Firm Resiliency: The Role of Spillovers
INTERNET APPENDIX

January 10, 2024

A Theoretical Motivation for the Role of Spillovers

In this section, we present a simple theoretical framework to motivate our empirical tests of local spillovers between firms. Following the standard benchmark model in Glaeser and Gottlieb (2009), consider the standard Cobb-Douglas production function for firm i in county c:

$$Y_i = \zeta_i A_c F_i^{\mu} K_i^{(1-\mu)} \tag{1}$$

where Y is Output produced by firm i by combining flexible inputs F_i and fixed capital K_i. Flexible inputs represent a combination of labor *and* material inputs, with μ representing the share of flexible inputs in production. ζ_i is a firm-specific productivity shifter affected by exposure to supply chain disruptions. A_c is a local area productivity shifter that is determined by both regional linkages and agglomeration forces and specified as $A_c = f(F_c)$.[1]

Previous frameworks (e.g. Bernstein et al. (2019)) highlight the role of labor in agglomeration economies, while allowing for flexible capital that has no role in regional linkages. Our setting features interactions between small and large firms with a specific focus on input-output linkages, and for that reason, firm-level demand for material inputs and final goods (which might include end-use product types such as industrial supplies, capital, and consumer goods) will play a key role in driving spillovers between geographically proximate firms. As our empirical framework leverages disruptions to global supply chains, felt by the sourcing firm and possibly passed on to spatially connected firms, these inputs reflect the potential detrimental impacts of fracturing a supply chain network on regional productivity as highlighted in Acemoglu et al. (2012); Carvalho et al. (2021); Barrot and Sauvagnat (2016). As in previous agglomeration literature, the flexible inputs aggregate might also capture negative employment spillovers such as a reduction in business synergies between proximate firms (Bernstein et al., 2019).[2] Finally, the agglomeration term also allows that

[1]It is straightforward to include separate material/capital goods and labor terms in the production function, but since both enter the regional shifter it is simpler to combine them.

[2]For example, studies refer to Marshall (1890)'s idea that locational proximity could reduce costs in "people, goods, ideas" (Ellison et al., 2010; Combes et al., 2012).

supply shocks will create multiplier effects as the loss of demand of displaced factors spills over to the local economy, felt through both a reduction in output and productivity (Moretti, 2010; Huber, 2018; Guerrieri et al., 2022; Verner and Gyngysi, 2020).[3]

To highlight the mechanism of this paper, our analysis treats firms as price-takers in factor markets, so that they take local factor prices for these inputs, p_c^F, as given.[4] The profit maximization of firm i is given by:

$$\pi_i = \zeta_i A_c F_i^{\mu} K_i^{(1-\mu)} - p_c^F F_i \tag{2}$$

Firms optimally set F_i so that the first-order conditions (FOCs) set the derivative of profit with respect to each input equal to zero.[5] Factors are paid their marginal product, and we make the further assumption that they do not significantly change between March and August 2020. The resulting firm demand for the flexible input is:

$$\log F_i = \frac{1}{(1-\mu)} \log(\zeta_i) + \frac{1}{(1-\mu)} \log(A_c) + \kappa \tag{3}$$

The first term represents firm-specific productivities, while the second term reflects county-level aggregates taken as given by the firm and the last term is a combination of constants and the local factor prices.[6]

The disruption to a firm's trade route network can be interpreted as a productivity shock as firms face higher costs, or even an inability, to source their typical supplies (at least as reflected in the previous year trade patterns). Firms exposed to route disruptions will face a productivity shock equal to $\frac{d\zeta_i}{CovidExposure_i}$. For expositional purposes, if only one firm is exposed to the pandemic disruption (the firm is notated by "*exposed*"), then the direct impact of the shock to factor demand of the firm will be:

$$d \log F_{exposed} = \frac{1}{(1-\mu)} \frac{d\zeta_{exposed}}{CovidExposure_{exposed}} < 0, \tag{4}$$

Assuming everything else held constant, $\zeta_{exposed}$ decreases with the level of exposure (defined below), resulting in a reduction in demand for inputs.

The expression in (4) represents the first hypothesis we bring to the data:

Hypothesis 1: Firms facing greater Covid exposure through supplier route disruptions have lower imports.

[3] One mechanism highlighted in this literature that is especially relevant for aggregate productivity is a reduction in productivity-enhancing investment, as suggested in Queralto (2020), Duval et al. (2019), and Anzoategui et al. (2019). We will show that "Exposure" to Covid disruptions, through a negative productivity shock, reduces the firm demand for materials (and labor).

[4] Note that we model intermediate inputs (along with labor) as one "aggregate" good, while obviously firms face various prices for their various inputs. One might interpret this price as reflecting the average price of a bundle of inputs. The average price could be micro-founded with a structural model of sourcing as in Antràs et al. (2017), Halpern et al. (2015), and Blaum et al. (2018).

[5] There is only one flexible factor, so the FOC is simply: $\frac{d\pi_i}{dF_i} = \mu f_i A_c F_i^{\mu-1} K_i^{1-\mu} - p_c^F = 0$.

[6] Specifically, the last term is given by: $\kappa = \frac{1}{(1-\mu)} \log \mu + \log K_i - \frac{1}{1-\mu} \log p_c^F$.

Import demand is treated as a proxy for the severity of the shock, or the loss of production for the firm. It is an outcome available we can track in real-time and at a high frequency during the height of the pandemic with our detailed bill-of-lading data. [7]

More importantly, this simple framework motivates how the overall firm demand also includes county-level linkages and local spillover forces, determined by A_c. Let $A_c = F_c^{\lambda_c}$, where λ_c is elasticity of county productivity due to a change in local demand for flexible inputs. By construction, $F_c = \sum_i F_i$.[8] For expositional purposes, we assume there is only one other firm, an SME without direct import exposure. We follow Bernstein et al. (2019) in expressing spillovers by the indirect impact on factor demand to a non-exposed firm (with no change in ζ_i) as:

$$d\log F_{k\neq exposed} = \frac{\lambda_c}{(1-\mu)} \frac{d\zeta_{exposed}}{CovidExposure_{exposed}} < 0, \tag{5}$$

where we have substituted A_c in the present example where the direct effect of the shock is to reduce factor demand in the one firm and we ignore endogenous factor price changes. Spillovers exist if $\lambda_c > 0$, in which case equation (5) makes clear that negative supply shocks include an indirect effect on both exposed and non-exposed firms in addition to the direct effect.

As large companies depend on smaller business as both consumers and suppliers, its supply shock likely spills over to their network of SMEs and feeds back as a demand shock as well. The role of PPP is to reduce the direct impact on SMEs, akin to a positive productivity shock concurrent with the supply disruptions, so that the direct effect will look like: $d\log F_k = \frac{1}{(1-\mu)} \frac{d\zeta_k}{PPP_k} > 0$. Continuing with the stylized example, the large importer not receiving PPP (typical of what we observe in our trade data), would face the aggregate effect:

$$d\log F_{exposed\neq PPP} = \underbrace{\frac{1}{(1-\mu)} \frac{d\zeta_{exp}}{CovidExposure_{exp}}}_{DirectEffect} + \underbrace{\lambda_c d\log F_c}_{IndirectEffect},$$

Through the indirect effect, import demand falls by less the smaller is the reduction in F_c, as we expect to be the case in high PPP counties (given the supply disruption). Furthermore, in the presence of spillovers, an equivalent injection of PPP will more greatly alleviate the negative shock the larger is λ_c. Therefore, as our second hypothesis we have:

[7] There may also be price effects through the endogenous changes in input costs and wages (both reflected in p_c^F) that also enter firm input demand. However, given the short-run nature of our study, we assume that wages or inputs costs are unlikely to significantly impact firm decisions beyond what is already captured by COVID exposure. We attempt to control for factor prices with county-month unemployment rates and small business revenue.

[8] Clearly, aggregate demand for factors is captured by summing over firm-level demand, but the aggregation of materials typically requires a functional form for how firms combine different inputs. We are agnostic over the functional form of this aggregation. As long as there is a monotonic relationship, the direct effect of disruption to the sourcing of one firm will be to lower the aggregate material demand. The simplest case reflects simply summing over all flexible inputs as Gathman et al. (2020) do for employment.

Hypothesis 2: Imports of firms facing greater Covid exposure are less affected when the firms are located in counties with greater PPP disbursements.

Empirically, we test the second *hypothesis* in two ways. The first comparison is on the import demand of firms with the same supply chain exposure but in counties that receive differing levels of support from PPP, where county-month fixed effects control for concurrent shocks due to the ongoing pandemic.[9] In the presence of spillover effects, where $\lambda_c > 0$, non-recipients of PPP loans are expected to benefit from the positive productivity change of the PPP recipients.[10] Second, we compare the effects of PPP across counties differentiated by the expected presence of linkages between small and large firms (proxied with regional measures). This reflects variation in λ_c as indirect spillovers increase with this parameter. Section C tests the positive association between agglomeration economies and the size of the PPP benefits. Finally, as a robustness check we can replace the PPP benefits with the supply shock of *other firms in the same county* and show in this case how negative spillovers operate through the same channels.

B COVID Disruptions and Imports by Product Type

We explore the heterogeneous effects of COVID disruption on imports across different types of products. We obtain the crosswalk from the US Census[11] and link each HS-6 product code in our data to a End-use category. The Census end-use codes can be aggregated into six main categories: 1) Foods, feeds, and beverages, 2) Industrial supplies and materials, 3) Capital goods, except automotive, 4) Automotive vehicles, parts, and engines, 5) Consumer goods, and 6) Other goods.

The main effects of *COVID Exposure* on $\Delta Imports^{Nbr}$ for each of the product types are reported in panel B of Table A5. All regressions contain firm, county-month, and product fixed effects. The results suggest that the disruption is felt across the board, in Industrial supplies and materials, Capital goods, and Consumer goods. A one standard deviation increase in the respective products' *Covid Exposure* reduces the number of import transactions by: 1.9 percentage points in Industrial supplies and materials; 3.3 percentage points for Capital Goods (except automotive); and 2.6 percentage points for Consumer goods. The impact on Foods, feeds, and beverages, as well as the "Other" category are weaker, again consistent with the country-industry level import changes found by Berthou and Stumpner (2022).

We find similar results removing HS products that include personal protective equipment

[9] During this period local economies are hit by multiple negative shocks that reduced local employment. Our identification assumption is that the exposure to changes in PPP benefits, instrumented by local branching networks, is not correlated with the severity of these shocks.

[10] We can match the names of firms in the import data to the PPP recipients data in order to test whether firms that did not receive PPP – which is the majority of importers as these tend to be larger firms – benefited indirectly through the spillover channel we highlight in this section. A more obvious results is that a higher level of PPP leads to higher import growth among recipients with equal exposure, which we also confirm.

[11] The crosswalk is directly available at https://www.census.gov/foreign-trade/reference/codes/index.html#enduse.

such as face masks, which account for many new imports in 2020. Results are almost identical without these products, which is not surprising since most of the suppliers of these masks were de-novo entrants (at least in the trade database) in 2020 and were not in the data set in the previous years.

C Import Growth and Covid Exposure - Robustness Checks

In this section, we discuss a number of robustness checks of the main specification used in section 4.1 of the paper. First, we amend the construction of the supply shock to alleviate concerns about the possibility that the change in total route transactions in 2020 might be correlated with pandemic-related demand shocks experienced by specific buyers. For example, a large negative demand shock in Los Angeles (LA) might be felt in specific routes that serve primarily LA buyers and suppliers that rely on these routes.[12] We mitigate this effect by leveraging disruptions on the *port of lading* only. Specifically, in the construction of the supply shock, we replace the route with the port of lading (POL), now regressing $\Delta\log(Supply_{j,p,k,t})$ on $\Delta\log(Transactions - POL_{p,t}^{-j})$ and the same fixed effects. Therefore, we capture disruptions at the supplier origin, which might be a more natural measure of the pandemic's effect, and do not capture effects in the US destination port. Notice that with supplier fixed effects we now estimate this effect only within suppliers that operate from multiple ports (which is more restrictive than the baseline procedure where suppliers operate multiple routes). Due to the higher restrictions placed on the data we only use this specification as robustness, but show in Table A6 that our results hold. Panel A repeats the specifications in Table 2 but with route transaction at the port of lading level; Panel B shows the summary statistics of *COVID Exposure* under this setting; Panel C reports the corresponding disruptive effects of *COVID Exposure* on imports. We report both the effect of the aggregate port disruptions on individual suppliers, and the respective Covid exposure effect on US importers.

Our second robustness exercise is based on the identification of US buyers. As covered in the data section above, Panjiva lists the name of the importing firm in its database, but we can also link it to its parent firm through Capital IQ. One might worry that the listed importers are small subsidiaries of the parent, or an intermediary being used to import. For that reason, we also aggregate the import data to the parent level and re-estimate equation 9 with total parent imports linked to their supply shock. Results are presented in Table A7, and it is clear that aggregating subsidiaries to their parent level has very little impact on the estimated *COVID Exposure* effect on imports.

Tables A8 and A9 check that the main interaction in Table III results are robust to several

[12] Our baseline analysis attempts to control for this with supplier fixed effects. Suppliers to Los Angeles might use several routes, for example they could ship to the port of Los Angeles or Long Beach, where the port of Los Angeles experiences a greater reduction in volume (`https://www.maritime-executive.com/article/differing-results-long-beach-los-angeles-as-covid-19-impacts-shipping`). Suppliers to the LA port in this example, and their buyers, experience a larger negative shock.

other tests for confounding factors. First, we re-estimate Table 3 using regular small business loans (SBL) - the amount of small business loans of each commercial and saving banks from the Loans to Small Business and Small Farms Schedule of the Call Reports - in place of PPP. As reported in columns 1 (PPP instrumented by *PPPE*) and 5 (PPP instrumented by *CB Share*) of Table A8, we find no effect on firm resiliency to the Covid supply shock. When we include Covid Exposure x PPP along with Covid Exposure x SBL, we find only the PPP interaction coefficient to be significant. Second, in columns 2,3,6 and 7 of Table A8, we control for Covid Exposure x Social Capital. We use two different measures of social capital - the rate of completion of the Census form by the population in the county following Knack (2002) and the voter turnout rate in the 2020 presidential election from Barrios et al. (2021). The voter turnout measure is aggregated to the county level by averaging voter turnout across all the precincts within the same county, using 2020 general election data from the New York Times and is computed by taking the ratio of total votes to the voting-age population reported by the U.S. Census. All our results hold controlling for different measures of social capital.

Finally, Fahlenbrach et al. (2021) suggest that firms' financial structure affects resilience, and this might be correlated with the local banking structure that facilitated fast access to PPP. To test this, we add to the specification used in the last two columns Table 3, where the regressions included interactions of Covid Exposure x PPPE (or CB Share). In Table A8 we also control for Covid Exposure x HM Financial Index which is the county-level average of firms' financial constraints index from Hoberg and Maksimovic (2015). As seen in columns 4 and 8, adding the interaction term does not alter our results on PPP.

Table A9, we repeat our specification in Table 3 controlling for the log of non-maritime imports (at the state-HS level from the Census) and its interaction with Covid exposure and find all our results to hold, suggesting that our results are not driven by non-maritime trade confounding the effect of PPP loans on large firms.

D Local Agglomeration Linkages

Chinitz Measure. To create the *Chinitz* Index, we use information from the Input-Output table provided by the Bureau of Economic Analysis combined with the 2018 Business Dynamics Statistics (BDS) provided by the U.S. Census:

$$Chinitz_{h,c} = \sum_{l=1,\cdots,L} \frac{Firms_{l,c}}{E_c} Input_{h \leftarrow l} \tag{6}$$

where $Firms_{l,c}$ represents the number of firms in industry l in county c, $E_{l,c}$ is the employment in industry i within county c directly available from 2018 BDS Data, while $Input_{h \leftarrow l}$ is the share of industry h's inputs that come from industry l. Thus the index essentially calculates the average firm size in county c in industries that typically supply a given industry h. Higher values of the index suggests that businesses source their inputs from a larger variety of suppliers. Since we do not have a reliable industry classification for our importing firms, we aggregate the *Chinitz* index to the county level by taking the average for each industry within the county, weighted by the industry level employment. Notice that this procedure is conducted with the county-industry data and not our trade data. At the county-level, we merge the trade data using the county listed for the business address of the US importers.

Input-Output Linkages. We measure the input-output linkages, $InputOutput$, as follows: First we measure the extent to which each industry receives input from or provides output to the local economies using:

$$Input_{h,c} = \sum_{l=1,\cdots,L} \frac{E_{l,c}}{E_c} Input_{h \leftarrow l} \tag{7}$$

$$Output_{h,c} = \sum_{l=1,\cdots,L} \frac{E_{l,c}}{E_c} Output_{h \rightarrow l} \tag{8}$$

where $Input_{h \leftarrow l}$ and E_c are analogous to what we use in calculating the $Chinitz$ measure, while $Output_{h \rightarrow l}$ is the share of industry h's output purchased by industry l.[13] Second, we calculate the county level $Input_c$ and $Output_c$ by averaging the above two measures over all industries within a county, weighted by the county-level industrial employment. Finally, the county level $InputOutput_c$ is measured as:

$$InputOutput_c = \max\{Input_c, Output_c\}$$

which could be considered as a proxy for the level connectedness over different industrial sectors within a county. After calculating the county level $Chinitz$ and $InputOutput$ measures, each county is assigned to High/Low agglomeration buckets based on whether the measure is above/below the median value for each measure across all counties in our sample.

Share of SMEs in Local Economy. The measure is computed as:

$$\text{SBS}_{\text{E},c} = \frac{N_{emp \leq E,c}}{N_{total,c}} \tag{9}$$

where $N_{emp \leq E,c}$ represents the number of establishments with employment less than $E = \{20, 500\}$ in county c, and $N_{total,c}$ is the total number of establishments in the same county. Further, we assign each county into *High* and *Low* agglomeration buckets as defined by the quartiles of SBS_{20} and SBS_{500}. Specifically, each county will be assigned into $Q_{20(500)} = \{1, 2, 3, 4\}$ if it's $\text{SBS}_{20}/\text{SBS}_{500}$ falls into the ith quartile by each measure. $Q_{20(500)} = 1$ indicates that the county has the smallest share of small/medium enterprises, while $Q_{20(500)} = 4$ indicates that the county has the largest share of small/medium enterprises.

County Employment Diversity. We follow Nakamura and Paul (2019) and proxy agglomeration by industrial employment diversity. We use the inverse of Herfindahl-Hirschman

[13] $Input_{h \leftarrow l}$ and $Output_{h \rightarrow l}$ provide us information on the importance of each industry to the local input-output networks.

Index (HHI) and construct the variable *Diversity* using the 2018 BDS data as follows:

$$Diversity_c = (\sum_h (s^2_{h,c}))^{-1} \tag{10}$$

where $s_{h,c}$ is the employment share in industry h in county c.

The higher value of $Diversity_c$ suggests industries are more evenly distributed with relatively smaller shares within a county. This measure has a history back to Glaeser et al. (1992) and Duranton and Puga (2001), where it is contrasted with *specialized* regions. The former paper argues that diversity is more important for growth, and the latter identifies diverse regions with new and growing industries while mature industries settle in specialized regions.[14] To allow for the possibility of input-output and firm-to-firm linkages *outside of a firm's own industry*, and given that PPP's aim was to limit the failures of SMEs, we hypothesize that diverse regions will be most prone to positive spillover effects.

We use $50^{th}(75^{th})(95^{th})$ percentile values as the cut-off values to assign each county in our sample into a *High* and *Low* agglomeration group. 48.2% of sample firms are located in counties that are ranked above 95th percentile in terms of the *Diversity*, which is consistent with the fact that a large portion of the importing firms are located in the metro areas.

To get at "high" and "low" agglomeration, we split *counties* as being above/below the median, 75th, and 95th percentiles. Since most of our observations are naturally in diversified counties, at the 95th percentile we have about the same number of observations in both sub-samples. Regardless of the cutoff, the positive coefficient on the *Covid Exposure-PPPE* interaction is only present in the "high" agglomeration counties, and the difference between the samples increases with the stringency of the "high" cutoff. As with the other measures, industry diversity proxies for the linkages across firms and sectors. This might be reflected not only in the supply chain networks but in demand multipliers. For example, in the framework of Guerrieri et al. (2022), Keynesian supply shocks that trigger changes in aggregate demand larger than the shock itself is only possible in economies with multiple sectors, so that diversified economies are likely more prone to spillovers.

County Import Distribution. We investigate another agglomeration measure from the literature on productivity sorting. Gaubert (2018) argues that agglomeration externalities disproportionately benefits larger firms, thus endogenously sorting better firms to these localities, making the distribution of firm size fat-tailed. A similar process could be reflected in imports as larger, more productive firms tend to importers (Bernard et al., 2009). Therefore, a thicker tail for firm import distributions within the county should reflect higher levels of agglomeration.

We follow the argument of Gaubert (2018) that the distribution of firm size within the geographic unit is partly determined by agglomeration, as the larger more productive firms are disproportionately benefited by the agglomeration benefits. In that setting, a fatter tail of the productivity distribution indicates larger agglomeration power. As a parallel argument,

[14] In a related result, Rosenthal and Strange (2003) find that diversity encourages new establishment births.

we make use of the distribution of imports across all importing firms within a county, where we use number of imports as the measure of size. As in Gaubert (2018), we estimate the shape parameter of the distribution of imports as a measure of dispersion. We estimate the county level shape parameter of the import distribution following Gabaix and Ibragimov (2011) with the regression:

$$\log(rank_{i,c}) = \alpha_c - \Phi_c \log(Import_i) + \varepsilon_{i,c}$$

where $rank_{i,c}$ is the ranking of *Number of Imports* of firm i among all firms in county c in 2019, while $Import_{i,c}$ is the total number of imports in 2019 for firm i. Φ_c is the shape parameter, with a lower value reflecting a fatter right tail. Each county is ranked into as *Low/Mid/High* tercile agglomeration accordingly.

Counties with a more dispersed distribution are expected to be more exposed to agglomeration forces. In Table A12 we report results with counties split by the shape of the import distribution, in this case by terciles. The coefficients for the interactive terms turns positive significant for the middle and top tercile, while the magnitude is larger for the top tercile, indicating the positive effects of PPP on import growth are most prominent in counties with higher degree of agglomeration as reflected in the sorting of larger importers into the county.

Figure A1: **Geographic Distribution of U.S. Importers in Sample**

The dots reflect the location of importers as reported in their address. Panjiva, as part of its universe of maritime transactions, reports from the Bill of Lading: names/address of importers, their foreign suppliers, volume imported, shipment arrival date, ports (lading and unlading) associated with the transactions, and product code (6-digit HS code (HS6)).

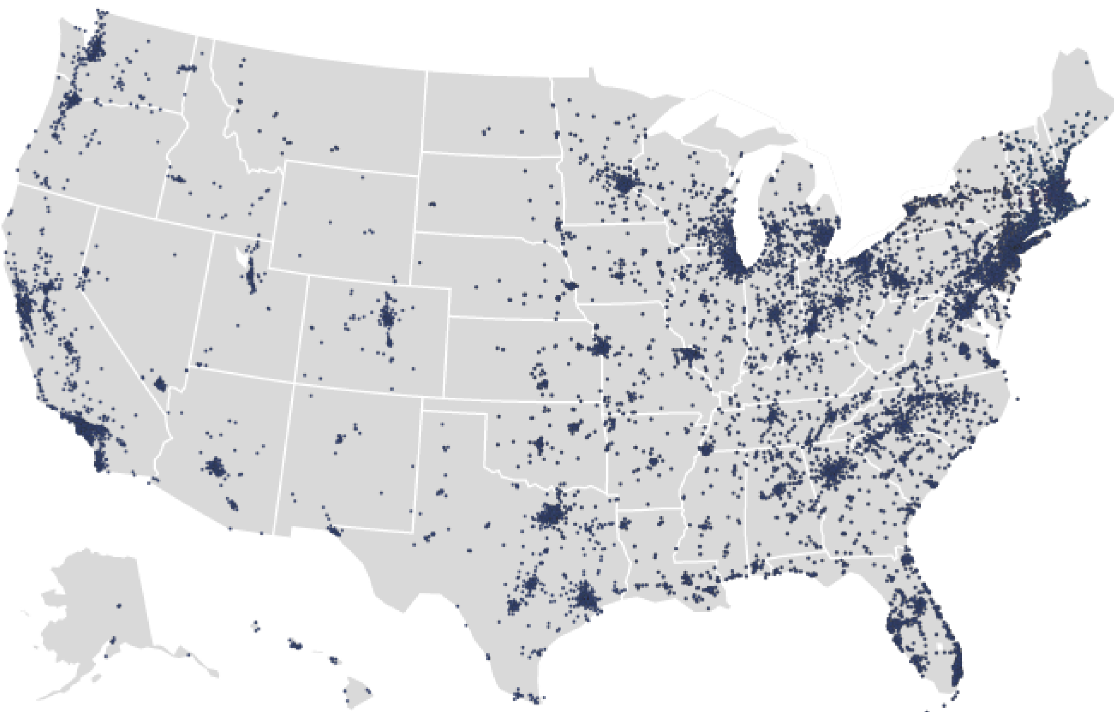

Table A1: **Variable Definition**
This table reports definition of each variable used in this paper.

Variable	Definition	Source
$\Delta Import^{Nbr}_{i,k,t}$	12-mo difference in logarithm values of import, measured by number of transactions	Panjiva
$\Delta Import^{Vol}_{i,k,t}$	12-mo difference in logarithm values of import, measured by volume	Panjiva
$\Delta \log(Supply_{j,r,k,t})$	12-month difference in the logarithm values of total number of transactions for each supplier-route-product at month t	Panjiva
$\Delta \log(RouteTransactions^{-j}_{r,t})$	12-month difference in the logarithm values of total number of transactions during the same route-month excluding the transactions by supplier j	Panjiva
COVID Exposure	Measured firm level COVID Exposure	Panjiva
PPP^{Nbr}	County-month level # of PPP loans normalized by total number of establishment in county	SBA & CBP
UnEmp_r	One-month lagged unemployment rate	Department of Labor
COVID_Case	Monthly confirmed Covid Cases	JHU Coronavirus Resource Center
Chg_SB_Rev	Monthly change of small business revenue	Opportunity Insight
PPPE^{Nbr}	County Exposure to PPP at the 2nd quarter of 2020, measured by number of PPP	SBA, Call Reports & DOS
CB Share	Share of community bank branches at county.	FDIC
$Chinitz$	Index on intensity on number of providers that supply to new entrants.	BDS
$InputOutput$	Index on within county industrial connectedness.	BDS
SBS$_{20}$	Share of small establishments with employment less thatn 20 within the county.	BDS
SBS$_{500}$	Share of small establishments with employment less thatn 500 within the county.	BDS
$Diversity$	Inverse of the Herfindahl-Hirschman Index for county industrial employment.	BDS
SBA Loan	The amount of small business loans of each commercial and saving banks at county level.	FDIC
Census Fill Rate	Number of population filled in Census survey divided by the total population in the same county in 2010.	Census
Voter Participation Rate	The ratio of total votes to the voting-age population in the same county in 2020.	Barrios et al. (2021) & Census
HM Financial Index	Average financial constrain index across all US public firms HQ in the same county.	Hoberg and Maksimovic (2015)
Coastal	Dummy variable equals to 1 if the county located at the coastal line of the US.	NOAA
Border	Dummy variable equals to 1 if the county located at the US border between either Canada or Mexico.	Authors' calculation
Student Loan Default Rate	Student loan default rate at county level.	Urban Data Catalog
30-89 days delinquency rate	Mortgages 3089 days delinquent rate at county level.	CFPB
90 days deliquency rate	Mortgages 90 days delinquent rate at county level.	CFPB
Non-maritime imports	Dollar value on non-maritime imports at each state-month in 2020.	Census.

Table A2: **COVID Disruption to Suppliers**

This table reports estimates from the following regression: $\Delta\log(Supply_{j,r,k,t}) = \beta\Delta\log(RouteTransactions^{-j}_{r,t}) + \mu_{j,k,t} + \nu_{j,r,k,t}$, where $\Delta\log(Supply_{j,r,k,m})$ is the difference in the logarithm values of total number of transactions for each supplier-route-product at month t, and $\Delta\log(RouteTransactions^{-j}_{r,t})$ is the difference in the logarithm values of total number of transactions during the same route-month excluding the transactions by supplier j. The difference is calculated relative to the same month in 2017-2019 (averaged across years) in the first two columns and relative to the same month in 2019 (last two columns). All regressions are estimated using supplier-product-month fixed effects. Standard errors clustered by supplier are reported in parentheses. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3	4
12-mo difference	2020 and 2017-2019 monthly average		2020 and 2019	
	$\Delta\log(Supply_{j,r,k,t})$			
	Transactions	Volume	Transactions	Volume
$\Delta\log(RouteTransactions^{-j}_{r,t})$	0.117***	0.117***	0.168***	0.173***
	(0.011)	(0.008)	(0.017)	(0.012)
Firm-HS-Month FE	Y	Y	Y	Y
N	246373	246636	153626	153818
F-Statistics	106.44	204.21	100.46	204.60
Adj-R sq	0.067	0.076	0.066	0.072

Table A3: **Falsification Tests**

The falsification test reports estimates from the following regression:

$$COVID\ Exposure_c = \alpha + \beta X_c + \varepsilon_c$$

COVID Exposure is the average disruption across all firms in county c, done separately for March and April. Covid Exposure is constructed at the importer-level as in the main text, then aggregated to the county-level for only March (first column) and April (second column). X_c is a set of county level descriptors including the level of population and its density; GDP per capita; two measures of the share of small businesses in all firms (share of businesses with less than 20 and 500 workers); a dummy for being in a coastal state and a dummy for being a dummy that shares a border with Mexico or Canada; the number of nursing homes; racial diversity; changes in small business revenue; case counts in that concurrent month; the unemployment rate in that month; 30-89 days and 90-day mortgage delinquency rates from aggregate CFPB data; and student loan default rates at the county level from Urban Data Catalog at Urban Institute. For any descriptors that can be time-varying, we use the value in March and April 2020. Robust standard errors are reported in parentheses. Note that the number of observations holds for all variables except GDP per capita (which is missing for 22 counties). (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3	4
	COVID Exposure			
	March		April	
Log(Population)	0.0008	0.0008	0.0001	0.0001
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Population Density	-0.0000	-0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Log(GDP per Capita)	0.0005	0.0005	0.0005	0.0005
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Small Business Share ($emp <= 20$)	-0.0005	-0.0004	-0.0006	-0.0007
	(0.0036)	(0.0036)	(0.0032)	(0.0032)
Small Business Share ($emp <= 500$)	-0.0010	-0.0010	-0.0012	-0.0012
	(0.0025)	(0.0025)	(0.0023)	(0.0023)
Coastal	0.0011	0.0011	0.0008	0.0008
	(0.0008)	(0.0008)	(0.0007)	(0.0007)
Border County	-0.0000	-0.0000	-0.0004	-0.0004
	(0.0016)	(0.0016)	(0.0014)	(0.0014)
Log(Number of Nursing Homes)	-0.0005	-0.0005	-0.0007	-0.0007
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Racial Diversity	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Chg_SB_Rev	0.0004	0.0004	-0.0005	-0.0005
	(0.0006)	(0.0006)	(0.0005)	(0.0004)
Log(Cases)	-0.0001	-0.0001	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
UnEmp_r	0.0000	0.0000	0.0003*	0.0003*
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
3089 days delinquency rate	-0.0003		0.0005	
	(0.0005)		(0.0006)	
90 days delinquency rate		-0.0009		0.0009
		(0.0010)		(0.0010)
Student Loan Default Rate	-0.0032	-0.0026	0.0017	0.0015
	(0.0104)	(0.0105)	(0.0095)	(0.0094)
N	1216	1216	1310	1310

Table A4: **Relationship between $\Delta Import$ and PPPE**
This table reports estimates from the following regression:

$$\Delta Import_c^{Nbr} = \alpha \cdot PPPE_c^{Nbr} + \beta \cdot COVID\ Exposure_c + \mathbf{X}_c + \varepsilon_c$$

where $\Delta Import_c^{Nbr}$ are the average 12-mo difference in logarithm values of import for product across all firms at county c, measured by *Number of Transactions*. Since the goal is to test whether PPP receipts are larger in counties with larger supply shocks, the 12-mo import differences are done for only March and April (separately in each column). We repeat the specification for employment growth as an outcome – the percent change of monthly employment relative to January. $PPPE_c^{Nbr}$ is the time-invariant PPP exposure at county c (which reflects PPP success from April to August). $\mathbf{X}$ is a vector of control variables. Each month contains estimation results with and without control variables. Standard errors clustered by county are reported in parentheses. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3	4	5	6	7	8
	$\Delta Import_c^{Nbr}$				Employment Growth			
	March		April		March		April	
$PPPE_c^{Nbr}$	0.081	0.091	0.142	0.146	-0.001	-0.000	-0.005	-0.003
	(0.086)	(0.086)	(0.104)	(0.104)	(0.002)	(0.002)	(0.015)	(0.014)
COVID Exposure		-0.813		-4.166		0.038		-0.102
		(3.910)		(3.422)		(0.137)		(0.511)
Chg_SB_Rev		-0.013		0.030		0.000		0.010**
		(0.038)		(0.026)		(0.001)		(0.004)
COVID Exposure X Chg_SB_Rev		1.635		-2.286		0.033		0.607**
		(1.977)		(1.455)		(0.042)		(0.251)
Log(COVID_Case)		-0.006		-0.001		-0.000***		-0.004***
		(0.006)		(0.008)		(0.000)		(0.001)
COVID X Log(COVID_Case)		0.247		-0.640		0.002		-0.007
		(0.297)		(0.569)		(0.007)		(0.064)
UnEmp		0.013		-0.002		0.001		-0.001
		(0.013)		(0.010)		(0.001)		(0.001)
COVID X UnEmp		0.227		0.865		-0.001		0.122
		(0.761)		(0.570)		(0.032)		(0.075)
N	1216	1216	1310	1310	1216	1216	1310	1310
Adj-R sq	0.000	0.002	0.002	0.011	0.001	0.053	0.001	0.125

Table A5: **COVID Disruption and Import Growth by Product Type**

	1	2	3	4	5	6
	$\Delta Import^{Nbr}_{i,k,t}$					
Census Enduse Product Type	Food, feeds, beverage	Industrial supplies and materials	Capital goods, except automotive	Automotive vehicles, parts, and engines	Consumer goods	Other goods
COVID Exposure	-0.487	-1.167***	-1.700***	-0.781	-1.623***	-0.162
	(0.369)	(0.272)	(0.294)	(0.661)	(0.300)	(1.405)
Firm FE	Y	Y	Y	Y	Y	Y
HS FE	Y	Y	Y	Y	Y	Y
County-Month FE	Y	Y	Y	Y	Y	Y
N	30160	59326	48257	12317	56474	3706
Adj-R sq	0.119	0.114	0.145	0.197	0.141	0.212

Table A6: **Robustness: COVID Disruption and Import Growth with Alternate Route Definition**
We estimate the following regression in the panel A of this table:

$$\Delta \log(Supply_{j,p,k,t}) = \beta \Delta \log(Transaction - POL_{p,t}^{-j}) + \mu_{j,k,t} + \nu_{j,r,k,t}$$

where $\Delta \log(Supply_{j,p,k,t})$ is the 12-month difference in the logarithm values of total number of transactions for each supplier-port of lading (POL)-product at month t, and $\Delta \log(Transaction - POL_{r,t}^{-j})$ is the 12-month difference in the logarithm values of total number of transactions during the same POL-month excluding the transactions by supplier j. Notice that we now capture only variation within suppliers that ship from multiple ports of lading. Standard errors clustered by supplier are reported in parentheses. All regressions are estimated using supplier-product-month fixed effects. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.
Panel B reports the summary statistics of the *COVID Exposure* estimated using *Port of Lading.*
Panel C estimates the following regression:

$$\Delta Import_{i,k,t} = \beta \cdot \text{COVID Exposure - } POL_{i,k,t} + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t}$$

$\Delta Import_{i,k,t}$ is the 12-mo difference in logarithm values of import for product k at firm i in month t, measured by *Number of Transactions* and *Volume*. *COVID Exposure- POL* is the COVID Exposure experienced by the same firm-product in same month. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

Panel A: COVID Exposure and Import: Supplier Shocks

	1	2	3	4
12-mo difference	2020 and 2017-2019 monthly average		2020 and 2019	
	$\Delta \log(Supply_{j,r,k,t})$			
	Transactions	Volume	Transactions	Volume
$\Delta \log(Transaction - POL_{r,t}^{-j})$	0.145***	0.136***	0.187***	0.151***
	(0.022)	(0.021)	(0.038)	(0.031)
Firm-HS-Month FE	Y	Y	Y	Y
N	113584	114350	71414	71928
Adj-R sq	0.072	0.076	0.070	0.073

Table A6: **Robustness: COVID Disruption and Import (Continued...)**

Panel B: Summary Statistics of COVID Exposure measured from Port of Lading

	N	Mean	S.D.	Min	P25	P50	P75	Max
COVID Exposure - POL	293337	0.015	0.024	-0.257	0.001	0.008	0.024	0.493

Panel C: COVID Exposure and Import: Baseline Results

	1	2	3	4
	$\Delta Import_{i,k,t}$			
	$\Delta Import^{Nbr}_{i,k,t}$		$\Delta Import^{Vol}_{i,k,t}$	
COVID Exposure - POL	-2.090***	-2.051***	-1.586***	-1.554***
	(0.085)	(0.087)	(0.087)	(0.089)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
N	275441	273716	275441	273716
Adj-R sq	0.120	0.116	0.128	0.122

Table A7: **Robustness: COVID Disruption and Import Growth Aggregated to the Parent Level**
In the following table we replicate the specification in Table II, but aggregate the importing data to the parent level using each subsidiaries' Capital IQ identification. It reports estimates from the following regression:

$$\Delta Import_{i,k,t} = \beta \cdot COVID\ Exposure_{i,k,t} + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t}$$

where firm i is now defined as a parent as identified from Capital IQ. *COVID Exposure* is the COVID Exposure experienced by the same firm-product in same month. Cols. 1 and 2 and cols. 3 and 4 report when *Import Difference* are measured by *Number of Transactions* and *Volume* respectively. Firm, product, and state-month fixed effects are used in cols 1 and 3; firm, product, and county-month fixed effects are used in cols 2 and 4. Standard errors clustered by firm are reported in parentheses. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3	4
	$\Delta Import^{Nbr}_{i,k,t}$		$\Delta Import^{Vol}_{i,k,t}$	
COVID Exposure - Parent	-1.407***	-1.360***	-1.436***	-1.388***
	(0.150)	(0.154)	(0.155)	(0.161)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
N	168299	166596	168299	166596
Adj-R sq	0.109	0.111	0.113	0.114

Table A8: **Does PPP Foster Resiliency to Supply Disruption? Robustness Checks**

This table reports estimates from the following regression:

$$\Delta Import^{Nbr}_{i,k,t} = \beta \cdot COVID\ Exposure_{i,k,t} + \theta COVID\ Exposure_{i,k,t} \times PPP_c + \theta COVID\ Exposure_{i,k,t} \times C_c + \delta X_{i,t} + \xi_i + \eta_k + \kappa_{c(s),t} + \varepsilon_{i,k,t}$$

where $\Delta Import^{Nbr}_{i,k,t}$ are the 12-mo difference in logarithm values of import for product k at firm i in month t, measured by Number of Transactions. PPP is measured by *PPPE* in columns 1-4, and is measured by *CB Share* in columns 5-8. C_c includes the following county level variables: SBA Loan, Census Fill Rate, Voter Participation Rate, and HM Financial Index in columns 1(5), 2(6), 3(7), 4(8). $X_{i,t}$ is a set of interactions where we interact the time-varying county-level control variables with the COVID Exposure. Firm, product, and county-month fixed effects are used across all columns. Standard errors clustered by county are reported in parentheses. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significant at 1%, 5%, and 10% levels respectively.

	1	2	3	4	5	6	7	8
DV					$\Delta Import^{Nbr}_{i,k,t}$			
COVID Exposure	-2.769***	-1.640	-2.175***	-2.626***	-3.048***	-1.850	-2.458***	-2.893***
	(0.561)	(2.179)	(0.626)	(0.542)	(0.601)	(2.177)	(0.650)	(0.581)
COVID Exposure X PPPE	4.127***	4.092***	4.103***	4.092***				
	(1.492)	(1.484)	(1.485)	(1.488)				
COVID Exposure X CB Share					1.396***	1.378***	1.370***	1.376***
					(0.405)	(0.402)	(0.401)	(0.403)
COVID Exposure X SBA Loan	0.157				0.163			
	(0.176)				(0.175)			
COVID Exposure X Census Fill Rate		-1.246				-1.319		
		(2.774)				(2.783)		
COVID Exposure X Voter Participation Rate			-1.159				-1.102	
			(0.993)				(0.996)	
COVID Exposure X HM Financial Index				0.664				0.693
				(3.210)				(3.268)
COVID X Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
HS FE	Y	Y	Y	Y	Y	Y	Y	Y
County-Month FE	Y	Y	Y	Y	Y	Y		
N	163144	163144	163144	163144	163144	163144	163144	163144
Adj-R2	0.128	0.128	0.128	0.128	0.128	0.128	0.128	0.128

Table A9: **Controlling for non-maritime trade**

This table reports estimates from the following regression:

$$\Delta Import^{Nbr}_{i,k,t} = \beta \cdot COVID\ Exposure_{i,k,t} + \theta COVID\ Exposure_{i,k,t} \times PPP_c + \theta COVID\ Exposure_{i,k,t} \times C_c + \gamma COVID\ Exposure_{i,k,t} \times Non\!-\!Maritime\ Imports_{s,t} + \delta X_{i,t} + \xi_i + \eta_k + \kappa_{c(s),t} + \varepsilon_{i,k,t}$$

where $\Delta Import^{Nbr}_{i,k,t}$ are 12-mo difference in logarithm values of import for product k at firm i in month t, measured by *Number of Transactions*. $PPP^{Nbr}_{c,(t)}$ includes: 1) the 1-month lagged PPP per establishment (PPP) at month t in cols.1-4, 2) exposure to PPP ($PPPE$) which is time-invariant as it captures all PPP receipts in the second quarter of 2020 in col.5, and 3) share of community banks for county c at the 2nd quarter of 2020 (also time-invariant) in col.6. *Non-maritime Imports* represents the log value of non-maritime imports from within the same state. $X_{i,t}$ is a set of interactions where we interact time-varying county-level control variables with the *COVID Exposure*. Firm, product, and state-month fixed effects are used in cols 1-3, and firm, product, and county-month fixed effects are used in cols 4-6. Standard errors clustered by county are reported in parentheses. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3	4	5	6
	$\Delta Import^{Nbr}_{i,k,t}$					
PPP Measure	PPP				PPPE	CB Share
COVID Exposure	-1.473***	-1.287***	-1.934***	-1.882***	-2.291***	-2.557***
	(0.127)	(0.275)	(0.535)	(0.561)	(0.601)	(0.641)
PPP	0.034	0.002	-0.001			
	(0.055)	(0.057)	(0.058)			
COVID Exposure X PPP		1.957**	2.355**	2.181**	4.027***	1.337***
		(0.800)	(0.950)	(0.998)	(1.502)	(0.411)
Non-maritime Import, log	-0.001	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
COVID Exposure X Non-maritime Import, log		-0.042*	-0.042*	-0.036	-0.036	-0.034
		(0.023)	(0.024)	(0.024)	(0.024)	(0.024)
Chg_SB_Rev			-0.001			
			(0.009)			
COVID Exposure X Chg_SB_Rev			0.086	0.214	-0.135	-0.094
			(0.222)	(0.233)	(0.198)	(0.190)
Log(Covid_Case)			-0.006			
			(0.006)			
COVID Exposure X Log(Covid_Case)			0.015	0.034	0.007	0.022
			(0.060)	(0.062)	(0.062)	(0.064)
UnEmp_r			-0.002			
			(0.002)			
COVID Exposure X UnEmp_r			0.057***	0.048**	0.054**	0.055**
			(0.022)	(0.022)	(0.022)	(0.022)
Firm FE	Y	Y	Y	Y	Y	Y
HS FE	Y	Y	Y	Y	Y	Y
State-Month FE	Y	Y	Y			
County-Month FE				Y	Y	Y
N	165022	165022	165022	163144	163144	163144
r2_a	0.134	0.134	0.134	0.128	0.128	0.128

Table A10: **Alternative Outcome: Local Employment**

Local Employment: This table reports estimates from the following regression: $Emp_{c,t} = \beta COVID\ Exposure_{c,t} + \gamma PPPE_c + \theta COVID\ Exposure \times PPPE_c + \delta X_c + \lambda_{s,t} + \varepsilon_{c,t}$, where Emp is the relative percentage change of monthly employment to January for county c at month t. PPP_c is the exposure to PPP ($PPPE$) at 2nd quarter of 2020 for county c. $X_{i,t}$ is a set of interactions where we interact the time-varying county-level control variables with the *COVID Exposure*. All regressions are estimated using state-month fixed effects.
(***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3	4	5
			$\Delta Emp_{t,Jan}$		
Chg_SB_Rev	0.003***	0.003***	0.003***	0.003***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(COVID_Case)	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
UnEmp	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
COVID Exposure		-0.152***	-0.150***	-0.211***	-0.224**
		(0.037)	(0.037)	(0.048)	(0.099)
PPPE			0.045***	0.035***	0.035***
			(0.004)	(0.006)	(0.006)
COVID X PPPE				0.628**	0.663**
				(0.318)	(0.318)
COVID X Chg_SB_Rev					0.111***
					(0.037)
COVID X Log(COVID_Case)					-0.004
					(0.013)
COVID X UnEmp					0.013
					(0.008)
State-Month FE	Y	Y	Y	Y	Y
N	8924	8924	8924	8924	8924
Adj-R sq	0.593	0.594	0.601	0.601	0.601

Table A11: **PPP and Agglomeration: County Level Employment Diversity**

Cols 1-2(3-4)(5-6) use 50(75)(95) percentile of county level diversity as the cut off to split high/low diversified counties. To get at "high" and "low" agglomeration, we split *counties* as being above/below the median, 75th, and 95th percentiles. Since most of our observations are naturally in diversified counties, at the 95th percentile we have about the same number of observations in both sub-samples. Regardless of the cutoff, the positive coefficient on the *Covid Exposure-PPPE* interaction is only present in the "high" agglomeration counties, and the difference between the samples increases with the stringency of the "high" cutoff. As with the other measures, industry diversity proxies for the linkages across firms and sectors. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3	4	5	6
	$\Delta Import^{Nbr}_{i,k,t}$					
Diversity percentile cutoff	50		75		95	
	Low	High	Low	High	Low	High
COVID Exposure	-1.008	-3.330***	-1.449*	-3.186***	-1.509**	-3.846***
	(0.959)	(0.691)	(0.854)	(0.763)	(0.731)	(0.910)
COVID Exposure X $PPPE^{Nbr}_c$	3.026	5.366***	3.711	5.115***	3.644	6.936***
	(3.187)	(1.637)	(2.783)	(1.661)	(2.260)	(1.955)
Firm FE	Y	Y	Y	Y	Y	Y
HS FE	Y	Y	Y	Y	Y	Y
County-Month FE	Y	Y	Y	Y	Y	Y
COVID Exposure X Control	Y	Y	Y	Y	Y	Y
N	46670	116124	57386	105388	79367	83396
Adj-R sq	0.126	0.136	0.120	0.140	0.122	0.144

Table A12: **PPP and Agglomeration: County Import Distribution**

In cols 1-3, we report results for sub-sample of counties that rank in the bottom to top tercile of the ζ measure described in Internet Appendix D. All regressions are estimated using firm, product, and county-month fixed effects. Standard errors clustered by county are reported in parentheses. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	1	2	3
		$\Delta Import^{Nbr}_{i,k,t}$	
Φ Tercile	Bottom 1/3	Middle 1/3	Top 1/3
COVID Exposure	-2.826***	-2.408**	-2.549***
	(0.898)	(1.090)	(0.881)
COVID Exposure X $PPPE^{Nbr}_{c}$	3.680	4.118*	6.372**
	(3.452)	(2.482)	(2.557)
Firm FE	Y	Y	Y
HS FE	Y	Y	Y
County-Month FE	Y	Y	Y
COVID Exposure X Control	Y	Y	Y
N	54787	54661	52980
Adj-R sq	0.132	0.139	0.143

Table A13: **Robustness: COVID Disruption and Spillovers**
In the following table we report estimates from the following regression:

$$\Delta Import_{i,k,t} = \beta \cdot COVID\ Exposure_{i,k,t} + \phi Other\ COVID\ Exposure_{-i,c,t} + \theta COVID\ Exposure_{i,k,t} \times Other\ COVID\ Exposure_{-i,c,t} + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t}$$

COVID Exposure is the COVID Exposure experienced by the same firm-product in same month. *Other COVID Exposure* is the average *COVID Exposure* for all other firms in the same county as the focal firm. Cols. 1 and 2 report when *Import Difference* are measured by *Number of Transactions*. Firm, product, and state-month fixed effects are used in cols 1; firm, product, and county-month fixed effects are used in cols 2. Standard errors clustered by firm are reported in parentheses. All variables are defined in the Variable Appendix. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	$\Delta Import^{Nbr}_{i,k,t}$	
COVID Exposure	-0.981***	-0.966***
	(0.216)	(0.220)
County Average COVID Exposure Exclude Focal Firm	-0.023	0.043
	(0.284)	(0.290)
COVID Exposure X County Average COVID ExposureExposure	-13.417***	-13.842***
	(4.830)	(4.948)
	(4.830)	(4.948)
Firm FE	Y	Y
HS FE	Y	Y
County-Month FE	Y	Y
N	225079	224822
Adj-R sq	0.129	0.124

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