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# Estimating Difference-in-Differences in the Presence of Spillovers\*

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## Abstract

I propose a method for difference-in-differences (DD) estimation in situations where the stable unit treatment value assumption is violated locally. This is relevant for a wide variety of cases where spillovers may occur between quasi-treatment and quasi-control areas in a (natural) experiment. A flexible methodology is described to test for such spillovers, and to consistently estimate treatment effects in their presence. This spillover-robust DD method results in two classes of estimands: treatment effects, and “close” to treatment effects. The methodology outlined describes a versatile and non-arbitrary procedure to determine the distance over which treatments propagate, where distance can be defined in many ways, including as a multi-dimensional measure. This methodology is illustrated by simulation, and by its application to estimates of the impact of state-level text-messaging bans on fatal vehicle accidents. Extending existing DD estimates, I document that reforms travel over roads, and have spillover effects in neighbouring non-affected counties. Text messaging laws appear to continue to alter driving behaviour as much as 30 km outside of affected jurisdictions.

*JEL codes:* C13, C21, D04, R23, K42.

*Keywords:* Policy evaluation, difference-in-differences, spillovers, natural experiments, SUTVA

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# 1 Introduction

Natural experiments often rely on territorial borders to estimate treatment effects. These borders separate quasi-treatment from quasi-control groups with individuals in one area having access to a program or treatment while those in another do not. In cases such as these where geographic location is used to motivate identification, the stable unit treatment value assumption (SUTVA) is, either explicitly or implicitly, invoked.<sup>1</sup>

However, often territorial borders are porous. Generally state, regional, municipal, and village boundaries can be easily, if not costlessly, crossed. Given this, researchers interested in using natural experiments in this way may be concerned that the effects of a program in a treatment cluster may spillover into non-treatment clusters—at least locally.

Such a situation is in clear violation of the SUTVA’s requirement that the treatment status of any one unit must not affect the outcomes of any other unit. In this paper I propose a methodology to deal with such spillover effects. I discuss how to test for local spillovers, and if such spillovers exist, how to estimate unbiased treatment effects in their presence. It is shown that this estimation requires a weaker condition than SUTVA: namely that SUTVA holds between *some* units, as determined by their distance from the treatment cluster. I discuss how to estimate treatment and spillover effects, and then propose a method to generalise the proposed estimator to a higher dimensional case where spillovers may depend in a flexible way on an arbitrary number of factors.

I show that this methodology recovers unbiased treatment estimates under quite general violations of SUTVA. While it is assumed that the distance of an individual to the nearest treatment cluster determines whether stable unit treatment type assumptions hold for that individual, ‘distance’ is defined very broadly. It is envisioned that this will allow for phenomena such as information flowing from treated to untreated areas, or of untreated individuals violating their treatment status by travelling from untreated to treated areas. In each case distance plays a clear role in the propagation of treatment; either information must travel out, or beneficiaries must travel in. Similarly, this framework allows for local general equilibrium-type spillovers, where a tightly applied program may have an economic effect on nearby markets, but where this effect dissipates as distance to treatment increases.

This methodology has two particular features that make it suitable for application to empirical work. Firstly, it places no strict restriction on the way in which spillovers propagate between individual observations *and* between treatment clusters. A range of other methods of estimating indirect policy

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<sup>1</sup>The SUTVA has a long and interesting history, under various guises. Cox (1958) refers to “no interference between different units”, before Rubin (1978) introduced the concept of SUTVA (the name SUTVA did not appear until Rubin (1980)). Recent work of Manski (2013), refers to this assumption as Individualistic Treatment Response (ITR). I provide additional discussion of related literature in Appendix A of this paper.

effects have been proposed which are based on a hierarchical treatment assignment, where treatment receipt is allowed to occur within a particular geographic cluster, but not to neighbouring clusters (see for example [Hudgens and Halloran \(2008\)](#); [Liu and Hudgens \(2014\)](#); [Baird et al. \(forthcoming\)](#) for some such cases). However, the spillover-robust difference-in-differences (DD) method laid out here allows spillovers of treatments from treated clusters to non-treated clusters, with the only restriction being a quite flexible geographical dependence of propagation. Secondly, the area over which spillovers occur is determined in an optimising (non ad-hoc) way. A cross-validation method is proposed to determine the size of distance bins to be considered, with some similarities to bandwidth search in regression discontinuity models. This optimising procedure provides a simple automated rule to determine spillover distances, which removes any parameter choices from a researcher's control, allowing for the avoidance of concerns that parameters may have been chosen in order to support a particular hypothesis. This procedure allows for spillovers to be determined endogenously from data. A data-snooping procedure is illustrated, along with refinements for use with large datasets. This described procedure is well-suited to difference-in-difference applications which previously have based the estimation of externalities or geographic spillovers on researcher-defined distance cut-offs (a number of important empirical examples of this type include [Miguel and Kremer \(2004\)](#) and [Almond et al. \(2009\)](#)).

In this paper, I first derive a simple closed form solution for the bias in DD models where spillovers are present. I show that the bias in naive DD models depends only on (a) the magnitude and direction of spillover effects on untreated observations, and (b) the proportion of the population impacted by spillovers. A generalised bias formula is proven, allowing for the exact derivation of biases even in cases where an arbitrary number of included and excluded treatment and "close to treatment" groups are present in a regression model. The performance of the proposed estimator is then examined, firstly, by simulation, and secondly by application to a particular empirical example. Under simulation I show that the proposed estimator recovers estimates of the treatment effect of interest, and has good size properties, even in cases where spillovers occur to a large proportion of control units. The estimator is documented to perform well, even under model mis-specification of the precise nature of spillovers, given the flexible modeling procedure employed.

In turning to empirics, this methodology is illustrated by considering the case of the passage of state-level text messaging bans for vehicle operators in the US. I return to the data and specifications of [Abouk and Adams \(2013\)](#), who document the impacts of these text-messaging bans on fatal vehicle accidents using single-vehicle single-occupant accidents, due to the increased likelihood that these accidents owe to the use of mobile telephones. I revisit their estimates using the precise geographic location of each accident, and county-level mortality figures for the US. Following their specifications and augmenting with the spillover-robust DD method proposed here, I find that allowing for spillovers suggests that counties which were not directly treated by the reform but which are located close to treated areas

are impacted in a similar way as those which were directly treated. This is a relevant result for policy evaluation, as it suggests that the reforms may have wider impacts than originally determined, and, importantly, that drivers did not simply delay the sending of text messages until they were travelling on roads in nearby areas without text messaging bans. The optimal spillover procedure finds, however, that changes in driver behaviour are perceptible over relatively short distances, of anywhere from 0-30 kilometres, depending on the reform type examined.

Although the empirical example uses a geographic measure of distance, this methodology should not be considered as limited to only spatial spillovers. Univariate measures of distance including propagation through nodes in a network, ethnic distance, ideological distance, or other quantifiable measures of difference between units can be used in precisely the same manner with the results and techniques described in this paper. I also show how multivariate measures of distance, or interactions between distance and other variables, can be similarly employed. This is particularly useful for cases where the effects of spillovers may be expected to vary by individual characteristics such as age, socioeconomic status, access to transport or access to information.

This paper joins recent literature which aims to loosen the strong structure imposed by the SUTVA. Perhaps most notably, it is (in broad terms) an application of [Manski's \(2013\)](#) social interactions framework, and [Aronow and Samii \(forthcoming\)](#)'s general interference framework, focusing on the case where spillovers are restricted to areas local to treatment clusters. However, as discussed above, unlike recent developments focusing on spillovers between treated and control units *within* a treatment cluster (notable examples in the economics literature include [McIntosh \(2008\)](#); [Baird et al. \(forthcoming\)](#); [Angelucci and Giorgi \(2009\)](#); [Angelucci and Maro \(2010\)](#)), this paper focuses on situations where entire clusters are treated, and the status of the *cluster* may affect nearby non-treated clusters. This is likely the case for quasi-experimental studies common in DD models, where 'experiments' are defined based on geographic boundaries, such as administrative political regions which set different policies.<sup>2</sup> A further discussion of the similarity and differences between the method described in this paper and other methodologies from the economic and statistical literature is provided in [Appendix A](#).

While being of direct relevance for the estimation of both treatment and spillover tests in a difference-in-difference setting, the spillover-robust DD procedure described in this paper is also a generally useful specification test which can be applied by authors wishing to partially test the assumptions underlying DD estimates. Empirical papers using DD estimates often estimate event-study specifications as a way to examine whether dependence over time is observed in changes between treatment and control areas around the reform date. The tests outlined in this paper provide a similar specification test, however

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<sup>2</sup>A very different case is that of (for example) PROGRESA/Oportunidades, where treatment clusters (ie localities or *localidades*) contained both treatment and control individuals, and the literature is concerned with spillovers between treatment and control individuals within this treatment cluster.

rather than considering temporal spillovers holding geography constant, we consider spatial spillovers holding time constant. Thus, as event studies can be considered as partial tests of the parallel trend assumption in difference-in-differences, the spillover-robust DD model can be considered as a partial test of the SUTVA, both of which fundamentally underlie the unbiasedness of DD estimates. The parallels between event studies and spillover-robust DD estimates are also drawn in that both can be considered necessary, but not sufficient to motivate the unbiased estimation of DD models.

## 2 Methodology

Define  $Y(i, t)$  as the outcome for individual  $i$  and time  $t$ . The population of interest is observed at two time periods,  $t \in \{0, 1\}$ . Assume that between  $t = 0$  and  $t = 1$ , some fraction of the population is exposed to a quasi-experimental treatment. As per [Abadie \(2005\)](#), I will denote treatment for individual  $i$  in time  $t$  as  $D(i, t)$ , where  $D(i, 1) = 1$  implies that the individual was treated, and  $D(i, 1) = 0$  implies that the individual was not directly treated. Given that treatment only exists between periods 0 and 1,  $D(i, 0) = 0 \forall i$ .

It is shown by [Ashenfelter and Card \(1985\)](#) that if the outcome is generated by a component of variance process:

$$Y(i, t) = \delta(t) + \alpha D(i, t) + \eta(i) + \nu(i, t) \quad (1)$$

where  $\delta(t)$  refers to a time-specific component,  $\alpha$  as the impact of treatment,  $\eta(i)$  a component specific to each individual, and  $\nu(i, t)$  as a time-varying individual (mean zero) shock, then a sufficient condition for identification (a complete derivation is provided by [Abadie \(2005\)](#)) is:

$$P(D(i, 1) = 1 | \nu(i, t)) = P(D(i, 1) = 1) \forall t \in \{0, 1\}. \quad (2)$$

In other words, identification requires that selection into treatment does not rely on the unobserved time-varying component  $\nu(i, t)$ . If this condition holds, then the classical DD estimator provides an unbiased estimate of the treatment effect:

$$\begin{aligned} \alpha = & \{E[Y(i, 1) | D(i, 1) = 1] - E[Y(i, 1) | D(i, 1) = 0]\} \\ & - \{E[Y(i, 0) | D(i, 1) = 1] - E[Y(i, 0) | D(i, 1) = 0]\} \end{aligned} \quad (3)$$

where  $E$  is the expectations operator.

Assume now, however, that treatment is not precisely geographically bounded. Specifically, I propose that those living in control areas ‘close to’ treatment areas are able to access treatment, either partially or completely. Such a case allows for a situation where individuals ‘defy’ their treatment

status, by travelling or moving to treated areas, or where spillovers from treatment areas are diffused through general equilibrium processes. Define  $R(i, t)$  as a binary variable which takes the value of 1 if an individual resides close to, but not in, a treatment area, and 0 otherwise. As treatment occurs only in period 1,  $R(i, 0) = 0$  for all  $i$ . Similarly, as living in a treatment area itself excludes individuals from living ‘close to’ the same treatment area,  $R(i, t) = 0$  for all  $i$  for whom  $D(i, t) = 1$ . In section 3 we return to the definition of  $R(i, t)$  to discuss the determination of “close” as well as to loosen the constant linear effect impositions that this binary variable places on the model.

Generalising from (1), now I assume that  $Y(i, t)$  is generated by:

$$Y(i, t) = \delta(t) + \alpha D(i, t) + \beta R(i, t) + \eta(i) + \nu(i, t) \quad (4)$$

If we observe only  $Y(i, t)$ ,  $D(i, t)$  and  $R(i, t)$ , a sufficient condition for estimation now consists of (2) and the following assumption:

$$P(R(i, 1) = 1 | \nu(i, t)) = P(R(i, 1) = 1) \forall t \in \{0, 1\}. \quad (5)$$

This requires that both treatment, and being close to treatment cannot depend upon individual-specific time-variant components. To see this, write (4), adding and subtracting the individual-specific component  $E[\eta(i)|D(i, 1), R(i, 1)]$ :

$$Y(i, t) = \delta(t) + \alpha D(i, t) + \beta R(i, t) + E[\eta(i)|D(i, 1), R(i, 1)] + \varepsilon(i, t) \quad (6)$$

where, following [Abadie \(2005\)](#),  $\varepsilon(i, t) = \eta(i) - E[\eta(i)|D(i, 1), R(i, 1)] + \nu(i, t)$ . We can write  $\delta(t) = \delta(0) + [\delta(1) - \delta(0)]t$ , and write  $E[\eta(i)|D(i, 1), R(i, 1)]$  as the sum of the expectation of the individual-specific component  $\eta(i)$  over treatment status and ‘close’ status<sup>3</sup>. Finally define  $\mu$  (the intercept at time 0) as:

$$\mu = E[\eta(i)|D(i, 1) = 0, R(i, 1) = 0] + \delta_0,$$

$\tau$ , a fixed effect for treated individuals, as

$$\tau = E[\eta(i)|D(i, 1) = 1, R(i, 1) = 0] - E[\eta(i)|D(i, 1) = 0, R(i, 1) = 0],$$

$\gamma$ , a similar fixed effect for individuals close to treatment, as

$$\gamma = E[\eta(i)|D(i, 1) = 0, R(i, 1) \neq 0] - E[\eta(i)|D(i, 1) = 0, R(i, 1) = 0]$$

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<sup>3</sup> $E[\eta(i)|D(i, 1), R(i, 1)] = E[\eta(i)|D(i, 1) = 0, R(i, 1) = 0] + (E[\eta(i)|D(i, 1) = 1, R(i, 1) = 0] - E[\eta(i)|D(i, 1) = 0, R(i, 1) = 0]) \cdot D(i, 1) + (E[\eta(i)|D(i, 1) = 0, R(i, 1) \neq 0] - E[\eta(i)|D(i, 1) = 0, R(i, 1) = 0]) \cdot R(i, 1)$ .

and  $\delta$ , a time trend, as  $\delta = \delta(1) - \delta(0)$ . Then from the above and (6) we have:

$$Y(i, t) = \mu + \tau D(i, 1) + \gamma R(i, 1) + \delta t + \alpha D(i, t) + \beta R(i, t) + \varepsilon(i, t). \quad (7)$$

Notice that this (estimable) equation now includes the typical DD fixed effects  $\tau$  and  $\delta$  and the double difference term  $\alpha$ . However it also includes ‘close’ analogues  $\gamma$  (an initial fixed effect), and  $\beta$ : the effect of being ‘close to’ a treatment area.

From the assumptions in (2) and (5) it holds that  $E[(1, D(i, 1), R(i, 1), D(i, t), R(i, t)) \cdot \varepsilon(i, t)] = 0$ , which implies that all parameters from (7) are consistently estimable by OLS. Importantly, this includes consistent estimates of  $\alpha$  and  $\beta$ : the effect of the program treatment and spillover effects on outcome variable  $Y(i, t)$ . Then, from (7), our coefficients of interest  $\alpha$  and  $\beta$  are:

$$\begin{aligned} \alpha = & \{E[Y(i, 1)|D(i, 1) = 1, R(i, 1) = 0] - E[Y(i, 1)|D(i, 1) = 0, R(i, 1) = 0]\} \\ & - \{E[Y(i, 0)|D(i, 1) = 1, R(i, 1) = 0] - E[Y(i, 0)|D(i, 1) = 0, R(i, 1) = 0]\}, \end{aligned}$$

and

$$\begin{aligned} \beta = & \{E[Y(i, 1)|D(i, 1) = 0, R(i, 1) = 1] - E[Y(i, 1)|D(i, 1) = 0, R(i, 1) = 0]\} \\ & - \{E[Y(i, 0)|D(i, 1) = 0, R(i, 1) = 1] - E[Y(i, 0)|D(i, 1) = 0, R(i, 1) = 0]\}. \end{aligned}$$

where the sample estimate of each parameter is generated by a least squares regression of (7) using a random sample of  $\{Y(i, t), D(i, t), R(i, t) : i = 1, \dots, N, t = 0, 1\}$ .

### 3 A Spillover-Robust Double Differences Estimator

The simple structure laid out in section 2 suggests that parameters are consistently estimable by difference-in-differences in the presence of spillovers if any geographic dependence is captured in the estimating equation. However, no discussion is provided related to actually estimating spillovers and treatment effects of interest. We are interested in estimating difference-in-difference parameters  $\alpha$  and  $\beta$  from (7). I will refer to these estimators respectively as the average treatment effect on the treated (ATT), and the average treatment effect on the close to treated (ATC). Average treatment effects are cast in terms of the [Rubin \(1974\)](#) Causal Model.

Following a potential outcome framework, I denote  $Y^1(i, t)$  as the potential outcome for some observation  $i$  at time  $t$  if they were to receive treatment, and  $Y^0(i, t)$  if the observation were not to receive

treatment. Our ATT and ATC are thus:

$$ATT = E[Y^1(i, 1) - Y^0(i, 1)|D(i, 1) = 1] \quad (8)$$

$$ATC = E[Y^1(i, 1) - Y^0(i, 1)|R(i, 1) = 1], \quad (9)$$

Given that for now we are interested in the *average* effect on those close to treatment we condition only on  $R(i, t)$ , however in the sections which follow extend to a more general form of  $R(i, t)$  to examine the rate of decay or propagations of spillovers over space.

As is typical in the potential outcomes literature, estimation is hindered by the reality that only one of  $Y^1(i, t)$  or  $Y^0(i, t)$  is observed for a given individual  $i$  at time  $t$ . The realised outcome can thus be expressed as  $Y(i, t) = Y^0(i, t) \cdot (1 - D(i, t))(1 - R(i, t)) + Y^1(i, t) \cdot D(i, t) + Y^1(i, t) \cdot R(i, t)$ , where, depending on an individual's time varying treatment and close status, we observe either  $Y^0(i, t)$  (untreated) or  $Y^1(i, t)$  (treated or close). Thus, in order to be able to estimate the quantities of interest, we rely on averages over the entire population, rather than average of individual treatment effects. As is typical in difference-in-differences identification strategies, consistent estimation requires parallel trends assumptions. In the case of treatment *and* local spillovers, this relies on:

**Assumption 1. Parallel trends in treatment and control:**

$$E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0] = E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 0, R(i, 1) = 0],$$

**Assumption 2. Parallel trends in close and control:**

$$E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 0, R(i, 1) = 1] = E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 0, R(i, 1) = 0].$$

In other words, assumption 1 and 2 state that in the absence of treatment, the evolution of outcomes for treated units and for units close to treatment would have been parallel to the evolution of entirely untreated units. This is the fundamental DD identifying assumption of parallel trends, generalised to hold for treatment *and* close to treatment status. Note that in the above, we no longer need to make *any* assumptions regarding how the impacts of treatment in treated and in close to treated areas are related (or unrelated), allowing for direct interactions between those living in treatment areas, and those living close by.<sup>4</sup>

However, as a matter of course, in order to consistently estimate any pure treatment effect, some form of the SUTVA must be invoked.<sup>5</sup> Typically, this requires that each individual's treatment status

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<sup>4</sup>From Assumptions 1 and 2 we know that  $E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0] = E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 0, R(i, 1) = 1]$ , or that the trends between treated and close to treated would have been constant in the case that no treatment were received anywhere, but we do not require that changes in outcomes are identical for treated and close to treated following the reform, ie  $E[Y^1(i, 1) - Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0] = E[Y^1(i, 1) - Y^0(i, 0)|D(i, 1) = 0, R(i, 1) = 1]$  need not hold.

<sup>5</sup>This is an identifying assumption. If all 'non-treatment' units are affected by spillovers from the treatment area, a consistent treatment effect cannot be estimated using this methodology. This is a general rule and can be couched in [Heckman](#)

does not affect each other individual’s potential outcome. Here, I loosen SUTVA. In the remainder of this article, it will be assumed that:

**Assumption 3. SUTVA holds for some units:**

*There is some subset of individuals  $j \in J$  of the total population  $i \in N$  for whom potential outcomes  $(Y_j^0, Y_j^1)$  are independent of the treatment status  $D = \{0, 1\} \forall_{i \neq j} \in N$ .*

Fundamentally, this assumption implies that SUTVA need not hold among all units. Now, rather than identification relying on each unit not affecting each other unit, it relies on there existing at least some subset of units which are not affected by the treatment status of others.

Finally, I assume that spillovers, or violations of SUTVA, do not occur randomly in the population:

**Assumption 4A. Assignment to close to treatment depends on observable  $X(i, t)$ :**

*There exists an assignment rule  $\delta(X(i, t)) = \{0, 1\}$  which maps individuals to close to treatment status  $R(i, t)$ , where  $\delta(X(i, t)) = \mathbf{1}_{X(i, t) < d}$ ,  $X(i, t)$  is an observed covariate, and  $d$  is a fixed scalar cutoff.*

This restriction is quite strong, and is loosened in coming sections. In other words, it simply states that violations of SUTVA occur in an observable way. For example, if SUTVA does not hold locally to the treatment area, assumption 4A implies that we are able to define what ‘local’ is. While this article focuses on an  $X_i$  representing geographic distance, these derivations do not imply that this must be the case. The ‘close’ indicator  $R(i, t)$  could depend on a range of phenomena including euclidean space, ethnic distance, edges between nodes in a network, strength of messaging transmission, travel time, or, as I return to discuss in section 3.3, multi-dimensional interactions between measures such as these and economic variables.

Based on assumptions 1 to 4A, difference-in-difference models can be proposed which allow for the consistent estimation of treatment parameters, even if spillovers occur. This leads to the following proposition:

**Proposition 1.** *Under assumptions 1 to 4A, the ATT and ATC can be consistently estimated by least squares when controlling, parametrically or non-parametrically, for  $R(i, t) = \mathbf{1}_{X(i, t) \leq d}$ .*

Proofs of propositions are offered in appendix B.  $\square$

In the following two subsections I examine these estimands in turn, discuss how to estimate them practically, and then loosen assumption 4A.

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and Vytlačil (2005)’s terms: ‘The treatment effect literature investigates a class of policies that have partial participation at a point in time so there is a ‘treatment’ group and a ‘comparison’ group. It is not helpful in evaluating policies that have universal participation’ (or in this case, universal participation and spillovers). Recent work by de Chaisemartin and D’Haultfoeuille (forthcoming) proposes Fuzzy Differences-in-Differences estimators, where treatment impacts are estimated based on variation in *intensity* of treatment, where all units receive treatment, but levels of treatment vary. Their setting is different to the setting here, as parallel-trend assumptions are still maintained between all high-intensity treatment areas and low-intensity treatment areas in Fuzzy Differences-in- Differences models.

### 3.1 Estimating the Treatment Effect in the Presence of Spillovers

From proposition 1, we can consistently estimate  $\alpha$  and  $\beta$ , our estimands of interest, with information on treatment status, and close to treatment status, along with outcomes  $Y(i, t)$  at each point in time. In a typical DD framework, we observe  $Y(i, t)$  and  $D(i, t)$ , however, do not fully observe  $R(i, t)$ , an individual's close/non-close status.

We do however, assume that  $X(i, t)$ , the variable measuring 'distance' to treatment is observed. From assumption 4A, we could thus map  $X(i, t)$  to  $R(i, t)$  (and later to the heterogeneous function  $R_M(i, t)$ ) using the indicator function, *if* we know the scalar value  $d$ , which represents the threshold of what is considered 'close to treatment'. *Ex ante*, in the absence some economic model, there is no reason to believe that  $d$  will be observed by researchers.<sup>6</sup> In the remainder of this section I discuss how to determine  $R(i, t)$  based on  $X(i, t)$ , in the absence of a known value for  $d$ .

Up until this point, the indicator  $R(i, t)$  has been considered as a single variable, based on the assignment rule  $\delta(X(i, t))$ . However, using the same underlying distance variable  $X(i, t)$ , the  $R(i, t)$  indicator can be further unpacked as:

$$R(i, t) = R^1(i, t) + R^2(i, t) + \dots + R^K(i, t), \quad (10)$$

where:

$$R^k(i, t) = \begin{cases} 1 & \text{if } X_i \geq (k-1) \cdot h \text{ and } X_i < k \cdot h \\ 0 & \text{otherwise} \end{cases} \quad \forall k \in (1, 2, \dots, K). \quad (11)$$

In the above expression,  $h$  refers to a bandwidth type parameter, which partitions the continuous distance variable  $X_i$  into groups of distance  $h$ .<sup>7</sup> When going forward, we will refer to this indicator function as  $R(i, t)$  when referring to the summation which results in a single binary vector, or  $R_M(i, t)$  if referring to the matrix of  $R^k(i, t)$  indicators themselves. Given the expansion of  $R(i, t)$  in equation 10, I add a final assumption:

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<sup>6</sup>That is not to say that economic intuition cannot play a role in suggesting what a reasonable value of  $d$  might be. For example, if treatment is the receipt of a program with a clear expected value and travel costs to access the program increase with distance, there will exist a cut-off point beyond which individuals will be unwilling to travel. Similarly, if treatment must be accessed in a fixed amount of time and propagation of treatment is not instantaneous, a limit for  $d$  may be calculable. This point is discussed in the comprehensive work on social interactions from Manski (2013), who states:

“response functions are not primitives but rather are quantities whose properties stem from the mechanism under study.” (Manski, 2013, p. S14)

In the model laid out here, response functions can be considered as the degree that distance from treatment can have an impact on outcomes of interest.

<sup>7</sup>So, if for example  $X_i$  refers to physical distance to treatment and the minimum and maximum distances are 0 and 100km respectively,  $h$  could be set as 5km, resulting in 20 different indicators  $R^k$ , of which each individual  $i$  in time  $t$  can have at most one switched on.

**Assumption 5. Monotonicity of Spillovers in Distance  $X(i, t)$ :**

The parameters on  $R^k(i, t)$  indicators for all  $k \in 1, \dots, K$  behave monotonely with distance when considering their impact on potential outcomes.

Beyond the assumptions made up to this point, we place no additional limits on how each  $R^k(i, t)$  variable is related to the outcome of interest. We thus allow the parameters on  $R^k(i, t)$ , which we will denote  $\beta_k$  when included in the DD model of interest, to be of indeterminate sign (but fixed across parameters due to monotonicity and assumption 3). As I document below, this assumption can be further loosened, simply requiring that treatment spillovers do not fade out at a certain distance, and then reappear at a greater distance. In terms of equation 7, this implies that the model can now be re-written as:

$$Y(i, t) = \mu + \tau D(i, 1) + \gamma_1 R^1(i, 1) + \dots + \gamma_K R^K(i, 1) + \delta t + \alpha D(i, t) + \beta_1 R^1(i, t) + \dots + \beta_K R^K(i, t) + \varepsilon(i, t), \quad (12)$$

where  $\beta_k$  terms capture and program spillovers, and  $\gamma_k$  terms are simply fixed effects.

From the above, we have partitioned  $X_i$  into  $K$  different groups. However, we are still unable to say anything about the distance  $d$  above which spillovers no longer occur. From assumptions 2 and 3, we do however know that  $d < Kh$ , implying that there are at least some units for whom spillovers do not occur. In order to motivate the estimation of  $d$ , I first layout the bias inherent in models where spillovers are not fully captured, and then suggest an iterative procedure to recover  $d$ , and unbiased treatment effects, under the maintained assumptions, while also considering how to optimally determine the definition of  $R_M(i, t)$ .

**Biasedness of Baseline DD models** Consider the estimation of the DD parameter  $\hat{\alpha}$  in equation 7 if the presence of spillovers is ignored entirely. In this simplified case, there are four included variables (including a constant term), and a compound error term equal to  $\gamma R(i, 1) + \beta R(i, t) + \varepsilon(i, t)$ . Typically, deriving the omitted variable bias in a regression with multiple omitted variables and multiple included variables is challenging, as we must consider the conditional correlation between each included variable and the omitted variables. However, in the current setting, I show that it is possible to derive a convenient closed-form formula for the omitted variable bias, given that  $R(i, t)$  is independent of  $D(i, 1)$ , conditional on  $D(i, t)$  and  $t$ , and similarly,  $D(i, t)$  is conditionally independent of  $R(i, 1)$ .<sup>8</sup> This allows for a very convenient calculation of the omitted variable bias when failing to condition on spillovers, as additional fixed effects can be ignored in our consideration of the bias in the estimate of interest  $\hat{\alpha}$ .

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<sup>8</sup>In other words,  $R(i, t) \perp \{R(i, 1), D(i, 1)\} | D(i, t), t$ . To see why, consider that knowing the distribution of  $R(i, t)$  and  $D(i, t) \forall t \in 0, 1$  then implies knowledge of the distribution of  $D(i, 1)$  and  $R(i, 1)$  when  $t=0$ . Thus, if the distributions of  $R(i, t)$  and  $D(i, t)$  are known for  $t = 1$ , knowledge of  $R(i, 1), D(i, 1), t$  provides no additional information related to these variables, and so we can conclude that  $R(i, t)$  is conditionally independent of additional dummy variables when  $D(i, t)$  is known.

To see this, consider the omitted variable bias formula for the estimated average treatment effect. The estimated parameter in a naive DD model results in the following expectation for  $\hat{\alpha}$ :

$$\begin{aligned}
E[\hat{\alpha}|\mathbf{X}] &= \alpha + \beta \frac{\text{Cov}[D(i, t), R(i, t)]|t}{\text{Var}[D(i, t)]|t} + \frac{\text{Cov}[D(i, t), \varepsilon(i, t)]|t}{\text{Var}[D(i, t)]|t} \\
&= \alpha + \beta \frac{\text{Cov}[D(i, t), R(i, t)]|t}{\text{Var}[D(i, t)]|t} \\
&= \alpha + \beta \left( \frac{(N_{D_T R_T} \cdot N - N_{D_T} N_{R_T})/N^2}{N_{D_T}(N - N_{D_T})/N^2} \middle| t \right) \tag{13}
\end{aligned}$$

$$= \alpha - \beta \left( \frac{N_{R_T}}{N - N_{D_T}} \middle| t \right). \tag{14}$$

Here, the second line comes from (2), which implies that  $[\text{Cov}(D(i, t), \varepsilon(i, t))|t] = 0$ . The remaining bias term is then the typical omitted variable bias, which depends on the conditional correlation between treatment and “close to treatment” status. While this conditional correlation can be estimated in a regression model, it also has a simple closed form solution. This closed form solution is based on the covariance and variance of binary variables, which are given in lines three and four. Given that both  $D(i, t)$  and  $R(i, t)$  are binary variables, their covariance and variance can be presented in terms of the number of observations for each variable which take values of one. These formula are presented in equation 13, where  $N$  refers to the total number of observations, and  $N_{D_T}$  and  $N_{R_T}$  the total number of observations for which  $D(i, t)$  and  $R(i, t)$  are equal to one (respectively). Finally,  $N_{D_T R_T}$  refers to the number of observations for which both  $D(i, t)$  and  $R(i, t)$  are simultaneously equal to one. Given that no treated units are “close to treatment” and vice-versa,  $N_{D_T R_T} = 0$  which allows for further simplification of the expectation of  $\hat{\alpha}$  in 14.

I demonstrate formally in Appendix B that the unconditional bias is:

$$\text{Bias}(\hat{\alpha}) = E[\hat{\alpha}|\mathbf{X}] - \alpha = -\beta \left( \frac{N_{R_T}}{N_T - N_{D_T}} \right) \tag{15}$$

where  $N_T$  refers to the total number of units when  $t = 1$ . This simple calculation documents the bias in any difference-in-difference model ignoring the presence of spillovers. It also has a logical link to the underlying structure of DD estimates and the naive treatment and control groups. The difference between the estimated value of  $\alpha$  and the true parameter owes to the contamination of the treatment group. In total, of the full control group ( $N_T - N_{D_T}$ ),  $N_R$  were exposed to treatment via spillover (where  $0 \leq N_R < (N_T - N_{D_T})$  due to assumption 4A). For this proportion of the control group, the impact of spillovers on the outcome of interest is equal to  $\beta$ . Thus, the average comparison unit will have outcomes which are  $\beta \times \left( \frac{N_{R_T}}{N - N_{D_T}} \right)$  higher than they would have had in the absence of the reform, and so treatment effects will be underestimated by this value. This formula also clearly suggests the cases in which naive DD models will *not* be biased. This will either occur in cases where  $N_{R_T} = 0$ —where

no units are “close” to treatment—or where  $\beta = 0$ —where there are no spillover effects associated with being close to the treatment of interest. This derivation of the bias due to spillovers also provides a clear analysis of the costs, in terms of bias, of not correcting for spillovers in DD analyses. The cost is higher to the degree that spillovers propagate more widely, and to the degree that spillover impacts are larger.

The expectation of  $\hat{\alpha}$  can also be denoted in terms of 12, where the reparametrised close to treatment indicators are used instead of the single measure:

$$E[\hat{\alpha}|\mathbf{X}] = \alpha - \beta_1 \left( \frac{N_{R_{1T}}}{N_T - N_{D_T}} \right) - \beta_2 \left( \frac{N_{R_{2T}}}{N_T - N_{D_T}} \right) - \dots - \beta_K \left( \frac{N_{R_{KT}}}{N_T - N_{D_T}} \right), \quad (16)$$

where  $N_{R_{1T}}$  refers to observations for which  $R^1(i, t) = 1$ , and similarly for other  $N_{R_{kT}}$  values. This follows from equation 14, and provides a characterisation of parameter bias. I demonstrate this formally in Appendix B, equation A9, using the matricial formula for the omitted variable bias. Thus, as above, when separating distance from treatment into contiguous blocks, failing to control for the impact of treatment on a particular block results in bias unless: (a) there is no unit in the block considered (ie  $N_{R_j} = 0$ ), or (b) there is no spillover effect for these units ( $\beta_j = 0$ ).

Now, finally, consider models in which distance to treatment spillovers are (at least partially) captured. We start by considering a specification in which one “close to treatment” indicator,  $R^1(i, t)$ , is included. In this case, we can once again take advantage of the regular structure of the  $\mathbf{X}$  matrix to derive the expectation of the estimated treatment effect, and hence the omitted variable bias. In Appendix B I demonstrate that the expectation of  $\hat{\alpha}$  (where I now add a superscript  $\hat{\alpha}^1$  to denote that 1 close to treatment indicator has been included) is:

$$E[\hat{\alpha}^1|\mathbf{X}] = \alpha - \beta_2 \left( \frac{N_{R_{2T}}}{N_T - N_{D_T} - N_{R_{1T}}} \right) - \dots - \beta_K \left( \frac{N_{R_{KT}}}{N_T - N_{D_T} - N_{R_{1T}}} \right), \quad (17)$$

where we now must condition on  $R^1(i, t)$  when considering the correlation between  $D(i, t)$  and all omitted distance to treatment indicators  $R^k(i, t)$ . A similar expression exists for the bias on the close to treatment parameter  $\hat{\beta}_1$ . Full details are provided in Appendix B. This suggests a general bias formula for traditional treatment effects estimated using DD, as well as spillover effects, if spillovers are not fully captured. These are, respectively:

$$Bias(\hat{\alpha}^k) = -\beta_{k+1} \left( \frac{N_{R_{k+1T}}}{(N_T - N_{D_T} - \sum_{l=1}^k N_{R_{lT}})} \right) - \dots - \beta_K \left( \frac{N_{R_{KT}}}{(N_T - N_{D_T} - \sum_{l=1}^k N_{R_{lT}})} \right) \quad (18)$$

$$Bias(\hat{\beta}_j^k) = -\beta_{k+1} \left( \frac{N_{R_{k+1T}}}{(N_T - N_{D_T} - \sum_{l=1}^k N_{R_{lT}})} \right) - \dots - \beta_K \left( \frac{N_{R_{KT}}}{(N_T - N_{D_T} - \sum_{l=1}^k N_{R_{lT}})} \right) \forall j \in 1, \dots, k. \quad (19)$$

As above, superscript  $k$  on estimates implies that  $k$  close to treatment indicators are included, and the resulting bias depends on the vector of estimates  $[\beta_{k+1}, \dots, \beta_K]$ , as well as the proportion of the remaining “control” group in each distance bin. Once again, these biases with multiple included and multiple omitted variables are demonstrated in equations A17-A18 of Appendix B.

**Determining Propagation of Spillovers** In the above, we see that without knowing the degree of spillovers, estimates of  $\alpha$  and each  $\beta_j$  will generally be biased. However, despite the derived bias in parameters, we can still use this setting to determine the distance over which spillovers propagate based on the maintained assumptions. The assumptions of there being at least some units un-impacted by spillovers, and non-monotonicity in treatment spillovers suggest an iterative procedure to determine the extent of spillovers. In what remains of this section we will assume that the optimal bandwidth search distance ( $h$  from equation 11) is known, however discuss the calculation of this parameter in a deterministic optimising procedure in the following sub-section.

If spillovers exist below some distance  $d$ , then  $\beta_j \neq 0 \ \forall \ jh < d$ . If this is the case, and if spillovers work in the same direction as treatment, then  $|E[\hat{\alpha}^0]| < |\alpha|$ , implying that the estimated treatment effect will be attenuated by spillovers of treatment to the control group. On the other hand, if treatment spillover has an opposite effect on outcomes as the impact of treatment,  $|E[\hat{\alpha}^0]| > |\alpha|$ , and the magnitude of treatment will be over-estimated. Given that, by definition, spillovers are of the same sign among themselves,  $|E[\hat{\beta}_j^k]| \leq |\beta_j|$ , with the strict equality only holding once all spillovers have been accounted for. Hence, if additional spillovers remain uncontrolled for, spillover estimates will be attenuated. *However*, due to monotonicity of spillovers,  $0 \leq |E[\hat{\beta}_j^k]| \leq |\beta_j|$ , where once again, strict equality in both relations will only hold when no additional spillovers remain uncontrolled for.<sup>9</sup>

Identification of the maximum spillover distance can then be determined iteratively, where models are tested in a step-wise process. First, the traditional DD model should be augmented with a single close to treatment indicator,  $R^1(i, t)$  (as well as its corresponding fixed effect  $R^1(i, 1)$ ). Upon estimation of this model, the following hypothesis test should be run,

$$H_0 : \beta_1 = 0 \quad H_1 : \beta_1 \neq 0,$$

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<sup>9</sup>To see this, note that in equation 19, due to the monotonicity assumption, the coefficient on each excluded “close to treatment” variable is less than or equal to the sign on the marginal included close to treatment variable. And due to Assumption 3, the proportion of the remaining control group with non-zero spillovers is strictly less than one, then:

$$|\beta_j| > \left| \beta_{k+1} \left( \frac{N_{R_{k+1}T}}{(N - N_{D_T} - \sum_{l=1}^k N_{R_{lT}})} \right) + \dots + \beta_K \left( \frac{N_{R_{KT}}}{(N - N_{D_T} - \sum_{l=1}^k N_{R_{lT}})} \right) \right| \ \forall j \in 1, \dots, k,$$

unless  $\beta_k = 0, \beta_{k+1} = 0, \dots, \beta_K = 0$ , in which case, a strict equality is observed. Thus,  $E[\hat{\beta}_j^k]$  will only converge on zero when all spillovers are controlled for, and  $E[\hat{\beta}_j^k] = 0 \Rightarrow \beta_j = 0$ .

where the test statistic  $t_{\hat{\beta}_1^1} = (\hat{\beta}_1^1 - \beta_1)/s.e.(\hat{\beta}_1^1)$  follows the  $t$ -distribution under the null hypothesis that  $\beta_1 = 0$ . Here, rejection of the null implies that spillovers occur at least up to distance  $h$ , while failure to reject the null suggests that treatment spillovers are not observed.

If this regression results in rejection of the null, this suggests that additional spillovers may still be present, and so the above model should be once again augmented with an additional close to treatment indicator  $R^2(i, t)$ , and corresponding fixed effect. The test-procedure above should then be followed, this time considering the estimate of  $\beta_2$ . Rejection of the null that  $\beta_2 = 0$  suggests that spillovers occur up to at least a distance of  $2h$  from treatment, while failure to reject the null suggests that spillovers only occur up to a distance of  $h$  from treatment.

This process should be followed iteratively up until the point that the marginal estimate  $\hat{\beta}_{k+1}$  is equal to 0. At this point, we can conclude that units at a distance of at least  $kh$  from the nearest treatment unit are not affected by spillovers, and hence, from this model, an unbiased estimate of each  $\beta_1, \dots, \beta_k$  can be formed, as well as, fundamentally, the original treatment effect  $\alpha$ . Finally, this leads to a conclusion regarding  $d$  and the indicator function  $R(i, t) = \mathbf{1}_{X(i,t) \leq d}$ . When controlling for the marginal distance-to-treatment indicator no longer results in a spillover impact, we can conclude that  $d = kh$ , and thus correctly identify  $R(i, t) = \mathbf{1}_{X(i,t) \leq kh}$  in data.

Thus far the iterative procedure described allows for the calculation of a series of spillover estimates  $\beta_j$ , which will produce (in some cases) a vector of average treatment effects on the close to treated. More information regarding the precise manner of propagation can be observed by estimating with this reparametrized  $R_M(i, t)$  matrix from (10) instead of the indicator variable  $R(i, t)$ . Nevertheless, if a single average treated effect on the close to treated (ATC) is desired (as laid out in equation 9), once the scalar value  $d$  has been determined, and with data  $\{Y(i, t), D(i, t), X(i, t) : i = 1, \dots, N, t = 0, 1\}$  in hand, we can use  $d$  to map  $X(i, t)$  into  $R(i, t)$ . Given the above we can now estimate equation 7, and form consistent estimates  $\hat{\beta}$  and a single  $\hat{\alpha}$  using OLS, recovering the ATT and ATC. In general, it is likely that the nature of spillovers over space is of interest in its own right, beyond simply unbiased estimates of the treatment effect. However, if spillovers are simply nuisance parameters rather than estimands of empirical interest, an alternative procedure can be applied for estimating the treatment effect in an unbiased way, using a similar iterative procedure, however directly considering iterations on the treatment effect of interest  $\alpha$ . We lay out this alternative procedure in Appendix C.

### 3.2 Determining Optimal Distance Bins

Up to this point it is assumed that  $h$ , the distance partition parameter, is either known, or in some other way exogenously given. However, the choice of  $h$  involves a well-known trade-off between precision

and bias. In the case that a very large value of  $h$  is chosen, very local spillovers may be concealed, and hence parameter estimates will be biased, while very small values of  $h$  will lead to imprecise estimates of spillover effects, and similarly, the iterative procedure will fail to produce unbiased estimates.

In order to optimally and non-arbitrarily determine the value of  $h$  used in spillover search, a data snooping procedure is suggested which minimises the Root Mean Squared Error (RMSE) of estimation. This minimum RMSE technique is quite closely related to bandwidth search in regression discontinuity models (see for example discussion in Ludwig and Miller (2007); Imbens and Lemieux (2008)). In order to do this, we consider the following Cross-Validation (CV) function:

$$CV(h) = \frac{1}{N} \sum_{i=1}^N (Y_i - \widehat{Y}^*(X_i(h); h, \widehat{\theta}_{-i}))^2. \quad (20)$$

This CV function calculates, for all  $i \in 1, \dots, N$ , the predicted value  $\widehat{Y}$  based on  $i$ 's realizations of  $X = X_i$ , a particular value of  $h$ , and regression parameters estimated using all observations with the exception of  $i$  ( $\widehat{\theta}_{-i}$ ). This “leave-one-out” procedure<sup>10</sup> provides a measure of the prediction error for each observation given a particular bandwidth  $h$ . In order to optimally choose  $h$ , we seek to minimize the CV function:

$$h_{CV}^* = \arg \min_h CV(h),$$

where here  $h_{CV}^*$  is the optimal bandwidth as calculated from the leave-one-out CV procedure. We return to consider the properties of this procedure under simulation in section 4.1. It is important to note that in the above CV procedure, the quantity  $\widehat{Y}$  depends on the degree that spillovers are captured using a particular bandwidth  $h$ . Thus, prior to calculating  $\widehat{Y}$ , the spillover-robust procedure described in section 3.1 is followed, with the given value of  $h$ . This is reflected in equation 20 where  $X_i(h)$ , the matrix of parameters used to predict  $\widehat{Y}$ , includes indicators for “close to treatment” areas, which depend explicitly on the chosen  $h$ .

Interestingly, in the above procedure there is nothing which limits the value of  $h$  to be constant between iterations. One could envisage a situation in which all possible combinations of  $h$  were chosen at each subsequent iteration, and hence rather than searching for a scalar  $h^*$ , the data snooping procedure would search for a vector  $\mathbf{h}_{CV}^*$ . However, computational complexity in this case increases when searching for the entire series of  $\mathbf{h}_{CV}^*$ . In particular, the algorithm complexity increases from  $O(N)$  to  $O(N^2)$ , where  $N$  refers to the number of observations for which the leave-one-out CV procedure must be performed. We return to examine these considerations, including in models where a constant  $h$  will

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<sup>10</sup>Calculating  $CV(h)$  following the leave-one-out procedure may be computationally demanding when the number of observations  $N$  is large. We thus examine alternative cross-validation procedures, including  $k$ -fold cross-validation, and stratified  $k$ -fold cross-validation, which produce estimates for  $CV(h)$  in a less computationally-demanding way. We return to these considerations in section 4.1.

result in a mis-specified model, in section 4.1.

### 3.3 Estimating with Multidimensional Spillovers

Previously it has been assumed that  $R(i, t)$  is a function of a unidimensional distance measure  $X(i, t)$ . We now generalise this to a multidimensional case where  $R(i, t)$  may depend upon an arbitrary number of variables  $\mathbf{X}(i, t)$ . This allows for cases where distance to treatment may interact with some other variable, such as income, ownership of a vehicle or access to information (among other things). In order to allow for spillovers to depend upon a range of observable variables, we must generalise assumption 4A. In order to do this, the following new terminology is introduced, following Zajonc (2012). An assignment rule,  $\delta$ , maps units with covariates  $\mathbf{X} = \mathbf{x}$  to close assignment  $r$ :

$$\delta : \mathcal{X} \rightarrow \{0, 1\}.$$

This leads to a close-to-treatment assignment set  $\mathbb{T}$  defined as:

$$\mathbb{T} \equiv \{\mathbf{x} \in \mathcal{X} : \delta(\mathbf{x}) = 1\}$$

whose complement  $\mathbb{T}^c$  is known as the control assignment set. Finally then, we can write the treatment assignment rule<sup>11</sup>:

$$\delta(x) \equiv \mathbf{1}_{\mathbf{x} \in \mathbb{T}}. \quad (21)$$

With this (multidimensional) treatment assignment rule in hand, a more general version of assumption 4A can now be provided:

**Assumption 4B.** *Assignment to close to treatment depends on observable  $\mathbf{X}(i, t)$ :*

*A multidimensional assignment rule  $\delta(x) = \mathbf{1}_{\mathbf{x} \in \mathbb{T}}$  exists which maps individuals to close to treatment status  $R(i, t)$ , where  $\mathbf{X}(i, t)$  are observed covariates, and  $\mathbb{T}$  is a fixed function of  $\mathbf{X}(i, t)$ .*

**Proposition 2.** *Under assumptions 1–3 and 4B, the ATT and ATC can be consistently estimated by least squares when controlling, parametrically or non-parametrically, for  $R(i, t) = \mathbf{1}_{\mathbf{x} \in \mathbb{T}}$ .*

Refer to appendix B for proof.  $\square$

Now, in the same manner, as laid out previously, we can go about generating our estimands of interest, replacing  $R(i, t) = \mathbf{1}_{X_i \leq d}$  with  $R(i, t) = \mathbf{1}_{\mathbf{x} \in \mathbb{T}}$ . The most computationally demanding step in this estimation procedure is in forming a parametric or non-parametric version of the underlying function

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<sup>11</sup>The uni-dimensional case discussed up to this point is just a particular application of the treatment assignment rule where  $\mathbf{X}(i, t) = X(i, t)$  and  $\mathbb{T} \equiv \{x < d : \delta(x) = 1\}$

$R(i, t)$  over which to search. In a unidimensional framework it is reasonably straightforward to form local linear bins for  $R(i, t)$ . However, in the multidimensional framework this is no longer the case. Additionally, as the dimensionality of  $\mathbf{X}$  rises, the number of search dimensions for spillovers also rises, leading to curse of dimensionality type considerations in the estimation of  $\alpha$ .

The particular functional form assigned to  $R(i, t)$  will be context-specific, and ideally driven by economic theory. As mode of example, below we consider the case where  $R(i, t) = f(X_1, X_2)$  is a function of two variables, one binary and the other continuous. Such a case would be appropriate for a situation in which spillovers depend upon distance to treatment and some indicator, such as having access to a motor vehicle. Consider the case where  $X_1 \in \{0, 1\}$  is binary, and  $X_2$  continuous. Then we can parametrise  $R(i, t)$  as:

$$\begin{aligned} R(i, t) &= f(X_1, X_2) \\ &= X_1 \cdot [\beta_{0,1}X_2^1(i, t) + \dots + \beta_{0,K}X_2^K(i, t)] \\ &+ (1 - X_1) \cdot [\beta_{1,1}X_2^1(i, t) + \dots + \beta_{1,K}X_2^K(i, t)]. \end{aligned}$$

where  $X_2^k(i, t) \forall k \in 1 \dots K$  is defined as per (11). Estimation of  $\alpha$  can then proceed iteratively as in section 3.1. First a traditional DD parameter is estimated ignoring the possibility that spillovers exist, leading to the proposed estimate  $\hat{\alpha}^0$ . Then  $X_1 \cdot [\beta_{0,1}X_2^1(i, t)]$  and  $(1 - X_1) \cdot [\beta_{1,1}X_2^1(i, t)]$  are included in the regression, leading to an updated estimate  $\hat{\alpha}^1$ , as well as two separate ‘‘spillover’’ estimates,  $\hat{\beta}_{0,1}$  and  $\hat{\beta}_{1,1}$ . If the hypothesis  $H_0 : \beta_{0,1} = 0$  cannot be rejected this suggests that marginal spillovers are a relevant phenomenon for this group, and a further iteration should proceed. Similarly, if  $H_0 : \beta_{1,1} = 0$  cannot be rejected, an additional ‘‘close to treatment’’ indicator should be included for units with  $X_1 = 1$ . Iteration proceeds in this fashion, until the marginal  $X_2^k(i, t)$  indicators for  $X_1 \in \{0, 1\}$  no longer result in perceived spillovers. At this point,  $\hat{\alpha}^k$  is accepted as the ATT, and the vector of  $\beta_{j,1}, \dots, \beta_{j,k}$  for  $j \in \{0, 1\}$  estimates describe treatment effects on the close to treated.

## 4 Results

To examine the performance of the spillover-robust DD estimator proposed in section 3, we first examine recovered estimates under a range of (known) data generating processes (DGPs) via Monte Carlo simulation in section 4.1. We then turn to an extended empirical example in section 4.2, where we examine whether state-level text messaging bans have local spillovers over roadways, given that driving behaviour does not update immediately at state borders.

## 4.1 Monte Carlo Evidence

We conduct a series of estimates under simulation, where we are principally interested in two considerations: the first, does the proposed estimation strategy adequately capture the nature of spillovers, and secondly, does this procedure allow us to correctly recover good estimates of the impact of treatment when naive control groups would otherwise be locally contaminated. In order to do so, we focus on a number of alternative DGPs. These are chosen as they will allow us to examine the estimand’s performance both when the spillover bins are correctly specified, and when the procedure suggested in the spillover-robust DD methodology is unable to precisely capture the geographical nature of spillovers (ie, in cases of model mis-specification).

We consider first a model which is entirely amenable to estimation following the proposed spillover-robust DD methodology. This first model is:

$$y_{it} = \alpha + \beta T_{it} + \sum_{j=1}^4 \gamma_j \text{close}_{it,((j-1) \times 5, j \times 5]} + \phi_t + \lambda_i + \varepsilon_{it},$$

where the outcome of interest is a function of treatment receipt ( $T_{it}$ ), as well as four spillover indicators, which capture spillovers occurring in bins of 5 units of distance, and are indicated in each variable’s subscript. These are defined to be mutually exclusive, and to refer to units between (0,5] units from treatment, from (5,10], from (10,15] and from (15,20]. Treatment is a binary indicator, fixed to be equal to 1 for 20% of the sample, and distance is simulated to allow for the examination of estimated parameters when spillovers occur to 5, 10 or 25% of the population. The difference-in-difference structure of the model is captured with the inclusion of a time fixed effect ( $\phi_t$ ) and unit fixed effects ( $\lambda_i$ ), where we consider two time periods, with treatment switched on for treated units in period 2. We define treatment effects  $\beta$  and close to treatment effects  $\gamma$  to decrease as the distance from treatment increases. Specifically  $\theta = (\beta, \gamma_1, \gamma_2, \gamma_3, \gamma_4) = (10, 5, 4, 3, 2)$ , and,  $\varepsilon \sim \mathcal{N}(0, \sigma)$ , where  $\sigma$  is allowed to vary between simulations, taking values of 1, 2 or 5.

Model 1, described above, is particularly suitable for estimation using the spillover-robust DD model given that spillovers are linear in nature, and are demarcated in constant bins of 5 geographic units. We thus consider two alternative models to examine the performance of the estimator where the spillover indicators will be, by necessity, mis-specified. The first consider spillovers which are linear, however occur in irregular distance intervals. Specifically, the outcome is generated as:

$$y_{it} = \alpha + \beta T_{it} + \sum_{j \in \{(0,2], (2,9], (9,16], (17,20]\}} \gamma_j \text{close}_{itj} + \phi_t + \lambda_i + \varepsilon_{it}$$

where now close to treatment effects are assigned in distances of (0,2],(2,9],(9,16] and (17,20] from

treatment, and where the remaining details, including parameter vector  $\theta$  follow those from model 1. Finally, we consider a specification where spillovers no longer follow a step-wise linear process, but do occur monotonically with distance. In particular, the third model examined is generated according to the following DGP:

$$y_{it} = \begin{cases} \alpha + \beta T_{it} + \gamma \exp(-dist) + \phi_t + \lambda_i + \varepsilon_{it} & \text{if } 0 < dist \leq 10 \\ \alpha + \beta T_{it} + \phi_t + \lambda_i + \varepsilon_{it} & \text{otherwise.} \end{cases}$$

Once again, here the treatment effect  $\beta = 10$ , and in this case  $\gamma = 5$ .

In Figure 1, we consider the cross validation search for  $h^*$  in each model following the LOOCV procedure described in section 3.2. These figures correspond to cases where the stochastic error term follows a normal distribution with mean zero and standard deviation of 1, and where spillovers occur to 10% of the total sample. Full sets of figures corresponding to spillovers reaching a smaller and larger portion of the sample are provided in Appendix Figure A1. These figures plot the Root Mean Squared Error associated with a range of potential search bins for spillovers,  $h$ , where in each case, a given  $h$  will then result in inclusion of “close to treatment” indicators up until the marginal close to treatment unit results in insignificant estimates. According to the procedure described above, the optimal spillover bin  $h^*$  to use in the spillover-robust DD procedure is that which minimises RMSE among all competing options.

Figure 1 displays the RMSE for each of a range of values of  $h$  varying from 1 to 25 (displayed on the  $x$ -axis). We stop searching at  $h = 25$  in this case as RMSE only increases after this point. The left-hand panel displays values associated with model 1, the centre panel displays values associated with model 2, and the right-hand panel describes values for model 3. In each case, thin gray lines represent a single simulation, while the thick green line represents averages over the full set of simulations. We see that overwhelmingly, the RMSE procedure recovers “correct” search bins based on the DGPs described previously. In model 1, optimal RMSE corresponds to distance of 5, in line with the 5 unit spillover bins, model 2 results in a minima at  $h = 2$ , and model 3 (the non-linear specification) results in optimally choosing a short search bandwidth to closest approximate the smooth decrease in spillovers over distance from the exponential function. In Appendix Figure A1 similar figures are presented for cases where spillovers reach larger (25%) or smaller (5%) portions of the population. Generally optimally determined bandwidths agree with those discussed here, however in cases where spillovers occur to a larger proportion of the population, greater power results in a smaller optimal bandwidth for model 2, in which irregular spillovers occur. In the case where spillovers reach 25% of the population (Appendix Figure A1f),  $h^*$  is determined as 1, allowing for irregular spillovers to be perfectly captured.

More important than the determination of the optimal search bin  $h^*$  is the estimation of treatment

effects themselves. In Table 1, I provide estimated treatment effects from the spillover-robust DD models, along with the size of tests when examining the null that  $\beta=10$ , at a 5% significance level. If the spillover-robust DD model correctly recovers parameters in repeated simulations, the size of the test should be approximately 0.05. Table 1 presents estimates associated with each of the 3 DGPs in separate panels, varying the degree of spillovers in columns, and the noise due to the stochastic error term between rows. In each case, we first present the naive estimate for  $\beta$  when not considering any spillovers. In each case, we observe that (unsurprisingly) naive models perform very poorly. Given that spillovers are of the same direction as treatment itself, we see that in each case, failing to account for spillovers results in attenuation of estimated treatment effects, as well as considerable over-rejection of the null. What's more, naive DD models perform increasingly worse when spillovers propagate to a larger proportion of the population. This results is exactly as outlined in the bias calculation documented in equation 16. Moving across columns, the proportion of the control group which is contaminated by spillovers increases, and so the bias increases in magnitude.<sup>12</sup>

Turning now to models in which spillover-robust DD models are estimated, we see that unlike the naive models, parameter estimates as well as the size of hypothesis tests perform correctly. For each model simulated, and for each value of the stochastic error term and degree of spillovers in Table 1, the size of the test is good, and estimated treatment effects are extremely close to  $\beta = 10$ . In considering the size of tests, rates of rejection of the null are always within  $\pm 0.008$  of the expected rate of 0.05, and indeed are often exactly 0.05 (based on 2,500 simulations). It is important to note that this occurs even when models are mis-specified in the spillover robust-DD search procedure. For model 2 and model 3, determining spillovers with a constant  $h$  will generally fail to capture the true DGP, but as we see in Monte Carlo tests, it does allow us to correctly estimate treatment effect, even when the simulation process is noisy. Similarly, even though at times we observe that average  $h^*$  across simulations are not precisely as in the DGP (especially when more noise is introduced), we observe that the proposed procedure results in good properties in testing.

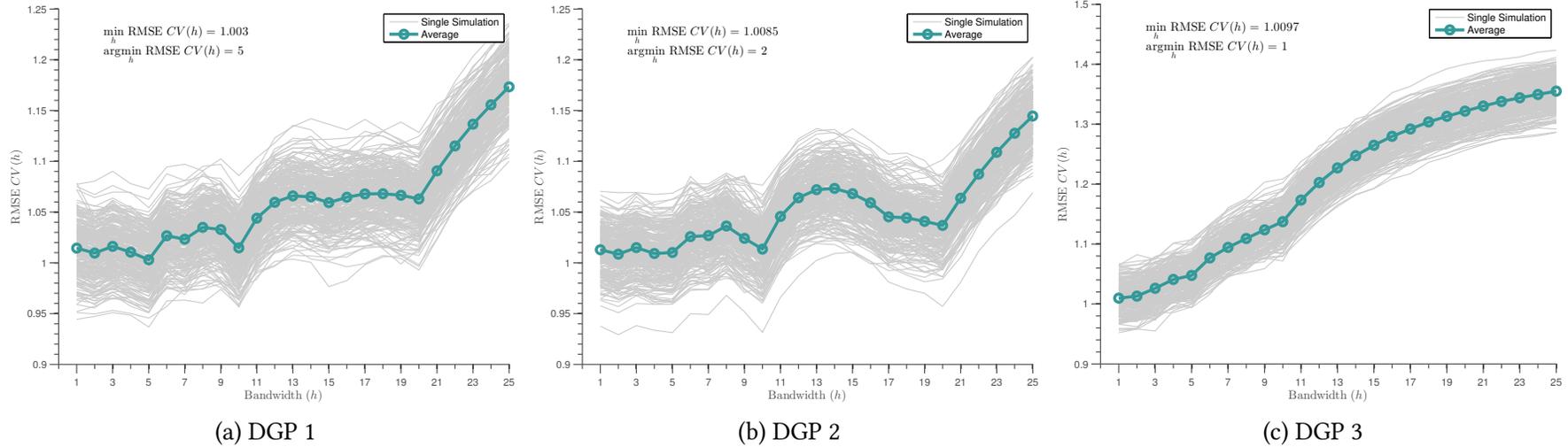
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<sup>12</sup>Indeed, we can also show that the bias formula holds as derived (subject to minor variation due to variation in simulation). Considering equation 16 and model 1 with 10% spillovers from Table 1, the expectation for  $\widehat{\beta}$  is:

$$E[\widehat{\beta}] = 10 - 5 \frac{0.025}{1 - 0.2} - 4 \frac{0.025}{1 - 0.2} - 3 \frac{0.025}{1 - 0.2} - 2 \frac{0.025}{1 - 0.2} = 9.5625$$

which is identical to the second decimal point to the average estimated parameter in simulation.

Figure 1: Root Mean Squared Error and Bandwidth Search



22

NOTES TO FIGURE 1: Root mean squared error (RMSE) under various data generating processes is displayed, allowing for spillovers calculated using bandwidth  $h$ . Gray solid lines present alternative simulations (250 simulations shown here), and the solid green line with circles documents the average RMSE for each bandwidth over the full set of simulations. DGPs are laid out fully in section 4.1. For each simulation,  $N = 1000$  observations and  $\varepsilon \sim \mathcal{N}(0, 1)$ . 20% of the sample are treated units, and 10% of the sample are “close to treated” in the original DGPs. Identical plots for alternative degrees of spillovers are displayed in Appendix Figure A1. The RMSE calculated for each bandwidth shown is based on the iterative procedure described in the text, with the optimal spillover-robust DD model corresponding to that estimated using the bandwidth which returns the lowest RMSE. The minimum RMSE and the minimum bandwidth associated with this RMSE (ie the optimal model bandwidth) is displayed in each panel.

Table 1: Monte Carlo Simulation: Optimal Bandwidth and Average Treatment Effects

	Spillover = 5%				Spillover = 10%				Spillover = 25%			
	$\bar{h}^*$	$\widehat{\beta}$	Std. Dev.	Size	$\bar{h}^*$	$\widehat{\beta}$	Std. Dev.	Size	$\bar{h}^*$	$\widehat{\beta}$	Std. Dev.	Size
<b>Model 1</b>												
Naive	—	9.782	0.235	0.132	—	9.565	0.234	0.402	—	8.902	0.245	0.990
$\varepsilon = 1$	5.036	9.998	0.079	0.050	4.859	9.999	0.081	0.049	4.931	9.999	0.085	0.056
$\varepsilon = 2$	8.126	10.001	0.161	0.050	6.074	10.000	0.158	0.048	4.928	10.001	0.166	0.050
$\varepsilon = 5$	14.923	9.997	0.386	0.042	13.058	9.981	0.404	0.056	8.935	10.003	0.412	0.046
<b>Model 2</b>												
Naive	—	9.795	0.243	0.133	—	9.574	0.243	0.381	—	8.930	0.239	0.988
$\varepsilon = 1$	5.114	9.998	0.079	0.053	3.336	9.998	0.080	0.055	1.534	9.997	0.082	0.052
$\varepsilon = 2$	10.179	9.997	0.157	0.048	7.499	10.002	0.159	0.051	4.413	10.003	0.167	0.050
$\varepsilon = 5$	16.068	9.986	0.407	0.057	14.926	10.005	0.403	0.056	11.532	10.009	0.440	0.061
<b>Model 3</b>												
Naive	—	9.875	0.232	0.071	—	9.732	0.240	0.176	—	9.333	0.231	0.730
$\varepsilon = 1$	1.916	9.998	0.079	0.046	1.325	10.002	0.079	0.052	1.004	9.999	0.081	0.053
$\varepsilon = 2$	3.159	9.993	0.158	0.051	2.284	9.991	0.159	0.051	1.540	9.998	0.165	0.054
$\varepsilon = 5$	6.202	9.992	0.398	0.052	4.789	9.985	0.401	0.048	3.286	9.958	0.413	0.057

NOTES: Models 1, 2 and 3 are described in section 4.1. The top line of each panel and model presents naive estimates without correcting for spillovers, while each line below consists of 2,500 simulations applying the spillover robust estimation technique.  $\bar{h}^*$  refers to the average spillover distance calculated in cross-validated models,  $\widehat{\beta}$  the average parameter estimate for  $\beta$  (which is equal to 10 in the true DGP) with its standard deviation among 2,500 trials, and the size reports the proportion of true null hypotheses  $H_0 : \beta = 10$  rejected at  $\alpha = 0.05$  across the 2,500 trials. The optimal bandwidth  $h^*$  for spillover-robust DD models are determined using Leave-One-Out Cross-validation.

Finally, it is important to note that while RMSE calculated using LOOCV leads to (generally) correctly captured spillover bandwidths, and estimated treatment effects with good size properties, in certain cases the use of LOOCV will be computationally infeasible, specifically as the number of observations grows. In these cases,  $k$ -fold CV offers a computationally convenient alternative manner to calculate  $h^*$ . We document in Appendix Figure A2 and Appendix Table A1, that the use of  $k$ -fold CV results in identical optimal spillover bins, even if calculations of the RMSE are generally higher when generated using  $k$ -fold CV, given that predictions are made using fewer training observations. Furthermore, when considering the parameters estimated using  $k$ -fold, rather than LOO, CV we find largely similar results in terms of size and estimated parameters. These are fully documented in Appendix Table A1, where performance is qualitatively identical. We demonstrate a case where  $k$ -fold CV is of considerable use below in an extended empirical example.

## 4.2 An Applied Example: Test Messaging Bans and Local Spillovers over Roadways

In order to examine the performance of the spillover-robust DD strategy in an applied setting we consider estimates from an existing DD study, in which only headline treatment are estimated, but in which propagation of treatment over space may plausibly occur. To do this, we revisit the results of [Abouk and Adams \(2013\)](#), who examine the passage of state-level laws in the US prohibiting the sending of text messages while driving. While [Abouk and Adams \(2013\)](#) document a range of impacts of these laws on rates of deaths in Single Vehicle Single Occupant (SVSO) accidents in a DD setting, they restricted their attention only to the impacts on accidents occurring in the same states in which reforms were implemented.

Nevertheless, there is reason to suspect that laws of this type will not be uniquely restricted to the states where they are passed. If drivers do actually alter their behaviour in the presence of the law, it is plausible that their behaviour may not immediately revert to be what it would have been in the absence of the law when driving across state boundaries. In particular, there are various outcomes which may be observed. Firstly, it is possible that drivers who are convinced by the state law to reduce the usage of mobile phones when driving will maintain their improved behaviour when crossing into nearby states, with perceptible reductions in mortality on roadways *also* in areas close to the borders of treated states. Alternatively, an unintended behaviour may be observed, where the law simply causes inter-state drivers to hold-off on using their mobile phone when driving until they cross into areas without a law, meaning that any estimates of the reform's impact using fatal accidents in-state actually overstates the true impact. These two potential behaviours work in opposite directions, suggesting that the actual impact of reforms may be either over or understated, implying that the estimation of

spillover robust DD models allows for the resolution of an empirically relevant policy and behavioural question.

[Abouk and Adams \(2013\)](#) collect data on all text-messaging bans passed into state law between 2007 and 2010, and use data on all fatal accidents over this period from the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration. They also classify text-messaging bans into two broad classes: weak or strong, which depends on whether text-messaging is a primary (strong) or secondary offense or restricted to young drivers (weak).<sup>13</sup> In the original paper the authors collapse accidents to state by month cells, for 48 months (Jan 2007-Dec 2010) for 49 states (Alaska is removed due to missing data). The original paper estimates DD models focusing on the rate of accidents by state, and the impact of the different text-messaging bans. In order to examine the occurrence of local spillovers, we consider the impact on a county-by-county basis here. This permits for a much finer analysis of the distance to treatment. In Appendix Table A2 we provide the original (state-level) summary statistics of dependent and independent variables from [Abouk and Adams \(2013\)](#), and below identical variables collected at a county-level. Appendix Figure A3 documents the geographic distribution of all accidents along with state and county boundaries. In Appendix Table A3 we replicate their full DD analysis at the county-level, finding largely identical results, and in Appendix Table A4 we show simple DD models *without* yet considering for the presence of spillovers at both the original (state) level in panel A, and the new (county) level in panel B.

Figure 2 displays the states which at any point pass different types of text-messaging bans during the period under study, as well as the distance to treatment from each (un-treated) county in the mainland US. These distances refer to the average distance from each county to the nearest treated state border.<sup>14</sup> We present treatment status as well as distance to treatment for each of the three types of text messaging bans considered in [Abouk and Adams \(2013\)](#): handheld bans in panel 2b, primarily-enforced bans in panel 2c, and secondarily-enforced bans in panel 2d, as well as the distance to any type of ban in panel 2a. The original list of states enacting test messaging bans along with the date of enactment, as well as the type of ban enacted can be found in Table 1 of [Abouk and Adams \(2013\)](#). The distance from each county displayed in Figure 2 is calculated in kilometres, ranging from 0 km (treated) to greater than 500 km. While it seems extremely unlikely that any impact of the bans would propagate even as much as 100 km from a treated state into nearby counties, we show distances up to 500 km to demonstrate that even if spillovers travelled for as much as 500 km, then there would still be additional untreated

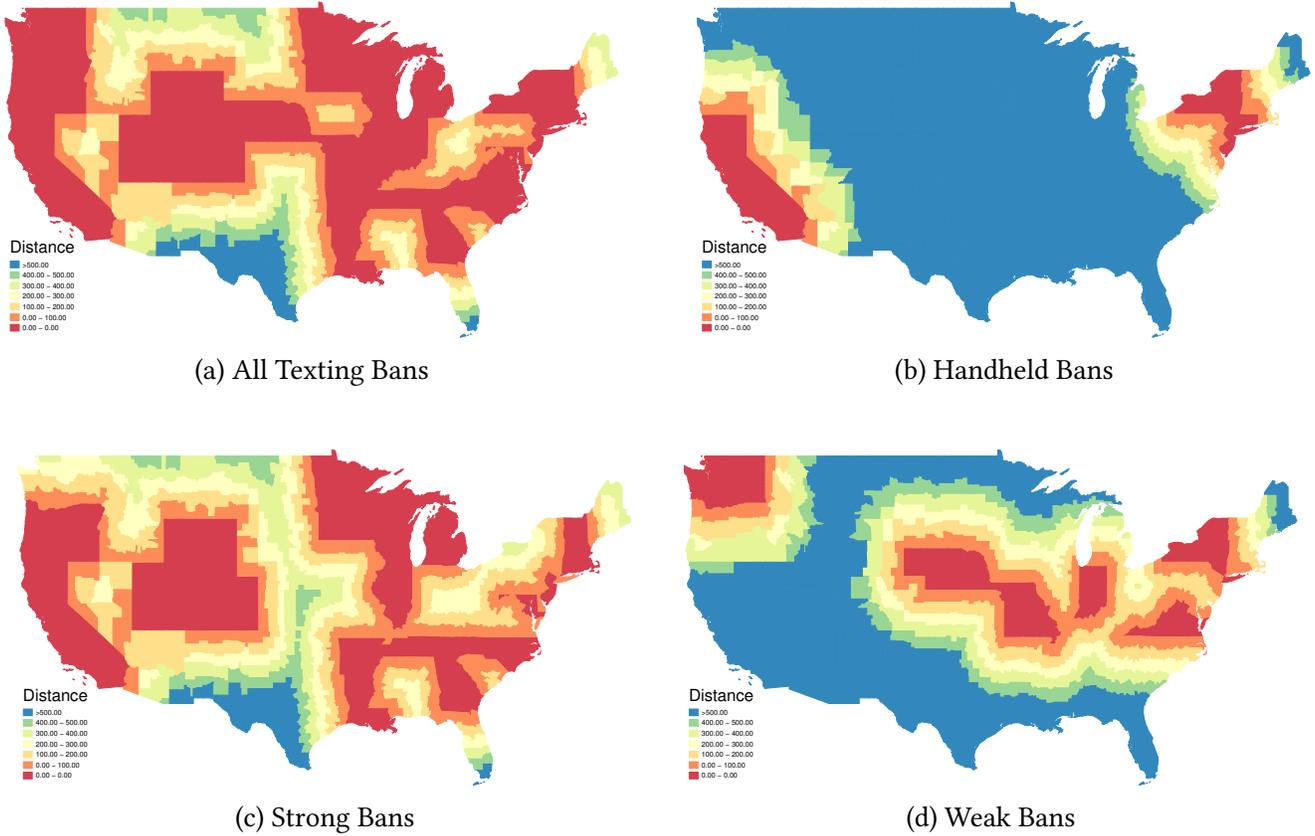
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<sup>13</sup>Classification as a primary offense allows for suspected drivers to be pulled over even if no other crime has been committed, while secondary offenses do not allow this. Further discussion is provided in [Abouk and Adams \(2013, pp. 183–184\)](#).

<sup>14</sup>In this case, the relevant distance is based on travelling over roads from the nearest treated state, and so closest state borders are used as a manner to capture distance from the nearest area where drivers will be directly affected by laws, rather than mid-point to mid-point distances, or alternative distance measures. In code distributed along with this paper, we also provide routines to find travel distance from point to point over roads, as well as average travel times in cars.

states for each case. This is a fundamental assumption for the spillover-robust DD method, where we require that SUTVA hold between at least some units.

Figure 2: Distances between Counties and Treatment States



NOTES TO FIGURE 2: Each panel displays distances of each county to the nearest treatment state once all bans have been enacted. States indicated in red are those treated in each case. Panel (a) displays distances to any types of bans, panel (b) displays cases with a universal concurrent hand-held ban, panel (c) displays only bans with primary enforcement, and panel (d) displays bans only with secondary enforcement. Distances are displayed by county, based on the distance from the centre of each county to the closest point on the border of the closest treated state.

We extend the results of [Abouk and Adams \(2013\)](#) to consider spillovers, in each case following precisely their variable measurement, controls, estimation sample, and probability weights, however at the county level, rather than the state level. Thus, the baseline DD specification for each type of ban is, following their equation 1:

$$Y_{im} = \alpha + \gamma_i + \delta_m + \mathbf{X}_{im}\beta + \omega B_{im} + \varepsilon_{im} \quad (22)$$

where  $Y_{im}$  refers to the log number of accidents + 1 for county  $i$  and month  $m$ ,  $\gamma$  a series of county fixed effects (for the 3,111 counties of the 49 states used in the original analysis), and  $\delta_m$  fixed effects for the 48 months of data. County by month cells are weighted by county population, and additional controls  $X_{im}$  follow the original analysis. In column 1 of Table 2 we present estimates of  $\omega$  from equation 22 with naive models assuming that no spillovers occur. These estimates are presented for each of the three

different types of bans discussed above, and suggest, as in [Abouk and Adams \(2013\)](#), that strong bans reduce the rate of fatalities occurring in SVSO accidents (in this case by approximately 6%), whereby weak bans appear to be worse than having no ban in place at all (a significant 7% increase in fatalities is observed following the introduction of weak bans). While [Abouk and Adams \(2013\)](#) focus largely on the results of strong bans, they do suggest that the introduction of weak bans may be a result of higher pre-ban rates of accidents. In the case of the introduction of handheld mobile telephone bans, baseline DD models do not find statistically significant reductions in accidents following their introduction, although the direction of the effect is negative, and the magnitude quite large, generally supporting [Abouk and Adams \(2013\)](#)'s finding of a negative effect of handheld bans.

In turning to spillovers, we implement the spillover robust DD methodology in the remaining columns of Table 2. In this table the distance bins over which spillovers are estimated is arbitrarily set at  $h = 10$ . This first approximation allows us to examine spillovers in a general way, before we turn to the non-arbitrary optimising formula in Table 3. In this first take, we *do* observe that spillovers are a relevant phenomenon in this case, and that spillovers are always of the same direction as the reform itself. This suggests that if drivers do alter behaviour in response to reforms, these behavioural changes are not immediately undone at state borders, but rather, seem to perdure in neighbouring and nearby counties.

In the case of weak bans and handheld bans, significant impacts of the text messaging bans are observed in neighbouring areas, while for strong bans, we do not find evidence to suggest that spillovers occur, at least when bandwidths are set at 10km. In general there is evidence that when these spillovers exist, they fade out over a relatively short distance. For the case of weak bans, effects appear to be significant from [0-10), [10,20), and [20,30) km, but then no longer remain distinguishable between [30,40) km, while in the case of handheld bans, spillovers are observed up to 10km, but then not after, with the exception of one coefficient above 30km. In each case, when spillovers are observed in nearby areas, we observe that controlling for these spillovers increases the magnitude of the treatment effect itself, as is demonstrated in the calculation of the bias in treatment effects derived in equation 18. Considering spillovers for both weak and handheld bans increases the magnitude of point estimates, however in no case are the estimated differences in treatment effects themselves statistically distinguishable across models. This points to an important result in the derived bias of spillover effects. Where spillovers occur only very locally, even if the effect of spillovers is large, given that spillovers only reach a very small pool of the full original control group, these do not result in large swings in the original treatment effects estimator. Nonetheless, in both weak and handheld bans, naive models do fail to identify significant treatment effects on the close to treated—a relevant consideration when determining the total effect of a policy.

While setting the bandwidth at 10 km does suggest the spillovers are present, an advantage of the

Table 2: Reform and Spillover Effects: Bins in 10km

	(1)	(2)	(3)	(4)	(5)
<b>Panel A: Strong Ban</b>					
Strong Ban	-0.062** [0.027]	-0.062** [0.027]	-0.061** [0.027]	-0.062** [0.026]	-0.062** [0.026]
Close to strong ban [0-10)km		-0.004 [0.048]	-0.003 [0.049]	-0.005 [0.050]	-0.005 [0.051]
Close to strong ban [10-20)km			0.024 [0.036]	0.024 [0.037]	0.023 [0.037]
Close to strong ban [20-30)km				-0.015 [0.032]	-0.016 [0.033]
Close to strong ban [30-40)km					-0.026 [0.049]
<b>Panel B: Weak Ban</b>					
Weak Ban	0.073*** [0.019]	0.073*** [0.019]	0.075*** [0.019]	0.076*** [0.019]	0.075*** [0.020]
Close to weak ban [0-10)km		0.071*** [0.017]	0.072*** [0.017]	0.073*** [0.017]	0.073*** [0.018]
Close to weak ban [10-20)km			0.047* [0.024]	0.048* [0.024]	0.047* [0.025]
Close to weak ban [20-30)km				0.061** [0.025]	0.060** [0.026]
Close to weak ban [30-40)km					-0.033 [0.070]
<b>Panel C: Handheld Ban</b>					
Handheld Ban	-0.073 [0.048]	-0.076 [0.047]	-0.076 [0.047]	-0.075 [0.048]	-0.075 [0.048]
Close to handheld ban [0-10)km		-0.094** [0.044]	-0.094** [0.044]	-0.093** [0.045]	-0.093** [0.046]
Close to handheld ban [10-20)km			0.001 [0.038]	0.002 [0.039]	0.001 [0.039]
Close to handheld ban [20-30)km				0.030 [0.024]	0.029 [0.024]
Close to handheld ban [30-40)km					-0.099* [0.058]
Observations	149328	149328	149328	149328	149328

NOTES: Each panel presents a separate difference-in-differences model with progressive controls capturing time-varying distance to treatment. Distances are arbitrarily split into bands of 10km. Controls follow the specifications described in [Abouk and Adams \(2013\)](#) exactly. Standard errors are clustered by state, and observations are weighted by county population. Each specification includes county by state fixed effects and month by year fixed effects. The dependent variable is the natural logarithm of fatal accidents + 1.

Table 3: Spillover Robust Difference-in-Difference Estimates

	(1) Strong Ban	(2) Weak Ban	(3) Handheld Ban
Treated (Strongly enforced ban)	-0.064** [0.027]		
Treated (Weakly enforced ban)		0.076*** [0.018]	
Close to Treated [0-30) km from Weak ban		0.054*** [0.018]	
Treated (Handheld ban)			-0.077 [0.048]
Close to Treated [0-6) km from Handheld ban			-0.111** [0.045]
Close to Treated [6-12) km from Handheld ban			-0.053* [0.029]
Observations	149,328	149,328	149,328
R-Squared	0.636	0.636	0.636
Optimal Bandwidth (h)	—	30.00	6.00
Maximum Spillover	—	30.00	12.00
RMSE CV(h)	0.585	0.628	0.465

NOTES: Each column presents a single spillover-robust difference-in-difference model. Optimal models are based on minimising the RMSE criterion, with the optimal cross-validated RMSE displayed at the foot of the table. Spillover bins ( $h$ ) are searched ranging from 2km to 40km, based on average distances from counties to the nearest treated state border. Optimal bandwidth, and maximum spillover distances in optimal models are displayed in the table footer.

spillover robust DD methodology proposed in this paper is that it does not actually rely on arbitrary decisions regarding searches. In table 3 we apply the proposed estimator to the case of text messaging bans, to determine the RMSE-optimal distance bandwidth, and examine for the presence of spillovers in this case. In order to estimate these models we use 10-fold cross validation rather than leave-one-out cross validation, given that LOOCV is computationally demanding with the large number of observations and models estimated. We present the three estimated models in columns 1-3 (strong, weak and handheld bans), and display estimated spillover impacts where they are identified using the proposed modeling strategy. As in Table 2, the spillover-robust DD estimates suggest no significant treatment spillovers for strong bans, finding that the original DD model is RMSE optimal (each RMSE minimising

bandwidth, as well as the RMSE itself is displayed at the foot of the table). For weak bans and handheld bans, optimal bandwidths are determined to be 30 km and 6 km respectively. When using a bandwidth of 30 km for weak bans we observe a single close to treated group in which spillovers are estimated to be significant, suggesting that the reform impacts propagated up to 30km from treatment. In the case of handheld bans, the 6 km bandwidth results in two perceptible spillover bins with estimated impacts of decreasing magnitude, suggesting that the impact of reforms may spread up to 12 km from the originally treated states. While the point estimate on handheld bans increases in magnitude and moves in the direction of becoming statistically significant ( $p$ -value  $\approx 0.11$ ), it does not become significant at frequently used significance levels. In considering only the point estimates on weak bans and handheld bans, the estimated impact of the reform increases by 4.1 and 5.5% respectively, however, as discussed above, given that spillovers occur to a relatively small proportion of the total original control group, these differences are not statistically distinguishable between models.

## 5 Conclusion

Echoing [Bertrand et al. \(2004\)](#), “Differences-in-Differences (DD) estimation has become an increasingly popular way to estimate causal relationships”. It is important to consider the assumptions underlying these estimators. There is now a broad literature examining assumptions relating to inference which can account for the serial correlation in outcomes over time in DD settings ([Bertrand et al., 2004](#); [Cameron et al., 2008](#); [Cameron and Miller, 2015](#)) as well as a necessary (but not sufficient) test for the validity of parallel trend assumptions ([Borusyak and Jaravel, 2016](#)), and manners to proceed when these assumptions are not met ([Abadie et al., 2010](#); [Doudchenko and Imbens, 2016](#))

In this paper we turn to examine the performance of DD estimates when the stable unit treatment value assumption does not hold locally. We document, firstly, how to determine if SUTVA is violated by local spillovers (which depend monotonically on distance to treatment), and secondly provide a convenient closed-form expression for determining the bias in models where treatment spillovers do occur. We finally derive a set of conditions by which DD estimates can produce unbiased estimates even in the absence of the SUTVA holding between all units. It is shown that under a weaker set of conditions, both the average effect on the treated and the average effect on the ‘close to treated’ can be estimated in a DD-type framework.

The frequency of papers in the economics literature estimating difference-in-difference models where treatment access varies within countries suggests that local spillovers may often be a relevant consideration. Such a situation may be particularly common in estimates of the causal effect of policy where compliance is imperfect. If policies entail a benefit to recipients, and if recipients living ‘close

to' treatment areas who are themselves untreated can somehow cross regional boundaries to receive treatment, we may be concerned that, locally at least, SUTVA is violated.

In this paper we examine a particular empirical example where geographic dependence of treatment seems likely. Focusing on text messaging bans, it is suggested that if driving behaviour is altered in the presence of state-level laws, it may not immediately revert when crossing into contiguous states (and counties) where no bans are in place. We observe that such a behavioural response to laws occurs. In particular, we observe that behavioural changes resulting from laws seem to endure, rather than observe that text messaging is simply delayed until traveling on roads in nearby untreated areas. These estimates suggest that both consideration of average treatment effects on the treated, as well as average treated effects on the close to treated, are relevant when determining the impacts of local policies. What's more, while we observe that correcting for spillovers does increase the magnitude of estimated average treatment effects on the treated, in the case examined these differences are not large enough to be statistically distinguishable from baseline estimates, in line with the intuition behind the nature of biases in estimates in DD models. I show, both formally in derivations of the omitted variable bias, and in a series of Monte Carlo simulations, that failure to account for spillovers in DD models becomes increasingly expensive in terms of the performance of the estimator for two reasons. Firstly, estimates suffer more when the impact of receiving spillover access to a policy on the outcome of interest is larger, and, secondly as spillover effects propagate to a larger portion of the untreated population.

This paper provides an optimising and deterministic way to estimate treatment effects as well as spillover effects in DD models. The proposed estimator does not require any ad-hoc or arbitrary specification decisions to be made by researchers. These tests are easy to run, and indeed a software package that automates this methodology in various languages is released with this paper. Given the nature of the assumptions underlying identification in many DD models in the applied econometric literature, tests of this nature should be included in a basic suite of falsification tests even if spillovers are not likely to occur, though will clearly be most illustrative in policy analysis when treatment spillover is a relevant phenomenon. While the example in this paper is illustrated using geographic spillovers over roads, spillover-robust DD estimation is certainly not limited to only geographic cases, also occurring in other family or social networks, such as contagion between peers in school, access to information in a network, ideological distance to different media sources, and so forth. How (and whether) treatment travels between units should be of fundamental concern to many applications in the economic and policy evaluation literature.

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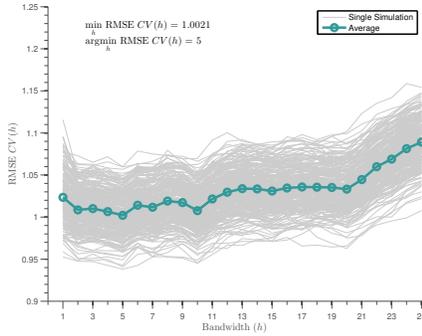
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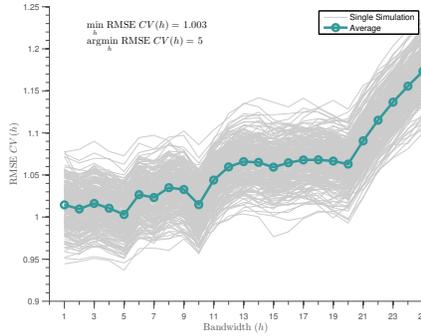
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# Appendix Figures and Tables

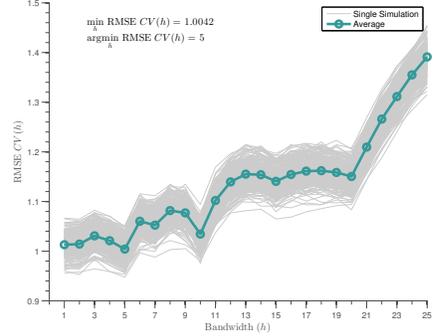
Figure A1: Root Mean Squared Error and Bandwidth Search Varying Degree of Spillovers



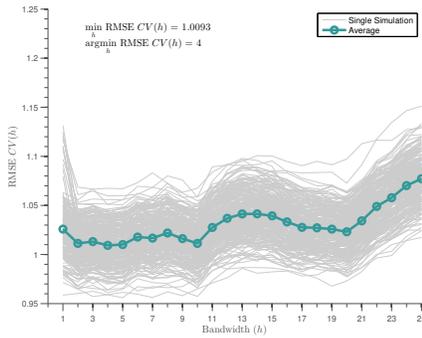
(a) DGP 1 (5% Spillovers)



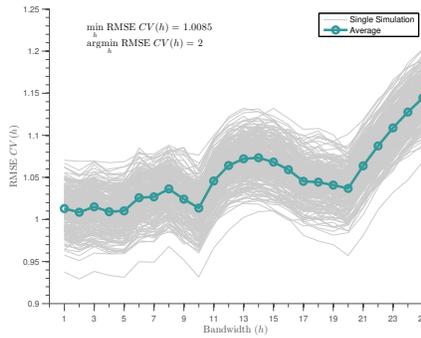
(b) DGP 1 (10% Spillovers)



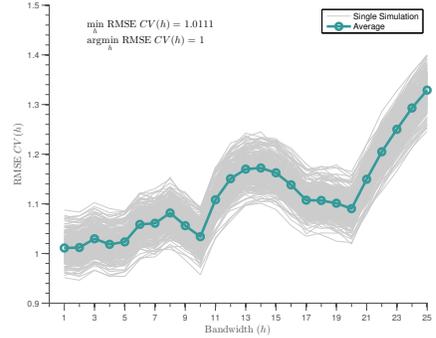
(c) DGP 1 (25% Spillovers)



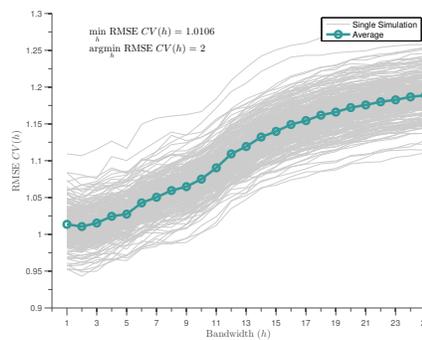
(d) DGP 2 (5% Spillovers)



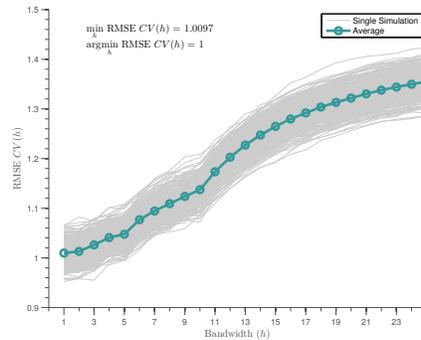
(e) DGP 2 (10% Spillovers)



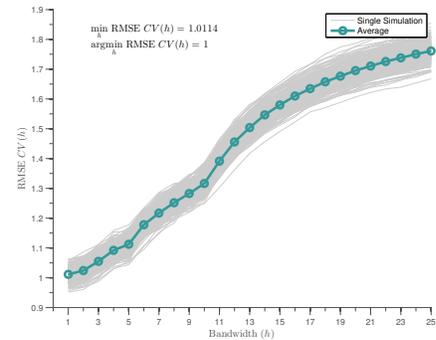
(f) DGP 2 (25% Spillovers)



(g) DGP 3 (5% Spillovers)



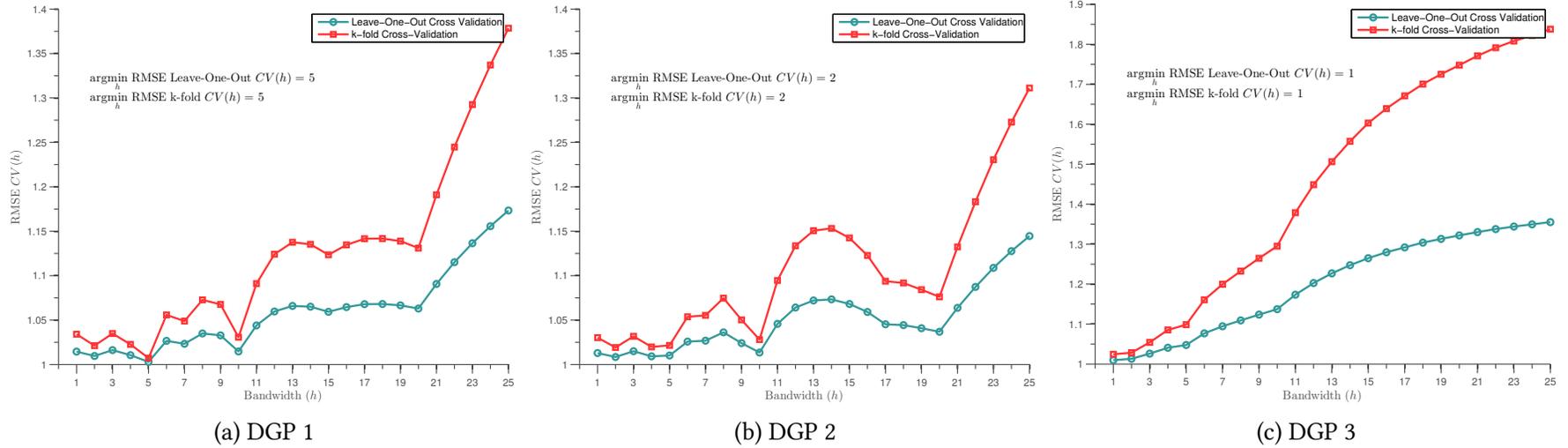
(h) DGP 3 (10% Spillovers)



(i) DGP 3 (25% Spillovers)

NOTES TO FIGURE A1: Refer to Figure 1 for full notes. Here we provide full RMSE plots for a range of degree of spillovers. The three figures in the left-hand column are based on 5% of the sample being affected by spillovers (for each of the three DGPs examined), the middle column are based on 10% spillovers (reproduced for comparison from Figure 1), and the right-hand panel are based on 25% of the sample being impacted by spillovers.

Figure A2: Root Mean Squared Error Criterion with  $k$ -fold versus Leave-One-Out Cross-Validation



NOTES TO FIGURE A2: Root Mean Squared Error (RMSE) by spillover bandwidth is plotted for Leave-One-Out (LOO) cross-validation (line with circles) and  $k$ -fold cross-validation (line with squares). The  $k$ -fold cross validation is calculated using 10 folds, where folds are stratified by spillover bins, to allow approximate balance of spillover variables in each of the  $k$  folds. The optimal bandwidth distance ( $h^*$ ) for each of the LOOCV and  $k$ -fold CV procedure are displayed in each panel of the figure, and DGPs are exactly as shown in identical panels of Figure 1.

Table A1: Monte Carlo Simulation: Optimal Bandwidth and Average Treatment Effects using k-fold Cross-Validation

	Spillover = 5%				Spillover = 10%				Spillover = 25%			
	$\bar{h}^*$	$\widehat{\beta}$	Std. Dev.	Size	$\bar{h}^*$	$\widehat{\beta}$	Std. Dev.	Size	$\bar{h}^*$	$\widehat{\beta}$	Std. Dev.	Size
<b>Model 1</b>												
Naive	—	9.782	0.235	0.132	—	9.565	0.234	0.402	—	8.902	0.245	0.990
$\varepsilon = 1$	5.099	9.998	0.080	0.050	4.810	9.999	0.081	0.049	4.902	9.999	0.085	0.056
$\varepsilon = 2$	8.064	10.001	0.161	0.050	6.082	10.000	0.159	0.048	4.936	10.002	0.166	0.050
$\varepsilon = 5$	14.341	9.996	0.386	0.041	12.561	9.979	0.405	0.056	8.688	10.003	0.425	0.046
<b>Model 2</b>												
Naive	—	9.795	0.243	0.133	—	9.574	0.243	0.381	—	8.930	0.239	0.988
$\varepsilon = 1$	5.176	9.998	0.079	0.052	3.488	9.998	0.080	0.054	1.664	9.997	0.082	0.052
$\varepsilon = 2$	9.886	9.997	0.157	0.047	7.374	10.001	0.160	0.051	4.409	10.003	0.167	0.049
$\varepsilon = 5$	15.475	9.985	0.405	0.057	14.298	10.004	0.403	0.054	11.062	10.009	0.436	0.060
<b>Model 3</b>												
Naive	—	9.875	0.232	0.071	—	9.732	0.240	0.176	—	9.333	0.231	0.730
$\varepsilon = 1$	2.072	9.998	0.079	0.046	1.379	10.002	0.079	0.052	1.020	9.999	0.081	0.053
$\varepsilon = 2$	3.257	9.993	0.158	0.052	2.317	9.991	0.159	0.050	1.584	9.998	0.165	0.054
$\varepsilon = 5$	6.255	9.992	0.398	0.050	4.776	9.985	0.401	0.046	3.303	9.957	0.413	0.055

NOTES: Refer to full notes in Table 1. This results in this table are generated following an identical procedure, however using  $k$ -fold stratified cross-validation, instead of Leave-One-Out cross-validation. In all cases, 10 folds are used for cross-validation.

Table A2: Summary Statistics: State and Municipal-Level

	Control Areas	Treatment Areas		
		All months	Pre-ban	Post-ban
<b>Panel A: State-level Data</b>				
Number of single-vehicle single-occupant accidents	16.84	16.13	16.12	16.16
Population (annual)	5,157,694	7,064,738.4	6,614,487.1	8,066,043.5
Unemployment rate (monthly)	6.51	6.83	6.01	8.63
Proportion male (monthly)	49.32	49.34	49.37	49.26
Real gas tax in 1983 cents (monthly)	19.94	20.57	20.50	20.73
Sample size	1,056	1,296	894	402
<b>Panel B: County-level Data</b>				
Number of single-vehicle single-occupant accidents	0.2583	0.2597	0.2594	0.2601
Population (annual)	79,198.5	113,713	106,437	129,901
Unemployment rate (monthly)	6.49	6.99	6.19	8.76
Proportion male (monthly)	49.33	49.33	49.39	49.21
Real gas tax in 1983 cents (monthly)	19.43	20.27	20.13	20.58
Sample size	68,832	80,496	55,533	24,963

NOTES: Treatment and control states follow classifications provided in [Abouk and Adams \(2013\)](#). Panel A presents original state-level data used in difference-in-differences estimates presented in [Abouk and Adams \(2013\)](#). Panel B presents county-level data used in spillover robust diff-in-diff estimates examined in this paper. All variables follow precisely the definitions from original analysis.

Figure A3: Geographical Distribution of Single Vehicle Single Occupant Accidents

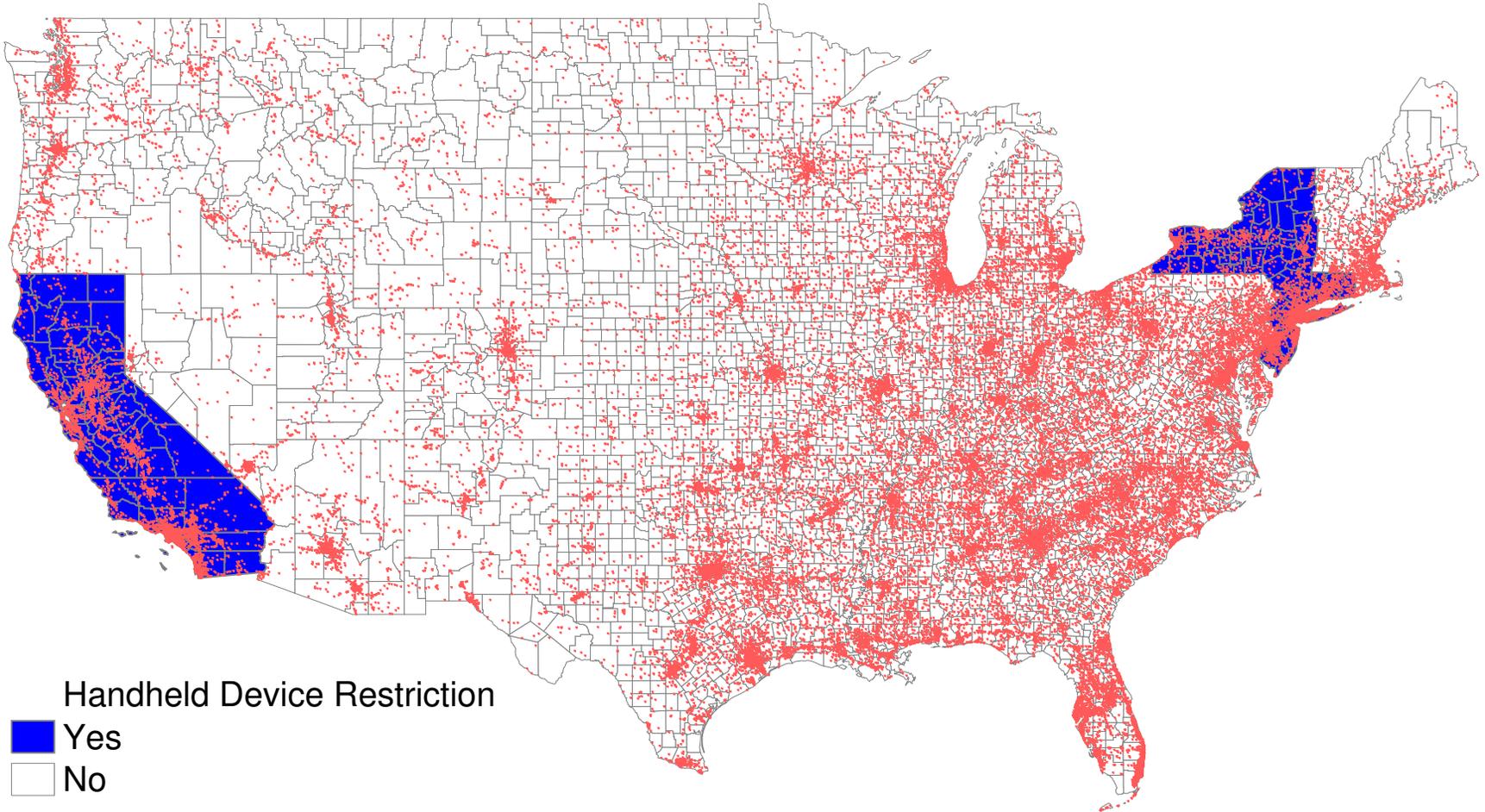


Table A3: Determinants of Fatal, Single-Vehicle, Single-Occupant Crashes (Municipal data)

	(1)	(2)	(3)	(4)	(5)
Texting ban in place	-0.0229 (0.0283)				
× universally applied, primarily enforced		-0.0525 (0.0274)	-0.0519 (0.0273)	-0.0471 (0.0296)	-0.0321 (0.0242)
× limited coverage/enforcement		0.0541*** (0.0128)	0.0538*** (0.0130)	0.0264 (0.0249)	0.0634** (0.0203)
Log of population	0.4603 (0.5645)	0.5280 (0.4889)	0.5267 (0.4880)	-0.9411 (0.8854)	0.6215 (0.7593)
Log of unemployment	-0.1787* (0.0716)	-0.1669* (0.0708)	-0.1623* (0.0697)	-0.0916* (0.0401)	0.0636 (0.0851)
Percent male	-0.0059 (0.0191)	-0.0093 (0.0109)	-0.0092 (0.0109)	0.0123 (0.0155)	-0.0147 (0.0155)
Log of gas tax	-0.1169 (0.0769)	-0.1044 (0.0648)	-0.1023 (0.0641)	-0.0681 (0.0678)	-0.0137 (0.0556)
Other accidents			0.0190* (0.0080)	0.0231* (0.0086)	0.0163* (0.0079)
Including 48 month fixed effects	Yes	Yes	Yes	No	Yes
Including differential monthly trend for all states	No	No	No	Yes	Yes

NOTES TO TABLE A3: Coefficients are reported from weighted least squares regressions, where weights are based on county population for 3,111 counties over 48 months (following original specifications, Alaska and Hawaii are not included). The dependent variable is the natural logarithm of fatal accidents + 1. All specifications include county by state fixed effects, and standard errors are clustered by state. Specifications follow precisely those of [Abouk and Adams \(2013\)](#), however with county-level data in place of state-level data. \*\*\* Significant at the 1 percent level. \*\* Significant at the 5 percent level. \* Significant at the 10 percent level.

Table A4: State and Municipal-Level Baseline Difference-in-Difference Model

	(1)	(2)	(3)
<b>Panel A: State-level Estimates</b>			
Strong Ban	-0.090*** [0.028]		
Weak Ban		0.103** [0.043]	
Handheld Ban			-0.048 [0.061]
Observations	2352	2352	2352
R-Squared	0.902	0.902	0.902
<b>Panel B: County-level Estimates</b>			
Strong Ban	-0.062** [0.027]		
Weak Ban		0.073*** [0.019]	
Handheld Ban			-0.073 [0.048]
Observations	149328	149328	149328
R-Squared	0.636	0.636	0.636

NOTES: Baseline difference-in-difference models without spillover estimates are presented. Panel A presents state-level models following [Abouk and Adams \(2013\)](#). Panel B presents identical specifications however at the county level, and weighted by county, rather than by states. Both models cluster standard errors by state, to allow arbitrary correlations among counties within states across time and across space.

## A Alternative Methodologies

The present paper is interested in quantifying the impacts of some externally defined policy of interest, on individuals in treated areas, and on individuals living in areas close to treatment. The definition of the policy itself is assumed to not be under the direct control of the treated and untreated individuals themselves. The setting of this paper is thus different to analyses where externalities occur due to coordination of individual behaviour, and spillovers from social interactions, such as conformation to reference group behaviour (for example, as in [Brock and Durlauf \(2001\)](#); [Bénabou \(1993\)](#)). Considerably extended summaries of the state of the literature of spillover effects and treatment interference are available in a number of review papers, including [Tchetgen Tchetgen and VanderWeele \(2010\)](#); [Angelucci and Maro \(2010\)](#); [Blume et al. \(2011\)](#). Work of [Rosenbaum \(2007\)](#) discusses a number of cases in which interference between units is observed, and derives non-parametric methods for estimation in cases in which no information regarding the degree of interference is known.

**Generalised Methods** The work of [Manski \(2013\)](#); [Aronow and Samii \(forthcoming\)](#) provide the most general considerations of social interactions in econometric models. Both of these provide models allowing for arbitrary forms of interdependence in treatment assignment between individuals, and document the identification requirements on treatment effects. When considering the nature of spillovers, [Aronow and Samii \(forthcoming\)](#) refer to an “exposure mapping”, which describes individual treatments and the propagation of treatment, while [Manski \(2013\)](#) refers to “effective treatment”, to describe the same process. Importantly, neither model requires that spillovers are limited to an individual’s reference group, however such a circumstance is nested in their models. In this paper, I also do not require spillovers to be isolated to an individual’s reference group, meaning that the method proposed can be couched in both [Manski \(2013\)](#) or [Aronow and Samii \(forthcoming\)](#)’s terms. The difference between this paper and the work of [Manski \(2013\)](#); [Aronow and Samii \(forthcoming\)](#) is that I focus on a particular type of spillovers, fully specify the manner in which interference occurs, and provide a systematic way to estimate the degree of interference. This can thus be considered an estimable application of these theories to capture geographic spillovers in a difference-in-difference setting.

In [Manski \(2013\)](#)’s terms, the model laid out here allows for monotone metric interactions, which can either be reinforcing (in the same direction as receipt of treatment) or opposing (in the opposite direction of receipt of treatment). This is generally known as a Semi-Monotone Treatment Response model in [Manski \(2013\)](#). The spillover-robust DD model defined here lays out restrictions to the shape of the “response function” (the degree to which treatment propagates over space), without distributional assumption or direction assumptions on this response function. In [Aronow and Samii \(forthcoming\)](#)’s terms, the spillover-robust DD model proposes a non-parametric “exposure mapping”

determining the treatment (and close to treatment) assignment based on any original treatment assignment. While [Aronow and Samii \(forthcoming\)](#) propose an exposure mapping which is based on a generalised propensity score for the likelihood of exposure with restrictions related to maximal propagation based on sample size (their “Local dependence” condition), the exposure mapping described in the spillover robust DD model proposes an exposure mapping based on distance to treatment (and potential interactions).

**Within but not between groups** An alternative series of papers focus on cases where interference is allowed within, but not between groups. A series of important theoretical results in this case are given by [Hudgens and Halloran \(2008\)](#); [Liu and Hudgens \(2014\)](#). These strategies have a number of names, such as “partial interference” ([Sobel, 2006](#)), “hierarchical treatment assignment”, or “randomised saturation” designs ([Baird et al., forthcoming](#)). The latter case is frequently employed in experimental applications where treatment is randomised first at the level of a cluster, and then to individuals within the cluster, and individuals within each cluster are permitted to interact freely (consider for example randomised treatment at the level of schools, such as in [Miguel and Kremer \(2004\)](#), local labour markets, such as in [Crépon et al. \(2013\)](#) or localities, such as in PROGRESA). A comprehensive discussion of randomised saturation designs and the resulting estimands is provided by [Baird et al. \(forthcoming\)](#). The methods discussed in this paper differ fundamentally from these hierarchical or partial interference designs in that spillovers are assumed to occur *between* treatment clusters.

**Applications** Finally, it is important to note that a number of applied studies estimate difference-in-difference models in which spillovers are posited (and identified). These include [Miguel and Kremer \(2004\)](#)’s study on externalities between schools, [Almond et al. \(2009\)](#)’s examination of the spread of radioactive fallout during a mother’s pregnancy on schooling outcomes, and [Allcott and Keniston \(forthcoming\)](#) who consider economic spillovers of local natural resource booms. In each case, these applied papers propose a structurally similar process to the one I lay out here. The main difference with these and the present study is that here we (a) document that such an estimation methodology is unbiased, and (b) provide a non ad-hoc way to choose distance bins to search for spillovers. In the past, these were chosen by researchers, likely in a way to best capture spillovers or explain modelled outcomes, but without a formal optimising method. Applications such as these are precisely the sorts of cases in which spillover-robust DD is likely to be most useful going forward.

## B Proofs

*Proof of Proposition 1.*  $Y(i, t)$  is generated according to (1), and from (7), a regression of  $Y(i, t)$  on  $D(i, t)$  and  $R(i, t)$  can be estimated. It is assumed that we have at a representative sample of size  $N$  consisting of  $\{Y(i, t), D(i, t), X(i, t) : i = 1, \dots, N, t = 0, 1\}$ . By assumption 4A, the assignment rule  $\delta$  forms  $R(i, t)$  allowing for the estimation of (7). By definition,  $\alpha$  in this regression is equal to:

$$\begin{aligned} \alpha &= \{E[Y(i, 1)|D(i, 1) = 1, R(i, 1) = 0] - E[Y(i, 1)|D(i, 1) = 0, R(i, 1) = 0]\} \\ &\quad - \{E[Y(i, 0)|D(i, 1) = 1, R(i, 1) = 0] - E[Y(i, 0)|D(i, 1) = 0, R(i, 1) = 0]\}, \end{aligned}$$

and from assumption 3, each of the expectation terms exists, as there are both fully treated and completely untreated units. Using the potential outcomes framework, we are free to re-write the above expression as:

$$\begin{aligned} \alpha &= \{E[Y^1(i, 1)|D(i, 1) = 1, R(i, 1) = 0] - E[Y^0(i, 1)|D(i, 1) = 0, R(i, 1) = 0]\} \\ &\quad - \{E[Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0] - E[Y^0(i, 0)|D(i, 1) = 0, R(i, 1) = 0]\}, \end{aligned}$$

given that only in the case where  $t = 1$  and  $D(i, 1) = 1$  we observe the potential outcome where the individual receives treatment:  $Y^1(i)$ . Using the linearity of the expectations operator, this can finally be re-written as:

$$\alpha = E[Y^1(i, 1) - Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0] - E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 0, R(i, 1) = 0].$$

Now, from assumption 1, we can appeal to parallel trends, and replace the second expectation term in the above expression with  $E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0]$ :

$$\alpha = E[Y^1(i, 1) - Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0] - E[Y^0(i, 1) - Y^0(i, 0)|D(i, 1) = 1, R(i, 1) = 0].$$

Expanding the expectations operator and cancelling out the second term in each of the above items gives:

$$\alpha = E[Y^1(i, 1)|D(i, 1) = 1, R(i, 1) = 0] - E[Y^0(i, 1)|D(i, 1) = 1, R(i, 1) = 0].$$

which finally, once again by the linearity of expectations, can be combined to give  $\alpha = E[Y^1(i, 1) - Y^0(i, 1)|D(i, 1) = 1, R(i, 1) = 0]$ , which can be rewritten as  $\alpha = E[Y^1(i, 1) - Y^0(i, 1)|D(i, 1) = 1]$  given that  $D(i, 1) = 1 \implies R(i, 1) = 0$ . Combining (8) and  $\alpha = E[Y^1(i, 1) - Y^0(i, 1)|D(i, 1) = 1]$  we thus have that  $\alpha = ATT$  as required.

Turning to the ATC, the same set of steps can be followed for  $\beta$  on the coefficient  $R(i, t)$ , however now instead of assumption 1 we must rely on parallel-trend assumption 2. This leads to  $\beta = E[Y^1(i, 1) - Y^0(i, 1)|R(i, 1) = 0]$ , and from (9) and the previous expression it holds that that  $\beta = ATC$ .  $\square$

*Proof of Proposition 2.* With the representative sample  $\{Y(i, t), D(i, t), X(i, t) : i = 1, \dots, N, t = 0, 1\}$ , assumption 4B implies that  $X(i, t)$  can be  $R(i, t)$  using assignment rule  $\delta$ . The remainder of the proof follows the same steps as the proof for proposition 1.  $\square$

**Derivation of Conditional Correlations for Bias Formula** Consider a sample of size  $N$ , consisting of  $\{Y(i, t), D(i, 1), R(i, 1), D(i, t), R(i, t) : i = 1, \dots, N, t = 0, 1\}$ . Define the number of observations for whom  $D(i, 1) = 1$  as  $N_D$ , and analogously define  $N_R$  for those observations for whom  $R(i, 1) = 1$ . As  $D(i, 1)$  and  $R(i, 1)$  are mutually exclusive, no observations can be counted in both  $N_D$  and  $N_R$ . Similarly, define  $N_{D_T}$  and  $N_{R_T}$  for the number of observations for whom  $D(i, t) = 1$  and  $R(i, t) = 1$ , where once again these are mutually exclusive, and mutually exclusive with each of  $D(i, 1) = 1$  and  $R(i, 1) = 1$ . Finally, define as  $N_T$  the quantity of observations in which  $t = 1$ . By definition, all observations for which  $R(i, t) = 1$  and  $D(i, t) = 1$  will be counted in  $N_T$ , but the inverse is not true (ie there are observations counted in  $N_T$  who have neither  $R(i, t) = 1$  nor  $D(i, t) = 1$ ).

In order to determine the bias outlined in the paper, we are interested in the correlation between  $D(i, t)$  and  $R(i, t)$ , conditional on  $D(i, 1)$ ,  $t$  and a constant term. The omission of a control for the ‘‘close to treated’’ indicator  $R(i, t)$  also implies that a similar binary variable  $R(i, 1)$  is omitted from the model. The well-known omitted variable bias formula gives that:

$$E[\hat{\alpha}|\mathbf{X}] = \alpha + (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{R}\beta_R \quad (\text{A1})$$

where the second term gives the bias in estimated parameters,  $\mathbf{R}$  refers to the omitted  $\{R(i, 1), R(i, t)\}$  indicators, and  $\beta_R$  is the direct effect of these omitted factors on the outcome variable of interest. The matrix  $\mathbf{X}$  is an  $N \times 4$  matrix, with  $\mathbf{X} = [1 \ D(i, 1) \ D(i, t) \ t]$ .

Given that  $\mathbf{X}$  is a Boolean matrix, and various columns are mutually exclusive, we can write  $\mathbf{X}'\mathbf{X}$  and  $\mathbf{X}'\mathbf{R}$  as:

$$(\mathbf{X}'\mathbf{X}) = \begin{bmatrix} N & N_D & N_{D_T} & N_T \\ N_D & N_D & 0 & 0 \\ N_{D_T} & 0 & N_{D_T} & N_{D_T} \\ N_T & 0 & N_{D_T} & N_T \end{bmatrix} \quad \mathbf{X}'\mathbf{R} = \mathbf{X}'[R(i, 1) \ R(i, t)] = \begin{bmatrix} N_R & N_{R_T} \\ 0 & 0 \\ 0 & 0 \\ 0 & N_{R_T} \end{bmatrix}.$$

In the  $\mathbf{X}'\mathbf{R}$  matrix above, only the constant term in  $\mathbf{X}$  takes values of one when  $R(i, 1) = 1$ , hence the first column only has one non-zero entry, while both the constant and the time dummy  $t$  in  $\mathbf{X}$  take values of 1 when  $R(i, t) = 1$ . Given that all occurrences of  $R(i, t) = 1$  are when  $t = 1$ , both entries in the second column of  $\mathbf{X}'\mathbf{R}$  are the sum of all observations for which  $R(i, t) = 1$ .

In order to calculate the omitted variable bias,  $(\mathbf{X}'\mathbf{X})$  must be inverted. This matrix is a symmetric block matrix, and so can be re-written as:

$$A = \begin{bmatrix} A_{11} & A_{12} \\ A'_{12} & A_{22} \end{bmatrix} \quad A^{-1} = \begin{bmatrix} B_{11} & B_{12} \\ B'_{12} & B_{22} \end{bmatrix}$$

where each of  $A_{11}$ ,  $A_{12}$  and  $A_{22}$  are the  $2 \times 2$  matrices in each corner of  $(\mathbf{X}'\mathbf{X})$ . Each element of the inverse has the formula given below (see for example [Lu and Shiou \(2002\)](#)):

$$\begin{aligned} B_{11} &= (A_{11} - A_{12}A_{22}^{-1}A'_{12})^{-1} \\ &= \frac{1}{N_{D_1}(N - N_T - N_{D_1})} \begin{bmatrix} N_D & -N_D \\ -N_D & N - N_T \end{bmatrix} \end{aligned} \quad (\text{A2})$$

$$\begin{aligned} B_{22} &= (A_{22} - A'_{12}A_{11}^{-1}A_{12})^{-1} \\ &= \frac{1}{(N_T - N_{D_T})} \begin{bmatrix} \frac{N_T}{N_{D_T}} & -1 \\ -1 & \frac{N - N_D - N_{D_T}}{(N - N_D - N_T)} \end{bmatrix} \end{aligned} \quad (\text{A3})$$

$$\begin{aligned}
B_{12} &= -A_{22}^{-1}A'_{12}(A_{11} - A_{12}A_{22}^{-1}A'_{12})^{-1} \\
&= \begin{bmatrix} 0 & 0 \\ -\frac{1}{N-N_T-N_D} & \frac{1}{N-N_T-N_D} \end{bmatrix}
\end{aligned} \tag{A4}$$

$$\begin{aligned}
B'_{12} &= -A_{11}^{-1}A_{12}(A_{22} - A'_{12}A_{11}^{-1}A_{12})^{-1} \\
&= \begin{bmatrix} 0 & -\frac{1}{(N-N_D-N_T)} \\ 0 & \frac{1}{(N-N_D-N_T)} \end{bmatrix}.
\end{aligned} \tag{A5}$$

The first line of each expression above is from the inverse formula for  $2 \times 2$  block matrices, while the second line for each sub-matrix is resolved by linear algebra.<sup>15</sup>

Based on the above, we combine A2-A5 to form  $(\mathbf{X}'\mathbf{X})^{-1}$ , which gives that the inverse is:

$$\begin{bmatrix} \frac{1}{N-N_T-N_D} & -\frac{1}{N-N_T-N_D} & 0 & 0 \\ -\frac{1}{N-N_T-N_D} & \frac{N-N_T}{N_D(N-N_T-N_D)} & -\frac{1}{N-N_T-N_D} & \frac{1}{N-N_T-N_D} \\ 0 & -\frac{1}{(N-N_D-N_T)} & \frac{N_T}{N_{D_T}(N_T-N_{D_T})} & \frac{-1}{N_T-N_{D_T}} \\ 0 & \frac{1}{(N-N_D-N_T)} & \frac{-1}{N_T-N_{D_T}} & \frac{N_T-N_{D_T}}{(N-N_D-N_T)(N_T-N_{D_T})} \end{bmatrix}. \tag{A6}$$

and, to complete the first part of the omitted variable bias, this is multiplied with  $(\mathbf{X}'R_T)$ . This gives  $(\mathbf{X}'\mathbf{X})^{-1}(\mathbf{X}'\mathbf{R})$ :

$$\begin{bmatrix} \frac{1}{N-N_T-N_D} & -\frac{1}{N-N_T-N_D} & 0 & 0 \\ -\frac{1}{N-N_T-N_D} & \frac{N-N_T}{N_D(N-N_T-N_D)} & -\frac{1}{N-N_T-N_D} & \frac{1}{N-N_T-N_D} \\ 0 & -\frac{1}{(N-N_D-N_T)} & \frac{N_T}{N_{D_T}(N_T-N_{D_T})} & \frac{-1}{N_T-N_{D_T}} \\ 0 & \frac{1}{(N-N_D-N_T)} & \frac{-1}{N_T-N_{D_T}} & \frac{N_T-N_{D_T}}{(N-N_D-N_T)(N_T-N_{D_T})} \end{bmatrix} \begin{bmatrix} N_R & N_{R_T} \\ 0 & 0 \\ 0 & 0 \\ 0 & N_{R_T} \end{bmatrix} = \begin{bmatrix} \frac{N_R}{N-N_T-N_D} & \frac{N_{R_T}}{N-N_T-N_D} \\ -\frac{N_R}{N-N_T-N_D} & 0 \\ 0 & \frac{-N_{R_T}}{N_T-N_{D_T}} \\ 0 & \frac{N_{R_T}(N-N_D-N_{D_T})}{(N-N_D-N_T)(N_T-N_{D_T})} \end{bmatrix}.$$

Here we are concerned with the bias on the parameter  $\hat{\alpha}$ , which corresponds to the third variable of the  $\mathbf{X}$  matrix:  $D(i, t)$ . Note from the first column of  $(\mathbf{X}'\mathbf{X})^{-1}(\mathbf{X}'\mathbf{R})$ , that  $D(i, t)$  has no correlation with  $R(i, 1)$  conditional on the included variables. It does, however, have a correlation with  $R(i, t)$ . This bias is then equal to the direct impact of the omitted variable  $R(i, t)$  on  $y$  ( $\beta$ ), multiplied by the conditional correlation:

$$\beta \times \frac{-N_{R_T}}{N_T - N_{D_T}}, \tag{A7}$$

giving

$$E[\alpha|\mathbf{X}] = \alpha - \beta \left( \frac{N_{R_T}}{N_T - N_{D_T}} \right) \tag{A8}$$

as stated in equation 15.

If, however, multiple “close to treatment” indicators (and corresponding baseline fixed effects) are included rather than

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<sup>15</sup>Extended calculations are available from the author by request.

a single  $R$  indicator, all holds as above, however now the  $\mathbf{X}'\mathbf{R}$  matrix becomes

$$\begin{bmatrix} N_{R_1} & \cdots & N_{R_J} & N_{R_{1T}} & \cdots & N_{R_{JT}} \\ 0 & \cdots & 0 & 0 & \cdots & 0 \\ 0 & \cdots & 0 & 0 & \cdots & 0 \\ 0 & \cdots & 0 & N_{R_{1T}} & \cdots & N_{R_{JT}} \end{bmatrix}$$

where  $R(i, 1)$  and  $R(i, t)$  have been further split into mutually exclusive  $R^1(i, 1), R^1(i, t), \dots, R^J(i, 1), R^J(i, t)$  distance indicators as in equation 10, and where analogous integers  $N_{R_1}, N_{R_{1T}}, \dots, N_{R_J}, N_{R_{JT}}$  refer to the number of observations which take one for a given binary indicator. In this case, the omitted variable bias for the parameter  $\alpha$  is given as:

$$E[\alpha|\mathbf{X}] = \alpha - \beta_1 \left( \frac{N_{R_{1T}}}{N_T - N_{D_T}} \right) - \cdots - \beta_J \left( \frac{N_{R_{JT}}}{N_T - N_{D_T}} \right), \quad (\text{A9})$$

as stated in equation 16.

Now, if we include an arbitrary number of close controls (and corresponding fixed effects) in the regression model of interest, forming

$$Y(i, t) = \mu + \tau D(i, 1) + \gamma_1 R^1(i, 1) + \cdots + \gamma_J R^J(i, 1) + \delta t + \alpha D(i, t) + \beta_1 R^1(i, t) + \cdots + \beta_J R^J(i, t) + \varepsilon(i, t),$$

with omitted spillover variables  $R^{>J}(i, 1), R^{>J}(i, t)$ , we can write the relevant matrices as:

$$(\mathbf{X}'\mathbf{X}) = \begin{bmatrix} N & N_{D_1} & N_{R_1} & \cdots & N_{R_J} & N_{D_T} & N_{R_{1T}} & \cdots & N_{R_{JT}} & N_T \\ N_{D_1} & N_{D_1} & 0 & \cdots & 0 & 0 & 0 & \cdots & 0 & 0 \\ N_{R_1} & 0 & N_{R_1} & \cdots & 0 & 0 & 0 & \cdots & 0 & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ N_{R_J} & 0 & 0 & \cdots & N_{R_J} & 0 & 0 & \cdots & 0 & 0 \\ N_{D_T} & 0 & 0 & \cdots & 0 & N_{D_T} & 0 & \cdots & 0 & N_{D_T} \\ N_{R_{1T}} & 0 & 0 & \cdots & 0 & 0 & N_{R_{1T}} & \cdots & 0 & N_{R_{1T}} \\ \vdots & \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ N_{R_{JT}} & 0 & 0 & \cdots & 0 & 0 & 0 & \cdots & N_{R_{JT}} & N_{R_{JT}} \\ N_T & 0 & 0 & \cdots & 0 & N_{D_T} & N_{R_{1T}} & \cdots & N_{R_{JT}} & N_T \end{bmatrix} \quad \mathbf{X}'R_{>J} = \begin{bmatrix} N_{R_{>J}} & N_{R_{>JT}} \\ 0 & 0 \\ 0 & 0 \\ 0 & \vdots \\ 0 & 0 \\ 0 & 0 \\ 0 & 0 \\ 0 & \vdots \\ 0 & 0 \\ 0 & N_{R_{>JT}} \end{bmatrix}.$$

Here  $A_{11}$  and  $A_{22}$  are arrowhead matrices, and have a known inverse, and  $A_{12}, A_{21}$  have values in the first row and column respectively, and the remainder of entries are zeroes. Thus, each required block of the  $2(J+2) \times 2(J+2)$  matrix is invertible, and it is a symmetric  $2 \times 2$  block matrix. We thus follow the procedure described above for calculating the inverse of  $2 \times 2$  block matrices, giving:

$$B_{11} = (A_{11} - A_{12}A_{22}^{-1}A_{21}')^{-1} = \begin{bmatrix} \frac{1}{\theta_0} & \frac{1}{\theta_0} & \frac{1}{\theta_0} & \cdots & -\frac{1}{\theta_0} \\ \frac{1}{\theta_0} & \frac{\theta_0 + N_{D_1}}{\theta_0 N_{D_1}} & \frac{1}{\theta_0} & \cdots & -\frac{1}{\theta_0} \\ \frac{1}{\theta_0} & \frac{1}{\theta_0} & \frac{\theta_0 + N_{R_1}}{\theta_0 N_{R_1}} & \cdots & -\frac{1}{\theta_0} \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ -\frac{1}{\theta_0} & -\frac{1}{\theta_0} & -\frac{1}{\theta_0} & \cdots & \frac{\theta_0 + N_{R_J}}{\theta_0 N_{R_J}} \end{bmatrix} \quad (\text{A10})$$

$$\text{where } \theta_0 = N - N_T - N_{D_1} - N_{R_1} - \cdots - N_{R_J},$$

and

$$B_{22} = (A_{22} - A'_{12}A_{11}^{-1}A_{12})^{-1} = \begin{bmatrix} \frac{\theta+N_{DT}}{\theta N_{DT}} & \frac{1}{\theta} & \cdots & \frac{1}{\theta} & -\frac{1}{\theta} \\ \frac{1}{\theta} & \frac{\theta+N_{R1T}}{\theta N_{R1T}} & \cdots & \frac{1}{\theta} & -\frac{1}{\theta} \\ \vdots & \vdots & \ddots & \vdots & \vdots \\ \frac{1}{\theta} & \frac{1}{\theta} & \cdots & \frac{\theta+N_{RJ T}}{\theta N_{RJ T}} & -\frac{1}{\theta} \\ -\frac{1}{\theta} & -\frac{1}{\theta} & \cdots & -\frac{1}{\theta} & \frac{\theta_0+\theta}{\theta\theta_0} \end{bmatrix} \quad (A11)$$

where  $\theta = N_T - N_{DT} - N_{R1T} - \cdots - N_{RJ T}$

for the diagonal blocks, and

$$B_{12} = -A_{22}^{-1}A'_{12}(A_{11} - A_{12}A_{22}^{-1}A'_{12})^{-1} = \begin{bmatrix} 0 & 0 & 0 & \cdots & 0 \\ 0 & 0 & 0 & \cdots & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & 0 & \cdots & 0 \\ -\frac{1}{\theta_0} & -\frac{1}{\theta_0} & -\frac{1}{\theta_0} & \cdots & \frac{1}{\theta_0} \end{bmatrix} \quad (A12)$$

and

$$B'_{12} = -A_{11}^{-1}A_{12}(A_{22} - A'_{12}A_{11}^{-1}A_{12})^{-1} = \begin{bmatrix} 0 & 0 & \cdots & 0 & -\frac{1}{\theta_0} \\ 0 & 0 & \cdots & 0 & -\frac{1}{\theta_0} \\ \vdots & \vdots & \ddots & \vdots & \vdots \\ 0 & 0 & \cdots & 0 & -\frac{1}{\theta_0} \\ 0 & 0 & \cdots & 0 & \frac{1}{\theta_0} \end{bmatrix}. \quad (A13)$$

for the off-diagonal terms. In each case, the required matrices are easily invertible using the arrowhead matrix formula (see [Najafi et al. \(2014\)](#) for discussion), and in the case of  $B_{22}$ , the final inverse is found using the bordering method for symmetric matrices.

Combining [A10-A13](#) gives the following for  $(\mathbf{X}'\mathbf{X})^{-1}$ :

$$(\mathbf{X}'\mathbf{X})^{-1} = \begin{bmatrix} \frac{1}{\theta_0} & \frac{1}{\theta_0} & \frac{1}{\theta_0} & \cdots & -\frac{1}{\theta_0} & 0 & 0 & \cdots & 0 & 0 \\ \frac{1}{\theta_0} & \frac{\theta_0+N_{D1}}{\theta_0 N_{D1}} & \frac{1}{\theta_0} & \cdots & -\frac{1}{\theta_0} & 0 & 0 & \cdots & 0 & 0 \\ \frac{1}{\theta_0} & \frac{1}{\theta_0} & \frac{\theta_0+N_{R1}}{\theta_0 N_{R1}} & \cdots & -\frac{1}{\theta_0} & 0 & 0 & \cdots & 0 & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ -\frac{1}{\theta_0} & -\frac{1}{\theta_0} & -\frac{1}{\theta_0} & \cdots & \frac{\theta_0+N_{RJ}}{\theta_0 N_{RJ}} & -\frac{1}{\theta_0} & -\frac{1}{\theta_0} & \cdots & -\frac{1}{\theta_0} & \frac{1}{\theta_0} \\ 0 & 0 & 0 & \cdots & -\frac{1}{\theta_0} & \frac{\theta_0+N_{DT}}{\theta_0 N_{DT}} & \frac{1}{\theta} & \cdots & \frac{1}{\theta} & -\frac{1}{\theta} \\ 0 & 0 & 0 & \cdots & -\frac{1}{\theta_0} & \frac{1}{\theta} & \frac{\theta_0+N_{R1T}}{\theta N_{R1T}} & \cdots & \frac{1}{\theta} & -\frac{1}{\theta} \\ \vdots & \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ 0 & 0 & 0 & \cdots & -\frac{1}{\theta_0} & \frac{1}{\theta} & \frac{1}{\theta} & \cdots & \frac{\theta_0+N_{RJ T}}{\theta N_{RJ T}} & -\frac{1}{\theta} \\ 0 & 0 & 0 & \cdots & \frac{1}{\theta_0} & -\frac{1}{\theta} & -\frac{1}{\theta} & \cdots & -\frac{1}{\theta} & \frac{\theta_0+\theta}{\theta\theta_0} \end{bmatrix}$$

where  $\theta$  and  $\theta_0$  are as laid out in [A10](#) and [A11](#). Note that the inverse in [A6](#) is just a special case of the above, where  $J = 0$ .

Now, to determine the omitted variable bias *on each* included variable where potentially multiple close to treatment variables are included and multiple close to treatment variables are excluded, we follow equation A1 which gives the expectation of  $\hat{\alpha}$  (the estimated average treatment effect), and similarly for each included “close to treatment” variable the expectation of the estimate is:

$$E[\widehat{\beta}_k|\mathbf{X}] = \beta_k + (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{R}\beta_R. \quad (\text{A14})$$

Considering the relevant columns of  $(\mathbf{X}'\mathbf{X})^{-1}$  and  $\mathbf{X}'\mathbf{R}$  thus results in the following expectations:

$$E[\hat{\alpha}|\mathbf{X}] = \alpha - \beta_R \frac{N_{R>JT}}{\theta} = \alpha - \beta_R \frac{N_{R>JT}}{N_T - N_{D_T} - N_{R_{T1}} - \dots - N_{R_{TJ}}} \quad (\text{A15})$$

$$E[\widehat{\beta}_k|\mathbf{X}] = \beta_k - \beta_R \frac{N_{R>JT}}{\theta} = \alpha - \beta_R \frac{N_{R>JT}}{N_T - N_{D_T} - N_{R_{T1}} - \dots - N_{R_{TJ}}} \forall k \in 1, \dots, J. \quad (\text{A16})$$

if a single aggregate  $R^{>J}(i, 1), R^{>J}(i, t)$  omitted dummy is excluded for additional marginal close to treatment areas, or:

$$E[\hat{\alpha}|\mathbf{X}] = \alpha - \beta_{R+1} \frac{N_{R_{J+1T}}}{\theta} - \beta_{R+2} \frac{N_{R_{J+2T}}}{\theta} \dots \quad (\text{A17})$$

$$E[\widehat{\beta}_k|\mathbf{X}] = \beta_k - \beta_{R+1} \frac{N_{R_{J+1T}}}{\theta} - \beta_{R+2} \frac{N_{R_{J+2T}}}{\theta} \dots \forall k \in 1, \dots, J. \quad (\text{A18})$$

if  $R^{>J}(i, 1), R^{>J}(i, t)$  is further partitioned into multiple “close to treatment” dummies of the form  $R^{J+1}(i, 1), R^{J+1}(i, t), \dots, R^K(i, 1), R^K(i, t)$ . These expectations from A17 and A18 are stated in equations 18 and 19 of the paper.

## C Spillovers as a Nuisance Parameter and Treatment Effect Stability

If we start by estimating a typical DD specification without controlling for spillovers, our estimated treatment effect  $\hat{\alpha}$  is given by 16. This suggests that bias will exist if any spillovers occur beyond the direct effect of the policy. If spillovers exist below some distance  $d$ , then  $\beta_k \neq 0 \forall kh < d$ , where  $h$  refers to a bandwidth parameter discussed in section 3.1. If this is the case, and if spillovers work in the same direction as treatment, then  $|E[\hat{\alpha}^0]| < |\alpha|$ , implying that the estimated treatment effect will be attenuated by treatment spillover to the control group, while if spillover effects work in the opposite direction of treatment, estimated treatment effects will be over-estimated. In what follows we assume w.l.o.g. that spillover effects are of the same direction as treatment effects. If the reverse is true, the below holds simply reversing the direction of convergence of  $\hat{\alpha}^k$  to  $\alpha$ .

In order to determine the plausibility of spillovers, we can re-estimate our baseline DD model, however now *also* condition on  $R^1(i, t)$  and  $R^1(i, 1)$  when estimating  $\alpha$ . We refer to this estimate as  $\hat{\alpha}^1$ . Our resulting estimate is displayed in equation 17, and once again, if spillovers exist and are of the same sign as treatment, then the estimate  $\hat{\alpha}^1$  will be attenuated, but not as badly as  $\hat{\alpha}^0$  given that we now partially correct for spillovers up to a distance of  $h$ . In this case:  $|E[\hat{\alpha}^0]| < |E[\hat{\alpha}^1]| < |\alpha|$ . If, on the other hand, spillovers do not exist, then we will have that  $|E[\hat{\alpha}^0]| = |E[\hat{\alpha}^1]| = |\alpha|$ . This leads to the following hypothesis test, where for efficiency reasons  $\hat{\alpha}^0$  and  $\hat{\alpha}^1$  are estimated by seemingly unrelated regression:

$$H_0 : \alpha^0 = \alpha^1 \quad H_1 : \alpha^0 \neq \alpha^1.$$

From Zellner (1962), the test statistic has a  $\chi_1^2$  distribution. If we reject  $H_0$  in favour of the alternative, this indicates that partially correcting for spillovers affects the estimated coefficient  $\alpha$ , implying that spillovers occur at least up to distance  $h$ , and that further tests are required.

Rejection of the null suggests that another iteration should be performed, this time adding  $R^1(i, t)$  and  $R^2(i, t)$  (along with baseline fixed effects) to the model, and the corresponding parameter  $\alpha^2$  be estimated. If spillovers do occur at least up to distance  $2h$ , we expect that  $|E[\hat{\alpha}^0]| < |E[\hat{\alpha}^1]| < |E[\hat{\alpha}^2]| < |\alpha|$ , however if spillovers only occur up to distance  $h$ , we will have  $|E[\hat{\alpha}^0]| < |E[\hat{\alpha}^1]| = |E[\hat{\alpha}^2]| = |\alpha|$ . This leads to a new hypothesis test:

$$H_0 : \alpha^1 = \alpha^2 \quad H_1 : \alpha^1 \neq \alpha^2,$$

where the test statistic is distributed as outlined above. Here, rejection of the null implies that spillovers occur at least up to distance  $2h$ , while failure to reject the null suggests that spillovers only occur up to distance  $h$ .

This process should be followed iteratively up until the point that the marginal estimate  $\hat{\alpha}^{k+1}$  is equal to the preceding estimate  $\hat{\alpha}^k$ . At this point, we can conclude that units at a distance of at least  $kh$  from the nearest treatment unit are not affected by spillovers, and hence a consistent estimate of  $\alpha$  can be produced. Finally, this leads to a conclusion regarding  $d$  and the indicator function  $R(i, t) = \mathbf{1}_{X(i,t) \leq d}$ . When controlling for the marginal distance to treatment indicator no longer affects the estimate of the treatment effect  $\alpha^k$ , we can conclude that  $d = kh$ , and thus correctly identify  $R(i, t) = \mathbf{1}_{X(i,t) \leq kh}$  in data.