce the equilibrium conditions. Given the fundamentals $\{Z_{i,m}(\omega)$, $\tau_{id,ml}(\omega,\psi)$, $N_{i,m}\}$ and production linkages $\{M_{id,ml}(\omega,\psi)\}$, the equilibrium is defined by the set of prices $\{p_{id,ml}(\omega,\psi), C_{i,m}(\omega), P_{i,km}(\omega), P_i^F, w_i\}$, trade flows $\{X_{id,ml}(\omega,\psi)\}$, firm sales $\{R_{i,m}(\omega), R^F_{i,m}(\omega)\}$, profit $\{\pi_{i,m}(\omega)\}$, and residents income $\{E_i\}$, that satisfy

$$
p_{id,ml}(\omega,\psi) = \frac{\sigma_m}{\sigma_m-1} C_{i,m}(\omega)\,\tau_{id,ml}(\omega,\psi) \qquad \text{(C.3)}
$$

$$C_{i,m}(\omega) = \frac{1}{Z_{i,m}(\omega)} w_i^{\beta_{m,L}} \prod_{k \in K} P_{i,km}(\omega)^{\beta_{km}} \tag{C.4}$$

$$P_{i,km}(\omega) = \left(\sum_{u \in \mathcal{L}} \sum_{\upsilon \in \Omega_{u,k}} M_{ui,km}(\upsilon,\omega) p_{ui,km}(\upsilon,\omega)^{1-\sigma_k} \right)^{\frac{1}{1-\sigma_k}} \tag{C.5}$$

$$X_{ui,km}(\upsilon,\omega) = \varsigma_k M_{ui,km}(\upsilon,\omega) \tau_{ui,km}(\upsilon,\omega)^{1-\sigma_k} C_{u,k}(\upsilon)^{1-\sigma_k} D_{i,km}(\omega) \tag{C.6}$$

$$D_{i,km}(\omega) = \frac{1}{P_{i,km}(\omega)^{1-\sigma_m}} \beta_{km} \frac{\sigma_m - 1}{\sigma_m} R^*_{i,m}(\omega) \tag{C.7}$$

$$R_{i,m}(\omega) = \sum_{l \in K} \sum_{d \in \mathcal{L}} \sum_{\psi \in \Omega_{d,l}} X_{id,ml}(\omega,\psi) \tag{C.8}$$

$$R^F_{i,m}(\omega) = \frac{\varsigma_m N_{i,m}(\omega) C_{i,m}(\omega)^{1-\sigma_k}}{\left(P^F_{i,m}\right)^{1-\sigma_m}} \alpha_m E_i L_i \tag{C.9}$$

$$P^F_{i,m} = \left(\varsigma_m \sum_{\omega \in \Omega_{i,m}} N_{i,m}(\omega) C_{i,m}(\omega)^{1-\sigma_m} \right)^{\frac{1}{1-\sigma_m}} \tag{C.10}$$

$$R^*_{i,m}(\omega) = R_{i,m}(\omega) + R^F_{i,m}(\omega) \tag{C.11}$$

$$w_i L_i = \sum_{m \in K} \sum_{\omega \in \Omega_{i,m}} \beta_{L,m} \frac{\sigma_m - 1}{\sigma_m} R^*_{i,m}(\omega) \tag{C.12}$$

$$E_i = w_i + \frac{1}{L_i} \sum_{m \in K} \sum_{\omega \in \Omega_{i,m}} \pi_{i,m}(\omega) \tag{C.13}$$

$$\pi_{i,m}(\omega) = \frac{1}{\sigma_m} R^*_{i,m}(\omega) \tag{C.14}$$

Now, we rewrite the equilibrium conditions given counterfactual changes in fundamentals. We denote the variable x in counterfactual equilibrium by x' (with a prime) and that as a ratio to baseline equilibrium as $\hat{x} = x'/x$ (with a hat). Given the change in TFP $\{\hat{Z}_{i,m}(\omega)\}$ and production linkages $\{\hat{M}_{id,ml}(\omega,\psi)\}$, the counterfactual equilibrium is derived as a solution to the following system of equations:

$$\hat{C}_{i,m}(\omega) = \frac{1}{\hat{Z}_{i,m}(\omega)} \hat{w}_i^{\beta_{m,L}} \prod_{k \in K} \hat{P}_{i,km}(\omega)^{\beta_{km}} \tag{C.15}$$

$$\hat{P}_{i,km}(\omega) = \left(\sum_{u \in \mathcal{L}} \sum_{\upsilon \in \Omega_{u,k}} \Lambda_{ui,km}(\upsilon, \omega) \hat{\tau}_{ui,km}(\upsilon, \omega) \hat{M}_{ui,km}(\upsilon, \omega) \hat{C}_{u,k}(\upsilon)^{1-\sigma_k} \right)^{\frac{1}{1-\sigma_k}} \tag{C.16}$$

$$\hat{X}_{ui,km}(\upsilon, \omega) = \hat{\tau}_{ui,km}(\upsilon, \omega) \hat{M}_{ui,km}(\upsilon, \omega) \hat{C}_{u,k}(\upsilon)^{1-\sigma_k} \frac{1}{\hat{P}_{i,km}(\omega)^{1-\sigma_k}} \hat{R}^*_{i,m}(\omega) \tag{C.17}$$

$$\hat{R}_{i,m}(\omega) = \sum_{l \in K} \sum_{d \in \mathcal{L}} \sum_{\psi \in \Omega_{d,l}} \Psi_{id,ml}(\omega, \psi) \hat{X}_{id,ml}(\omega, \psi) \tag{C.18}$$

$$\hat{P}^F_{i,m} = \left(\sum_{\omega \in \Omega_{i,m}} \Lambda^F_{i,m}(\omega) \, \hat{C}_{i,m}(\omega)^{\,1-\sigma_m} \right)^{\frac{1}{1-\sigma_m}} \tag{C.19}$$

$$\hat{R}^F_{i,m}(\omega) = \frac{\hat{C}_{i,m}(\omega)^{\,1-\sigma_m}}{\left(\hat{P}^F_{i,m}\right)^{1-\sigma_m}} \hat{E}_i \tag{C.20}$$

$$\hat{R}^*_{i,m}(\omega) = S_{i,m}(\omega) \hat{R}_{i,m}(\omega) + (1 - S_{i,m}(\omega)) \, \hat{R}^F_{i,m}(\omega) \tag{C.21}$$

$$\hat{w}_i = \sum_{m \in K} \sum_{\psi \in \Omega_{i,m}} \Phi^W_{i,m}(\omega) \hat{R}^*_{i,m}(\omega) \tag{C.22}$$

$$\hat{E}_i = \sum_{m \in K} \sum_{\omega \in \Omega_{i,m}} \Phi_{i,m}(\omega) \hat{R}^*_{i,m}(\omega) \tag{C.23}$$

where $\{\Lambda_{ui,km}(\upsilon, \omega), \Psi_{id,ml}(\omega, \psi), S_{i,m}(\omega), \Phi^W_{i,m}(\omega), \Phi_{i,m}(\omega), \Lambda^F_{i,m}(\omega)\}$ are shares in baseline equilibrium, defined by

$$\Lambda_{ui,km}(\upsilon, \omega) = \frac{X_{ui,km}(\upsilon, \omega)}{\sum_{\tilde{u} \in \mathcal{L}} \sum_{\tilde{\upsilon} \in \Omega_{u,k}} X_{\tilde{u}i,km}(\tilde{\upsilon}, \omega)} \tag{C.24}$$

$$\Lambda^F_{i,m}(\omega) = \frac{R^F_{i,m}(\omega)}{\sum_{\tilde{\omega} \in \Omega_{i,m}} R^F_{i,m}(\tilde{\omega})} \tag{C.25}$$

$$\Psi_{id,ml}(\omega, \psi) = \frac{X_{id,ml}(\omega, \psi)}{\sum_{\tilde{l} \in K} \sum_{\tilde{d} \in \mathcal{L}} \sum_{\tilde{\psi} \in \Omega_{d,l}} X_{i\tilde{d},m\tilde{l}}(\omega, \tilde{\psi})} \tag{C.26}$$

$$S_{i,m}(\omega) = \frac{R_{i,m}(\omega)}{R_{i,m}(\omega) + R^F_{i,m}(\omega)} \tag{C.27}$$

$$\Phi^W_{i,m}(\omega) = \frac{\beta_{L,m} \frac{\sigma_m - 1}{\sigma_m} R^*_{i,m}(\omega)}{\sum_{\tilde{m} \in K} \sum_{\tilde{\omega} \in \Omega_{i,\tilde{m}}} \beta_{L,\tilde{m}} \frac{\sigma_{\tilde{m}} - 1}{\sigma_{\tilde{m}}} R^*_{i,\tilde{m}}(\tilde{\omega})} \tag{C.28}$$

$$\Phi_{i,m}(\omega) = \frac{\left(\beta_{L,m}\frac{\sigma_m-1}{\sigma_m} + \frac{1}{\sigma_m}\right) R^*_{i,m}(\omega)}{\sum_{\tilde{m}\in K}\sum_{\tilde{\omega}\in\Omega_{i,\tilde{m}}}\left(\beta_{L,\tilde{m}}\frac{\sigma_{\tilde{m}}-1}{\sigma_{\tilde{m}}} + \frac{1}{\sigma_{\tilde{m}}}\right) R^*_{i,\tilde{m}}(\tilde{\omega})} \tag{C.29}$$

C.3 Incorporate Entry/Exit Effects

In the counterfactual simulation in Section 5.3, we abstract from the changes in the measure of active firms. Our model can be easily extended to accommodate such cases. To do so, we assume that we observe changes in the measure of firms in response to shocks, $\{\hat{N}_{i,m}(\omega)\}$, similarly to our approach for the changes in production networks $\{\hat{M}_{ui,km}(\upsilon,\omega)\}$. Then, the system of equations for the counterfactual equilibrium remains the same from Appendix C.2, except that Equations (C.19) and (C.20) are modified as follows:

$$\hat{P}^F_{i,m} = \left(\sum_{\omega\in\Omega_{i,m}} \Lambda^F_{i,m}(\omega)\,\hat{N}_{i,m}(\omega)\,\hat{C}_{i,m}(\omega)^{1-\sigma_m}\right)^{\frac{1}{1-\sigma_m}} \tag{C.30}$$

$$\hat{R}^F_{i,m}(\omega) = \frac{\hat{N}_{i,m}(\omega)\,\hat{C}_{i,m}(\omega)^{1-\sigma_m}}{\left(\hat{P}^F_{i,m}\right)^{1-\sigma_m}}\hat{E}_i \tag{C.31}$$

C.4 Multiple Shipment Modes

In our baseline model in Section 4, we abstracted from the presence of multiple shipment modes. In reality, firms may source from multiple shipment modes, not only through railways. This appendix discusses how our analysis is affected by incorporating multiple shipment modes.

Suppose that when suppliers of type $\omega\in\Omega_{i,k}$ sell to buyers of type $\upsilon\in\Omega_{j,m}$, they can choose whether to ship through railways or through roads. The iceberg shipment cost is $\tau^m_{ij,km}(\upsilon,\omega)\varepsilon^m_{ij,km}(\upsilon,\omega)$ for $m\in\{\text{Rail},\text{Road}\}$, respectively, where $\tau^m_{ij,km}(\upsilon,\omega)$ denotes the common component of mode-specific shipment cost, and $\varepsilon^m_{ij,km}(\upsilon,\omega)$ denotes the idiosyncratic components for each supplier. We follow Allen and Arkolakis (2014) and assume that $\varepsilon^m_{ij,km}(\upsilon,\omega)$ follows i.i.d. Fréchet distribution with a shape parameter κ. Then, the probability that suppliers choose to ship through railways is given by

$$\pi^{\text{Rail}}_{ij,km}(\upsilon,\omega) = \frac{\left(\tau^{\text{Rail}}_{ij,km}(\upsilon,\omega)\right)^{\kappa}}{\left(\tau^{\text{Rail}}_{ij,km}(\upsilon,\omega)\right)^{\kappa} + \left(\tau^{\text{Road}}_{ij,km}(\upsilon,\omega)\right)^{\kappa}} \tag{C.32}$$

and the probability they choose to ship through road is given by $\pi^{\text{Road}}_{ij,km}(\upsilon,\omega) = 1 - \pi^{\text{Rail}}_{ij,km}(\upsilon,\omega)$. Therefore, trade flows and the measure of supplier linkages over railway networks are given by

$$X^{\text{Rail}}_{ij,km}(\upsilon,\omega) = \pi^{\text{Rail}}_{ij,km}(\upsilon,\omega)X_{ij,km}(\upsilon,\omega), \quad M^{\text{Rail}}_{ij,km}(\upsilon,\omega) = \pi^{\text{Rail}}_{ij,km}(\upsilon,\omega)M_{ij,km}(\upsilon,\omega) \tag{C.33}$$

where $X_{ij,km}(\upsilon,\omega)$ and $M_{ij,km}(\upsilon,\omega)$ are overall trade flows and the measure of supplier linkages.

This analysis justifies our reduced-form analysis in Section 3 to use railway-shipment data as an outcome variable. It is certainly possible that the coverage of railway shipments out of the overall shipments, i.e., $\pi^{\text{Rail}}_{ij,km}(\upsilon,\omega)$, may systematically differ across firms and locations. However, under our difference-in-differences approach, all time-invariant firm-specific components of $\pi^{\text{Rail}}_{ij,km}(\upsilon,\omega)$ will drop out. Therefore, the identification concern arises only if the supplier exposure and the buyer exposure are systematically related to the changes in relative shipment costs between railways and roads. This assumption is plausible, especially when we study the reorganization of production networks *strictly outside conflict areas* (in Section 3.3), as there are no systematic disruptions in shipment costs for either railways or roads outside the conflict areas.

Next, we show that our model remains isomorphic by incorporating multiple shipment modes. To see this, note that the expected shipment cost is given by

$$\tau_{ij,km}(\upsilon,\omega) = \varrho\left(\left(\tau^{\text{Rail}}_{ij,km}(\upsilon,\omega)\right)^{\kappa} + \left(\tau^{\text{Road}}_{ij,km}(\upsilon,\omega)\right)^{\kappa}\right)^{\frac{1}{\kappa}} \tag{C.34}$$

where ϱ is a constant. Therefore, our model remains isomorphic to Section 4 by replacing $\tau_{ij,km}(\upsilon,\omega)$ with the expression given by Equation (C.34).

D Calibration Appendix

This appendix discusses the details of the model calibration. To execute the counterfactual simulation following the procedure specified in Section C.2, besides the structural parameters $\{\beta_{L,m}, \beta_{km}, \alpha_k, \sigma_k\}$, we need baseline trade flows of intermediate inputs $\{X_{ui,km}(\upsilon,\omega)\}$ and final-goods sales $\{R^F_{i,m}(\omega)\}$. However, the observed data do not necessarily satisfy all the equilibrium conditions due to measurement error and unmodeled factors. To enable a well-defined counterfactual, we adjust the trade flows so that equilibrium conditions are satisfied in the following manner.

We start by assuming that the true baseline trade flow satisfies $X_{ui,km}(\upsilon,\omega) = \check{X}_{ui,km}(\upsilon,\omega)\chi_{i,m}(\omega)$, where $\check{X}_{ui,km}(\upsilon,\omega)$ is the observed imputed transaction values in our railway-shipment data, and $\chi_{i,m}(\omega)$ captures the buyer-specific measurement errors and unmodeled factors. We obtain $\chi_{i,m}(\omega)$ so that the following equilibrium conditions are exactly satisfied.

First, by summing up Equation (C.9) across all firm types $\omega \in \Omega_{i,m}$, we have $\sum_{\omega\in\Omega_{i,m}} R^F_{i,m}(\omega) = \alpha_m E_i L_i$. Combining with Equations (C.11), (C.12), (C.13), and (C.14),

$$\begin{aligned}\tilde{E}_i &= \sum_{m\in K}\left(\beta_{L,m}\frac{\sigma_m-1}{\sigma_m}+\frac{1}{\sigma_m}\right)\left(\sum_{\omega\in\Omega_{i,m}} R_{i,m}(\omega)+\alpha_m E_i L_i\right)\\ &= \left[1-\sum_{m\in K}\left(\beta_{L,m}\frac{\sigma_m-1}{\sigma_m}+\frac{1}{\sigma_m}\right)\alpha_m\right]^{-1}\sum_{m\in K}\left(\beta_{L,m}\frac{\sigma_m-1}{\sigma_m}+\frac{1}{\sigma_m}\right)\left(\sum_{\omega\in\Omega_{i,m}} R_{i,m}(\omega)\right)\end{aligned} \tag{D.1}$$

where $\tilde{E}_i = E_i L_i$, and $R_{i,m}(\omega) = \sum_{l\in K}\sum_{d\in\mathcal{L}}\sum_{\psi\in\Omega_{d,l}} X_{id,ml}(\omega,\psi)$.

Second, given the lack of data, we simply assume that the final-goods sales are proportional to those of the intermediate-goods sales $\{R_{i,m}(\omega)\}$. That is,

$$R^F_{i,m}(\omega) = \frac{R_{i,m}(\omega)}{\sum_{\tilde{\omega}\in\Omega_{i,m}} R_{i,m}(\tilde{\omega})}\alpha_m \tilde{E}_i \tag{D.2}$$

Third, by summing up Equations (C.6) and (C.7), we have

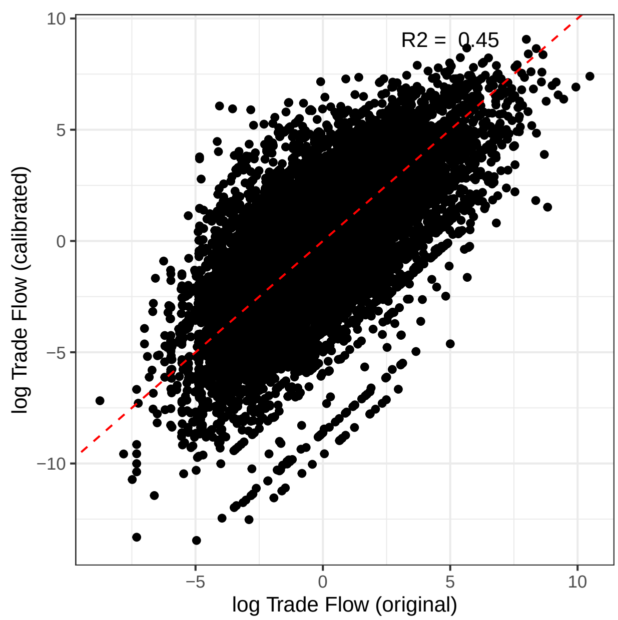
$$\sum_{u,k,\upsilon} X_{ui,km}(\upsilon,\omega) = \beta_{km}\frac{\sigma_m-1}{\sigma_m}\left(R^F_{i,m}(\omega)+R_{i,m}(\omega)\right) \tag{D.3}$$

We back out $\{\chi_{i,m}(\omega)\}$, together with variables $\{X_{ui,km}(\upsilon,\omega)\}$, $\{R_{i,m}(\omega)\}$, $\{R^F_{i,m}(\omega)\}$, and $\{\tilde{E}_i\}$, so that Equations (D.1), (D.2), and (D.3) are exactly satisfied. Specifically, starting from a guess of $\{\chi_{i,m}(\omega)\}$, we iteratively use the three equations to update $\{R_{i,m}(\omega)\}$, $\{R^F_{i,m}(\omega)\}$, and $\{\tilde{E}_i\}$ using Equations (D.1) and (D.2), and we update the value of $\{\chi_{i,m}(\omega)\}$ using Equation (D.3). We repeat this process until the procedure converges.

The trade flows $\{X_{ui,km}(\upsilon,\omega)\}$, along with $\{R_{i,m}(\omega)\}$, $\{R^F_{i,m}(\omega)\}$, exactly satisfy all the equilibrium conditions of our model, enabling us to undertake a well-defined counterfactual simulation.

Figure D.1 shows that the recalibrated and original trade flows have high correlations, with an R-squared of 0.45.

Figure D.1: Original and Calibrated Trade Flows



For our model validation in Section 5.2, we also use proxies for wages $\{w_{i,t}\}$. Using the calibrated trade flows $\{X_{ui,km,t}(\upsilon,\omega)\}$, the set of structural parameters $\{\beta_{L,m},\beta_{km},\alpha_k,\sigma_k\}$, and population size $\{L_i\}$, we look for wages $\{w_{i,t}\}$ that satisfy the set of Equations (16) for each year.

E Additional Tables for Model Validation

Table E.1: Model Validation: Estimate Gravity Equations and Access Using Aggregate Flows

	$\log w_{i,t}^{\beta_{m,L}(1-\sigma_m)} \tilde{A}^S_{i,m,t}(\omega)\tilde{A}^B_{i,m,t}(\omega)$				
	(1)	(2)	(3)	(4)	(5)
$\log R_{i,m,t}(\omega)$	1.44	1.39	1.45	1.55	1.64
	(0.21)	(0.19)	(0.19)	(0.42)	(0.29)
p-value (coefficient = 1)	0.03	0.04	0.02	0.19	0.03
Effective First-Stage F-Statistics	50.1	54.1	55.6	9.9	23.8
IV	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer Exposure	High Supplier Exposure
Firm-Type-Region-Sector Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Sector × Year Fixed Effects		X	X	X	X
Region × Year Fixed Effects			X	X	X
Observations	439	439	439	439	439
Adjusted R^2	0.99	0.99	0.99	0.99	0.99

Notes: This is a version of Panel (B) of Table 4; here we estimate Equation (22) by eliminating $M_{ui,km,t}(\upsilon,\omega)$ from the denominator of the left-hand side, and we compute the access using Equations (23) and (24) eliminating $M_{ui,km,t}(\upsilon,\omega)$.

Table E.2: Model Validation: No Buyer-Link Adjustment

	$\log w_{i,t}^{\beta_{m,L}(1-\sigma_m)} \tilde{A}^S_{i,m,t}(\omega)\tilde{A}^B_{i,m,t}(\omega)$				
	(1)	(2)	(3)	(4)	(5)
$\log R_{i,m,t}(\omega)$	0.31	0.26	0.31	0.40	0.39
	(0.12)	(0.11)	(0.10)	(0.22)	(0.14)
p-value (coefficient = 1)	0.00	0.00	0.00	0.01	0.00
Effective First-Stage F-Statistics	50.1	54.1	55.6	9.9	23.8
IV	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer Exposure	High Supplier Exposure
Firm-Type-Region-Sector Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Sector × Year Fixed Effects		X	X	X	X
Region × Year Fixed Effects			X	X	X
Observations	439	439	439	439	439
Adjusted R^2	1.00	1.00	1.00	1.00	1.00

Notes: This is a version of Panel (A) of Table 4; here we construct supplier access using observed supplier-link changes but construct buyer access abstracting from changes in buyer links.

Table E.3: Model Validation: No Supplier-Link Adjustment

	$\log w_{i,t}^{\beta_{m,L}(1-\sigma_m)} \tilde{\mathcal{A}}_{i,m,t}^{S}(\omega)\tilde{\mathcal{A}}_{i,m,t}^{B}(\omega)$				
	(1)	(2)	(3)	(4)	(5)
$\log R_{i,m,t}(\omega)$	0.92	0.91	0.96	1.07	1.00
	(0.14)	(0.12)	(0.11)	(0.19)	(0.17)
p-value (coefficient = 1)	0.58	0.46	0.70	0.71	0.99
Effective First-Stage F-Statistics	55.3	59.2	63.5	15.3	20.9
IV	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer Exposure	High Supplier Exposure
Firm-Type-Region-Sector Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Sector × Year Fixed Effects		X	X	X	X
Region × Year Fixed Effects			X	X	X
Observations	440	440	440	440	440
Adjusted R^2	0.99	1.00	1.00	0.99	1.00

Notes: This is a version of Panel (A) of Table 4; here we construct buyer access using observed buyer-link changes but construct supplier access abstracting from changes in supplier links.

Table E.4: Model Validation: Use All Years

	$\log w_{i,t}^{\beta_{m,L}(1-\sigma_m)} \tilde{\mathcal{A}}_{i,m,t}^{S}(\omega)\tilde{\mathcal{A}}_{i,m,t}^{B}(\omega)$				
	(1)	(2)	(3)	(4)	(5)
$\log R_{i,m,t}(\omega)$	1.28	1.27	1.40	1.67	1.68
	(0.19)	(0.17)	(0.19)	(0.58)	(0.34)
p-value (coefficient = 1)	0.13	0.12	0.03	0.24	0.04
Effective First-Stage F-Statistics	35.2	39	40.3	4.7	15.2
IV	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer and Supplier Exposure	High Buyer Exposure	High Supplier Exposure
Firm-Type-Region-Sector Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Sector × Year Fixed Effects		X	X	X	X
Region × Year Fixed Effects			X	X	X
Observations	1,101	1,101	1,101	1,101	1,101
Adjusted R^2	0.99	0.99	0.99	0.99	0.99

Notes: This is a version of Panel (A) of Table 4 that uses all of 2012–2016.

F Appendix for Counterfactual Simulation

F.1 Additional Robustness

Table F.1: Counterfactual Simulation: Robustness

	Welfare Change (Percentage)			
Alternative Specifications	(1) Baseline (With Supplier Link Adjustment)	(2) Shut Down Supplier Link Adjustment by Supplier Exposure	(3) Shut Down Supplier Link Adjustment by Buyer Exposure	(4) No Supplier Link Adjustment)
(a) Baseline	-10.2	-12.4	-6.6	-8.8
(b) Match Impacts on Both Supplier and Buyer Linkages	-9.1			
(c) Add Entry/Exit Effects	-11.2	-13.4	-7.6	-9.8
(d) Alternate Value Imputation (log(average Value/Weight))	-10.7	-12.9	-7.1	-9.3
(e) Alternate Value Imputation (average log(Value/Weight), Export)	-11.9	-14.0	-8.4	-10.5
(f) Alternate Value Imputation (log(average Value/Weight), Export)	-12.5	-14.6	-9.0	-11.1
(g) Define Types by Link Exposures	-10.7	-12.6	-7.0	-8.9
(h) Define Types by Weight Exposures	-10.3	-12.2	-6.6	-8.6

Notes: This table presents the results of the alternative robustness specifications of counterfactual simulations in Table 5, reporting the percentage change in population-weighted welfare (real income). Row (a) replicates our baseline results in Table 5. Row (b) changes the measure of supplier linkages depending on whether the suppliers are exposed to shocks through their own suppliers and buyers, thereby rationalizing the patterns of the changes in *buyer* linkages as well (see Appendix F.2 for details). Row (c) assumes that $\{N_{i,k}(\omega)\}$ changes in a way consistent with our difference-in-differences estimates of column (6) of Table 1, interpreting "no sales reported" as the exit of the firm, and assuming that $\{N_{i,k}(\omega)\}$ does not change if firms have low supplier and buyer exposure. Rows (d)–(f) calibrate the baseline trade flows using alternative methods for value imputation, i.e., by using simple means instead of geometric means to compute the value per weight [rows (d) and (f)] and using export data only instead of both import and export data to compute the value per shipment weight [rows (e) and (f)]; see Appendix B for details. Rows (g) and (h) define firm types using the exposure defined by the shares of links and shares of weights instead of using value shares.

F.2 Match the Effects on Both Supplier and Buyer Linkages

In this appendix, we explain the robustness exercise of our counterfactual simulation, where we assume that this change also differs by whether the suppliers are exposed to shocks through their own suppliers and buyers instead of assuming a uniform change of supplier linkages conditional on the buyers' exposure. In doing so, we calibrate the implied changes in production linkages to rationalize both the patterns of supplier *and buyer* linkages change as a function of supplier and buyer exposure, as we find in columns (3) and (4) of Table 2 in Section 3.3.

Denote $D^S_{j,m}(\omega)$, $D^B_{j,m}(\omega)$ as a dummy variable that takes the value one if a firm type $\omega \in \Omega_{j,m}$ has high supplier and buyer exposure, respectively. We assume that the number of links between suppliers and buyers increases according to the following function:

$$\Delta \log M_{ij,km}(\upsilon,\omega) = \left[\nu^{SS} D^S_{i,k}(\upsilon) + \nu^{SB} D^B_{i,k}(\upsilon) + 1\right] \left[\nu^{BS} D^S_{j,m}(\omega) + \nu^{BB} D^B_{j,m}(\omega)\right] \quad \text{(F.1)}$$

where $\{\nu^{SS}, \nu^{SB}, \nu^{BS}, \nu^{BB}\}$ are parameters. Notice that our main specification in Section 5.3 corresponds to the special case where we assume $\nu^{SS} = \nu^{SB} = 0$ and set ν^{BS} and ν^{SS} according to column (3) of Table 2.

We estimate $\{\nu^{SS}, \nu^{SB}, \nu^{BS}, \nu^{BB}\}$ through the indirect-inference approach. Specifically, we choose these parameters to rationalize the reduced-form effects of the supplier and buyer exposure on the measures of supplier and buyer linkages, targeting the reduced-form estimates reported in columns (3) and (4) of Table 2.

Given $\{\nu^{SS}, \nu^{SB}, \nu^{BS}, \nu^{BB}\}$, model-predicted change in the measure of suppliers is given by

$$\Delta \log M^S_{j,m}(\omega) = \Delta \log \sum_{i,k,\upsilon} M_{ij,km}(\upsilon,\omega) = \sum_{i,k,\upsilon} \frac{M_{ij,km}(\upsilon,\omega)}{\sum_{i,k,\upsilon} M_{ij,km}(\upsilon,\omega)} \Delta \log M_{ij,km}(\upsilon,\omega) \quad \text{(F.2)}$$

The changes in the measure of buyers are given by

$$\Delta \log M^B_{i,k}(\upsilon) = \Delta \log \sum_{j,m,\omega} M_{ij,km}(\upsilon,\omega) = \sum_{j,m,\omega} \frac{M_{ij,km}(\upsilon,\omega)}{\sum_{j,m,\omega} M_{ij,km}(\upsilon,\omega)} \Delta \log M_{ij,km}(\upsilon,\omega) \quad \text{(F.3)}$$

We then project these model-predicted changes in the measure of suppliers and buyers on the dummy of high supplier and buyer exposure:

$$\Delta \log M^S_{j,m}(\omega) = \beta^{SS} D^S_{j,m}(\omega) + \beta^{SB} D^B_{j,m}(\omega) + \epsilon^S_{j,m}(\omega) \tag{F.4}$$

$$\Delta \log M^B_{j,m}(\omega) = \beta^{BS} D^S_{j,m}(\omega) + \beta^{BB} D^B_{j,m}(\omega) + \epsilon^B_{j,m}(\omega) \tag{F.5}$$

where $\epsilon^S_{j,m}(\omega)$ and $\epsilon^B_{j,m}(\omega)$ are residuals. We choose the values of parameters $\{\nu^{SS}, \nu^{SB}, \nu^{BS}, \nu^{BB}\}$ that generate $\{\beta^{SS}, \beta^{SB}, \beta^{BS}, \beta^{BB}\}$ that minimize the squared sum of the differences between the coefficients in reduced-form regression as reported in columns (3) and (4) of Table 2 and the model counterpart. Through this procedure, we obtain the values of $\nu^{SS} = -0.20$, $\nu^{SB} = -1.17$, $\nu^{BS} = 0.55$, and $\nu^{BB} = -0.72$, which generate approximately the same regression coefficients as columns (3) and (4) of Table 2.