Estimating Difference-in-Differences in the Presence of Spillovers: Theory and Application to Contraceptive Reforms in Latin America*

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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 methodology is illustrated using two recent examples of contraceptive reform. It is shown that with both the arrival of abortion to Mexico DF, as well as the arrival of the emergency contraceptive pill to certain areas of Chile, reductions in teenage pregnancy occurred in both reform neighbourhoods as well as nearby (but theoretically untreated) neighbourhoods. Where reforms are geographically disperse, I demonstrate that spillovers can cause considerable concern regarding the unbiasedness of the traditional DD estimates widely employed in the economic literature. Applying this methodology provides considerable insight into the estimation of the impact of contraceptive reform on teenage pregancy: a topical and important policy issue for governments in Latin America.

JEL codes: C13, C21, J13, R23.

*I thank participants in the Impact Evaluation Meeting at the Inter-American Development Bank for useful comments on this draft. Full source code, including the Stata module `cdifdif` is available for download and use at https://github.com/damiancclarke/spillovers. Affiliation: Faculty of Economics, The University of Oxford, Manor Road, Oxford. Contact email: damian.clarke@economics.ox.ac.uk

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.[1]

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 show 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.

It is shown 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 propogation 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 disipates as distance to treatment increases.

Turning to empirics, this methodology is illustrated with two examples. I examine how spillovers of reforms across municipal boundaries may contaminate 'traditional' difference-in-

[1]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).

differences (DD) estimators. This is applied to two contraceptive reforms where individuals from contiguous or nearby areas can travel to a treatment region to access the reform. It is shown that both the arrival of the morning after pill to certain municipalities of Chile, and abortion to certain districts in Mexico, results in a reduction of births in the given area, as well as in close-by quasi-control areas. As a result, the spillover-robust DD estimator propsed here flexibly captures this effect, correcting for any (local) spillover bias that traditional DD fails to identify. The choice of empirical example: contraceptive reform in Latin America, is not casual. I illustrate spillovers in Latin America, firstly, given the importance of correctly estimating the effect sizes of reforms in this context. Latin America is a region with high rates of adolescent pregnancy, second only to Sub-Saharan Africa world-wide (see appendix figure 5). Within Latin America, countries have had varying success in curbing these very high rates (figure 6). Secondly, the costs of undesired pregnancy are very high, resulting in considerable incentives to travel to receive access to contraception or abortion if available in other areas. Taken together, these stylised facts suggest that the analysis of contraceptive reform is both of importance to policy makers, and also potentially particularly likely to suffer from the shortfalls of traditional DD estimation.

Although in both these examples distance is a geographic measure, calculated variously as Euclidean distance, shortest distance over roads, and shortest travel times between areas, this methodology should not be considered as limited to spatial spillovers. Univariate measures of distance including propogation 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. 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, focusing on the case where spillovers are restricted to areas local to treatment clusters. However, unlike recent developments focusing on spillovers between treated and control units *within* a treatment cluster (notable examples include McIntosh (2008); Baird et al. (2014); 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, where 'experiments' are defined based on geographic boundaries, such as administrative political regions which set different policies.[2]

[2]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.

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:

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

where $\delta(t)$ refers to a time-specific component, α 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:

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

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:

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

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 is diffused through general equilibrium processes. Define $R(i,t)$ where:

$$R(i,t) = \begin{cases} f\Big(X(i,t)\Big) > 0 & \text{if an individual resides close to, but not in, a treatment area} \\ 0 & \text{otherwise} \end{cases}$$

Where $X(i,t)$ is an individual covariate measuring distance (in a very general sense) to treatment and $f(\cdot)$ is a positive monotone function. 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 treament area, $R(i,t)=0$ for all i such that $D(i,t)=1$.

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

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

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:

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

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 $E[\eta(i)|D(i,1), R(i,1)]$:

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

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[3]. Finally define μ (the intercept at time 0) as:

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

τ, a fixed effect for treated individuals, as

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

γ, a similar fixed effect for individuals close to treatment, as

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

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

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

Notice that this (estimable) equation now includes the typical DD fixed effects τ and δ and the double difference term α. However it also includes 'close' analogues γ (an initial fixed effect), and β: 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 α and β: the effect of the program treatment and

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

spillover effects on outcome variable $Y(i,t)$. Then, from (7), a our coefficients of interest α and β 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

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

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

3 A Spillover-Robust Double Differences Estimator

We are interested in estimating difference-in-difference parameters α and β 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 person i at time t if they were to receive treatment, and $Y^0(i,t)$ if the person were not to receive treatment. Our ATT and ATC are thus:

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

$$ATC = E[Y^1(i,1) - Y^0(i,1)|C(i,1)=0], \tag{9}$$

where I define a new binary variable $C(i,t)$, which indicates if individuals are close or not close to treatment. This is simply a redefinition of $R(i,t)$, where $C(i,t) = \mathbf{1}_{R(i,t)\neq 0}$. Given that for now we are interested in the *average* effect on those close to treatment we condition only on $C(i,t)$, however in the sections which follow extend to a more general form of $R(i,t)$ to examine the rate of decay or propogations of spillovers over space.

As is typical in the potential outcome 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 $Y(i,t) = Y^0(i,t)\cdot(1-D(i,t))(1-C(i,t)) + Y^1(i,t)\cdot D(i,t) + Y^1(i,t)\cdot C(i,t)$, 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:***
$E[Y^0(i,1) - Y^0(i,0)|D(i,1) = 1, C(i,1) = 0] = E[Y^0(i,1) - Y^0(i,0)|D(i,1) = 0, C(i,1) = 0]$,

Assumption 2. ***Parallel trends in close and control:***
$E[Y^0(i,1) - Y^0(i,0)|D(i,1) = 0, C(i,1) = 1] = E[Y^0(i,1) - Y^0(i,0)|D(i,1) = 0, C(i,1) = 0]$.

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 parallel trends between treatment and close to treatment units allowing for direct interactions between those living in treatment areas, and those living close by.

However, as a matter of course, in order to consistently estimate any treatment effect, some form of the SUTVA must be invoked. Typically, this requires that each individual's treatment status 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.[4]

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

[4]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 and Vytlacil (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.

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 $C(i,t)$ could depend on a range of phenomena including euclidean space, ethnic distance, edges between nodes in a network, or, as I return to discuss in section 3.3, multi-dimensional interactions between measures such as these and economic variables.

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* $C(i,t) = \mathbf{1}_{X(i,t)\leq d}$.

In the following two subsections I examine these estimands in turn.

3.1 Estimating the Treatment Effect in the Presence of Spillovers

From proposition 1, we can consistently estimate α and β, 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 $C(i,t)$, 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 $C(i,t)$ (and later to the heterogeneous function $R(i,t)$) 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.[5] In the remainder of this section I discuss how to determine $C(i,t)$ based on $X(i,t)$, in the absence of a known value for d.

In order to do so, we re-write (7) as:

$$\tilde{Y}(i,t) = \mu + \alpha D(i,t) + \upsilon(i,t). \tag{10}$$

where $\upsilon(i,t) = \beta R(i,t) + \varepsilon(i,t)$, and for ease of notation the fixed effects $D(i,1), R(i,1)$ and t have been concentrated out to form $\tilde{Y}(i,t)$ in line with the Frisch–Waugh–Lovell (FWL) theorem.

[5] 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 clear cut-off point beyond which individuals will be unwilling to travel. Similarly, if treatment must be accessed in a fixed amount of time and propogation of treatment is not instantaneous, a limit for d may be calculable. This is a point I return to in empirical estimates where one illustration is based on access to the emergency contraceptive pill.

If we were to estimate $\hat{\alpha}$ from the above regression ignoring the potential presence of spillovers, then we have that the expectation of $\hat{\alpha}$ is:

$$\begin{aligned} E[\hat{\alpha}] &= \alpha + \beta \frac{\text{Cov}[D(i,t), R(i,t)]}{\text{Var}[D(i,t)]} + \frac{\text{Cov}[D(i,t), \varepsilon(i,t)]}{\text{Var}[D(i,t)]} \\ &= \alpha + \beta \frac{\text{Cov}[D(i,t), R(i,t)]}{\text{Var}[D(i,t)]}, \end{aligned} \tag{11}$$

where the second line comes from (2), which implies that $E[\text{Cov}(D(i,t), \varepsilon(i,t))] = 0$. So far I have attached no functional form to $R(i,t)$. Define $R(i,t)$ as:

$$R(i,t) = \beta_1 R^1(i,t) + \beta_2 R^2(i,t) + \cdots + \beta_K R^K(i,t) \tag{12}$$

where:

$$R^k(i,t) = \begin{cases} 1 & \text{if } \; X_i \geq (k-1) \cdot h \;\; \text{and} \;\; X_i < k \cdot h \\ 0 & \text{otherwise} \end{cases} \qquad \forall k \in (1, 2, \ldots, K). \tag{13}$$

In the above expression h refers to a bandwidth type parameter, which partitions the continuous distance variable X_i into groups of distance h.[6]

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. From (12) and the preceding logic, this suggests that d can be recovered following the iterative procedure laid out below, so long as $R(i,t) = f(X(i,t))$ is montononic in X.

If we start by estimating a typical DD specification like (10), our estimated treatment effect, which I now denote $\hat{\alpha}^0$ is:

$$\begin{aligned} E[\hat{\alpha}^0] = \alpha + \beta\beta_1 \frac{\text{Cov}[D(i,t), R^1(i,t)]}{\text{Var}[D(i,t)]} + \beta\beta_2 \frac{\text{Cov}[D(i,t), R^2(i,t)]}{\text{Var}[D(i,t)]} + \\ \ldots + \beta\beta_K \frac{\text{Cov}[D(i,t), R^K(i,t)]}{\text{Var}[D(i,t)]}. \end{aligned}$$

If spillovers exist below some distance d, then $\text{Cov}[D(i,t), R^k(i,t)] > 0 \;\; \forall \;\; kh < d$, given that $D(i,t)$—the treatment status in a treated area—affects the close to treated status in nearby areas. 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.

[6] 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.

We can then re-estimate (10), however now *also* condition out $R^1(i,t)$ prior to estimating α. Our resulting estimate, $\hat{\alpha}^1$, will have the expectation:

$$E[\hat{\alpha}^1] = \alpha + \beta\beta_2 \frac{\text{Cov}[D(i,t), R^2(i,t)]}{\text{Var}[D(i,t)]} + \ldots + \beta\beta_K \frac{\text{Cov}[D(i,t), R^K(i,t)]}{\text{Var}[D(i,t)]}.$$

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:

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

From Zellner (1962), the test statistic has a χ^2_1 distribution. If we reject H_0 in favour of the alternative, this indicates that partially correcting for spillovers affects the estimated coefficient α, 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 removing $R^1(i,t)$ and $R^2(i,t)$ from the error term $\upsilon(i,t)$ in (10), and the corresponding parameter α^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:

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

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 α can be produced. Finally, this leads to a conclusion regarding d and the indicator function $C(i,t) = \mathbf{1}_{X(i,t)\leq d}$. When controlling for the marginal distance to treatment indicator no longer affects the estimate of the treatment effect α^k, we can conclude that $d = kh$, and thus correctly identify $C(i,t) = \mathbf{1}_{X(i,t)\leq kh}$ in data.

3.2 Estimation the Magnitude of Spillovers

In section 3.1, I discuss the consistent estimation of α, the effect of being in a treatment area. The extension of this methodology to consistently estimate β, the effect of being close to treatment, is reasonably straightforward. Once the scalar value d has been determined, and with data $\{Y(i,t), D(i,t), X(i,t) : i = 1, \ldots, N, t = 0, 1\}$ in hand, we can use d to map $X(i,t)$ into $C(i,t)$. Given the above we can now estimate (7), and form consistent estimates $\hat{\beta}$ and $\hat{\alpha}$ using OLS.

The estimate $\hat{\beta}$ will be the average treated effect on the close to treated (ATC), and will be one summary value for all areas to which spillovers occur. However, more information regarding the precise manner of propogation can be observed by estimating with the re-parametrized $R(i,t)$ from (12) instead of the indicator variable $C(i,t)$. This suggests an alternative spillover test, in the style of that proposed in section 3.1. Rather than observing $\hat{\alpha}^j$ at each stage of the estimation process, $\hat{\beta}^j$ can be directly observed. If $\hat{\beta}_j \neq 0$, this suggests that the effect on the marginal close to treatment area is different to the effect in the (remaining) control area. If spillovers are the estimand of interest, additional $R^j(i,t)$ controls can be added until the hypothesis: $H_0 : \beta_j = 0$ cannot be rejected for the marginal parameter. The empirical illustrations in section 4 estimate both the treatment effect, as well as spillovers at varying distances from treatment.

3.3 Estimating with Multidimensional Spillovers

Previously it has been assumed that $R(i,t)$ is a function of a unidimensional distance measure $X(i,t)$. I 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). Now:

$$R(i,t) = \begin{cases} f\Big(\mathbf{X}(i,t)\Big) & \text{if an individual resides close to, but not in, a treatment area} \\ 0 & \text{otherwise} \end{cases}$$

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, δ, maps units with covariates $\mathbf{X} = \mathbf{x}$ to close assignment r:

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

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[7]:

$$\delta(x) \equiv \mathbf{1}_{\mathbf{x}\in\mathbb{T}}. \tag{14}$$

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

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* $C(i,t) = \mathbf{1}_{\mathbf{x}\in\mathbb{T}}$*.*

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

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 exceeding some income threshold. Consider the case where $X_1 \in \{0,1\}$ is binary, and X_2 continuous. Then we can parametrise $R(i,t)$ as:

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

where $X_2^k(i,t) \forall k \in 1 \ldots K$ is defined as per (13). Estimation of α can then proceed iteratively as in section 3.1. First a traditional DD parameter is estimated ignoring the possiblity that spillovers exist, leading to the proposed estimate $\hat{\alpha}^0$. Then $X_1 \cdot [\beta_{0,1} X_2^1(i,t)]$ and $(1 - X_1) \cdot [\beta_{1,1} X^1(i,t)]$

[7] 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 $\mathbb{T} \equiv \{x < d : \delta(x) = 1\}$

are included in the regression, leading to an updated estimate $\hat{\alpha}^1$. If the hypothesis $H_0 : \alpha^0 = \alpha^1$ cannot be rejected this suggests that spillovers are not a relevant phenomenon for either group, and the estimate of $\hat{\alpha}^0$ is accepted as the ATT. Otherwise, an additional iteration is made until the inclusion of the marginal $X_2^k(i,t)$ indicators for $X_1 \in \{0,1\}$ no longer affect the estimated effect α^k.

4 An Empirical Illustration: Spillovers and Contraceptive Reforms

I consider two empirical examples to motivate and demonstrate spillover-robust DD estimation. I focus on two localised contraceptive reforms in different countries. The first is the legalisation of abortion in Mexico city in April of 2007, and the second the expansion of morning after pill availability in certain municipalities of Chile in 2008. Both reforms were sharp, resulting in a large jump in reported rates of contraceptive access, and arrived to only certain areas of the country. In both cases, the geographic location of the reform was defined by the nature of local municipal-level policies, resulting in seperate policies in different municipalities in the country.[8]

Contraceptive reform provides a useful test of a spillover-robust DD methodology. Firstly, the arrival is plausibly exogenous at the level of the treated woman.[9] Secondly, the incentives to access contraceptives, especially post-coital treatments such as the morning after pill and abortion is high. Even if a woman is geographically excluded from a treatment municipality, given that the economic and psychic costs of an undesired birth are very high, considerable incentives will exist to travel from a non-treatment area to a treatment area in order to access fertility control policies. Thirdly, contraceptive information may also be important in determining contraceptive behaviour, and this information may travel through (local) friendship networks.

Some further details regarding each reform are provided in the sections below. In each case we estimate traditional difference-in-differences parameters under the assumption that spillovers do not exist (and hence the SUTVA holds), and then augment these estimates with the estimator discussed in the previous sections.

[8] I refer to geographic units in each case as municipalities. In Mexico these are referred to as *municipios*, or in the case of Mexico City *delegaciones* and are the level below the State (there are 2,473 in total). In Chile these are known as *comunas*, (of which there are 346) and are also the level below the state.

[9] Both reforms in question were due to legislative changes which were eventually upheld by the supreme court of the country. Additional details regarding the Chile reform are described in appendix C and additional details on the Mexican reform are provided in appendix B.

4.1 Abortion Reform in Mexico

On April 24, 2007 Mexico City passed a law which which legalised abortion under all circumstances in the first 12 weeks of pregnancy (see for example Fraser (2014) for a discussion). This was a radical change from previous laws which outlawed abortion in all but the extreme circumstances of rape, to save the mother's life, or in the case of fetal inviability. This law was *only* passed in Mexico City (or *Distrito Federal*), the administrative capital, and a region of Mexico containing approximately 8% of the population.

This reform resulted in free and legal access, with legal abortions being widely used. This service has accounted for slightly than 89,000 abortions between 2007 and 2012 Becker and Díaz-Olavarrieta (2013). Women of all reproductive ages have accessed abortion (ages 11-50), with slightly more than 20% of users being teenaged women. In this paper I focus on the effect of legal abortion usage on teenagers, however figures for non-teenagers (showing broadly similar patterns) are provided in appendices E and F. A more comprehensive discussion of the Mexico abortion reform is provided in appendix B.

The effect of this reform on the number of teenage births is examined.[10] In order to do so, data from various sources is collected. Microdata on all registered births is collated from yearly vital statistics registers provided by the Mexican National Institute of Statistics and Geography for the years 2001–2010. This is crossed with a range of municipality×year varying measures including spending on medical staff by municipalities, educational investments and stocks, municipal involvement in the *Seguro Popular* program,[11] and access to other types of contraceptives. This results in data on 22.20 million births, of which 1.47 million are to teenage mothers. Basic descriptive statistics of births in Mexico DF, births in municipalities close (<30 km) from Mexico DF, births in other states, along with municipal controls are provided in table 1.

Table 2 provides estimates of the effect of the reform on the total number of births by teenagers in treatment municipalities. In column 1 we estimate a specification similar to (10): the traditional DD estimate which does not account for spillovers.[12] This is then extended in columns 2 to 5 to account for spillovers (if necessary). These additional columns show the iterative estimation process entailed in the spillover-robust DD process, as described in section 3 and equation (7). In the case that spillovers exist, and that these spillovers work in the same direction as the

[10] Numbers of births are used rather than rates given the difficulty in obtaining precise measures of the number of women of each age living in each municipality in each year. Although censal counts of women *are* available at the municipality level, this is only for census years (2000, 2005 and 2010).

[11] *Seguro Popular* is one of the largest publicly-funded health insurance programs in the world, offering coverage to all individuals not covered by private (employer financed) health insurance (Bosch and Campos-Vazquez, 2014). This covers (among many other things) basic antenatal care and contraceptive access.

[12] Rather than estimating (10) precisely as written, we estimate a more flexible specification including time varying controls, full time and municipality fixed effects, and municipal trends. The intution however is unchanged.

Table 1: Descriptive Statistics (Mexico)

	Observations	Mean	Std. Dev.	Min.	Max.
Treatment	24550	0.00	0.04	0	1
Close to Treatment	24550	0.00	0.05	0	1
Number of Births (Mexico DF)	160	11744.75	8835.83	1550	34729
Number of Births (Close to DF)	250	12419.40	9254.72	1550	39745
Number of Births (Other Areas)	24300	785.99	3153.99	0	86659
Year (2001-2010)	24550	2005.50	2.87	2001	2010
Number of Medical Staff	24550	57.97	250.81	0	6212
Number of Classrooms	24550	303.51	1000.80	0	19280
Number of Libraries	24550	4.27	16.95	0	708
Municipal Income	24550	75.51	254.56	0	6615
Municipal Spending	24550	82.71	271.05	0	6615
Regional Unemployment Rate	24550	2.93	1.46	0	9

NOTES: Observations are for 2,455 municipalities in 10 years. Number of births refers to total counts for all women aged 15-49 in each municipality within the given area. Municipal income and municipal spending refer to tax receipts and outlays, and are expressed in millions of pesos.

treatment effect itself, we should expect that we can reject the null that $\beta < 0$ for coefficient estimates on 'Close' controls. If however, $\hat{\beta}$ is not significantly different to zero, this suggests that areas 'close to treatment' are not different from areas far away from treatment, and that augmenting the specification to account for local spillovers is unnecessary.

Estimates from table 2 suggest that, firstly, the effect of the abortion policy is significant in magnitude. It reduces births among teenagers by 125 births per municipality per year. When comparing this to the average level of 1632.1 in treatment municipalities, this is a sizeable (and statistically significant) effect. When augmenting to control for local spillovers in columns 2 to 5, it appears that municipalities 'close to' treatment also are affected by the reform. For those municipalities within 10km of treatment municipalities (but not themselves treated), the effect is a highly statistically significant reduction of approximately 120 births. Column 3 extends to include a range of close controls. Here it becomes apparent that statistically significant effects remain at least up to areas between 10 and 20km from the nearest treatment, and negative point estimates only disappear when travelling greater than 30km away from treatment.

In examining estimates of living in treatment and close to treatment areas, it is worth noting that although spillover estimates are significantly different to zero over ranges of approximately 30 km, the correction for spillovers does not result in statistically significant changes in estimates of α: the effect of living in Mexico DF, after the introduction of the reform (though coefficients move as theoretically hypothesised). Considering the relative small number of municipalities which are 'close' to Mexico DF (only 0.5% of total municipalities, containing 1.5% of total births in Mexico lie within a 30km radius of Mexico DF), this is not remarkably surprising, as the attenuation bias caused by these municipalitis is small. This is a point I return to discuss more

Table 2: Treatment Effects and Spillovers: Mexico (15-19 year olds)

	N Birth (1)	N Birth (2)	N Birth (3)	N Birth (4)	N Birth (5)
Treatment	-125.3***	-126.0***	-127.0***	-127.2***	-127.2***
	(45.33)	(45.36)	(45.33)	(45.32)	(45.32)
Close 1		-119.9**	-120.7**	-120.9**	-120.9**
		(52.69)	(52.87)	(52.88)	(52.88)
Close 2			-40.51**	-40.70**	-40.70**
			(19.92)	(19.92)	(19.92)
Close 3				-9.295	-9.296
				(15.62)	(15.62)
Close 4					-0.0524
					(13.95)
Mean	1,632	1,632	1,632	1,632	1,632
Regions×Time	24,550	24,550	24,550	24,550	24,550

NOTES: Each column represents a separate difference-in-differences regression including full time and municipal fixed effects and linear trends by municipality. Standard errors are clustered at the level of the geographic region of treatment (municipality). Close variables are included in bins of 10km, so Close 1 refers to distances of [0,10)km, Close 2 refers to [10,20)km, and so forth. The dependent variable is a count of all births in the municipality, and is estimated by OLS. Further details regarding controls can be found in section 4.1.

extensively when examining the case of Chile, where treatment is geographically disperse, and a much larger proportion of the population lies 'close' to treatment areas.

Figure 3 presents a graphical representation of estimates of a vector of β coefficients from equation (7).[13] While the largest effect of treatment is felt in the treatment municipality itself, effects clearly remain even outside of treatment municipalities, suggesting that the spillover robust specification is necessary to estimate causal effects $\hat{\alpha}$ and $\hat{\beta}$.

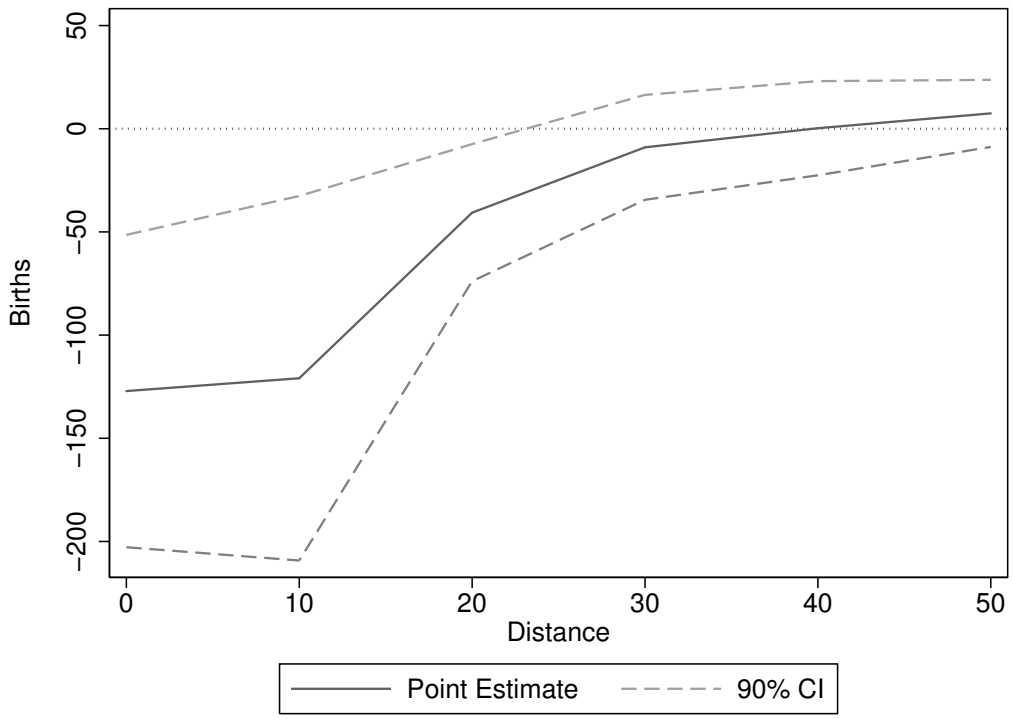
4.2 Emergency Contraceptive Reform in Chile

After considerable juridical challenges against the legality of emergency (post-coital) contraception in the country, a Chilean constitutional tribunal in 2008 issued a summary expressly allowing the morning after pill[14] to be prescribed to women. However, this finding was limited to munici-

[13] While here we focus on teenaged girls, appendix E presents similar graphical results for other age groups.

[14] The morning after pill is a hormonal treatment composed of progestin and estrogen which acts to prevent ovulation after sexual intercourse in which alternative forms of contraceptives were not used, or believed to have failed.

Figure 1: Treatment and close to treatment effects: 15-19 year olds Mexico



NOTES TO FIGURE: Each point represents a treatment effect for the group living $d \in [0, 50]$ km from the nearest treatment municipality. As such, the point at 0 includes all municipalities to directly receive treatment (Mexico DF). Standard errors are clustered at the level of the municipality. Dotted lines display the 90% confidence interval for all estimates.

pal health centres, which are administered by mayors and local governing councils. This resulted in a period of approximately 4 years where the morning after pill was available to women *only* if the mayor of her municipality deemed it appropriate. The reform eventually resulted in morning after pill availability in approximately 150 of the 346 municipalities of the country. Further figures and details of the reform are discussed in Bentancor and Clarke (2014). A description of the constitutional details of the reform are provided in appendix C, and summary statistics for areas with and without the emergency contraceptive pill are provided in table 3.

As for the case of the Mexico abortion reform, a 'traditional' DD specification is estimated, and compared with a spillover-robust DD estimator as proposed in section 3. A generalised version of (10) is estimated (where full year and municipal fixed effects are added, and municipal linear trends and time-varying controls are included), and compared to an identical version of the equation robust to spillovers between treatment and close-to-treatment areas (7). If the traditional DD approach adequately captures the treatment—or in other words, if the SUTVA holds globally—then we should see two things. Firstly, our estimate of α from (10) should not be significantly different to that from (7). Secondly, the coefficient on each $R^k(i,t)$ should not be

Table 3: Summary Statistics

	No Pill Available	Pill Available	Total
Municipality Characteristics			
Poverty	16.4	17.0	16.6
	(7.47)	(7.56)	(7.49)
Conservative	0.286	0.267	0.281
	(0.452)	(0.443)	(0.45)
Education Spending	4,817	5,980	5,108
	(5,649)	(6,216)	(5,818)
Health Spending	1,866	2,788	2,096
	(2,635)	(3,381)	(2,867)
Out of School	4.07	3.98	4.05
	(3.16)	(3.06)	(3.13)
Female Mayor	0.120	0.134	0.123
	(0.325)	(0.341)	(0.329)
Female Poverty	60.5	62.0	60.8
	(10.64)	(9.48)	(10.4)
Pill Distance	5.94	0.00	4.46
	(18.4)	(0.0)	(16.1)
Individual Characteristics			
Live Births	0.054	0.053	0.054
	(0.226)	(0.224)	(0.226)
Fetal Deaths	0.0558	0.0513	0.0547
	(0.269)	(0.256)	(0.266)
Birthweight	3322.7	3334.3	3324.7
	(540.0)	(542.3)	(540.4)
Maternal education	11.92	12.03	11.94
	(2.967)	(2.894)	(2.955)
Percent working	0.295	0.395	0.312
	(0.456)	(0.489)	(0.463)
Married	0.340	0.309	0.335
	(0.474)	(0.462)	(0.472)
Age at Birth	27.05	27.15	27.07
	(6.777)	(6.790)	(6.779)
N Comunas	346	280	346
N Fetal Deaths	9,999	3,064	13,063
N Births	1,214,088	391,212	1,605,300

Notes: Group means are presented with standard deviations below in parentheses. Poverty refers to the % of the municipality below the poverty line, conservative is a binary variable indicating if the mayor comes from a politically conservative party health and education spending are measured in thousands of Chilean pesos, and pill distance measures the distance (in km) to the nearest municipality which reports prescribing emergency contraceptives. Pregnancies are reported as % of all women giving live birth, while fetal deaths are reported per live birth. All summary statistics are for the period 2006-2012.

significantly different to zero. Formally, if we cannot reject the null that $\beta = 0$, this is evidence against the need for spillover robust DD in this case.

Table 4: Treatment Effects and Spillovers: Chile (15-19 year olds)

	Pr(Birth) (1)	Pr(Birth) (2)	Pr(Birth) (3)	Pr(Birth) (4)	Pr(Birth) (5)
Treatment	-0.046^{***}	-0.058^{***}	-0.066^{***}	-0.073^{***}	-0.074^{***}
	(0.011)	(0.013)	(0.014)	(0.014)	(0.015)
Close 1		-0.049^{***}	-0.056^{***}	-0.062^{***}	-0.062^{***}
		(0.015)	(0.014)	(0.014)	(0.014)
Close 2			-0.040^{*}	-0.047^{*}	-0.048^{**}
			(0.023)	(0.024)	(0.024)
Close 3				-0.038^{*}	-0.038^{*}
				(0.023)	(0.023)
Close 4					-0.014
					(0.023)
Mean	0.052	0.052	0.052	0.052	0.052
Regions× Time	1,929	1,929	1,929	1,929	1,929

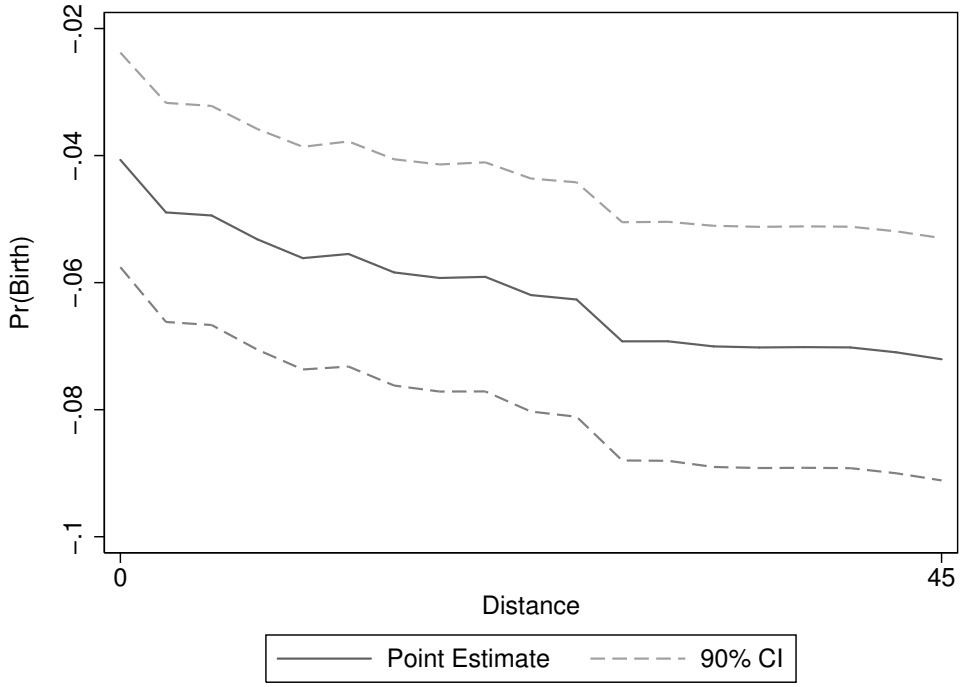
NOTES: Each column represents a separate difference-in-differences regression including full time and municipal fixed effects and linear trends by municipality. Standard errors are clustered at the level of the geographic region of treatment (municipality). Close variables are included in bins of 10km, so Close 1 refers to distances of [0,10)km, Close 2 refers to [10,20)km, and so forth. Models are estimated using a binary (logit) model for birth versus no birth. Coefficients are expressed as log odds.

Table 4 presents estimates from the Chile reform. In this case the variable $Y(i,t)$ represents the probability of giving birth at time t, a binary outcome taking either 0 or 1 for each individual aged 15-19 years. Column 1 presents an estimate where treatment is defined as having the morning after pill available in the municipality where a woman lives one year prior to the realised birth outcome (birth versus no birth). The lag of one year accounts for the mechanical delay in realisations of $Y(i,t)$ due to child gestation. This alone suggests important effects of the reform: having the reform available in the municipality of residence of the woman is associated with a 4.5% reduction[15] in births the following year. However, in columns (2) to (5), we see that naïve estimates which fail to account for (local) spillovers *understate* the true importance of the reform. Column 2 suggests that for teenages living very close to the reform area, the reform appears to be nearly as important (a 4.8% versus a 5.6% reduction in pregnancy rates), even though their municipality is not directly treated. Columns 3-5 progressively include additional 'close' binary variables, up to a distance of 40km. These tests suggest that the effect of the reform is able to travel around 30km, after which point marginal areas are not significantly affected by the reform, and the estimate of the treatment effect in other areas is not affected by additional

[15]Each binary model is estimated by logistic regression and odds ratios are reported. Hence, the percentage reduction in the outcome of interest for a coefficient of -0.046 is calculated as $1 - \exp(-0.046) = 0.045$, or 4.5%. In the remainder of this section, coefficients will always be converted to percentage reductions of the outcome variable when discussed.

distance controls. The spillover distance of this reform is reasonably similar to the effects of the Mexico abortion reform discussed in the previous section. Similar tests are run with women aged over 20, as well as using alternative distance measures (distance by road and travel time by road) in appendices D and F.

Figure 2: Treatment Effects: 15-19 year olds Chile



NOTES TO FIGURE: Each point represents the estimated treatment effect on the treated ($\hat{\alpha}$), conditioning on close controls for $d \in [0, 45]$ km from the nearest treatment municipality. As such, the point at 0 includes all municipalities with the exception of treatment municipalities in the control group. The point at 2.5 controls for spillovers up to 2.5 km (removing these areas from the control group), and so forth at other distances. Standard errors are clustered at the level of the municipality. Dotted lines display the 90% confidence interval for all estimates.

These results clearly suggest that we *can* reject the null that $\beta = 0$, as a number of 'close' coefficients are significant, in some cases even up to $p = 0.01$. However, tests directly on α do not allow for us to reject that values estimated for various models are significantly different. Examining estimates $\hat{\alpha}$ more carefully suggests that as we move further away from the reform, the effect size monotonically decreases (figure 2). This is precisely in-line with what we would expect if SUTVA were violated locally, and the cost (both psychic and economic) of travelling to treatment municipalities increased with distance. This figure suggests that traditional DD estimates are attenuated when the presence of spillovers are not accounted for, and that the bias in estimates of α are corrected once controlling adequately for spillover distance. The reported estimates of $\hat{\alpha}^k$ in figure 2 demonstrate the result derived in section 3.1 that if spillovers occur, if they are monotonic in distance, and if they are of the same direction of the treatment itself, then: $|E[\hat{\alpha}^0]| < |E[\hat{\alpha}^1]| < \cdots < |E[\hat{\alpha}^{d/h}]| = |\alpha|$, where d is the maxiumum distance at which

spillovers occur, and h is the bandwidth measure, which in the above is 2.5 km.

4.3 Running Additional Placebo Tests

Typically, DD estimates are presented along with placebo tests which define 'false' lagged reforms. In other words, by examining outcomes entirely *before* the policy of interest has been implemented, null results are presented as evidence in favour of an appropriately specified functional form of the DD set-up.

In the case of the spillover-robust DD estimate, there are now (at least) two relevant placebos which should be tested. Firstly, the reform must not have any effect on outcomes *before* treatment in treatment municipalities. This is precisely the same as the 'traditional' placebo test described above. Secondly however, the reform should have no effect on predetermined outcomes in municipalites *close* to treatment municipalities. Below we present an example of such placebo tests from the Mexico City abortion reform. Now, as well as having a treatment estimate not significantly different from 0 (ie confidence intervals at $distance = 0$), the same result should hold for close municipalities ($distance > 0$).

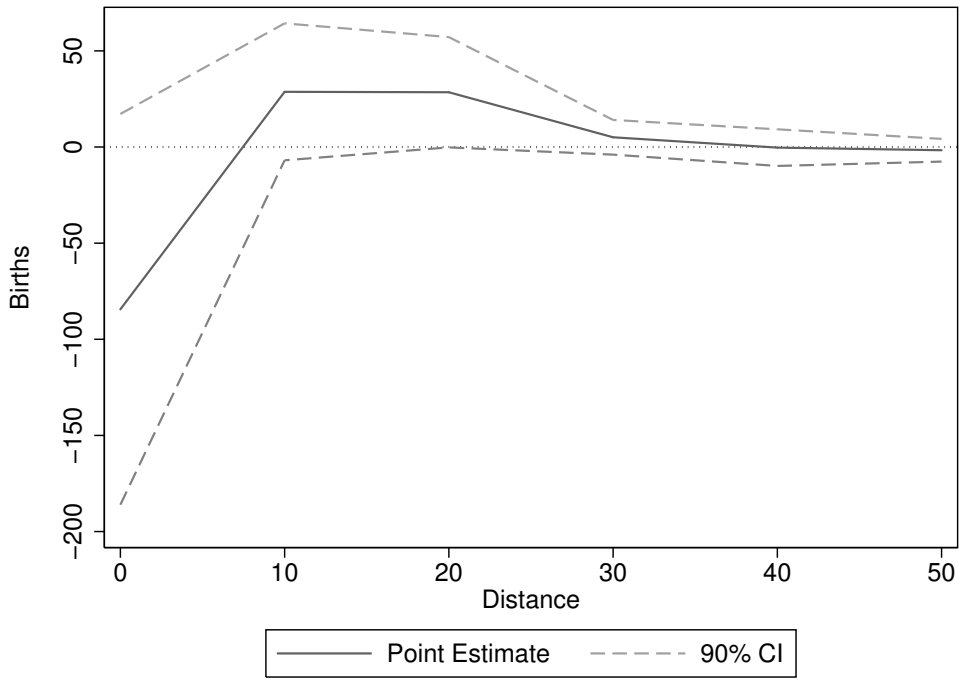
A more demanding series of placebo tests involves the estimation of a full event study based on the DD specification. In this case, instead of estimating a single treatment effect for all periods following the arrival of the natural experiment in question, a binary variable for living in a treatment area is interacted with a series of lags and leads around the date of the reform. This allows for a direct test of the timing of effect. In a Granger (1969) causality framework, any difference between treatment and control states should only emerge following the introduction of the reform: not prior to the date of the reform.

In traditional DD this leads to estimations of event study where coefficients and standard errors are plotted which compare treated to control areas. Insignificant differences prior to the reform and significant differences posterior to the reform are evidence in favour of the parallel trend assumption, and that the reform causes the effect, rather than the other way around[16]. In the case of spillover robust DD estimates, there are now two logical tests to employ. These (seperately) test both parallel trend assumptions (assumption 1 and 2). Both treated *and* close to treated areas can be compared with control areas in an event study framework.

Figures 4a and 4b present these event studies for the case of Chile. The omitted base year is 3

[16]This is also a test for the presence of phenomena similar to Ashenfelter's dip (Ashenfelter, 1978; Heckman and Smith, 1999), where a reform or program may be the result of a poor outcome prior to the program. Ashenfelter's dip refers to the fact that earnings are often seen to fall prior to entry into labour market training programs, though similar phenomena may ocurr where public policy responds to particularly concerning social indicators such as high rates of teenage pregnancy.

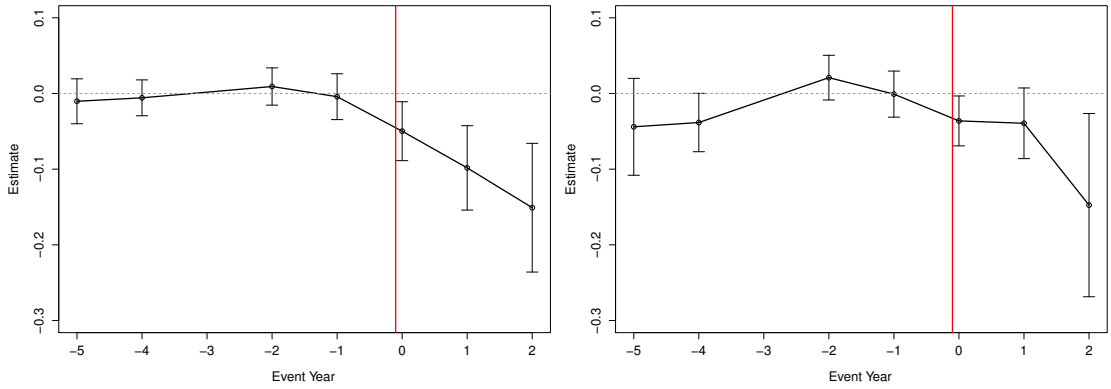
Figure 3: Treatment and Close, Placebo Tests: 15-19 year olds Mexico



NOTES TO FIGURE: Each point represents a placebo treatment effect for the group living $d \in [0, 50]$ km from the nearest treatment municipality three years *prior* to the reform. All births were realised entirely before the reform began. Standard errors are clustered at the level of the municipality. Dotted lines display the 90% confidence interval for all estimates.

birth cohorts prior to the reform (a group which gave birth entirely before the year in which the emergency contraceptive pill arrived to Chile). Comparing treatment to control municipalities (4a), the parallel trend assumption appears to be valid, with all estimates being close to zero and tightly estimated. Only in years following the reform does the effect diverge from zero, with coefficients at least providing some evidence that the effect of the reform has grown as knowledge of the morning after pill has become more widespread. For close-to-treatment areas the event study is slightly noisier, however once again the divergence between these areas and control areas occurs only *after* the vertical line signalling the first cohort affected by the reform. In this case there is more variation in the magnitude of pre-reform coefficients, though at a 95% confidence level, equality with zero cannot be rejected in any case (evidence broadly in favour of assumption 2).

Figure 4: Chile Event Study for ATT and ATC



(a) Full Event Study: Treatment

(b) Full Event Study: Close to Treatment

NOTES TO FIGURE: In each figures the horizontal dotted line represents an effect size of 0. The vertical solid line represents the first birth cohort affected by the reform. Each point on the plot represents the effect of living in a treatment (panel A) or close to treatment (panel B) municipality x years before or after the reform took effect. Error bars represent 95% confidence intervals of these estimates.

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. In this paper we examine how DD estimates perform when the stable unit treatment value assumption does not hold locally. Such a situation may be 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 I 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. It is suggested that in the absence of this correction for local violations of SUTVA that (if spillovers actually *do* occur) the true effect of the policy is likely to be attenuated.

Using two empirical examples from recent contraceptive policy expansions, it is shown that this is—at least in these cases—an important consideration for the estimation of treatment effects, and effects on nearby neighbourhoods. For both Chile and Mexico, it is shown that

pregnancy rates in neighbourhoods located close to areas where contraceptive reforms took place had subsequent reductions in rates of teenage pregnancy. What's more, in Chile (but not in Mexico), the correction for spillovers results in a significant reduction in estimated treatment effects on the treated, correcting an attenuation bias when control units are partially treated. This is a useful reflection on this methodology: where treatment is geographically disperse, and hence many people live close to treatment areas (as in Chile), correcting for failures of the SUTVA is likely to be particularly important. In cases where treatment is only available in a reduced geographic area (such as Mexico), the degree of importance of spillovers are likely to be considerably less when considering estimates of average effects on treated areas.

These tests are easy to run, and indeed a software package that automates this methodology is released with this paper. Given the nature of the assumptions underlying identification in many DD models in the literature, tests of this nature should be included in a basic suite of falsification tests. While the examples in this paper are illustrated using geographic spillovers, spillover-robust DD estimation is certainly not limited to only geographic cases. How (and whether) treatment travels between units should be of fundamental concern to many applications in the economic literature.

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A 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 $C(i,t)$ can be estimated. It is assumed that we have at a representative sample of size N consisting of $\{Y(i,t), D(i,t), X(i,t) : i = 1, \ldots, N, t = 0, 1\}$. By assumption 4A, the assignment rule δ forms $C(i,t)$ allowing for the estimation of (7). By definition, α in this regression is equal to:

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

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:

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

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 β on the coefficient $R(i,t)$, however now instead of assumption 1 we must rely on parallel-trend assumption 2. This leads to $\beta = E[Y^1(i,1) - Y^0(i,1)|R(i,1) \neq 1]$, and from (9) and the previous expression it holds that that $\beta = ATC$. ■

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

B Additional Details: 2007 Mexico Abortion Reform

On April 26, 2007 the legislative assembly of the Federal District of Mexico City (Mexico DF), voted to legalise abortion (termed legal interruption of pregnancy) whenever requested by the woman up to 12 weeks of gestation, reforming article 144 of the penal code of Mexico DF. This immediately permitted women from DF to request (free) legal interruption of pregnancy in public health clinics, with a large influx of requests (Contreras et al., 2011). On August 29, 2008 this decision was ratified by the Supreme Court of Mexico.

As well as decriminalising abortion, the law dictated that Mexico DF Department of Health facilities offer free abortion to residents of DF, and on a variable pay scale for women from other areas of the country (Becker and Díaz-Olavarrieta, 2013). Prior to the April 2006 findings, abortion was illegal in Mexico DF (and all of Mexico) in all but a very limited set of circumsances (depending on the state, these circumstances include none, some, or all of rape, fetal inviability or grave danger to the health of the mother). Along with free pregnancy terminations at Ministry of Health clinics, following the reform private health centres were also allowed to provide abortions.

Abortion services were widely accessed following the reform. Between April of 2007 and the end of 2011, 80,000 abortions were performed. These were accessed by women over the entire age range of the fertility distribution, reasonably closely mirroring mother's age at birth in birth data (figure 7), though with a slightly higher rate for younger women. Prior to April 2007 very few legal abortions were performed (n line with the restrictions listed above). Between 2001 and 2007 only 62 legal abortions were performed, though clandestine abortion was very common (Becker and Díaz-Olavarrieta, 2013). Further details regarding the reform, demand and subsequent state decisions can be found in (Becker and Díaz-Olavarrieta, 2013), and references therein.

C The Chilean Legislative Environment and the Adoption of Emergency Contraception

Discussions surrounding the introduction of emergency contraception in Chile have taken place since at least 1996, when the Chilean Institute of Reproductive Medicine (ICMER for its initials in Spanish) proposed the use of this method to avoid undesired pregnancies in a country where abortion was entirely outlawed (Dides Castillo, 2006). However, the first legislative attention given to this matter occurred when the Chilean Institute of Public Health emitted a resolution allowing for the production and sale of 'Postinol', a drug containing levonogestrel by a Chilean laboratory in 2001. The Constitionality of this was quickly challenged, and the drug was prohibited by the Supreme Court.

The emergency contraceptive pill again entered legislative attention in 2004, following the Ministry of Health's publication of a guide suggesting that emergency contraception be used following cases of rape. Following this in 2005, the Subsecretary of Health Dr. Antonio Infante announced that emergency contraception would be freely available to *all* women who requested it, however the President of Chile and the Ministry of Health later declared that this was not the case, leading to removal of the Subsecretary from office.

In November of 2005, the Supreme Court of Chile provided the first constitutional support for the emergency contraceptive pill, voting 5-0 to reverse the decision taken in 2001, allowing emergency contraception to be provided in the case that the mother's life was in danger. Once again however, this finding was challenged shortly thereafter. The same non-governmental institution which had earlier raised a case against ICMER, now challenged the private commercial laboratory in charge of producing and distributing the drug. However, before this case could reach court, this laboratory voluntarily gave up their license to produce the drug, in a three line statement issued by the General Director of the company on February 14, 2006 (Casas Becerra, 2008).

In the same year, a group of 36 parliamentary deputies from conservative parties raised a case with the Constitional Tribunal, claiming that the provision of the emergency contraceptive pill contravened the "National Laws for the Regulation of Fertility", a set of rules issued by the Ministry of Health. This case was only resolved in 2008, with the Constitional Tribunal's finding in favour of this group, hence making illegal any provision by hospitals or health centres controlled by the Ministry of Health (and hence under the jurisdiction of the National Fertility Laws). Fundamentally however, this left the door open for Municipal health centres to distribute the pill freely to women. These Municipal Health Centres are run under the directive of the elected mayor of each Municipality, leaving all remaining legislation regarding the distribution of the pill up to the 346 mayors in Chile.

In this study I examine the period surrounding this 2008 legislation as the cutoff of interest. However, even after this finding the emergency contraceptive pill has not been far from legislative action, with a number of other cases raised. These cases never entirely threatened the continuity of supply of the morning after pill by municipalities, however did cause some confusion for mayors and municipal health bodies in determining whether or not they were legally allowed to prescribe the contraceptive. These cases also resulted in the passing of a number of laws and standards. Most importantly, they resulted in national Law 20.418 which "creates standards for information, guidance and regulatory services in fertility" (author's translation), and the passing of a decree on March 3, 2013, which makes obligatory the provision of the morning after pill to women of any age in any health centre in Chile. This became operative on May 28, 2013, meaning that—at least officially—there are no longer any restrictions in place in the country.

D Measuring Distance to Treatment Clusters

Principal measures of distance from treatment is calculated by taking a Euclidean distance from the centroid of non-treatment clusters, to the centroid of the nearest cluster which did receive treatment. However, alternative measures may more accurately capture the true distance of an individual to treatment. As a robustness check, two alternative measures of distance to treatment are calculated and used.

Firstly, I collated the shortest distance over roads from non-treatment to treatment areas. This was calculated using repeated calls to the Google Distance Matrix API[17], which finds the shortest path over roads. In the case of Chile, this requires calculating the distances between all 346 municipalities ($346^2/2 = 59,858$ distance pairs), while in the case of México this requires calculating only the distance from each municipality outside of Mexico DF to each municipality inside Mexico DF ($2457 \times 16 = 39,312$). Secondly, rather than distance in kilometres, as in Euclidean or road distance, a measure of travel *time* was calculated. As a proxy for total travel time, travel time by car was caclulated between areas. This was similarly generated using calls to Google Maps, resulting in one value for each municipality pair. In each case "distance to treatment" is then the minimum value to the nearest treatment area, which varies by municipality and year.

These alternative measures of distance do not majorly affect the quantitative implication of findings in either Chile or Mexico. Appendix figure 8 is the analogue of figure 2, using travel time rather than Euclidean distance between municipalities as a measure of spillover distance. Results from both figures suggest a treatment effect of approximately -0.075 once accounting for spillovers of 30 minutes travel time or 30km of distance respectively. Regression results for all age groups and all measures did not result in significantly different estimates of the effect of treatment in any case.

[17]Full details can be found at: https://developers.google.com/maps/documentation/distancematrix/#api_key. I have made the computational routine used available on the web at: https://github.com/damiancclarke/spillovers/blob/master/source/distCalc/queryDist.py.

E Appendix Figures

Figure 5: Adolescent Pregnancy Rates in Latin America and the World

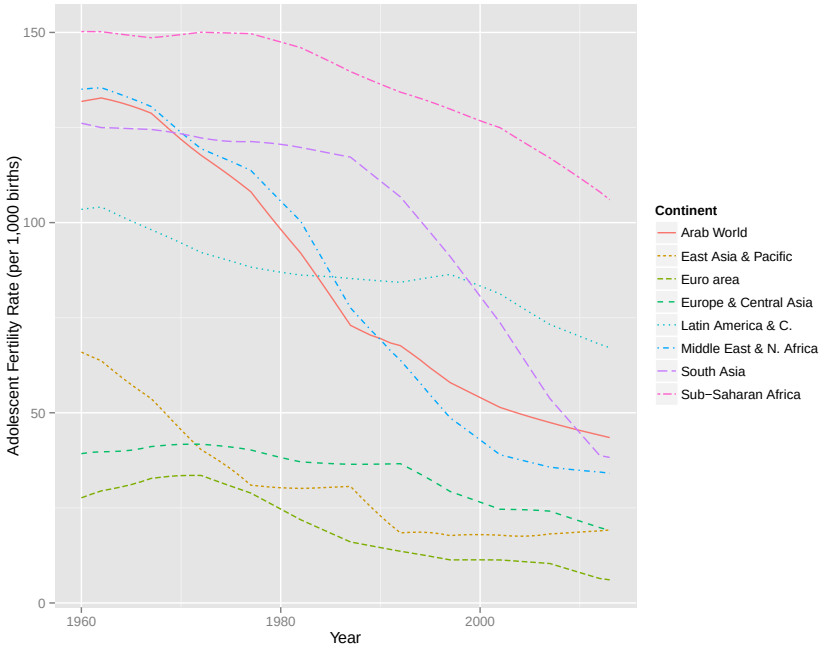
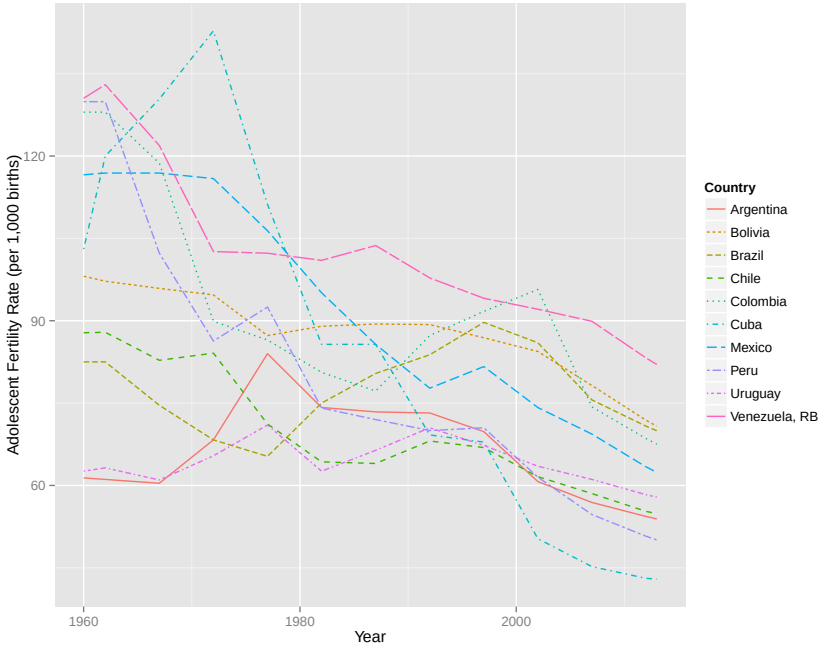
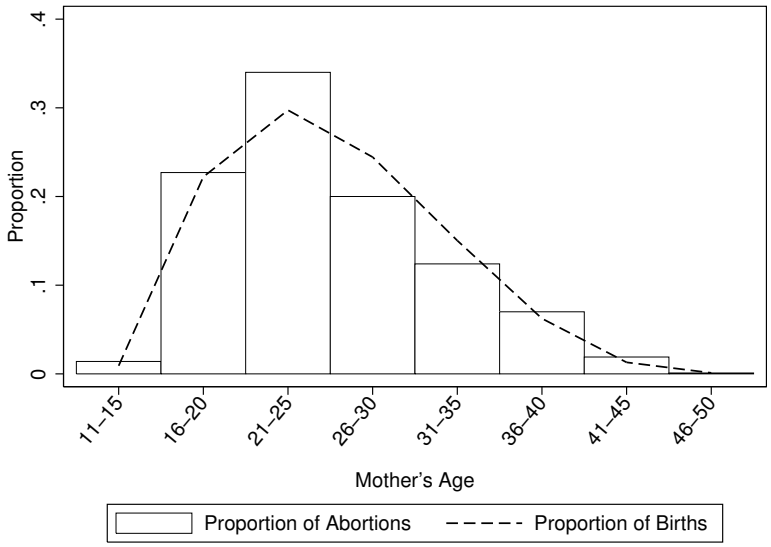


Figure 6: Adolescent Pregnancy Rates In Various Latin American Countries



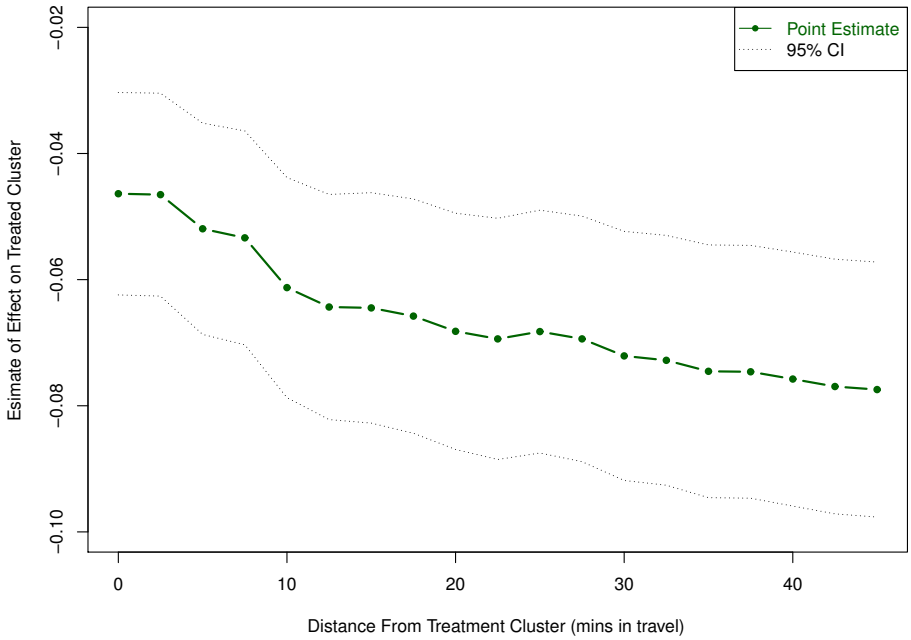
Notes to figures 5-6: Adolescent pregnancy rates come from the World Bank Data Bank, and are expressed as number of births per 1,000 15-19 year-old women.

Figure 7: Birth and Abortion Descriptives: Mexico



NOTES TO FIGURE: Total births are plotted between 2001 and 2010. Abortions are plotted from the date of reform (April 26, 2007) until 2011. The total quantity of births is 22.20 million (all of Mexico), and total abortions are 69,861 (Mexico DF only). Births are calculated from administrative data (INEGI) and abortions from administrative data (Secretary of Health, Mexico DF).

Figure 8: Estimate of Average Treatment Effect when Controlling for Travel Time



F Appendix Tables

Table 5: Treatment Effects and Spillovers: Mexico (20-34 year olds)

	N Birth (1)	N Birth (2)	N Birth (3)	N Birth (4)	N Birth (5)
Treatment	-1,157**	-1,167**	-1,176**	-1,178**	-1,179**
	(468.4)	(469.2)	(469.2)	(469.3)	(469.3)
Close 1		-1,623**	-1,631**	-1,633**	-1,634**
		(655.4)	(656.6)	(656.7)	(656.8)
Close 2			-460.7**	-462.2**	-463.2**
			(192.0)	(192.0)	(192.0)
Close 3				-87.40	-88.38
				(129.3)	(129.3)
Close 4					-94.58
					(156.1)
Mean	10,394	10,394	10,394	10,394	10,394
Regions×Time	24,550	24,550	24,550	24,550	24,550

NOTES: Refer to table 2.

Table 6: Treatment Effects and Spillovers: Mexico (35-49 year olds)

	N Birth (1)	N Birth (2)	N Birth (3)	N Birth (4)
Treatment	-207.8**	-208.9**	-209.7***	-209.8***
	(80.96)	(81.05)	(81.08)	(81.09)
Close 1		-175.2**	-176.0**	-176.1**
		(71.70)	(71.79)	(71.80)
Close 2			-37.39**	-37.49**
			(17.25)	(17.25)
Close 3				-5.058
Mean	1,415	1,415	1,415	1,415
Regions×Time	24,550	24,550	24,550	24,550

NOTES: Refer to table 2.

Table 7: Treatment Effects and Spillovers: Chile (20-34 year olds)

	Pr(Birth) (1)	Pr(Birth) (2)	Pr(Birth) (3)	Pr(Birth) (4)
Treatment	-0.031^{***}	-0.040^{***}	-0.042^{***}	-0.043^{***}
	(0.007)	(0.008)	(0.009)	(0.010)
Close 1		-0.034^{***}	-0.035^{***}	-0.036^{***}
		(0.012)	(0.012)	(0.012)
Close 2			-0.007	-0.008
			(0.017)	(0.017)
Close 3				-0.006
				(0.012)
Mean	0.085	0.085	0.085	0.085
Regions× Time	1,929	1,929	1,929	1,929

NOTES: Refer to notes in table 4.

Table 8: Treatment Effects and Spillovers: Chile (35-49 year olds)

	Pr(Birth) (1)	Pr(Birth) (2)
Treatment	-0.010	-0.011
	(0.010)	(0.011)
Close 1		-0.007
		(0.013)
Mean	0.021	0.021
Regions× Time	1,929	1,929

NOTES: Refer to notes in table 4.