# Firm Resiliency: The Role of Spillovers

# Abstract

Using high-frequency data on over 7 million import transactions, we study the disruptions to US firms' trade patterns and growth immediately following the initial COVID-19 trade shock. While large firms are not direct recipients of government fiscal support, they see fewer disruptions if located in counties where small businesses (SMEs) receive government stimulus loans under the Paycheck Protection Program. These effects are largest in counties with greater share of SMEs and stronger input-output linkages between large firms and SMEs. Our results point to local spillovers between SMEs and large firms as being an important determinant of firm resiliency during crises.

# 1 Introduction

Firm resilience and recovery is once again at the forefront of academic research and policy debate with the unprecedented economic disruption brought about by the COVID-19 pandemic. In analyzing the drivers of firm recovery, most of the existing studies have focused on factors internal to the firm such as financial structure (e.g. Levine et al. (2020), Albuquerque et al. (2020)) or firm labor flexibility (e.g. Bai et al. (2021)) rather than firms' external linkages with the local economy. While a large agglomeration literature has shown that firm investment and economic activity are spatially concentrated (e.g. Dougal et al. (2015), Greenstone (2010)) and that local buyer-supplier linkages play an important role in propagating shocks (e.g. Alfaro et al. (2021), Giannetti and Saidi (2019)), less understood is the role played by local linkages in sustaining firms during times of economic crises.

In this paper, we study whether the trade shock due to the Covid-19 pandemic had a differential impact on firms depending on their exposure to regional spillovers arising from large-small firm linkages. To isolate the role of spillovers, we use the context of the Paycheck Protection Program (PPP), one of the largest firm-based fiscal policy programs announced by the U.S. Government that offered guaranteed, forgivable loans to small and mid-sized businesses to provide liquidity and prevent job losses. In particular, we explore whether large importing firms that faced COVID related trade disruptions fared better when located in counties that had large disbursements to small businesses under the PPP.

On the one hand, PPP was designed to aid small businesses keep their workforce employed and we should not expect to see any benefits accruing to large importers who were not direct recipients of PPP loans. On the other hand, there is increasing evidence that PPP increased the survival rates of small businesses (see Bartik et al. (2021), Agarwal et al. (2022), and Gourinchas et al. (2021)), even as the employment effects are debated. Given that mass layoffs and liquidation events are known to have highly localized large negative spillover effects (e.g. Bernstein et al. (2019), Gathman et al. (2020)), one would expect to see spillover effects from the PPP program on large firms which are linked to the smaller PPP-recipients as both suppliers and customers. In particular, we hypothesize that in areas where small firms benefited from PPP and were able to avoid shutdown and maintain activity (Bartlett and Morse (2021), Denes et al. (2021b)), large firms' imports recovered faster than in areas where small firms did not benefit as much from PPP. We also expect this dynamic to be more salient in local economies with a large share of small and medium enterprises, integrated input-output linkages, and a diverse industrial base. In contrast, in areas where there was not much PPP support and small firms exited the market, we would expect greater import disruptions because of potential reduction in demand and loss of business synergies between proximate firms, consistent with the negative spillover effects from local bankruptcies as in Bernstein et al. (2019).

To investigate these hypotheses, we use high-frequency data with detailed information on shippers and importers on the universe of US maritime import transactions (at the HS-6 product level) between March and September 2020, the period that saw the maximum supplier-linked disruption due to the pandemic. This period also overlaps with the first wave of the PPP program between April and August 2020.

We first show that importing firms were indeed impacted by the external supply disruptions. To separate the *supply* disruption to importers from simultaneous local demand effects, we measure the importers' supply exposure as a weighted average of the Covid-related trade disruptions faced by each of its exporting suppliers. The suppliers' disruptions are in turn estimated by trade disruptions along their shipping routes (excluding the supplier's own activity along those shipping routes). The changes in route-level activity capture disruptions caused by pandemic related lockdowns, quarantines, and labor shortages at ports worldwide, as shown by Notteboom et al. (2021) using data on shipping ports. Our identifying assumption is that the importers' demand due to the pandemic is uncorrelated with the disruption its suppliers' experience along their shipping routes.<sup>1</sup> This measure of Covid-19 exposure is

<sup>&</sup>lt;sup>1</sup>Since we are focused on imports, we are also abstracting away from any disruptions the importers face if

correlated with the real cost implications from either delayed shipments or re-routing that a supplier must work around but plausibly uncorrelated with firm import demand in the months during which the lockdowns were most harshly felt.<sup>2</sup> The measure also presumably identifies exogenous supply shocks since it is twice removed from the importer's own transactions in that it reflects shipping route disruptions faced by the importer's suppliers not related to the suppliers' own transactions.

Controlling for firm, product (HS-6), and county-month fixed effects (and therefore pandemic-related health and mobility effects), we find that US importers who were more exposed to suppliers affected by route-specific trade disruptions had larger reductions in import growth. A one standard deviation decrease of the suppliers' shipping activities (or a rise in our measure of exposure) translates to a 2.4 percentage point reduction in the import growth rate of the US importer. Adopting the Census classification of products by their end-use category, we find that the disruptions to trade are widespread, affecting Capital goods, Consumer goods, and Industrials.

Next, we examine if PPP had a mitigating role on the importers' disruptions. For this analysis, we restrict our sample to importers that were not direct recipients of PPP loans.<sup>3</sup> In addition to controlling for county-month fixed effects and other time-varying factors such as concurrent policy responses to the pandemic which might confound the influence of PPP, we address endogeneity in the disbursement of PPP loans using two strategies: First, following Granja et al. (2022), we use the Bartik-style measure of geographic exposure to bank branches and the success of individual banks in distributing PPP loans. The assumption here is that the measure isolates bank-supply frictions prevailing prior to the pandemic but instrumental

they are also exporting as their own export routes are not necessarily related to their suppliers'. See section 3 for a detailed example of how we identify the supply shock.

 $<sup>^{2}</sup>$ To validate this latter assumption we regress a county-aggregated Covid exposure measure on countyfactors related to where the pandemic was felt the strongest and do not find any significant associations. In addition, the measure uses direct evidence of shipping disruptions to routes that suppliers relied on in 2019 instead of using indirect measures such as the Covid cases reported in a particular location, which are particularly noisy in the early periods of the pandemic when testing was not widely available.

<sup>&</sup>lt;sup>3</sup>Note that our sample is made up of mostly large firms, and since firms that engage in trade are typically larger than purely domestic firms (Bernard et al., 2009)), most do not receive PPP.

in quickly allocating PPP funds, while orthogonal to differences in local demand for funds. Second, following Faulkender et al. (2021), we proxy PPP exposure with the market share of community banks. This measure exploits the variation in the timing of PPP receipt by leveraging the faster pace at which community banks approved and disbursed PPP funds compared to other banks.

Using either measure, we find that PPP loans to small firms also benefits the large importers. A one standard deviation increase in exposure to PPP reduces the effect of supply exposure by 0.46 percentage points, or approximately one-fifth of the effect of the supply shock. A model with dynamic effects clearly shows parallel trends pre-April, with a sharp effect of PPP on import demand after the program is implemented. Large economic effects are mainly found in capital and consumer goods, suggesting that the PPP program boosted demand within local economies thus likely maintaining the production of nearby suppliers. Overall, we see that the PPP program that was intended for small businesses had positive spillover effects on large importers by reducing the demand disruption the importers may have faced from their external supply shocks.

While we focus the main analysis on import demand given the data availability in realtime, as an alternative outcome variable, we study firm growth by aggregating the analysis to the level of the parent firm-quarter. We find that while trade disruptions led to a reduction in firm growth, an increase in exposure to PPP mitigated this effect. At a more aggregate level, we also see that while trade driven COVID disruptions reduced county-level monthly employment in March through September of 2020 relative to January of 2020, this effect is ameliorated in counties that received a large amount of PPP funds.

Although we are unable to show directly the specific feedback effect on demand from the external supply shock, we attempt to capture this indirectly by using geographic variation in the extent of small-large firm linkages. First, we use the *Chinitz* index from Glaeser and Kerr (2009) which reflects areas with many small suppliers and interdependencies among

industries. Counties with a higher Chinitz index should have more small, heterogeneous suppliers and thus greater PPP loans to these areas must have a larger effect on importers. Second, we use the *InputOutput* index from Ellison et al. (2010) to proxy for the connection between industries within a county. Higher values of this index indicate stronger input-output linkages or greater industrial diversity in a county. Third, we use the *SME Share* from Denes et al. (2021a) which captures the share of establishments that are small and medium enterprises (based on employment size) in a county.

We find that the effects of PPP on fostering resiliency for importers is largest in counties that rank high on the Chinitz index, high on input-output linkages, those that are more diverse, and have greater share of small businesses. Although exposure to PPP is expected to have increased import demand for all firms in general, the effect is significantly larger for firms in counties where one would expect spillover effects to be largest. For example, firms in counties that rank in the top half in terms of exposure to input-output linkages have five times the import demand response to the same level of PPP relative to firms in the other counties.

To summarize, our results show that local resiliency plays an important role in mitigating the effects of a trade supply shock when stability is provided for small and medium enterprises. As input-output linkages propagate negative supply shocks, a key role for policy is to sustain businesses that lack the resources to hold out severe recessions. In this sense, the benefits of the Paycheck Protection program extend beyond providing liquidity to recipient SMEs, to building resiliency for the broader regional economy.

Our results contribute to several streams of literature. First, our paper contributes to an emerging body of research studying firms' differential resilience during the Covid-19 crisis. These studies point to a number of factors including access to liquidity and financial structure (e.g. Acharya and Steffen (2020), Ramelli and Wagner (2020), Levine et al. (2020), Berger et al. (2020), Chodorow-Reich et al. (2022), Greenwald et al. (2020), and Fahlenbrach et al.

(2020)), social capital (e.g. Albuquerque et al. (2020), Lins et al. (2017)), and workplace flexibility (e.g. Bai et al. (2021), Barry et al. (2022)). Our study expands the understanding of firm resilience during a crisis period by focusing on the factors external to the firm - their linkages with other firms in the economy. Our finding that greater exposure to COVID-19 through global supply chains is costly for firms is consistent with studies showing the impact of supply chains on firm stock returns. Pre-Covid, Jain and Wu (2023) establish that a firm's global sourcing strategy predicts stock market returns, while Ding et al. (2021) and Ramelli and Wagner (2020) have shown negative returns for firms more exposed to global supply chains and China in particular during the pandemic. Our paper suggests a likely mechanism for firms' financial losses stemming from reduction in their imports.

Second, our paper relates to the broad literature studying the effect of the PPP on the corporate sector. Faulkender et al. (2021) exploit variation in the timing of the PPP loan receipt caused by differences in local banking market structure across US counties and find significantly larger employment effects while others find smaller employment effects (e.g. Autor et al. (2020), Granja et al. (2022), and Chetty et al. (2020)). More robust are the findings on survival resiliency due to the program as highlighted in Bartlett and Morse (2021), Wang et al. (2020), Denes et al. (2021b), Gourinchas et al. (2021) and Bartik et al. (2021). Our proposed mechanism, that avoiding a mass liquidation event limited the negative spillovers that take place in the local economy, is consistent with this literature, which has thus far mostly ignored any spillover effects of PPP on the overall economy. Our paper focuses on the *non-recipients* and shows that these spillovers are large enough to be a first-order consideration in assessing the overall effects of the PPP program.<sup>4</sup>

Our paper also relates to the large literature on agglomeration economies that has emphasized input-output linkages and spillovers between geographically proximate firms (see

<sup>&</sup>lt;sup>4</sup>One concern may be the potential cost of misallocating resources or crowding out of the non-recipients of funds. For instance, Denes et al. (2021a) highlight that policies that discourage expansion might be counterproductive. In our setting, although the PPP was discriminatory in its size cutoff for obtaining funds, it is clear that larger firms had alternative methods to access credit (Chodorow-Reich et al., 2022; Acharya and Steffen, 2020) and therefore it is unlikely that it led to the crowding out of ineligible firms.

Duranton and Puga (2004), Glaeser and Gottlieb (2009), and Moretti (2010)). Dougal et al. (2015) show that local agglomeration economies are an important determinant of firm investment and growth. In contrast to these studies, we highlight the role of SMEs and the potential spillover effects of a policy that prevents a cluster of closures. A related literature has identified supply shock propagation from finance and natural disasters. For example, Peek and Rosengren (2000) find that an exogenous loan supply shock, through US firm links to Japanese banks, has aggregate real effects. Our finding that the survival of small firms have indirect spillovers to larger firms is consistent with the evidence on the role of input-output linkages in propagating natural disaster shocks as seen in Carvalho et al. (2020) and Bonadio et al. (2021).

Relatedly, there is literature examining the effects of small business lending and subsidy programs on increasing net job gain (Brown and Earle (2017)), credit supply (Bachas et al. (2021)), firm growth through attracting venture investment (Lerner (2000)) and innovation (Howell, 2017). None of these papers are focused on estimating the externalities from the small business lending programs on *other* firms.

# 2 Data and Measures of Exposure

### 2.1 U.S. Import Data

We use the universe of maritime U.S. import transactions from S&P Global's Panjiva database which sources the data directly from U.S. Customs.<sup>5</sup> Our beginning sample consists of 7,362,502 U.S. maritime import transactions across 996,891 firms from March to September of 2020. For each transaction, we have the following elements reported on the Bill of Lading (BoL): names and addresses of the consignees (importers), a unique identification number for each importer (importer ID), their foreign shippers, description of the traded goods,

<sup>&</sup>lt;sup>5</sup>According to data from the Bureau of Transportation Statistics, maritime trade accounts for over 70% of US international trade activities, measured by total weights. https://www.trade.gov/maritime-services-trade-data

quantity imported, shipment arrival date, ports (lading and unlading) associated with the transactions, and product code (6-digit HS code (HS6)). We define a trading route r by a unique Port of Lading (PL)-Port of Unlading (PUL) pair. For instance, a commonly used trading route in our data is the PL-PUL pair, Shanghai-Los Angeles. We have 8,708 unique trading routes in our sample and 4,900 unique HS6 codes.

Since our analysis involves comparing trade disruption in the pandemic to pre-Covid times, we first restrict our sample to firms with import transactions in both 2020 and at least one year between 2017-2019.<sup>6</sup> After excluding transactions with missing information on ports (both PL and PUL), missing importer ID, missing addresses, or addresses outside the U.S (typically foreign MNEs doing business in the U.S.), our raw data sample consists of 4,811,056 import transactions across 151,298 unique firms, involving 4,687 unique HS6 codes and 7,168 unique trading routes.

We aggregate the transaction data to the firm-product(HS6)-month level as shown below. To quantify the import disruption of US importing firms, we compare the imports in each month of 2020 to the average imports in the same month during 2017 to 2019. Specifically, for importer i importing product k in month t, we compute:

$$\Delta Import_{i,k,t}^{Nbr} = \log(Import_{i,k,t}^{2020}) - \log(Import_{i,k,t}^{(17-19)Avg})$$
(1)

where variable  $Import_{i,k,t}^{2020}$  is the Total Number of Import Transactions in 2020, and  $Import_{i,k,t}^{(17-19)Avg}$  is the Average Total Number of Import Transactions for the same month between 2017 and 2019. While we use the Number of Import Transactions as our main variable, we also use Volume of Imports in robustness tests and find similar results. We prefer the specification with transactions as our main specification since trade volume is missing or zero for just over 9% of the transactions in our sample.<sup>7</sup>

<sup>&</sup>lt;sup>6</sup>Our results are robust to restricting the sample to firms with imports in just 2019 and 2020.

<sup>&</sup>lt;sup>7</sup>We also have information on dollar value of trade but this is missing for a more sizeable portion (30%) of the sample.

Figure 1 in the Internet Appendix provides a map of the geographic distribution of U.S. importers in our sample using the firms' addresses in the BoL. Not surprisingly, importers are concentrated in the places with largest economic activity (metro areas around the west and east coasts). While our main analysis is conducted at the subsidiary level, using the Panjiva-Capital IQ link we are also able to identify the parent firm of the subsidiaries and repeat our analysis at the parent firm level.

For our benchmark sample, we exclude observations with missing values on  $\Delta Imports^{Nbr}$ , our constructed Covid supply exposure, or control variables (described below). Next, we exclude large logistic and freight firms from the sample.<sup>8</sup> Flaaen et al. (2021) also report that for some large importers (e.g. Walmart), there is a large variation year-to-year in the number of addresses associated with them in the BoL data likely due to redacted data. Although we include these firms in our main results, in robustness checks we find our results to be materially similar if we were to drop these firms. Finally, we drop the top and bottom 1% outliers on  $\Delta Imports^{Nbr}$  and the Covid supply exposure. Our final sample for which we have data on  $\Delta Imports^{Nbr}$  consists of 244,367 observations over 49,230 firms, 3,340 product codes, in 1,574 counties in the U.S, over the months March-September 2020.

# 2.2 Paycheck Protection Program (PPP) Data

One of the key fiscal stimulus measures used in the United States to combat the Covid-19 pandemic has been the \$2 trillion Coronavirus Aid, Relief, and Economic Security (CARES) Act, which extends support in varying degrees to workers, businesses, and local governments. Our focus is on the portion of the CARES Act package designed to aid small businesses, the Paycheck Protection Program (PPP), which allocated \$669 billion in the form of cheap, forgivable debt to small businesses.

<sup>&</sup>lt;sup>8</sup>We drop importers that are on the list of the largest logistic firms in the US complied by Armstrong & Associates, Inc., a leading third-party logistics (3PL) market research company. The list can be found at https://www.logisticsmgmt.com/article/top\_50\_us\_and\_global\_third\_party\_logistics\_2020. We also drop firms where the importer name contains the words "logistic", "distribution", or "freight".

The first wave of the PPP program was launched April 3, 2020 and expired August 8, 2020, during which period over 5 million PPP loans were granted with the average loan amount being \$102,259. We use PPP loan-level data from the first wave released by the Small Business Administration (SBA) to measure firm's exposure to PPP across geographic regions.<sup>9</sup> We compute  $PPP^{Nbr}$  as the total number of PPP loans approved in each county-month scaled by the total number of establishments in each county in 2018 (pre-pandemic).<sup>10</sup> Data on the number of establishments in each county in 2018 is obtained from the County Business Patterns (CBP) data provided by the US Census Bureau.

One concern with this measure is that it may be highly correlated with other regional economic factors and may not represent an exogenous measure of a firm's exposure to PPP in a region. To address this, following Granja et al. (2022), we construct a measure of the regional exposure to PPP loans (*PPPE*). A large literature on bank relationship lending since Petersen and Rajan (1994), Berger et al. (2005), and Degryse and Ongena (2005) has highlighted the role of distance (as a proxy for relationships) in small business lending. For instance, Agarwal and Hauswald (2010) show that shorter geographic distance improves the ability of lenders to produce soft information and extend credit to small businesses; Granja et al. (2017) show that geographic proximity is a significant determinant of who acquires failed banks in the economy; and Nguyen (2019) finds that bank branch closures are associated with declines in small business lending. More recently, Li and Strahan (2021) show that close bank relationships can help firms gain access to PPP funds and Bartik et al. (2021) argue that program take-up was determined by bank decisions (as is assumed in our exposure measure).

Motivated by these observations, we construct a Bartik-style measure of counties' ex-

<sup>&</sup>lt;sup>9</sup>There was also a second wave of PPP loans from January 2021 to May 2021. See SBA Press Release on Tranche2. The criteria for PPP loan disbursements changed between the first wave and second wave and hence we restrict our sample to the first wave.

<sup>&</sup>lt;sup>10</sup>In unreported results, we also use  $PPP^{Vol}$ , which is the total volume of PPP loans approved in each county-month scaled by the total number of establishments in each county in 2018. We have similar results across the two settings.

posure to bank performance in PPP lending by using the distribution of deposits across counties. Thus we are able to compare counties exposed to lenders that gave more PPP loans relative to other small business lending, to counties exposed to lenders who gave fewer PPP loans relative to other small business lending.

To obtain a bank's small business lending (SBL) and PPP loans lending data, we rely on Call Reports data from the Federal Financial Institutions Examination Council (FFIEC).<sup>11</sup> The Call Reports data is updated quarterly and we collect information on banks' SBL and PPP lending in the 2nd and 3rd quarter of 2020 for 5,132 unique banks in the U.S.

We define the PPP Exposure for bank b in quarter q exactly as in Granja et al. (2022):

$$PPPE_{b,q} = \frac{\text{Share } PPP_{b,q} - \text{Share } SBL_{b,q}}{\text{Share } PPP_{b,q} + \text{Share } SBL_{b,q}} \times 0.5$$
(2)

where Share  $PPP_{b,q}$  and Share  $SBL_{b,q}$  are bank *b*'s market share in distributing PPP loans and SBL respectively in quarter *q* among all banks. We use total number of loans as our main measure to compute market share but also use the volume of lending as a supplementary measure and find similar results.

Next, we compute a county's exposure to PPP by using bank branch location information as of June 30th, 2020 from the Summary of Deposit (DOS) data maintained by Federal Deposit Insurance Corporation (FDIC). The exposure to PPPE at the county c level in quarter q as:

$$PPPE_{c,q} = \sum_{b} w_{b,c} PPPE_{b,q} \tag{3}$$

where  $w_{b,c}$  is the share of bank b's branches among total number of bank branches in county c and  $PPPE_{b,q}$  is the PPP exposure measure for bank b in quarter q from Equation 2.

<sup>&</sup>lt;sup>11</sup>The information on banks' SBL is available in the Schedule RC-C Part II - Loans to Small Businesses and Small Farms of the Call Report. Since the 2nd quarter of 2020, the FFIEC also requires banks to report their PPP loan issuance under the Schedule RC-M - Memoranda, in which banks report the following information: Number of PPP loans outstanding and the Outstanding balance of PPP loans.

Finally, the previous measure is aggregated across all PPP funds received for a county, such that the benchmark measure of PPP exposure is time-invariant,  $PPPE_c$ . This is done for several reasons. First, the measure in (2) is quarterly and would require us to pool three months of monthly import data to match with when the PPP funds are dispersed.<sup>12</sup> Second, it is not clear how quickly the PPP expenditures should show up in the real economy. Finally, the funds are no longer constrained by the end of the program and are most constrained at the very beginning in April. The  $PPPE_c$  measure captures the relative exposure across counties using heterogeneity in access at the *outset of the government program*, with potential persistent effects across several months.<sup>13</sup> We therefore capture the average effect on imports across all months and afterwards interact this measure with month indicators. In the latter, we hypothesize that the effects increase over time for the first few months (with no effects pre-April), and then should disappear.

We note that, although we take our measure from Granja et al. (2022), our aim differs from theirs. They intend to show the misallocation of PPP loans, especially in the first round of the first wave. In doing so, they convincingly argue that the allocation was not based on "need", but pre-pandemic bank supply-side factors. Our strategy is to leverage the nature of the rollout in a way that takes advantage of the exogenous variation in exposure to PPP loans given that the allocation of PPP is independent of demand.<sup>14</sup>

Our results also feature a specification where we proxy for PPP exposure using the market share of community banks as in Faulkender et al. (2021), once again leveraging the variation in the timing of receipt of PPP across counties. The idea is to take advantage of cross-sectional county differences in banking market structure, as the aforementioned paper argues that community banks were quicker to approve and disburse first-round PPP funds. Specification checks confirm that our PPP exposed counties did not have a different exposure

 $<sup>^{12}</sup>$ We also present results using the *actual* disbursement of funds which varies by month.

<sup>&</sup>lt;sup>13</sup>Relatedy, we have found that the results hold by creating the  $PPPE_c$  measure using only funds disbursed in the second quarter.

<sup>&</sup>lt;sup>14</sup>Our identifying assumption will be that the bank supply frictions in making PPP loans, conditional on controls, are not correlated with outcomes as we show below.

to Covid supply shocks pre-April.

# 2.3 Measures of Local Linkages and Small Firm Share

To test if positive effects of PPP are indeed due to it countering negative local spillovers, specifically through linkages between large and small firms, we employ several different measures of linkages and the importance of small firms at the county level: First is the *Chinitz* index developed in Glaeser and Kerr (2009) which specifically addresses the dynamics between small businesses and external suppliers. The presence of a large number of small businesses that use inputs from a variety of suppliers will reflect an agglomerated economy with improved efficiency (Chinitz, 1961) due to lower transport costs. 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:

$$Chinitz_{h,c} = \sum_{l=1,\cdots,L} \frac{Firms_{l,c}}{E_c} Input_{h\leftarrow l}$$
(4)

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.

In addition to the *Chinitz* measure, we follow Ellison et al. (2010) and employ a related measure called *InputOutput*, which captures more generally the extent to which industries buy and sell from/to each other and is measured as follows: First we measure the extent to

which each industry receives input from or provides output to the local economies using:

$$Input_{h,c} = \sum_{l=1,\dots,L} \frac{E_{l,c}}{E_c} Input_{h\leftarrow l}$$
(5)

$$Output_{h,c} = \sum_{l=1,\cdots,L} \frac{E_{l,c}}{E_c} Output_{h\to l}$$
(6)

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.<sup>15</sup> 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.

Our next measure explicitly accounts for the share of small and medium enterprises in the local economy, as small businesses have been shown to play important roles in agglomeration economies (e.g. See Delgado et al. (2010) and Glaeser et al. (2015).) Specifically, with county-industry level employment data we follow Denes et al. (2021a) to construct the share of establishments with fewer than 500 employees  $(SBS_{500})$ .<sup>16</sup>

We provide more details on the construction of these county measures in the Internet Appendix D. As additional robustness, we define two other measures in the Appendix:

<sup>&</sup>lt;sup>15</sup>*Input*<sub> $h \leftarrow l</sub> and$ *Output* $<sub><math>h \rightarrow l$ </sub> provide us information on the importance of each industry to the local inputoutput networks.</sub>

<sup>&</sup>lt;sup>16</sup>500 employees is in the lower range for the maximum employment size of an establishment to be labeled "small" by the SBA based on industry-specific size standards. In robustness checks we find similar results using a 20 employee cutoff to define small businesses.

Following Nakamura and Paul (2019), we proxy agglomeration by industrial employment diversity. Next, following Gaubert (2018) we study the shape of the import distribution where a thicker tail within a county reflects higher levels of agglomeration.

## 2.4 Data Summary

To control for other concurrent confounding factors that might also impact firms' trading activities (e.g. stimulus payments and initial business conditions), we use the one-month lagged unemployment rate,  $UnEmp_r$  from the Department of Labor, the number of confirmed COVID cases,  $COVID_Case$  from Johns Hopkins Coronavirus Resource Center and the monthly change of small business revenue  $Chg_SB_Rev$  from Opportunity Insight (Chetty et al., 2020). Table A1 in the appendix provides a full list of the variables used in our analysis along with descriptions and sources.

Summary statistics for all the key variables are reported in Table 1. On average, the monthly import transactions (volume) reduced by 5.1%(4.7%) in 2020 compared to the 2017-2019 average for the same month (for the firms in our sample). The summary statistics for PPP in a county show that nine out of 100 establishments within a county receives a PPP loan per month.

# 3 Empirical Specification

In the Internet Appendix A we outline a formal model for the empirical tests in this section. To provide intuition, we adopt the simple framework of Glaeser and Kerr (2009) where the firm's production function has a firm-specific productivity shifter that is affected by exposure to supply chain disruptions. Import demand serves as a proxy for the severity of the shock, or the loss of production for the firm. This gives our first prediction that firms facing greater Covid exposure through supplier route disruptions have lower imports.

In this framework, the firms production function also includes a local area productivity shifter that is determined by both regional linkages and agglomeration forces. This allows for the importers supply shock to spill over to the local economy and importers network of SMEs and also feed-back as a demand shock. At the same time, PPP subsidies not only reduce the direct impact on SMEs but through this agglomeration term indirectly benefit the large importers by reducing the disruption in factor demand. This gives us our second prediction that importing firms facing a given level of Covid exposure are less affected when they are located in counties with greater PPP disbursements and greater input-output linkages between SMEs (PPP recipients) and large firms (importers).

Empirically, first, we construct a *Covid Exposure* measure for US importers. The aim is to identify a supply shock based on importers' reliance of foreign exporters, while purging any local demand effects. Next, we use this measure to quantify its effect on import growth of a US importer and finally the degree to which local PPP expenditures mitigate the firmspecific effects.

Figure 1 provides a pictorial representation of the whole procedure for an example from our data. Consider the case where Boeing's plant in King county in Washington State is importing parts of airplanes and helicopters (HS 880330) from four different foreign suppliers: Alouette (France), Leonardo (Italy), Israel Aerospace Industries(IAI) (Israel), and AVIC International (China). Each of these suppliers faces trade disruptions due to Covid. The top figure shows how we identify the supply shock faced by one of these suppliers - Alouette. Alouette uses five different shipping routes to ship this product to different US importers (excluding Boeing). We first identify an exogenous component of the disruption faced by Alouette along each of these routes by regressing the 12-month difference in Alouette's number of transactions along each of these routes on the 12-month difference in total number of transactions (excluding Alouette's) along each of these routes (Equation 7 below). Once we have the monthly disruption faced by Alouette along each route, we compute a weighted average of the disruptions along the routes with the weights (width of the arrows) reflecting the importance of each route in the total number of transactions for Alouette to estimate a monthly Supply Shock for Alouette. We repeat this process to compute the monthly supply shock for each of the other foreign suppliers of Boeing.<sup>17</sup>

The lower figure shows that we then take a weighted average of the monthly supply shocks across all foreign suppliers to estimate Boeing's monthly Covid Exposure (Equation 8 below) for this product. The weights once again are the share of total transactions for this product from each supplier in that month and are represented by the width of the arrows. This figure also shows that there are local spillover effects of Boeing's supply driven Covid exposure onto small firms in the county through local linkages which in turn feeds back as demand effects on Boeing. These feedback effects are ameliorated through PPP (Equation 10 below). Since mass layoffs and liquidation events are known to have large negative spillover effects that are highly localized (e.g. Bernstein et al. (2019), Gathman et al. (2020)), if small firms in King County benefited from PPP and did not shut down, Boeing's imports in this plant would recover faster than in another county where small firms did not benefit as much from PPP. The following sub-sections detail out the mathematical expressions for each of the steps outlined above.

# 3.1 Construction of COVID Exposure Measure

To construct an exogenous measure of importers' supply disruption, we rely on the importing firms' dependence on their supplier networks. We first construct a measure of disruption faced by each supplier and then construct a weighted average across all suppliers of an importer. Importantly, the supplier shock is constructed at the exporter level only. The goal is to capture US importers' exposure to shipping disruptions by comparing their import growth from pre-Covid times to the same month in the year 2020, given different suppliers who are differentially affected by Covid-19 related shipping disruptions due to the trading routes that they use.

Trade relationships are very persistent from one year to the next as shown in Monarch

<sup>&</sup>lt;sup>17</sup>Parallel to the literature that identifies bank shocks withinfirms borrowing from multiple banks (Khwaja and Mian, 2008), we estimate supply shocks to COVID within suppliers that ship through multiple routes and avoid confounding the supply shock with firm or location characteristics related to changes in demand.

(2022). We are interested in the networks, or relationships with suppliers established before the onset of the pandemic and hence we use relationships that exist anytime between 2017 and 2019. We interpret a negative shock to the established suppliers as a negative productivity shock to US importers, implicitly assuming relation-specific fixed costs as in Antras (2003) and Bernard et al. (2018).

To measure the supplier shock, we isolate Covid-induced route-specific disruptions by leveraging variation in exports by individual suppliers across multiple routes with supplierproduct-time fixed effects. The disruptions can be due to many reasons, such as lockdowns, port regulations/shutdowns, reduced labor availability, etc, and likely reflects the severity of the pandemic in the areas (port cities) that a route travels through. Although this might reflect some reduction in demand by consumers, freight rates stayed steady and even increased by May (see Notteboom et al. (2021)), as container ships mostly traveled full.<sup>18</sup>

For supplier j, exporting product k along route r in month t, we estimate the following specification to identify how route-specific disruptions affect the suppliers:

$$Supply \ Shock_{j,r,k,t} = \Delta \log(Supply_{j,r,k,t}) = \beta \Delta \log(Route \ Transactions_{r,t}^{-j}) + \mu_{j,k,t} + \nu_{j,r,k,t}$$
(7)

where  $\Delta \log(Supplier_{j,r,k,t})$  is the 12-month difference in the log number of transactions (or volume) for each supplier (j)-route (r)-product (k) in month t and  $\Delta Route Transactions_{r,t}^{-j}$  is the 12-month difference in the log number of total transactions through route r in that month excluding the transactions by supplier j. Thus  $\Delta Route Transactions_{r,t}^{-j}$  captures all trade disruptions along that particular route.  $\mu_{j,k,t}$  controls for supplier×product×month fixed effects, which absorb supplier shocks such as demand for its products. Notice that we rely on supplier-product combinations across multiple routes in order to isolate the route-specific disruption. The predicted value from this regression provides an estimate of the exogenous

 $<sup>^{18}</sup>$ We also attempt to further reduce the effect of demand by replacing route disruptions with only the port of lading.

component of the disruption faced by the supplier for product k along this particular route.

Appendix Table A2 reports the estimation results of the specification in Equation 7 for  $\Delta \log(Supply_{j,r,k,t})$  measured both by the number of transactions and total volume. We construct the difference in total route trade using the log difference between 2020 and either the average between 2017-2019 (columns 1 and 2) or only 2019 (columns 3 and 4). The results show a significant reduction in exports from a specific route in response to changes in total route activity both in number of transactions and volume, in 2020 relative to either time period. Given the similarity in results across the two benchmarks, going forward, our analysis will use the difference between 2020 and the average of 2017-2019.<sup>19</sup> The F-statistic for all specifications is above 100, which implies that aggregate route-specific disruptions are a strong indicator of reductions in supplier exports.

Next, we construct the monthly Supply Shock at each supplier-product level by aggregating the predicted  $\Delta \log(Supply_{j,r,k,t})$  across all the routes used by firm to ship a product, weighted by the importance of each route in a firm-product combination.<sup>20</sup> Finally, we define the monthly COVID Exposure faced by each US importing firm i as the negative values of weighted aggregate Supply Shock<sub>j,k,t</sub> across all its supplier-product pairs. Suppose firm ibuys product k from j = 1, 2, ...N suppliers in month t, firm i's Covid exposure is given by:

$$COVID \ Exposure_{i,k,t} = -1 \times \left(\sum_{j=1}^{N} \omega_{i,j,k,t} \times Supply \ Shock_{j,k,t}\right)$$
(8)

where  $\omega_{i,j,k,t}$  is the share of *i*'s total transactions in product k that come from supplier j in month  $t^{21}$  The variation in *COVID Exposure* is therefore generated from the shock to a firm's suppliers and varies monthly (March to September) over the course of the pandemic. A higher

<sup>&</sup>lt;sup>19</sup>We should be clear that suppliers can reallocate their activity across routes, and in fact we find that they partially do so. However, we interpret this route disruption as a cost since previous years provide information about the "cost minimizing" solution for the firm.

 $<sup>^{20}</sup>$ If a supplier is using a single route to export a product, then that firm would not be included in the estimation of equation (7), but we do generate its predicted shock due to the route it is using. Hence, it would be included in the estimation of the *Supply Shock*.  ${}^{21}\omega_{i,j,k,t} = \frac{transactions_{i,j,k,t}}{\sum_{j,k,t} transactions_{i,j,k}}$ , constructed using the same month in 2019.

value of the *COVID Exposure* indicates importing firms face more pandemic disruptions.

Panel A of Table 1 shows that the sample average COVID Exposure is 0.014, suggesting that the predicted disruption-related decline in supplier shipments is 1.4%. The average exposure can also be expressed in dynamic terms, to test how it tracks with aggregate U.S. imports at the same time. Figure 2 displays the average exposure measure along with the U.S. aggregate import index. Our exposure measure moves in the opposite direction as the aggregate import index with a drastic increase from March to May, and then a sharp decline starting in June (though it is still positive until September).

# **3.2** COVID Exposure and Imports

To examine the impact of the COVID-19 pandemic on firms import activities, we estimate the following equation:

$$\Delta Import_{i,k,t}^{Nbr} = \beta \cdot COVID \ Exposure_{i,k,t} + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t} \tag{9}$$

where  $\Delta Import_{i,k,t}$  and the *COVID Exposure*<sub>*i,k,t*</sub> are defined in sections 2.1 and 3.1 respectively.  $\xi_i$  and  $\eta_k$  are firm and product fixed effects to control for time consistent factors that vary across each firm and product and may be correlated with import activities. State (county)-month fixed effects,  $\kappa_{s(c),t}$ , are used to capture any variation along time across different states (counties) that might affect import activities of the firms. In the most stringent specification with county-month fixed effects, we compare, within counties, firms with different changes in their exposure. Standard errors are clustered at the firm level to address the serial correlation in the dependent variable.<sup>22</sup>

It is important to highlight that COVID Exposure includes the disruptions to each firms' suppliers, but does not include any direct demand effects of importer i due to the pandemic. For example, it is plausible that a company such as Boeing also faced lower demand for its

<sup>&</sup>lt;sup>22</sup>While we use transactions on all firms in estimating firms' *COVID Exposure* to comprehensively capture the route disruptions, when we analyze how *COVID Disruption* affects imports we drop logistic companies.

output given the effect of uncertainty on durable manufacturing, which is then reflected in Boeing's lower import demand for materials. Boeing's negative demand shock shows up in the error term. Our identifying assumption is that this is uncorrelated with the *COVID Exposure* shock that Boeing experiences, conditional on controls.

Appendix Table A3 conducts a falsification test with aggregated data in an attempt to validate our identifying assumption that *COVID Exposure* is not correlated with the concurrent demand for imports related to the pandemic. If the assumption fails, we should find that importers in counties with large supply disruptions were also responding to other Covid-related factors, altering total import growth in those counties beyond what was due to our exposure measure. Table A3 shows that our exposure measure is not significantly associated with any of the county specific pandemic related variables, e.g. population density, income per capita, racial diversity, etc., neither in March nor April.

# **3.3 PPP and COVID Exposure**

To explore whether PPP fosters resiliency in response to trade disruptions, we estimate the following equation:

$$\Delta Import_{i,k,t}^{Nbr} = \beta \cdot COVID \ Exposure_{i,k,t} + \gamma PPPE_c + \theta COVID \ Exposure_{i,k,t} \times PPPE_c + \delta X_{i,t} + \xi_i + \eta_k + \kappa_{c(s),t} + \varepsilon_{i,k,t}$$
(10)

where  $PPPE_c$  is the time-invariant measures of PPP in county c described in section 2.2 and  $X_{i,t}$  is a set of interactions where we interact the time-varying county-level control variables with the *COVID Exposure*.<sup>23</sup> For the specifications including the PPP interaction, standard errors are clustered at the county level.

The assumption in this specification is that a counties' receipt of PPP is not driven by

<sup>&</sup>lt;sup>23</sup>In the results, we also present cases with monthly  $PPP_{c,t}$  using direct county-month expenditures from the SBA data. With monthly expenditures that specification is possible, however, our benchmark exposure measure is time-invariant.

repercussions from the supply shock itself, which we test in Appendix Table A4. In this case we can conduct a pre-trend analysis where we compare pre-PPP changes in import growth across counties that were later heterogeneously treated with PPP exposure. To this end, we regress county-level changes in import transactions (for March and April only) on future PPP exposure, also controlling for Covid Exposure and other controls. We find that there is no significant correlation between counties that see larger import shocks and counties with higher PPP exposure. We also conduct the same analysis with county employment growth as an outcome and similarly find no effect from the future PPP exposure.<sup>24</sup>

We will also report a specification where  $PPPE_c$  in (10) is replaced with a measure of the market share of community banks as in Faulkender et al. (2021). By construction, this county-level exposure measure is also time-invariant. As with  $PPPE_c$  the idea is to exploit geographical variation in banking market structure to identify an exogenous component of the intensity of the subsidized loans. Although the supply of loans was not constrained by the end of our sample, our specification assumes that immediate access has positive effects that reverberate at the county level for the next few months. Our time-invariant proxies of PPP capture heterogeneity in access at the outset of the government program.

The coefficient  $\theta$  captures how PPP could moderate the disruptive effects of COVID on the imports. If  $\beta$  and  $\theta$  are of the same sign, it implies the disruptive effects of COVID are amplified by the PPP while an opposite sign indicates a positive productivity effect of PPP that dampens the spillovers of the Covid supply disruption. Since the program was aimed at helping businesses weather the various shocks related the pandemic, we expect  $\theta$  to be positive for all firms.

Our theoretical framework (see Appendix A) predicts that  $\theta$  is positive for *non-recipients* of PPP as well, so we focus on this sample of firms (see below). Furthermore, given that this

 $<sup>^{24}</sup>$ Notice that we will also complement the specification in (10) with a dynamic effects model that includes February-April to test the parallel trends assumption for firms in countries with low versus high PPP exposure before the pandemic.

prediction is driven by the presence of an *agglomeration term* discussed at the beginning of the section, we examine heterogeneous effects. In these cases, we adopt the specification in (10) to sub-samples that reflect the level of local spillover exposure in a county.

# 4 Results

# 4.1 Import Growth and COVID Exposure

The estimation results of the baseline model are reported in the panel A of Table 2. Columns 1-2 report the results using  $\Delta Imports^{Nbr}$  as a dependent variable while columns 3-4 report the results for  $\Delta Imports^{Vol}$ . In both settings, we report results with either state x month or county x month fixed effects. All regressions contain firm and product fixed effects. The coefficients on *COVID Exposure* are negative and significant in columns 1 and 2 implying that the COVID related trade disruptions that occur on the supply side affect the number of import transactions of US importers. These effects are also economically significant. The regression coefficient for column 2 suggests that a one standard deviation increase of the *COVID Exposure* reduces the import growth rate by 2.4 percentage points (1.433 × 0.017 – see Table 1) after controlling for firm and product time invariant factors as well as other factors varying at the county-month level. Since the average import growth in our sample is equal to -5%, this magnitude is around 50% of the mean.<sup>25</sup> These results imply a strong disruptive effect of COVID Exposure on the import activities of US firms. We find similar results using volume of imports as our dependent variable in columns 3 and 4. Going forward, we will present results with  $\Delta Imports^{Nbr}$  as our main dependent variable

<sup>&</sup>lt;sup>25</sup>This share of the change in import growth attributed specifically to supply disruptions given our research design, is large but reasonable given the time-frame explored and the scope of shipping cancellations described above. Similarly, Berthou and Stumpner (2021), Aiyar et al. (2022), and Cerdeiro and Komaromi (2022) each identify a very large short-term trade effect due specifically to lockdown policies tied to supply of trade partners using more aggregate global data. For example, Aiyar et al. (2022) find that trade partners lockdowns explain up to 60 percent of the observed decline in imports. Lafrogne-Joussier et al. (2022) highlight that in February 2020, before the pandemic had reached France, French imports from China had already dropped by more than 10%, highlighting the supply-specific disruptions.

but note that results are very similar if we were to use  $\Delta Imports^{Vol}$ .<sup>26</sup>

Finally, in Appendix Table A5, we explore the heterogeneous effects of COVID disruption on imports across different types of products and find that the disruption is felt across the board, in Industrial supplies and materials, Capital goods, and Consumer goods.

Our results are robust to a number of robustness checks. First, in Appendix Table A6 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 pandemicrelated demand shocks experienced by specific buyers (on the US side), replacing the route with the port of lading (POL) and find very consistent results. Second, to control for intermediaries importing or decision-making taking place above the subsidiary level, we aggregate the import data to the parent level in Appendix Table A7 and re-estimate equation 9 with total parent imports linked to their supply shock. Finally, to deal with the issue that some firms request the US Customs to redact some address locations from the Bill of Lading in some years, we flag potential redactors (see Appendix A8). All the robustness checks confirm the main results in this subsection, that greater exposure to COVID-19 through global supply chains is costly for firms.

# 4.2 Does PPP Spillover to firms disrupted by supply shocks?

In this section, we explore whether there are spillover effects on large importers from PPP, a program specifically designed to sustain small businesses. We rely on the strong recent evidence that PPP increased the survival rates of small businesses (Bartik et al., 2021; Gourinchas et al., 2021; Bartlett and Morse, 2021) along with past work showing that liquidation effects are known to have large negative and local spillover effects (Bernstein et al., 2019). If in areas where small firms benefited from PPP and did not shut down,

 $<sup>^{26}</sup>$ We find similar results removing HS products that include personal protective equipment 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.

large importers recovered faster, then we should observe smaller COVID disruptive effects on imports for firms located in regions with higher PPP exposure.

PPP could directly enable recipients to sustain their import demand via additional funds and not necessarily via stimulating a more COVID-immune local environment. However, if PPP only affects direct recipients, such limited effects would raise the specter of the government bailing out failing firms at a high social cost. On the other hand, if the spillover effects indeed exist, we should observe firms that do not directly receive PPP loans also benefit from being exposed to a higher level of PPP loans within the local areas. Hence, we estimate the model as described in Equation (10) for the sub-sample of firms did not receive PPP loans. To identify whether a importing firm is a direct recipient of the PPP loans, we match firms in our sample with the ones in the SBA-PPP data via firm names and the county they are located in. Of the total 49,421 unique importing firms in our sample, we identify 14,671 firms to be the direct recipients of PPP loans. A majority (70.4%) of firms in the sample did *not* receive any PPP loans and we turn our attention to those firms.<sup>27</sup>

We report the estimation results of the specification described in (10) for the sample of non-PPP recipients in Table 3. Columns 1-4 reflect the results with 1-month lagged  $PPP_{c,t}^{Nbr}$ in a county, which are the *actual* number of loans disbursed.<sup>28</sup> Due to the endogeneity concerns of the raw PPP measure that we outline above, we replace the  $PPP_{c,t}^{Nbr}$  with  $PPPE_{c}^{Nbr}$  in column 5, constructed following the procedure in Granja et al. (2022). In the last column we instead proxy for early PPP exposure with the market share of community banks. State-month fixed effects are used in the first 3 columns, while county-month fixed effects are used in the latter three. With county-month fixed effects, we compare import changes relative to March (when the PPP measure is zero by design) across firms located in

<sup>&</sup>lt;sup>27</sup>This is not necessarily surprising as importers are likely represented by larger firms (or subsidiaries) with alternative funding opportunities. For example, Giroud and Mueller (2019) document the possibility of within-firm reallocations. We note that that the names are easily matched between the two datasets.

 $<sup>^{28}</sup>$ Notice in this case we do allow *PPP* to be time varying. *Nbr* refers to the fact we use number of loans in measuring the intensity of PPP, although the volume of lending yields similar results.

different counties and control for any concurrent county-specific shocks.<sup>29</sup>

The interaction coefficients are positive and highly significant across all the settings indicating that for firms located in counties that received more PPP, the disruptive effects of COVID are smaller as measured by import demand. This holds whether we use the likely endogenous PPP calculated as number of loans per establishment or our instrument of PPP exposure through the nearby bank branches. The result is also consistent as we gradually include county-level controls such as the number of known Covid cases and the unemployment rate, interacting these with the *Covid Exposure* measure. The results are similar with statemonth and county-month fixed effects, implying that cross-sectional variation across counties within a state does not seem to be a driver in the PPP effects. Column 5 is our baseline specification and conducted with the most restrictive specification. In terms of economic effects, one standard deviation increase in  $PPPE_c^{Nbr}$  mitigates the COVID disruptive effects by 0.46 percentage points (4.1×0.111). This indicates that PPP generates resiliency to the COVID disruption to non-direct recipients. Overall, our results indicate that PPP stimulates immunity within the local economy that helps firms build resiliency towards the COVID shock.

In the last column, we replace  $PPPE_c^{Nbr}$  measure with the county-level community bank share from Faulkender et al. (2021). Here again, a one standard deviation increase in this measure has an almost identical effect relative to  $PPPE_c^{Nbr}$  on changes in log imports for firms that face equal supply shocks (now equal to 0.4 percentage points).

#### 4.2.1 Dynamic Effects of PPP

Although the PPP was first implemented in April, its disbursement is not immediate as it required borrowers to work with their local bank. Furthermore, the large demand for the first round crowded out many small lenders and prompted the U.S. Congress to authorize new money for the program (Granja et al., 2022). One would expect the spillover effects to not be

<sup>&</sup>lt;sup>29</sup>A bias would be introduced only if there are other fiscal policies during the same time that are targeted towards the same counties that receive a larger amount of PPP.

immediate given the lag in firm closures, etc. Furthermore, given the nature of our outcome, import changes, a lag is built in by construction because we observe when a shipment *arrives* at the port of unlading.<sup>30</sup> Therefore, any resiliency offered by (time-invariant) higher PPP funds' exposure should start to show up in late April and would increase over the next few months.

We explore the dynamic effects of PPP by interacting our resiliency interaction in (3.3) with month dummies. In including a full set of interactions between  $COVID \ Exposure_{i,k,t} \times PPPE_c^{Nbr}$  and month dummies, the March term is dropped –  $\theta_{March} = 0$  – such that all effects are relative to the resiliency of PPP exposed counties in March. In order to expand our analysis and check for longer parallel trends across counties before the disbursement of funds can realistically have an effect, we also include February data to the main specification.

We report the estimation results of the time trends in Figure 3, using the specification in column (2) of Table 3 expanded to include the new set of month interactions. The estimation result confirms that PPP ameliorates the disruptive effects of COVID on imports with the expected trend over time. Firms across counties differentiated by PPP exposure are on parallel trends February-April, consistent with our identifying assumption that firms in counties with more exposure to PPP do not respond differently prior to funds being disbursed. The effect is to some degree present in April (though the coefficient is not significant at the 5% level), which likely captures the immediate effects in the last half of the month after the CARES act is passed.<sup>31</sup> The effect grows significantly over time as it peaks in June, and by July the coefficient is still very large and significant. By July, being in a country with a one standard deviation higher PPP funding implies the reduction in imports is around 0.9 percentage points lower for firms, controlling for supply shocks. This magnitude is about one-third of the effect of the supply shock. By August the effects of PPP are negligible and

<sup>&</sup>lt;sup>30</sup>For reference, in correspondence with a trading partner familiar with port activities, we were estimated that a shipment from China to Los Angeles would have 1-3 weeks shipping time plus 1-2 weeks in customs (with obvious variability).

<sup>&</sup>lt;sup>31</sup>Given the aforementioned lags, especially in the shipments coming in, it is not obvious when we should start observing effects but it is likely to start in the last couple weeks of April.

this continues into September, consistent with the fact that PPP officially ends on August 8th and most of the inequality in implementation is set in well before then.<sup>32</sup> This also aligns with the fact that trade starts to rebound in the third quarter of 2020,<sup>33</sup> and thus the effects are less prominent.<sup>34</sup>

#### 4.2.2 Firm Growth as an alternate outcome.

In this section, we examine if trade disruptions as captured by our measure of *COVID Exposure* has an effect on other firm-level and county-level outcomes. We focused the main analysis on import demand given the data availability in real-time, but firm resiliency would ideally be tested with output measures as well. To this end, we next expand the outcomes in our regression to include firm growth and county employment.

Firm sales are available at the parent-level from Compustat, with the caveat that we must aggregate from the establishment to the parent-level, and can only match a subset of the firms in our main sample.<sup>35</sup> With that in mind, we follow the specification in (10), where the outcome is now the sales difference of firm i in a quarter in 2020 relative to the same quarter in 2019 (*Firm Growth*).<sup>36</sup>

Table 4 reports the results as we incrementally include county-time varying controls. The results show the expected negative effect of exposure to supply disruptions on firm growth. Importantly however, this negative effect is significantly ameliorated by the county exposure to PPP funds. Thus, local resiliency is reflected in the higher growth rate of large importers

 $<sup>^{32}</sup>$ A similar time pattern arises if we replace the *PPPE* measure with the share of community banks.  $^{33}$ See Noah (2021) and WTO (2021).

 $<sup>^{34}</sup>$ We examine the effects of PPP across product groups in Appendix A9, where effects are large and significant for both capital and consumer goods. The importance for consumer goods might represent demand factors that spill over to the local economy, while the important effects in capital goods reflect that input-output linkages might hold up better in places with loan support to small businesses.

 $<sup>^{35}</sup>$ In all, across the first three quarters of 2020, we match 1872 Compustat firms with our sample of importers (see sample refinement in Section 2.1 for details on importers). Notice that here we do not capture any private firms, and among subsidiaries of public firms we aggregate to the parent. We have 1425 firms in Q2 and 1494 in Q3, the two quarters we use in this specification.

 $<sup>^{36}</sup>$ Since the data is now at the quarterly level, we include Q2 and Q3 of 2020, with a total of 2030 firmquarter observations. Notice that the firm in this case refers to the parent firm. In order to get *Covid* and *PPP* exposures, we take these at the subsidiary level and aggregate up using an average within the parent-firm.

in counties with larger subsidies to small and medium enterprises.

We also collect data on monthly employment at the *county* level from the Quarterly Census of Employment and Wages (QCEW) database maintained by the Bureau of Labor Statistics (BLS). We calculate the percentage change in employment in each county-month from March to September relative to January (pre-Covid benchmark).<sup>37</sup> Next, we aggregate *COVID Exposure* to the county-month level by taking the weighted average of *COVID Exposure* across all firm-product combinations within the same county-month, using number of transactions per firm-pair as weights.<sup>38</sup>

The estimation results are reported in Appendix Table A10. The *COVID Exposure* coefficient is negative and significant suggesting that in counties with higher exposure to COVID disruptions, there are also larger decreases in employment compared to pre-covid employment in January. The *PPPE* coefficient is positive and significant, which is aligned with other studies on the (arguably small) positive effects of PPP on local employment.<sup>39</sup> Importantly, the interaction of *PPPE* and *COVID Exposure* is positive and significant suggesting that the negative impact of COVID related trade disruptions on county level employment is lower in counties with greater exposure to the Paycheck Protection Program.

# 4.3 PPP and County Agglomeration

In this section, we explore the role of county-level agglomeration in fostering the role of PPP's positive spillovers on local economies. In our context, agglomeration forces are present if SMEs are an important part of the local economy ecosystem. Therefore, we create measures at the county-level that proxy for the role of SMEs in the local economy.

We re-estimate equation (10) separately for low and high agglomeration sub-samples and report the results in different panels of Table 5. In panel A we report the results in counties

<sup>&</sup>lt;sup>37</sup>We find similar results if we were to use February or March as the benchmark month.

 $<sup>^{38}{\</sup>rm The}$  final sample for this specification is an unbalanced panel with 8,974 county-month observations across 1,581 counties.

<sup>&</sup>lt;sup>39</sup>e.g. See Autor et al. (2020), Granja et al. (2022), Faulkender et al. (2021), etc.

ranked as "Low" and "High" in terms of exposure to local spillovers, split by the median value *Chinitz* index and *InputOutput*. The positive significant coefficients only show up in the subsample of high agglomeration counties, consistent with our prediction that PPP is especially important in counties with a larger degree of linkages across firms.

In panel B, firms are separated based on the share of small/medium establishments in their respective counties. As a way to get balanced samples of firms and also show the effect of PPP as the share of SMEs increases, we group counties into quartiles. For the share of small establishments (less than 500 employees), the interaction coefficient is significant only for the top two quartiles. We find similar results in unreported robustness if we change the definition of small size establishments to less than 20 employees. The resiliency effects therefore are only present in counties with the largest share of small and medium sized establishments. In Appendix tables A11 and A12, we use alternate measures of agglomeration in terms of employment diversity (Nakamura and Paul, 2019; Duranton and Puga, 2001) and the distribution of firm imports (Gaubert, 2018) and find similar results.

The results suggest that across all the settings, regardless of our definitions, the effects of PPP are primarily in the highly agglomerated counties with a larger share of SMEs. This confirms the conjecture that agglomeration economies could trigger larger spillovers providing greater immunity to negative shocks among the local firms as a result of PPP exposure.

# 4.4 Alternative Identification of Spillovers

Thus far, we have relied on the PPP implementation to examine a positive productivity shock to recipients which, in the presence of local spillovers, should be reflected in the resilience of the importers which are not PPP recipients. We next augment our analysis of the spillover channel without relying on the PPP loans. For each (importing) firm, we calculate COVID Exposure for the *other firms in the county*. If the spillover channel exists, a firms response to its own COVID Exposure will depend also on shocks to other firms in the same county. Specifically, we interact the firm exposure measure with a county-aggregated Covid exposure that includes the supply shocks of all other firms in the county *excluding* firm i. We expect the interaction to be *negative* in this case if the negative shock to other firms in the same county has spillover effects that affect a firm beyond its own exposure to supply shocks. This is exactly what we find in Appendix Table A13, where the interaction is negative and significant. The result provides further evidence that the spillover channel is indeed operational and it is therefore reasonable to expect that we would observe it in response to the PPP implementation as well.

# 5 Conclusion

Governments around the world announced a slew of programs to support the recovery of businesses affected by the COVID-19 pandemic. One such program administered by the US Small Business Administration was the Paycheck Protection Program (PPP) intended to help small businesses maintain payrolls as the US economy shrank amid the coronavirus crisis. As expected, most of the studies examining the effect of the PPP program have focused on employment and survival of the businesses that were direct PPP recipients but have found mixed evidence on the effectiveness of the PPP.

This paper shows that PPP also had significant positive externalities on the local economy. Using data on the universe of import transactions in the US, we find that large firms that were not direct beneficiaries of the program had lower disruptions to imports and firm sales when located in counties that had large PPP disbursements to small firms. We address endogeneity in the disbursement of PPP loans to counties using strategies from Granja et al. (2022) and Faulkender et al. (2021) that leverage geographic differences in banking structure (small banks and community banks respectively) in the receipt and timing of disbursement of PPP funds. We find evidence consistent with agglomeration spillovers between small firms that were PPP recipients and large importing firms through their input-output linkages. We also see that PPP reduced the impact of the trade disruptions on county-level employment growth.

More generally, our study suggests that local spillover effects may have first order considerations in a cost benefit analysis of government support programs to the corporate sector.

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### Figure 1: Pictorial Representation of full empirical specification

The figure provides a pictorial representation of our full empirical specification (Equations 7-10) using an example from our data. Boeing's plant in King County, WA imports airplane and helicopter parts (HS 880330) from four main foreign suppliers. The top figure shows the supply shock of one of the suppliers - Alouette - for this product. Alouette's supply shock is a weighted average of the disruptions it faces along each of the four shipping routes it uses to ship HS 880330 to US importers (other than Boeing). The bottom figure takes the weighted average supply shocks across Boeing's four foreign suppliers of HS880330 to construct Boeing's Covid Exposure for this product. The left part of that figure expresses the local spillover effects of disruption to Boeing's imports and how that is ameliorated through PPP.

#### (a)



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### Figure 2: COVID Exposure Measure & US Import Index

COVID Exposure is an unweighted average of the COVID Exposure measure used in the analysis. Aggregate import index is sourced from CBP World Trade Monitor: https://www.cpb.nl/en/world-trade-monitor-march-2021.



Figure 3: Time Trends of PPP Effects

The figure reports coefficient estimates by month for the COVID Exposure -  $PPPE_c^{Nbr}$  interaction.  $PPPE_c^{Nbr}$  is the total received over the time period, and therefore does not vary over time.



#### Table 1: Summary Statistics

	N	Mean	S.D.	Min	P25	P50	P75	Max
Firm-Month Level								
$\Delta Import_{i,k,t}^{Nbr}$	244367	-0.051	0.722	-2.118	-0.511	0.000	0.405	2.015
$\Delta Import_{i,k,t}^{Vol}$	241920	-0.047	0.681	-2.276	-0.422	0.000	0.340	2.069
COVID Exposure	244367	0.014	0.017	-0.024	0.001	0.008	0.021	0.089
County-Month Level								
$PPP^{Nbr}$	8931	0.091	0.152	0.000	0.000	0.019	0.092	1.119
Unemp_r	8931	8.610	4.522	1.600	5.000	7.900	11.200	34.600
COVID_Case	8931	2308.577	8578.233	0.000	60.000	327.000	1397.000	267512
Chg_SB_Rev	8931	-0.869	0.883	-4.108	-1.417	-0.906	-0.402	3.745
County Level								
$PPPE^{Nbr}$	1574	0.100	0.111	-0.500	0.069	0.123	0.165	0.361
CB_Share	1574	0.458	0.288	0.000	0.231	0.474	0.667	1.000
Chinitz	1574	0.004	0.003	0.000	0.003	0.003	0.005	0.064
InputOutput	1574	0.145	0.060	0.000	0.110	0.135	0.168	0.988
$SBS_{500}$	1574	0.602	0.204	0.000	0.486	0.592	0.735	1.000

This table reports the summary statistics of the key variables used in our analysis. All variable definitions are in the Variable Appendix.

### Table 2: COVID Disruption and Import Growth

This table reports estimates from the following regression:  $\Delta Import_{i,k,t} = \beta \cdot COVID \ Exposure_{i,k,t} + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t}$ .  $\Delta Import_{i,k,t}$  are 12-mo difference in logarithm values of import for product k at firm i in month t, measured by Number of Transactions and Volume. 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	
	$\Delta Imp$	$port_{i,k,t}^{Nbr}$	$\Delta Import_{i,k,t}^{Vol}$		
COVID Exposure	-1.468***	-1.433***	-1.343***	-1.311***	
	(0.128)	(0.131)	(0.120)	(0.124)	
Firm FE	Y	Y	Y	Y	
HS FE	Υ	Υ	Y	Υ	
State-Month FE	Υ		Y		
County-Month FE		Υ		Υ	
N	227498	225659	225068	223225	
Adj-R sq	0.130	0.124	0.130	0.124	

### Table 3: Does PPP Foster Resiliency to Supply Disruption?

The table reports estimates from the following regression:  $\Delta Import_{i,k,t}^{Nbr} = \beta \cdot COVID \ Exposure_{i,k,t} + \gamma PPP_{c,(t)}^{Nbr} + \theta COVID \ Exposure_{i,k,t} \times PPP_{c,(t)}^{Nbr} + \delta X_{i,t} + \xi_i + \eta_k + \kappa_{c(s),t} + \varepsilon_{i,k,t}$ , where  $\Delta Import_{i,k,t}^{Nbr}$  are 12-mo difference in logarithm values of import for product k at firm i in month t, measured by Number of Transactions.  $PPP_{c,(t)}^{Nbr}$  includes: 1) the 1-month lagged PPP per establishment (PPP) at month t, 2) exposure to PPP (PPPE) which is time-invariant as it captures all PPP receipts in the second quarter of 2020, and 3) share of community banks for county c at the 2nd quarter of 2020 (also time-invariant).  $X_{i,t}$  is a set of interactions where we interact the time-varying county-level control variables with the COVID Exposure. Cols 1-4 use PPP Direct while col 5 uses the PPPE and col 6 uses the share of community banks as the exposure to PPP. 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
	$\Delta Import_{i,k,t}^{Nbr}$					
PPP Measure		PP	$P^{Nbr}$		$PPPE^{Nbr}$	CB Share
COVID Exposure	-1.474***	-1.695***	-2.323***	-2.210***	-2.626***	-2.893***
PPP	(0.127) 0.034 (0.055)	(0.163) 0.001 (0.057)	(0.480) -0.002 (0.058)	(0.504)	(0.542)	(0.581)
COVID Exposure X PPP	(0.055)	(0.057) $2.059^{**}$ (0.798)	(0.058) $2.449^{**}$ (0.952)	$2.262^{**}$	$4.101^{***}$	$1.379^{***}$
Chg_SB_Rev		(0.100)	(0.002) -0.001 (0.009)	(1.000)	(1.100)	(0.102)
COVID Exposure X Chg_SB_Rev			0.084	0.212	-0.149	-0.107
$Log(Covid_Case)$			(0.222) -0.006 (0.006)	(0.233)	(0.197)	(0.189)
COVID Exposure X Log(Covid_Case)			(0.000) (0.011) (0.060)	0.031	0.003	0.019
UnEmp_r			(0.000) -0.002 (0.002)	(0.002)	(0.001)	(0.001)
COVID Exposure X UnEmp_r			(0.002) $0.058^{***}$ (0.022)	$0.049^{**}$	$0.055^{**}$	$0.056^{**}$
Firm FE	Υ	Υ	(0.022) Y	(0.022) Y	Y	(0.022) Y
HS FE	Υ	Υ	Υ	Υ	Y	Υ
State-Month FE	Υ	Υ	Υ			
County-Month FE				Υ	Y	Υ
Ν	165022	165022	165022	163144	163144	163144
Adj-R sq	0.134	0.134	0.134	0.128	0.128	0.128

### Table 4: Alternative Outcome: Firm Growth

This table reports estimates from the following regression: Firm  $Growth_{i,t} = \beta COVID Exposure_{i,t} + \gamma PPPE_{\hat{c}} + \theta COVID Exposure_{i,t} \times PPPE_{\hat{c}} + \delta X_i + \lambda_i + \theta_t + \varepsilon_{i,t}$ , where Firm Growth is measured by the difference in log sales in 2nd and 3rd quarter of 2020 relative to the same quarter in 2019, for public firm *i*.  $PPP_i$  is the average exposure to PPP (PPPE) in the 2nd quarter of 2020 across all the counties where firm *i* has subsidiaries. Similarly, COVID Exposure is the average across the firms' subsidiaries in quarter *i*.  $X_{i,t}$  is a set of interactions where we interact the time-varying county-level control variables (described in the data section) with the COVID Exposure. All regressions are estimated using firm and quarter fixed effects.

	1	2	3	4	5	6	
	Growth (Sale Difference, Quarter 2020-Quarter						
	2019)						
COVID Exposure	-0.534***	-0.392**	-0.773***	-1.030***	-1.644**	-1.545**	
	(0.196)	(0.169)	(0.239)	(0.358)	(0.693)	(0.759)	
COVID Exposure X PPPE			$3.799^{**}$	$3.902^{**}$	$3.949^{**}$	$3.871^{**}$	
			(1.678)	(1.682)	(1.685)	(1.685)	
Log(Asset)		$0.015^{***}$	$0.015^{***}$	$0.015^{***}$	$0.015^{***}$	$0.015^{***}$	
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Chg_SB_Rev				0.012**	0.012*	0.007	
				(0.006)	(0.006)	(0.007)	
COVID Exposure X Chg_SB_Rev				-0.193	-0.207	-0.188	
				(0.210)	(0.211)	(0.219)	
Log(Covid_Case)					-0.003	-0.004	
					(0.003)	(0.003)	
COVID Exposure X Log(Covid_Case)					0.068	(0.088)	
II D					(0.067)	(0.070)	
UnEmp_r						$(0.002^{+})$	
COVID Euroceuro V UnEmp n						(0.001)	
COVID Exposure & Ulterinp.						(0.022)	
Firm FE	V	V	V	V	V	$\left( 0.057 ight)$ V	
Quarter FE	Y	V V	I V	I V	V V	V V	
N	2388	2312	2312	2312	2312	2312	
Adi-R sa	0.907	0.930	0.931	0.931	0.931	0.931	
Chg_SB_Rev COVID Exposure X Chg_SB_Rev Log(Covid_Case) COVID Exposure X Log(Covid_Case) UnEmp_r COVID Exposure X UnEmp_r Firm FE Quarter FE N Adj-R sq	Y Y 2388 0.907	Y Y 2312 0.930	Y Y 2312 0.931	0.012** (0.006) -0.193 (0.210) Y Y 2312 0.931	0.012* (0.006) -0.207 (0.211) -0.003 (0.003) 0.068 (0.067) Y Y 2312 0.931	$\begin{array}{c} 0.007\\ (0.007\\ -0.188\\ (0.219\\ -0.004\\ (0.003\\ 0.088\\ (0.070\\ 0.002^{\circ}\\ (0.001\\ -0.022\\ (0.037\\ Y\\ Y\\ 2312\\ 0.931\\ \end{array}$	

### Table 5: Resiliency by County Agglomeration Potential

The specification in this table follows that column 5 of of Table 3, but for subsamples based on county characteristics. In panel A, we report the results for counties with different level of input-output linkages. We use *Chinitz* index in cols 1-2, and *InputOutput* in cols 3-4. In panel B, we report results of PPP across counties with different share of small/medium establishments (defined as share of workers in establishments with less than 500 employees), where quartile 1 indicates smallest share while quartile 4 indicates largest share of small/intermediate business in the county. 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.

Panel A: Agglomeration Measured as Input-Output Linkages

	1	2	3	4		
	$\Delta Import_{i,k,t}^{Nbr}$					
Agglomeration Measure	Chin	itz	InputOutput			
	Bottom $50\%$	Top $50\%$	Bottom $50\%$	Top $50\%$		
COVID Exposure	-2.606***	-2.608***	-2.406***	-3.039***		
	(0.864)	(0.743)	(0.771)	(0.755)		
COVID Exposure X $PPP^{Nbr}$	2.880	7.340***	1.823	$10.492^{***}$		
	(1.786)	(2.601)	(1.334)	(2.598)		
Firm FE	Y	Y	Y	Y		
HS FE	Υ	Υ	Y	Υ		
County-Month FE	Υ	Υ	Y	Υ		
COVID Exposure X Control	Υ	Υ	Y	Υ		
Ν	80554	82209	83508	79254		
Adj-R sq	0.136	0.133	0.139	0.127		

Panel B: Agglomeration Measured as county-level Small/Medium Establishments Share,  ${\rm SBS}_{500}$ 

	1	2	3	4			
	$\Delta Import_{i,k,t}^{Nbr}$						
Quartile	1	2	3	4			
COVID Exposure	-2.897**	-3.318***	-1.350	-2.279**			
	(1.187)	(1.191)	(1.156)	(1.023)			
COVID Exposure X $PPP^{Nbr}$	3.087	4.227	$5.154^{**}$	$4.778^{**}$			
	(4.033)	(4.840)	(2.312)	(2.315)			
Firm FE	Y	Y	Y	Y			
HS FE	Υ	Υ	Υ	Υ			
County-Month FE	Υ	Υ	Υ	Υ			
COVID Exposure X Control	Υ	Υ	Υ	Υ			
Ν	40946	38988	42311	39875			
Adj-R sq	0.141	0.125	0.143	0.146			