

# Handling Spillover Effects in Empirical Research: An Application using Credit Supply Shocks

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## **Abstract**

Despite their importance, the discussion of spillover effects in empirical research misses the rigor dedicated to endogeneity concerns. We show that i) even with random treatment, spillovers lead to an intricate bias in estimating treatment effects, ii) there is a trade-off between endogeneity and spillover concerns, iii) the current practice of using individual level regressions to identify direct effects and aggregate level regressions to learn about spillover effects can lead to misleading conclusions. We develop a simple guidance for empirical researchers, apply it to a credit supply shock, and highlight differences in the results compared to current empirical practice.

**Keywords:** Spillovers, Credit Supply, Direct vs. Indirect Effects, Aggregate Effects

**JEL:** C13, C21, G21, G32, R11, R23

# 1 Introduction

Much of the debate in empirical studies revolves around identifying as good as random variation in treatment assignment. However, even with random assignment, identification of treatment effects can be confounded by spillover effects. While the problem of spillovers is well understood [going back at least to Cox (1958)], it is often ignored or improperly handled in empirical research, with possibly severe consequences for the interpretation of results. The empirical discussion of spillovers often follows an ad-hoc approach, missing the rigor that is dedicated to the discussion of endogeneity. In this paper, we discuss challenges arising in the presence of spillover effects, provide guidance to empirical researchers, and apply our framework to a setting where a credit supply shock has both direct effects on treated firms as well as spillover effects on other firms.<sup>1</sup>

We use a simple model that includes a direct treatment effect as well as spillover effects that depend linearly on the fraction of treated units in a group, for example, a region or an industry. Our conceptual discussion can be summarized in three key messages: First, spillover effects bias the treatment effects estimate even if treatment assignment is completely random. Positive spillovers to treated (control group) units lead to an overestimation (underestimation) of the treatment effect. Importantly, even in a simple model the exact bias is intricate and depends on higher-order moments of the treatment distribution across groups. Second, there is a tension between endogeneity and spillover concerns. We show that commonly applied methods such as absorbing unobserved heterogeneity through (group-) fixed effects can exacerbate the spillover-induced estimation bias. Third, researchers often assess spillover concerns by comparing aggregate, i.e., group level, regressions with individual level regressions and attribute differences in the estimates to spillover effects. We show that such an interpretation is generally not warranted. This is because i) the individual level regressions do not measure the direct effect, ii) the group level averages are a non-linear function of treatment intensity even if treatment and spillover effects are both linear, and iii) any regression hinges on the precise definition of the level at which spillovers occur.

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<sup>1</sup>In the following, we use the terms “spillovers”, “spillover effects”, and “indirect effects” interchangeably.

A simple example may foster the intuition of these results. There are several coffee shops in a city, some of which are randomly exposed to a fire that destroys their coffee machines. The researcher is interested in whether the fire decreases sales for treated coffee shops.<sup>2</sup> Suppose that non-treated coffee shops experience a spillover effect, that is, an increase in coffee sales that is stronger the more coffee shops are exposed to a fire in the same city. We can write this example as:<sup>3</sup>

$$y_{ig} = \beta_0 + \beta_1 d_{ig} + \text{Spillovers} + \epsilon_{ig},$$

$$\text{Spillovers} = \begin{cases} \beta_C \bar{d}_g & \text{for } d_{ig} = 0 \\ \beta_T \bar{d}_g & \text{for } d_{ig} = 1 \end{cases} \quad (1)$$

where  $y_{ig}$  are sales at coffee shop  $i$  in city  $g$ ,  $d_{ig}$  is a treatment indicator for coffee shop  $i$  in city  $g$  and  $\bar{d}_g$  is the average fraction of treated units in the city. In our example,  $\beta_C > 0$  and  $\beta_T = 0$ . Suppose the researcher estimates equation (2) below instead of (1).

$$y_{ig} = \tilde{\beta}_0 + \tilde{\beta}_1 d_{ig} + \epsilon_{ig}. \quad (2)$$

As  $\tilde{\beta}_1$  measures the difference between treated and control shops, it captures both the direct effect on treated shops (a decrease in sales) and the spillover effect on non-treated shops (an increase in sales). In this example this would result in an overestimation of the direct effect.

Now suppose that coffee shops in several different cities are exposed to the shock but spillover effects are confined within cities. If half of the coffee shops in every city are treated, the other coffee shops experience a positive spillover effect. If, on the other hand, *all* coffee shops are treated in half of the cities but none in the other half, spillovers do not affect the estimate  $\tilde{\beta}_1$ . More precisely, the degree of the bias when estimating (2) not only depends on the presence of spillovers in (1), but also on higher-order moments of group-average treatment intensities  $\bar{d}_g$  in the data at hand. We show that the bias arising from estimating a model without spillovers is hard to understand at best, and impossible

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<sup>2</sup>A coffee lover might argue that this is trivial. On the other hand, someone lacking the appreciation for delicious coffee might believe that people switch to buying hot chocolate at the same place instead.

<sup>3</sup>The equation can be written as  $y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_T \bar{d}_g d_{ig} + \beta_C \bar{d}_g (1 - d_{ig}) + \epsilon_{ig}$ . We choose the illustration in (1) for ease of exposition.

to interpret at worst.

The example also highlights the trade-off between endogeneity concerns and spillover effects. Being geographically close and hence likely exposed to similar confounding factors, non-treated coffee shops in the same city form a natural control group. This group, however, is precisely the group most affected by spillovers. Further, including (city) fixed effects, a common approach to strengthen identification in the presence of endogeneity concerns, can exacerbate the bias arising from spillovers.

An approach that is often used to address spillover effects is to compare estimates at different aggregation levels, i.e. to estimate

$$\bar{y}_g = \gamma_0 + \gamma_1 \bar{d}_g + u_g, \quad (3)$$

where, in our example,  $\bar{y}_g$  are city level average coffee sales and  $\bar{d}_g$  is the city level average of coffee shops exposed to a fire. Firm level estimates, i.e.,  $\tilde{\beta}_1$ , are often interpreted as the direct treatment effect, while aggregate effects, that is,  $\gamma_1$ , are interpreted as comprising both direct and spillover effects. This approach is problematic for three reasons: i) estimating (2) does not yield the direct effect ( $\beta_1$ ), as discussed above, and the difference between  $\gamma_1$  and  $\tilde{\beta}_1$  can therefore not be interpreted as resulting from spillover effects either. ii) Even in the simple linear model (1), group level averages are non-linear functions of  $\bar{d}_g$ . We show that the spillover effects ( $\beta_C, \beta_T$ ) can therefore be under- or overestimated by up to 100% when estimating (3). iii) The estimate of  $\gamma_1$  in (3) crucially hinges on the correct definition of the aggregation level. Last but not least, estimating regressions on group level aggregates makes inefficient use of the data at hand. Direct effects, spillover effects, and aggregate effects can be directly obtained from the parameters of the individual level regression (1).

We provide a simple three-step guidance to analyze direct effects, spillover effects, and aggregate effects. First, researchers should consider the most plausible dimension for spillovers. Guidance in this first step will ultimately come from economic theory as well as from institutional knowledge of the setting at hand. Second, researchers should estimate the four key parameters from the individual level regressions that include spillover terms

(that is, equation (1)). Third, we provide a simple way to illustrate direct effects, spillover effects, and aggregate effects in a single graph. We hope that this guidance will be useful to academics in future research.

We apply our framework to a setting in which a credit supply shock has both direct and indirect effects. We follow Huber (2018) and examine the effects of bank lending on the real economy exploiting an exogenous lending cut by Commerzbank, a large German bank, during the financial crisis. The lending cut was induced by losses in the bank's international trading book, i.e., the event constitutes an exogenous shock from the perspective of any given German borrower. Huber (2018) documents that firms' directly dependent on loan supply from Commerzbank reduce employment over the 2008 to 2012 period as result of the shock. Further, he provides evidence for indirect effects at the county level. The average Commerzbank exposure across all firms in a county affects firms' employment growth, keeping the firms' direct exposure to Commerzbank constant.

Applying our framework to this setting uncovers novel insights. We document that spillover effects at the county level differentially affect treated, i.e., Commerzbank dependent, firms and control firms, i.e., firms without a lending relationship with Commerzbank. In particular, any negative spillover effects are purely driven by the control group firms, who are negatively affected by the presence of treated firms in the same county. Commerzbank dependent firms, in contrast, experience a negative *direct* effect on employment as result of the shock but this effect is *independent* of the Commerzbank dependence of other firms in the same county.

More generally, this highlights that not accounting for (asymmetric) spillover effects leads to a biased estimate of the direct effect, i.e., the effect of the lending cut on employment for a firm fully dependent on Commerzbank in a county where no other firm is Commerzbank dependent. We document a baseline estimate for the direct effect on employment of -2 percentage points when allowing for symmetric spillover effects only, which is consistent with Huber (2018). This estimate increases at least by a factor of 2-3 when applying our framework. That is, part of the differential spillover effect will be included in the "direct effect estimate" in the baseline specification resulting in a severe downward bias in the estimation.

Further, we more explicitly shift attention to the level at which spillover effects should be estimated. In particular, we argue that without guidance by economic theory or a direct empirical investigation of different levels at which spillover effects can potentially occur, the group definition can be arbitrary. The choice at which level spillovers are accounted for, however, can severely affect the estimation results. We provide evidence that spillover effects are likely not confined within counties. Controlling for county level spillovers, the average Commerzbank dependence of firms in other counties in the same federal state continues to negatively affect control group firms.

Finally, we compare firm level regression with estimations aggregated at the county level as this approach is often used by researchers to address concerns that results are distorted by spillover effects. Consistent with our conceptual framework we find that group level averages are a non-linear function of treatment intensity even if treatment and spillover effects are assumed to be linear at the disaggregated level. For example, based on the parameters of our model, aggregate county-level employment decreases by 2.2% if county-level treatment intensity increases from 0% to 10%, but it only decreases by a further 1.1%, that is, half the 2.2%, if county-level treatment intensity increases from 30% to 40%. In other words, the first firms hit by a credit supply shock have a proportionally larger effect on county-level employment than the next 10%.

We contribute to the literature on identification in the presence of spillover effects.<sup>4</sup> This discussion is significantly influenced by the literature on social interactions. A commonly applied model is a simple linear-in-means (LIM) model, where an individual's outcome varies with the average outcome of other individuals in the reference group. In a seminar paper, Manski (1993) discusses identification challenges in the LIM model. This has spurred significant follow-up work, e.g. allowing for more complex and flexible interaction effects and providing microfoundations (e.g., Manski, 2013; Blume, Brock, Durlauf, and Jayaraman, 2015). A related strand of the literature deals with spillover effects in experimental design (e.g., Duflo and Saez, 2003; Baird, Bohren, McIntosh, and Özler, 2018). Here, studies can be designed in a way such that indirect effects can be explicitly identified. Duflo and Saez (2003), for instance, define spillover effects as the average

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<sup>4</sup>The literature on spillover effects is large, i.e., the reference list provided here is not meant to be exhaustive.

difference in outcomes between control units in treated clusters and control units in pure control clusters.

In this paper we focus on identification problems arising from spillover effects in (quasi) natural experiments. We highlight identification challenges using a simple but flexible model with spillovers that is closely linked to current practice in empirical research in financial economics. We further propose an easily applicable roadmap how to handle spillover effects in such situations and use our framework to estimate the real effects of a credit supply shock to highlight differences in the results compared to standard approaches.

Second, we contribute to the literature on the effects of credit supply on the real economy (see, for instance, Chodorow-Reich, 2014; Khwaja and Mian, 2008).<sup>5</sup> The general focus of these papers is on the effect of (plausibly exogenous) credit supply changes on firms affected by the shock. In most papers, spillovers on firms in the vicinity are not explored or directly accounted for. A notable exception is Huber (2018), who provides evidence for spillover effects at the county level. We use this setting to document that the indirect effects of a credit supply shock can asymmetrically affect treated and control firms and that not properly accounting for spillover effects can bias the estimated direct effect of a credit supply shock on affected firms.

## 2 Conceptual framework

### 2.1 Current approach in the literature

We start by providing basic notation using the potential outcome framework.<sup>6</sup> Let  $d_{ig}$  be a treatment indicator for unit  $i$  in group  $g$  (the meaning of the group will be discussed below) that is equal to 1 if treatment is received, and zero otherwise. There are two potential outcomes, the outcome under treatment,  $y_{ig}(1)$ , and the outcome under no treatment,

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<sup>5</sup>The literature that examines the effects of credit supply shocks is large. See also Acharya, Eisert, Eufinger, and Hirsch (2018), Aiyar (2012), Almeida, Campello, Laranjeira, and Weisbenner (2011), Amiti and Weinstein (2011), Chava and Purnanandam (2011), Haas and Horen (2012), Gan (2007), Iyer, Peydró, da Rocha-Lopes, and Schoar (2014), Ivashina and Scharfstein (2010), Paravisini, Rappoport, Schnabl, and Wolfenzon (2014), Peek and Rosengren (1997), Peek and Rosengren (2000), Santos (2011), and Sufi (2009), among others.

<sup>6</sup>The following notation closely follows Angrist and Pischke (2009) and Roberts and Whited (2012).

$y_{ig}(0)$ . The treatment effect is given by  $y_{ig}(1) - y_{ig}(0)$ . If treatment assignment is random, then the average difference between the outcomes of treated units and the outcomes of non-treated units, i.e.,  $E[y_{ig}|d = 1] - E[y_{ig}|d = 0]$ , is equal to the average treatment effect.

In settings where spillovers and aggregate effects are of interest, the current literature follows a two-step procedure: Regressions on a disaggregated level provide estimates of the treatment effect. Regressions on an aggregated level are used to inform the researcher about spillovers and aggregated effects:

$$y_{ig} = \tilde{\beta}_0 + \tilde{\beta}_1 d_{ig} + \tilde{\epsilon}_{ig} \quad (4)$$

$$\bar{y}_g = \gamma_0 + \gamma_1 \bar{d}_g + u_g \quad (5)$$

As an example, Khwaja and Mian (2008) use firm-bank level data to establish the existence of a credit supply effect. Data aggregated on the firm level is used to analyze spillovers (i.e., firms substituting a loss in funding via other banks) and aggregate effects. Other examples of firms following this two-step procedure are Huber (2018), Bentolila and Jiminéz (2018), Berton, Mocetti, Presbitero, and Richiardi (2018), and Cingano and Sette (2016).

## 2.2 Estimating treatment in the presence of spillovers

### 2.2.1 The general case

The notations  $y_{ig}(0)$  (outcome under treatment) and  $y_{ig}(1)$  (outcome under no treatment) are not well defined in the presence of spillovers. For example, does  $y_{ig}(0)$  measure the no-treatment outcome when all other units are also not treated, or in case other units are treated? In the absence of spillovers, these are the same. However, in the presence of spillovers, we need to be more precise in defining control and treatment outcomes.

Implicit in the notation  $y_{ig}(0)$  and  $y_{ig}(1)$  is thus the no spillover assumption, also referred to as either “no interference between units assumption” or “stable unit treatment value assumption (SUTVA)”. The no spillover assumption assumes that the outcome for unit  $i$  does not depend on the treatment assignment of other units. In practice, outcomes for one unit might not only depend on its own treatment status, but also on the treatment status of other units. For example, a firm is likely to be affected differently by a credit

supply shock if other firms in the same industry are also affected by the same shock.

In order to formalize this in a tractable framework, we assume that each unit  $i$  belongs to a group  $g$  (such as industry or region) that is known and observable to the researcher. Spillovers occur within groups but not across groups. The potential outcome of unit  $i$  depends on treatment status  $d_i$  and the total fraction of units treated in her group  $g$ , denoted by  $\bar{d}_g$ .<sup>7</sup> The no-program outcome  $y_{ig}(-1)$  is defined as the outcome in case that no unit receives treatment. In case that a fraction of  $\bar{d}_g$  units receives treatment in group  $g$ , the outcome for the treated units in group  $g$  is defined as  $y_{ig}(1, \bar{d}_g)$  and the outcome for the control group units is defined as  $y_{ig}(0, \bar{d}_g)$  with  $y_{ig}(0, 0) = y_{ig}(-1)$ .<sup>8</sup>

$$y_{ig} = y_{ig}(d_i, \bar{d}_g) = \begin{cases} y_{ig}(-1) & \text{no program} \\ y_{ig}(1, \bar{d}_g) & d_i = 1, \text{ fraction } \bar{d}_g \text{ of units treated in group } g \\ y_{ig}(0, \bar{d}_g) & d_i = 0, \text{ fraction } \bar{d}_g \text{ of units treated in group } g \end{cases} \quad (6)$$

### 2.2.2 A simple model

We rely on a linear specification of spillover effects in the remaining part of the paper:

$$\text{True model : } y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_T \bar{d}_g d_{ig} + \beta_C \bar{d}_g (1 - d_{ig}) + \epsilon_{ig}. \quad (7)$$

The spillover model (7) contains a direct treatment effect ( $\beta_1$ ) as well as spillover effects to treated units ( $\beta_T$ ) and to control units ( $\beta_C$ ).<sup>9</sup> We choose a linear model to stay as close as possible to the current practice in empirical research. In Appendix A, we show that a model with linear spillover effects arises naturally from a simple Cournot model.

[Figure 1 here]

Figure 1 provides an illustration of three possible scenarios. Case 1 shows a situation

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<sup>7</sup>The latter two assumptions are also referred to as exchangeability (spillovers do not depend on specific identity of treated “neighbors”) and partial interference (spillovers confined within group), see Vazquez-Bare (2018).

<sup>8</sup>The identity  $y_{ig}(0, 0) = y_{ig}(-1)$  holds by definition and the notation  $y_{ig}(-1)$  is therefore formally redundant. We use the notation  $y_{ig}(-1)$  to foster understanding of the following formulae.

<sup>9</sup>Note that model (7) is equivalent to  $y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_2 \bar{d}_g + \beta_3 \bar{d}_g d_{ig} + \epsilon_{ig}$  with  $\beta_2 = \beta_C$  and  $\beta_3 = \beta_T - \beta_C$ . We prefer to use (7) because it more clearly disentangles the two spillover dimensions (spillover on treated units, spillover on control units).

without spillovers effects ( $\beta_T = 0$  and  $\beta_C = 0$ ). In this case, the outcome of the treated and the control units is independent of the fraction of units who receive treatment. The average of the outcome variable is therefore a linear function of the treatment fraction  $\bar{d}_g$ .

Case 2 illustrates a case with homogenous spillovers ( $\beta_C = \beta_T$ ). Consider a situation in which some firms in the economy receive cheaper financing. This can have negative externalities on control group firms: the more of their direct competitors receive cheaper financing, the lower their output. However, there are negative spillovers effects on treatment group firms as well: the more of their direct competitors receive cheaper financing, the smaller is the advantage vis-a-vis their competitors, and hence the lower their output.<sup>10</sup>

Case 3 illustrates a case with spillover effects on control group units but no spillover effects on treatment group units ( $\beta_C > 0, \beta_T = 0$ ). Consider for instance a credit supply shock that results in insolvency of the treated firms. Control group firms benefit from insolvent firms dropping out of the market, and the benefit is larger the more firms become insolvent.

### 2.2.3 Interpretation of spillover effects as an omitted variable

The crux of (7) is that the spillover effects are mechanically correlated with the treatment effect. Even if treatment is assigned randomly and thus

$$Cov(d_{ig}, \epsilon_{ig}) = 0, \tag{8}$$

the spillover terms are still correlated with treatment:

$$Cov(d_{ig}, \beta_T \bar{d}_g d_{ig} + \beta_C \bar{d}_g (1 - d_{ig})) \neq 0, \tag{9}$$

and the estimate for  $\beta_1$  is therefore affected by spillovers to treated and control units.<sup>11</sup>

We need both the conditional independence assumption (CIA:  $Cov(d_{ig}, \epsilon_{ig}) = 0$ ) and the

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<sup>10</sup>Case 2 arises from a Cournot competition with a linear inverse demand function, see Appendix A.

<sup>11</sup>Note that this also holds when  $\beta_T = \beta_C$ . In this case,  $\beta_T \bar{d}_g d_{ig} + \beta_C \bar{d}_g (1 - d_{ig}) = \beta_T \bar{d}_g$ . However,  $Cov(d_{ig}, \bar{d}_g) \neq 0$  for  $Var(\bar{d}_g) \neq 0$  because treated units are disproportionately more prevalent in high-treatment groups while control units are disproportionately more prevalent in low-treatment groups.

no spillover assumption (SUTVA) to estimate  $\beta_1$  from the observed difference between treatment and control units:

$$E[y_{ig}|d_{ig} = 1] - E[y_{ig}|d_{ig} = 0] \quad (10)$$

$$= E[\beta_0 + \beta_1 + \beta_T \bar{d}_g + \epsilon_{ig}|d_{ig} = 1] - E[\beta_0 + \beta_C \bar{d}_g + \epsilon_{ig}|d_{ig} = 0] \quad (11)$$

$$\stackrel{CIA}{=} \beta_1 + \beta_T E[\bar{d}_g|d_{ig} = 1] - \beta_C E[\bar{d}_g|d_{ig} = 0] \quad (12)$$

$$\stackrel{SUTVA}{=} \beta_1. \quad (13)$$

Under random assignment or, more precisely, the conditional independence assumption,  $E[\epsilon_{ig}|d_{ig} = 1] = E[\epsilon_{ig}|d_{ig} = 0]$ . Under the SUTVA, the spillover term  $\beta_T E[\bar{d}_g|d_{ig} = 1] - \beta_C E[\bar{d}_g|d_{ig} = 0]$  equals zero.

One might argue that the term  $\beta_T E[\bar{d}_g|d_{ig} = 1] - \beta_C E[\bar{d}_g|d_{ig} = 0]$  is a feature rather than a bug because the spillover effects are also caused by treatment assignment (even though by treatment assignment to other units). This argument misses two important points. First,  $E[\bar{d}_g|d_{ig} = 1]$  is generally not equal to  $E[\bar{d}_g|d = 0]$ . As an illustrative example, assume that  $\bar{d}_g \in \{0.2, 0.8\}$ . In this case, most treated units are in groups with  $\bar{d}_g = 0.8$  while most control group units are in groups with  $\bar{d}_g = 0.2$ . Thus,  $\beta_T E[\bar{d}_g|d = 1] - \beta_C E[\bar{d}_g|d = 0]$  does not only measure the change in spillovers from a change in treatment status, but also the effect of a change in group affiliation from a high-treatment to a low-treatment group.

Second, in the presence of spillovers, the term “treatment effect” is not precisely defined. The three key parameters in equation (7) can be used to calculate various treatment effects that are of potential interest to a researcher. In particular, a researcher might be interested in the treatment-minus-control effect ( $y(1, \bar{d}_g) - y(0, \bar{d}_g) = \beta_1 + (\beta_T - \beta_C)\bar{d}_g$ ), the treatment-minus-no-program effect ( $y(1, \bar{d}_g) - y(0, 0) = \beta_1 + \beta_T \bar{d}_g$ ), or the control-minus-no-program effect ( $y(0, \bar{d}_g) - y(0, 0) = \beta_C \bar{d}_g$ ). In the absence of spillovers, the treatment-minus-control effect and the treatment-minus-no-program effect are the same, and the control-minus-no-program effect is equal to zero. In the presence of spillover effects, it is important to clearly state which of these effects one intends to measure.

## 2.2.4 Bias in estimating treatment effects when spillovers are ignored

In the following, we discuss the results of estimating (4) and (5) when the true model is (7). Proposition 1 and 2 show that estimating (4) does not generally yield the direct treatment effect, while Proposition 3 shows that estimating (5) does not generally yield the aggregate effect.

**Proposition 1** *Assume  $y_i$  follows (7). Estimating (4) yields*

$$\tilde{\beta}_1 = \beta_1 + (\beta_T - \beta_C)\bar{d} + \beta_T \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \beta_C \frac{\text{Var}(\bar{d}_g)}{1 - \bar{d}} \quad (14)$$

Proof: See Appendix B.

Proposition 1 establishes that estimating (4) does not generally yield the direct treatment effect  $\beta_1$ . The bias in (14) is driven by two effects. The first term  $((\beta_T - \beta_C)\bar{d})$  arises from differential spillover effects. If spillover effects are different for treated versus control group units, then this difference is attributed to  $\beta_1$  when estimating (4). Second, treated units are by definition more prevalent in high-treatment groups while untreated units are disproportionally present in low-treatment groups. This unintentionally leads to an additional bias  $(\beta_T \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \beta_C \frac{\text{Var}(\bar{d}_g)}{1 - \bar{d}})$  that is increasing in the variance of  $\bar{d}_g$ . If, for example, all groups have a treatment intensity of  $\bar{d}_g = 0.5$ , then both treated and control group units are trivially part of groups with  $\bar{d}_g = 0.5$ . If, however, half of the groups have a treatment intensity of  $\bar{d}_g = 0.1$  and half of the groups have a treatment intensity of  $\bar{d}_g = 0.9$ , then treated units are predominantly in groups with  $\bar{d}_g = 0.9$  while control group units are predominantly in groups with  $\bar{d}_g = 0.1$ . This induces another bias in  $\tilde{\beta}_1$ . Thus, even with homogenous spillover effects ( $\beta_C = \beta_T$ ), estimating (4) does not lead to an unbiased estimator of the direct treatment effect  $\beta_1$ .

Researchers might be inclined to use group fixed effects in order to focus on within-group variation in treatment status. The following proposition shows that group fixed effects do not necessarily decrease the bias in  $\tilde{\beta}_1$ . To the contrary, it makes the bias even harder to interpret at best, or senseless to understand at worst:

**Proposition 2** *Assume  $y_i$  follows (7) and  $\bar{d}_g \notin \{0, 1\}$  for at least one group  $g$ . Estimating*

(4) with group fixed effects yields:

$$\tilde{\beta}_1 = \beta_1 + (\beta_T - \beta_C) \left[ \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} - \underbrace{\frac{\bar{d}E(\bar{d}_g^3) - (E(\bar{d}_g^2))^2}{\bar{d}(\bar{d} - E(\bar{d}_g^2))}}_{=: \theta} \right] \quad (15)$$

with  $0 \leq \theta \leq \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}}$

Proof: See Appendix B.

Using fixed effects removes any bias in estimating  $\beta_1$  if spillover effects are symmetric ( $\beta_T = \beta_C$ ). This is intuitive because within-group treatment and control group units are subject to the same group level treatment intensity  $\bar{d}_g$ . However, with non-symmetric spillover effects ( $\beta_T \neq \beta_C$ ), the bias is more convoluted and includes the third moment of the distribution of group means. Fixed effects regressions are econometrically equal to regressions on de-meaned variables. Given that the mean of the outcome variable  $y_i$  is a non-linear function of the treatment intensity  $\bar{d}_g$  (see Proposition 3 below), the bias includes the third-order moment of  $\bar{d}_g$ . This is likely to be hard to interpret at best, and senseless to understand at worst. We are not aware of any studies that would provide statistics on higher order distribution of group level mean treatment intensities. However, these moments would be necessary to understand the extent of the bias when using fixed effects estimators in set-ups with spillover effects.

Table 1 summarizes the results from Proposition 1 and Proposition 2, it provides upper/lower bounds for the bias as well as results for specific cases. The proof can be found in Appendix B.

Three key messages stand out: First, positive spillovers to treated units generally increase the estimate of  $\beta_1$ , while positive spillovers to control group units decrease the estimate of  $\beta_1$ . This is intuitive, given that any spillover to treated units increases the difference between treated and control group outcomes while any spillover to control units decreases the difference between treated and control units.

Second, while the bias in the case without fixed effects depends on the variance of the group intensities  $\bar{d}_g$ , the fixed effects estimator depends on higher-order moments of the

distribution of group intensities. The bias of the fixed effects estimator is therefore likely to be hard to interpret at best, and impossible to understand at worst.

Third, Table 1 highlights the trade-off of using fixed effects: While the bias stemming from  $\beta_T$  can potentially be reduced when using group fixed effects, the bias stemming from  $\beta_C$  falls in a wider region compared to the estimation without fixed effects ( $[-1, 0]$  vs.  $[-\bar{d}, 0]$ ). That is, the estimator without fixed effects is less susceptible to include spillovers to control groups in the treatment effect. This highlights the trade-off between endogeneity concerns and concerns over spillovers to control group units affecting the treatment estimate.

The following proposition provides a formula for the group level averages as a function of  $\beta_0, \beta_1, \beta_C$ , and  $\beta_T$  when the true model follows (7):

**Proposition 3** *Group level averages are a non-linear function of treatment intensity even if treatment and spillover effects are both linear:*

$$\bar{y}_g = \beta_0 + (\beta_1 + \beta_C)\bar{d}_g + (\beta_T - \beta_C)\bar{d}_g^2 = \begin{cases} \beta_0 + (\beta_1 + \beta_C)\bar{d}_g & \text{for } \bar{d}_g \approx 0 \\ \beta_0 + (\beta_1 + \beta_T)\bar{d}_g & \text{for } \bar{d}_g \approx 0.5 \\ \beta_0 + (\beta_1 - \beta_C + 2\beta_T)\bar{d}_g & \text{for } \bar{d}_g \approx 1 \end{cases} \quad (16)$$

Proof: See Appendix B.

To gain an intuition for (16), it helps to look at two special cases, one where spillovers only affect control group units and one where spillovers only affect treatment group units:

Spillover to control group units only :

$$\begin{aligned} \bar{y}_g &= \beta_0 + \bar{d}_g\beta_1 + (1 - \bar{d}_g)\beta_C\bar{d}_g \\ \frac{\partial \bar{y}_g}{\partial \bar{d}_g} &= \underbrace{\beta_1}_{[A]} - \underbrace{\beta_C\bar{d}_g}_{[B]} + \underbrace{\beta_C(1 - \bar{d}_g)}_{[C]} \end{aligned}$$

Spillover to treatment group units only :

$$\begin{aligned} \bar{y}_g &= \beta_0 + \bar{d}_g(\beta_1 + \beta_T\bar{d}_g) \\ \frac{\partial \bar{y}_g}{\partial \bar{d}_g} &= \underbrace{\beta_1}_{[A]} + \underbrace{\beta_T\bar{d}_g}_{[B]} + \underbrace{\beta_T\bar{d}_g}_{[C]} \end{aligned}$$

In both cases, [A] captures the direct effect of treatment. [B] captures a composition effect, i.e. the fraction of treatment group units increases as  $\bar{d}_g$  increases. [C] captures a change in the spillover effect for each treatment or control group unit, i.e. the spillover changes for each unit as  $\bar{d}_g$  increases. For spillovers to control group units, [B] and [C] work in opposite directions: If  $\beta_C > 0$ , then an increase in  $\bar{d}_g$  implies that the share of control group units decreases, but the spillover per control group unit increases. For  $\bar{d}_g \approx 0$ , [C] dominates [B], for  $\bar{d}_g \approx 0.5$  both effects cancel each other out, implying that  $\beta_C$  does not play a role, and for  $\bar{d}_g \approx 1$  [B] dominates [C]. A similar argument holds for spillovers to treatment group units, with the differences that [B] and [C] work in the same direction. Thus, while the effect of  $\beta_T$  cannot be observed for  $\bar{d}_g \approx 0$ ,  $\bar{y}_g$  increases one-to-one with  $\beta_T$  for  $\bar{d}_g \approx 0.5$ , and it is equal to  $2\beta_T$  for  $\bar{d}_g \approx 1$ . This suggests that researchers looking at the same data generating process (7) will get strikingly different results on aggregate effects depending on the value of  $\bar{d}_g$  in the data set at hand. The only exception is the case with homogenous spillovers  $\beta_C = \beta_T$  where  $\bar{d}_g = \beta_0 + (\beta_1 + \beta_C)\bar{d}_g$ . In all other cases, (16) not only results in a non-linear relationship, but also a relationship where the impact of  $\beta_T$  and  $\beta_C$  is highly heterogenous across the  $[0, 1]$ -interval and – in the case of  $\beta_C$  – can even flip signs.

### 2.2.5 Example

Assume there are two similar firms, both producing 10 bicycles a year. Firm A's production is hit with a fire, wiping out half of its production capacity. The direct effect of the fire therefore destroys production capacity for 5 bicycles. Without further effects, the production of the treated firm A are  $y(1) = 10 - 5 = 5$  and the production of control group firm B remains at  $y(0) = 10$ . In many economic situations, such as the one described above, spillovers are likely to exist. The drop in supply caused by the fire leaves some demand unsatisfied, providing both firms with an incentive to increase production capacity. Let us assume that spillovers follow the following model:

$$y = 10 - 5d_i + 3\bar{d}d_i + 5\bar{d}(1 - d_i) \quad (17)$$

This simple model assumes that control group firms increase production more as a response to the shock (for example, because the treated firm is financially constrained after the fire). Clearly, responses to the shock could also either go the other way around (for example, because the treated firm has workers sitting idle to ramp up production quickly) or even be symmetric. Production as a response to the number of firms that are treated are depicted in the following table:

Scenario	Firm A	Firm B	Total
No fire	10	10	20
Fire at A, not at B	6.5	12.5	19
Fire at A and B	8	8	16

This simple table already highlights some of our previous results. First, the difference between firm B's and firm A's production when A is hit with a fire but B not ( $12.5 - 6.5 = 6$ ) is larger than the direct effect of the fire ( $\beta_1 = 5$ ). This is because firm B increases production more than firm A as a result of the fire at firm A. Second, the aggregate effect is non-linear in  $\bar{d}_g$ : when treatment intensity increases from 0% to 50%, aggregate production only decreases by 1 unit. However, when treatment intensity increases from 50% to 100%, then aggregate production decreases by 3 units, i.e. the aggregate effect is three times larger compared to a change in the treatment intensity from 0% to 50%.

## 2.3 Summary and guidance for empirical researchers

The gist of the conceptual discussion in this subsection can be summarized in three simple steps:

1. Step 1: Consider the most plausible dimension for spillovers (i.e., the groups  $g$  in the preceding discussion). Candidates can be dimensions such as region or industry, but guidance ultimately has to come from economic theory and from institutional knowledge of the setting at hand.

2. Step 2: Estimate the model

$$y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_T \bar{d}_g d_{ig} + \beta_C \bar{d}_g (1 - d_{ig}) + \epsilon_{ig} \quad (18)$$

and test for the appropriate aggregation level of spillovers (for example, 4-digit versus 3-digit industry or county versus state). Note that (18) should be estimated without group fixed effects (but further individual-level or group-level controls can be added if appropriate).<sup>12</sup>

3. Step 3: Using the coefficients  $\beta_0, \beta_1, \beta_T$  and  $\beta_C$  estimated from (18), plot the outcome variable as a function of the treatment intensity, i.e.  $E[y_{ig}|\bar{d}_g]$ , separately for the treatment units, the control units and the group level averages:

$$E[y_C|d_g] = \beta_0 + \beta_C \bar{d}_g \quad (19)$$

$$E[y_T|d_g] = \beta_0 + \beta_1 + \beta_T \bar{d}_g \quad (20)$$

$$E[\bar{y}_g|d_g] = \beta_0 + (\beta_1 + \beta_C) \bar{d}_g + (\beta_T - \beta_C) \bar{d}_g^2 \quad (21)$$

The resulting figure (see Figure 1 for an example) provides information about all key treatment effects: The difference between the graphs for the treated and control units at  $\bar{d}_g = 0$  provides an estimate of the direct treatment effect  $\beta_1$ . The graph for the group level averages provides the aggregate effect if all groups are treated with a treatment intensity of  $\bar{d}_g$ . The difference between the graph for treated units and the graph for control units provides an estimate for the treatment-minus-control effect at various levels of  $\bar{d}_g$ .

### 3 An empirical application

In this section we apply our framework to the setting in Huber (2018), who analyzes the causal effect of exposure to a bank lending cut on firms and counties. In particular, he examines the effects of the lending cut by a large German bank, Commerzbank, during the financial crisis of 2008-2009 and argues that this event represents an exogenous shock

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<sup>12</sup>Estimating (18) is thus based on the assumption that  $d_{ig}$  and  $\bar{d}_g$  are exogenous.

to its German borrowers. We use this setting as the research question addressed in Huber (2018) is of general economic importance. Further, because of the careful documentation and execution the study lends itself to replication.

Commerzbank decreased lending primarily as result of losses suffered on its trading portfolio. Specifically, trading losses were due to investments in asset-backed securities related to the U.S. subprime mortgage market and its exposure to the insolvencies of Lehman Brothers and large Icelandic banks. Huber (2018) provides evidence suggesting that the losses were unrelated to Commerzbank’s domestic loan portfolio, supporting the conjecture that the lending cut constitutes an exogenous event from the perspective of any given firm. In the presence of credit market frictions, such as switching costs in long-term lending relationships (Sharpe, 1990), lending cuts can negatively affect borrowers and, e.g., result in decreased employment or investment. We refer the reader to Huber (2018) for an in-depth discussion of institutional details, potential endogeneity concerns, and the effect of the lending cut on credit availability for Commerzbank dependent firms.

Given our interest in spillover effects we focus on the regional variation of the employment effects resulting from Commerzbank’s lending cut. Huber (2018) investigates the existence of “indirect effects” at the county level. Specifically, he tests if the negative effect of the lending cut on employment is increasing in the Commerzbank dependence of other firms in the same county, while keeping constant the firms’ direct exposure to Commerzbank. We re-visit this evidence and apply our framework introduced in the preceding sections.

### 3.1 Data

We follow Huber (2018) in the data collection and processing as close as possible. Firm level data is obtained from Bureau van Dijk’s AMADEUS database, which contains financial information on private and publicly owned firms. We restrict the dataset to German firms with non-missing information on the number of employees in the year 2007.<sup>13</sup> We

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<sup>13</sup>Huber (2018) uses 2006 as the base year to define control variables. We base our analysis on a 2018 snapshot of AMADEUS data obtained via WRDS. AMADEUS provides at most 10 recent (fiscal) years of data for the same company (Kalemli-Ozcan, Sorensen, Villegas-Sanchez, Volosovych, and Yesiltas, 2015), i.e., coverage is (reasonably) complete from 2007 onwards. Hence, we use 2007 as base year. Further note that Huber (2018) uses the database Dafne by Bureau van Dijk, which comprises information on

further require information on firms’ date of incorporation (to infer age), county, and industry to be available to construct basic firm level control variables. We follow Huber (2018) and drop firms in the financial and public sectors. Specifically, AMADEUS assigns companies to four-digit NACE codes, following the NACE Rev. 2 classification. We drop financial services and related industries, including holding companies (NACE codes 65-70). We further drop industries that are mainly public sector in Germany. In particular we exclude administrative services, education, healthcare, and arts & culture (NACE codes 81, 82, 84-88, and 90-92). Finally, we drop activities of organizations, private households, and firms that cannot be classified (NACE codes 94 and 97-99). We restrict the sample to firms with available information on their relationship banks, obtained from the AMADEUS BANKERS database.<sup>14</sup>

We follow Huber (2018) and calculate the employment change from 2008 to 2012 (symmetric growth rate) to construct the firm employment cross section. We define a variable  $CB\ dep_{ic}$  as the fraction of firm  $i$ ’s relationship banks that are Commerzbank branches out of the firm’s total number of relationship banks. We define  $\overline{CB\ dep_{ic}}$  for each firm  $i$  as the average Commerzbank dependence of all other firms in the same county ( $c$ ), excluding firm  $i$  itself. The final sample comprises 23,436 firms.

[Table 2 here]

Table 2 shows summary statistics for the final sample. Firms have an average of two relationship banks. Consistent with Huber (2018), the average value of firm Commerzbank dependence is about 0.17. The average number of employees is 177 and the average firm age is about 23 years.<sup>15</sup>

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German firms only. While the overlap between both databases may not be perfect our baseline estimates are very close to those reported in Huber (2018), cf. Section 3.2.1 below.

<sup>14</sup>Huber (2018) fixes bank lending relationships in 2006. We use a 2018 snapshot of the AMADEUS BANKERS database, however, firm-bank relationships are extremely sticky [cf. Giannetti and Ongena (2012) and Kalemli-Ozcan, Laeven, and Moreno (2018) who compare different vintages of the AMADEUS BANKERS database]. While using a 2018 snapshot of firm-bank relationships may not be perfect our baseline estimates are very close to those reported in Huber (2018), cf. Section 3.2.1 below.

<sup>15</sup>Note that the firms in our sample are smaller and younger compared to the values reported in Huber (2018) Table 1. However, these figures are not directly comparable as Huber (2018) uses two different firm level samples in his study: i) a more restrictive “firm panel” that comprises only firms with non-missing data from 2007 to 2012 for several balance sheet and income statement items and ii) a “firm employment cross section” that comprises all firms for which the employment change from 2008 to 2012 can be calculated. Summary statistics in Huber (2018) are only reported for sample i), which is a subset

## 3.2 Indirect effects of Commerzbank’s lending cut

### 3.2.1 Baseline results

We start with a baseline estimation of potential indirect effects on firms in counties with a high Commerzbank dependence, independent of the firms’ individual banking relationships, following Huber (2018). In particular, we estimate the following model:

$$employment\ growth_{ic} = \beta_0 + \beta_1 CB\ dep_{ic} + \beta_2 \overline{CB\ dep_{ic}} + \gamma' X_{ic} + \epsilon_{ic}, \quad (22)$$

where *employment growth* is the firm’s symmetric employment growth rate over the 2008 to 2012 period, defined as:  $2 * (employment_{2012} - employment_{2008}) / (employment_{2012} + employment_{2008})$ .  $CB\ dep_{ic}$  measures the Commerzbank dependence of firm  $i$ , located in county  $c$ .  $\overline{CB\ dep_{ic}}$  is the average Commerzbank dependence of all other firms located in the same county  $c$ , excluding firm  $i$  itself.  $X$  is a set of firm specific controls. In particular, we include indicator variables for 4 firm size bins (1-49, 50-249, 250-999, and over 1,000 employees as of 2007), the log firm age as of 2007, and industry indicators at the 2-digit NACE code level. Standard errors are clustered at the county level. Results are shown in Table 3.

[Table 3 here]

Our results are broadly consistent with Huber (2018), whose baseline estimates we report in column 5 for comparison. The coefficient on  $CB\ dep_{ic}$  ranges between -0.016 and -0.03 [vs. -0.03 in Huber (2018)] and the coefficient on  $\overline{CB\ dep_{ic}}$  ranges between -0.100 and -0.155 [vs. -0.166 in Huber (2018)]. Using the estimates from Table 3 column 4, these results imply that full Commerzbank dependence would reduce employment growth by about 2 percentage points for a firm in a county where no other firm had Commerzbank among their relationship banks (the “direct effect”). This effect is amplified by Commerzbank dependence of other firms in the region: a one standard deviation greater Commerzbank dependence of other firms (6%, cf. Table 2) would reduce employment growth by about 1.2 percentage points. This effect is likely biased towards larger firms. Given that the focus of our paper is on spillover effects we aim at reconstructing sample ii) used in Huber (2018) to examine indirect effects at the county level.

growth by  $6\% \times 0.155 \approx 1$  percentage point more. Given the implicit assumption of symmetric spillover effects for treated and control group firms, -1 percentage point is also the indirect effect on firms not dependent on Commerzbank.

### 3.2.2 Full spillover model

Next, we amend the model and allow for asymmetric spillover effects for treated and control group firms, i.e., we estimate the flexible spillover model introduced in Section 2, cf. eq. (7):

$$\begin{aligned} \text{employment growth}_{ic} = & \beta_0 + \beta_1 \text{CB dep}_{ic} + \beta_T \text{CB dep}_{ic} \times \overline{\text{CB dep}_{ic}} \\ & + \beta_C (1 - \text{CB dep}_{ic}) \times \overline{\text{CB dep}_{ic}} + \gamma' X_{ic} + \epsilon_{ic}. \end{aligned} \quad (23)$$

Note that Huber (2018) interprets  $\beta_1$  in eq. (22) as the direct effect of Commerzbank’s lending cut, i.e., the effect of the lending cut on treated firms in absence of spillovers. Using eq. (23), this interpretation implies that either no spillovers exist at all, i.e.,  $\beta_T = \beta_C = 0$  (which we can reject given the evidence reported in Table 3), or that spillover effects are completely symmetric for treated and control group firms, i.e.,  $\beta_T = \beta_C \neq 0$  (in which case the spillover effects would be fully captured by  $\beta_2$  in eq. (22)). Else,  $\beta_1$  is biased and will reflect both the direct effect and spillover effects, cf. Proposition 1. The results from estimating eq. (23) are shown in Table 4.

[Table 4 here]

The results uncover that the negative spillover effects are purely driven by the control group firms.  $\beta_C$  is negative and highly statistically significant, while  $\beta_T$  is close to zero and not statistically significant (Table 4, column 2). The coefficient  $\beta_C$  indicates that a one standard deviation greater Commerzbank dependence of other firms would result in an employment growth reduction by about  $6\% \times 0.184 \approx 1.1$  percentage points for firms with no relationship to Commerzbank. The effect for a firm fully dependent on Commerzbank is -5.5 percentage points *irrespective of the Commerzbank dependence of other firms in the same county*. Note that this “direct effect” is more than twice as large compared to the estimate that does not account for potentially asymmetric spillover effects for treated

and control group firms (cf. Table 4, column 1). Knowing that  $\beta_C < 0$  the fact that estimating eq. (22) results in an underestimation of  $\beta_1$  follows directly from Proposition 1.

[Figure 2 here]

Figure 2 depicts the county level spillover effects using equations (19) - (21) and the estimates from Table 4, column 2. The figure illustrates several points. First, one can easily read off the direct effect from the figure, i.e., the implied employment growth at a treatment fraction of zero (here: -5.5 percentage points). Second, the difference between the employment growth for treatment and control units diminishes quickly with increasing county level treatment fraction as result of the asymmetric spillover effects. This visualizes why not accounting for asymmetric spillover effects leads to an underestimation of the direct effect. Third, the employment decline is similar for treatment and control units in counties with a treatment fraction of  $\sim 0.3$ . In counties with a higher treatment fraction the implied effect for firms without Commerzbank dependence would even be below the effect for firms dependent on Commerzbank. This result, however, should be treated with caution given that the 95th percentile of the county Commerzbank dependence is 0.26 (cf. Table 2), i.e., there are very few observations above this value and the standard errors are large. Fourth, there is a non-linear relationship between the average employment growth in a county and the average Commerzbank dependence, as implied by Proposition 3. We discuss this point in more detail in Section 3.2.4 below.

Overall, this discussion highlights that ignoring spillover effects in the estimation or assuming that spillover effects are symmetric for treated and control group firms can lead to a biased (i.e., over- or under-) estimation of the direct treatment effect, as shown formally in Proposition 1. In this specific setting our results suggest that the direct effect of Commerzbank's lending cut on affected firms reported by Huber (2018) likely underestimates the true effect. This is because spillovers are asymmetric: while control groups firms are indirectly affected, treatment firms do not exhibit a differential employment growth decline in regions with more/less other Commerzbank dependent firms.

### 3.2.3 Spillover level

A natural question that arises is at which level spillovers should be measured. In our conceptual discussion we assume that group affiliation is known and that no spillovers exist across groups. Clearly, when taking the framework to the data the researcher has to take a stance on the level at which spillovers occur. Huber (2018), for instance, measures spillovers at the county level. However, it is a priori not clear whether this better captures the underlying market dynamics than other classifications at, for instance, the federal state or zip code level. Note that if spillovers are not confined to the county level also the estimates from eq. (23) are biased.

Generally, this issue can be approached in different, non-mutually exclusive, ways. (i) Economic theory can guide the empirical design. For instance, if spillovers arise as result of indirect effects operating through local demand, e.g., direct employment effects may lead to a contraction in households' consumption, then it may be plausible that spillovers are confined in geographic regions. Alternatively, if spillovers are the result of product market interactions among firms, e.g., as in the Cournot model with spillovers discussed in Appendix A of this paper, then potential spillovers at the industry level should be taken into account. (ii) Setting-specific economic arguments can be used to define boundaries between groups. For instance, the researcher may plausibly argue (and provide evidence) that trade restrictions or language barriers make spillovers unlikely across certain regions. (iii) The researcher can explicitly test for potential spillovers using definitions at different levels and compare results.

In this section we take a closer look at (iii) and exemplarily test for the existence of spillovers beyond the county level in the Huber (2018) setup. In particular, we amend eq. (23) and *additionally* include variables that capture Commerzbank dependence at the federal state level. We define a variable,  $\overline{\text{CB } dep_{cb}}$ , to capture the Commerzbank dependence of all other counties in the same federal state ("Bundesland"  $b$ ), excluding county  $c$  itself.<sup>16</sup> Again, we interact this variable with firms' treatment status, i.e., Commerzbank dependence, and allow for differential effects for treated and control group firms. The

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<sup>16</sup>We first take the county level average across all firms in the county ( $\overline{\text{CB } dep_c}$ ).  $\overline{\text{CB } dep_{cb}}$  is then the unweighted average of  $\overline{\text{CB } dep_c}$  across all counties in the same federal state, excluding county  $c$  itself.

results are reported in Table 5.

[Table 5 here]

The results suggest that spillovers are potentially not confined within county borders. Controlling for county level spillovers, the average Commerzbank dependence of firms in other counties in the same federal state negatively affects control group firms. In particular, the results reported in column 4 suggest that a one standard deviation greater federal state level Commerzbank dependence (0.032) corresponds to a  $3.2\% \times 0.218 \approx 0.7$  percentage points lower employment growth for firms without Commerzbank dependence. Again, we find no indications for spillover effects on the treatment group.

This example highlights that an estimation yields biased results if spillover effects are not confined within the defined clusters. This holds both for the estimated direct effect and the estimated indirect effects. For instance, column 4 would suggest that the direct effect of Commerzbank’s lending cut is -11 percentage points. This estimate is significantly above the effect estimated solely taking county level spillover effects into account (-6.2 percentage points,<sup>17</sup> cf. column 2).

More generally, the approach suggested in this section can enable the researcher to get a better understanding of the underlying market dynamics in her setting. That is, one can start with a narrow group definition, e.g., small regions or a granular industry classification, and then gradually include higher level spillover effects until the next level no longer helps explaining the variation in the data.

### 3.2.4 Aggregated regressions

As discussed in Section 2.1, the current literature often follows a two-step procedure when dealing with potential spillover effects. First, regressions on a disaggregated level are reported as estimates of the treatment effect. Then data are aggregated to test for reallocation or spillover effects. Berton, Mocetti, Presbitero, and Richiardi (2018), for

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<sup>17</sup>Note that the estimated direct effect in Table 4, column 2 is -5.5 percentage points. We re-estimate this specification using the same sample that is used when testing for federal state level spillovers to make the coefficients comparable across specifications. The number of observations is slightly lower as  $CB\ dep_{cb}$  is per definition not defined for “city-states”, i.e., federal states that comprise only one county (Berlin, Hamburg, and Bremen).

instance, also analyze the effect of credit supply shocks on employment using data on Italian firms. Having established an effect at the firm level, the authors aggregate data on the province-industry-quarter level to rule out that the firm level estimates merely reflect an employment reallocation across firms.

In this section we perform and discuss a similar approach, i.e., we analyze the effect of Commerzbank dependence on average employment growth at the county level. As per Proposition 3, however, we explicitly take potential non-linear effects into account. In particular, we estimate the following model:

$$\overline{employment\ growth}_c = \lambda_0 + \lambda_1 \overline{CB\ dep}_c + \lambda_2 \overline{CB\ dep}_c^2 + \gamma' \overline{X}_c + \epsilon_c, \quad (24)$$

where the dependent variable is the average symmetric growth rate of firm employment from 2008 to 2012 across all firms located in county  $c$ .  $\overline{CB\ dep}_c$  is the average Commerzbank dependence across all firms in county  $c$ .  $\overline{X}_c$  is a set of firm controls based on county level averages. The results are reported in Table 6.

[Table 6 here]

The results indicate that the average county level Commerzbank dependence is negatively correlated with employment growth. The baseline estimates from column 1, i.e., without accounting for potential non-linear effects, would indicate that a one standard deviation higher county level Commerzbank dependence (0.066) is associated with a  $6.6\% \times 0.137 \approx 0.9$  percentage points lower county level employment growth.

More importantly, column 2 confirms that the aggregated effects are non-linear. The functional form is consistent with the predictions from Proposition 3. Given that  $\beta_1 < 0$  and  $\beta_C < 0$ , cf. Table 6, from Proposition 3  $\Rightarrow \lambda_1 < 0$ . Further,  $\beta_T \approx 0$  and  $\beta_C < 0 \Rightarrow \lambda_2 > 0$ . This highlights again that even if treatment and spillover effects are both linear, group level averages are a *non-linear* function of treatment intensity.

Finally, it should be noted that group level aggregation does not solve problems arising as result of an incorrect group definition, cf. Section 3.2.3. For instance, to the extent that spillovers are not confined within counties, also the estimates from eq. (24) are biased if these additional spillover effects are not properly accounted for.

## 4 Conclusion

Spillover effects are ubiquitous in many economic settings. Yes, despite their importance, the discussion of spillover effects in empirical research misses the rigor dedicated to endogeneity concerns. In this paper, we have provided a conceptual discussion of spillover effects when spillovers can occur within groups (such as industries or regions), but not across groups. We further provide a guide for empirical researchers, and we apply our guide to spillover effects of credit supply shocks.

Conceptually, we highlight three key results. First, even with random treatment, spillovers lead to an intricate bias in estimating treatment effects. The bias is convoluted and depends on second or, in the case of fixed effects regressions, third-order moments of group-level treatment intensities. The bias is likely to be hard to understand at best, and can lead to differences in estimated treatment effects across different studies even if the underlying data generating process is the same. Simple rules (such as “divide the treatment effect by two” in case that control group units benefit from a negative shock to treated units) are insufficient to describe the resulting bias. Second, we document that there is a trade-off between endogeneity and spillover concerns. For example, including fixed effects, a common approach to strengthen identification in the presence of endogeneity concerns, can exacerbate the bias arising from spillovers. Third, the current practice of using individual level regressions to identify direct effects and aggregate level regressions to learn about spillover effects can lead to misleading conclusions. It also makes inefficient use of the data at hand as direct effects, spillover effects, and aggregate effects can be better obtained from estimating a simple spillover model directly.

We develop a simple guidance for empirical researchers, apply it to a credit supply shock, and highlight differences in the results compared to current empirical practice. For example, we demonstrate in one empirical setting that direct effects of a credit supply shock are underestimated by a factor of 2-3 using current practice. We hope that this guidance will be useful to academics in future research.

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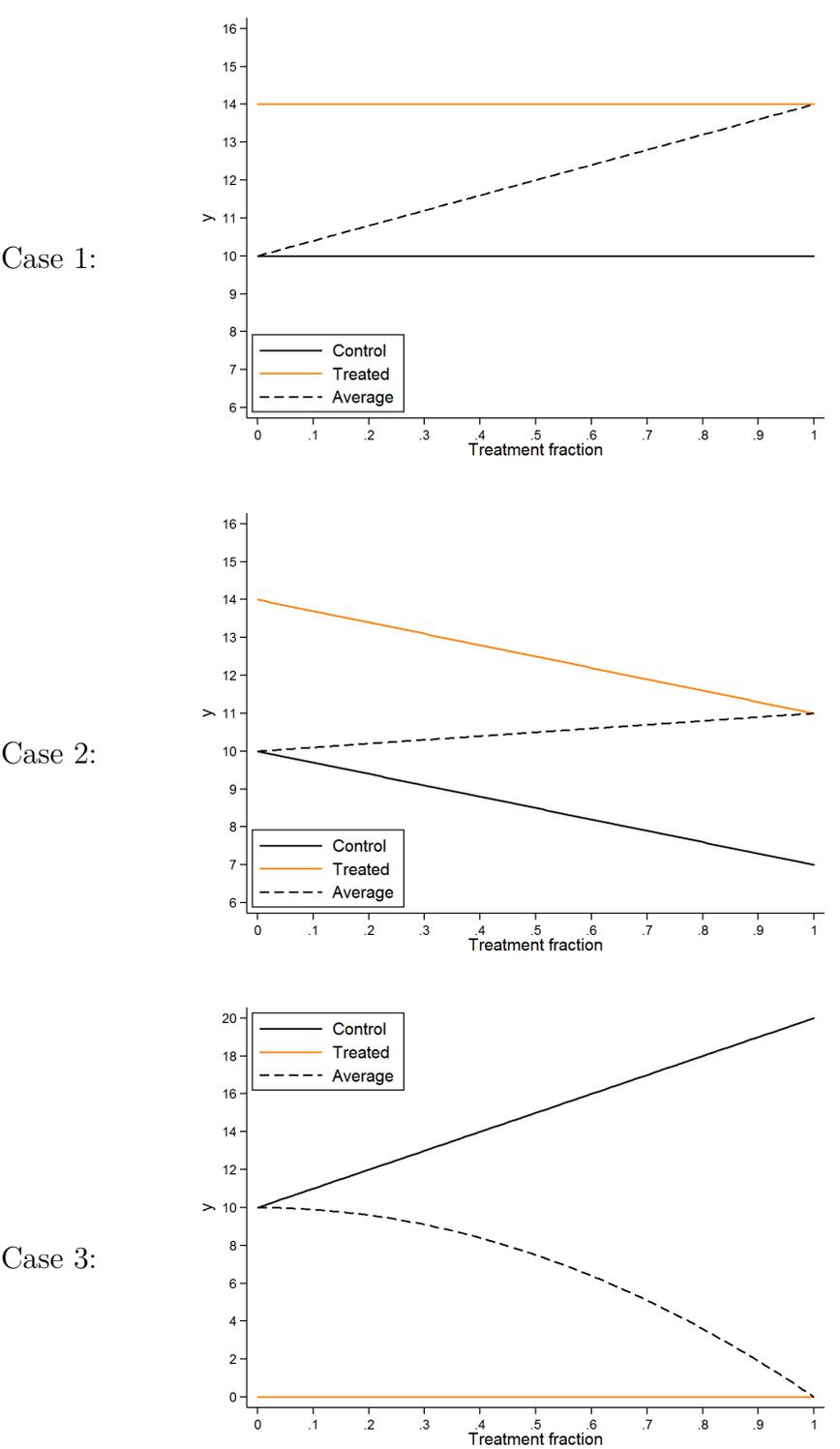
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# Figures

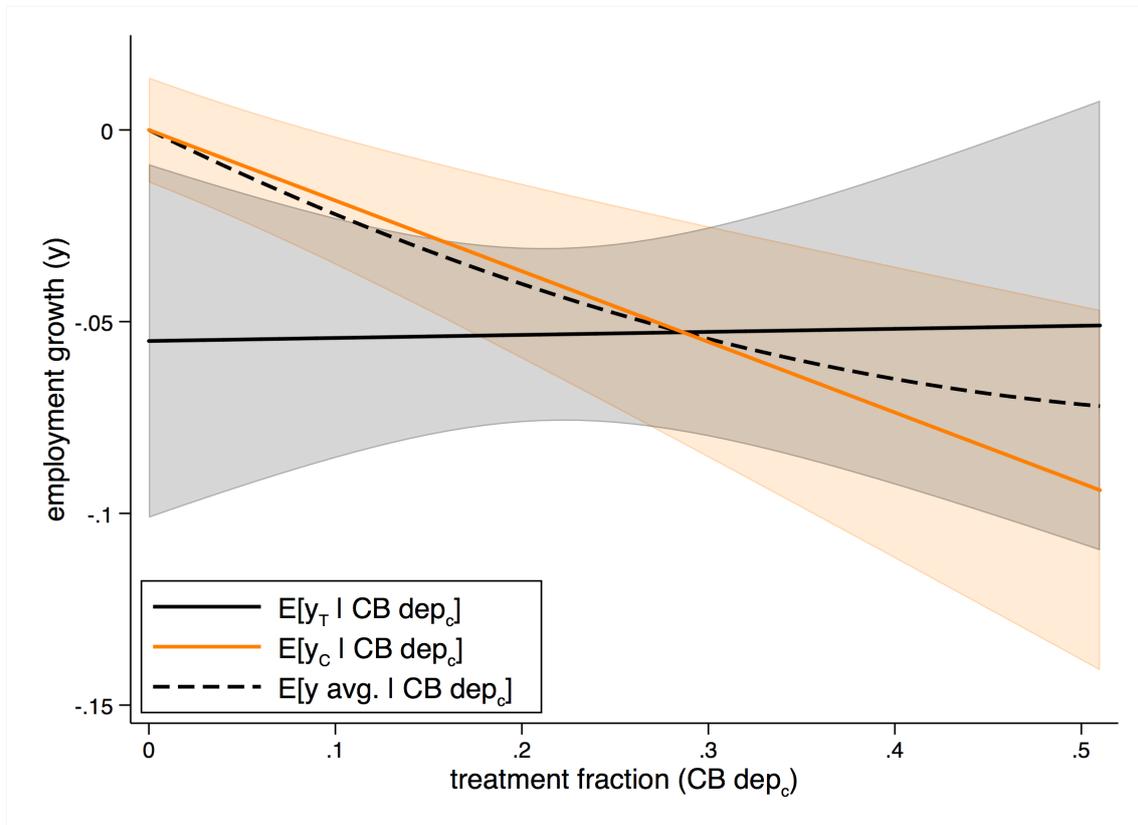
**Figure 1:** Illustration of three spillover scenarios

This figure illustrates three spillover scenarios. Case 1 provides a scenario without spillovers ( $\beta_0 = 10, \beta_1 = 4, \beta_T = \beta_C = 0$ ). Case 2 provides a scenario with homogenous spillovers ( $\beta_0 = 10, \beta_1 = 4, \beta_T = \beta_C = -3$ ). Case 3 provides a scenario with spillovers to control group firms only ( $\beta_0 = 10, \beta_1 = -10, \beta_T = 0, \beta_C = 10$ ).



**Figure 2:** Commerzbank's lending cut and spillover effects at the county level

This figure illustrates the county level spillover effects of Commerzbank's lending cut on firms with and without Commerzbank dependence. In particular, the figure plots employment growth from 2008 to 2012 as a function of the average Commerzbank dependence of a county using equations (19) - (20) and the estimated coefficients from Table 4, column 2. Further shown are 90% confidence intervals and the county level average employment growth ( $y_{avg}$ ; cf. equation (21)).



# Tables

**Table 1:** Bias when ignoring spillovers for estimating treatment effects

This table provides the bias arising from estimating a model without spillovers (equation 4) on data generated with spillovers (7). The cases  $\bar{d}_g \in \{0, k\}$  and  $\bar{d}_g \in \{k, 1\}$  assume that there are "many" groups with  $\bar{d}_g = 0$  and  $\bar{d}_g = 1$ , respectively. See the proof in Appendix B for a precise formulation.

Case	Proposition 1 (no fixed effects)	Proposition 2 (fixed effects)
General	$(\beta_T - \beta_C)\bar{d} + \beta_T \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \beta_C \frac{\text{Var}(\bar{d}_g)}{1-\bar{d}}$	$(\beta_T - \beta_C) \left[ \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} - \frac{\bar{d}E(\bar{d}_g^3) - (E(\bar{d}_g^2))^2}{\bar{d}(\bar{d} - E(\bar{d}_g^2))} \right]$
Bounds	$\beta_T [\bar{d}, 1] + \beta_C [-\bar{d}, 0]$	$\beta_T [0, 1] + \beta_C [-1, 0]$
33 <i>Case 1:</i> $\bar{d}_g \in \{\bar{d}\}$	$\beta_T \bar{d} - \beta_C \bar{d}$	$\beta_T \bar{d} - \beta_C \bar{d}$
<i>Case 2:</i> $\bar{d}_g \in \{0, k\}$	$\beta_T k$	$\beta_T k - \beta_C k$
<i>Case 3:</i> $\bar{d}_g \in \{k, 1\}$	$\beta_T - \beta_C c$	$\beta_T k - \beta_C k$
<i>Case 4:</i> $\bar{d}_g \in \{0, 1\}$	$\beta_T$	n.a.

**Table 2: Summary statistics**

This table shows summary statistics for the firm employment cross section.

	Mean	SD	p5	p50	p95	N
$CB\ dep_{ic}$	0.17	0.29	0.00	0.00	1.00	23,436
$\overline{CB\ dep_{ic}}$	0.17	0.06	0.07	0.17	0.26	23,436
Number of relationship banks $_{ic}$	2.02	1.15	1.00	2.00	4.00	23,436
Employment (fiscal year 2007) $_{ic}$	176.78	2,645.54	2.00	49.00	455.00	23,436
Age (fiscal year 2007) $_{ic}$	22.67	21.31	4.00	17.00	62.00	23,436

**Table 3: Baseline results**

The unit of observation is the firm level  $i$ . The dependent variable is the symmetric growth rate of firm employment from 2008 to 2012.  $CB\ dep_{ic}$  is the fraction of the firm's relationship banks that are Commerzbank branches.  $\overline{CB\ dep_{ic}}$  is the average Commerzbank dependence of all other firms in the same county ( $c$ ), excluding firm  $i$  itself. The following control variables are included when indicated: indicator variables for 4 firm size bins (1-49, 50-249, 250-999, and over 1,000 employees as of 2007), the ln of firm age (as of 2007), and industry fixed effects (2-digit NACE codes). Robust standard errors, clustered at the county level, are in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively. Column 5 shows the estimates from Huber (2018) Table 10, column 1 for comparison.

	(1)	(2)	(3)	(4)	Huber (2018) estimates (5)
$CB\ dep_{ic}$	-0.033*** (0.008)	-0.030*** (0.009)	-0.016* (0.009)	-0.019** (0.009)	-0.030*** (0.009)
$\overline{CB\ dep_{ic}}$		-0.100** (0.048)	-0.143*** (0.045)	-0.155*** (0.044)	-0.166** (0.076)
Industry fixed effects	No	No	Yes	Yes	Yes
Size bin fixed effects	No	No	Yes	Yes	Yes
ln age	No	No	No	Yes	Yes
Observations	23,436	23,436	23,436	23,436	48,101

**Table 4: Full spillover model**

The unit of observation is the firm level  $i$ . The dependent variable is the symmetric growth rate of firm employment from 2008 to 2012.  $CB\ dep_{ic}$  is the fraction of the firm's relationship banks that are Commerzbank branches.  $\overline{CB\ dep_{ic}}$  is the average Commerzbank dependence of all other firms in the same county ( $c$ ), excluding firm  $i$  itself. The following control variables are included when indicated: indicator variables for 4 firm size bins (1-49, 50-249, 250-999, and over 1,000 employees as of 2007), the ln of firm age (as of 2007), and industry fixed effects (2-digit NACE codes). Robust standard errors, clustered at the county level, are in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)
$CB\ dep_{ic}$	-0.019** (0.009)	-0.055** (0.027)
$\overline{CB\ dep_{ic}}$	-0.155*** (0.044)	
$CB\ dep_{ic} \times \overline{CB\ dep_{ic}}$		0.008 (0.112)
$(1 - CB\ dep_{ic}) \times \overline{CB\ dep_{ic}}$		-0.184*** (0.052)
Industry fixed effects	Yes	Yes
Size bin fixed effects	Yes	Yes
ln age	Yes	Yes
Observations	23,436	23,436

**Table 5: Spillover level**

The unit of observation is the firm level  $i$ . The dependent variable is the symmetric growth rate of firm employment from 2008 to 2012.  $CB\ dep_{ic}$  is the fraction of the firm's relationship banks that are Commerzbank branches.  $\overline{CB\ dep_{ic}}$  is the average Commerzbank dependence of all other firms in the same county ( $c$ ), excluding firm  $i$  itself.  $\overline{CB\ dep_{cb}}$  is the average Commerzbank dependence of all other counties in the same federal state ( $b$ ), excluding county  $c$  itself. The following control variables are included when indicated: indicator variables for 4 firm size bins (1-49, 50-249, 250-999, and over 1,000 employees as of 2007), the ln of firm age (as of 2007), and industry fixed effects (2-digit NACE codes). Robust standard errors, clustered at the county level, are in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
$CB\ dep_{ic}$	-0.055** (0.027)	-0.062** (0.027)	-0.104** (0.043)	-0.109** (0.045)
<i>County level spillovers:</i>				
$CB\ dep_{ic} \times \overline{CB\ dep_{ic}}$	0.008 (0.112)	0.044 (0.114)		0.004 (0.120)
$(1 - CB\ dep_{ic}) \times \overline{CB\ dep_{ic}}$	-0.184*** (0.052)	-0.202*** (0.053)		-0.154** (0.061)
<i>Federal state level spillovers:</i>				
$CB\ dep_{ic} \times \overline{CB\ dep_{cb}}$			0.196 (0.225)	0.190 (0.237)
$(1 - CB\ dep_{ic}) \times \overline{CB\ dep_{cb}}$			-0.346*** (0.105)	-0.218* (0.122)
Industry fixed effects	Yes	Yes	Yes	Yes
Size bin fixed effects	Yes	Yes	Yes	Yes
ln age	Yes	Yes	Yes	Yes
Observations	23,436	21,870	21,870	21,870

**Table 6: Aggregated regressions**

The unit of observation is the county level  $c$ . The dependent variable is the average symmetric growth rate of firm employment from 2008 to 2012 across all firms located in county  $c$ .  $\overline{CB\ dep}_c$  is the average Commerzbank dependence across all firms in county  $c$ . The following control variables are included when indicated: indicator variables for 4 firm size bins (1-49, 50-249, 250-999, and over 1,000 employees as of 2007) and the ln of firm age (as of 2007). Employees and age are average values across all firms in the county. Standard errors are in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)
$\overline{CB\ dep}_c$	-0.137** (0.056)	-0.606*** (0.173)
$\overline{CB\ dep}_c^2$		1.457*** (0.508)
Size bin fixed effects	Yes	Yes
ln age	Yes	Yes
Observations	400	400

## A Spillovers in a model with Cournot competition

Consider a simple model of Cournot competition with  $n$  firms and a linear inverse demand function of  $p = 1 - \sum_{i=1}^n y_i = 1 - Y$ , where  $p$  denotes the price and  $y_i$  the quantity produced by firm  $i$ .  $Y$  is the aggregate quantity produced across all firms. Following the notation from the prior section,  $d_i$  denotes the treatment indicator, implying that a proportion  $\bar{d} \equiv \frac{1}{n} \sum_{i=1}^n d_i$  of firms is treated, while control group firms present a proportion  $(1 - \bar{d})$  of all firms. Control group firms have costs  $c^C = c$ , while treatment group firms have costs of  $c^T = c - \gamma$ , for instance, as result of a positive credit supply shock. The resulting profit function  $\pi_i$  and first-order conditions are:

$$\pi_i = y_i(p - c_i) = y_i \left[ 1 - \underbrace{\left( \sum_{j \neq i} y_j + y_i \right)}_Y - c_i \right] \quad (25)$$

$$\frac{\partial \pi_i}{\partial y_i} = 1 - Y - c_i - y_i = 0 \quad (26)$$

Deducting the first-order conditions for control and treatment group firms provides the relationship between treatment and control group quantities:

$$y_i^T - y_i^C = -(c_i^T - c_i^C) = \gamma \Leftrightarrow y_i^T = y_i^C + \gamma \quad (27)$$

Plugging (27) into the first-order condition for control group firms yields the control group quantities

$$1 - n(1 - \bar{d})y_i^C - n\bar{d}(y_i^C + \gamma) - c - y_i^C = 0 \Leftrightarrow y_i^C = \frac{1 - c - \gamma n \bar{d}}{n + 1} \quad (28)$$

Combining (28) and (27) yields a simple linear model with spillovers:

$$y_i = \frac{1 - c}{n + 1} + \gamma d_i - \gamma \frac{n}{n + 1} \bar{d} \quad (29)$$

If some firms benefit from a cost advantage of  $\gamma$  – for example, due to lower funding costs, adaption of a better technology, or regulatory relief – these firms have an output that is  $\gamma$  higher compared to the output of control group firms, see the term  $+\gamma d_i$  in (29). However, some of this increase comes at the expense of other firms as represented by the spillover effect  $\frac{n}{n+1} \bar{d}$  in (29). Thus, even this simple Cournot example clearly contains spillover effects, thereby violating the no interference between units assumption.

Note that the example above illustrate one potential spillover, where control group firms are negatively affected by a cost reduction at treatment group firms. Spillovers can, however, come in many ways: for example, a firm might be positively affected by increases in credit availability of firms along the supply chain; a firm might increase its production

after a cost reduction at a firm producing complementary goods (such as car producers and gasoline stations); or spillovers at control and treatment group firms can differ in the presence of network effects.

## B Proof of Proposition 1-3 and the cases in Table 1

**Proof of Proposition 1:** Using  $y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_2 \bar{d}_g + \beta_3 \bar{d}_g d_{ig} + \epsilon_{ig}$  with  $\beta_2 = \beta_C, \beta_3 = \beta_T - \beta_C$  and the standard omitted-variable bias formula yields:

$$\begin{aligned}
\tilde{\beta}_1 &= \beta_1 + \beta_2 \frac{Cov(d_{ig}, \bar{d}_g)}{Var(d_{ig})} + \beta_3 \frac{Cov(d_{ig}, d_{ig} \bar{d}_g)}{Var(d_{ig})} \\
&= \beta_1 + \beta_2 \frac{E[d_{ig} \bar{d}_g] - \bar{d}^2}{\bar{d}(1 - \bar{d})} + \beta_3 \frac{E[d_{ig} \bar{d}_g] (1 - \bar{d})}{\bar{d}(1 - \bar{d})} \\
&= \beta_1 + \beta_2 \frac{Var[\bar{d}_g] + \bar{d}^2 - \bar{d}^2}{\bar{d}(1 - \bar{d})} + \beta_3 \frac{Var(\bar{d}_g) + \bar{d}^2}{\bar{d}} \\
&= \beta_1 + \beta_2 \frac{Var(\bar{d}_g)}{\bar{d}(1 - \bar{d})} + \beta_3 \left( \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} \right)
\end{aligned} \tag{30}$$

Plugging in  $\beta_2 = \beta_C, \beta_3 = \beta_T - \beta_C$  yields

$$\begin{aligned}
\tilde{\beta}_1 &= \beta_1 + \beta_C \frac{Var(\bar{d}_g)}{\bar{d}(1 - \bar{d})} + (\beta_T - \beta_C) \left( \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} \right) \\
&= \beta_1 + (\beta_T - \beta_C) \bar{d} + \beta_T \frac{Var(\bar{d}_g)}{\bar{d}} + \beta_C \frac{Var(\bar{d}_g)}{1 - \bar{d}}
\end{aligned} \tag{31}$$

**Proof of Proposition 2:** The proof proceeds in two steps:

Step 1: We show the equivalence of

$$\tilde{\beta}_1 = \beta_1 + (\beta_T - \beta_C) \frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{Var(d_{ig}) - Var(\bar{d}_g)} \tag{32}$$

$$= \beta_1 + (\beta_T - \beta_C) \cdot \left[ \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} - \underbrace{\frac{\bar{d}E(\bar{d}_g^3) - (E(\bar{d}_g^2))^2}{\bar{d}(\bar{d} - E(\bar{d}_g^2))}}_{=: \theta} \right] \tag{33}$$

To see (32), note that a regression with group fixed effects  $y_i = \tilde{\beta}_1 d_{ig} + \gamma_g + \epsilon_i$  is equivalent to the de-measured regression  $y_{ig} - \bar{y}_g = \tilde{\beta}_1 (d_{ig} - \bar{d}_g) + \epsilon_i$ . For the following steps, it helps to recognize that  $E(d_{ig} - \bar{d}_g) = 0$ ,  $E(d_{ig} \bar{d}_g) = E(\bar{d}_g^2)$ ,  $E(d_{ig} \bar{d}_g^2) = E(\bar{d}_g^3)$ , and  $Var(d_{ig} - \bar{d}_g) = Var(d_{ig}) - Var(\bar{d}_g)$ . Using  $y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_2 \bar{d}_g + \beta_3 \bar{d}_g d_{ig} + \epsilon_{ig}$  with  $\beta_2 = \beta_C, \beta_3 = \beta_T - \beta_C$  and the standard omitted-variable bias formula yields

$$\begin{aligned}
\tilde{\beta}_1 &= \frac{\text{Cov}(y_{ig} - \bar{y}_g, d_{ig} - \bar{d}_g)}{\text{Var}(d_{ig} - \bar{d}_g)} \\
&= \frac{\text{Cov}(\beta_1(d_{ig} - \bar{d}_g) + \beta_3(d_{ig}\bar{d}_g - \bar{d}_g^2), d_{ig} - \bar{d}_g)}{\text{Var}(d_{ig} - \bar{d}_g)} \\
&= \beta_1 + \beta_3 \frac{E(d_{ig}\bar{d}_g - 2d_{ig}\bar{d}_g^2 + \bar{d}_g^3)}{\text{Var}(d_{ig} - \bar{d}_g)} \\
&= \beta_1 + \beta_3 \frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{\text{Var}(d_{ig}) - \text{Var}(\bar{d}_g)} \tag{34}
\end{aligned}$$

Plugging in  $\beta_3 = \beta_T - \beta_C$  yields (32).

To see (32), note that  $\text{Var}(d_{ig}) - \text{Var}(\bar{d}_g) = \bar{d} - E(\bar{d}_g^2)$ , implying that (32) can also be written as

$$\tilde{\beta}_1 = \beta_1 + \beta_3 \cdot \frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{\bar{d} - E(\bar{d}_g^2)} \tag{35}$$

Adding a zero ( $0 = \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} - \bar{d} - \frac{\text{Var}(\bar{d}_g)}{\bar{d}}$ ) yields:

$$\begin{aligned}
\tilde{\beta}_1 &= \beta_1 + \beta_3 \cdot \left[ \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \frac{\left(-\bar{d} - \frac{\text{Var}(\bar{d}_g)}{\bar{d}}\right) d(d - E(\bar{d}_g^2)) + E(\bar{d}_g^2) - E(\bar{d}_g^3)}{\bar{d}(\bar{d} - E(\bar{d}_g^2))} \right] \\
&= \beta_1 + \beta_3 \cdot \left[ \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \frac{(E(\bar{d}_g^2))^2 - \bar{d}E(\bar{d}_g^3)}{\bar{d}(\bar{d} - E(\bar{d}_g^2))} \right] \\
&= \beta_1 + \beta_3 \left[ \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \theta \right], \text{ with } \theta < 0 \tag{36}
\end{aligned}$$

Plugging in  $\beta_3 = \beta_T - \beta_C$  yields (33).

Step 2:  $0 \leq \theta \leq \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}}$

$\theta \leq \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}}$  follows directly from (32) and  $\frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{\text{Var}(d_{ig}) - \text{Var}(\bar{d}_g)} > 0$ . To see that  $\theta \geq 0$ , first note that the denominator of  $\theta$  is larger than zero. It remains to be shown that the nominator is larger than zero. To see this, using  $n$  as the total number of units, write

$$\begin{aligned}
\bar{d}E(\bar{d}_g^3) - (E(\bar{d}_g^2))^2 &= \frac{1}{n^2} \left( \sum_i \bar{d}_{ig} \sum_i \bar{d}_{ig}^3 - \sum_i [\bar{d}_{ig}^2]^2 \right) \\
&= \frac{1}{n^2} \sum_{i,j} \bar{d}_{ig} \bar{d}_{jg}^3 - \bar{d}_{ig}^2 \bar{d}_{jg}^2 \\
&= \frac{1}{n^2} \sum_i \sum_{j>i} \bar{d}_{ig} \bar{d}_{jg} (\bar{d}_{ig} - \bar{d}_{jg}^2) > 0
\end{aligned}$$

**Proof of lower/upper bounds for Proposition 1:** Lower/upper bounds for Proposition 1: To get lower/upper bounds for (14), note that  $Var(\bar{d}_g) \in [0, \bar{d}(1 - \bar{d})]$  which directly yields the lower/upper bounds given in Table 1.

**Proof of lower/upper bounds for Proposition 2:** We need to show that  $\bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} + \theta \in [0, 1]$ .  $\bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} + \theta \geq 0$  follows directly from 32.  $\bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} + \theta \leq 1$  follows from 33 and the fact that  $Var(\bar{d}_g) \leq \bar{d}(1 - \bar{d})$ . The fact that the minimum and maximum values can be obtained follows from the special cases in Table 1.

**Proof of the special cases for Proposition 1:** Cases 1 and 4 in Table 1 follow directly from (14) and the fact that  $Var(\bar{d}_g) = 0$  in case 1 and  $Var(\bar{d}_g) = \bar{d}(1 - \bar{d})$  in case 4.

Cases 2 and 3 are slightly more involved, while  $Var(\bar{d}_g) \rightarrow 0$  in both cases, one of the denominators in (14) also converges towards zero. To see case 2 ( $\bar{d}_g \in \{0, c\}$ ), note that  $\frac{Var(\bar{d}_g)}{1 - \bar{d}} \rightarrow 0$  because the nominator converges towards 0 and the denominator converges towards 1. Defining  $p_c := \frac{N_c}{N_0}$  yields:

$$Var(\bar{d}_g) = E(\bar{d}_g^2) - E(\bar{d}_g)^2 = p_c c^2 - p_c^2 c^2 = p_c c^2 (1 - p_c)$$

This implies that

$$\frac{Var(\bar{d}_g)}{\bar{d}} = \frac{p_c c^2 (1 - p_c)}{p_c c} = c(1 - p_c) \rightarrow c$$

Plugging  $\frac{Var(\bar{d}_g)}{\bar{d}} = c$  and  $\frac{Var(\bar{d}_g)}{1 - \bar{d}} = 0$  in (14) yields case 2.

To see case 3 ( $\bar{d}_g \in \{c, 1\}$ ), note that  $\frac{Var(\bar{d}_g)}{\bar{d}} \rightarrow 0$  because the nominator converges towards 0 and the denominator converges towards 1. Defining  $p_c := \frac{N_c}{N_0}$  yields:

$$Var(\bar{d}_g) = E(\bar{d}_g^2) - E(\bar{d}_g)^2 = (p_c c^2 + (1 - p_c)1^2) - (1 - p_c(1 - c))^2 = pc(1 - c)^2(1 - p_c)$$

This implies that

$$\frac{Var(\bar{d}_g)}{1 - \bar{d}} = \frac{pc(1 - c)^2(1 - p_c)}{pc(1 - c)} = (1 - p_c)(1 - c) \rightarrow 1 - c$$

Plugging  $\frac{Var(\bar{d}_g)}{1 - \bar{d}} = 1 - c$  and  $\frac{Var(\bar{d}_g)}{\bar{d}} = 0$  in (14) yields case 3.

**Proof of the special cases for Proposition 2:** Case 1: If all groups have the same proportion of treated units, i.e.  $\bar{d}_g = \bar{d} \forall g$ , then  $Var(\bar{d}_g) = 0$ ,  $E(\bar{d}_g^2) = \bar{d}^2$ , and  $E(\bar{d}_g^3) = \bar{d}^3$ , implying that  $\tilde{\beta}_1 = \beta_1 + (\beta_T - \beta_C)\bar{d}$ .

Case 2 and 3 follow from the fact that pure control groups ( $\bar{d}_g = 0$ ) and pure treatment groups ( $\bar{d}_g = 1$ ) are absorbed by the group fixed effects.

**Proof of Proposition 3:** The group level average of a group with treatment intensity  $\bar{d}_g$  is equal to  $\bar{y}_g = \bar{d}_g y(1, \bar{d}_g) + (1 - \bar{d}_g) y(0, \bar{d}_g) = \bar{d}_g(\beta_0 + \beta_1 + \beta_T \bar{d}_g) + (1 - \bar{d}_g)(\beta_0 + \beta_C \bar{d}_g) = \beta_0 + (\beta_1 + \beta_C) \bar{d}_g + (\beta_T - \beta_C) \bar{d}_g^2$ . The approximation for  $\bar{d}_g \approx 0$ ,  $\bar{d}_g \approx 0.5$  and  $\bar{d}_g \approx 1$  follows from a Taylor-Expansion and  $\frac{\partial x^2}{\partial x} = 2x$ .