FINANCIAL CONSTRAINTS AND PROPAGATION OF SHOCKS IN PRODUCTION NETWORKS

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Abstract—We examine the propagation of a small unexpected supply shock through a production network and the role financial constraints play in its transmission. Using data on almost all Turkish supplier-customer links, we exploit the heterogeneous impact of an unexpected import-tax increase for identification. We find that this relatively minor shock had a nontrivial economic impact on exposed firms and propagated downstream through affected suppliers. Importantly, we show that low-liquidity firms amplified its transmission.

I. Introduction

SINCE the seminal work of Acemoglu et al. (2012) on the transmission of idiosyncratic shocks in production networks, leading to aggregate fluctuations in the economy, research has focused on investigating the economic channels through which such perturbation propagate. This article contributes to the existing literature in this area in two dimensions. First, we examine the propagation of a small unexpected shock that had a heterogeneous cost-push impact on a portion of firms in the production chain. Surprisingly, this relatively unimportant shock does not quickly dissipate in the larger production network but gets transmitted downstream. Second, and more importantly, we examine the role of financial constraints in the spread of this cost shock. We find that supplier firms with higher financing costs ex ante are not only more likely to transmit the unexpected shock downstream but also to *amplify* its magnitude. Our findings suggest a financial liquidity channel for the transmission of cost-push shocks in production networks.

The shock that we study unexpectedly increased the cost of import financing in a heterogeneous manner. On October 13, 2011, through an unanticipated decree, the Turkish government doubled the rate of the Resource Utilization Support Fund (RUSF) tax from 3% to 6%.[1] This tax applies only to import transactions backed by international trade financing, which in effect amount to credit from nondomestic sources. This regulatory shock had a heterogeneous impact across Turkish importers because the use of international trade credit, which is the subject to the increased tax, differs across firms. Since the 100% increase in the RUSF import duty was unexpected, an adjustment to other sources of international trade financing may not have been possible in the short run. For similar reasons, replacing the imported inputs with equivalents that are sourced domestically is unlikely to have taken place immediately for most firms due to search costs. Consequently, we examine the most plausible factor that could have delayed firms' reaction to the RUSF increase: whether their preshock financial cost burden played a role in the transmission or absorption of the shock.

Unlike most of the existing literature, we can examine the economy-wide transmission of a common shock using disaggregated data that cover all firms (and are not limited, say, to larger firms with publicly listed securities). This allows to examine the impact of a macrolevel shock with microlevel data, as we can observe the quasitotality of the production network in our firm-level data. Our analysis takes the form of a difference-in-differences approach and exploits the heterogenous exposure of firms to the tax prior to the rate increase. Moreover, given our focus on the role of financial constraints in shock transmission, we distinguish between liquidity-constrained and liquidity-unconstrained firms.

We proceed as follows. First, we empirically investigate the extent to which the input-cost shock affected the importers that were directly exposed to the RUSF tax prior to its increase, and how this impact differed between liquidity-constrained and liquidity-unconstrained firms. Second, we examine whether liquidity constraints mattered for firms being *indirectly* exposed to the shock through their suppliers and buyers. Third, we investigate the interplay between financial liquidity constraints of buyers and suppliers in the shock transmission.

Our analysis can be motivated with a simple partial equilibrium model that elicits the role of liquidity constraints in the shock's transmission. To do this, we extend an otherwise standard model (e.g., Halpern et al., 2015) by allowing firms to choose between paying for imports immediately or delaying payment by using international trade credit. This model (see the online appendix) presents a simple, yet useful, setting for understanding the propagation of an input cost-shock, such as the increase in the RUSF tax, in a production network. Importantly, it also allows us to illustrate how liquidity constraints affect this propagation.[2]

Our results can be summarized as follows. First, in a direct verification of our mechanism, we show that firms with a direct exposure to the RUSF tax prior to its increase changed

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[1]For more detail on RUSF, see section II.

[2]Unlike the existing literature (e.g., Chaney, 2016), our model focuses on liquidity for financing importer's working capital needs.

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their import-payment methods away from those payment-types targeted by the tax if they were financially unconstrained, but paid the increased levy if they were financially constrained. This suggests that there was a differential cost-push effect of the RUSF tax across firms.

Second, we find that firms' other adjustments to the shock (beyond the choice of the import-payment type) were also driven by their liquidity constraints. Firms that were liquidity constrained prior to the policy change were hit harder in its aftermath: they experienced an increase in their material input costs, a decline in imported inputs, and a drop in sales. In contrast, financially unconstrained firms fared better: they did not experience an increase in their input costs, or face a fall in their sales, although the share of imports in their total costs decreased and the number of their domestic suppliers increased. For these firms, the *permanent* input-cost increase, due to the doubling of the RUSF tax rate, seems to have been higher than the costs of switching to domestic suppliers. While the full adjustment of imports and sales took place in the year following the shock, the process of adding new suppliers for financially unconstrained firms continued for three years (the post-shock period considered in the analysis).

Third, we show that the shock propagated downstream through liquidity-constrained suppliers (potentially via increased prices, as their costs increased and sales fell). The magnitude of this propagation was comparable to the direct effect of the shock on exposed firms. Firms that were not liquidity constrained were able to dampen the impact of indirect exposure to the shock.

Fourth, we find no evidence of unconstrained suppliers transmitting the shock. Neither is there any evidence of the upstream shock propagation through affected buyers, which is in line with Acemoglu et al. (2016) that used sector-level data.

Our paper is closely related to three strands of existing research. First, our work contributes to the literature on the transmission of shocks through production networks, which originated with the work of Acemoglu et al. (2012) and has been extended by others. For example, Barrot and Sauvagnat (2016) show that for large American firms with shares listed on the stock market, economic shocks caused by localized natural disasters that affect the suppliers have economically important effects on their client-firms. Carvalho et al. (2020) and Boehm et al. (2019) focus on the 2011 Tohoku Earthquake in Japan and provide more evidence on the propagation of shocks through production networks. Acemoglu et al. (2016) investigate the impact of various shocks on the U.S. economy using a model of sectoral network structure, which they identify based on the industry-level U.S. input-output tables. They find sizeable network propagation effects for both demand and supply shocks. The demand shocks, such as increases in Chinese imports and changes in U.S. government spending, propagate upstream; whereas the supply shocks, such as those affecting TFP and patenting, tend to be transmitted downstream. We extend this literature by drawing attention to the importance of short-term financial constraints (in the form of low financial liquidity) for shock propagation. Importantly, we show that even a relatively small cost-push shock can propagate through a production network and have a non-negligible impact in the presence of financially constrained firms. We also confirm, with detailed data on firm-to-firm linkages, the sector-level finding of Acemoglu et al. (2016) that a supply shock propagates to downstream firms but has no discernible impact on upstream firms.[3]

Second, our paper extends the literature on the role of financial constraints in production networks (see Acemoglu et al., 2016; Alfaro, García-Santana and Moral-Benito, 2021; Altinoglu, 2018; Bigio & La'O, 2020; Boissay & Gropp, 2013; Costello, 2020; Jacobson & von Schedvin, 2015; Kalemli-Ozcan et al., 2014; and Reischer, 2019). In contrast to these papers, we are able to examine the firm-level transmission of an unexpected shock through a country's entire production network. Our findings suggest that even relatively minor cost-push shocks can have economically non-negligible effects. While the focus of our paper is not about (domestic or foreign) network formation, our results also suggest that, in the face of an input shock, the exposed firms alter their supplier network. They appear to do so by substituting foreign inputs, whose prices went up due to the unexpected regulatory change, with local alternatives from domestic suppliers.

Third, we also add to the literature that renewed interest in analyzing the effects of incidence of tariff policy (most recently Amiti, Redding et al., 2019; Fajgelbaum et al., 2019; Flaaen et al., 2020; Cavallo et al., 2021). As our shock is equivalent to an increase in tariffs to some firms causing import costs to increase, we are able to trace how such a policy reverberates throughout the production network, and through which channels. We obtain pass-through estimates interpreted through the lens of our model similar to the above-mentioned studies. Our results suggest that raising trade barriers in times when many firms are financially constrained (e.g., during recessions) could have especially negative amplification effects in the production network on firm costs and sales.

The rest of the paper is organized as follows. Section II describes the exogenous shock that we use for identification in the empirical analysis. Section III details the data and variable definitions. In section IV, we discuss a simple partial equilibrium theory framework (which is provided in the online appendix) that guides our empirical tests on the role of liquidity constraints in a cost-push shock's transmission in a

[3]Our work is also related, albeit less directly, to the growing literature on domestic production networks. On the theoretical front, there has been significant progress in explaining the formation of production networks (e.g., Oberfield, 2018; Lim, 2018; Dhyne et al., 2020; Huneeus, 2018). On the empirical front, Bernard et al. (2019) use firm-to-firm trade data similar to ours to study the sources of firm size heterogeneity in Belgium. While we do not study network formation per se, we document a substitution between foreign and domestic intermediates in the face of an unexpected cost-push shock.

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production network. Section V presents the results, and section VI concludes the paper.

II. Institutional Context

The imports-related RUSF contribution was instituted by the Turkish Council of Ministers on May 12, 1988. The management of this tax, which is considered a statutory import duty by the U.S. Department of Commerce (e.g., ICF 201304), is within the realm of the executive branch, as changes therein do not require a prior parliamentary debate or approval. Before 2011, RUSF imposed a 3% levy on the value of imports involving explicit or implicit nondomestic credit made available during an international trade transaction. In the face of a growing current account deficit, on October 13, 2011, a Council of Ministers' decree unexpectedly increased the RUSF levy on imports from 3% to 6%.

The RUSF tax is administered by the Turkish Customs and Trade Ministry, which requires that all import transactions' details be entered into an electronic database by its officers during the customs clearing process. The resulting dataset includes product and payment details for all imported goods. These are comprehensive since the Turkish Customs' Law No. 4458 imposes high penalties (at the order of three times the mandated RUSF payment, which is proportional to the value of the imported goods) if the RUSF tax is not paid as due or its avoidance is detected.

In practice, the implementation of the RUSF levy is based on the type of internationally defined financing method used in an import transaction. The RUSF statute applies the levy to imports using open account (OA), acceptance credit (AC), and deferred-payment letter of credit (DLC). In the case of OA, the payment to the exporter is typically due within 30 to 90 days after the receipt of the goods. AC is a type of letter of credit financing that involves a time draft for a delayed payment after receipt of the trade documents. DLC is another type of letter of credit financing with deferred payment, but one that does not involve a time draft. These three types of international trade financing amount to international trade credit being provided by the foreign exporter to the the Turkish importer.

In contrast, the levy does not apply to cash in advance (CIA) transactions (in which the importer prepays for the goods), transactions financed through a standard letter of credit (LC) (in which the payment is guaranteed by the importer's bank provided that the conditions stipulated in the trade contract are met), or documentary collection (which involves bank intermediation without a payment guarantee).

Finally, the RUSF levy applies only to ordinary imports. Processing imports, used to manufacture products solely destined for exports, have always been exempted from import taxes in Turkey.[4]

After the RUSF duty increase in October 2011, importing firms that relied on the international trade financing methods that exposed them to the tax (i.e., OA, AC, or DLC), could react in one of the following ways. They could avoid the tax altogether by switching to CIA payment, in which case they would have to increase their working capital (i.e., the cash that is devoted to their operations). Another way of avoiding RUSF completely is to switch to domestic suppliers of the same inputs or their alternates presuming that such substitutes existed at an acceptable quality and price. Importing firms weighed the costs of these options against paying the higher RUSF tax and preserving their preshock foreign input suppliers. The final option was to continue buying from the same exporter under the same conditions, but now pay a 100% higher RUSF tax.[5] Therefore, whether, and to what extent, the increase in the RUSF rate affected Turkish firms is shaped by their initial reliance on external financing for imports and, as our analysis will show, by their short-term financial condition (i.e., their liquidity condition). In particular, for a given level of reliance on external financing, financially unconstrained firms could have avoided or dampened the effect of the RUSF increase on their costs by switching to other payment methods that are not subject to the RUSF tax, for example, CIA.

III. Data and Variable Definitions

A. Data

To conduct our analysis, we use two Turkish administrative datasets with microlevel data that can only be accessed on the premises of the Turkish Statistical Institute (TSI) and the Turkish Ministry of Industry and Technology (MoIT), respectively.[6]

The first dataset, available at the TSI, contains detailed customs data and allows us to trace the universe of Turkish imports disaggregated by the importing firm, source country, six-digit Harmonized System (HS6) product code, trade regime (i.e., ordinary or processing), and most importantly for our purposes, trade financing type involved (i.e., CIA, LC, OA, etc.) This dataset can be merged with the business survey data, which include information on firm-level costs,

[4]The fact that processing imports are not subjected to RUSF allows us to use them in a placebo test.

[5]For all these reasons, it is sensible for our analysis to consider only changes to the domestic supplier network. Searching for new foreign suppliers would have been the dominated strategy for all importers: not only would they need to pay (potentially higher than domestic) search costs for new foreign suppliers possibly in other countries, but they would have also faced the same international trade financing cost increases either via CIA terms or paying the tax on OA transactions. In support of this hypothesis evidence in online appendix table II3 suggests that Turkish firms that were exposed to the RUSF shock did not switch to other source countries within product categories after October 2011 (we cannot conduct a similar analysis for switches across exporters in the same country, as the datasets we use do not allow us to identify them, even anonymously).

[6]Similar to the U.S. Census microdata utilization requirements, access to these confidential datasets requires a special permission involving a background check, and the results can only be exported upon approval by the TSI and MoIT staff, respectively.

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sales and employment (but does not contain any balance sheet data) and cover all firms with more than 20 employees together with a representative sample of smaller firms.

The second administrative dataset that we use is maintained by the MoIT for the purpose of calculating and collecting the value added tax (VAT). The firm-to-firm domestic trade data that are collected by the MoIT cover all domestic between-firm flows so long as the total value of transactions for a seller-buyer pair is above 5,000 Turkish Liras (TLs), or roughly $2,650 (based on the Dec 31, 2011 exchange rate) in a given year. This low threshold allows us to observe almost all domestic supplier-buyer pairs in Turkey. Between 2010 and 2014, we are able to trace, on average, roughly 600,000 firms, approximately 6,000,000 buyer-seller connections, with close to 20,000,000 transactions per year. We also match these firm-pair transaction data with corporate financial statements (income statement and balance sheet) as well as the customs data on imports (with one important caveat, see the next paragraph). The financial statement data available allow us to calculate outcome variables (such as sales growth), as well as the control variables (such as firm size or financial ratios). Finally, the MoIT dataset also reports the four-digit NACE industry and province-level location of the firm, which allow us to include fixed-effects to control for sector- and locality-level unobservables that might otherwise confound our estimates.

Each of the two administrative datasets, described above, reports some information essential for our purposes but misses some other information. While the MoIT dataset allows us to track the buyer and supplier network of firms operating in Turkey as well as firms' imports data (at the six-digit HS level), it does *not* include information on the types of trade financing used for their import transactions. Since the RUSF is charged based on the type of trade financing, the absence of this information in the MoIT dataset prevents us from constructing an actual firm-level measure of exposure to the policy change. In contrast, the TSI dataset allows us to observe Turkish importers' use of RUSF-affected financing terms, but it does *not* include any information on buyer and supplier links. Moreover, confidentiality restrictions prevent us from transferring firm-level import financing information from the TSI dataset and matching it with the MoIT dataset. That said, we are permitted to transfer aggregated data from one dataset into another so long as such data imports do not allow us to identify individual firms.

Given these restrictions, we use the TSI data to create an (aggregated) variety-year-level measure of exposure to the RUSF tax based on the import financing mode (as the latter can only be observed in the TSI dataset). Throughout the paper, a variety is defined as a HS6 product and import source country combination. This HS6-product-country-year-level variable, which is constructed based on ordinary imports (i.e., it excludes processing imports), is defined as follows:

$$Exposure_{vt} = \frac{\sum_{m\in\{OA,AC,DLC\}} M_{vmt}}{\sum_m M_{vmt}},$$

where v indexes input variety (i.e., HS6-product-country pairs), m trade financing types (including *OA*, *AC*, and *DLC* targeted by RUSF), M denotes imports, and t is the time (i.e., year) index. The numerator represents the total value of imports subject to the RUSF tax for a given product variety in a given year, and the denominator represents the total value of imports of the variety in the same year. We eventually merge this variety-level *Exposure* measure with the MoIT dataset to create a Bartik-type exposure variable for each firm given the varieties it imports (see section IIIB).

The variety-level *Exposure* measure covers about 150 source countries, roughly 4,700 HS6 product codes, and corresponds to approximately 75,000 country-product pairs.[7] Online appendix figure II.1 presents the frequency distribution of $Exposure_{vt}$ for $t = 2010$, $t = 2011$ (which we consider to be the preshock years, as the tax increase took place in mid-October 2011) and $t = 2012$. The right panel plots the distribution for the full sample, while in the left panel we exclude observations with zero exposure in order not to overwhelm the rest of the frequency distribution graph. As illustrated in the left panel, the distribution remained almost unchanged between 2010 and 2011 but it shifted to the left in 2012 after the increase in the RUSF rate.[8] In the full sample, the average value of the share of imports with a foreign source of financing was 20% and 19% in 2010 and 2011, respectively, but decreased to roughly 13% after the shock. When observations with zero exposure are excluded, the corresponding mean values are 39%, 37%, and 30% (for this restricted sample, the median value of $Exposure_{vt}$ also shifts significantly after the shock: from 21% in 2011 to 12% in 2012).

In the empirical analysis, we measure *Exposure* as of 2010.[9] An important feature of $Exposure_{v,t=2010}$ is that it pools information from a large and heterogeneous set of importers. As presented in table II9 in the online appendix, import values are not concentrated among a few firms within a typical variety: the average number of importers per variety is 24, and the average share of the largest importer is 58%.[10] While the distribution of the number of importers per variety is skewed, the median number of importers per variety is still

[7] As illustrated in online appendix table II8, there is a lot of variation in *Exposure* across products within source countries, as well as across source countries within the imported product category.

[8] A Kolmogorov-Smirnov test rejects the equality of the two distributions as of 2012.

[9] In the preshock period, reliance on external financing at the variety level is quite stable over time. Figure II3 in the online appendix plots $Exposure_{v,t=2010}$ against the variety-level average over the 2006–2011 period, which is obtained by regressing $Exposure_{vt}$ on country-year, product-year, and variety-level fixed effects. The correlation between the two measures is high, and the linear slope is almost one (0.98, with a standard error of 0.05). As importers frequently switch products and source countries (e.g., Manova & Zhang, 2009), that is, the set of importers for a given variety changes over time, this correlation is likely to be driven by time-invariant characteristics of varieties.

[10] To make sure that these numbers are not unduly influenced by a few outliers, we trim the number of importers per variety at the 99th percentile of its distribution.

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large (close to 10). These statistics are almost unchanged for varieties with $Exposure_{v,t=2010} > 0$.

As our identification is driven by the increase in a border tax that applies to imported goods with foreign trade financing, we focus on manufacturing firms as the unit of observation in our estimations. However, we take wholesale firms into account when we construct the exposure variables through suppliers and buyers.[11] Moreover, we drop "microentities" (very small firms) that do not report balance sheets or income statements.[12] These restrictions leave us with a sample of about 60,000 manufacturing firms, for which we observe domestic trade links, detailed income statement and balance sheet information, and customs records. This sample constitutes our core MoIT dataset, to which we add *variety-level Exposure* constructed from the TSI dataset. In the next subsection, we describe how we construct the variables that we use in our empirical analysis.

B. Variables of Interest and Descriptive Statistics

Exposure to the tax increase. Firm-level exposure to the RUSF tax, which is the key variable in our analysis, is constructed as a Bartik-type variable for $t = 2010$ as follows:

$$Exposure_{f,t=2010} = \sum_{v} \omega_{fv,t=2010} \times Exposure_{v,t=2010}, \quad (1)$$

where $\omega_{fv,t=2010}$ denotes the share of imports of variety v in firm f's total variable costs (defined as the sum of labor costs, purchases from other domestic firms and imports) at time $t = 2010$. We create the exposure variable based on the 2010 trade figures in order to avoid the possibility that the exposure measure is affected by the developments during the last two and a half months of 2011 (when the policy change was already in effect). One can think of $Exposure_{f,t=2010}$ as a prediction of the actual firm-level exposure to the RUSF tax based on the firm's preshock import composition and the typical tax exposure of a given imported variety.

To investigate the effects of the RUSF tax increase, we construct $\Delta \ln \tau_f$, a variable that captures the effective tax increase at the firm level:

$$\Delta \ln \tau_f = Exposure_{f,t=2010} \times \ln\left(\frac{1+\tau_{2012}}{1+\tau_{2011}}\right), \quad (2)$$

where τ_{2011} takes on the preincrease rate of the RUSF tax of 3%, while the τ_{2012} is the postincrease rate of 6%.

In appendix A, we compare our Bartik-type exposure variable to the *actual* firm-level exposure to the RUSF shock, which we can construct using the TSI dataset where we observe both imports as well as the associated type of payment. We show that our Bartik-type exposure variable is highly correlated with the the *actual* firm-level exposure, and hence highly informative about the actual firm-level exposure to the RUSF shock.

Suppliers' and buyers' exposure. We are also interested in measuring firms' indirect exposure via their domestic suppliers and domestic buyers. To capture the former, we define

$$Exposure^{Suppliers}_{f,t=2010} = \sum_{s} \omega^{S}_{f,s,t=2010} \times Exposure_{s,t=2010}, \quad (3)$$

where $Exposure^{Suppliers}_{f,t=2010}$ is the firm f's exposure to the shock through its suppliers. $Exposure_{s,t=2010}$ measures the direct exposure of supplier s to the shock, and $\omega^{S}_{f,s,t=2010}$ is the share of supplier s in firm f's total variable costs (defined as the sum of labor costs, purchases from other domestic firms and imports) in year 2010. Similarly, we also construct firm f's exposure to RUSF levy increase through its domestic buyers, indexed by b:

$$Exposure^{Buyers}_{f,t=2010} = \sum_{b} \omega^{B}_{f,b,t=2010} \times Exposure_{b,t=2010}, \quad (4)$$

where $Exposure_{b,t=2010}$ measures the direct exposure of buyer b to the shock, and $\omega^{B}_{f,b,t=2010}$ is the share of buyer b in firm f's total sales in year 2010. As we did for the direct exposure to the RUSF shock, we construct the following variables that capture the effective tax increase at the firm-level through the firm's suppliers and buyers:

$$\Delta \ln \tau^{Suppliers}_{f} = Exposure^{Suppliers}_{f,t=2010} \times \ln\left(\frac{1+\tau_{2012}}{1+\tau_{2011}}\right), \quad (5)$$

$$\Delta \ln \tau^{Buyers}_{f} = Exposure^{Buyers}_{f,t=2010} \times \ln\left(\frac{1+\tau_{2012}}{1+\tau_{2011}}\right). \quad (6)$$

Financial friction measures. To investigate how financial frictions affect the propagation of the RUSF shock, we also need a measure of a firm's financial position. In our empirical setting, we define financially constrained firms relative to the mean cost of financing for their industry (defined as one of the 22 two-digit-level NACE sectors). We measure the ease of access to financing with total financing costs divided by sales revenue, $FC_{f,t=2010}$. This variable captures the interest burden borne by the firm in the year prior to the tax increase. A high value of interest costs-to-sales revenue ratio indicates a higher cost of financing faced by such firms. Based on the value of $FC_{f,t=2010}$, we split firms into two groups: financially constrained firms (for which the indicator variable $Constrained_{f,t=2010}$ is equal to 1) have an interest-to-sales ratio that is above their industry average; and unconstrained firms (for which the indicator variable $Unconstrained_{f,t=2010}$ is equal to 1) have an interest-to-sales ratio below the industry mean. The underlying assumption is that, if firm f is among those with a high interest burden in its industry, the additional external financing it seeks in

[11]Firms in other service industries, such as professional services, typically do not rely on imported inputs.

[12]Such firms keep records using a single-entry bookkeeping system.

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the face of the unexpected RUSF shock will have the same average, or possibly even higher, interest rate.[13]

Summary statistics. The summary statistics for the various measures of RUSF exposure and financial liquidity introduced above are presented in online appendix table II1. As expected, most firms do not import and hence have no direct exposure to the RUSF tax.[14] The median direct exposure in both MoIT and TSI samples is zero. For importers, the average direct exposure amounts to 1.9% of the total variable costs in the MoIT dataset, and 2.5% in the TSI dataset. This low level exposure is primarily due to low import intensity, as the average value of exposure of imports to the tax ($Exposure^M$) is 17% among importing firms. The mean of direct exposure among importers is slightly larger in the TSI dataset, since it excludes small firms (with fewer than 20 employees) that typically have a lower import intensity.

In the TSI dataset, which allows us to construct both the actual and the Bartik-type firm-level exposure, the mean value of the two variables is identical both for importers and all firms. However, as illustrated in the table, their distributions are not identical.

Online appendix table II1 shows that almost all firms are indirectly exposed to the tax via their suppliers or customers. The median value of exposure via suppliers and customers equals 0.3% and 0.1%, respectively, in the full sample and 0.5% and 0.2% in the importer sample.

[13]This measure has been used extensively in the literature (see, for instance, Nickell & Nicolitsas, 1999). Ideally, we would have liked to have firm-bank-level loan data, which would have given us the *marginal* cost of new short-term credit for the firm. Unfortunately, such data are not available to us, so we have to rely on the proxy described above. We reduce the measurement error associated with this indirect measure by classifying firms into high- and low-financing-cost groups in their industries. In an earlier version of the paper, we constructed the ratio of total financing costs to the value of existing debt stock—a proxy for *imputed average* cost of debt financing (that we calculate using the firm's year-end $t = 2010$ financial statements). The correlation between that measure and the interest-to-sales ratio is above 90%. We rely on the ratio of interest payments to sales as a measure of firm-level cost of borrowing because we are able to construct it using both the MoIT and TSI datasets in a consistent way. The lack of information on firms' assets and liabilities in the latter dataset prevents us from constructing the ratio of total financing costs to the value of existing debt stock in the TSI dataset.

[14]For a given set of import varieties, the direct exposure increases with the firm's import intensity. To see this, consider an alternative representation of our Bartik-type exposure variable, which has two components: the predicted share of firm's imports subject to the RUSF tax ($Exposure^M$) and firm's import intensity (i.e., the share of imported inputs in firm's variable costs):

$$\begin{aligned} Exposure_{f,t=2010} &= \sum_v \omega_{f,v,t=2010} \times Exposure_{v,t=2010} \\ &= \underbrace{\frac{M_{f,t=2010}}{TotalCosts_{f,t=2010}}}_{\text{Import intensity}} \\ &\quad \times \overbrace{\underbrace{\sum_v \frac{M_{v,f,t=2010}}{M_{f,t=2010}} \times Exposure_{v,t=2010}}_{\text{Share of firm's imports subject to RUSF}}}^{Exposure^M_{f,t=2010}} \end{aligned}$$

Finally, the fraction of liquidity unconstrained firms is slightly lower than a half in both the MoIT and TSI datasets. Moreover, importers tend to be larger. As a result, we will control for firm size and import intensity in all of our empirical specifications.

Firm-level determinants of exposure. To investigate the determinants of firm-level exposure to the RUSF shock, we regress $Exposure_{f,t=2010}$ on the initial firm size (as measured by the number of employees), import intensity (share of imported inputs in total costs), and the indicator variable for liquidity unconstrained firms, all as of 2010. Table II2 in the online appendix presents the results for the full sample and for importers only. In both samples, conditional on industry and region fixed effects, import intensity appears as a statistically significant determinant of firm-level exposure to the RUSF shock. This is not surprising since, for a given set of imported varieties, firms with higher import intensity have a higher value of exposure. However, neither firm size nor liquidity position appears to matter for $Exposure_{f,t=2010}$.

Validity of the shift-share design. In appendix B, we further test the validity of our shift-share design based on the "shocks" view of identification in shift-share instrumental variable regressions as proposed by Borusyak et al. (2021).

Unanticipated nature of the RUSF shock. Finally, it is crucial for our identification strategy that the increase in the RUSF rate on October 13, 2011 was unanticipated. We provide two pieces of evidence to support this. First, the Google Trends statistics for the number of searches involving "Kaynak Kullanımını Destekleme Fonu" (which is the Turkish name of the tax) or "KKDF" (its acronym) presented in online appendix figure II.2, do not show any pattern suggesting that the RUSF increase was anticipated before the week of October 9, 2011, during which the number of searches increased suddenly. Second, in sections VA and VC, we provide formal statistical tests, which cannot detect pre-existing trends in outcome variables.

IV. Conceptual Framework

To fix ideas more formally, this section presents predictions from a simple theoretical framework, in which we introduce the import-payment-type decision and liquidity constraints into a partial-equilibrium static model of a small open production economy with firm networks.[15] In section V, we take the model's predictions to the data.

A. Model Setup

Firms and technology. Consider a fixed number of firms which combine labor, capital, and composite intermediate

[15]The full model and derivations are presented in online appendix I.

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inputs to produce a single distinct variety with a constant returns to scale technology. Each composite input is represented as a constant elasticity of substitution (CES) aggregate of domestic and imported material inputs. Firm's cost minimization (taking the input prices as given) leads to a constant marginal cost of production. Firms are perfectly competitive, and the price they charge is equal to their marginal cost. We consider a standard production network, with each firm selling some of its production as intermediate inputs and the rest as final goods.

Payment choice. When firms import, they choose between paying immediately and delaying payment (i.e., using international trade financing subjected to RUSF). By paying immediately, firm f incurs a firm-specific financing cost (say by borrowing from a domestic bank to obtain the necessary working capital), which increases the input price by $r_f > 1$, but saves on the import tax $\tau_0 > 1$.

We assume that firms already agreed on the optimal types of payment terms for each imported intermediate through bargaining with their international suppliers *before the shock*. This gives rise to an exogenous firm distribution of exposure to the RUSF shock *at the time of the policy change*.

B. Model Predictions

The simple model summarized above has clear predictions as to how firms respond to the RUSF increase.

RUSF increase and payment choice. The increase in the RUSF rate from τ_0 to τ_1 leaves the exposed firms with a choice: for the next batch of inputs to be imported, they can either switch to an immediate payment (which entails a cost of funds of r_f) or pay the increased tax (τ_1) on their international trade financing. Given that firms are heterogeneous in the cost of liquidity they are facing, companies with low financing costs switch to cash-in-advance financing and avoid paying the higher RUSF import duty altogether, whereas high-cost-of-funds firms continue to rely on international trade financing that is subject to RUSF despite its higher cost after the shock.

Impact of RUSF increase on firms' costs. The direct effect of a change in τ on the firm f's unit costs increases with the firm's exposure to international trade financing. Moreover, for a given exposure, firms that have low funding costs experience a lower increase in their unit costs. The RUSF can also impact a firm's cost indirectly, through increases of firm's suppliers' direct costs caused by the increase of this import levy.

Change in sales as a result of the RUSF tax. As there is a negative effect on the firm's production costs due to the increase in the input prices, given the perfect competition assumption, such cost increases are fully transmitted by firms onto their output prices (which is consistent with our empirical results). Moreover, firm f's buyers substitute away from f's variety towards other domestic varieties. As this increases the overall price level of domestic intermediates faced by buyer firms, there is some substitution towards foreign intermediates. Final demand for firm f's variety also falls. Importantly, for a given level of reliance on international trade financing subject to RUSF, firms with high cost of funds would be subjected to a larger fall in sales because they experience a higher increase in their costs. These observations are summarized in the following proposition.[16]

Proposition 1. *The impact of a RUSF change on firm's sales is negative for firms using international trade financing subject to the RUSF tax and, ceteris paribus, increasing:*

- *(i) in the initial exposure of a firm to purchasing foreign intermediates with international trade financing that is subject to RUSF,*
- *(ii) in the firm's cost of funds, given the firm's initial exposure to international trade financing that is subjected to RUSF.*

Another relevant cost channel operates through RUSF-exposed importers that act as domestic suppliers. As these importer-suppliers' imported-input costs go up due to RUSF increase, there is an increase in their total costs, generating a passthrough to the buyer's costs, which in turn affects buyer's sales.[17] As a result, we can state the following.

Proposition 2. *The impact of a RUSF increase on firm's sales through domestic suppliers is negative and increasing in:*

- *(i) the domestic input share,*
- *(ii) the imported input share of the firm's domestic suppliers, and*
- *(iii) the share of domestic suppliers that face high cost of funds, provided that at least some of such suppliers are exposed to RUSF.*

As mentioned above, among exposed firms, the costs and sales of those with high financing costs are more affected by the RUSF levy increase (as they have to pay the tax), and they are the primary source of propagation of the shock in the domestic production network. In the next section, we test these empirical implications.

[16] We report here the theoretical results when elasticities of substitution among domestic varieties and between domestic and foreign inputs are greater than one. This is because we are studying medium- and long-term effects of the shock (one to three years after impact). Hence, the substitution within domestic varieties (Broda & Weinstein, 2006) and between domestic and foreign inputs (see Imbs & Mejean, 2015 or Feenstra et al., 2018) should be greater than 1.

[17] Since the substitution between material inputs is not Cobb-Douglas, there could be also an effect on firms' sales coming from buyers (i.e., an upstream propagation of the shock) that are exposed to RUSF. In particular, such buyers' own sales could be reduced (and hence lowering their input purchases), but at the same time they could substitute away from foreign to domestic intermediates, increasing their expenditures. However, the results presented in table 1 suggest that this channel is not at play. See online appendix section I.4.4 for an analytical discussion.

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V. Empirical Results

A. Effect of the Shock on the Choice of Payment Terms

First, we investigate whether importers responded to the increase in the RUSF tax rate by changing the composition of the payment terms of their imports after October 2011 in order to avoid the increased RUSF duty and how their behavior was shaped by their initial financial position. To do so, we use the TSI dataset which reports firm-level imports as well as their financing terms. We estimate an event-study design as follows:

$$\Delta Y_{ft} = \sum_{l=2009}^{2014} \beta_l \left(D_t^l \times Exposure_{f,t=2010}\right) + \sum_{l=2009}^{2014} \theta_l \left(D_t^l \times X_{f,t=2010}\right) + \alpha_{ir} + \alpha_{it} + \alpha_{rt} + e_{ft}, \quad (7)$$

where Δ denotes cumulative changes relative to 2011. Y_{ft} is an outcome variable pertaining to firm f operating in industry i and region r in year t. The $X_{f,t=2010}$ vector represents firm-level control variables from 2010, such as initial size in terms of employment and import intensity (share of cost of imported inputs in total costs). The latter is particularly important as the shares in our Bartik-type variable are incomplete: since the denominator of $\omega_{fv,t=2010}$ in equation (1) is the firm's total costs rather than its total imports, the shares do not add up to one at the firm level. In this, we follow Borusyak et al. (2021) who suggest controlling for incomplete shares in the regression, as failing to do so could pose a threat to identification. Moreover, adding the initial import intensity of firms to the specification controls for other trade related shocks such as exchange rate movements.[18]

The specification controls for industry-region fixed effects α_{ir}, where i indexes four-digit NACE industry segments and r corresponds to the 81 contiguous administrative regions. These fixed effects control for confounding factors or shocks that could vary at the economy, industry, region, or industry-region levels. We also include industry-year (α_{it}) and region-year (α_{rt}) fixed effects to take into account time-varying industry and regional shocks, respectively. As our firm-level dependent variable is differenced, time-invariant firm-level unobservables, which might otherwise influence our results, are also eliminated. In all regressions, standard errors are clustered at the industry-region level.[19]

[18] As shown by Gopinath and Neiman (2014), exchange rates movements affect the use of imported inputs. However, because 97% of Turkish imports during the period under study were denominated in foreign currencies, changes in import intensity will reflect changes in exchange rates. Nevertheless, we revisit the issue of exchange rate movements in our robustness checks in section VD.

[19] Our results are robust to clustering standard errors at the four-digit NACE industries. The number of two-digit manufacturing industries in NACE classification is not sufficiently large for us to use as our alternative clustering variable.

The coefficients of interest, β_l, capture the impact of the importing firm's exposure to the RUSF tax (measured prior to the tax rate increase in October 2011) on various outcome variables. The sample covers the years from 2009 to 2014, with 2009–2011 defined as the preshock period. Including observations for 2009–2011 allows us to test for the existence of pretrends in the relationship between the firm-level outcome and its exposure to the RUSF shock, while including later years is useful for understanding the persistence of the effects.

The top panel of figure 1 plots the estimates of β_l from a specification where the dependent variable is defined as the share of imports subject to the RUSF tax purchased by firm f in year t. We first present the estimates for the full sample of importers, and the plot the estimates separately for financially constrained and unconstrained importers (as defined in section IIIB). The estimates show that firms highly exposed to the RUSF tax before the tax rate increase changed the composition of their imports with respect to financing terms. Namely, they decreased the share of imports benefiting from foreign trade credit subjected to the RUSF tax. The decline took place in 2012 and persisted in the two subsequent years. The decline was very substantial in the case of financially unconstrained firms, and much smaller (albeit still statistically significant) in the case of financially constrained firms. These results suggest that at least some firms that were using RUSF-affected import-payment terms intensively before the tax rate increase, switched to alternative payment terms to avoid paying the higher tax. Their ability to do so was determined by whether or not they were facing liquidity constraints.[20]

Reassuringly for our identification strategy, the coefficient estimates for pre-shock years are all small in magnitude and not significantly different from zero. This finding suggests that the assumption of parallel trends for the treated and control groups in the pre-treatment period, which is a prerequisite for a valid difference-in-differences estimation, cannot be rejected.

In the middle panel of figure 1, we consider the share of imports purchased on cash-in-advance (CIA) terms, which are not subject to the RUSF tax. We show that importers that were initially highly exposed to the RUSF tax and were unconstrained in terms of liquidity switched to CIA financing after the increase in the tax rate. The large positive and statistically significant estimates found in all post-shock years imply that the switch to a greater reliance on CIA terms appears to be permanent. In contrast, for liquidity constrained firms, exposed to RUSF prior to the tax increase, the shift to CIA imports was less pronounced and temporary. In fact, only the estimate for the year immediately following the tax increase is statistically significant.

[20] 2SLS estimates obtained from using the Bartik-type exposure variable as an instrument for the firm's actual exposure yield very similar estimates to the reduced-form estimates presented in figure 1.

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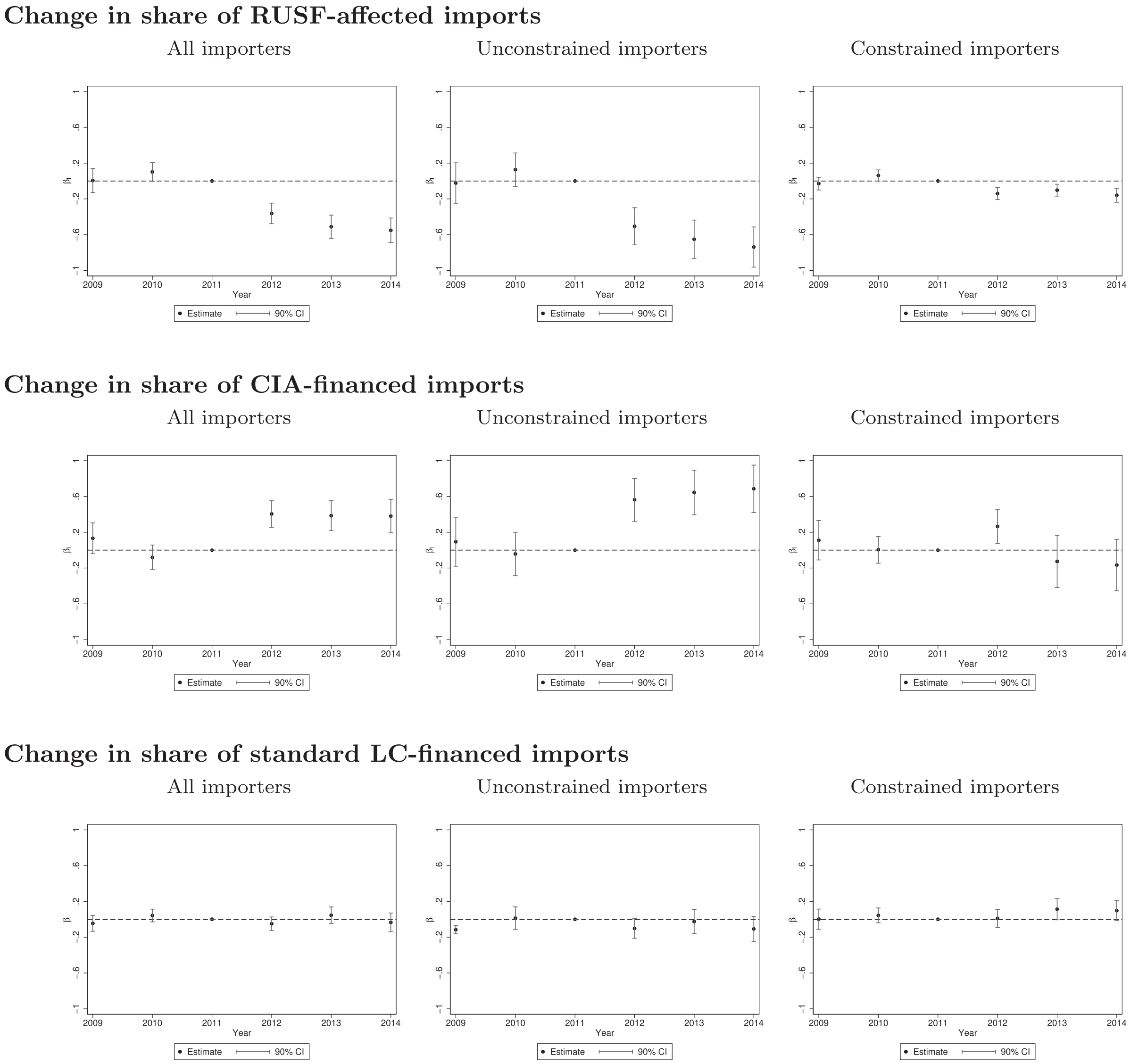
FIGURE 1.—EFFECT OF THE SHOCK ON IMPORT FINANCING: LAGS AND LEADS



The figure plots the estimates of β_l, together with 90% confidence intervals, obtained from estimating the specification in equation (7). Dependent variable changes across sub-figures as stated in the titles. Unconstrained firms are those which have an financing costs-to-sales ratio below their industry average in 2010. Robust standard errors (in parentheses) are clustered at the industry-region level.

In the bottom panel of the figure, we focus on the share of imports financed by standard letters of credit, that is, an international trade financing instrument that is not subject to the RUSF tax. We find no economically discernible increase in the LC-financed share of imports after the shock in any of the graphs. This is not surprising, as LCs involve a bank guarantee that a payment will be made to the exporter and hence a fee that needs to be paid to the bank issuing an LC.

All these findings are in line with the intuition developed in section IV. They suggest that financially unconstrained firms that were exposed to the RUSF tax prior to the rate change mitigated the impact of the policy change by switching to CIA financing. Financially constrained firms appear not to have been able to do so and as a result may have faced a greater increase in their material inputs costs. Consequently, financially constrained exposed firms were more affected by the tax increase than unconstrained ones.

B. *Effect of the Shock on Sales*

Next, we focus on the direct impact of the shock on the affected firms' performance, which we measure in terms of sales. We estimate the same equation in equation (7) using the MoIT data set except that we replace *Exposure* variable with τ (as defined in equation (2) in order to facilitate the interpretation of our results by converting the estimates into tax elasticities.

As visible in the top-left panel of figure 2, the tax increase had a negative and statistically significant impact on sales of the affected firms in 2012, but not in the subsequent years. The estimated coefficient for 2012, equal to -5.7, is

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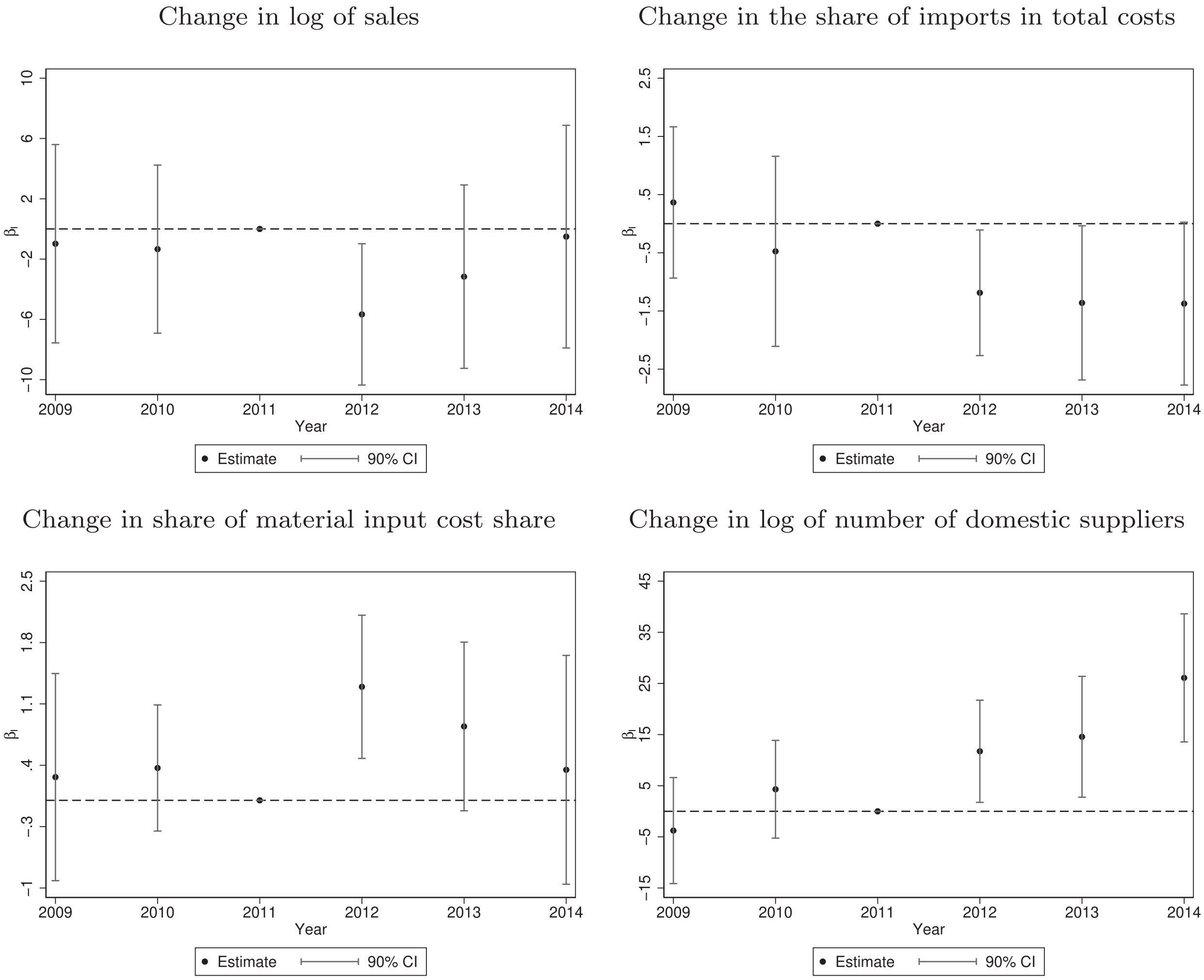
FIGURE 2.—DIRECT EFFECT OF THE SHOCK: LAGS AND LEADS



The figure plots the estimates of β_l, together with 90% confidence intervals, obtained from estimating the specification in equation (7). The dependent variable changes across sub-figures, as clearly stated in the titles. It is the growth rate of sales of firm f operating in industry i and located in region r in the top-left panel, the change in the share of imports in total costs (where total costs are defined as input purchases and wages) in top-right panel, the change in the share of input purchases in total costs in bottom-right panel, and the change in the logarithm of the number of domestic suppliers in bottom-right panel. All changes are relative to the year 2011. Robust standard errors (in parentheses) are clustered at the industry-region level.

statistically significant at the 5% level. To interpret this estimate's magnitude we revert to our conceptual framework (see online appendix section I.4) that gives an expression for the estimated tax elasticity of firm sales.

The said elasticity is composed of two parts: (i) the elasticity of price with respect to RUSF tax ($\partial \ln p_f / \partial ln \tau_f$), and (ii) the price elasticity of Y_f. The latter is equal to $(1 - \varepsilon_H)$ under CES demand with elasticity ε_H. The size of this elasticity depends on the passthrough of taxes onto costs and firm's mark-ups. To recover the value of the elasticity of price with respect to tax, we assume the price elasticity of demand to be $\varepsilon_H = 5$ (Broda & Weinstein, 2006). This gives a passthrough rate of tax to prices of about 1.4, which is comparable to the estimates of tariff passthrough onto producer prices reported in a recent paper by Amiti, Redding et al. (2019).[21] Assuming a less conservative estimate of the price elasticity of demand implies a lower passthrough rate. For instance, assuming $\varepsilon_H = 9.65$, based on the estimates reported by Head and Ries (2001), would imply a passthrough rate of tax onto prices of 0.7.[22] Depending on the assumed value of price elasticity of demand, the estimated price response to a one standard deviation increase in *Exposure* for importers ranges between 0.07% and 0.13%.[23]

[21]Using the tariff changes introduced during the 2018 trade war, Amiti, Redding et al. (2019) estimate that the passthrough rate of input tariffs onto domestic producer prices is 1.8. In another paper, Fajgelbaum et al. (2019) also exploit the recent changes in the U.S. trade policy and report a complete passthrough of the tariffs to import prices.

[22]Head and Ries (2001) estimate price elasticities focusing on the U.S.-Canada trade. As the two economies are very similar, we believe that their estimates may be more appropriate in our within-Turkey context than others available in the literature. Head and Ries (2001) obtain price elasticities of demand ranging between 7.9 and 11.4 depending on the specification. In our calculations, we use the average of these two values, namely, 9.65.

[23]One can cite additional factors that may play a role increasing the tax passthrough. For example, RUSF introduction may nudge firms to increase their prices earlier than planned—as shown by Gagnon (2009) prices can be staggered even in economies with medium inflation such as Turkey. Moreover, as exposed firms move away from foreign intermediates (as

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To gauge the economic significance of the estimated effect on sales, let us consider a one-standard-deviation increase in *Exposure*. Our baseline estimate implies a 0.55% decline in sales for such importer. This effect is economically important as the average sales growth between 2011 and 2012 observed in the data is 8%.

C. Effect of the Shock on Sourcing Patterns

As we expect firms directly exposed to the tax shock to move away from imported inputs and increase their reliance on domestic suppliers, we proceed to analyse firms' sourcing patterns. Our findings below confirm that such a substitution does indeed take place.

The top-right panel of figure 2 shows the estimation results for the cumulative change in the share of imports in total costs (defined as the sum of input purchases and wages) relative to 2011 as the outcome variable.[24] The increase in the RUSF tax in October 2011 discouraged exposed firms from using imported inputs: their import intensity fell in 2012 and stayed low for at least two more years relative to the preshock level.

In the bottom-left panel, the dependent variable is the change in the share of input purchases in total variable costs. The estimated coefficient for 2012 is positive and statistically significant, implying that firms exposed to the shock experienced an increase in their input costs relative to their overall costs immediately after the shock. However, the effect is short-lived and disappears in 2013 and 2014. To the extent that the exposed firms pass this initial cost increase into their prices, the shock will affect their buyers regardless of whether they are themselves directly affected by the shock or not. Therefore this result provides the first piece of evidence for the network channel of shock propagation that we investigate further in the paper.

Consistent with the observed decrease in the imported goods as a fraction of total input purchases, firms directly exposed to the tax appear to have substituted foreign inputs with domestic ones. This is illustrated in the bottom-left panel of figure 2, where the outcome variable is the change in the logarithm of the number of domestic suppliers relative to 2011. The coefficients of interest are positive and statistically significant for the entire post-shock period, implying that, similar to the effect of the shock on the exposed firms' import intensity, its effect on firms' domestic supplier network was permanent over the three years that follow post-RUSF increase.

The results presented in figure 2 convey two important messages. First, as in figure 1, they are in line with the assumption of parallel pretrends for all of the outcome variables considered, lending credibility to our identification assumption. Second, the RUSF shock appears to have had a temporary effect on sales and costs but a permanent effect on the share of imported inputs and the number of domestic suppliers. The finding that adjustment to the domestic supplier network continues beyond 2012 is consistent with the view that it takes time to switch from imported inputs purchased from foreign suppliers to domestically produced ones.

In the remainder of the paper, we will restrict our focus to firms' short-term responses to the RUSF shock (i.e., those between 2011 and 2012).

D. Robustness Checks

To verify that the coefficient estimates that we observe in figures 1 and 2 are really driven by the changes in the RUSF tax, we conduct a number of robustness tests. We summarize the findings here and present the results in the online appendix.

Switching foreign suppliers. Recall that in our theoretical framework searching for new foreign suppliers would be a dominated strategy for all importers affected by the RUSF increase. In support of this view, online appendix table II3 presents evidence indicating that Turkish importers, exposed to the RUSF shock, did not switch to other source countries within product categories after the tax increase.[25]

A placebo test. As explained in section II, processing imports are not subject to the RUSF tax. We take advantage of this fact and construct a placebo measure of exposure τ_f based on processing imports. As presented in panel A of online appendix table II4, the estimated coefficients of $\Delta \ln \tau_f$ are not economically or statistically significant for any of the outcome variables. This outcome lends further credibility to our findings as it suggests that our baseline results are indeed driven by exposure to the RUSF tax and not by other macroeconomic changes that were unaccounted for in our estimations.

Omitted variables. To address the concern that our baseline estimates could be driven by omitted variables, we include additional control variables in the baseline specification. These variables are constructed using similar shares as in equation (1) and shifts that are potentially correlated with $Exposure_{v,t=2010}$. These alternative "shifts" are changes in source-country real per capita GDP between 2010 and 2012, and the share of USD-denominated imports at the variety level as of 2010. The former controls for economic developments in the import-source-country during the same period, whereas the latter accounts for cross-currency movements

documented earlier in this section), they bear search costs and adjustment costs for the usage of new intermediate inputs. In the short run, these factors are likely to increase the variable costs.

[24]Since MoIT dataset does not report firm output in terms of quantities produced, it is not possible to calculate the average costs. Therefore we normalize input costs by total variable costs.

[25]We are constrained to conduct this test at the supplier-country level (and not at the supplier level) because we cannot trace Turkish importers' individual foreign suppliers in the data.

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that are potentially associated with switches between payment methods.[26] As discussed in Borusyak et al. (2021), adding such controls in the estimation is useful not only for consistency but also for obtaining valid inference (see Adao et al., 2019). As reported in panels B and C of online appendix table II4, our results are robust to the additional of the above-mentioned control variables. In another robustness check, we investigate whether accounting for exposed firms' capital structure, which is likely to be representative of their longer-term financial constraints, matters for the baseline results. The results presented in the bottom panel of table II4, show that adding the firm's initial leverage ratio, defined as the ratio of total debt to total assets as of 2010, as an additional control does not affect the baseline estimates for the coefficients of interest.

Exclusion of influential product categories and source countries. In the spirit of the suggestions by Goldsmith-Pinkham et al. (2020), we check the sensitivity of our estimates to the exclusion of influential product categories and source countries.[27] We do so by constructing various restricted samples by dropping (i) the top 20 2-digit HS codes in terms of RUSF exposure in 2010; (ii) the top 20 source countries in terms of RUSF exposure in 2010; (iii) the top 5 2-digit HS codes, (iv) the top 5 import source countries. Note that either (iii) or (iv) alone accounted for close to half of Turkey's nonenergy imports in 2010. Online appendix table II5 presents the results. The estimates for sales growth obtained from each restricted sample are close to our baseline estimate, implying that our findings are not driven by certain product categories or source countries.

2SLS approach. Finally, we confirm in online appendix table II7 our earlier claim that 2SLS estimation, where $Exposure_{f,t=2010}$ is used as an instrument for the actual firm-level RUSF exposure, would yield an estimate that is very close to the reduced-form estimate (i.e., our baseline estimates). To do so, we examine the impact of the shock on firm sales using the TSI as well as the MoIT datasets.[28] In the TSI dataset, we observe the *actual* firm-level exposure to the RUSF tax change and can also construct the Bartik-type exposure variable, while in the MoIT dataset we have to rely on the latter alone, that is, the *predicted* exposure based on the composition of firm-level imports. The take-away message from the results presented in table II7 is that the estimates from the reduced form approach are very close to those obtained from the IV specification. This validates our choice of relying on MoIT data whose main advantage is observing firm-to-firm linkages that will allow us to investigate propagation of the shock through production networks.

E. *Network Effects and the Role of Financial Constraints*

Network effects. In this section, we focus on the short-term effects of the shock [i.e., changes between the preshock period ($t - 1 = 2011$) and the post-shock period ($t = 2012$)] and extend the analysis in two important directions.[29]

First, we examine whether the RUSF shock propagates beyond the directly exposed firms by including in the estimation a firm's indirect exposure to the RUSF shock through its suppliers and buyers [as defined in equations (5) and (6)]. Second, and importantly, we investigate the role of financial constraints in this secondary shock propagation through suppliers and buyers. As before, the baseline regression takes the form of a difference-in-differences model:

$$\begin{aligned}\Delta \ln Y_f = {} & \gamma \Delta \ln \tau_f + \gamma_s \Delta \ln \tau_f^{Suppliers} + \gamma_b \Delta \ln \tau_f^{Buyers} \\ & + \delta \text{Unconstrained}_{f,t=2010} \times \Delta \ln \tau_f \\ & + \delta_s \text{Unconstrained}_{f,t=2010} \times \Delta \ln \tau_f^{Suppliers} \\ & + \delta_b \text{Unconstrained}_{f,t=2010} \times \Delta \ln \tau_f^{Buyers} \\ & + \Gamma X_{f,t=2010} + \alpha_{ir} + \upsilon_f \qquad (8)\end{aligned}$$

We consider the same firm-level outcomes as before, namely, changes in sales, the share of imports in total costs, the share of input purchases in total costs, and the number of domestic suppliers. As before, we control for firm size (employment) and import intensity as of 2010. To investigate the role of financial constraints in shock propagation, we interact proxies for the tax exposure with an indicator for whether or not the firm was initially liquidity unconstrained ($\text{Unconstrained}_{f,t=2010}$) (defined in section IIIB). We use several variants of equation (8) in our analysis.[30]

We start by investigating the network effect without considering financial constraints. The results are presented in the bottom panel of table 1, with the top panel showing for comparison the estimates of a specification capturing just the direct effects. Three observations emerge from the table.

First, taking network effects into account does not affect the magnitudes and significance levels of the coefficients on the direct exposure to the RUSF tax: directly exposed firms reduced their reliance on imported inputs, experienced an increase in the cost of their input purchases, expanded their domestic supplier network, and suffered lower sales growth compared to nonexposed firms.

[26] During the period under consideration, the share of USD-denominated Turkish imports was 62%, with the rest distributed between Euro (33%), and TL (3%), and other currencies (2%).

[27] These tests are conducted to address potential endogeneity of the shares. Unfortunately, given the sheer number of our shifts (approximately 75,000) we cannot perform other tests suggested by Goldsmith-Pinkham et al. (2020).

[28] We have to restrict our attention to gross sales, as it is the only outcome variable that we can construct using both datasets in a comparable way.

[29] The tax increase took place on October 13, 2011, so for the purposes of this analysis we consider 2011 to be the preshock year. If firm-level outcomes were to be affected by the short period (between mid-October and end of December in 2011) during which the higher tax rate was in effect, this would work *against* us finding any impact of the shock.

[30] We focus on first-order network effects. Adding higher-order network exposures does not affect the size of the estimates.

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TABLE 1.—EFFECTS OF THE SHOCK ON FIRMS' INPUTS AND SALES

Dep var:	$\Delta_{11-12} \ln Sales_f$	$\Delta_{11-12} \left(\frac{\text{Imports}}{\text{Total costs}}\right)_f$	$\Delta_{11-12} \left(\frac{\text{Total input purchases}}{\text{Total costs}}\right)_f$	Δ_{11-12} ln Domestic suppliers$_f$
$\Delta \ln \tau_f$	−6.592**	−1.413**	1.370**	9.980*
	(3.057)	(0.632)	(0.535)	(5.644)
R^2	0.0725	0.102	0.0805	0.0838
$\Delta \ln \tau_f$	−6.527**	−1.414**	1.242**	10.02*
	(3.055)	(0.631)	(0.539)	(5.654)
$\Delta \ln \tau_f^{Suppliers}$	−6.260*	0.0486	1.578**	−3.925
	(3.538)	(0.194)	(0.706)	(6.366)
$\Delta \ln \tau_f^{Buyers}$	−0.0017	0.0002	−0.0021	−0.0037
	(0.0022)	(0.0002)	(0.0013)	(0.0099)
R^2	0.0726	0.102	0.0828	0.0838
N	53,397	53,397	53,397	53,397
Fixed effects	i-r	i-r	i-r	i-r
Firm-level controls	Yes	Yes	Yes	Yes

This table shows the results from estimating specification in equation (8) where the dependent variable changes across columns as follows. It is the growth rate of sales in column 1, the annual change in the share of imports in total costs (where total costs are defined as input purchases and wages) in column 2, annual change in the share of input purchases in total costs in column 3, and the change in the logarithm of the number of domestic suppliers of firm f operating in industry i and located in region r in column 4. τ_f captures the firm-level effective tax rate, as defined in equation (2). $\tau_f^{Suppliers}$ and τ_f^{Buyers} are defined similarly in equations (5) and (6). All columns include ln Employment$_{f,t=2010}$, that is the logarithm of the number of employees, and Import Intensity$_{f,2010}$, that is the share of imports in total costs of firm f in 2010, as additional controls. The fixed effects are at the industry-region (ir) level. Robust standard errors (in parentheses) are clustered at the industry-region level. *, **, and *** represent significance at the 10, 5, and 1 percent levels, respectively.

Second, and more importantly, we find evidence of a downstream propagation of the RUSF shock from suppliers to their customers, as indicated by a decrease in the sales growth of the latter. In other words, we observe that firms are indirectly affected by the shock if their suppliers are directly exposed to the RUSF increase. This finding, which is separate from firms' possible own-shock exposure, is consistent with a pricing channel, assuming that (i) importers reflect the RUSF increase in their prices, and (ii) it is costly for firms to switch from suppliers affected by the tax increase to other domestic suppliers, at least in the short-term. Indeed, in the third column, we find that firms that are exposed to the tax shock through their suppliers experience an increase in the cost of their input purchases. When we consider the impact of the shock on sales (column 1), the estimated coefficient on $\Delta \ln \tau_f^{Suppliers}$, equal to -6.3, is of the same magnitude as the direct effect. This suggests that, for an equal-sized direct exposure and supplier-driven exposure to the RUSF increase, the effect on sales through suppliers is comparable to the direct effect: there is propagation of the shock through the network.

Third, and equally importantly, we find no evidence of upstream propagation of the shock from the exposed buyers to their domestic suppliers. The coefficient on $\Delta \ln \tau_f^{Buyers}$ is small and statistically insignificant for all of the outcome variables presented in table 1. This finding is consistent with the results of Acemoglu et al. (2016) and our model (see online appendix section I.4.4).

The role of financial constraints. Now we focus on the role of financial constraints in shock transmission. The results in section VA indicate that liquidity constrained firms tend to pay the increased tax rather than to switch to an immediate payment for the inputs they continue to import. This means they may be more likely to transmit the shock to their business partners. Our prediction that financial constraints, in the form of liquidity constraints, play an important role in economic shocks' transmission find support in the results obtained from estimating equation (8) and presented in table 2.

The estimates presented in the first column suggest that the direct impact of the shock on sales growth is negative and statistically significant only for financially constrained firms. The implied passthrough of tax onto prices for financially constrained firms is twice as large as the one obtained for the full sample, and it is close to zero for unconstrained firms. This means that the direct effects of the tax increase on firm sales reported earlier are driven primarily by firms with a relatively high cost of funding.

The cost channel appears relevant here: while import intensity decreased for both types of firms that were exposed to the RUSF tax (see column 2), estimates in column 3 suggest that financially constrained firms experienced a statistically significant increase in their input costs, while financially unconstrained firms were not affected. Moreover, only unconstrained firms were able to expand their domestic supplier network (see column 4), which is presumably related to their ability to bear supplier search costs. This finding is especially important, because it corroborates the financial constraints channel as opposed to a general cost-push mechanism: access to short-term liquidity allows a firm to invest in alternative suppliers and decrease import intensity, weathering the shock.[31]

Finally, financially unconstrained firms were also more successful in dampening the negative effect of the RUSF shock that they faced through their exposed suppliers (cf. the combined effects in the middle of table 2): in contrast

[31] We would like to thank an anonymous referee for this interpretation of our findings.

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TABLE 2.—ROLE OF FINANCIAL CONSTRAINTS

Dep var:	$\Delta_{11-12} \ln Sales_f$	$\Delta_{11-12} \left(\frac{\text{Imports}}{\text{Total costs}}\right)_f$	$\Delta_{11-12} \left(\frac{\text{Total input purchases}}{\text{Total costs}}\right)_f$	$\Delta_{11-12} \ln \text{Domestic suppliers}_f$
	(1)	(2)	(3)	(4)
$\Delta \ln \tau_f$	−11.62***	−1.823**	2.154***	−3.608
	(4.367)	(0.818)	(0.663)	(11.23)
$\Delta \ln \tau_f^{Suppliers}$	−8.786*	0.0125	1.940***	−18.29**
	(5.199)	(0.224)	(0.953)	(8.449)
$\Delta \ln \tau_f^{Buyers}$	−0.0007	0.0002	−0.0021	0.0006
	(0.0027)	(0.0002)	(0.0013)	(0.0086)
Unconstrained $_{f,t=2010} \times$				
$\Delta \ln \tau_f$	11.04**	0.126	−1.305*	23.44**
	(4.789)	(0.951)	(0.696)	(10.06)
$\Delta \ln \tau_f^{Suppliers}$	0.409	0.622	−0.710	34.50***
	(7.359)	(0.402)	(1.460)	(12.81)
$\Delta \ln \tau_f^{Buyers}$	−0.894	0.253	−0.392	−2.122
	(1.806)	(0.153)	(1.640)	(15.61)
Combined effects for unconstrained firms (sum of the main effect and interaction)				
$\Delta \ln \tau_f$	−0.58	−1.697**	0.849	19.832**
$\Delta \ln \tau_f^{Suppliers}$	−8.377	0.635*	1.230	16.21*
$\Delta \ln \tau_f^{Buyers}$	−0.895	0.253	−0.394	−2.121
R^2	0.0726	0.102	0.0828	0.0919
N	53,397	53,397	53,397	53,397
Fixed effects	i-r	i-r	i-r	i-r
Firm-level controls	Yes	Yes	Yes	Yes

This table shows the results from estimating equation (8). Dependent variable changes across columns as follows. It is the growth rate of sales in column 1, the annual change in the share of imports in total costs (where total costs are defined as input purchases and wages) in column 2, annual change in the share of input purchases in total costs in column 3, and the change in the number of domestic suppliers of firm f operating in industry i and located in region r in column 4. τ_f captures the firm-level effective tax rate, as defined in equation (2). $\tau_f^{Suppliers}$ and τ_f^{Buyers} are defined similarly in equations (5) and (6). Unconstrained$_{f,t=2010}$ is a dummy variable indicating liquidity-unconstrained firms, which have an financing costs-to-sales ratio below their industry average in 2010. All columns include $\ln \text{Employment}_{f,t=2010}$, that is the logarithm of the number of employees, and Import Intensity$_{f,2010}$, that is the share of imports in total costs of firm f in 2010, and Unconstrained $_{f,t=2010}$ as additional controls. The fixed effects are at the industry-region (i-r) level. Robust standard errors (in parentheses) are clustered at the industry-region level. *, **, and *** represent significance at the 10, 5, and 1 percent levels, respectively.

to the constrained firms, they were more able to substitute away from their exposed domestic suppliers.[32]

In our model, the RUSF shock affecting firm prices is equivalent to an iceberg trade shock (as we do not consider the tax rebates to households) and with nested-CES input demand has an impact on affected firms isomorphic to a (negative) productivity shock (see Baqaee & Farhi, 2019 for details). The first order effect of our shock on real output of an exposed firm is approximated by a result akin to the Hulten theorem and is equal to the ratio of the firm sales to GDP multiplied by the RUSF price elasticity (see the online appendix I.5). Empirically, the RUSF price elasticities are negative and statistically significant only for liquidity constrained firms. Consequently, only these firms have a negative impact on real output.

F. *The Role of Financially Constrained Suppliers*

Our observation about financially constrained firms propagating the shock through the production network finds further support when we study how the shock is transmitted by financially constrained suppliers.

To distinguish between liquidity-constrained and -unconstrained suppliers as well as buyers when considering the indirect effects of the shock we create two measures of indirect exposure via suppliers ($Exposure_{f,t=2010}^{Suppliers,Uncons}$, $Exposure_{f,t=2010}^{Suppliers,Cons}$), and do the same for buyers ($Exposure_{f,t=2010}^{Buyers,Uncons}$, $Exposure_{f,t=2010}^{Buyers,Cons}$). We then estimate equation (8) augmented with these additional variables.

The results presented in the top panel of table 3 suggest that the indirect effects are driven only by financially constrained suppliers. The statistically significant coefficients on financially constrained suppliers found in columns 1 and 3 demonstrate how their role in shock propagation is visible in their customers' input cost share and sales. In contrast, none of the coefficients on financially unconstrained suppliers reaches conventional significance levels. Put differently, suppliers with relatively low cost of funds, who are exposed to the tax increase, do not transmit the shock to their customers regardless of whether their customers are themselves constrained or not. The cost-shock propagation occurs downstream only through financially constrained firms. Our point estimates suggest that there might be some amplification of the effects in supply chains that include financially constrained firms.

As before, there is no evidence of shock being propagated upstream: we observe no statistically discernible effect for financially constrained or unconstrained buyers.

[32]As a robustness check, we investigate whether allowing the passthrough rate to change with firm size affects our results. For instance, in a recent paper, Amiti, Itskhoki et al. (2019) show that large firms have lower passthrough rates, and thus their sales respond less to changes in their costs. To do so, we add an interaction between initial firm size and exposure to the RUSF shock to the baseline specification. As reported in online appendix table II11, this modification strengthens our main result that financial constraints, in the form of liquidity constraints, play a role in economic shocks' transmission.

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TABLE 3.—ROLE OF FINANCIAL CONSTRAINTS: DIRECT AND INDIRECT EFFECTS

Dep var:	$\Delta_{11-12} \ln Sales_f$ (1)	$\Delta_{11-12} \left(\frac{\text{Imports}}{\text{Total costs}}\right)_f$ (2)	$\Delta_{11-12} \left(\frac{\text{Total input purchases}}{\text{Total costs}}\right)_f$ (3)	$\Delta_{11-12} \ln \text{Domestic suppliers}_f$ (4)
$\Delta \ln \tau_f$	−6.529**	−1.415***	1.348**	9.971*
	(3.054)	(0.631)	(0.536)	(5.650)
$\Delta \ln \tau_f^{Suppliers,Cons}$	−11.13**	−0.0238	2.547***	−7.956
	(4.799)	(0.283)	(0.957)	(8.427)
$\Delta \ln \tau_f^{Suppliers,Uncons}$	−2.644	0.287	1.153	4.453
	(6.267)	(0.284)	(1.149)	(11.35)
$\Delta \ln \tau_f^{Buyers,Cons}$	−0.0010	0.0000	−0.435	0.0147
	(0.0071)	(0.0003)	(0.0633)	(0.0121)
$\Delta \ln \tau_f^{Buyers,Uncons}$	−0.0022	0.0003	1.774	−0.0146
	(0.0028)	(0.0005)	(1.569)	(0.0204)
R^2	0.0726	0.102	0.0808	0.0839
N	53,397	53,397	53,397	53,397
Fixed effects	i-r	i-r	i-r	i-r
Firm-level controls	Yes	Yes	Yes	Yes
Effects for constrained firms				
$\Delta \ln \tau_f$	−12.41**	−1.369*	2.277***	−3.645
$\Delta \ln \tau_f^{Suppliers,Cons}$	−19.35**	−0.386	3.981*	−16.24
$\Delta \ln \tau_f^{Suppliers,Uncons}$	−5.514	0.361	1.842	−22.33
$\Delta \ln \tau_f^{Buyers,Cons}$	0.0567	0.0000	−0.244	0.0055
$\Delta \ln \tau_f^{Buyers,Uncons}$	−0.0113	0.0027	−0.177	−0.0024
Effects for unconstrained firms				
$\Delta \ln \tau_f$	−2.503	−1.443*	0.649	19.90**
$\Delta \ln \tau_f^{Suppliers,Cons}$	−14.126*	0.566	2.940**	6.54
$\Delta \ln \tau_f^{Suppliers,Uncons}$	−9.283	0.039	0.828	36.62
$\Delta \ln \tau_f^{Buyers,Cons}$	−2.151	−0.0002	−2.687	−14.475
$\Delta \ln \tau_f^{Buyers,Uncons}$	−1.256	0.226	1.842	5.186

This table shows the results from estimating an extended version of equation (8) where the dependent variable changes across columns as follows. It is the growth rate of sales in column 1, the annual change in the share of imports in total costs (where total costs are defined as input purchases and wages) in column 2, annual change in the share of input purchases in total costs in column 3, and the change in the number of domestic suppliers of firm f operating in industry i and located in region r in column 4. τ_f captures the firm-level effective tax rate, as defined in equation (2). $\tau_f^{Suppliers}$ and τ_f^{Buyers} are defined similarly in equations (5) and (6). $\tau_f^{Suppliers,Uncons}$ ($\tau_f^{Buyers,Uncons}$) denotes the weighted average of liquidity-unconstrained suppliers' (buyers') effective tax rate (i.e., suppliers (buyers) with the financing costs-to-sales ratio below their industry average in 2010). $\tau_f^{Suppliers,Cons}$ ($\tau_f^{Buyers,Cons}$) denotes the weighted average of liquidity-constrained suppliers' (buyers') effective tax rate (i.e., suppliers (buyers) with the financing costs-to-sales ratio above their industry average in 2010) of firm f. All columns include $\ln \text{Employment}_{f,t=2010}$, that is the logarithm of the number of employees, and $\text{Import Intensity}_{f,2010}$, that is the share of imports in total costs of firm f in 2010, and $\text{Unconstrained}_{f,t=2010}$ as additional controls. The estimated effects for constrained and unconstrained firms presented in the lower panel of the table are reproduced from table II.13 in the online appendix. The fixed effects are at the industry-region (ir) level. Robust standard errors (in parentheses) are clustered at the industry-region level. *, **, and *** represent significance at the 10, 5, and 1 percent levels, respectively.

Finally, the bottom panel of table 3 shows that financially unconstrained firms are better able to weather the negative effects of the RUSF shock originating from their exposed and financially constrained suppliers. Their sales decline less and they experience a lower increase in the input cost share.

To double check that we are really capturing the impact of suppliers' financial constraints rather than the effects of an omitted variable correlated with size (since access to finance is easier for larger firms), we include two additional variables in our regression specification: the size-weighted average exposures of the firm to its suppliers and buyers as of the year 2010.[33] The results presented in online appendix table II12 confirm our earlier findings: the indirect effect is much larger for firm's cost of input purchases and sales when it comes from financially constrained suppliers.[34]

Summarizing, the results in this subsection support the predictions of our simple theory framework. They are also consistent with the view that it is predominantly the liquidity-constrained firms that magnify and propagate the economic perturbation downstream.

VI. Conclusions

This paper examines the role of financial constraints in propagation of a cost-push shock in a production network. Our analysis focuses on an unexpected policy change in Turkey that increased the price of imports relying on particular types of international financing terms from 3% to 6% overnight. Given differential exposure of importers to

[33]These variables are defined as follows: $\Delta\tau_f^{Suppliers,SizeWeighted} = \sum_s \omega_{fs,t=2010} \times \ln \overline{Employment}_{s,t=2010}$ where $\omega^S_{fs,t=2010}$ is the share of supplier s in firm f's total variable costs in year 2010; and $\overline{Employment}_{s,t=2010}$ is supplier s's employment relative to its industry average as of 2010. Similarly, we define $\Delta\tau_f^{Buyers,SizeWeighted} = \sum_s \omega^B_{fb,t=2010} \times \ln \overline{Employment}_{b,t=2010}$, where $\omega^B_{fb,t=2010}$ is the share of buyer b in firm f's total sales in year 2010, and $\overline{Employment}_{b,t=2010}$ is buyer b's employment relative to its industry average as of 2010.

[34]Tables II14–II17 in online appendix present results using two alternative measures of financial constraints, namely, the ratio of interest payments to existing debt stock and the sum of cash and cash equivalents to total assets.

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the international trade financing terms subject to the tax, the policy change had a heterogeneous impact across importers, a feature that we exploit in our analysis.

The results, based on the quasi universe of Turkish production network data, can be summarized as follows. First, we show that firms directly exposed to the tax prior to its increase adjusted differentially depending on theirs costs of obtaining financing. Unconstrained firms, that is, those with low financing costs, changed the payment methods used in import transactions to escape paying the increased tax rate. Financially constrained firms were unable to do so.

Second, we find that the tax increase induced changes in the input sourcing pattern away from imports. Financially constrained firms were hit harder by the shock and experienced an increase in their material input costs and a decline in sales. In contrast, financially unconstrained firms did not experience an increase in their input costs or a fall in their sales, but they lowered the share of imports in total costs and increased the number of domestic suppliers.

Third, in line with Acemoglu et al. (2016), we find that the supply shock that we consider propagated downstream through the exposed domestic suppliers, but not upstream through the affected domestic buyers. The indirect transmission of the shock through domestic suppliers was almost as large in magnitude as the effect of the direct exposure to the shock. The shock was transmitted by liquidity-constrained firms who amplified its magnitude.

Our findings suggest that even relatively minor economic shocks can affect open economies in ways that are non-negligible as they are transmitted over the production networks. The impact we observe is not limited to direct exposure only: indirect exposure through suppliers appears to have been equally important in terms of economic magnitudes. Importantly, the relatively small shock that we consider changed the exposed firms' supplier networks: the affected firms switched from imported varieties to their domestic counterparts. The resulting effects on firm performance appear to have been temporary, with the exception of the changes to exposed firms' supplier networks, which took place over a longer time period. A likely explanation is that the search costs involved in finding new suppliers: the observed changes in the supplier networks suggest that such costs are not economically trivial even in a domestic setting. We believe that quantifying the explicit and implicit search costs for substitute products and suppliers, which we do not undertake here, is worthy of further research.

Appendix

A. *Informativeness of the Bartik-Type Exposure*

As explained in the main text, because the MoIT dataset (despite including firm-level imports) does *not* contain any information on import payment methods, we rely on the Bartik-type *Exposure* variable to capture the extent to which Turkish firms were actually affected by the RUSF rate's increase.

To check the validity of this approach we conduct the following test. In the TSI customs dataset, which reports imports data at the firm-product-source country-payment method level, we create two exposure variables. The first one is the TSI-dataset equivalent of the Bartik-type exposure variable that we create in the MoIT-dataset: we apply equation (1) in the TSI dataset as if we could not observe the payment methods. The second variable that we create is the *actual* firm-level exposure based on payment-type information that is observed in the TSI dataset:

$$Exposure^{Actual}_{f,t=2010} = \frac{\sum_{m\in\{OA,AC,DLC\}} M_{fm,t=2010}}{\text{Total variable costs}_{f,t=2010}}, \tag{9}$$

where *Total variable costs* is equal to the sum of the costs of labor and domestic and imported material inputs. Using $Exposure^{Actual}_{f,t=2010}$, we also construct the actual effective tax increase as follows:

$$\Delta \ln \tau^{Actual}_{f} = Exposure^{Actual}_{f,t=2010} \times \ln\left(\frac{1+\tau_{2012}}{1+\tau_{2011}}\right) \tag{10}$$

Figure II4 in the online appendix shows that there is a significant overlap between the distributions of $Exposure_{f,t=2010}$ and $Exposure^{Actual}_{f,t=2010}$. To check the informativeness of our Bartik-type variable, using the TSI dataset we regress the actual firm-level RUSF exposure on the Bartik-type variable and present the results in table II6 in the online appendix. In the first column, we control only for industry-region fixed effects, where industries are defined at the four-digit NACE level and regions refer to 81 contiguous administrative regions into which Turkey is subdivided, with each region corresponding to a Turkish city (such as Ankara, Istanbul, Izmir, etc.) In the second column, we additionally control for firm size and import intensity (defined as cost of imported inputs in total costs). This exercise is first conducted on the sample of importing firms and later repeated for all firms (including nonimporters). In all specifications, the coefficient estimate of interest is positive and statistically significant at the one percent level. The estimate in the second column implies that a one-percent increase in the *predicted* firm-level $Exposure_{f,t=2010}$ is associated with a one-percent increase in the *actual* firm-level exposure to the RUSF shock for importing firms. The results are similar when the sample includes all firms in columns 3 and 4.

We draw two conclusions from these results. First, our Bartik-type exposure variable is highly informative about the actual firm-level exposure to the RUSF shock. Second, the magnitudes of the estimates presented in columns 2 and 4 of table II6 are not statistically different from one. This implies that a 2SLS regression, where the Bartik-type exposure variable constitutes an instrument for the actual firm-level

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exposure, would generate an estimate that is very close to the reduced-form estimate obtained by regressing the outcome variable directly on the instrument.[35]

B. Validity of the Shift-Share Design

We established in the previous section that $Exposure_{f,t=2010}$ is highly informative about importing firms' actual exposure, that is, their actual reliance on nondomestic financing of their imports. Moreover, our shocks are generated via imports (given the trade-financing type) from about 150 distinct source countries and 4,700 HS6 products, which amounts to a large number (approximately 75,000) of unique varieties. Finally, these shocks are highly dispersed: the standard deviation of $Exposure_{v,t=2010}$ is 0.28, and the inter-quartile range is 0.40. This observed dispersion only marginally decreases when the shocks are de-meaned by their country or two-digit HS code averages. All of this indicates that our Bartik-type (shift-share) instrument is in effect constructed from a large number of highly dispersed "shift-share shocks" (i.e., $Exposure_{v,t=2010}$) distributed heterogeneously across firms in different industries and regions.

Pertinent for our case, Borusyak et al. (2021) show that the "shocks" view of identification in shift-share instrumental variable regressions relies on an important condition: the average importance of any shock should be sufficiently small. To check whether this holds true in our data, we construct the average import share of each variety across importers: $\omega_v = \sum_f (1/N)\omega_{fv,t=2010}$, where N is the number of firms. The condition put forward by Borusyak et al. (2021) requires that even the largest ω_v must be small. In our data, the value of ω_v varies between (approximately) zero and 0.006.[36] Both the mean and median values of ω_v are close to zero. The concentration of shocks, as measured by the Herfindahl-Hirschman Index, is also low: the inverse of this index, which is close to 15,000 in our data, is informative about the effective number of shocks. These statistics suggest that in our specific case the average importance of any shock is sufficiently small. This means that in our setting identification is achieved through firm-level "shocks," this even in the presence of possibly endogenous shares. Nevertheless, following Goldsmith-Pinkham et al. (2020), we conduct a number of additional tests to check the sensitivity of our baseline results to potential endogeneity of shares (see section VD).

[35]In an exactly identified model with one endogenous variable, the following relationship holds between the 2SLS estimate (β^{2SLS}) and the reduced form estimate (β^{RF}): $\beta^{2SLS} = \beta^{RF}/\beta^{FS}$, where β^{FS} denotes the first-stage estimate. As the estimated coefficient in the fourth column of table II6 (i.e., β^{FS}) is not statistically different from one, the values of 2SLS and reduced-form estimates would be very close. We confirm this later in online appendix table II7.

[36]The relevant descriptive statistics are presented in online appendix table II10.

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