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 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

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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 these effects are generally well understood (going back at least to Cox [1958]), they are often ignored or improperly handled in empirical research, with possibly severe consequences for the interpretation of results. For example, only 23% of the difference-in-differences papers published in the major economics and finance journals in 2017 include some discussion of spillovers. In these few cases, 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.

We use a simple model that includes a direct treatment effect as well as spillover effects that depend on the fraction of treated units in a group (for example, a region or industry). Our conceptual discussion can be summarized in three messages: First, spillover effects bias the treatment effects estimate even if treatment assignment is completely random. 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.

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

\[1\] In the following, we use the terms "spillovers", "spillover effects", and "indirect effects" interchangeably.
Suppose that non-treated coffee shops experience a spillover effect, that is, an increase in sales that is stronger the more coffee shops are exposed to a fire in the same city. We can write this example as:

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

where

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

(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).

$$y_{ig} = \tilde{\beta}_0 + \tilde{\beta}_1 d_{ig} + \tilde{\epsilon}_{ig}.$$  

(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 results in an estimation of the treatment effect which is even smaller than the direct treatment 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 and can even result in the wrong sign, and not only the wrong magnitude, of the coefficient of interest.

The example also highlights the trade-off between endogeneity concerns and spillover effects. Being geographically close and hence likely exposed to similar confounding factors,

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2 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.

3 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.
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. This is because by focusing on within group variation the estimate of a treatment effect is always subject to within-group spillovers to control units. In contrast, if treated units are also compared to observations in a group with only or predominantly control units, then spillovers to control units do not bias the estimate as much.

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

\[
\bar{y}_g = \gamma_0 + \gamma_1 \bar{d}_g + u_g, \tag{3}
\]

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 estimate (\(\hat{\beta}_1\)) are often interpreted as the direct treatment effect, while aggregate effects (\(\gamma_1\)) 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 \(\hat{\beta}_1\) can therefore not be interpreted as resulting from spillover effects either, ii) \(\gamma_1\) will be mainly driven by \(\beta_C\) for low values of \(\bar{d}_g\) and by \(\beta_T\) for high values of \(\bar{d}_g\). This implies that \(\gamma_1\) will be different across data sets even if the underlying model is the same, iii) 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. Researchs should also provide arguments whether the key assumptions behind (1) are plausible (no spillovers across groups, \(\bar{d}_g\) is a sufficient statistic for the extent of spillovers). Second, researchers
should estimate the four key parameters from the individual level regressions that include spillover terms, i.e., 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. We replicate the analysis and find very similar results: firms fully dependent on Commerzbank cut employment by 2-3 percentage points (direct effect); and a one standard deviation increase in the county-level Commerzbank dependence reduces employment by 1 percentage point (indirect effect).

Applying our framework to this setting uncovers three novel insights. First, the direct effect of Commerzbank dependence on firm employment is 2-3 times larger than estimated in Huber (2018). For a firm fully dependent on Commerzbank, the direct effect amounts to 5.5 percentage points (compared to 2-3 percentage points reported in Huber (2018)). Second, the negative county-level spillovers are purely driven by the control group firms. Third, 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. There are therefore no spillover effects for firms fully dependent on Commerzbank. These three insights are consistent with the credit supply reduction being the binding constraint for firms fully dependent on Commerzbank. For firms not dependent on Commerzbank, the associated drop in demand (and not the credit supply) is the binding constraint. The spillover effects narrow the gap between control and treatment group firms, leading researchers to underestimate the direct effects of the cut in lending when not accounting for (asymmetric) spillover effects. These results are of crucial importance for policy re-
responses in a financial crisis: while direct effects can be addressed on the bank level (e.g. recapitalization), indirect effects are not internalized even by a well-capitalized bank. It is therefore of particular importance to correctly assess the relative contribution of direct and indirect effects in order to formulate an effective policy response.

This paper is related to several strands of literature. The identification of treatment response in settings with spillovers is central to studies of social economics. Typically it is assumed that an individual’s outcome varies with average group characteristics and/or the average outcome of other individuals in the reference group (e.g., other pupils in the class). This is often referred to as the linear-in-means (LIM) model (Manski [1993]).

Separately developed but conceptually linked is the literature on spatial econometrics (Anselin [1988]; Elhorst [2014]). While social economics defines groups in term of social space, spatial econometrics focuses on physical space. The reduced form econometric modelling of interference between regions, however, is closely related to the techniques employed in social economics: the outcome of interest for a region is allowed to depend on the outcomes of other (neighbouring) regions (Corrado and Fingleton [2012]). This is referred to as spatial spillovers or spatial lags.

Our model can be considered a special case of Manski (1993) and spatial econometrics models, but we provide several results that go beyond the existing literature. First, we show that even with random assignment, spillovers lead to an intricate bias in estimating treatment effects. While the existing literature has focused on the measurement of spillovers, we show that the existence of spillovers matters for estimating treatment effect even if the researcher is not interested in spillovers per se. Second, we formally discuss the trade-off between endogeneity concerns [conditional independence assumption (CIA)] and spillover concerns [stable unit treatment value assumption (SUTVA), see Rubin (1978)].

Manski (1993) shows that identification of spillover effects can fail in LIM models as one cannot distinguish between endogenous interactions (mean behavior in the reference group) and contextual interactions (exogenous attributes of group members). He coined this the “reflection problem”. See also Angrist and Pischke (2009) and Angrist (2014) on identification challenges in models that use group average outcomes to capture peer effects. Imbens and Goldsmith-Pinkham (2013) discuss the potential that the reference group is itself endogenously formed. A large literature has evolved around potential identification challenges in LIM models, including the use of non-linear generalizations, dynamic models, or network models to achieve identification. Blume, Brock, Durlauf, and Ioannides (2011) and Manski (2000) provide a comprehensive overview of this literature.

In contrast to most of the literature on social economics, a weighting matrix $W$ of spatial weights is typically used to model the effects that different regions have on each other. This allows to e.g. assign higher weights to neighbouring regions, however, requires explicit assumptions on the spatial dependence.
In particular, we show that group fixed effects, while helpful to absorb unobserved confounding factors, can exacerbate the bias that arises from spillover effects.

Another strand of the literature deals with the design of experiments in the presence of spillover effects (e.g., Duflo and Saez 2003, Baird, Bohren, McIntosh, and Özler 2018, Vazquez-Bare 2018). Here, studies can be designed in such a way that spillover effects can be explicitly identified. Importantly, in experiments a trade-off between endogeneity concerns and spillover concerns – a key focus of this paper – is non-existent by design as the researcher has full control over the setup.

The discussion about spillover effects in empirical research is also related to the literature on estimating general equilibrium effects. While we focus on identification in partial equilibrium, correctly identifying causal effects is informative for structural macroeconomic work. As argued by Nakamura and Steinsson (2018), “identified moments”, i.e., causal effects estimated using partial equilibrium techniques such as DiD setups, are helpful as target moments that macro models should match. In this paper, we show that spillover effects have to be taken into account in partial equilibrium estimations to generate meaningful identified moments. Else, estimates are biased and without clear economic interpretations.

Finally, we relate to the literature on the econometrics of difference-in-difference studies (see e.g. Bertrand, Duflo, and Mullainathan 2004, Goodman-Bacon 2018, Borusyak and Jaravel 2017). Several papers use a specific institutional background to elicit spillover effects. For example, Boehmer, Jones, and Zhang (2019) uses high-frequency (daily) data and proposes to use the average post-vs-pre change in control firm outcome as a measure for spillover effects. Barrot and Sauvagnat (2016) uses detailed supply chain data to track spillovers from natural disasters to other firms along the supply chain.

We also contribute to the literature on the effects of credit supply on the real economy (see, for instance, Chodorow-Reich 2014, Khwaja and Mian 2008). 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 Survey of papers in major journals

We provide a survey of papers that estimate treatment effects in settings where there is a clear control and treatment group. This setting is most natural in difference-in-differences settings, which account for 82 out of 610 papers published in the main economics and finance journals in 2017 (see Table 1). The difference-in-difference approach is thus clearly an important methodology in economics and finance.

The existence of spillovers implies that i) treatment effects are not uniquely defined, ii) even under random treatment assignment, the direct treatment effect (to be defined more formally below) is not correctly estimated, and iii) there can be a trade-off between endogeneity concerns and spillover concerns.

Out of the 82 difference-in-difference papers, only 19 papers (23%) contain some discussion of spillovers. Papers that discuss the effects of spillovers typically use economic theory to argue that estimates ignoring spillovers are either a lower or upper bounds (for example, because of agglomeration or demand spillovers). While the arguments might be intuitive, we show in the following section that spillovers that intuitively lead to an

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upward bias can cause a downward bias in estimating the treatment effect of interest in practice (and vice versa).

Only 14 papers (17%) contain a quantitative analysis of spillovers. Of those 14 papers only 8 papers (10%) analyze spillovers as a potential bias in the estimation of the direct treatment effect of interest. Six papers solely focus on spillovers to control group observations, but ignore the effect of spillovers on the estimate of the direct treatment effect.

Papers that address spillovers mostly follow one of three strategies: 1) control group observations where spillovers are most plausible are dropped from the sample\textsuperscript{9} 2) control group outcomes are regressed on an exposure measure to spillovers\textsuperscript{10} 3) estimates on the individual level are compared with estimates on a more aggregate level\textsuperscript{11} Existing papers use a variety of different spillover levels, with the most frequently used spillover levels being region, industry, and school/course.

Most importantly, all three strategies are applied in a relatively ad-hoc way. We show that all three strategies are viable under specific assumptions (though these assumptions are not the same for the three strategies discussed above). We argue that researchers should be aware of these assumptions in order to evaluate the adequacy of each of these three strategies. Furthermore, these strategies make inefficient use of the data and we therefore propose an estimation strategy that generalizes the three ad-hoc approaches and makes more efficient use of the data at hand.

\textsuperscript{9}An example is Hornbeck and Keniston (2017) who analyze the impact of the 1872 Boston Fire on land values. Landplots close to the Boston Fire region are excluded from the control group in order to mitigate the effect of spillovers. Another example is Dessaint and Matray (2017) who look at how managers react to salient risks by analyzing the change in cash holdings around hurricanes that did not directly affect the firm, but made hurricanes more salient to the firms’ managers. To mitigate spillover concerns, the paper analyzes the reaction of firms that are prone to earthquake risk to news about violent earthquakes outside the U.S.

\textsuperscript{10}An example includes Shue and Townsend (2017) who explain the rise in CEO compensation during the 1990s and 2000s by number-rigidity, i.e., the practice of granting the same number of stock options every year. In one specification, they regress CEO compensation on the proportion of peers with number-rigid pay to analyze spillovers to other CEOs.

\textsuperscript{11}An example includes Maggio, Kermani, Keys, Piskorski, Rancharan, Seru, and Yao (2017) who exploit variation in the timing of resets of adjustable rate mortgages to analyze the effect of lower interest rates on consumption. Regressions are performed on the borrower level and the region level in order to assess indirect effects from the resulting increase in local demand.
3 Conceptual framework

3.1 Notations and framework: The general case

We start by providing basic notation using the potential outcome framework. 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, $y_{ig}(0)$. 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.

The notations $y_{ig}(1)$ (outcome under treatment) and $y_{ig}(0)$ (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, both outcomes are the same. However, in the presence of spillovers, one needs 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 assumption states 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 the group $g$,

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The following notation closely follows Angrist and Pischke (2009) and Roberts and Whited (2012).
denoted by $\overline{d}_g$. In case that a fraction of $\overline{d}_g$ units receives treatment in group $g$, the outcome for the treated units in group $g$ is defined as $y_{ig}(1, \overline{d}_g)$ and the outcome for the control group units is defined as $y_{ig}(0, \overline{d}_g)$:

$$y_{ig} = y_{ig}(d_i, \overline{d}_g) = \begin{cases} y_{ig}(1, \overline{d}_g) & d_i = 1, \text{fraction } \overline{d}_g \text{ of units treated in group } g \\ y_{ig}(0, \overline{d}_g) & d_i = 0, \text{fraction } \overline{d}_g \text{ of units treated in group } g. \end{cases} \quad (4)$$

The treatment effect is not uniquely defined in the presence of spillovers because the treatment-minus-control difference $y_{ig}(1, \overline{d}_g) - y_{ig}(0, \overline{d}_g)$ is a function of the mean treatment intensity $\overline{d}_g$. In the following, we will frequently refer to the direct effect, which we define as $\lim_{\overline{d}_g \to 0} y(1, \overline{d}_g) - y(0, \overline{d}_g)$. The direct effect is well defined if $y(d, \overline{d}_g)$ is continuous in $\overline{d}_g$ and it is meaningful if $\lim_{\overline{d}_g \to 0} y(0, \overline{d}_g) = y(0, 0)$. We call this quantity the direct effect because it measures the treatment versus control group difference when almost all units are control group units. Given the continuity assumption, this quantity therefore has the natural interpretation of being a direct treatment effect that is not affected by spillover effects.

### 3.2 A simple model

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

**True model**:  

$$y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_T \overline{d}_g d_{ig} + \beta_C \overline{d}_g (1 - d_{ig}) + \epsilon_{ig}. \quad (5)$$

The spillover model (5) contains a direct treatment effect ($\beta_1$) as well as spillover effects to treated units ($\beta_T$) and to control units ($\beta_C$). We choose a linear model to stay as close as possible to the current practice in empirical research. In Appendix A, we show

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

14. Note that model (5) is equivalent to $y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_2 \overline{d}_g + \beta_3 \overline{d}_g d_{ig} + \epsilon_{ig}$ with $\beta_2 = \beta_C$ and $\beta_3 = \beta_T - \beta_C$. We prefer to use (5) because it more clearly disentangles the two spillover dimensions (spillover on treated units and spillover on control units). Model (5) is a special case of both Manski (1993) and of models typically used in the spatial econometrics literature (see, for example, Anselin, 1988; Elhorst, 2014). In our case, spillovers occur via $\overline{d}_g$, while Manski (1993) and spatial spillover models also allow for spillovers via $y_g$ (endogenous spillover effects) and via the error term, (see Born and Breitung, 2011) for diagnostic tests for the existence of endogenous spillover effects and spillover effects via the error term). Specific to our setup, the spillover variable $\overline{d}_g$ is bounded between $[0, 1]$, allowing us to derive statements that go beyond the results in the spatial econometrics literature.
that a model with linear spillover effects arises naturally from a simple Cournot model.

Figure 1 provides an illustration of three possible scenarios. In each scenario, the treated units are subject to a negative shock. Case 1 shows a situation 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 the case where some firms are subject to a credit supply shock which increases their funding costs. Control group firms benefit from the increase in funding costs for their competitors. The more of their direct competitors receive a credit supply shock, the higher their output. However, there are positive spillovers effects on treated firms as well: the more of their direct competitors are subject to a credit supply shock, the smaller is the disadvantage vis-à-vis their competitors, and hence the higher their output.

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 treated firms. Control group firms benefit from treated firms dropping out of the market, and the benefit is larger the more firms become insolvent.

3.3 Interpretation of spillover effects as an omitted variable

The crux of (5) is that the spillover effects are mechanically correlated with the treatment effect. Even if treatment is assigned randomly and thus

$$\text{Cov}(d_{ig}, \epsilon_{ig}) = 0, \quad (6)$$

the spillover terms are still correlated with treatment:

$$\text{Cov}(d_{ig}, \beta_T d_{ig} + \beta_C \bar{d}_g (1 - d_{ig})) \neq 0. \quad (7)$$

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15 Case 2 arises from a Cournot competition with a linear inverse demand function, see Appendix A.

16 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, $\text{Cov}(d_{ig}, \bar{d}_g) \neq 0$ for $\text{Var}(\bar{d}_g) \neq 0$ because treated units are disproportionately more prevalent in high-treatment groups, while control units are disproportionately more prevalent in low-treatment groups.
The observed difference between treatment and control units therefore only provides an estimate of $\beta_1$ if both the conditional independence assumption (CIA: $\text{Cov}(d_{ig}, \epsilon_{ig}) = 0$) and the no spillover assumption (SUTVA) are fulfilled:

\[
E[y_{ig}|d_{ig} = 1] - E[y_{ig}|d_{ig} = 0] = E[\beta_0 + \beta_1 + \beta_T \bar{d}_g + \epsilon_{ig}|d_{ig} = 1] - E[\beta_0 + \beta_C \bar{d}_g + \epsilon_{ig}|d_{ig} = 0] \tag{8}
\]

\[
\text{CIA} \Rightarrow \beta_1 + \beta_T E[\bar{d}_g|d_{ig} = 1] - \beta_C E[\bar{d}_g|d_{ig} = 0] \tag{9}
\]

\[
\text{SUTVA} \Rightarrow \beta_1. \tag{11}
\]

Under 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. This argument misses two important points. First, $E[\bar{d}_g|d_{ig} = 1]$ is generally larger than $E[\bar{d}_g|d = 0]$ because treated units are overrepresented in high treatment groups\footnote{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 $\bar{d}_g = 0.2$.}. This implies that $\beta_T E[\bar{d}_g|d = 1] - \beta_C E[\bar{d}_g|d = 0]$ 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 (5) 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 at a given level of $\bar{d}_g$, i.e. $y(1, \bar{d}_g) - y(0, \bar{d}_g) = \beta_1 + (\beta_T - \beta_C)\bar{d}_g$, or in the direct treatment effect $\beta_1$ that measures the treatment minus control difference when only few units are treated. In the presence of spillover effects, any researcher should clearly state the economic quantity of interest that she intends to measure.
3.4 Bias in estimating treatment effects when spillovers are ignored

In the following, we discuss the results of estimating

\[ y_{ig} = \tilde{\beta}_0 + \tilde{\beta}_1 d_{ig} + \tilde{\epsilon}_{ig} \] (12)

when the true model is (5). Throughout the paper, we assume that \( d_{ig} \) fulfills the conditional independence assumption. We also assume that researchers are interested in measuring the direct treatment effect \( \beta_1 \) (but the propositions below can easily be amended if a researcher aims to estimate a different function of \( \beta_1, \beta_T, \) and \( \beta_C \)). Proposition 1 and 2 show that, even when treatment is randomly assigned, estimating (12) does not generally yield the direct treatment effect, while Proposition 3 shows that the difference between estimates on the indivual level and the group level are not necessarily informative about spillover effects.

**Proposition 1** Assume \( y_i \) follows (5). Estimating (12) yields

\[
E[\hat{\beta}_1] = \beta_1 + (\beta_T - \beta_C)\bar{d} + \beta_T \frac{\text{Var}(d_g)}{\bar{d}} + \beta_C \frac{\text{Var}(\bar{d}_g)}{1 - \bar{d}}
\] (13)

Proof: See Appendix B.

Proposition 1 establishes that estimating (12) does not generally yield the direct treatment effect \( \beta_1 \). The bias in (13) 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 (12). 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, \( \beta_T \frac{\text{Var}(d_g)}{\bar{d}} + \beta_C \frac{\text{Var}(\bar{d}_g)}{1 - \bar{d}} \), 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

---

18 We make this assumption because methods to tackle endogeneity have been widely discussed in the literature, see [Angrist and Pischke (2009)] and [Roberts and Whited (2012)] for an overview. Note that, if \( d_{ig} \) does not fulfill the CIA, then researchers might spuriously detect spillover effects due to the correlation between individual outcomes and group-level means, see [Angrist (2014)].
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 $\bar{d}_g = 0.1$ while control group units are predominantly in groups with $\bar{d}_g = 0.9$. This induces another bias in $\tilde{\beta}_1$ that is increasing in $\text{Var}(\bar{d}_g)$. Thus, even with homogenous spillover effects ($\beta_C = \beta_T$), estimating (12) does not lead to an unbiased estimator of the direct treatment effect $\beta_1$.

Two special cases are worth mentioning: With an even distribution of mean treatment intensities ($\bar{d}_g \in \{\bar{d}\}$), treated units are no longer disproportionally in high-treatment groups and $\text{Var}(\bar{d}_g) = 0$. The bias in estimating $\beta_1$ thus reduces to $(\beta_T - \beta_C)\bar{d}$. With an extremely uneven distribution $\bar{d}_g \in \{0, 1\}$, $\text{Var}(\bar{d}_g) = \bar{d}(1 - \bar{d})$ and the bias in estimating $\beta_1$ reduces to $\beta_T$. These special cases thus result in estimates of $\beta_1 + (\beta_T - \beta_C)\bar{d}$ and $\beta_1 + \beta_T$. This implies that not only the magnitude, but also the direction of the bias can depend on the distribution of $\bar{d}_g$ (for example, if $\beta_T > 0$ and $\beta_C > \beta_T$).

How can we interpret the strategies applied in the empirical literature to address spillover concerns (see Table 1) in our framework? Dropping controls where spillovers are most likely can be interpreted as a regression $y_{ig} = \tilde{\beta}_0 + \tilde{\beta}_1 d_{ig} + \epsilon_{ig}$ using the entire sample of treated units but dropping control units with $d_{ig} > 0$. This results in an estimate:

$$E\left[\tilde{\beta}_1\right] = \beta_1 + \beta_T \bar{d} + \beta_T \frac{\text{Var}(\bar{d}_g)}{\bar{d}}$$  \hspace{1cm} (14)

Thus, for this strategy to provide an unbiased estimate of $\beta_1$, we need to assume $\beta_T \neq 0$ in addition to the general assumptions (exchangeability and partial interference). Testing for spillovers within control group units (see Panel B of Table 1) can be interpreted as testing for $\beta_C = 0$ in the regression $y_{ig} = \tilde{\beta}_0 + \tilde{\beta}_C \bar{d}_g + \epsilon_{ig}$ using the sample of control group units only. If $\beta_C = 0$, this results in the same estimate as (14). Again, testing for $\beta_C = 0$ is a potential avenue to deal with spillover concerns, but requires the assumption $\beta_T \neq 0$.

---

\[19\] If no pure control ($\bar{d}_g = 0$) and no pure treatment ($\bar{d}_g = 0$) group exists, then the bias associated with $\text{Var}(\bar{d}_g)$ in (13) can be avoided by using sampling weights (see Baird, Bohren, McIntosh, and Ozler (2018) for example). More specifically, using weights $1/\bar{d}_g$ for treated units and weights $1/(1 - \bar{d}_g)$ for control units offsets the overrepresentation of treated units in high-treatment groups and the overrepresentation of control units in low-treatment groups. Our target in this section is, however, not to provide an estimator that avoids bias, but rather to show the bias that arises when estimating a model without spillovers (12) in a setting where spillovers are present (5).

\[20\] If no pure control ($\bar{d}_g = 0$) and no pure treatment ($\bar{d}_g = 0$) group exists, then the bias associated with $\text{Var}(\bar{d}_g)$ in (13) can be avoided by using sampling weights (see Baird, Bohren, McIntosh, and Ozler (2018) for example). More specifically, using weights $1/\bar{d}_g$ for treated units and weights $1/(1 - \bar{d}_g)$ for control units offsets the overrepresentation of treated units in high-treatment groups and the overrepresentation of control units in low-treatment groups. Our target in this section is, however, not to provide an estimator that avoids bias, but rather to show the bias that arises when estimating a model without spillovers (12) in a setting where spillovers are present (5).
in addition to the general assumptions (exchangeability and partial interference).

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 $\hat{\beta}_1$. To the contrary, it can make the bias even harder to interpret.

**Proposition 2** Assume $y_i$ follows (5) and $\bar{d}_g \not\in \{0, 1\}$ for at least one group $g$. Estimating (12) with group fixed effects yields:

$$E \left[\hat{\beta}_1\right] = \beta_1 + \left(\beta_T - \beta_C\right) \left[\bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} - \frac{\bar{d}E\left(\bar{d}_g^3\right) - (E\left(\bar{d}_g^2\right))^2}{\bar{d} \left(\bar{d} - E\left(\bar{d}_g^2\right)\right)}\right]$$

(15)

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

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.\[21\] This is likely to be hard to interpret. 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. The proposition therefore highlights the trade-off between endogeneity concerns and spillover concerns: improving identification via fixed effects can increase concerns that spillovers bias the results.

Table 2 summarizes the results from Proposition 1 and Proposition 2, it provides

\[21\] 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 $\bar{d}_g$.  

\[Table 2 here\]
upper/lower bounds for the bias as well as results for specific cases.\textsuperscript{22} The proof can be found in Appendix B.

Table 2 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.

In settings where spillovers and aggregate effects are of interest, another common approach in the literature is a two-step procedure (see Table 1): Regressions on a disaggregated level provide estimates of the direct treatment effect. Regressions on an aggregated level are used to inform the researcher about spillovers and aggregated effects:

$$y_{ig} = \tilde{\beta}_0 + \tilde{\beta}_1 d_{ig} + \tilde{\epsilon}_{ig},$$

$$y_g = \gamma_0 + \gamma_1 \bar{d}_g + u_g.$$ (16)

This approach faces two challenges: first, $\tilde{\beta}_1$ does not measure the direct effect as discussed in details above. Second, $\gamma_1$ either disproportionately captures spillovers to controls ($\beta_C$) or spillovers to treated units ($\beta_T$) depending on the distribution of $\bar{d}_g$. This can be seen by looking at the group level averages as a function of $\beta_0, \beta_1, \beta_C$, and $\beta_T$ when the true model follows (5):

**Proposition 3** Group level averages are a non-linear function of treatment intensity even.

\textsuperscript{22} We further show in Appendix B that the bias in Proposition 2 is equal to $(\beta_T - \beta_C) \cdot \bar{d}$ if both conditions i) $\bar{d} = 0.5$ and ii) the distribution of $\bar{d}_g$ is symmetric (i.e., $Skew(\bar{d}_g) = 0$) are fulfilled. We discuss this specific example not because of its empirical relevance, but because the Beta distribution – a frequently used parametric distribution for random variables with support in $[0, 1]$ – is symmetric iff $\bar{d} = 0.5$. Given this peculiarity of the Beta distribution (symmetric for $\bar{d} = 0.5$, non-symmetric for $\bar{d} \neq 0.5$), we have chosen to use a more general two-point distribution in the simulation exercise discussed further below.
if treatment and spillover effects are both linear:

\[ E[\bar{y}_g] = \beta_0 + (\beta_1 + \beta_C)\bar{d}_g + (\beta_T - \beta_C)\bar{d}_g^2 \approx \begin{cases} 
\beta_0 + (\beta_1 + \beta_C)\bar{d}_g & \text{for } \bar{d}_g = 0 \\
\beta_0 + (\beta_1 + \beta_T)\bar{d}_g & \text{for } \bar{d}_g = 0.5 \\
\beta_0 + (\beta_1 - \beta_C + 2\beta_T)\bar{d}_g & \text{for } \bar{d}_g = 1 
\end{cases} \quad (18) \]

Proof: See Appendix B.

Thus, if \( \bar{d}_g \) is close to zero, the slope of the regression on the aggregate level will be determined by \( \beta_1 \) and \( \beta_C \). On the other hand, for larger \( \bar{d}_g \) it will mainly be determined by \( \beta_1 \) and \( \beta_T \). This makes intuitive sense: if group level average treatment intensities are low, then control units dominate and spillovers to control units dominate the group level average (and vice versa). This suggests that researchers looking at the same data generating process \([5]\) will get strikingly different slopes for regressions on the aggregate level depending on the value of \( \bar{d}_g \) in the data set at hand. The slopes might not only be different in magnitude, but can also have a different sign \([23]\).

### 3.5 Example

Assume that half of all coffee shops are subject to a negative shock (for example, a broken coffee machine) that wipes out 40% of their capacity. Without further effects, the sales of the treated firms are 40% lower and the sales of the control firms remain unchanged. In many economic situations, such as the one described above, spillovers are likely to exist. The drop in supply caused by the broken coffee machines leaves some demand unsatisfied, providing both treated and control firms with an incentive to increase capacity. Let us assume that spillovers follow a simple homogenous spillover model and that coffee shops compete within but not across cities (\( g \)):

\[ y_{ig} = 10 - 4d_i + 2\bar{d}_gd_i + 2\bar{d}_g(1 - d_i). \quad (19) \]

\[ ^{23}\text{One might argue that this is just a matter of estimating the correct functional form: if the data generating model on the individual level is linear, then the model on the aggregate level is quadratic. Given that the linearity assumption on the individual level is ad-hoc, why worry about the functional form on the aggregate level. However, we would argue that regressions on the individual and aggregate level should at least be internally consistent with each other.} \]
The situation is depicted in case 1 of Figure 2. The direct treatment effect is equal to $\beta_1 = -4$. Because spillovers are homegenous, the difference between control and treated coffee shops at the mean treatment intensity $\bar{d} = 0.5$ is also equal to $-4$ (difference between the black and orange square in the figure).

Let us further assume that in half of the cities, 90% of coffee shops are subject to the shock, while in the other half of the cities 10% of coffee shops are subject to the shock. Treated coffee shops are therefore overrepresented in high-treatment cities $[E(\bar{d}_g|d_{ig} = 1) = 0.9 \cdot 0.90\% + 0.1 \cdot 10\% = 0.82]$, and the sales of treated coffee shops are equal to $7.64 (10 - 4 + 2 \cdot 0.82)$. Control group coffee shops are underpresented in high-treatment cities $[E(\bar{d}_g|d_{ig} = 0) = 0.9 \cdot 10\% + 0.1 \cdot 90\% = 0.18]$, and the sales of control group coffee shops are $10.72 (10 + 4 \cdot 0.18 = 10.72)$. Thus, when spillovers are ignored, the treatment effect has the same sign, but a different magnitude (difference between the black and the orange triangle in the figure: $\tilde{\beta}_1 = 7.64 - 10.72 = -3.08$ versus the true direct treatment effect of $\beta_1 = -4$).

Ignoring spillovers can also lead to wrong conclusions about the direction of the treatment effect. First, estimates that ignore spillovers can have the wrong sign even when spillovers are homogenous. This can happen when the spillover coefficients ($\beta_T, \beta_C$) are large in magnitude relative to the direct treatment effect ($\beta_1$). An example is depicted in case 2 of Figure 2 while the direct treatment effect is negative, a researcher who ignores spillovers estimates a positive treatment effect ($\tilde{\beta}_1$).\footnote{More formally, in the case of homogenous spillovers ($\beta_T = \beta_C$), Proposition 1 simplifies to $E\left[\hat{\beta}_1\right] = \beta_1 + \beta_T \frac{\text{var}(\bar{d}_g)}{\bar{d}(1-\bar{d})}$. Since $\frac{\text{var}(\bar{d}_g)}{\bar{d}(1-\bar{d})} \in [0,1]$, the sign of the treatment effect estimate can flip if $\beta_T > -\beta_1$.}

Second, when spillovers are heterogenous, intuitive statements on the bounds of treatment effects can be invalid. As an example, assume that a negative shock is accompanied by spillovers that widen the difference between treated and control units (see case 3 in Figure 2). Researchers might argue that ignoring spillover effects results in a lower bound for the direct treatment effect. However, treatment estimates that ignore spillovers might actually be closer to zero than the direct treatment effect ($\beta_1$ vs $\tilde{\beta}_1$ in the figure). This is because spillovers are governed by the terms $\beta_T \cdot \bar{d}_g$ and $\beta_C \cdot \bar{d}_g$, and even if $\beta_T < \beta_C$, $\bar{d}_g$ will usually be larger for treated units than for control units.
3.6 Simulation

In this section, we more generally simulate the bias arising if spillover effects are not accounted for in the estimation. We assume a model similar to equation (19), i.e., we set the intercept to +10 and the direct treatment effect to $\beta_1 = -4$. We simulate three cases for different $\beta_T$ and $\beta_C$. Figure 3 depicts the bias as a function of the (randomly drawn) group-level treatment intensity $\bar{d}$. Panel A provides a setting where spillovers to control group units are larger than spillovers to treated units, Panel B provides the opposite setting, while Panel C provides a setting with homogenous spillovers.

Three key results stand out: First, the variation in results is large, implying that the same model can yield a high variety of treatment effect estimates if spillovers are ignored (the only exception being the fixed effects estimator with homogenous spillovers). This is potentially worrying, because – if spillovers are ignored – treatment effect estimates might not only depend on the underlying economic model, but also on the specific values of $\bar{d}$ and $Var(\bar{d}_g)$ in the data at hand. Second, the estimator without fixed effects usually has a smaller bias than the fixed effects estimator if spillovers are larger for control units than for treated units (see Panel A of Figure 3). This highlights the potential trade-off between endogeneity and spillover concerns. Third, the fixed effects estimator always leads to an “intuitive bias”: if the direct effect is negative ($\beta_1 < 0$), then the fixed effects estimator has a negative bias if spillovers are larger for control group units than for treated units (see Panel A of Figure 3) and vice versa (see Panel B of Figure 3).

[Figure 3 here]

3.7 Summary and guidance for empirical researchers

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

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

See Figure 3 for further details on the assumptions underlying the simulation exercise.
guidance ultimately has to come from economic theory and from institutional knowledge of the setting at hand.

Step 2 Estimate the model

\[ y_{ig} = \beta_0 + \beta_1 d_{ig} + \beta_T \bar{d}_g d_{ig} + \beta_C \bar{d}_g (1 - d_{ig}) + \epsilon_{ig}, \quad (20) \]

and test for the appropriate spillover level (for example, 4-digit versus 3-digit industry or county versus state). Note that (20) should be estimated without group fixed effects (unit-level or group-level controls can be added if appropriate).²⁶

Step 3 Using the coefficients \( \beta_0, \beta_1, \beta_T \) and \( \beta_C \) estimated from (20), 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:

\[
\begin{align*}
E[y_C | \bar{d}_g] & = \beta_0 + \beta_C \bar{d}_g \\
E[y_T | \bar{d}_g] & = \beta_0 + \beta_1 + \beta_T \bar{d}_g \\
E[\bar{y}_g | \bar{d}_g] & = \beta_0 + (\beta_1 + \beta_C) \bar{d}_g + (\beta_T - \beta_C) \bar{d}_g^2,
\end{align*}
\]

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 \( \bar{d}_g \).

4 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

²⁶Estimating (20) is based on the assumption that \( d_{ig} \) and \( \bar{d}_g \) are exogenous.
the financial crisis of 2008-2009 and argues that this event represents an exogenous shock 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.

4.1 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. We use 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).
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.

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 $CB_{dep_i}$ 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 $CB_{dep_{ic}}$ 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 3 shows summary statistics for the final sample. Firms have an average of two relationship banks. Consistent with Huber (2018), the average value of firm’s Commerzbank dependence is about 0.17. The average number of employees is 177 and the average firm age is about 23 years.

---

28 Huber (2018) uses proprietary data on historical bank lending relationships of German firms and fixes lending relationships in 2006. We use a 2018 snapshot of the AMADEUS BANKERS database. AMADEUS BANKERS only reports current bank lending relationships, 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 introduce noise in the estimation, our baseline estimates are very close to those reported in Huber (2018), cf. Section 4.2.1 below.

29 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
4.2 Indirect effects of Commerzbank’s lending cut

4.2.1 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:

\[
\text{employment growth}_{ic} = \beta_0 + \beta_1 \text{CB dep}_{ic} + \beta_2 \overline{\text{CB dep}}_{ic} + \gamma' X_{ic} + \epsilon_{ic},
\](24)

where \(\text{employment growth}\) is the firm’s symmetric employment growth rate over the 2008 to 2012 period, defined as: \(2 \times (\text{employment}_{2012} - \text{employment}_{2008}) / (\text{employment}_{2012} + \text{employment}_{2008})\). \(\text{CB dep}_{ic}\) measures the Commerzbank dependence of firm \(i\), located in county \(c\). \(\overline{\text{CB dep}}_{ic}\) 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 4.

Our results are broadly consistent with [Huber (2018)], whose baseline estimates we report in column 5 for comparison. The coefficient on \(\overline{\text{CB dep}}_{ic}\) ranges between -0.016 and -0.03 (vs. -0.03 in [Huber, 2018]) and the coefficient on \(\text{CB dep}_{ic}\) ranges between -0.100 and -0.155 (vs. -0.166 in [Huber, 2018]).30 Using the estimates from Table 4 column 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 of ii) and 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.

30The fact that the coefficient on the average Commerzbank dependence (\(\overline{\text{CB dep}}_{ic}\)) is extremely close to the estimates in [Huber (2018)] while the coefficient estimate on the individual Commerzbank dependence (\(\text{CB dep}_{ic}\)) is somewhat closer to zero than the corresponding estimate in [Huber (2018)] is consistent with our measure of Commerzbank dependence being a somewhat noisy estimate given that we rely on more recent bank-firm relationship data. To be specific, assume the true model is \(y = \beta \cdot \text{CB dep} + \epsilon\) (we omit subindices \(ic\) for ease of notation). Instead of the true model, we estimate \(y = \gamma \cdot (\text{CB dep} + \text{Switch}) + \epsilon\) with \(\text{Switch} \in \{-1, 0, 1\}\) capturing switching decisions to/from Commerzbank between 2006-2018. Assume
umn 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 3) would reduce employment 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.

### 4.2.2 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 3, cf. eq. (5):

\[
employment \ growth_{ic} = \beta_0 + \beta_1 \text{CB dep}_{ic} + \beta_T \text{CB dep}_{ic} \times \text{CB dep}_{ic} + \beta_C \text{CB dep}_{ic} \times (1 - \text{CB dep}_{ic}) + \gamma' X_{ic} + \epsilon_{ic}. \tag{25}
\]

Note that Huber (2018) interprets \( \beta_1 \) in eq. (24) 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. (25), 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 4), 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. (24)). Else, \( \beta_1 \) is biased and that \( P(\text{Switch} = -1) = p_S \text{CBdep} \), \( P(\text{Switch} = +1) = p_S \text{CBdep} \) (implying that average Commerzbank dependence does not change), \( \text{Switch} = 0 \) in all other cases, and \( \text{Cov}(\epsilon, \text{Switch}) = 0 \). These assumptions yield \( \text{plim} \hat{\gamma} = \frac{\text{Cov}(\text{CBdep+Switch,y})}{\text{Var}(\text{CBdep+Switch,y})} = \beta \cdot \left[ 1 - \frac{\sigma_{\text{Switch}}^2 + \sigma_{\text{Switch, CBdep}}^2 + 2\sigma_{\text{Switch, CBdep}}}{\text{Var}(\text{CBdep+Switch,y})} \right] \). Using \( \sigma_{\text{CBdep}}^2 = \text{CBdep}(1 - \text{CBdep}) \), \( \sigma_{\text{Switch}}^2 = E(\text{Switch}^2) = 2p_S \text{CBdep} \) and \( \sigma_{\text{Switch, CBdep}} = E(\text{Switch} \cdot \text{CBdep}) = -p_S \text{CBdep} \) yields \( \text{plim} \hat{\gamma} = \beta \left[ 1 - \frac{p_S}{1 - p_S \text{CBdep}} \right] \). For \( \text{CBdep} = 0.17 \) (see Table 3), a switching probability of \( p_S = 10\% \) implies an attenuation bias of 12\%, \( p_S = 20\% \) implies an attenuation bias of 24\% and \( p_S = 30\% \) implies an attenuation bias of 36\%. Switching probabilities between 20 – 30\% are thus consistent with our results (coefficient of 1.9\% in our replication versus 3.0\% in Huber (2018)). Note that the calculations above are based on the assumption \( \text{Cov}(\epsilon, \text{Switch}) = 0 \); overgreening implies \( \text{Cov}(\epsilon, \text{Switch}) < 0 \) (weak banks like Commerzbank continue providing loans to poorly performing firms) while the covenant channel (Chodorow-Reich and Falato (2018)) implies \( \text{Cov}(\epsilon, \text{Switch}) > 0 \) (weak banks cut credit in particular to poorly performing that violate covenants) so that \( \text{Cov}(\epsilon, \text{Switch}) 0 \) is plausibly consistent with prior evidence.
will reflect both the direct effect and spillover effects, cf. Proposition 1. The results from estimating eq. (25) are shown in Table 5.

Table 5

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 5, 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 5, column 1). Knowing that $\beta_C < 0$ the fact that estimating eq. (24) results in an underestimation of $\beta_1$ follows directly from Proposition 1.

Figure 4 depicts the county level spillover effects using equations (21) - (23) and the estimates from Table 5, 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

31 We obtain similar results when we use an indicator variable that equals one if $CB \ dep_{ic} \geq 0.5$, and zero otherwise, instead of the continuous measure of Commerzbank dependence.

25
caution given that the 95th percentile of the county Commerzbank dependence is 0.26 (cf. Table 3), 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 4.2.4 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.

4.2.3 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. (25) 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, 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, 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. (25) and additionally include variables that capture Commerzbank dependence at the federal state level. We define a variable, $\text{CB}_{\text{dep}}$, to capture the Commerzbank dependence of all other counties in the same federal state ("Bundesland" $\text{b}$), excluding county $\text{c}$ itself. 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 results are reported in Table 6.

![Table 6 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, cf. column 2).

---

32 We first take the county level average across all firms in the county ($\text{CB}_{\text{dep}_{\text{c}}}$). $\text{CB}_{\text{dep}_{\text{cb}}}$ is then the unweighted average of $\text{CB}_{\text{dep}_{\text{c}}}$ across all counties in the same federal state, excluding county $\text{c}$ itself. 33 Note that the estimated direct effect in Table 5, column 2 is -5.5 percentage points. We re-estimate
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.

4.2.4 Aggregated regressions

The current literature sometimes 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 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:

\[
\text{employment growth}_c = \lambda_0 + \lambda_1 \text{CB dep}_c + \lambda_2 \text{CB dep}_c^2 + \gamma' X_c + \epsilon_c, \tag{26}
\]

where the dependent variable is the average symmetric growth rate of firm employment from 2008 to 2012 across all firms located in county \(c\). \(\text{CB dep}_c\) is the average Commerzbank dependence across all firms in county \(c\). \(X_c\) is a set of firm controls based on county level averages. The results are reported in Table 7.

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 \(\text{CB dep}_c\) is per definition not defined for “city-states”, i.e., federal states that comprise only one county (Berlin, Hamburg, and Bremen).
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% × 0.137 ≈ 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 7, 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 4.2.3. For instance, to the extent that spillovers are not confined within counties, also the estimates from eq. (26) are biased if these additional spillover effects are not properly accounted for.

5 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.
References


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 ($\beta_0 = 10, \beta_1 = -4, \beta_T = \beta_C = 2$). Case 3 provides a scenario with spillovers to control group firms only ($\beta_0 = 10, \beta_1 = -10, \beta_T = 0, \beta_C = 10$).
This figure illustrates the bias that arise when spillover effects exist, but are ignored in the estimation of treatment effects. All graphs are based on the linear spillover model \( \text{(5)} \) and we assume that half of the groups have a treatment intensity of \( \theta_g = 90\% \) and half of \( \theta_g = 10\% \). Case 1 provides a scenario with homogenous spillovers \((\beta_0 = 10, \beta_1 = -4, \beta_T = \beta_C = 2)\). If spillovers are ignored, the treatment effect estimate has the same sign, but a different magnitude. Case 2 provides a scenario with larger, but still homogenous, spillovers \((\beta_0 = 10, \beta_1 = -4, \beta_T = \beta_C = 8)\). If spillovers are ignored, the treatment effect estimate has the wrong sign. Case 3 provides a scenario with heterogenous spillovers \((\beta_0 = 10, \beta_1 = -4, \beta_T = 2, \beta_C = 4)\). If treatment effects are ignored, the treatment effect estimate is closer to zero despite the fact that the heterogenous treatment effects widen the difference between treatment and control.

![Case 1](image1)

Case 1:

![Case 2](image2)

Case 2:

![Case 3](image3)

Case 3:
This figure illustrates the bias in estimating the treatment effect if the true model contains spillovers which are not accounted for in the estimation. The simulated model is \( y_{ig} = 10 - 4d_{ig} + \beta_T \bar{d}_g d_{ig} + \beta_C \bar{d}_g (1 - d_{ig}) \), with \( \beta_T \) and \( \beta_C \) given below. The mean group-level treatment intensities \( \bar{d}_g \) are assumed to follow a two-point distribution \( X \) with \( X \in \{a, b\} \) and \( P(X = b) = p, \ P(X = a) = 1 - p \) where \( a \), \( b \), and \( p \) are drawn independently from a uniform distribution. The figure provides results for \( N=10,000 \) independent simulations, the bias is calculated via Proposition 1 and Proposition 2. Orange dots depict the bias without fixed effects (Proposition 1), black dots depict the bias with fixed effects (Proposition 2). The grey line indicates a bias of zero.

**Panel A:**
\[ \beta_C = 4, \beta_T = 2 \]

**Panel B:**
\[ \beta_C = 2, \beta_T = 4 \]

**Panel C:**
\[ \beta_C = 2, \beta_T = 2 \]
**Figure 4:** 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 (21) – (22) and the estimated coefficients from Table 5 column 2. Further shown are 90% confidence intervals and the county level average employment growth ($y_{avg}$; cf. equation (23)).
Tables

Table 1: Survey of papers in Economics and Finance journals

This table provides a survey of difference-in-differences papers published in the main economics and finance journals in 2017. In step 1, we automatically search for different versions of the terms “difference-in-difference”. In step 2, we manually check all papers marked by the algorithm as difference-in-differences papers and exclude those that were marked by the algorithm but did not contain a difference-in-difference analysis. In step 3, we automatically check for terms related to spillovers. In step 4, we manually check whether spillovers were indeed discussed and/or analyzed in these papers.

<table>
<thead>
<tr>
<th>Panel A: Spillovers</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Number of Papers published in 2017 (Top 5 Econ + Top 3 Finance)</td>
<td>610</td>
</tr>
<tr>
<td>Number of difference-in-differences papers</td>
<td>82 13%</td>
</tr>
<tr>
<td>thereof: Some discussion of spillovers</td>
<td>19 23%</td>
</tr>
<tr>
<td>thereof: Some discussion and analysis of spillovers</td>
<td>14 17%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Informal methods to analyze spillovers</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Spillovers as potential concern in direct effects estimation</td>
<td>8 10%</td>
</tr>
<tr>
<td>thereof: Drop controls where spillovers are most likely</td>
<td>6 7%</td>
</tr>
<tr>
<td>thereof: Regression on individual and aggregate level</td>
<td>1 1%</td>
</tr>
<tr>
<td>thereof: Other</td>
<td>1 1%</td>
</tr>
<tr>
<td>Spillovers as supplementary evidence</td>
<td>6 7%</td>
</tr>
<tr>
<td>thereof: Within control group estimation of spillovers</td>
<td>6 7%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel C: Spillover level</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Region</td>
<td>7 37%</td>
</tr>
<tr>
<td>Industry</td>
<td>3 16%</td>
</tr>
<tr>
<td>School/Course</td>
<td>3 16%</td>
</tr>
<tr>
<td>Occupation</td>
<td>2 11%</td>
</tr>
<tr>
<td>Other (e.g. fund family, medical service type)</td>
<td>4 21%</td>
</tr>
</tbody>
</table>


35 After removing hyphens and upper case letters, we search for all versions of XinY where X and Y are either “difference”, “differences”, or “diff”. Note that this also includes versions of triple difference-in-differences. In addition, we searched for the acronyms “DiD” and “DID”.

40
We searched for the terms "spillover", "spillovers", "indirect effect", "general equilibrium", "aggregation", and "aggregate".
**Table 2:** Bias when ignoring spillovers for estimating treatment effects

This table provides the bias arising from estimating a model without spillovers (equation 12) on data generated with spillovers (5). The cases $d_g \in \{0, k\}$ and $d_g \in \{k, 1\}$ assume that there are "many" groups with $d_g = 0$ and $d_g = 1$, respectively. See the proof in Appendix B for a precise formulation.

<table>
<thead>
<tr>
<th>Case</th>
<th>Proposition 1 (no fixed effects)</th>
<th>Proposition 2 (fixed effects)</th>
</tr>
</thead>
<tbody>
<tr>
<td>General</td>
<td>$(\beta_T - \beta_C)\bar{d} + \beta_T \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \beta_C \frac{\text{Var}(\bar{d}_g)}{1-\bar{d}}$</td>
<td>$(\beta_T - \beta_C) \left[ \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} - \frac{\text{dE}(\bar{d}_g) - (\text{E}(\bar{d}_g))^2}{\bar{d} - \text{E}(\bar{d}_g)} \right]$</td>
</tr>
<tr>
<td>Bounds</td>
<td>$\beta_T [\bar{d}, 1] + \beta_C [-\bar{d}, 0]$</td>
<td>$\beta_T [0, 1] + \beta_C [-1, 0]$</td>
</tr>
<tr>
<td>Case 1:</td>
<td>$d_g \in {\bar{d}}$</td>
<td>$\beta_T \bar{d} - \beta_C \bar{d}$</td>
</tr>
<tr>
<td>Case 2:</td>
<td>$d_g \in {0, k}$</td>
<td>$\beta_T k$</td>
</tr>
<tr>
<td>Case 3:</td>
<td>$d_g \in {k, 1}$</td>
<td>$\beta_T - \beta_C k$</td>
</tr>
<tr>
<td>Case 4:</td>
<td>$d_g \in {0, 1}$</td>
<td>$\beta_T$</td>
</tr>
</tbody>
</table>
### Table 3: Summary statistics

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

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>SD</th>
<th>p5</th>
<th>p50</th>
<th>p95</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>CB $\text{dep}_{ic}$</td>
<td>0.17</td>
<td>0.29</td>
<td>0.00</td>
<td>0.00</td>
<td>1.00</td>
<td>23,436</td>
</tr>
<tr>
<td>CB $\text{dep}_{ic}$</td>
<td>0.17</td>
<td>0.06</td>
<td>0.07</td>
<td>0.17</td>
<td>0.26</td>
<td>23,436</td>
</tr>
<tr>
<td>Number of relationship banks$_{ic}$</td>
<td>2.02</td>
<td>1.15</td>
<td>1.00</td>
<td>2.00</td>
<td>4.00</td>
<td>23,436</td>
</tr>
<tr>
<td>Employment (fiscal year 2007)$_{ic}$</td>
<td>176.78</td>
<td>2,645.54</td>
<td>2.00</td>
<td>49.00</td>
<td>455.00</td>
<td>23,436</td>
</tr>
<tr>
<td>Age (fiscal year 2007)$_{ic}$</td>
<td>22.67</td>
<td>21.31</td>
<td>4.00</td>
<td>17.00</td>
<td>62.00</td>
<td>23,436</td>
</tr>
</tbody>
</table>
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. $CB \ dep_{ic}$ 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.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$CB \ dep_{ic}$</td>
<td>-0.033***</td>
<td>-0.030***</td>
<td>-0.016*</td>
<td>-0.019**</td>
<td>-0.030***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>$CB \ dep_{ic}$</td>
<td>-0.100**</td>
<td>-0.143***</td>
<td>-0.155***</td>
<td>-0.166**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
<td>(0.045)</td>
<td>(0.044)</td>
<td>(0.076)</td>
<td></td>
</tr>
<tr>
<td>Industry fixed effects</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Size bin fixed effects</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>ln age</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>
Table 5: 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. $CB\ dep_{ic}$ 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.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$CB\ dep_{ic}$</td>
<td>-0.019**</td>
<td>-0.055**</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>$CB\ dep_{ic}$</td>
<td>-0.155***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td></td>
</tr>
<tr>
<td>$CB\ dep_{ic} \times CB\ dep_{ic}$</td>
<td>0.008</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.112)</td>
<td></td>
</tr>
<tr>
<td>$CB\ dep_{ic} \times (1 - CB\ dep_{ic})$</td>
<td>-0.184***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td></td>
</tr>
</tbody>
</table>

Industry fixed effects  Yes Yes
Size bin fixed effects  Yes Yes
ln age                Yes Yes
Observations           23,436 23,436
Table 6: 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. $CB \ dep_{ic}$ is the average Commerzbank dependence of all other firms in the same county ($c$), excluding firm $i$ itself. $CB \ dep_{cb}$ 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.

<table>
<thead>
<tr>
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<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$CB \ dep_{ic}$</td>
<td>-0.055**</td>
<td>-0.062**</td>
<td>-0.104**</td>
<td>-0.109**</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.027)</td>
<td>(0.043)</td>
<td>(0.045)</td>
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<td>County level spillovers:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$CB \ dep_{ic} \times CB \ dep_{ic}$</td>
<td>0.008</td>
<td>0.044</td>
<td>0.004</td>
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<tr>
<td></td>
<td>(0.112)</td>
<td>(0.114)</td>
<td>(0.120)</td>
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<tr>
<td>$CB \ dep_{ic} \times (1-CB \ dep_{ic})$</td>
<td>-0.184***</td>
<td>-0.202***</td>
<td>-0.154**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.053)</td>
<td>(0.061)</td>
<td></td>
</tr>
<tr>
<td>Federal state level spillovers:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$CB \ dep_{cb} \times CB \ dep_{ic}$</td>
<td></td>
<td>0.196</td>
<td>0.190</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.225)</td>
<td>(0.237)</td>
<td></td>
</tr>
<tr>
<td>$CB \ dep_{cb} \times (1-CB \ dep_{ic})$</td>
<td></td>
<td>-0.346***</td>
<td>-0.218*</td>
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</tr>
<tr>
<td></td>
<td></td>
<td>(0.105)</td>
<td>(0.122)</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Size bin fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>ln age</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Observations</td>
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<td>21,870</td>
<td>21,870</td>
<td>21,870</td>
</tr>
</tbody>
</table>
Table 7: 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 $c$. $\text{CB dep}_c$ 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.

<table>
<thead>
<tr>
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<tbody>
<tr>
<td>$\text{CB dep}_c$</td>
<td>-0.137**</td>
<td>-0.606***</td>
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<tr>
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<td>(0.056)</td>
<td>(0.173)</td>
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<tr>
<td>$\text{CB dep}_c^2$</td>
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<tr>
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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:

\[
\pi_i = y_i(p - c_i) = y_i \left[ 1 - \left( \sum_{j \neq i} y_j + y_i \right) \right] - c_i \tag{A.1}
\]

\[
\frac{\partial \pi_i}{\partial y_i} = 1 - Y - c_i - y_i = 0 \tag{A.2}
\]

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

\[
y^T_i - y^C_i = -(c^T_i - c^C_i) = \gamma \Leftrightarrow y^T_i = y^C_i + \gamma \tag{A.3}
\]

Plugging (A.3) into the first-order condition for control group firms yields the control group quantities

\[
1 - n(1 - \bar{d})y^C_i - nd_i(y^C_i + \gamma) - c - y^C_i = 0 \Leftrightarrow y^C_i = \frac{1 - c - \gamma n \bar{d}}{n + 1} \tag{A.4}
\]

Combining (A.4) and (A.3) yields a simple linear model with spillovers:

\[
y_i = \frac{1 - c}{n + 1} + \gamma d_i - \gamma \frac{n}{n + 1} \bar{d} \tag{A.5}
\]

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 (A.5). 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 (A.5). 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 2

Proof of Proposition 1: For the following proofs, note that for a dummy variable \( d_{ig} \) the following equations hold:

\[
\begin{align*}
\text{Var}(d_{ig}) & = \bar{d}(1 - \bar{d}) \quad \text{(B.1)} \\
E(d_{ig} \bar{d}_g) & = E(\bar{d}_g^2) \quad \text{(B.2)} \\
\text{Cov}(d_{ig}, \bar{d}_g) & = E(\bar{d}_g) - \bar{d}^2 = \text{Var}(\bar{d}_g) \quad \text{(B.3)} \\
\text{Cov}(d_{ig}, d_{ig} \bar{d}_g) & = E(d_{ig} \bar{d}_g) - E(d_{ig}) \cdot E(d_{ig} \bar{d}_g) = E(\bar{d}_g^2)(1 - \bar{d}) \quad \text{(B.4)}
\end{align*}
\]

Using (B.1)-(B.4) and the standard omitted-variable bias formula yields:

\[
E[b] = \beta_1 + \beta_T \frac{\text{Cov}(d_{ig}, d_{ig} \bar{d}_g)}{\text{Var}(d_{ig})} + \beta_C \frac{\text{Cov}(d_{ig}, (1 - d_{ig}) \bar{d}_g)}{\text{Var}(d_{ig})}
\]

\[
= \beta_1 + \beta_T \cdot \left[ \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \bar{d} \right] + \beta_C \cdot \left[ \frac{\text{Var}(\bar{d}_g)}{\bar{d}(1 - \bar{d})} - \frac{\text{Var}(\bar{d}_g)}{\bar{d}} - \bar{d} \right]
\]

\[
= \beta_1 + (\beta_T - \beta_C)\bar{d} + \beta_T \frac{\text{Var}(\bar{d}_g)}{\bar{d}} + \beta_C \frac{\text{Var}(\bar{d}_g)}{1 - \bar{d}}
\]

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

Step 1: We show that the following relation holds:

\[
E[b] = \beta_1 + (\beta_T - \beta_C) \left[ \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} \right] - \bar{d} E\left(\bar{d}_g^2\right) - \frac{\text{Var}(\bar{d}_g)}{\bar{d}} E\left(\bar{d}_g^3\right)
\]

\[
= \beta_1 + (\beta_T - \beta_C) \left[ \bar{d} + \frac{\text{Var}(\bar{d}_g)}{\bar{d}} \right] - \frac{\bar{d} E\left(\bar{d}_g^2\right) - \left( E\left(\bar{d}_g^3\right)\right)^2}{\bar{d} E\left(\bar{d}_g^2\right) - \bar{d}^2 E\left(\bar{d}_g^3\right)}
\]

To see (B.7), note that a regression with group fixed effects \( y_i = \tilde{\beta}_1 d_{ig} + \gamma_g + \epsilon_i \) is equivalent to the de-meaned 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

\[
y_{ig} - \bar{y}_g = \beta_1 (d_{ig} - \bar{d}_g) + (\beta_T - \beta_C)(d_{ig} \bar{d}_g - \bar{d}_g^2)
\]

Using \( E(d_{ig} - \bar{d}_g) = 0, \) (B.1) - (B.4), \( E(d_{ig} \bar{d}_g^2) = E(\bar{d}_g^3), \) \( \text{Var}(d_{ig} - \bar{d}_g) = \text{Var}(d_{ig}) - \)
\( Var(\bar{d}_g) \) and the standard omitted-variable bias formula yields:

\[
E \left[ \hat{\beta}_1 \right] = \frac{Cov(\bar{d}_g - \bar{y}_g, d_g - \bar{d}_g)}{Var(d_g - \bar{d}_g)}
\]

\[
= \frac{Cov(\beta(\bar{d}_g - \bar{d}_g) + (\beta_T - \beta_C)(d_g \bar{d}_g - d_g^2), d_g - \bar{d}_g)}{Var(d_g - \bar{d}_g)}
\]

\[
= \beta_1 + (\beta_T - \beta_C) \frac{E(d_g \bar{d}_g - 2d_g \bar{d}_g^2 + \bar{d}_g^3)}{Var(d_g - \bar{d}_g)}
\]

\[
= \beta_1 + (\beta_T - \beta_C) \frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{Var(d_g) - Var(\bar{d}_g)} \tag{B.10}
\]

To see (B.7), note that \( Var(d_g) - Var(\bar{d}_g) = \bar{d} - E(\bar{d}_g^2) \), implying that (B.7) can also be written as

\[
E \left[ \hat{\beta}_1 \right] = \beta_1 + (\beta_T - \beta_C) \frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{\bar{d} - E(\bar{d}_g^2)} \tag{B.11}
\]

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

\[
E \left[ \hat{\beta}_1 \right] = \beta_1 + (\beta_T - \beta_C) \left[ \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} + \frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{\bar{d} - E(\bar{d}_g^2)} \right]
\]

\[
= \beta_1 + (\beta_T - \beta_C) \left[ \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} + \frac{(E(\bar{d}_g^2))^2 - \bar{d}E(\bar{d}_g^3)}{\bar{d} - E(\bar{d}_g^2)} \right]
\]

\[
= \beta_1 + (\beta_T - \beta_C) \left[ \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} + \theta \right], \text{ with } \theta < 0 \tag{B.12}
\]

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

\( \theta \leq \bar{d} + \frac{Var(\bar{d}_g)}{\bar{d}} \) follows directly from (B.7) and \( \frac{E(\bar{d}_g^2) - E(\bar{d}_g^3)}{Var(d_g) - Var(\bar{d}_g)} > 0 \). 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

\[
\bar{d}E(\bar{d}_g^2) - (E(\bar{d}_g^2))^2 = \frac{1}{n^2} \left( \sum_i \bar{d}_i \sum_i \bar{d}_i^3 - \sum_i [\bar{d}_i^2]^2 \right)
\]

\[
= \frac{1}{n^2} \sum_{i,j} \bar{d}_{ij} \bar{d}_{ij}^3 - \bar{d}_{ij}^2 \bar{d}_{ij}^2
\]

\[
= \frac{1}{n^2} \sum_{i} \sum_{j>i} \bar{d}_{ij} \bar{d}_{ij} (\bar{d}_{ij} - \bar{d}_{ij}^2) > 0
\]
Proof of Proposition 2 with symmetric distributions for \( \tilde{d}_{g} \): In the following, we assume that the distribution of \( \tilde{d}_{g} \) is symmetric, i.e.

\[
Skew(\tilde{d}_{g}) = E \left[ \frac{(\tilde{d}_{g} - \bar{d})}{\sigma(\tilde{d}_{g})} \right]^{3} = 0. \tag{B.13}
\]

Skewness implies that

\[
E(\tilde{d}_{g}^{3}) = 3E(\tilde{d}_{g}^{2})E(\tilde{d}_{g}) - 2 \left( E(\tilde{d}_{g}) \right)^{3} \tag{B.14}
\]

Substituting (B.14) into (B.7) and using \( E(\tilde{d}_{g}) = \bar{d} \) yields:

\[
E \left[ \hat{\beta}_{1} \right] = \beta_{1} + (\beta_{T} - \beta_{C}) \frac{E(\tilde{d}_{g}^{2}) - 3E(\tilde{d}_{g})\bar{d} + 2\tilde{d}_{g}}{Var(\tilde{d}_{g}) - Var(\bar{d}_{g})} \tag{B.15}
\]

Using \( E(\tilde{d}_{g}^{2}) = Var(\tilde{d}_{g}) + \bar{d}^{2} \) and, in the second step, \( \bar{d}(1 - \bar{d}) = Var(\bar{d}_{g}) \) yields:

\[
E \left[ \hat{\beta}_{1} \right] = \beta_{1} + (\beta_{T} - \beta_{C}) \frac{\bar{d}(1 - \bar{d}) - \bar{d}Var(\tilde{d}_{g}) + (1 - 2\bar{d})Var(\bar{d}_{g})}{Var(\tilde{d}_{g}) - Var(\bar{d}_{g})} = \beta_{1} + (\beta_{T} - \beta_{C}) \left[ \bar{d} + (1 - 2\bar{d}) \frac{Var(\bar{d}_{g})}{Var(\tilde{d}_{g}) - Var(\bar{d}_{g})} \right] \tag{B.16}
\]

For \( \bar{d} = 0.5 \), this further simplifies to

\[
E \left[ \hat{\beta}_{1} \right] = \beta_{1} + (\beta_{T} - \beta_{C}) \cdot \bar{d} \tag{B.17}
\]

Proof of lower/upper bounds for Proposition 1: Lower/upper bounds for Proposition 1: To get lower/upper bounds for (13), note that \( Var(\tilde{d}_{g}) \in [0, \bar{d}(1 - \bar{d})] \) which directly yields the lower/upper bounds given in Table 2.

Proof of lower/upper bounds for Proposition 2: We need to show that \( \bar{d} + \frac{Var(\tilde{d}_{g})}{\bar{d}} + \theta \geq 0 \) follows directly from (B.7) \( \bar{d} + \frac{Var(\tilde{d}_{g})}{\bar{d}} + \theta \leq 1 \) follows from (B.8) and the fact that \( Var(\tilde{d}_{g}) \leq \bar{d}(1 - \bar{d}) \). The fact that the minimum and maximum values can be obtained follows from the special cases in Table 2.

Proof of the special cases for Proposition 1: Cases 1 and 4 in Table 2 follow directly from (13) and the fact that \( Var(\tilde{d}_{g}) = 0 \) in case 1 and \( Var(\tilde{d}_{g}) = \bar{d}(1 - \bar{d}) \) in case 4.

Cases 2 and 3 are slightly more involved, while \( Var(\tilde{d}_{g}) \to 0 \) in both cases, one of the denominators in (13) also converges towards zero. To see case 2 \( (\tilde{d}_{g} \in \{0, k\}) \), note that \( \frac{\text{Var}(\tilde{d}_{g})}{1 - \bar{d}} \to 0 \) because the nominator converges towards 0 and the denominator converges...
towards 1. Defining $p_k := \frac{N_k}{N_0}$ yields:

$$Var\left(\bar{d}_g\right) = E\left(\bar{d}_g^2\right) - E(\bar{d}_g)^2 = p_kk^2 - p_k^2k^2 = p_kk^2(1 - p_k)$$

This implies that

$$\frac{Var\left(\bar{d}_g\right)}{d} = \frac{p_kk^2(1 - p_k)}{p_kk} = k(1 - p_k) \to k$$

Plugging $\frac{Var(\bar{d}_g)}{d} = c$ and $\frac{Var(\bar{d}_g)}{1 - d} = 0$ in (13) yields case 2.

To see case 3 ($\bar{d}_g \in \{k, 1\}$), note that $Var\left(\bar{d}_g\right) \to 0$ because the nominator converges towards 0 and the denominator converges towards 1. Defining $p_k := \frac{N_k}{N_0}$ yields:

$$Var\left(\bar{d}_g\right) = E\left(\bar{d}_g^2\right) - E(\bar{d}_g)^2 = \left(p_kk^2 + (1 - p_k)^2\right) - (1 - p_k)(1 - k) = pk(1 - k)^2(1 - p_k)$$

This implies that

$$\frac{Var\left(\bar{d}_g\right)}{1 - d} = \frac{pk(1 - k)^2(1 - p_k)}{pk(1 - k)} = (1 - p_k)(1 - k) \to 1 - k$$

Plugging $\frac{Var(\bar{d}_g)}{1 - d} = 1 - k$ and $\frac{Var(\bar{d}_g)}{d} = 0$ in (13) 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 $\tilde{\beta}_1 = \beta_1 + (\beta_T - \beta_C)d$.

Case 2 and 3 follow from the fact that pure control groups ($\bar{d}_g = 0$) and pure treatment groups ($\bar{d}_g = 0$) 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 $\bar{y}_g = \bar{d}_gy(1, \bar{d}_g) + (1 - \bar{d}_g)y(0, \bar{d}_g) = \bar{d}_g(\beta_0 + \beta_1 + \beta_T\bar{d}_g) + (1 - \bar{d}_g)(\beta_0 + \beta_C\bar{d}_g) = \beta_0 + (\beta_1 + \beta_C)\bar{d}_g + (\beta_T - \beta_C)\bar{d}_g^2$. 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 $\frac{\partial^2 x}{\partial x^2} = 2$. 
