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## Surviving Pandemics: The Role of Spillovers

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## Surviving Pandemics: The Role of Spillovers

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# Surviving Pandemics: The Role of Spillovers

## Abstract

What role do spillover effects play in firm resilience during crises? Using high-frequency data on over 7 million import transactions, we ask this question in the context of the large trade disruption faced by US importers in the months immediately following the initial COVID-19 shock. While US firms saw a reduction in imports due to Covid-related trade disruptions to their suppliers, these effects were lower for importers in counties that received greater loans under the Paycheck Protection Program (PPP), a government stimulus program aimed at small businesses. A one standard deviation increase in exposure to PPP reduces the effect of the supply shock faced by the firm by approximately one-fifth. These effects exist even when the importer is not a direct recipient of PPP loans. The effects are largest in counties with larger number of small suppliers and higher input-output industry linkages, and those with greater share of small and medium enterprises (SMEs). We also see similar effects of PPP preserving job growth at the county level even as the trade shock takes a negative toll on local employment. Our results point to local spillovers between SMEs that were PPP recipients and large importers as being an important determinant of firm resiliency during the pandemic.

JEL-Codes: G300, H810, R100, R120.

Keywords: agglomeration spillovers, Paycheck Protection Program, supply chains, Covid-19.

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# 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 if 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 if large importing firms that faced COVID related trade disruptions fared better when located in counties that had large disbursements to small businesses under the Paycheck Protection Program (PPP).

On the one hand, the PPP program was designed to aid small businesses keep their workforce employed and we should not expect to see any benefits accruing to large firms. On the other hand, there is increasing evidence that PPP increased the survival rates of small businesses (see [Bartik et al. \(2021\)](#), [Gourinchas et al. \(2021\)](#) and [Bartlett and Morse \(2020\)](#)), 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. In particular, we hypothesize that in areas where small firms benefited from PPP and did not shut down, large firms' imports recovered faster than in areas where small firms did not benefit as much from PPP. In addition, we 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 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. There are two important reasons why we build our study around the trade disruption. First, it allows for an exogenous supply shock (expanded on below), with plausible local spillover effects on demand. Since large companies depend on smaller businesses as both consumers and suppliers, any supply shock they face likely spills over to the local economy and feeds back as a demand shock as well. Second, the level of detail and real-time nature of the import data yields a proxy for the severity of the shock and corresponding recovery that is not available from other sources. We have detailed bill-of-lading data on over 7 million import transactions for over 1 million importers, including their HS-6 product category, maritime shipping route, shipping vessel, date, name and location of both the foreign exporter and US importer, as well as the volume, weight, and dollar value of the transactions. To this data, we merge in data at the county-level on the exposure to the PPP program.

We face two main econometric challenges in examining the effect of the PPP program in reducing disruptions to trade for large importers. The first challenge is to separate the *supply* disruption to importers due to the Covid-19 pandemic from demand effects. To isolate the exogenous component of the trade shock to US importers, we use the weighted average of the Covid exposure of their suppliers. This, in turn, is determined by the 12-month difference in shipping-route level activity of the suppliers. Changes in route-level activity captures disruptions caused by lockdowns, quarantines, and labor shortages at ports worldwide, as confirmed with port data by [Notteboom et al. \(2021\)](#).<sup>1</sup> The advantage of this measure of Covid-19 exposure is that it is 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. In addition, the measure uses direct evidence of shipping disruptions to routes that suppliers relied on in 2019 instead of using 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>2</sup>

The second econometric challenge is to address endogeneity in the disbursement of PPP loans to counties. Here, we exploit the fact that most small business lending is local (see [Brevoort et al. \(2010\)](#) and [Granja et al.](#)

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<sup>1</sup>Specifically, for each supplier, we compare the export transactions for each product across all the multiple shipping routes used to ship the product in a particular month to the transactions for the same product in the same month pre-pandemic (i.e. 12 months ago). Thus, for example, when the Shanghai-Los Angeles route experiences a sharp negative decline in the total number of transactions in April 2020 relative to April 2019, supplier-product combinations that relied on that route in 2019 will be considered “exposed” to the Covid-19 shock.

<sup>2</sup>The identification of supply shocks follows from the empirical banking literature pioneered by [Khwaja and Mian \(2008\)](#). In parallel to that literature which identifies bank shocks within firms that borrow from multiple banks, 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. To further distance ourselves from possible demand effects in the US, we also provide a robustness exercise where we measure supply shocks from changes in total transactions only at each port of lading.

(2018)) and close bank relationships were especially important in helping firms gain access to PPP funds (Li and Strahan (2020)). Following Granja et al. (2020), we construct a Bartik-style measure of geographic exposure to bank branches and the success of individual banks in distributing PPP loans. The measure captures a bank’s proficiency in providing PPP loans by comparing these loans to previous small business lending. Given the branch locations of these banks and their local market share, locations with branches that belong to more proficient banks will have higher exposure to the PPP lending.<sup>3</sup>

Using the above empirical approach, first we show that importing firms were indeed impacted by the supply disruptions. Controlling for firm, product (HS-6), and county-month fixed effects (and therefore pandemic-related health and mobility effects), we find that US importers that were more exposed to suppliers affected by route-specific trade disruptions had larger reductions in import growth. Note that the US importer-supplier transaction is not a part of the calculation of the supplier’s trade disruption along this shipping route and hence this is purely a supply disruption rather than driven by demand effects faced by the importer. A one standard deviation decrease of the suppliers’ shipping activities (or a rise in our measure of exposure) translates to a 2.5 percentage point reduction in the import growth rate a US importer. Adopting the Census classification of products by their end-use category,<sup>4</sup> we see that the disruptions to trade are widespread, affecting Capital goods, Consumer goods, and Industrials.

Second, we find that importers located in counties that received a larger amount of PPP funds had a smaller reduction in import demand for the same exposure to COVID disruptions. A one standard deviation increase in exposure to PPP reduces the effect of supply exposure by 0.4-0.5 percentage points, or approximately one-fifth of the effect of the supply shock. Importantly, as the real allocation constraints with the PPP occurred during the first tranche of the program and not after the program was replenished by early May (Bartik et al., 2021; Granja et al., 2020), the import demand boosting effect of PPP starts to show up in April, increases gradually until July, and disappears by August, when take-up is no longer constrained. Large economic effects are mainly found in capital and consumer goods. This suggests that the PPP program was successful in boosting demand within local economies and likely maintaining the production of nearby suppliers. Our specification controls for county-month fixed effects as well as other time-varying factors such as concurrent policy responses to the pandemic which might confound the influence of PPP.

Third, we find large spillover effects from the program to firms that did not receive loans. Our sample

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<sup>3</sup>Our results are also robust to instrumenting for PPP exposure using the market share of community banks as in Faulkender et al. (2021).

<sup>4</sup>End-use is a commodity classification system used by the Bureau of Economic Analysis to identify merchandise based on principal use rather than the physical characteristics of the merchandise. There are six principal categories including Foods, feeds, and beverages; Industrial supplies and materials; Capital goods, except automotive; Automotive vehicles, parts, and engines; Consumer goods; and Other goods.

is made up of mostly large firms, since firms that engage in trade are typically larger than purely domestic firms (Bernard et al., 2009)) and most do not receive PPP. When we match our sample of importers to PPP recipients, we find that a large part of the import demand is driven by firms that do not receive any PPP at all, so that their higher imports are driven by the broader economy.

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 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. In counties where there are greater linkages between small firms and large importers, we should expect to see a greater disruption in demand and thus a greater role for the PPP program.

To provide evidence on the channels through which PPP loans to small firms benefit co-located large firms, we look at several different but related measures of within-county linkages. First, we use the *Chinitz* index from Glaeser and Kerr (2009) which reflects areas with many small suppliers and interdependencies among industries. Chinitz (1961) argued that the presence of small, independent suppliers leads to increasing returns by fostering productive consumer/supplier linkages and thus helps explain why industrially diversified cities such as New York were much more entrepreneurial than cities dominated by a single oligopolistic industry such as Pittsburgh. In our context, counties with a higher Chinitz index should have a greater presence of small, heterogeneous suppliers and thus greater PPP loans to these areas must have a larger effect on importers. Second, as an additional measure of industrial linkages, 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. (2021) which captures the share of establishments that are small and medium enterprises (based on employment size) in a county. Finally, following Rosenthal and Strange (2003) and Duranton and Puga (2001), we measure industrial *Diversity* of a county by the inverse of a Herfindahl index of sectoral concentration of local employment. In the framework of Duranton and Puga (2001), *diverse* and *specialized* economies coexist with both types of cities having separate agglomeration benefits.<sup>5</sup>

We find that the effects of PPP on reducing trade disruption 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

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<sup>5</sup>Relatedly, these types of counties might also be more exposed to a Keynesian multiplier effect from supply shocks, as demand shortfalls spillover to other sectors (e.g. Guerrieri et al. (2020) highlight the role of multiple sectors for a Keynesian effect to be possible). In contrast, in counties represented by few large firms in few sectors, the possibilities for spillovers are limited.



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 triple the import demand response to the same level of PPP relative to firms in the other counties.

To see if the effects exist beyond trade patterns, as an alternate dependent variable, we also look at county-level employment growth. Although this can only be done at a more aggregate level, we once again find that trade driven COVID disruptions reduce local monthly employment in March through September relative to January of 2020 but this effect is ameliorated in counties that received a large amount of PPP funds.

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. \(2020\)](#), [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. \(2021\)](#)). 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 \(2020\)](#) establish that a firm's global sourcing strategy predicts stock market returns, while [Ding et al. \(2020\)](#) 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 literature on the effect of the PPP in alleviating the impact of the Covid-19 pandemic on the corporate sector. Several papers including [Autor et al. \(2020\)](#), [Granja et al. \(2020\)](#), and [Chetty et al. \(2020a\)](#) find positive, albeit small effects on employment. [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.<sup>6</sup> More robust are the findings on survival resiliency due to the program as highlighted in [Gourinchas et al. \(2021\)](#) and [Bartik et al. \(2021\)](#). [Wang et al. \(2020\)](#) show that SME failures have been limited by the program. Additionally, [Bartlett and Morse \(2020\)](#) point out that the increase in survival probability is mainly for micro-businesses. 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 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>7</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 [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. [Greenstone \(2010\)](#) find agglomeration spillovers in counties that “win” entry of large manufacturing plants while [Bloom et al. \(2013\)](#) and [Criscuolo et al. \(2019\)](#) find evidence for spillovers from R&D and investment subsidies to firms. Others such as [Engelberg et al. \(2018\)](#) highlight the role of information spillovers in geographically proximate firms. 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.<sup>8</sup> 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 [Barthlmy et al. \(2020\)](#).

Relatedly, there is a broader 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 \(2017\)](#), [Bloom et al. \(2013\)](#)). Others such as [Wallsten \(2000\)](#) and [Lerner \(2009\)](#) contend that SBA R&D subsidy programs crowd out firm-financed R&D or allocate funds inefficiently.<sup>9</sup> None of these papers are focused on estimating the ex-

<sup>6</sup>[Doniger and Kay \(2021\)](#), [Bartik et al. \(2020\)](#), and [Kurmman et al. \(2021\)](#) also find positive employment effects from PPP.

<sup>7</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. \(2021\)](#) highlight that policies that discourage expansion might be counter-productive ([Martin et al., 2017](#)). 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., 2020](#); [Acharya and Steffen, 2020](#)) and therefore it is unlikely that it led to the crowding out of ineligible firms.

<sup>8</sup>More recently, studies have used firm-level data to show how buyer-seller linkages lead to propagation of shocks that are set off by the financial sector ([Huber, 2018](#); [Giannetti and Saidi, 2019](#); [Alfaro et al., 2021](#); [Bigio and Lao, 2020](#); [Costello, 2020](#); [Demir et al., 2020](#)).

<sup>9</sup>Studies examining the effects of loan guarantee programs outside the US include [Gonzalez-Uribe and Wang \(2020\)](#) (UK),

ternalities from the small business lending programs on other firms. Since PPP acts in a similar manner to these SME lending programs, our analysis aims to provide further evidence of spillovers using a novel outcome (import demand). If firms receiving PPP were indeed credit-constrained due to disruptions from the pandemic, the program should act to stabilize not just employment but the import of intermediates through agglomeration spillovers highlighted in the previous work.

## 2 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  $c$ :

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

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  $\mu$  representing the share of flexible inputs in production.  $\zeta_i$  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  $A_c = f(F_c)$ .<sup>10</sup>

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. \(2020\)](#); [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](#)).<sup>11</sup> Finally, the agglomeration term also allows that supply

[Bertoni et al. \(2019\)](#) (sample of EU countries), [Core and De Marco \(2021\)](#) (Italy), and [Barrot et al. \(2019\)](#) (France).

<sup>10</sup>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.

<sup>11</sup>For example, studies refer to [Marshall \(1890\)](#)'s idea that locational proximity could reduce costs in "people, goods, ideas"

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., 2020; Verner and Gyngysi, 2020).<sup>12</sup>

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.<sup>13</sup> The profit maximization of firm  $i$  is given by:

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

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.<sup>14</sup> 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:

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

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.<sup>15</sup>

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:

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

Assuming everything else held constant,  $\zeta_{exposed}$  decreases with the level of exposure (defined below), result-

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(Ellison et al., 2010; Combes et al., 2012).

<sup>12</sup>One mechanism highlighted in this literature that is especially relevant for aggregate productivity is a reduction in productivity-enhancing investment, as suggested in Queraltó (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).

<sup>13</sup>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).

<sup>14</sup>There is only one flexible factor, so the FOC is simply:  $\frac{d\pi_i}{dF_i} = \mu f_i A_c F_i^{\mu-1} K_i^{1-\mu} - p_c^F = 0$ .

<sup>15</sup>Specifically, the last term is given by:  $\kappa = \frac{1}{(1-\mu)} \log \mu + \log K_i - \frac{1}{1-\mu} \log p_c^F$ .

ing 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.*

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 at a high frequency during the height of the pandemic with our detailed bill-of-lading data.<sup>16</sup>

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  $\lambda_c$  is elasticity of county productivity due to a change in local demand for flexible inputs. By construction,  $F_c = \sum_i F_i$ .<sup>17</sup> 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  $\zeta_i$ ) as:

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

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

<sup>16</sup>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.

<sup>17</sup>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.

effect:

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

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  $\lambda_c$ . Therefore, as our second hypothesis we have:

*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.<sup>18</sup> 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.<sup>19</sup> 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  $\lambda_c$  as indirect spillovers increase with this parameter. Section 5.4 tests the positive association between agglomeration economies and the size of the PPP benefits.

### 3 Data and Measures of Exposure

#### 3.1 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>20</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),

<sup>18</sup>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.

<sup>19</sup>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.

<sup>20</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>

a unique identification number for each importer (importer ID), their foreign shippers, description of the traded goods, quantity imported, shipment arrival date, ports (loading and unloading) 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>21</sup> We exclude 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.). After applying the above filters, 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.

For our analysis, 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:

$$\Delta Import_{i,k,t}^{Nbr} = \log(Import_{i,k,t}^{2020}) - \log(Import_{i,k,t}^{(17-19)Avg}) \quad (6)$$

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.<sup>22</sup> 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>23</sup>

Figure 1 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.

Finally, the sample in our benchmark analysis is subject to the following cleaning procedures. First,

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<sup>21</sup>Our results are robust to restricting the sample to firms with imports in just 2019 and 2020.

<sup>22</sup>A similar definition will apply to suppliers, covered in section 4.1.

<sup>23</sup>We also have information on dollar value of trade but this is missing for a more sizeable portion (30%) of the sample.

we exclude observations with missing values on  $\Delta Imports^{Nbr}$ , our constructed Covid supply exposure, or control variables (described below). Second, a novel aspect that we leverage from the BoL data is the detailed information on importers, but some caveats apply (see [Flaen et al. \(2021\)](#)) and we restrict the sample to account for these. We exclude large logistic and freight firms from the sample.<sup>24</sup> [Flaen 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 245,234 observations over 49,421 firms, 3,344 product codes, in 1,581 counties in the U.S, over the months March-September 2020.

### 3.2 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.

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>25</sup> For each PPP loan transaction, we have the name and address of both borrowers and lenders, borrower’s 6-digit NAICS industrial code, loan amount, approval date, and other complementary information such as loan status, demographic information of the owner for the borrowing companies. In our analysis, we leverage both the across-time variation in PPP loans across counties as well as the full PPP disbursement at the county-level.

We use the following measures to proxy for the geographic variation in PPP disbursements. First, we compute  $PPP^{Nbr}$  as the total number of PPP loans approved in each county-month scaled by the total

<sup>24</sup>We drop importers that are on list of the largest logistic firms in US (here is a [list of the US top 50 third party logistic firms](#) compiled by Armstrong & Associates, Inc, a leading third-party logistics (3PL) market research company.) or if the importer name contains the words "logistic", "distribution", or "freight".

<sup>25</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.



number of establishments in each county in 2018 (pre-pandemic).<sup>26</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 these measures is that they 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. \(2020\)](#), 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.<sup>27</sup> 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 \(2020\)](#) 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’ exposure 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>28</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  $q$  as:

$$PPPE_{b,q} = \frac{\text{Share PPP}_{b,q} - \text{Share SBL}_{b,q}}{\text{Share PPP}_{b,q} + \text{Share SBL}_{b,q}} \times 0.5 \quad (7)$$

<sup>26</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.

<sup>27</sup>[Petersen and Rajan \(2002\)](#) however show that the adoption of information and credit scoring technologies in the 1980s and 1990s has increased the average distance between banks and borrowers.

<sup>28</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*.

where  $\text{Share PPP}_{b,q}$  and  $\text{Share SBL}_{b,q}$  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:

$$PPPE_{c,q} = \sum_b w_{b,c} PPPE_{b,q} \quad (8)$$

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 7.

Finally, the previous measure is aggregated across time such that the benchmark measure of PPP exposure is  $PPPE_c$ . This is done for several reasons. First, the quarterly measure would require us to pool three months of monthly import data to match with when the PPP funds are dispersed. 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 throughout the program, which should be dominated by the initial differences. We therefore capture the average effect on imports across all months and also interact this measure with month indicators. In the latter, we hypothesize that the effects increase over time for the first few months and then should disappear.

We note that our aim differs from [Granja et al. \(2020\)](#), who intend to show the misallocation of PPP loans, especially in the first round of the first wave. However, the finding in their paper that PPP receipts at the regional level are uncorrelated with the "need" for funds is important for our analysis. 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 their finding that the allocation of PPP is independent of demand. As robustness, our results will also feature a specification where we instrument for PPP exposure using the market share of community banks as in [Faulkender et al. \(2021\)](#).

### 3.3 Measures of Local Linkages and Small Firm Share

If positive effects of PPP are indeed due to it countering the negative spillovers, then this should be felt strongest in the localities that are most prone to the type of spillovers identified in section 2. To test whether

this is the case, we employ several different measures of linkages and the importance of small firms at the county level in order to evaluate the heterogeneous effects of PPP across different regions based on the expected prominence of spillover opportunities. The different measures aim to classify counties according to their ex-ante exposure to mechanisms summarized in the introduction. We expect agglomeration economies to be fundamental in the PPP loans’ spillover potential in places where we can identify a key role for firm-to-firm linkages, have an important share of small and medium enterprises most exposed to the pandemic, and where sector diversity makes multiplier effects from cross-sector demand shocks more likely. With this in mind, we bring forward various measures that allow us to stratify counties by their exposure.

To start, we employ two measures to capture the input-output linkage across industries within a county. The 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. In this setting, a negative supply shock can spillover across firms and industries. 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:

$$Chinitz_{h,c} = \sum_{l=1, \dots, L} \frac{Firms_{l,c}}{E_c} Input_{h \leftarrow l} \quad (9)$$

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.

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. As paraphrased in [Ellison et al. \(2010\)](#), “Marshallian” factors of industrial agglomeration work in reducing transport costs of goods, people, and ideas. Although the latter two relating to labor market pooling and intellectual spillovers, are likely more important in the longer-term, the cost of goods in a local network can

change very quickly as relationships are broken. 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:

$$Input_{h,c} = \sum_{l=1,\dots,L} \frac{E_{l,c}}{E_c} Input_{h \leftarrow l} \quad (10)$$

$$Output_{h,c} = \sum_{l=1,\dots,L} \frac{E_{l,c}}{E_c} Output_{h \rightarrow l} \quad (11)$$

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>29</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.<sup>30</sup> Specifically, with county-industry level employment data we follow [Denes et al. \(2021\)](#) to construct the share of establishments with fewer than 20(500) employees ( $SBS_{20}(SBS_{500})$ ).<sup>31</sup> Specifically:

$$SBS_{E,c} = \frac{N_{emp \leq E,c}}{N_{total,c}} \quad (12)$$

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  $SBS_{20}/SBS_{500}$  falls into the  $i$ th quartile by each measure.  $Q_{20(500)} = 1$  indicates that the county has the smallest share of small/medium enterprises, while

<sup>29</sup> $Input_{h \leftarrow l}$  and  $Output_{h \rightarrow l}$  provide us information on the importance of each industry to the local input-output networks.

<sup>30</sup>See [Delgado et al. \(2010\)](#) and [Glaeser et al. \(2015\)](#).

<sup>31</sup>The former cutoff is the one used in [Denes et al. \(2021\)](#) and captures micro-businesses such as those highlighted by [Bartlett and Morse \(2020\)](#) that most benefited from PPP. The latter number 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.

$Q_{20(500)} = 4$  indicates that the county has the largest share of small/medium enterprises.

Finally, we follow [Nakamura and Paul \(2019\)](#) and proxy agglomeration by industrial employment diversity. We use the inverse of Herfindahl-Hirschman Index (HHI) and construct the variable *Diversity* using the 2018 BDS data as follows:

$$Diversity_c = (\sum_h (s_{h,c}^2))^{-1} \quad (13)$$

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

The higher value of *Diversity* <sub>$c$</sub>  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.<sup>32</sup> 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<sup>th</sup>(75<sup>th</sup>)(95<sup>th</sup>) 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.<sup>33</sup>

Note that we use several different categorizations to stratify firms in counties with high and low agglomeration - counties above/below median in terms of the *Chinitz* and *InputOutput* measure; quartiles of counties for share of small firms; counties at 50<sup>th</sup>(75<sup>th</sup>)(90<sup>th</sup>) percentiles of *Diversity* - to ensure that we have a relatively even number of firm observations in each of these buckets. For example, most import transactions are in the top 5% most diverse counties, but about half of the observations are in the top half of counties with the highest exposure to local intermediate inputs.

<sup>32</sup>In a related result, [Rosenthal and Strange \(2003\)](#) find that diversity encourages new establishment births.

<sup>33</sup>In the Appendix 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. Our estimation results from this method are reported in the Appendix.

### 3.4 Other Data

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, *Un-Emp\_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., 2020b). 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. Panel A shows that on average, the monthly import transactions (volume) reduced by 5%(5%) in 2020 compared to the 2017-2019 average for the same month, for the firms in our sample. Panels B and C report the summary statistics for different measures of PPP in a county. Panel B shows that on average, nine out of every 100 establishments within a county receives a PPP loan in a month.

The monthly average unemployment rate is 8.61% for the counties in our sample and there are 2302 confirmed Covid cases on average. In addition, the monthly average revenue change of small businesses is -0.87%, confirming the negative effects of Covid on the operation of small business.

## 4 Empirical Specification

In this section we detail out the different steps of our empirical specification. First, we construct a measure of *Covid Exposure* for US importers. The aim is to identify a supply shock based on importers' reliance of foreign exporters, while purging any local demand effects. We then use this measure to quantify its effect on import growth of a US importer and the degree to which local PPP expenditures mitigate the firm-specific effects.

Figure 2 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 14 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 (Equation 15 below). We repeat this process to compute the monthly supply shock for each of the other foreign suppliers of Boeing.

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 16 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 18 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.

## 4.1 COVID Exposure

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.<sup>34</sup>

We are interested in the networks, or relationships with suppliers, that are established before the onset of the pandemic and hence we use relationships that exist anytime between 2017 and 2019. We interpret

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<sup>34</sup>This technique parallels [Khawaja and Mian \(2008\)](#)’s novel method of isolating supply side bank liquidity shocks by focusing on firms borrowing from multiple banks which differ in their exposure to liquidity shocks. Our variation across routes within suppliers is akin to their comparison of loan demand across banks by the same firm.

a negative shock to the established suppliers as a negative productivity shock to US importers, implicitly assuming relation-specific fixed costs as modeled in [Antras \(2003\)](#) and [Bernard et al. \(2018\)](#). This is also consistent with the finding of persistence in US customs data in [Monarch \(2021\)](#) who shows that around one half of the buyer-supplier links in a given year persist into the next year.

To measure the supplier shock, one could use the change in total exports for each supplier to the US in a specific month in 2020 relative to previous years, but of course it is possible that suppliers' exports are endogenous to the demand of US buyers. If suppliers specialize in particular markets, the supplier-specific outcome may be correlated with factors that affected US buyers through channels other than just pandemic-related shipping disruptions. It might be the case that importers based in Los Angeles buy from a related set of suppliers pre-pandemic and when the pandemic hits there is a larger than average drop in demand in LA, which disproportionately hurts those suppliers. Instead, we isolate route-specific disruptions by leveraging variation in exports by individual suppliers across multiple routes with supplier-product-time fixed effects.

Covid-induced shipping disruptions therefore reflect delays and bottlenecks across routes, which differ in their exposure to Covid at a particular time. This 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. [UNCTAD \(2020\)](#) describes the pandemic in stages: after the usual Chinese New Year stoppages, these were extended for extra weeks with blank (canceled) sailings continuing, then the cargo that was originally scheduled to be transported from the Far East got delayed by the lockdown in Wuhan and was transported with a lag. This was followed by the COVID-19 outbreak outside China and the impact of lockdowns and restrictions on economic activity in Europe and North America. [Notteboom and Pallis \(2021\)](#) and [Notteboom et al. \(2021\)](#) report on the number of blank sailings, with a survey of ports between April and July indicating that over 50% had container vessel calls down by more than 5%.<sup>35</sup> The cancellations started before March and by April/May, carriers were withdrawing up to 20% of their network capacity on the main trade lanes and idling more than 2.7 million TEU of fleet capacity (or 11% of the world container fleet). Although this might reflect some reduction in demand by consumers, freight rates stayed steady and even increased by May ([Notteboom et al. \(2021\)](#), Figure 6), as container ships mostly traveled full.<sup>36</sup> This suggests that the cancellations were due to supply disruptions along the routes due to pandemic-related measures which led to many sailings being canceled or delayed.<sup>37</sup>

Our aim is to *avoid* a reliance on imperfect health or mobility data to capture pandemic disruptions

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<sup>35</sup>These surveys also indicate that operations were much improved by September, consistent with the pickup in world trade.

<sup>36</sup>As robustness, we attempt to further reduce the effect of demand by replacing route disruptions with only the port of lading.

<sup>37</sup>The aforementioned surveys report that about half of ports had imposed extra restrictions on container vessels in March-May, though this had improved by mid-May.



since these are not uniformly collected.<sup>38</sup> Instead, the observed reduction in trade from a specific route is an indicator that the pandemic impedes certain operations that a supplier relies on. For supplier  $j$ , exporting product  $k$  along route  $r$  in month  $t$ , we construct a *Supply Shock* $_{j,k,r,t}$  to be used as an instrument for Covid Exposure of importers  $i$ , by isolating exogenous route-specific disruptions that are felt by suppliers. Specifically, we estimate the following specification to estimate how route-specific disruptions affect the suppliers:

$$\Delta \log(\text{Supply}_{j,r,k,t}) = \beta \Delta \log(\text{Route Transactions}_{r,t}^{-j}) + \mu_{j,k,t} + \nu_{j,r,k,t} \quad (14)$$

where  $\Delta \log(\text{Supply}_{j,r,k,t})$  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  $\Delta \text{Route Transactions}_{r,t}^{-j}$  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  $\Delta \text{Route Transactions}_{r,t}^{-j}$  captures all trade disruptions along that particular route.  $\mu_{j,k,t}$  controls for supplier  $\times$  product  $\times$  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 component of the disruption faced by the supplier for product  $k$  along this particular route.

Table 2 reports the estimation results of the specification in Equation 14 for  $\Delta \log(\text{Supply}_{j,r,k,t})$  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<sup>39</sup> both in number of transactions and volume, in 2020 relative to either time period. Specifically, the coefficient in column 1 is 0.117, suggesting that for a 10% reduction in total route activity, suppliers reduce their activity by about 1.17% from that route. 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>40</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

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<sup>38</sup>We note that Berthou and Stumpner (2021) find that the intensity of lockdowns in exporting countries are correlated with trade partners reducing their imports, and most strongly before the summer of 2020. Their result, using more aggregate data from the Trade Monitor, is consistent with our interpretation that the variation we exploit is picking up a supply shock.

<sup>39</sup>Note that we discount the previous activity by supplier  $j$ .

<sup>40</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.

$\Delta \log(\text{Supply}_{j,r,k,t})$  across all the routes used by firm to ship a product, weighted by the importance of each route in a firm-product combination. Specifically, we define the supplier-product shock as:

$$\text{Supply Shock}_{j,k,t} = \sum_r \rho_{r,j,k,t} \Delta \log(\widehat{\text{Supply}}_{j,r,k,t}) \quad (15)$$

where  $\rho_{r,j,k,t}$  is the share of average monthly usage of a route in the total transactions for the supplier-product in a month during 2017-2019, and  $\Delta \log(\widehat{\text{Supply}}_{j,r,k,t})$  is estimated values from (14).<sup>41</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  $i$  buys product  $k$  from  $j = 1, 2, \dots, N$  suppliers in month  $t$ , firm  $i$ 's Covid exposure is given by:

$$\text{COVID Exposure}_{i,k,t} = -1 \times \left( \sum_{j=1}^N \omega_{i,j,k,t} \times \text{Supply Shock}_{j,k,t} \right) \quad (16)$$

where  $\omega_{i,j,k,t}$  is the share of  $i$ 's total transactions in product  $k$  that come from supplier  $j$  in month  $t$ .<sup>42</sup> 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 value of the *COVID Exposure* indicates importing firms face more disruptions from COVID.

Panel A of Table 1 shows that the sample average COVID Exposure is 0.013, suggesting that the predicted disruption-related decline in supplier shipments is 1.3%. 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 3 displays the average exposure measure along with the U.S. aggregate import index.<sup>43</sup> 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).

## 4.2 Impact of COVID Exposure and Imports

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

$$\Delta \text{Import}_{i,k,t}^{\text{Nbr}} = \beta \cdot \text{COVID Exposure}_{i,k,t} + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t} \quad (17)$$

<sup>41</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 (14), 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*.

<sup>42</sup> $\omega_{i,j,k,t} = \frac{\text{transactions}_{i,j,k,t}}{\sum_{j,k,t} \text{transactions}_{i,j,k,t}}$ , constructed using the same month in 2019.

<sup>43</sup>As reported by CBP World Trade Monitor: <https://www.cbp.nl/en/world-trade-monitor-march-2021>.

where  $\Delta Import_{i,k,t}$  and the *COVID Exposure* $_{i,k,t}$  are defined in sections 3.1 and 4.1 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>44</sup>

It is important to highlight that *COVID Exposure* includes the disruptions to each firms' suppliers, but does not include any demand effects due to the pandemic. For example, it is plausible that a company such as Boeing also faced lower demand for its 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. The key coefficient of interest,  $\beta$ , captures the magnitude of firms' import activities being affected by COVID disruptions. From our first *hypothesis* in the theoretical motivation, we expect that  $\beta < 0$  because a more disruptive *COVID Exposure* – which reflects larger negative shocks to suppliers – translates to a larger reduction in productivity.

### 4.3 PPP and COVID Exposure

To explore whether the trade disruptions are ameliorated by PPP, we estimate the following equation:

$$\Delta Import_{i,k,t}^{Nbr} = \beta \cdot COVID\ Exposure_{i,k,t} + \gamma PPPE_c + \theta COVID\ Exposure_{i,k,t} \times PPPE_c + \delta X_{i,t} + \xi_i + \eta_k + \kappa_{c(s),t} + \varepsilon_{i,k,t} \quad (18)$$

where  $PPPE_c$  is the time-invariant measures of PPP in county  $c$  described in section 3.2 and  $X_{i,t}$  is a set of interactions where we interact the time-varying county-level control variables described in section 3.4 with the *COVID Exposure*.<sup>45</sup>

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

<sup>44</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 large logistic companies.

<sup>45</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.

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. More specifically, our second *hypothesis* in the theoretical motivation predicts that  $\theta$  is positive for *non-recipients* of PPP as well, which we will test with firm-level data on PPP receipts. We assume a counties' receipt of PPP is not driven by repercussions from the supply shock itself, which we test in Appendix Table A3.<sup>46</sup>

Evidence for these positive spillovers of the program is consistent with  $\lambda_c > 0$  in equation (5). The second part of our *hypothesis* for indirect spillovers involves testing for heterogeneity in the effect of PPP given different levels of  $\lambda_c$ . Proxies for  $\lambda_c$  are the measures of agglomeration described in section 3.3. In order to test for heterogeneous effects, we follow the specification in (18), but with sub-samples that reflect the level of agglomeration in a county.

## 5 Results

### 5.1 Import Growth and COVID Exposure

In this section, we start to explore how the disruptions from the supply side faced by the importers (i.e. the *COVID Exposure*) affect their import activities.

The estimation results of the baseline model are reported in the panel A of Table 3. 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 2.5 percentage points ( $1.478 \times 0.017$  – 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 (the outcome in this specification) in our sample is equal to -5%, this magnitude is around 50% of the mean. These results imply a strong disruptive effect of COVID Exposure on the import activities of US firms. When we examine the volume of imports as our dependent variable in columns 3 and 4, we find very similar results. An increase in Covid exposure reduces the import volume of US importing

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<sup>46</sup>In that case we regress the county-level changes in import transactions on the future PPP exposure. We find that there is no significant correlation between counties that see larger import shocks and counties with higher PPP exposure.

firms. Going forward, we will present results with  $\Delta Imports^{Nbr}$  as our main dependent variable but note that results are very similar if we were to use  $\Delta Imports^{Vol}$ .

Next, we explore the heterogeneous effects of COVID disruption on imports across different types of products. We obtain the crosswalk from the US Census<sup>47</sup> 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 3. 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.<sup>48</sup> 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 (2021). 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.

Our results are robust to a number of robustness checks. 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.<sup>49</sup> We mitigate this effect by leveraging disruptions on the *port of lading* only. Specifically, in equation 15, 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

<sup>47</sup>The crosswalk is directly available at <https://www.census.gov/foreign-trade/reference/codes/index.html#enduse>.

<sup>48</sup>The economic effects are calculated by multiplying the coefficients in column 8,9 and 11 with the standard deviation of COVID Exposure for each enduse category in Table A2.

<sup>49</sup>Our baseline analysis attempts to control for this with supplier fixed effects in equation 15. 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.

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 A4 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 17 with total parent imports linked to their supply shock. Results are presented in Table A5, and it is clear that aggregating subsidiaries to their parent level has very little impact on the estimated *COVID Exposure* effect on imports.

Finally, some firms request the US Customs to redact some address locations from the Bill of Lading in some years. To deal with this issue, we first count the number of unique addresses for a firm in every year in our sample period. If there is a 25% or larger change in the number of addresses associated with a firm from year  $t-1$  to  $t$ , we flag the firm as a potential redactor. We estimate our baseline model dropping all redacted firms in Table A6 in the appendix. Our results with this smaller sample are consistent with those reported in Table 3 for all specifications.

Overall, this section shows that greater exposure to COVID-19 through global supply chains is costly for firms.

## 5.2 Does PPP Ameliorate COVID Disruption?

In this section, we explore our second hypothesis of Section 2, 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, 2020) 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, large importers recovered faster, then we should observe smaller COVID disruptive effects on imports for firms located in regions with higher PPP exposure.

We report the estimation results for the sample of all firms (or subsidiaries) in Table 4. Columns 1-6 reflect the results with 1-month lagged  $PPP_{c,T}^{Nbr}$  in a county as the PPP measure.<sup>50</sup> Due to the endogeneity concerns of the raw PPP measure that we outline above, we replace the  $PPP^{Nbr}$  with  $PPPE^{Nbr}$  in column 7, constructed following the procedure in Granja et al. (2020). State-month fixed effects are used in the first four columns, while county-month fixed effects are used in the latter three. Interaction terms between *COVID Exposure* and control variables are added in columns 2, 4, 6, and 7.

The coefficients for interaction terms 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. Importantly, 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. It also holds when we include county level controls such as the number of known Covid cases and the unemployment rate, interacting these with the *Covid Exposure* measure. Notice that results are similar with state-month 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. We view column 7 as the most restrictive and therefore our baseline specification. In terms of economic effects, one standard deviation increase in  $PPPE^{Nbr}$  mitigates the COVID disruptive effects by 0.4 percentage points.

The implication of the interaction we examine can be tied to the hypothesis based on factor demands in equation (3). We interpret the *Covid Exposure* shock as a reduction in  $\zeta_i$ , the firm-specific productivity shifter, which reduces both flexible inputs,  $F_i$ , and correspondingly the local area productivity shifter,  $A_c$ , through the general equilibrium effect. Differential effects on changes in imports given the same reduction in  $\zeta_i$  can therefore be attributed to heterogeneous responses in the spillover term. Our finding suggests that PPP was successful in limiting further spillovers from the negative shock felt by firms relying on disrupted global supply chains. With county-month fixed effects, we compare import changes relative to March (when the PPP measure is zero by design) across firms located in different counties and control for any concurrent county-specific shocks.<sup>51</sup>

A potential explanation of the above results is that PPP directly enables recipients sustain their import demand via additional funds and not necessarily via stimulating a more COVID-immune local environment. If PPP can only affect direct recipients, we would expect the ameliorating effects of PPP to be strictly observed in a sub-sample of firms which are the direct recipients of PPP loans. 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

<sup>50</sup>Notice in this case we do allow  $PPP$  to be time varying.

<sup>51</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.

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.

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, meaning a majority (70.4%) of firms in the sample didn't receive any PPP loans.<sup>52</sup> We estimate the same model as described in Equation (18) but for the sub-sample of firms did not receive PPP loans. The estimation results are reported in panel B of Table 4. Similar to the estimation results in panel A, the coefficients of the interaction terms are also positive and significant – a one standard deviation increase in  $PPPE^{Nbr}$  mitigates the COVID disruptive effects by 0.5 percentage points. This indicates that PPP generates resiliency to the COVID disruption to non-direct recipients as well. Overall, our results indicate that PPP stimulates immunity within the local economy that helps firms build resiliency towards the COVID shock.

### 5.2.1 Time Trends of PPP's Effects

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., 2020). Finally, one would expect the spillover effects to not be immediate given the lag in firm closures, etc. For this reason we explore more in detail the dynamic effects of PPP by estimating the following specification:

$$\Delta Import_{i,k,t}^{Nbr} = \beta \cdot COVID\ Exposure_{i,k,t} + \sum_t \theta_t \cdot COVID\ Exposure_{i,k,t} \times PPPE_c^{Nbr} + \delta X_{i,t} + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t} \quad (19)$$

where  $PPPE_c^{Nbr}$  is the county level exposure to the program during the *whole* sample period, and coefficients  $\theta_t$  capture the effects of PPP on COVID disruptions during each month relative to March, a month before the initiation of PPP program. The inclusion of state (county)-month fixed effects allows us to compare the time trends for firms within the same counties. The expectation is that the positive spillover effects of PPP would increase over time, but then taper off even before the program comes to an end by early August as funds were no longer a binding constraint.

<sup>52</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 report the estimation results of the time trends in Table 5. The first two columns report the estimated results using the entire sample, while we use the sub-sample with firms didn't receive PPP in columns 3-4. In each setting, we estimate the results using state-month fixed effects and county-month fixed effects separately. In addition, we also plot the coefficients of the interaction terms in each month in Figure 4. The estimation results confirm the fact that PPP indeed ameliorates the disruptive effects of COVID on imports, with the effect decrease in size over time. Although the effect is present in April, it grows significantly over time, with the effect about 50% larger in May relative to April and peaks in July with a 80% larger coefficient. The difference of  $PPPE^{Nbr}$  (time-invariant in this specification) between the top and bottom 25th percentiles counties is 0.13, so that by July an increase in PPP funding implies the reduction in imports is 0.75 percentage points ( $5.741 \times 0.13$ ) lower for firms in counties in the 75th percentile of PPP exposure relative to those in the 25th percentile. This magnitude is almost 30% of the effect of the supply shock. By August the effects of PPP are negligible and this continues into September, consistent with the fact that PPP officially ends on August 8th and most of the money is distributed well before then. This also aligns with the fact that trade starts to rebound in the third quarter of 2020,<sup>53</sup> and thus the effects are less prominent.

As robustness check, we have also adopted the the method of [Faulkender et al. \(2021\)](#) that replaces the  $PPPE^{Nbr}$  measure with the county-level community bank share to re-estimate Equation 19. The estimation results are reported in Table A7 and are similar to our main results.

Finally, we examine the effects of PPP across product groups using the definitions detailed above. We re-estimate the Equation 18 across all end-use product groups and report the results in Table 6. Across all the settings, we use the  $PPPE^{Nbr}$  as the measure of regional PPP, county-month fixed effects and add the interaction terms of *COVID Exposure* with all control variables. The coefficients of the interactions are positive for all products, although vary in size.<sup>54</sup> Most importantly, they 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. Economically speaking, the results suggest that import growth rates of Capital goods Consumer goods, and Industrial supplies are each helped significantly by a larger presence of PPP in the county.

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<sup>53</sup>See [Noah \(2021\)](#) and [WTO \(2021\)](#)

<sup>54</sup>Partly, this might represent different sample sizes across groups.

### 5.3 PPP and County Employment

In this section, we examine if trade disruptions as captured by our measure of *COVID Exposure* has an effect on other economic outcomes. We 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>55</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. The final sample for this specification is an unbalanced panel with 8,974 county-month observations across 1,581 counties. Formally, we estimate the following model:

$$Emp_{c,t} = \beta COVID\ Exposure_{c,t} + \gamma PPPE_c + \theta COVID\ Exposure_{c,t} \times PPPE_c + \delta X_{c,t} + \lambda_{s,t} + \varepsilon_{c,t} \quad (20)$$

where *Emp* is the percentage change of county monthly employment relative to January; *PPPE* is the PPP exposure for county *c* during the second quarter of 2020; *X* is the vector of the previous control variables including the lagged unemployment rate, the number of confirmed COVID cases, and the monthly change of small business revenue (each interacted with *Covid Exposure*); and  $\lambda_{s,t}$  are state-month fixed effects to account for all other factors that vary at the state-month level.

The estimation results are reported in Table 7. 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>56</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.

### 5.4 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. Our theoretical framework in section 2 argued that PPP will act upon the county-level statistic,  $A_c$ , by reducing the county-level losses of employment and imports through its lifeline

<sup>55</sup>We find similar results if we were to use February or March as the benchmark month.

<sup>56</sup>e.g. See Autor et al. (2020), Granja et al. (2020), Faulkender et al. (2021), etc.

to smaller businesses. Up until now, we implicitly assumed that  $A_c$  behaves similarly across counties, but it is likely that counties are heterogeneous in their sensitivity to agglomeration forces. In our context, these forces are present if SMEs are indeed 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 18 separately for low and high agglomeration sub-samples with various methods of and report the results in different panels of Table 8 correspondingly. In panel A we report the results in counties ranked as “Low” and “High” agglomeration split by the median value *Chinitz* index and *InputOutput*. The positive significant coefficients only show up in the subsample of higher agglomerate counties, consistent with our prediction that PPP is especially important in counties with a larger degree of linkages across firms.

As a separate specification reported 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 20 employees), the positive coefficient is significant only for the top two quartiles and it is biggest for the top quartile. When we change the definition to medium size establishments (less than 500 employees), again the amelioration of import growth due to PPP is present only in the top two quartiles and is biggest in the top quartile. The ameliorative effects therefore are only present in counties with the largest share of small and medium sized establishments.

In panel C, we report the results across counties with different level of “industrial diversity”. Specifically, 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. (2020), 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.

In a further setting that we relegate to the appendix, 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, 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. Counties with a more dispersed distribution are expected to be more exposed to agglomeration forces. In the appendix Table [A8](#) 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.

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 and echoes the conclusion of a stronger immunity towards negative shocks among the local economy as a result of PPP exposure.

## 6 Conclusion

Governments around the world have announced a slew of programs to support the recovery and resilience 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.

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 when located in counties that had large PPP disbursements to small firms. 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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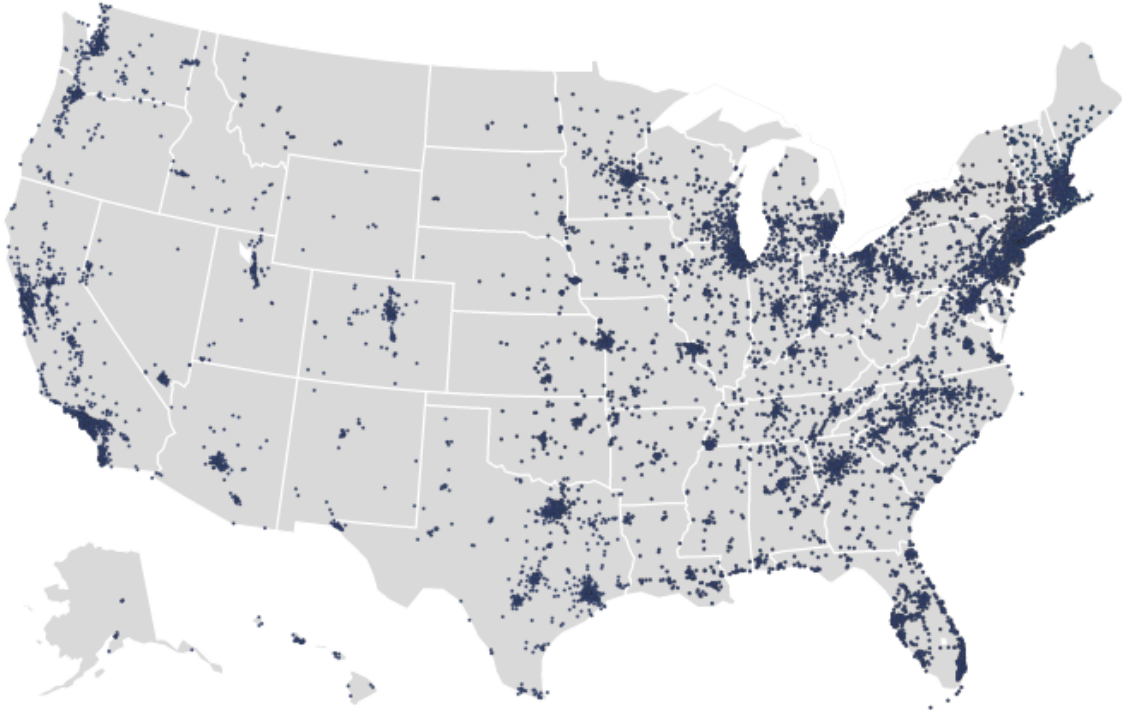
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Figure 1: **Geographic Distribution of U.S. Importers in Sample**



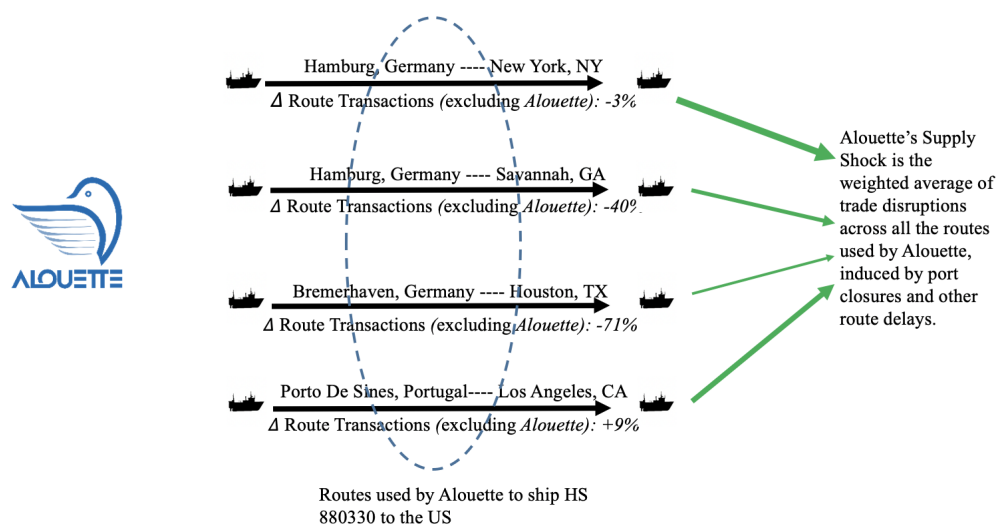
The dots reflect the location of importers as reported in their address. Panjiva, as part of its universe of maritime transactions, reports from the Bill of Lading: names/address of importers, their foreign suppliers, volume imported, shipment arrival date, ports (loading and unloading) associated with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

Figure 2: Pictorial Representation of full Empirical Specification

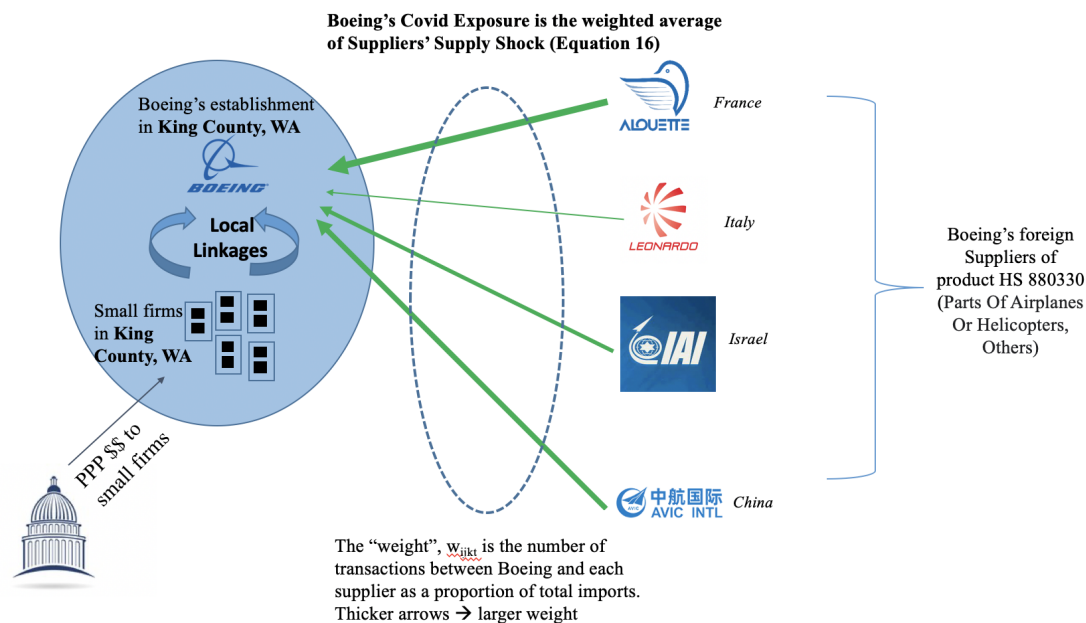
(a)

### Representation of Supply Shock (Equations 14 and 15)

**Example:** All routes used by *Alouette* to ship to United States for Product HS 880330 (Parts Of Airplanes Or Helicopters, Others)

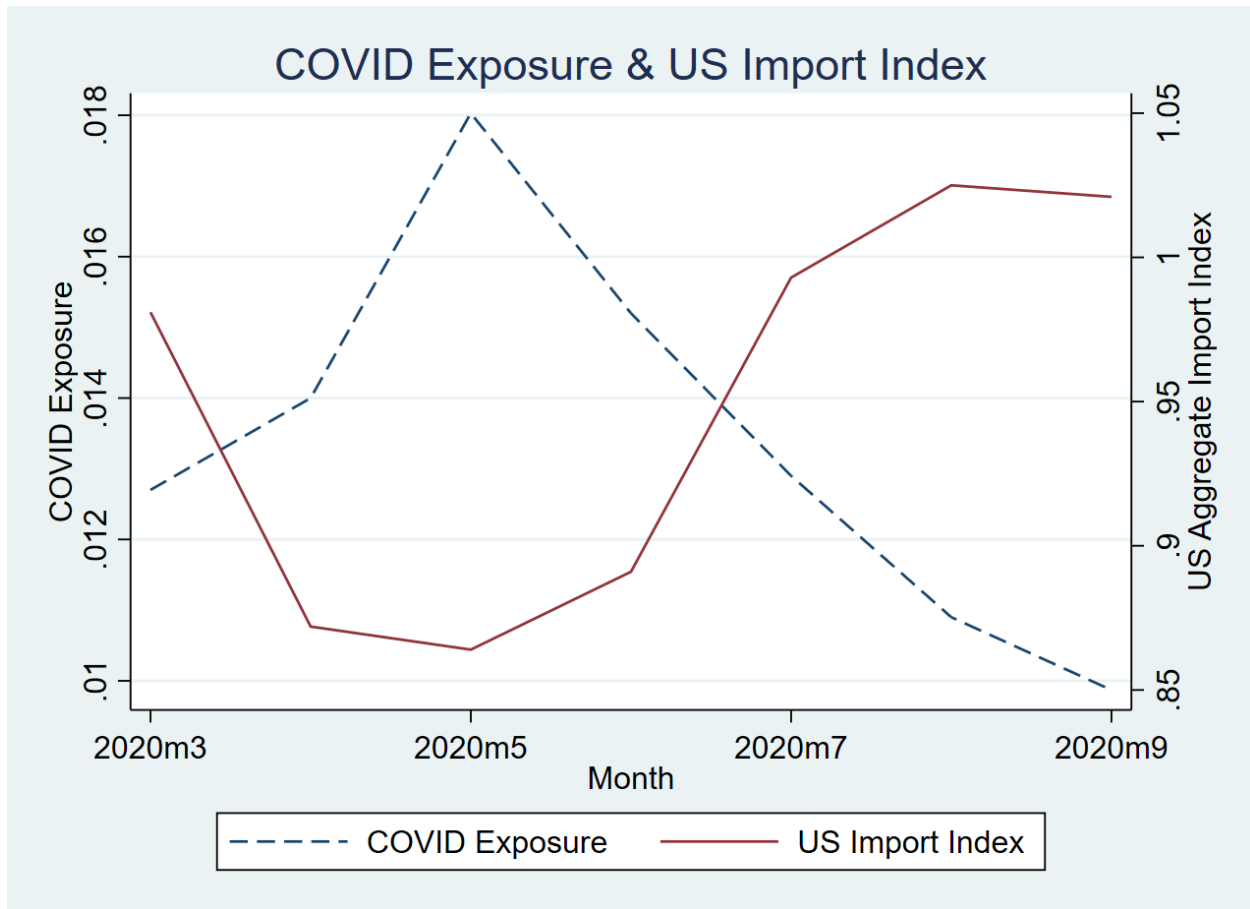


(b)



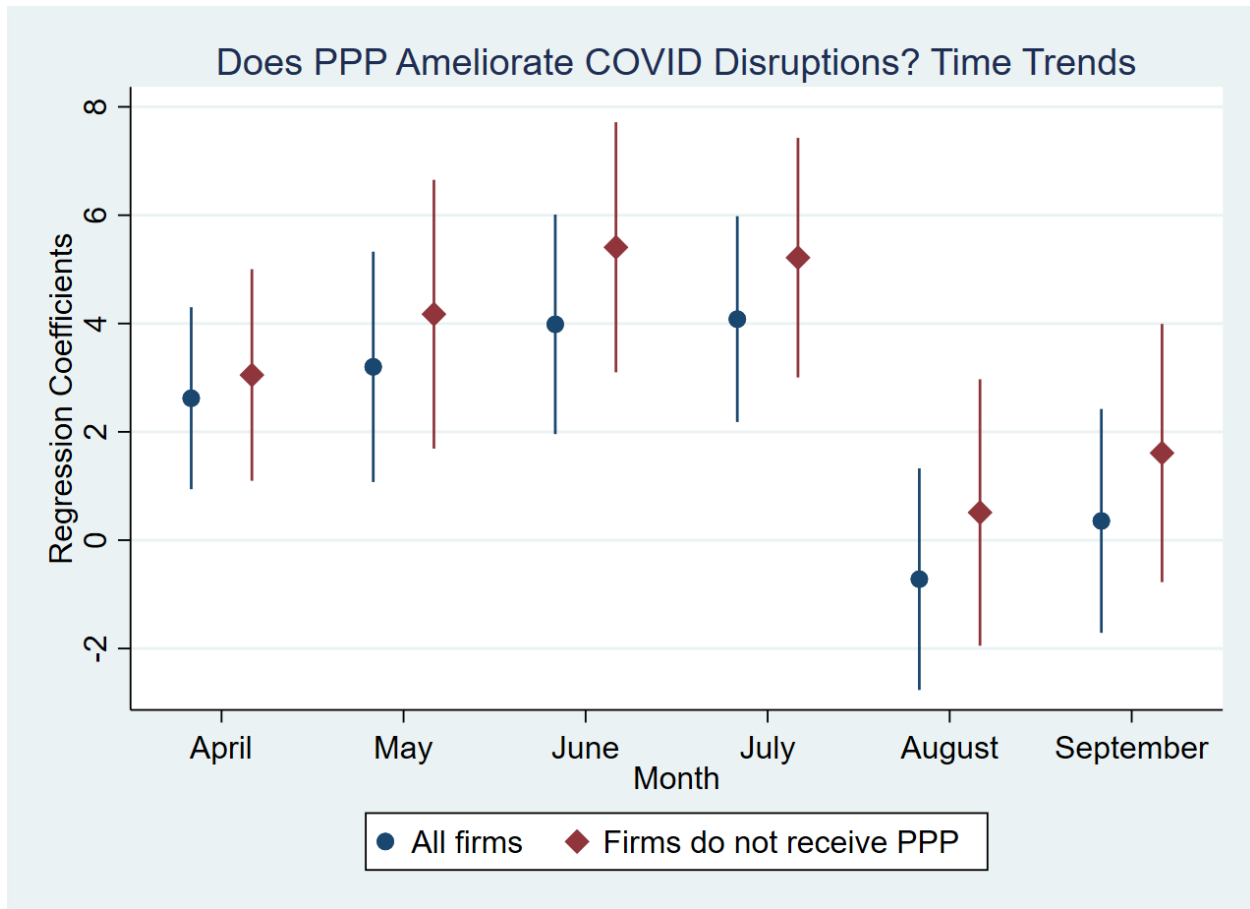
The figure provides a pictorial representation of our full empirical specification (Equations 14-18) using an example from our data. We look at the case of Boeing's plant in King County, WA which imports airplane and helicopter parts (HS 880330) from four main foreign suppliers. The top figure represents how we construct the supply shock of one of the suppliers - Alouette - for this product. Alouette's supply shock is given by 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.

Figure 3: COVID Exposure Measure & US Import Index



Notes: COVID Exposure is an unweighted average of our own measure. Aggregate import index is sourced from CBP World Trade Monitor: <https://www.cpb.nl/en/world-trade-monitor-march-2021>.

Figure 4: Time Trends of PPP Effects



The figure reports coefficient estimates by month for the COVID Exposure -  $PPPE^{Nbr}$  interaction as reported in Table 5.  $PPPE^{Nbr}$  is the total received over the time period, and therefore does not vary over time.

Table 1: **Summary Statistics**

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

	N	Mean	S.D.	Min	P25	P50	P75	Max
<b><i>Firm-Month Level</i></b>								
$\Delta Import_{i,k,t}^{Nbr}$	245234	-0.051	0.722	-2.118	-0.511	0	0.405	2.015
$\Delta Import_{i,k,t}^{Vol}$	245234	-0.047	0.681	-2.276	-0.422	0	0.340	2.067
COVID Exposure	245234	0.014	0.017	-0.024	0.001	0.008	0.021	0.086
<b><i>County-Month Level</i></b>								
PPP <sup>Nbr</sup>	8974	0.091	0.151	0	0	0.019	0.092	1.119
UnEmp_r	8974	8.614	4.529	1.6	5	7.9	11.2	34.6
COVID_Case	8974	2302.1	8556.149	1	60.99	325.99	1394	267513
Chg_SB_Rev	8974	-.870	.882	-4.108	-1.418	-.906	-.403	3.745
<b><i>County Level</i></b>								
PPPE <sup>Nbr</sup>	1581	0.092	0.128	-.5	0.068	0.122	0.165	0.360
Chinitz	1581	0.003	0.001	0.001	0.002	0.003	0.003	0.012
InputOutput	1581	0.35	0.14	0.07	0.13	0.42	0.64	0.84
SBS <sub>20</sub>	1581	0.661	0.063	0.459	0.616	0.655	0.702	0.920
SBS <sub>500</sub>	1581	0.820	0.049	0.624	0.784	0.819	0.855	0.966
Diversity	1581	7.914	1.825	2.239	6.765	7.893	9.089	13.307

Table 2: **COVID Disruption to Suppliers**

This table reports estimates from the following regression:

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

where  $\Delta \log(Supply_{j,r,k,t})$  is the difference in the logarithm values of total number of transactions for each supplier-route-product at month  $t$ , and  $\Delta \log(RouteTransactions_{r,t}^{-j})$  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
$\Delta \log(RouteTransactions_{r,t}^{-j})$	0.117*** (0.011)	0.117*** (0.008)	0.168*** (0.017)	0.173*** (0.012)
Firm-HS-Month FE	Y	Y	Y	Y
N	246352	246620	153623	153814
F-Statistics	106.44	204.21	100.46	204.60
Adj-R sq	0.067	0.076	0.066	0.072



Table 3: **COVID Disruption and Import Growth**

This table reports estimates from the following regression:

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

panel A reports the results for the entire sample, while panel B reports the results for different end-use types defined by the U.S. Census.  $\Delta Import_{i,k,t}$  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.

**Panel A: COVID Exposure and Import: Baseline Results**

	1	2	3	4
	$\Delta Import_{i,k,t}^{Nbr}$		$\Delta Import_{i,k,t}^{Vol}$	
COVID Exposure	-1.478*** (0.129)	-1.446*** (0.133)	-1.352*** (0.122)	-1.323*** (0.126)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
N	228300	226457	225859	224012
F-Statistics	130.789	118.711	123.095	110.767
Adj-R sq	0.129	0.124	0.130	0.124

Table 3: COVID Disruption and Import Growth (Continued...)

Panel B: COVID Distuption and Import: By Product Type							
		1	2	3	4	5	6
		$\Delta Import_{i,k,t}^{Nbr}$					
Census Product	Enduse Type	Food, feeds, beverage	Industrial supplies and materials	Capital goods, except automo-tive	Automotive vehicles, parts, and engines	Consumer goods	Other goods
COVID Exposure		-0.494 (0.370)	-1.160*** (0.274)	-1.850*** (0.304)	-0.837 (0.662)	-1.534*** (0.296)	-0.319 (1.416)
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		30447	59455	48288	12326	57265	3724
Adj-R sq		0.118	0.114	0.137	0.197	0.139	0.213

Table 4: **Does PPP Ameliorate COVID Disruption?**

This table reports estimates from the following regression:

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

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*.  $PPP_{c,T}$  is either the 1-month lagged PPP per establishment (*PPP*) at month  $t$  or exposure to PPP (*PPPE*) for county  $c$  at the 2nd quarter of 2020.  $X_{i,t}$  is a set of interactions where we interact the time-varying county-level control variables described in section 3.4 with the *COVID Exposure*. Cols 1-6 use *PPP Direct* while cols 7 use the *PPPE* as the exposure to PPP. Firm, product, and state-month fixed effects are used in cols 1-4, and firm, product, and county-month fixed effects are used in cols 5-7. 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: All Subsidiaries							
	1	2	3	4	5	6	7
	$\Delta Import_{i,k,t}^{Nbr}$						
PPP Measure	<i>PPP</i> <sup>Nbr</sup>						<i>PPPE</i> <sup>Nbr</sup>
COVID Exposure	-1.478*** (0.115)	-2.016*** (0.405)	-1.736*** (0.145)	-2.187*** (0.406)	-1.680*** (0.147)	-2.063*** (0.421)	-2.364*** (0.447)
PPP	0.011 (0.048)	0.017 (0.049)	-0.026 (0.049)	-0.028 (0.050)			
COVID Exposure X PPP			2.379*** (0.739)	2.978*** (0.855)	2.144*** (0.752)	3.100*** (0.886)	3.397*** (1.303)
Chg_SB_Rev		0.006 (0.008)		0.001 (0.008)			
COVID Exposure X Chg_SB_Rev		-0.156 (0.164)		0.190 (0.189)		0.344* (0.204)	-0.131 (0.184)
log(COVID_Case)		-0.004 (0.006)		-0.005 (0.006)			
COVID Exposure X log(COVID_Case)		-0.014 (0.050)		0.024 (0.050)		0.046 (0.051)	0.006 (0.052)
UnEmp_r <sub>t-1</sub>		-0.002 (0.001)		-0.002 (0.001)			
COVID Exposure X UnEmp_r <sub>t-1</sub>		0.045** (0.019)		0.040** (0.019)		0.030 (0.019)	0.038** (0.019)
Firm FE	Y	Y	Y	Y	Y	Y	Y
HS FE	Y	Y	Y	Y	Y	Y	Y
State-Month FE	Y	Y	Y	Y			
County-Month FE					Y	Y	Y
N	228300	228300	228300	228300	226457	226457	226457
Adj-R sq	0.129	0.129	0.129	0.129	0.124	0.124	0.124

Table 4: Does PPP Ameliorate COVID Disruption? (Only subsidiaries that do not receive PPP)

Panel B: Subsidiaries that do not receive PPP directly							
	1	2	3	4	5	6	7
	$\Delta Import_{i,k,t}^{Nbr}$						
PPP Measure	$PPP^{Nbr}$						$PPPE^{Nbr}$
COVID Exposure	-1.464*** (0.125)	-2.162*** (0.470)	-1.693*** (0.159)	-2.332*** (0.468)	-1.625*** (0.161)	-2.184*** (0.492)	-2.508*** (0.516)
PPP	0.040 (0.054)	0.046 (0.055)	0.007 (0.055)	0.006 (0.056)			
COVID Exposure X PPP			2.122*** (0.798)	2.661*** (0.930)	1.627** (0.816)	2.447** (0.975)	3.531*** (1.277)
Chg_SB_Rev		0.003 (0.009)		-0.001 (0.009)			
COVID Exposure X Chg_SB_Rev		-0.160 (0.183)		0.144 (0.216)		0.269 (0.228)	-0.115 (0.197)
$\log(\text{COVID\_Case})$		-0.007 (0.006)		-0.007 (0.006)			
COVID Exposure X $\log(\text{COVID\_Case})$		-0.017 (0.059)		0.017 (0.059)		0.031 (0.061)	-0.001 (0.060)
UnEmp_r <sub>t-1</sub>		-0.003** (0.002)		-0.003** (0.002)			
COVID Exposure X UnEmp_r <sub>t-1</sub>		0.063*** (0.021)		0.059*** (0.021)		0.052** (0.021)	0.059*** (0.021)
Firm FE	Y	Y	Y	Y	Y	Y	Y
HS FE	Y	Y	Y	Y	Y	Y	Y
State-Month FE	Y	Y	Y	Y			
County-Month FE					Y	Y	Y
COVID Exposure X Control	Y	Y	Y	Y	Y	Y	Y
N	170892	170892	170892	170892	169020	169020	169020
Adj-R sq	0.132	0.132	0.132	0.132	0.125	0.125	0.125

Table 5: **Does PPP Ameliorate COVID Disruption? Time Trends**

This table reports estimates from the following regression:

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

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*.  $PPPE_c$  is the measured exposure to PPPE at county  $c$  at the 2nd quarter of 2020. Cols 1-2 report results for all types of subsidiaries and cols 3-4 report results for subsidiaries that do not receive PPP. Firm, product, and state-month fixed effects are used in cols 1 and cols 3, and firm, product, and county-month fixed effects are used in cols 2 and cols 4. All regressions are estimated using firm, product, and 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	4
	$\Delta Import_{i,k,t}^{Nbr}$			
Sample	All Subsidiaries		Subsidiaries do not receive PPP	
COVID Exposure	-2.190*** (0.409)	-2.154*** (0.425)	-2.348*** (0.469)	-2.316*** (0.493)
COVID Exposure X $PPPE^{Nbr}$ (April)	1.879** (0.937)	2.781** (1.377)	3.136* (1.762)	3.600* (1.842)
COVID Exposure X $PPPE^{Nbr}$ (May)	3.898*** (1.131)	4.380** (1.842)	3.276** (1.423)	4.547** (2.162)
COVID Exposure X $PPPE^{Nbr}$ (June)	4.997*** (1.360)	6.020*** (1.789)	5.156*** (1.680)	7.125*** (2.020)
COVID Exposure X $PPPE^{Nbr}$ (July)	4.997*** (1.417)	5.741*** (1.933)	4.771*** (1.621)	5.787** (2.263)
COVID Exposure X $PPPE^{Nbr}$ (August)	1.375 (1.549)	-0.564 (1.920)	1.643 (1.731)	-0.948 (2.043)
COVID Exposure X $PPPE^{Nbr}$ (September)	2.318 (1.805)	1.637 (2.388)	3.179 (1.938)	2.275 (2.399)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
COVID Exposure X Control	Y	Y	Y	Y
N	228300	226457	170892	169020
Adj-R sq	0.129	0.124	0.132	0.125

Table 6: **Does PPP Ameliorate COVID Disruption? Product Heterogeneity**

This table reports estimates from the following regression:

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

where  $\Delta Import_{i,k,t}^{Nbr}$  are 12-mo difference in logarithm values of import for product  $k$  at firm  $i$  in month  $m$ , measured by *Number of Transactions*.  $PPPE_c$  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 described in section 3.4 with the *COVID Exposure*. 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	4
	$\Delta Import_{i,k,t}^{Nbr}$			
Census Product Type	Industrial supplies and materials	Capital goods, except automotive	Automotive vehicles, parts, and engines	Consumer goods
COVID Exposure	-2.190** (1.033)	-3.671*** (1.138)	-0.251 (1.851)	-3.392*** (1.248)
COVID Exposure X $PPPE^{Nbr}$	0.106 (2.320)	3.980* (2.588)	2.525 (6.021)	4.213** (2.036)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
County-Month FE	Y	Y	Y	Y
COVID Exposure X Control	Y	Y	Y	Y
N	59455	48288	12326	57265
Adj-R sq	0.114	0.137	0.197	0.139

Table 7: **Alternative Outcome: 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  $t$ .  $PPPE_c$  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 described in section 3.4 with the  $COVID\ Exposure$ . All regressions are estimated using state-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	4	5
	$\Delta Emp_{t,Jan}$				
Chg_SB_Rev	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.004*** (0.001)	0.002* (0.001)
Log(COVID_Case)	-0.004*** (0.001)	-0.004*** (0.001)	-0.004*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)
UnEmp_r t-1	-0.005*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)	-0.005*** (0.001)
COVID Exposure		-0.021** (0.010)	-0.021** (0.011)	-0.047** (0.024)	-0.042** (0.20)
PPPE			0.006** (0.004)	0.013** (0.006)	0.013** (0.006)
COVID Exposure X PPPE				0.283** (0.132)	0.310** (0.148)
COVID Exposure X Chg_SB_Rev					0.089* (0.051)
COVID Exposure X Log(COVID_Case)					-0.013 (0.024)
COVID Exposure X UnEmp_r t-1					0.014 (0.017)
State-month FE	Y	Y	Y	Y	Y
N	8974	8974	8974	8974	8974
r <sup>2</sup> .a	0.627	0.627	0.627	0.609	0.609

Table 8: **Does PPP Ameliorate COVID Disruption? Agglomeration**

This table reports estimates from the following regression:

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

where  $\Delta Import_{i,k,t}^{Nbr}$  are 12-mo difference in logarithm values of import for product  $k$  at firm  $i$  in month  $m$ , measured by *Number of Transactions*.  $PPPE_c$  is the exposure to PPP ( $PPPE$ ) at 2nd quarter of 2020 for county  $c$ . 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/intermediate establishment. Cols 1-4(5-8) use the share of establishment with employment less than 20(500), where quartile 1 indicates smallest share while quartile 4 indicates largest share of small/intermediate business in the county. In panel D, 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. 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
	$\Delta Import_{i,k,t}^{Nbr}$			
Agglomerate Measure	<i>Chinitz</i>		<i>InputOutput</i>	
	Bottom 50%	Top 50%	Bottom 50%	Top 50%
COVID Exposure	-2.351*** (0.656)	-2.260*** (0.697)	-2.465*** (0.712)	-2.136*** (0.750)
COVID Exposure X $PPPE^{Nbr}$	1.820 (1.279)	4.037*** (1.222)	1.442 (1.480)	3.982*** (1.279)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
County-Month FE	Y	Y	Y	Y
COVID Exposure X Control	Y	Y	Y	Y
N	111371	108729	101905	122220
Adj-R sq	0.131	0.126	0.130	0.118



Table 8: Does PPP Ameliorate COVID Disruption? (Continued...)

Panel B: Agglomeration Measured as County Level Small/Intermediate Establishments Share								
	1	2	3	4	5	6	7	8
	$\Delta Import_{i,k,t}^{Nbr}$							
Agglomeration Measure	Share of Small Business, SBS <sub>20</sub>				Share of Small Business, SBS <sub>500</sub>			
Quartile	1	2	3	4	1	2	3	4
COVID Exposure	-3.617*** (1.215)	-0.529 (1.105)	-2.113** (0.906)	-1.908** (0.859)	-2.460** (1.018)	-2.406** (0.991)	-1.530* (0.902)	-2.279** (1.045)
COVID Exposure X $PPPE^{Nbr}$	2.595 (2.312)	1.523 (2.364)	4.357** (1.813)	3.891*** (1.325)	3.354 (2.226)	0.121 (2.208)	4.720*** (1.578)	5.171*** (1.541)
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	Y	Y
COVID Exposure X Control	Y	Y	Y	Y	Y	Y	Y	Y
N	41278	46861	58954	78203	52521	49527	58907	64381
Adj-R sq	0.135	0.141	0.131	0.131	0.141	0.134	0.125	0.132

Table 8: Does PPP Ameliorate COVID Disruption? (Continued...)

Panel C: Agglomeration Measured as County Level Employment Diversity						
	1	2	3	4	5	6
	$\Delta Import_{i,k,t}^{Nbr}$					
<i>Diversity</i> percentile cutoff	50		75		95	
	Low	High	Low	High	Low	High
COVID Exposure	-1.247 (1.127)	-2.469*** (0.522)	-1.376 (0.905)	-2.629*** (0.567)	-2.010*** (0.623)	-2.594*** (0.807)
COVID Exposure X $PPPE^{Nbr}$	3.561 (2.868)	3.184*** (0.936)	2.635 (2.283)	3.631*** (0.964)	1.715 (1.294)	5.282*** (1.192)
Firm FE	Y	Y	Y	Y	Y	Y
HS FE	Y	Y	Y	Y	Y	Y
County X Month FE	Y	Y	Y	Y	Y	Y
COVID Exposure X Control	Y	Y	Y	Y	Y	Y
N	21789	204304	38378	187675	104906	121185
Adj-R sq	0.136	0.125	0.126	0.127	0.124	0.133

# Appendix

Table A1: **Variable Definition**

This table reports definition of each variable used in this paper.

Variable	Definition	Source
$\Delta Import_{i,k,t}^{Nbr}$	12-mo difference in logarithm values of import, measured by number of transactions	Panjiva
$\Delta Import_{i,k,t}^{Vol}$	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
$\Delta \log(RouteTransactions_{r,t}^{-j})$	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 <sup>Nbr</sup>	County-month level # of PPP loans normalized by total number of establishment in county	SBA & CBP
UnEmp <sub>L</sub>	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 <sup>Nbr</sup>	County Exposure to PPP at the 2nd quarter of 2020, measured by number of PPP	SBA, Call Reports & DOS
<i>Chinitz</i>	Index on intensity on number of providers that supply to new entrants.	BDS
<i>InputOutput</i>	Index on within county industrial connectedness.	BDS
SBS <sub>20</sub>	Share of small establishments with employment less than 20 within the county.	BDS
SBS <sub>500</sub>	Share of small establishments with employment less than 500 within the county.	BDS
<i>Diversity</i>	Inverse of the Herfindahl-Hirschman Index for county industrial employment.	BDS.

Table A2: **Summary Statistics of COVID Exposure by Enduse Category**

This table reports the summary statistics of *COVID Exposure* for each enduse category.

Category	N	Mean	S.D.	Min	P25	P50	P75	Max
Food, feeds, beverage	33,558	0.015	0.017	-0.024	0.002	0.009	0.024	0.086
Industrial supplies and materials	69,344	0.014	0.017	-0.024	0.001	0.008	0.021	0.086
Capital goods, except automotive	56,540	0.014	0.018	-0.024	0.001	0.008	0.021	0.086
Automotive vheicles, parts, and engines	15,719	0.014	0.017	-0.024	0.001	0.009	0.022	0.086
Consumer goods	66,788	0.012	0.017	-0.024	0.001	0.006	0.019	0.086
Other goods	5,621	0.013	0.015	-0.024	0.002	0.01	0.021	0.085

Table A3: **Relationship between  $\Delta Import$  and COVID Exposure**

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*.  $PPPE_c^{Nbr}$  is the time-invariant PPP exposure at county  $c$ .  $\mathbf{X}$  is a vector of control variables. Cols 1-2, 3-4 report results using  $\Delta Import_c^{Nbr}$  at the month of March, and April. 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
	$\Delta Import_c^{Nbr}$ (March)		$\Delta Import_c^{Nbr}$ (April)	
$PPPE_c^{Nbr}$	0.104 (0.085)	0.101 (0.084)	0.099 (0.153)	0.061 (0.154)
COVID Exposure		-8.595 (5.781)		-3.254 (4.085)
Chg_SB_Rev		0.027 (0.041)		0.047 (0.030)
COVID Exposure X Chg_SB_Rev		-0.969 (2.800)		-2.755* (1.582)
Log(COVID_Case)		0.007 (0.016)		0.012 (0.014)
COVID Exposure X Log(COVID_Case) Lcase		-1.143 (1.167)		-0.827 (0.890)
UnEmp_r <sub>t-1</sub>		-0.009 (0.019)		0.010 (0.012)
COVID Exposure X UnEmp_r <sub>t-1</sub>		1.619 (1.213)		0.608 (0.616)
N	1223	1223	1317	1317
Adj-R	0.007	0.008	0.014	0.020

Table A4: **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  $\Delta \log(Transaction - POL_{p,t}^{-j})$  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:

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

$\Delta Import_{i,k,t}$  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
$\Delta \log(Transaction - POL_{r,t}^{-j})$	0.219*** (0.055)	0.183*** (0.043)	0.189*** (0.044)	0.150*** (0.033)
Firm-HS-Month FE	Y	Y	Y	Y
N	136265	137609	136178	137548
F-Statistics	15.605	18.009	18.876	20.754
Adj-R sq	0.102	0.102	0.095	0.099

Table A4: 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	247570	0.025	0.045	-0.009	0.009	0.015	0.024	0.82

**Panel C: COVID Exposure and Import: Baseline Results**

	1	2	3	4
	$\Delta Import_{i,k,t}^{Nbr}$		$\Delta Import_{i,k,t}^{Vol}$	
	$\Delta Import_{i,k,t}^{Nbr}$		$\Delta Import_{i,k,t}^{Vol}$	
COVID Exposure - POL	-0.095** (0.039)	-0.128*** (0.039)	-0.087** (0.037)	-0.111*** (0.037)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
N	230534	230544	228027	228027
Adj-R sq	0.128	0.119	0.128	0.119

Table A5: **Robustness: COVID Disruption and Import Growth Aggregated to the Parent Level**

In the following table we replicate the specification in Table 3, 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
	$\Delta Import_{i,k,t}^{Nbr}$		$\Delta Import_{i,k,t}^{Vol}$	
COVID Exposure - Parent	-1.322*** (0.144)	-2.020*** (0.142)	-1.432*** (0.145)	-2.044*** (0.143)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
N	181967	181967	180233	180233
Adj-R sq	0.122	0.113	0.123	0.116



Table A6: **Robustness: COVID Disruption and Import Growth – Exclude Firms Potentially Redact Addresses in Some Years**

In the following table we replicate the specification in Table 3, but exclude large importers with locations redacted. It reports estimates from the following regression:

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

*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
	$\Delta Import_{i,k,t}^{Nbr}$		$\Delta Import_{i,k,t}^{Vol}$	
COVID Exposure	-1.492*** (0.130)	-1.462*** (0.133)	-1.377*** (0.122)	-1.348*** (0.126)
s Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
N	225181	223336	222820	220971
Adj-R sq	0.130	0.125	0.131	0.125

Table A7: **Robustness: PPP Time Trends, with Community Bank Share as Exposure to PPP**

This table reports estimates from the following regression: This table reports estimates from the following regression:

$$\Delta Import_{i,k,t}^{Nbr} = \beta \cdot COVID\ Exposure_{i,k,t} + \sum_t \theta_t \cdot COVID\ Exposure_{i,k,t} \times CB\_Share_c + \delta X + \xi_i + \eta_k + \kappa_{s(c),t} + \varepsilon_{i,k,t}$$

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*.  $CB\_share_c$  is community bank share at county  $c$ . Cols 1-2 report results for all types of subsidiaries and cols 3-4 report results for subsidiaries that do not receive PPP. Firm, product, and state-month fixed effects are used in cols 1 and cols 3, and firm, product, and county-month fixed effects are used in cols 2 and cols 4. All regressions are estimated using firm and product 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.

Use Community Bank Share as the exposure for PPP				
	1	2	3	4
Sample	All Subsidiaries		Subsidiaries do not receive PPP	
COVID Exposure	-1.775*** (0.291)	-1.754*** (0.298)	-1.836*** (0.313)	-1.860*** (0.315)
COVID Exposure X CB.Share (April)	1.592* (0.873)	1.426 (1.004)	2.089** (0.943)	1.220 (1.090)
COVID Exposure X CB.Share (May)	1.403* (0.837)	1.808* (0.989)	1.557* (0.922)	2.424** (1.084)
COVID Exposure X CB.Share (June)	1.203 (0.943)	2.478** (1.083)	2.085** (1.042)	3.459*** (1.232)
COVID Exposure X CB.Share (July)	2.309*** (0.881)	3.176*** (1.001)	2.340** (0.992)	3.406*** (1.142)
COVID Exposure X CB.Share (August)	-1.140 (1.120)	-2.241* (1.315)	-0.534 (1.258)	-1.210 (1.495)
COVID Exposure X CB.Share (September)	-1.426 (1.105)	-1.924 (1.325)	-1.382 (1.202)	-1.813 (1.476)
Firm FE	Y	Y	Y	Y
HS FE	Y	Y	Y	Y
State-Month FE	Y		Y	
County-Month FE		Y		Y
COVID Exposure X Control	Y	Y	Y	Y
N	230420	228577	172844	170969
Adj-R sq	0.129	0.124	0.131	0.125

Table A8: **Robustness: PPP and Agglomeration: County Import Distribution**

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  $i$ .  $\Phi_c$  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.

This table reports estimates from the following regression:

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

where  $\Delta Import_{i,k,t}^{Nbr}$  are 12-mo difference in logarithm values of import for product  $k$  at firm  $i$  in month  $m$ , measured by *Number of Transactions*.  $PPP_{c,T}$  is the exposure to PPP (*PPPE*) at 2nd quarter of 2020 for county  $c$ . In cols 1-3, we report results for sub-sample of counties that rank in the bottom to top tercile of the  $\zeta$  measure. 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.

Agglomeration Measured as $\zeta$			
	1	2	3
	$\Delta Import_{i,k,t}^{Nbr}$		
$\Phi$ Tercile	Bottom 1/3	Middle 1/3	Top 1/3
COVID Exposure	-2.758*** (0.802)	-1.123 (0.738)	-3.445*** (0.981)
COVID Exposure X PPPE <sup>Nbr</sup>	0.911 (1.573)	4.454*** (1.525)	6.177*** (1.692)
Firm FE	Y	Y	Y
HS FE	Y	Y	Y
County-Month FE	Y	Y	Y
COVID Exposure X Control	Y	Y	Y
N	79249	77237	71462
Adj-R sq	0.135	0.123	0.136