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## Estimating General Equilibrium Spillovers of Large-Scale Shocks

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# Estimating General Equilibrium Spillovers of Large-Scale Shocks

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# Estimating General Equilibrium Spillovers of Large-Scale Shocks

## Abstract

Researchers use (quasi-)experimental methods to estimate how shocks affect directly treated firms and households. Such methods typically do not account for general equilibrium spillover effects. I outline a method that estimates spillovers operating among groups of firms and households. I argue that the presence of multiple types of spillovers, measurement error, and nonlinear effects can severely bias estimates. I show how instrumental variables, heterogeneity tests, and flexible functional forms can overcome different sources of bias. The analysis is particularly relevant to the estimation of spillovers following large-scale financial and business cycle shocks.

Keywords: general equilibrium effects, spillovers, estimation, macroeconomic shocks, financial shocks.

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# **I Introduction**

In recent decades, researchers in economics and finance have increasingly adopted experimental and quasi-experimental methods to study the effects of shocks. These methods compare a group of firms or households that are directly exposed to a given shock to an unexposed control group. By using credibly exogenous variation, researchers can ensure that the shock caused any differences in outcomes between treated and control groups.

A shortcoming of quasi-experimental methods is that they typically do not measure the total effect of a shock. Most studies exclusively estimate the effect of direct treatment, which captures only part of the total effect. The remaining part is driven by spillover effects from directly exposed firms and households to other firms. Spillovers operate through general equilibrium channels, including price and wage changes, agglomeration forces, and input-output networks. For instance, researchers interested in the effects of fiscal stimulus might compare firms that receive fiscal support to firms that do not. If stimulus causes directly exposed firms to increase hiring, wages in local labor markets might rise, which affects all firms in the region.

Estimating spillovers allows us to understand which general equilibrium channels need to be included in economic models and whether micro data estimates are informative about higher levels of aggregation. Estimating spillovers is particularly important when researchers study large-scale financial and business cycle shocks, such as fiscal stimulus programs, tax changes, and banking crises. In these cases, many firms and households are simultaneously affected, so general equilibrium forces are likely large and operate through many different channels.

The aim of this paper is threefold. First, I outline how researchers can estimate spillovers operating among firms and households that are connected in some way, for example firms in the same region, sector, or network. Second, I highlight three issues that can introduce mechanical bias into spillover estimates: multiple types of spillovers, measurement error, and nonlinear effects. For instance, estimates are biased when researchers do not account for the fact that spillovers may operate simultaneously across multiple groups, such as when a shock to firms generates spillovers both onto firms in the same region and same sector. Third, I propose practical solutions to these estimation challenges, such as instrumental variables, testing for heterogeneous effects, and flexible functional forms.

The paper begins with a simple empirical framework. I use the language of firm-level analysis, but the framework applies equally to household-level analyses. A researcher studies whether a shock to a subset of firms (the “treatment”) generates spillovers onto other firms that are in the same “group” as treated firms. Firms that belong to the same group are in some way connected, for example because they are in the same region, sector, production network, technology space, or any other type of grouping. To test for spillovers, the researcher includes the average treatment

status of all other firms in the same group in the regression (the “leave-out mean”). For example, if the researcher is interested in regional spillovers, one regressor is the average treatment status of all other firms in the region. This approach is appropriate under three assumptions. First, the magnitude of the spillover generated by a firm needs to be proportional to the number of firms in the group. Second, the spillover generated by a firm needs to be identical across firms in the same group. Third, treatment needs to be exogenous both across individuals and across groups.<sup>1</sup>

If these assumptions hold, the method of estimating spillovers using leave-out means has several attractive features. It is relatively easy to apply to existing research designs. It estimates a standard error on the spillover, which allows formal inference on whether spillovers are statistically significant. This is in contrast to studies using only group-level data (e.g., region or sector data) where the magnitude of spillovers cannot be formally tested. The method allows researchers to directly compare the magnitude of different types of spillovers, by simultaneously estimating multiple spillover types (for instance, including both regional and sectoral leave-out means in the regression). Researchers can also use the method to test specific general equilibrium theories, by examining whether spillovers are heterogeneous across firm types (e.g., comparing tradable versus non-tradable producers to assess theories about regional demand).

Using leave-out means to estimate spillovers is not without its challenges, however. I focus on three issues that are common when researchers study financial and business cycle shocks: multiple types of spillovers, measurement error, and nonlinear effects. These issues can mechanically bias spillover estimates even if researchers make correct assumptions about the magnitude and exogeneity of spillovers. The nature and severity of bias due to these issues in the case of spillover estimation is distinct from standard concerns about spurious confounders or measurement error. I discuss the three issues in turn and suggest ways to avoid biased estimates.

First, imagine that spillovers operate across several groups at the same time. For instance, a productivity shock to a subset of firms might simultaneously affect firms in the same region (through wages, as directly treated firms raise hiring on local labor markets) and the same sector (through output prices, as directly treated firms raise production). Researchers are often interested in testing one specific model of spillovers. For instance, urban economists might focus on regional spillovers, while industrial organization economists might focus on sectoral spillovers. I show that testing for spillovers within only one group will bias the spillover estimate if in fact there are multiple types of spillovers, even if treatment is exogenous across individuals and groups. Spillover estimates may even have the wrong sign, leading to a complete misinterpretation of general equilibrium forces.

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<sup>1</sup>The exogeneity criterion is a high bar. In many settings, treatment is exogenous within groups (e.g., comparing firms within the same region), but not across groups (e.g., comparing firms across different regions). Exogeneity can be achieved through quasi-experimental or experimental treatments.

The obvious solution to this problem is to include all relevant group-level leave-out means in the regressions. However, this may be difficult in practice. For one, it is usually not possible to measure all relevant types of connections between firms. For example, firms connected through a common production technology, product space, or bank lender might generate spillovers onto each other, but detailed data on technology, products, or lenders are not typically available in standard datasets. In addition, the full set of spillover channels is not known *ex ante* and regressions become underpowered with many regressors.

An alternative solution is to find an instrumental variable (IV). To overcome the bias, the instrument needs to be correlated with individual treatment status, but uncorrelated with the treatment status of other firms in the group. Spillover estimates based on IV are unbiased even when there are multiple spillover groups. If an individual-level instrument is not available, a group-level instrument still generates unbiased estimates of group aggregate effects (for example, an instrument for regional variation). A further approach to assessing bias is to test for heterogeneous effects. Economic theory may predict which firms should not be affected by a given type of spillover. For example, tradable firms are not affected by local demand spillovers (Moretti 2010; Mian and Sufi 2014). If estimated demand spillovers are zero for tradable firms and only exist among non-tradable firms, as theory predicts, spillover estimates are unlikely to be biased by the multiple spillover problem (see, for example, Huber 2018).

The second estimation issue I discuss is measurement error. A modest degree of classical measurement error can generate large spillover estimates, even if true spillovers are zero. The reason is that part of the true direct effect erroneously loads onto the spillover estimate. This bias is distinct from the standard bias toward zero due to measurement error in non-spillover settings. With spillover estimates, measurement error can cause bias toward and away from zero, depending on the magnitude of the true spillover. IV estimates and heterogeneous effects are promising approaches to overcome bias from measurement error.

The third estimation issue are nonlinear direct effects. In many settings in finance and macroeconomics, the direct effects of shocks are nonlinear, for example due to liquidity or capital constraints (Brunnermeier and Sannikov 2014; Cloyne et al. 2019). Researchers might not be aware of underlying nonlinearity and instead use a linear treatment measure as regressor. This can bias the estimates of spillovers. For instance, I simulate data where only households with strictly positive treatment status respond to treatment (e.g., only households with positive house price shocks extract equity in Cloyne et al. 2019). I find that regressions based on linear treatment status generate large and significant spillover estimates when the true spillover is zero. I show that IV and flexible functional forms can overcome the bias due to nonlinear direct effects.

Throughout the paper, I use as examples spillovers that operate within regions and sectors. However, the leave-out mean method can be applied whenever treatment varies across individuals

and across a group of connected firms. Examples include firms and households with the same production technology, bank lender, and input-output network. Estimating shock transmission through networks requires slightly different notation. However, the three estimation issues can still cause bias due to the same intuition, as I describe in Appendix A.

Overall, the results in this paper suggest that researchers can directly test for general equilibrium spillovers by using leave-out means. This helps in designing realistic general equilibrium models and in learning about higher levels of aggregation from micro data. However, estimating spillovers is prone to mechanical biases and requires careful implementation.

A recent wave of research calibrates the size of spillovers using financial and macroeconomic models (Chodorow-Reich 2014, 2019; Nakamura and Steinsson 2014, 2018; Auclert et al. 2019; Beraja et al. 2019; Herreno 2020) and converts micro estimates to different levels of aggregation (Auclert et al. 2018; Sarto 2018; Sraer and Thesmar 2019; Wolf 2019; Chodorow-Reich et al. 2020; Guren et al. 2020). Theoretical approaches can flexibly calculate the total effect of a shock at any level of aggregation, but rely on hard-to-verify assumptions about the nature and size of spillovers. In contrast, the method described in this paper directly estimates spillovers using (quasi-)experimental variation.

Few existing papers directly estimate spillovers following financial or business cycle shocks. Huber (2018), Egger et al. (2019), Giroud and Mueller (2019), and Verner and Gyöngyösi (2020) find that regional spillovers magnify the effects of direct shocks, using the leave-out mean method.<sup>2</sup> A related set of papers examines spillovers across neighboring regions, also using leave-out mean methods (Dupor and McCrory 2018; Adão et al. 2020; Auerbach et al. 2020; Colonnelli and Prem 2020). Overall, these papers have paid little attention to the issues of multiple spillovers, measurement error, and nonlinear effects that I emphasize. The issues described in this paper relate both to the within-region and across-region studies.<sup>3</sup>

A literature in the economics of education studies peer effects (reviewed by Epple and Romano 2011; Sacerdote 2011; Angrist 2014) and mismeasured peer characteristics (Ammermueller and Pischke 2009). Peer effects usually operate at the level of one pre-defined group, for example the classroom or school. In contrast, I focus on spillovers following large-scale financial and business cycle shocks. In these cases, spillovers operate through multiple overlapping groups and it is not clear *ex ante* which spillovers are relevant. Moreover, nonlinear effects are common in these settings (e.g. due to borrowing, liquidity, and capital constraints). That is why I highlight

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<sup>2</sup>In contrast to these shocks at business cycle frequency, the agglomeration and innovation literatures study longer-term spillovers in response to persistent shocks (Moretti 2010; Greenstone et al. 2010; Bloom et al. 2013; Bernstein et al. 2019; Gathmann et al. 2020; Helm 2020).

<sup>3</sup>On the methodological side, Berg and Streitz (2020) emphasize that direct estimates are hard to interpret if spillover effects exist; Mian et al. (2019) discuss the difference between estimates based on region-level and firm-level data; Gabaix and Koijen (2020) propose a granular IV approach to estimate aggregate multipliers; and Borusyak and Hull (2020) analyze a distinct form of omitted variable bias affecting network spillovers.



estimation issues due to multiple groups and nonlinear effects, which have not been extensively discussed in the peer effects literature.

## II Empirical Framework

### II.A Basic Model of Direct and Spillover Effects

Consider an economic shock (treatment) that affects firms or households with varying intensity, as indicated by their treatment status. For example, if the shock is a credit supply disruption, treatment status is a firm's dependence on failing banks. If the shock is fiscal stimulus policy, treatment status measures whether firms or households receive a stimulus check.

Economic theory suggests that the treatment status of a given firm or household can affect the outcomes of other firms and households. For instance, if two firms are located in the same region, they hire on the same local labor market. When one firm is treated, it may change its labor demand, thereby affecting local wages. If two firms are in the same product sector, they are competitors. When a competitor is treated, product prices may change, affecting all firms in the sector. In general, whenever firms are in some way connected, the treatment status of one firm can generate spillovers onto other firms. Similarly, whenever households are connected, there can be cross-households spillovers. I henceforth use firm-level language, but the analysis applies equally to households.

While in reality there are many channels that connect firms, to simplify exposition, I assume that there are just two: firms are connected either if they operate in the same region or in the same sector. The treatment status of an individual firm  $i$  in region  $r(i)$  and sector  $s(i)$  is given by  $x_i$ . An outcome, such as firm investment or employment growth, is given by  $y_i$ . Assuming linearity, the relationship between outcome and treatment status of various firms is:

$$y_i = \beta x_i + \sum_{j \neq i, r(j)=r(i)} \gamma^j x_j + \sum_{k \neq i, s(j)=s(i)} \lambda^k x_k + \alpha + \varepsilon_i. \quad (1)$$

The first coefficient  $\beta$  is the direct effect of individual treatment ( $x_i$ ) on the outcome. The direct effect represents by how much the outcome would change if firm  $i$  alone was treated. In addition, there are spillover effects  $\gamma^j$  and  $\lambda^k$ . Each spillover effect represents by how much outcome  $y_i$  of firm  $i$  would change if another firm in the same region (firm  $j$  with treatment status  $x_j$ ) or in the same sector (firm  $k$  with treatment status  $x_k$ ) was treated. I assume that treatment of all firms is exogenous to the error, such that  $E(x_i \varepsilon_i) = 0 \forall i$ .

The superscripts on the coefficients  $\gamma^j$  and  $\lambda^k$  indicate that spillover effects are firm-specific, since spillovers arising from two different firms are not necessarily identical. It is, however, difficult to estimate one spillover coefficient per firm in the data. Instead, researchers commonly

assume that spillovers are identical for firms in the same sector or region, which facilitates estimation:

$$\gamma^j = \frac{\gamma}{N_{r(j)} - 1} \forall j,$$

$$\lambda^k = \frac{\lambda}{N_{s(j)} - 1} \forall k.$$

The number of firms in a region and sector is  $N_{r(j)}$  and  $N_{s(j)}$ , respectively. Intuitively, the assumptions imply that the greater the number of firms in a region or sector, the less important the region- or sector-level spillovers generated by an individual firm.

Under these assumptions, the outcome depends on only three coefficients, individual treatment status and two “leave-out means”:

$$y_i = \beta x_i + \gamma \overline{x_{r(i)}} + \lambda \overline{x_{s(i)}} + \alpha + \varepsilon_i. \quad (2)$$

The leave-out mean  $\overline{x_{r(i)}}$  is the average treatment status of all other firms in region  $r(i)$  apart from firm  $i$ :

$$\overline{x_{r(i)}} = \frac{\sum_{j \neq i} x_j}{N_{r(i)} - 1} \quad (3)$$

and  $\overline{x_{s(i)}}$  is defined analogously.

## II.B Interpreting Spillover Coefficients

The coefficients  $\gamma$  and  $\lambda$  are the region- and sector-level spillovers. They measure the change in the outcome of firm  $i$  if the average exposure of other firms in its region or sector increases. The direct effect (i.e., the change if firm  $i$  alone is treated) is given by  $\beta$ .

The best way to report the magnitude of spillovers are the ratios  $\frac{\gamma}{\beta}$  and  $\frac{\lambda}{\beta}$ . Spillover coefficients on their own are hard to interpret. The reason is that the coefficients  $\beta, \gamma$ , and  $\lambda$  represent treatment effects relative to a control group of firms for whom direct treatment and leave-out means are all zero.<sup>4</sup> This means, the coefficients do not capture the total difference in firm outcomes relative to a world where the shock did not happen. Instead, they capture the effect of treatment relative to firms that were treated neither directly nor through spillovers.

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<sup>4</sup>To be precise,  $\gamma$  and  $\lambda$  measure direct and spillover effects relative to a firm that was not directly exposed to the shock ( $x_i = 0$ ) and in whose region and sector no other firm was directly exposed to the shock ( $\overline{x_{r(i)}} = \overline{x_{s(i)}} = 0$ ). See Chodorow-Reich (2020) for a formal analysis of relative versus absolute effects, which is not the focus of this paper.

## II.C Variation in Treatment Is Exogenous

Throughout the paper, I assume that individual treatment status as well as treatment status of firms in the same region and sector is exogenous to all other determinants of firm outcomes:

$$E(x_i \varepsilon_i) = 0 \forall i.$$

In practice, exogenous variation means that researchers have either experimentally randomized treatment status or identified quasi-random variation. As a result of this assumption, all estimation issues described below are not driven by the usual endogeneity concerns about correlations between treatment and unobserved errors. As shown below, the issues are more subtle and depend on the distribution of treatment across regions and sectors.

Exogenous variation is a high bar in practice. In many studies, variation in direct treatment may be exogenous within region and sector, but variation in treatment of firms in the same region and sector is not. For instance, exposure to failing banks may be exogenous when comparing firms within regions, but the distribution of failing banks across regions may be correlated with other shocks to firm growth. In such cases, the group definition fails the exogeneity criterion and cannot be used to estimate region-level spillover effects.

## II.D Treatment May Vary Systematically Across Regions and Sectors

I assume that direct treatment status depends on several random variables:

$$x_i = u_{r(i)} + u_{s(i)} + z_i + v_i, \quad (4)$$

where  $u_{r(i)}$  is a common factor for all firms in region  $r(i)$  and  $u_{s(i)}$  is a common factor for all firms in sector  $s(i)$ . The other components vary at the individual level:  $z_i$  is an observed variable, which is uncorrelated within regions and sectors and can serve as instrument for  $x_i$ , and  $v_i$  is an unobserved random error. The variables  $u_{r(i)}$ ,  $u_{s(i)}$ ,  $z_i$ , and  $v_i$  are uncorrelated with each other and with the error  $\varepsilon_i$  in equation 2.

If  $u_{r(i)}$  is identical across regions and  $u_{s(i)}$  is identical across sectors, treatment status does not vary systematically across regions and sectors. However, variation across regions and sectors is systematic in most research designs. Variation is always systematic in experiments where researchers intentionally treat some groups more than others. In most naturally occurring settings, variation is also systematic. For instance, exposure to the 2008/09 credit crisis varied systematically across regions and sectors because banks tend to specialize in certain regions and sectors, rather than picking borrowers at random (Chodorow-Reich 2014; Bentolila et al. 2018; Huber 2018). As a result, certain areas and sectors were systematically more exposed to failing banks.

Similarly, fiscal stimulus tends to be concentrated in specific regions (Chodorow-Reich 2019).

Systematic variation guarantees that there is a large degree of variation across regions and sectors when the number of firms per region and sector is large, making it easier to estimate spillovers. In contrast, when variation across regions and sectors is not systematic, there will be hardly any variation in large samples, making it hard to precisely estimate spillovers.

The challenge is that naturally occurring systematic variation is often not exogenous. The factors generating systematic variation may also drive differences in firm outcomes across groups. For example, failing banks might be more likely to operate in regions with low growth potential. This would generate a correlation between the leave-out mean and other shocks to firm growth (correlation between  $u_{r(i)}$  and the error term in equation 2). For the purpose of this paper, I leave aside concerns of exogeneity and focus on other estimation issues.

## II.E Setup for the Simulations

I investigate the properties of spillover estimates by running simulations. In each simulation, I randomly sort 5,000 observations (indexed by  $i$ ) into 500 equally-sized regions and 500 equally-sized sectors. In the baseline simulations, I assume that the region and sector terms  $u_{r(i)}$  and  $u_{s(i)}$  are both independently and log-normally distributed with mean 0 and standard deviation 1. This implies that variation is systematic across regions and sectors in the baseline simulations. In additional simulations, I assume that variation is not systematic, in which case  $u_{r(i)}$  and  $u_{s(i)}$  are zero.  $\varepsilon_i$ ,  $z_i$ , and  $v_i$  are normally distributed with mean 0 and standard deviation 1. Throughout the paper, I report coefficients and standard errors averaged over 100 simulations.

## III Bias Due to Multiple Types of Spillovers

Having laid out the basic empirical framework, I highlight the difficulties that arise when estimating spillovers. I first show that spillover estimates can be biased if there are multiple types of spillovers and then propose ways to avoid this bias.

### III.A Specification Challenges of Multiple Spillovers

To begin, I assume that spillovers operate within regions and sectors with a coefficient of one ( $\gamma = \lambda = 1$ ). The true data-generating process is thus:

$$y_i = x_i + \overline{x_{r(i)}} + \overline{x_{s(i)}} + \varepsilon_i. \quad (5)$$

I assume that treatment varies systematically across regions and sectors (i.e.,  $u_{r(i)}$  and  $u_{s(i)}$  in equation 4 are not identical across regions and sectors). If the regression correctly tests for both

types of spillovers, by including both leave-out means, the estimation results are consistent (Table I, column 1).

In practice, researchers often test for just one type of spillover and therefore include only one leave-out mean in their regression. One reason is that theoretical models tend to focus on one specific spillover mechanism. For instance, urban models focus on regional agglomeration forces. As a result, urban economists might only be interested in testing for regional spillovers. Industrial economists model competition, so their empirical tests might consider only within-sector spillovers. Financial economists study credit reallocation, so they might test only for spillovers among borrowers of the same bank.

Apart from testing single-spillover models, the difficulties of measurement are another reason why researchers may miss out on relevant spillover forces. Some economic connections between firms are not recorded in standard datasets. For example, firms might use the same production technologies, so technological progress at one firm generates spillovers onto other firms (Bloom et al. 2013). Firms might sell substitute products, so productivity shocks to one firm might harm competitors. The default of one firm might affect other firms borrowing from the same lender because the lender becomes capital constrained. Most datasets do not contain precise measures of technology space, product space, or lender information. As a result, researchers are usually unable to include all relevant spillover forces in their specifications.

### III.B Bias Due to a Missing Type of Spillover

To illustrate the effects of ignoring relevant spillovers, I run regressions that only contain direct treatment status  $x_i$  and the regional leave-out mean  $\bar{x}_{r(i)}$ . Recall that the regional and sectoral leave-out means are uncorrelated by construction, so it is not obvious that the coefficient on the regional leave-out mean should be biased. However, the estimates are indeed inconsistent (Table I, column 2). The direct estimate is inflated and the spillover is biased downward. While the true ratio of spillover to direct effect is 100 percent, the estimated ratio is 29 percent.

The reason for the inconsistent estimates is the presence of systematic variation across regions and sectors. When  $x_i$  and  $\bar{x}_{r(i)}$  are the only regressors, the omitted  $\bar{x}_{s(i)}$  enters the error term. The two leave-out means  $\bar{x}_{r(i)}$  and  $\bar{x}_{s(i)}$  are uncorrelated, so  $\bar{x}_{r(i)}$  and the error term are uncorrelated. However, both  $x_i$  and the omitted  $\bar{x}_{s(i)}$  are functions of the regional factor  $u_{s(i)}$  (equation 4). As a result,  $x_i$  and the error term are positively correlated, which biases the estimated coefficient on  $x_i$ . The spillover estimate is then also biased because  $x_i$  and  $\bar{x}_{r(i)}$  are positively correlated (due to the common factor  $u_{r(i)}$ ). This pattern of correlations ultimately generates downward bias in the spillover coefficient on  $\bar{x}_{r(i)}$ .

### III.C Avoiding Bias Due to a Missing Spillover

Instrumental variables solve the problem even when only one leave-out mean is in the regression. Using  $z_i$  and  $\overline{z_{r(i)}}$  as instruments, as defined in equation 4, renders both direct and spillover estimates consistent (column 3). It is often easier for researchers to find instruments at the regional level rather than the individual level, for instance in the cases of fiscal stimulus (Nakamura and Steinsson 2014; Chodorow-Reich 2019), house prices (Saiz 2010; Mian and Sufi 2011), or banking crises (Huber 2018). By instrumenting for  $\overline{x_i}$  (average of  $x_i$  across all firms in the region) using  $\overline{z_i}$  (average of  $z_i$  across all firms in the region), we get a consistent estimate of the total effect at the region-level (column 4).

An alternative approach to gauging whether important types of spillovers are missing is to test for heterogeneous spillover effects. Economic theory often predicts that spillovers for certain types of firms or households should only exist among a subgroup of firms. For instance, firms selling non-tradable goods are likely to be affected by regional demand shocks, but firms selling tradables are not (Mian and Sufi 2014). Testing for heterogeneity by tradability can reveal whether regional spillovers in response to a demand shock are consistent with theory. For instance, Huber (2018) shows that regional spillovers exist only among firms that depend on local forces, consistent with theoretical models. If instead significant spillovers also show up for other firm types, the estimates are likely biased.

Finally, note that multiple spillover types do not lead to bias if there is no systematic group-level variation (column 5). This requires that both  $u_r$  and  $u_s$  are identical across regions and sectors, respectively (equation 4). However, this condition is often not met in large-scale shocks, as outlined in Section II.D.

### III.D Bias Due to Including the Wrong Type of Spillover

Next, I assume that there is no true spillover within regions, but a spillover within sectors ( $\gamma = 0; \lambda = 1$ ). The true data-generating process is thus:

$$y_i = x_i + \overline{x_{s(i)}} + \varepsilon_i. \quad (6)$$

Testing for the wrong group-level spillover, by including only  $x_i$  and  $\overline{x_{r(i)}}$  as regressors, can also generate bias (Table II, column 1). The estimated spillover coefficient on  $\overline{x_{r(i)}}$  is *negative* and significant (column 2). The ratio of estimated spillover to direct effect is large at -33 percent. Note that the true spillover within sectors is positive 100 percent. The focus on the wrong spillover leads to a severe misinterpretation of the economic spillover forces. As before, systematic variation is key. If group-level variation is not systematic, the estimation results are consistent (column 3).

## IV Measurement Error Can Bias Spillover Estimates

In this section, I outline how measurement error can bias spillover estimates and suggest ways to avoid bias.

### IV.A Definition of Measurement Error

I assume that there is only a regional spillover, given by  $\gamma$ , so that the true data-generating process is:

$$y_i = \beta x_i + \gamma \overline{x_{r(i)}} + \varepsilon_i. \quad (7)$$

Imagine that direct treatment status  $x_i$  can only be measured with error. Observed treatment status is:

$$x_i^* = x_i + \eta_i.$$

Measurement error  $\eta_i$  is normally distributed with mean 0 and standard deviation  $\sigma$ . It is uncorrelated with  $\varepsilon_i$ ,  $u_{r(i)}$ ,  $u_{s(i)}$ ,  $z_i$ , and  $v_i$ . The leave-out mean is constructed from individual-level data, so measurement error affects the observed leave-out mean too:

$$\overline{x_{r(i)}}^* = \overline{x_{r(i)}} + \overline{\eta_{r(i)}}.$$

The distortion caused by measurement error can be measured using the signal-to-total variance ratio, which is:

$$STV = \frac{V[x_i]}{V[x_i^*]}.$$

The greater the standard deviation of the measurement error, the lower the information content of the observed variable.<sup>5</sup>

### IV.B Bias Due to Measurement Error

Using simulated data, I illustrate how estimates of spillovers depend on measurement error. I generate data based on equation 7, assuming that there is no spillover effect ( $\gamma = 0$ ). In the absence of measurement error ( $STV = 1$ ), the regression results are consistent. The estimated direct effect (coefficient on  $x_i^*$ ) is close to one and significant, while the estimated spillover (coefficient on  $\overline{x_{r(i)}}^*$ ) is small and insignificant (Table III, panel A, column 1).

With low measurement error ( $STV = 0.95$ ), the spillover becomes statistically significant. The ratio of spillover to direct effect rises to 5 percent (column 2). The greater the measurement error,

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<sup>5</sup>Under the assumptions in the simulations:  $STV = \frac{V[x_{ig}]}{V[x_{ig}^*]} = \frac{V[r_g] + V[z_{ig}] + V[u_{ig}]}{V[r_g] + V[z_{ig}] + V[u_{ig}] + \sigma^2} = \frac{6.671}{6.671 + \sigma^2}.$

the greater the spillover estimate. Bound and Krueger (1991) document that measurement error in earnings growth in the Current Population Survey leads to  $STV = 0.7$ . The ratio of spillover to direct effect is 223 percent with  $STV = 0.7$  (column 4). Hence, with an empirically plausible degree of measurement error, the estimated spillover is more than twice as large as the estimated direct effect, even though the true spillover is 0.

The intuitive reason for the overestimated spillover is the presence of systematic group-level variation (i.e., the variation in  $u_{r(i)}$ ). The individual measurement error partially gets averaged out when calculating  $\overline{x_{r(i)}}^*$ . As a result,  $\overline{x_{r(i)}}^*$  contains relatively less measurement error than  $x_i^*$  and relatively more information about the group-level component  $u_{r(i)}$ . That means some of the true direct effect (the part that is caused by high  $u_{r(i)}$ ) shows up in the spillover estimate.

#### IV.C Avoiding Bias Due to Measurement Error

As with multiple spillovers, IV overcomes the bias from measurement error. Using  $z_i$  as individual-level instrument (equation 4), the IV estimates are consistent (Table III, column 5).

Turning to group-level data without instrumenting does not eliminate the bias. The total effect at the group-level is underestimated relative to the true coefficient when using the group-level regressor  $\overline{x_i}^*$  (column 6). However, group-level instruments, such as  $\overline{z_i}$ , produce consistent estimates of the group-level effect (column 7).

Testing for heterogeneous spillovers, based on theory, can also help again. If spillovers are only significant for a subset of firms, for which theory predicts they should be, measurement error across all firm types cannot explain the spillover results.

#### IV.D The Direction of Bias Due to Measurement Error

The examples so far showed that measurement error can inflate a spillover estimate when the true spillover is zero. In general, measurement error can cause bias in either direction. Algebraically, the spillover estimate based on regression 7 converges to:

$$plim \hat{\gamma} = \beta C_1 + \gamma C_2,$$

where  $0 \leq C_1$ ;  $0 \leq C_2 \leq 1$ ; and  $C_1 = 0$  if  $u_{r(i)}$  is identical across regions.<sup>6</sup>

This equation shows that the spillover estimate is always attenuated if variation is not systematic (i.e.,  $u_{r(i)}$  is identical across regions). If there is systematic variation (i.e.,  $u_{r(i)}$  varies across regions), the relative magnitude of direct and spillover effects determines the bias. If the true direct effect is non-zero and the true spillover is zero ( $\beta \neq 0$  and  $\gamma = 0$ ), the direction of bias of

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<sup>6</sup>The full derivation and definitions of  $C_1$  and  $C_2$  are in Appendix B.



the spillover estimate has the sign of the direct effect. If the true direct effect is zero and the true spillover is non-zero ( $\beta = 0$  and  $\gamma \neq 0$ ), the spillover estimate is attenuated.

To illustrate this result, I generate data where the true direct and spillover effects are both one ( $\beta = \gamma = 1$ ). Under systematic variation, the spillover is overestimated (Table III, panel B, column 1). Under random group-level variation ( $u_{r(i)} = 0$ ), the spillover is attenuated (column 2).

## V Nonlinear Direct Effects Can Bias Spillover Estimates

The final estimation issue I analyze is bias caused by nonlinear direct effects.

### V.A Definition of Nonlinear Direct Effects

Nonlinear responses to shocks are common in financial settings. For instance, liquidity-constrained households extract housing equity when house prices go up, but do not inject equity when house prices fall (Cloyne et al. 2019). Similarly, large losses in bank capital have disproportionate effects on lending and real outcomes, relative to small losses (Brunnermeier and Sannikov 2014). I simulate a nonlinear direct effect by specifying the true data generating equation as:

$$y_i = w_i + \varepsilon_i,$$

where  $w_i$  is a nonlinear variable based on  $x_i$ :

$$w_i = \begin{cases} x_i & \text{if } x_i > 0, \\ 0 & \text{otherwise.} \end{cases}$$

The true spillover effects are zero. All variables are measured without error.

### V.B Bias Due to Nonlinear Direct Effects on Spillover Estimates

If researchers correctly account for the nonlinear relationship between  $y_i$  and  $x_i$ , the regression produces consistent estimates. The estimated direct coefficient on  $w_i$  is close to one and significant, while the regional spillover coefficient on  $\overline{w_{r(i)}}$  is small and insignificant (Table IV, column 1).

Researchers may not be aware of the underlying data generating process, however. Standard practice is to use linear regressors. In that case, the estimated spillover on the linear leave-out mean is positive and significant (column 2). The ratio of estimated spillover to direct effect is 20 percent. This result falsely suggests that spillover effects played an important role in amplifying the effects of the shock.

The ratio of estimated spillover to direct effect rises with the degree of nonlinearity. For instance, I redefine:

$$w_i = \begin{cases} x_i^2 & \text{if } x_i > 0, \\ 0 & \text{otherwise.} \end{cases}$$

The correctly specified regressors are still consistently estimated (column 3). However, using the linear regressors leads to an estimated ratio of 166 percent (column 4). This estimated ratio incorrectly implies that the spillover is quantitatively more important than the direct effect.

### **V.C Reason and Solutions for Bias Due to Nonlinearity**

The reason for the overestimated spillover is, once again, the systematic group-level variation in direct treatment status (i.e.,  $u_{r(i)}$  differs across groups). Intuitively, the specification with linear regressors fits the same coefficient for observations with  $x_i > 0$  and for observations with  $x_i \leq 0$ . As a result, the direct estimate is too low for observations with  $x_i > 0$  (relative to the true effect). Instead, some of the true direct effect for observations with  $x_i > 0$  (the part that is caused by high  $u_{r(i)}$ ) loads on the coefficient on the leave-out mean and generates bias. The bias gets worse with the degree of nonlinearity, as the wedge between true and estimate direct effect rises. With random variation (i.e.,  $u_{r(i)}$  identical across groups), using linear regressors does not produce a biased spillover estimate because there is no common component in direct exposure that could load onto the leave-out mean (columns 5 and 6).

The natural solution to nonlinearity is to relax the linearity assumption. For instance, plotting direct effects by bins of  $x_i$  should reveal which parts of the distribution of  $x_i$  are treated. Alternatively, approaches similar to the case of measurement error can help. IV estimates are consistent (column 7) and testing for heterogeneous spillovers can reject that spillover estimates are entirely spurious.

## **VI Conclusion**

Large-scale economic shocks affect individual firms and households through two types of channels: direct treatment effects and indirect spillovers due to treatment of others. I outline a simple method based on leave-out means that can be used to estimate spillovers. However, the method requires careful implementation because spillover estimates suffer from distinct sources of mechanical bias. For example, spillover estimates can be of the wrong sign, large, and statistically significant if true spillovers operate through channels that are not part of the empirical model. I show that instrumental variables, testing for heterogeneous effects, and flexible functional forms can alleviate

the bias.

Despite the implementation challenges, estimating spillovers using leave-out means offers great possibilities for researchers interested in general equilibrium forces. By estimating spillovers directly, researchers can contribute to the development of realistic general equilibrium models and improve our understanding of the connection between micro data and aggregate outcomes.

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## Tables

Table I: Testing for just one type of spillover biases estimates

	(1)	(2)	(3)	(4)	(5)
Coefficient on $x_i$ (true coefficient = 1)	1.001 (0.008)	1.625 (0.018)	0.993 (0.045)		1.001 (0.011)
Coefficient on $\overline{x_{r(i)}}$ (true coefficient = 1)	0.999 (0.009)	0.467 (0.025)	0.980 (0.199)		0.994 (0.033)
Coefficient on $\overline{x_{s(i)}}$ (true coefficient = 1)	0.999 (0.009)				
Coefficient on $\overline{x_i}$ (true coefficient = 2)				1.973 (0.214)	
Group-level variation Estimator	Systematic OLS	Systematic OLS	Systematic IV	Systematic IV	Random OLS

Notes: The variable  $x_i$  is the direct treatment status of firm  $i$ , which is in region  $r(i)$  and sector  $s(i)$ ; and  $\overline{x_{r(i)}}$  and  $\overline{x_{s(i)}}$  are the average treatment status over all other firms in  $r$  and  $s$ , respectively, apart from firm  $i$  (leave-out means). The IV specification in column 3 instrument for  $x_i$  and  $\overline{x_{r(i)}}$  using  $z_i$  and  $\overline{z_{r(i)}}$ . The IV specification in column 4 instruments for  $\overline{x_i}$  (the average of  $x_i$  across all firms in the region) using  $\overline{z_i}$  (the average of  $z_i$  across all firms in the region). Systematic variation means that  $u_{r(i)}$  and  $u_{s(i)}$  (from equation 4) are log-normally distributed with mean 0 and standard deviation 1. Random variation indicates that  $u_{r(i)}$  and  $u_{s(i)}$  are 0 for every firm. The reported coefficients and standard errors are averaged over 100 simulations.

Table II: Testing for the wrong spillovers biases estimates

	(1)	(2)	(3)
Coefficient on $x_i$ (true coefficient = 1)	1.001 (0.008)	1.625 (0.018)	1.001 (0.011)
Coefficient on $\overline{x_{r(i)}}$ (true coefficient = 0)	-0.001 (0.009)	-0.533 (0.025)	-0.006 (0.033)
Coefficient on $\overline{x_{s(i)}}$ (true coefficient = 1)	0.999 (0.009)		
Group-level variation Estimator	Systematic OLS	Systematic OLS	Random OLS

Notes: The variable  $x_i$  is the direct treatment status of firm  $i$ , which is in region  $r$  and sector  $s$ ; and  $\overline{x_{r(i)}}$  and  $\overline{x_{s(i)}}$  are the average treatment status over all other firms in  $r$  and  $s$ , respectively, apart from firm  $i$  (leave-out means). Systematic variation means that  $u_{r(i)}$  and  $u_{s(i)}$  (from equation 4) are log-normally distributed with mean 0 and standard deviation 1. Random variation indicates that  $u_{r(i)}$  and  $u_{s(i)}$  are 0 for every firm. The reported coefficients and standard errors are averaged over 100 simulations.

Table III: Measurement error biases spillover estimates

Panel A: Specifications with zero true spillover effect							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient on $x_i^*$ (true coefficient = 1)	1.002 (0.009)	0.948 (0.010)	0.806 (0.010)	0.261 (0.008)	1.010 (0.051)		
Coefficient on $\overline{x_{r(i)}}^*$ (true coefficient = 0)	-0.002 (0.011)	0.049 (0.011)	0.182 (0.013)	0.583 (0.030)	0.045 (0.221)		
Coefficient on $\overline{x_i}^*$ (true coefficient = 1)						0.845 (0.033)	1.055 (0.238)
Measurement error Estimator	No OLS	Low OLS	Medium OLS	High OLS	High IV	High OLS	High IV

Panel B: Specifications with zero true spillover effect

	(1)	(2)
Coefficient on $x_i^*$ (true coefficient = 1)	0.328 (0.009)	0.197 (0.007)
Coefficient on $\overline{x_{r(i)}}^*$ (true coefficient = 1)	1.376 (0.057)	0.197 (0.035)
Measurement error Estimator	High OLS	High OLS
Group-level variation	Systematic	Random

Notes: The variable  $x_i$  is the direct treatment status of firm  $i$ , which is in region  $r(i)$  and sector  $s(i)$ ; and  $\overline{x_{r(i)}}$  is the average treatment status over all other firms in  $r$ , respectively, apart from firm  $i$  (leave-out mean). An asterisk indicates that the variable is observed and may contain measurement error. The signal-to-total-variance ratio of  $x_i$  is 95 percent for low measurement error, 90 percent for medium measurement error, and 70 percent for high measurement error. The IV specification in panel A, column 5 instruments for  $x_i^*$  and  $\overline{x_{r(i)}}^*$  using  $z_i$  and  $\overline{z_{r(i)}}$ . The IV specification in panel A, column 7 instruments for  $\overline{x_i}^*$  (the average of  $x_i^*$  across all firms in the region) using  $\overline{z_i}$  (the average of  $z_i$  across all firms in the region). In all specifications, variation is systematic, which means that  $u_{r(i)}$  (from equation 4) is log-normally distributed with mean 0 and standard deviation 1. The reported coefficients and standard errors are averaged over 100 simulations.

Table IV: Nonlinear direct effects bias spillover estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient on $w_i$ (true coefficient = 1)	1.002 (0.012)		1.000 (0.002)		1.002 (0.017)		
Coefficient on $\bar{w}_{r(i)}$ (true coefficient = 0)	-0.002 (0.013)		-0.000 (0.002)		-0.004 (0.051)		
Coefficient on $x_i$ (true coefficient > 0)		0.793 (0.013)		4.594 (0.363)		0.503 (0.012)	0.773 (0.021)
Coefficient on $\bar{x}_{r(i)}$ (true coefficient = 0)		0.148 (0.016)		7.648 (2.025)		-0.003 (0.032)	-0.019 (0.102)
Regressor correctly specified	Yes	No	Yes	No	Yes	No	No
Definition of $w_i$	$w_i = x_i \text{ if } x_i > 0$	$w_i = x_i \text{ if } x_i > 0$	$w_i = x_i^2 \text{ if } x_i > 0$	$w_i = x_i^2 \text{ if } x_i > 0$	$w_i = x_i \text{ if } x_i > 0$	$w_i = x_i \text{ if } x_i > 0$	$w_i = x_i \text{ if } x_i > 0$
Group-level variation	OLS	OLS	OLS	OLS	OLS	OLS	IV
Group-level variation	Systematic	Systematic	Systematic	Systematic	Random	Random	Systematic

Notes: In columns 1, 2, 5, and 6,  $w_i = x_i \text{ if } x_i > 0$  and  $w_i = 0 \text{ if } x_i \leq 0$ . In columns 3 and 4,  $w_i = x_i^2 \text{ if } x_i > 0$  and  $w_i = 0 \text{ if } x_i \leq 0$ . The variable  $x_i$  is the direct treatment status of firm  $i$  in region  $r(i)$  and sector  $s(i)$ ; and  $\bar{x}_{r(i)}$  is the average treatment status over all other firms in region  $r(i)$ , apart from firm  $i$  (leave-out mean). The variable  $\bar{w}_{r(i)}$  is the leave-out mean of  $w_i$  in region  $r(i)$ , where  $w_i$  is defined as described in the column. The IV specification in column 5 instruments for  $x_i$  and  $\bar{x}_{r(i)}$  using  $z_i$  and  $\bar{z}_{r(i)}$ . Systematic variation means that  $u_{r(i)}$  (from equation 4) is log-normally distributed with mean 0 and standard deviation 1. Random variation indicates that  $u_{r(i)}$  is 0 for every firm. The reported coefficients and standard errors are averaged over 100 simulations.



# Online Appendix

## Appendix A Estimating Spillover Effects Through Networks

The estimation issues studied in the paper are relevant for researchers using variation at the individual and group level to estimate spillovers. In this section, I show that similar issues apply to the estimation of spillover effects through networks. Network analysis requires slightly different notation, but the intuition is similar. I then explicitly show how measurement error and nonlinear direct effects can bias the estimates of network spillovers.

### Appendix A.A Setup of a Network

Researchers often study how networks amplify shocks. For instance, an active literature focuses on the transmission of firm-level shocks to other firms, through production or financial linkages (Barrot and Sauvagnat 2016; Boehm et al. 2019; Carvalho and Tahbaz-Salehi 2019; Biermann and Huber 2020; Carvalho et al. 2020; Tintelnot et al. 2020). For the sake of concreteness, I describe the following analysis using the language of supply linkages in production networks, but the insights are more general.

A typical specification to analyze production networks is:

$$y_i = \theta x_i + \delta \overline{x_{(i)}} + \varepsilon_i, \quad (\text{A1})$$

where  $y_i$  is a firm-level outcome and  $x_i$  is the direct treatment status of firm  $i$ . The average treatment status of firms that are direct suppliers to firm  $i$  is:

$$\overline{x_{(i)}} = \frac{\sum_{j \neq i} (x_j \cdot \mathbb{1}\{j \text{ supplies to } i\})}{N_i}, \quad (\text{A2})$$

where  $\mathbb{1}\{j \text{ supplies to } i\}$  indicates whether firm  $j$  is a supplier to firm  $i$ . The number of suppliers to firm  $i$  is  $N_i$ . In the general network case and in all simulations below, links are directed, so that a link from  $j$  to  $i$  ( $j$  supplies  $i$ ) does not imply that there is also a link from  $i$  to  $j$ .<sup>A1</sup>

The key assumption is how direct treatment status is determined. I specify that:

$$x_i = r_i + \sum_{j \neq i} (r_j \cdot \mathbb{1}\{j \text{ supplies to } i\}) + u_i. \quad (\text{A3})$$

The first term  $r_i$  is a random factor associated with firm  $i$ . The second term is the sum of all factors associated with the suppliers to firm  $i$ . The third term  $u_i$  is a random error. The variables  $r_i$ ,  $\varepsilon_i$ , and  $u_i$  are uncorrelated, and each component is independently distributed across firms.

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<sup>A1</sup>The model in equation A1 can be generalized to include not just the treatment status of direct links, but also the treatment status of second order links (i.e., the treatment status of a supplier's supplier) and further higher order links (as in Carvalho et al. 2020). The intuition below also applies to such higher order analyses.

The second term implies that the treatment status of each firm is correlated with the treatment status of its suppliers. Such correlated treatment status occurs naturally if the creation of supply links is correlated with the process determining treatment status. For instance, if firms linked to the same supplier happen to be located in the same region (as in the case of sectoral clustering) and if treatment status is regionally concentrated (as in the case of natural disasters), then treatment status can be approximated by equation A3. Note that treatment status is still exogenous (i.e., uncorrelated with the error term  $\varepsilon_i$  in equation A1). Correlated treatment status simply means that the process determining treatment status is not exogenous to supply links. In experimental settings, treatment status is less likely to be correlated with suppliers' treatment status because researchers can randomize treatment status independently of regional concentration or other types of clustering.

### Appendix A.B Effects of Measurement Error on Network Spillover Estimates

To highlight the consequences of measurement error in network analysis, I run 100 simulations. In each simulation, I generate a random network among 500 firms with density 0.002. This implies that firms have on average one supplier, with a standard deviation of one. I assume that  $r_i$  is log-normally distributed with mean 0 and standard deviation 1. The error terms  $\varepsilon_i$  and  $u_i$  are drawn from a normal distribution with mean 0 and standard deviation 0.1.

I generate data where the true direct effect is one ( $\theta = 1$ ) but the network spillover effect is zero ( $\delta = 0$ ). If treatment status is measured without error, a regression of the firm outcome on  $x_i$  and  $\bar{x}_{(i)}$  produces consistent estimates (Table A.I, column 1). However, with measurement error, the network spillover effect is positive and significant (column 2).<sup>A2</sup> The ratio of network spillover to direct effect is 24 percent.

The intuitive reason for the bias in the network analysis is similar to above. There is a common factor in direct treatment status and supplier's treatment status. The common factor is relatively stronger, and measurement error is relatively weaker, in the measure of suppliers' treatment status. As a result, some of the true direct effect loads onto the spillover estimate.

### Appendix A.C Effects of Nonlinear Direct Effects on Network Spillover Estimates

The network spillover estimate can also be biased if the true direct effect is nonlinear. To analyze the impact of nonlinearity, I define:

$$w_i = \begin{cases} x_i^2 & \text{if } x_i > 0, \\ 0 & \text{otherwise.} \end{cases}$$

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<sup>A2</sup>The specification of measurement error is the same as in Section IV.A above. Direct treatment status  $x_i$  can only be measured with error, such that  $x_i^* = x_i + \eta_i$ . Measurement error  $\eta_i$  is draw from a normal distribution with mean 0 and standard deviation  $\sigma$ . It is uncorrelated with  $\varepsilon_i$ ,  $r_i$ , and  $u_i$ . I set  $\sigma$  so that the signal-to-total variance ratio equals 0.7.

Table A.I: Estimates of network spillovers are biased under measurement error and nonlinear direct effects

	(1)	(2)	(3)	(4)
Coefficient on $x_i^*$ (true coefficient = 1)	1.000 (0.001)	0.656 (0.042)		0.411 (0.050)
Coefficient on $\bar{x}_{(i)}^*$ (true coefficient = 0)	0.000 (0.002)	0.158 (0.036)		0.077 (0.021)
Coefficient on $w_i$ (true coefficient = 1)			1.000 (0.003)	
Coefficient on $\bar{w}_{(i)}$ (true coefficient = 0)			0.000 (0.004)	
Measurement error	No	Yes	No	No
True direct effects are nonlinear	No	No	Yes: $w_i = x_i^2$ if $x_i > 0$	

Notes: In columns 1 and 2, the true data generating equation is  $y_i = x_i + \varepsilon_i$ . The variable  $x_i^*$  is the observed direct treatment status of firm  $i$  and  $\bar{x}_{(i)}^*$  is the observed average treatment status over all suppliers of firm  $i$ . The variables are measured correctly in columns 1, 3, and 4. The variables are measured with error in column 2, so that the signal-to-total-variance ratio of  $x_i$  is 0.7. In columns 3 and 4, the true data generating equation is  $y_i = w_i + \varepsilon_i$ , where  $w_i = x_i^2$  if  $x_i > 0$  and  $w_i = 0$  if  $x_i \leq 0$ . The reported coefficients and standard errors are averaged over 100 simulations.

Direct treatment status  $x_i$  is determined as in equation A3 above.<sup>A3</sup> I assume that the true direct effect of  $w_i$  is one ( $\theta = 1$ ) and the network spillover effect is zero ( $\delta = 0$ ), so that the true data generating process is given by:

$$y_i = w_i + \varepsilon_i. \quad (\text{A4})$$

If researchers specify the nonlinear relationship between  $x_i$  and  $y_i$  correctly, the regression produces consistent estimates (Table IV, column 3). But if researchers use linear regressors, as is standard practice, the estimates are biased and the ratio of network spillover to direct effect is 19 percent (column 4).

The reason for the bias is, once again, the factor  $r_i$  that is common to the direct treatment status of firm  $i$  and suppliers' treatment status. The coefficient on  $x_i$  estimates a linear direct effect. Conditional on this linear effect, there remains a nonlinear correlation between suppliers' treatment status and the outcome  $y_i$ , induced by the factor  $r_i$  in suppliers' treatment status. This leads to a significant, large, and inconsistent estimate of the network spillover.

<sup>A3</sup>The random network and other random terms also follow the calibration above. The only difference is that the mean of the random error  $u_i$  is negative for the purpose of this section (equal to the negative of the 90th percentile of the distribution of  $r_i + \sum_{j \neq i} (r_j \cdot \mathbb{1}\{j \text{ supplies to } i\})$ ). If this mean was not negative, almost all observations would have positive  $x_i$  and there would not be a nonlinear direct effect of  $x_i$  and  $y_i$ .

## Appendix B Derivation of the Bias Due to Measurement Error

The true model is:

$$y_i = \beta x_i + \gamma \overline{x_{r(i)}} + \varepsilon_i. \quad (\text{A5})$$

Direct treatment status  $x_i$  is measured with error. The observed variables are:

$$\begin{aligned} x_i^* &= x_i + \eta_i = u_{r(i)} + z_i + v_i + \eta_i, \\ \overline{x_{r(i)}^*} &= \overline{x_{r(i)}} + \overline{\eta_{r(i)}} = \overline{u_{r(i)}} + \overline{z_{r(i)}} + \overline{v_{r(i)}} + \overline{\eta_{r(i)}}. \end{aligned}$$

I assume that the variables  $\varepsilon_i$ ,  $u_{r(i)}$ ,  $z_i$ , and  $v_i$  are uncorrelated with each other.

The OLS estimator of  $\gamma$  is:

$$\begin{aligned} \hat{\gamma} &= \frac{\sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (y_i - \overline{y_i}) \sum_i (x_i^* - \overline{x_i^*})^2 - \sum_i (x_i^* - \overline{x_i^*}) (y_i - \overline{y_i}) \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*})}{\sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right)^2 (x_i^* - \overline{x_i^*})^2 - \left( \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*}) \right)^2} \\ &= \frac{\sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (\beta (x_i - \overline{x_i}) + \gamma (\overline{x_{r(i)}} - \overline{\overline{x_{r(i)}}}) + (\varepsilon_i - \overline{\varepsilon_i})) \sum_i (x_i^* - \overline{x_i^*})^2}{\sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right)^2 (x_i^* - \overline{x_i^*})^2 - \left( \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*}) \right)^2} \\ &= \frac{\sum_i (x_i^* - \overline{x_i^*}) (\beta (x_i - \overline{x_i}) + \gamma (\overline{x_{r(i)}} - \overline{\overline{x_{r(i)}}}) + (\varepsilon_i - \overline{\varepsilon_i})) \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*})}{\sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right)^2 (x_i^* - \overline{x_i^*})^2 - \left( \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*}) \right)^2} \\ &= \frac{\sum_i \left( \beta (x_i - \overline{x_i}) \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) + \gamma (\overline{x_{r(i)}} - \overline{\overline{x_{r(i)}}}) \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) + (\varepsilon_i - \overline{\varepsilon_i}) \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) \right) \sum_i (x_i^* - \overline{x_i^*})^2}{\sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right)^2 (x_i^* - \overline{x_i^*})^2 - \left( \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*}) \right)^2} \\ &= \frac{\sum_i (\beta (x_i - \overline{x_i}) (x_i^* - \overline{x_i^*}) + \gamma (\overline{x_{r(i)}} - \overline{\overline{x_{r(i)}}}) (x_i^* - \overline{x_i^*}) + (\varepsilon_i - \overline{\varepsilon_i}) (x_i^* - \overline{x_i^*})) \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*})}{\sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right)^2 (x_i^* - \overline{x_i^*})^2 - \left( \sum_i \left( \overline{x_{r(i)}^*} - \overline{\overline{x_{r(i)}^*}} \right) (x_i^* - \overline{x_i^*}) \right)^2}, \end{aligned} \quad (\text{A6})$$

where the first equality is the definition of the OLS estimator with two regressors. The second equality comes from substituting the true equation A5 for  $y_i$ . The third equality comes from rearranging terms.

The probability limit of the OLS estimator is:

$$\begin{aligned}
plim \hat{\gamma} &= \frac{\left( \beta Cov \left( x_i, \overline{x_{r(i)}^*} \right) + \gamma Cov \left( \overline{x_{r(i)}}, \overline{x_{(i)}^*} \right) + Cov \left( \varepsilon_i, \overline{x_{r(i)}^*} \right) \right) V \left( x_i^* \right)}{V \left( \overline{x_{r(i)}^*} \right) V \left( x_i^* \right) - Cov \left( \overline{x_{r(i)}^*}, x_i^* \right)^2} \\
&\quad - \frac{\left( \beta Cov \left( x_i, x_i^* \right) + \gamma Cov \left( \overline{x_{r(i)}}, x_i^* \right) + Cov \left( \varepsilon_i, x_i^* \right) \right) Cov \left( \overline{x_{r(i)}^*}, x_i^* \right)}{V \left( \overline{x_{r(i)}^*} \right) V \left( x_i^* \right) - Cov \left( \overline{x_{r(i)}^*}, x_i^* \right)^2} \\
&= \frac{\left( \beta V \left( u_{r(i)} \right) + \gamma \left( V \left( u_{r(i)} \right) + \frac{V(z_i) + V(u_i)}{\bar{N} - 1} \right) \right) \left( V \left( u_{r(i)} \right) + V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)}{\left( V \left( u_{r(i)} \right) + V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right) \left( V \left( u_{r(i)} \right) + \frac{V(z_i) + V(u_i) + V(\eta_i)}{\bar{N} - 1} \right) - V \left( u_{r(i)} \right)^2} \\
&\quad - \frac{\left( \gamma \left( V \left( u_{r(i)} \right) + V \left( z_i \right) + V \left( u_i \right) \right) + \gamma \left( V \left( u_{r(i)} \right) \right) \right) V \left( u_{r(i)} \right)}{\left( V \left( u_{r(i)} \right) + V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right) \left( V \left( u_{r(i)} \right) + \frac{V(z_i) + V(u_i) + V(\eta_i)}{\bar{N} - 1} \right) - V \left( u_{r(i)} \right)^2}
\end{aligned}$$

where the first equality comes from substituting covariances and variances for the probability limits of the individual terms in equation A6. The second equality comes from solving for the covariances and variances.  $\bar{N}$  is the average number of firms per group. Finally, rearranging gives:

$$\begin{aligned}
plim \hat{\gamma} &= \beta \frac{(\bar{N} - 1) V \left( u_{r(i)} \right) V \left( \eta_i \right)}{\left( V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)^2 + \bar{N} V \left( u_{r(i)} \right) \left( V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)} \\
&\quad + \gamma \frac{(\bar{N} - 1) V \left( u_{r(i)} \right) \left( V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)}{\left( V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)^2 + \bar{N} V \left( u_{r(i)} \right) \left( V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)} \\
&\quad + \gamma \frac{\left( V \left( z_i \right) + V \left( u_i \right) \right) \left( V \left( u_{r(i)} \right) + V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)}{\left( V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)^2 + \bar{N} V \left( u_{r(i)} \right) \left( V \left( z_i \right) + V \left( u_i \right) + V \left( \eta_i \right) \right)}.
\end{aligned}$$