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Abstract

We analyze the conditions in which ignoring spatial correlation is problematic for inference in differences-in-differences (DID) models. Assuming that the spatial correlation structure follows a linear factor model, we show that inference ignoring such correlation remains reliable when either (i) the second moment of the difference between the pre- and post-treatment averages of common factors is low, or (ii) the distribution of factor loadings has the same expected values for treated and control groups, and do not exhibit significant spatial correlation. We present simulations with real datasets that corroborate these conclusions. Our results provide important guidelines on how to minimize inference problems due to spatial correlation in DID applications.

Keywords: inference, differences-in-differences, spatial correlation

JEL Codes: C12; C21; C23; C33

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1 Introduction

Differences-in-Differences (DID) is one of the most widely used methods for identification of causal effects in applied economics. However, inference in DID models can be complicated by both serial and spatial correlations. After an influential paper by Bertrand et al. (2004), showing that serial correlation can lead to severe over-rejection in DID applications if not taken into account, most papers applying DID models use inference methods that are robust to arbitrary forms of serial correlation.[1] In contrast, most of these papers do not take spatial correlation into account. Barrios et al. (2012) show that ignoring spatial correlation is not a problem for inference when treatment is randomly assigned at the cluster level. However, such assumption may be too strong in many empirical applications. In this paper, we consider the consequences of ignoring spatial correlation in DID models when treatment is possibly not randomly assigned.

The main insight in this paper is that the relevant spatial correlation for DID models reflects the spatial correlation of unobserved variables that affect the outcome variable after controlling for the time and group fixed effects. As a consequence, we show in Section 2 that, if the spatial correlation structure is based on a linear factor model, then inference ignoring spatial correlation remains reliable when *either* (i) the second moment of the difference between the pre- and post-treatment averages of common factors is low, or (ii) the distribution of factor loadings has the same expected values for treated and control groups, and do not exhibit significant spatial correlation. If either one of these conditions hold, then the time or group fixed effects would absorb most of the relevant spatial correlation, and inference ignoring spatial correlation would be reliable. In contrast, it is only when both of these conditions do not hold that spatial correlation can lead to significant over-rejection.

We present in Section 3 simulations with the American Community Survey (ACS) and with the Current Population Survey (CPS). We show in these simulations that ignoring the

[1] The importance of clustering at a group level to take serial correlation into account had been previously noted by, for example, Arellano (1987). However, Bertrand et al. (2004) show that such strategies had not been widely incorporated in DID applications.

spatial correlation does not significantly affect inference when either the distance between the pre- and post-treatment periods is short, or when the treated and control groups are alike. In contrast, we find severe over-rejection when both the distance between the pre- and post-treatment periods is large, and when the treated and control groups are very different. These results are consistent with the conclusions from the spatial correlation model we analyze in Section 2, and suggests that this structure provides a good approximation for real datasets like the ACS and the CPS.

Our results provide important guidelines on when we should expect spatial correlation to be relevant in DID models. In Section 4, we present recommendations for applied researchers on how to minimize the relevance of spatial correlation. Section 5 concludes.

2 The Inference Problem

Consider a standard DID model

$$Y_{jt} = \alpha d_{jt} + \theta_j + \gamma_t + \eta_{jt}, \tag{1}$$

where Y_{jt} is the outcome variable for group j at time t, and d_{jt} is an indicator variable equal to one if group j is treated at time t, and zero otherwise. The parameter α is defined as the causal effect of d_{jt} on Y_{jt}, while θ_j and γ_t are, respectively, group and time fixed effects. The error term η_{jt} represent unobserved variables that are not captured by the fixed effects.

There are N_1 treated groups, N_0 control groups, and T time periods. For simplicity, we assume that d_{jt} changes to 1 for all treated groups starting after date t^*. Let $\mathcal{I}_1$ ($\mathcal{I}_0$) be the set of indices for treated (control) groups, while $\mathcal{T}_1$ ($\mathcal{T}_0$) be the set of indices for post- (pre-) treatment periods. Following Ferman and Pinto (2019), we consider the post-pre difference

in average errors for each group j, which is given by

$$W_j = \frac{1}{T-t^*}\sum_{t\in\mathcal{T}_1}\eta_{jt} - \frac{1}{t^*}\sum_{t\in\mathcal{T}_0}\eta_{jt}. \tag{2}$$

In this simpler case, the DID estimator is numerically equivalent to the two-way fixed effects estimator of α, which is given by

$$\begin{aligned}\hat{\alpha} &= \frac{1}{N_1}\sum_{j\in\mathcal{I}_1}\left[\frac{1}{T-t^*}\sum_{t\in\mathcal{T}_1}Y_{jt} - \frac{1}{t^*}\sum_{t\in\mathcal{T}_0}Y_{jt}\right] - \frac{1}{N_0}\sum_{t\in\mathcal{I}_0}\left[\frac{1}{T-t^*}\sum_{t\in\mathcal{T}_1}Y_{jt} - \frac{1}{t^*}\sum_{t\in\mathcal{T}_0}Y_{jt}\right] \\ &= \alpha + \frac{1}{N_1}\sum_{j\in\mathcal{I}_1}W_j - \frac{1}{N_0}\sum_{j\in\mathcal{I}_0}W_j.\end{aligned} \tag{3}$$

If $\mathbb{E}[W_j|j\in\mathcal{I}_1] = \mathbb{E}[W_j|j\in\mathcal{I}_0]$, then the DID estimator $\hat{\alpha}$ will be unbiased for α, regardless of the assumptions on the serial and spatial correlations of η_{jt}. However, inference in DID models is only possible if we impose assumptions on either the serial or the spatial correlation of η_{jt}. Most commonly, inference methods for DID models do not impose restrictions on the correlation η_{jt} across time, which is captured by this linear combination of the errors, W_j, but assumes that η_{jt} are independent across j.[2]

The most common alternative when independence across j is assumed is to rely on cluster robust variance estimator (CRVE), clustering at the group level. In this case, up to a degrees-of-freedom correction, the CRVE is given by

$$\widehat{var(\hat{\alpha})}_{\text{Cluster}} = \left[\frac{1}{N_1}\right]^2\sum_{j\in\mathcal{I}_1}\widehat{W}_j^2 + \left[\frac{1}{N_0}\right]^2\sum_{j\in\mathcal{I}_0}\widehat{W}_j^2, \tag{4}$$

where $\widehat{W}_j = \frac{1}{T-t^*}\sum_{t=t^*+1}^{T}\hat{\eta}_{jt} - \frac{1}{t^*}\sum_{t=1}^{t^*}\hat{\eta}_{jt}$ is a linear combination of the residuals of the DID regression. Assuming independence across j, the CRVE provides asymptotically valid inference when $N_1, N_0 \to \infty$. If W_j is correlated across j, however, then not taking such spatial correlation into account can lead to severe underestimation of the true standard error,

[2]See, for example, Arellano (1987), Bertrand et al. (2004), Cameron et al. (2008), Brewer et al. (2017), Conley and Taber (2011), Ferman and Pinto (2019), Canay et al. (2017), and MacKinnon and Webb (2019).

resulting in over-rejection. The intuition is the following. Imagine there is an unobserved variable in W_j that equally affects all treated groups, but does not affect the control groups.[3] If the null $H_0 : \alpha = 0$ is true, then, from equation (3), we have that $\hat{\alpha} = \frac{1}{N_1}\sum_{j\in\mathcal{I}_1} W_j - \frac{1}{N_0}\sum_{j\in\mathcal{I}_0} W_j$. Therefore, under the null, finding a "large" value for $\hat{\alpha}$ would only be possible if many of those W_j for $j \in \mathcal{I}_1$ were positive.[4] If we (mistakenly) assume that W_j are all independent, we would attribute a much lower probability that such event may happen relative to when we take into account that those W_j's might be correlated, leading to over-rejection.

When the assumption that η_{jt} is independent across j is relaxed, there are some alternatives for inference, but these alternatives often assume that there is a distance metric across groups, impose assumptions on the serial correlation, and/or rely on more data.[5] One important case in which spatial correlation does not generate problems for inference even when such correlation is ignored is when cluster-level explanatory variables are randomly allocated across clusters. In this case, Barrios et al. (2012) show that ignoring spatial correlation is not a problem in the estimation of the standard errors of the estimator.[6]

The main insight in this paper is to show that η_{jt} represents the unobserved variables in the DID model that remains after controlling for the group and year fixed effects. Therefore, the relevance of the spatial correlation problem in DID models will depend crucially on the amount of the spatial correlation that is not absorbed by the group and year fixed effects.

[3] We assume that the expected value of this variable is equal to zero, so that the presence of such correlated shock does not affect the identification assumption of the DID model

[4] Or when many of those W_j for $j \in \mathcal{I}_0$ are negative.

[5] For example, Kim and Sun (2013), Conley and Taber (2011) (in their online appendix A.3), and Bester et al. (2011) rely on distance measures across groups. Adao et al. (2010) show that spatial correlation leads to over-rejection in shift-share designs, and propose an inference method that is asymptotically valid when there are many shifters. This method, however, does not apply in more general settings. Other papers exploit the time dimension to perform inference in the presence of spatially correlated shocks. However, these methods rely on a large number of periods. For example, Vogelsang (2012) and Ferman and Pinto (2019) (Section 4) present inference methods that work with arbitrary spatial correlation when the number of periods goes to infinity, while Dailey (2017) proposes the use of randomization inference using long series of past data when the explanatory variable is rainfall data.

[6] While they show this result in a cross-section model, in this case in which all treated groups start treatment at the same treatment, it is easy to show that that the DID model can be re-written as cross-section model where each observation j is the different between the post- and pre-treatment means.

To illustrate this idea, we consider a model in which potential outcomes follow a linear factor model, and derive the W_j that is implied when we consider such underlying model. Let $Y_{jt}(0)$ ($Y_{jt}(1)$) be the outcome of group j at time t when this group is untreated (treated). Consider then

$$\begin{cases} Y_{jt}(0) = \lambda_t \mu_j + \epsilon_{jt} \\ Y_{jt}(1) = \alpha + Y_{jt}(0) \end{cases}, \tag{5}$$

where λ_t is an $(1 \times F)$ vector of common shocks, while μ_j is an $(F \times 1)$ vector of factor loadings that determines how group j is affected by the common shocks λ_t. We assume that all spatial correlation is captured by this linear factor structure, so that ϵ_{jt} is independent across j. We do allow, however, for arbitrary serial correlation in both ϵ_{jt} and λ_t. We consider a super-population setting in which λ_t, μ_j, and ϵ_{jt} are treated as random variables.[7]

In this case, we have that

$$\begin{aligned} \hat{\alpha} - \alpha &= \frac{1}{N_1} \sum_{j \in \mathcal{I}_1} \left[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j]) + (\bar{\epsilon}_{j,\text{post}} - \bar{\epsilon}_{j,\text{pre}}) \right] - & (6) \\ &\quad - \frac{1}{N_0} \sum_{j \in \mathcal{I}_0} \left[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j]) + (\bar{\epsilon}_{j,\text{post}} - \bar{\epsilon}_{j,\text{pre}}) \right] & (7) \end{aligned}$$

where $\bar{\lambda}_{\text{post}} = \frac{1}{T - T^*} \sum_{t \in \mathcal{T}_1} \lambda_t$, and $\bar{\lambda}_{\text{pre}}$, $\bar{\epsilon}_{j,\text{post}}$, and $\bar{\epsilon}_{j,\text{pre}}$ are defined in a similar way. Therefore, the potential outcomes model (5) generates a DID model (1) such that $W_j = (\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j]) + (\bar{\epsilon}_{j,\text{post}} - \bar{\epsilon}_{j,\text{pre}})$.

We have that $\hat{\alpha}$ is unbiased if $\mathbb{E}[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j]) + \bar{\epsilon}_{j,\text{post}} - \bar{\epsilon}_{j,\text{pre}} | j \in \mathcal{I}_1] = \mathbb{E}[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j]) + \bar{\epsilon}_{j,\text{post}} - \bar{\epsilon}_{j,\text{pre}} | j \in \mathcal{I}_0]$. Assuming that the idiosyncratic error is not correlated with the treatment assignment, this will be the case when either one of two conditions hold. First, it may be that $\mathbb{E}[\bar{\lambda}_{\text{post}}] = \mathbb{E}[\bar{\lambda}_{\text{pre}}]$, so the first moment of the distribution of

[7] Since we are focusing on the problem of inference with spatially correlated shocks, we simplify the analysis by considering the case with homogeneous treatment (α is constant). See Callaway and Sant'Anna (2018), Athey and Imbens (2018), and Goodman-Bacon (2018) for a discussion on the setting with heterogeneous treatment effects.

the common factors are stable in the pre- and post-treatment periods. In this case, even if treated and control groups are differentially affected by the common factors, the DID estimator is unbiased over the distribution of λ_t. Alternatively, it may be that $\mathbb{E}[\mu_j|j \in \mathcal{I}_1] = \mathbb{E}[\mu_j|j \in \mathcal{I}_0]$. In this case, even if the expected value of λ_t differs in the pre- and post-treatment periods, these common factors do not systematically affect treated groups differently relative to control groups, so the DID estimator is unbiased over the distribution of μ_j. Since the focus in this paper is on inference, we assume that the conditions for unbiasedness hold.

We consider now under which conditions inference based on standard errors clustered at the group level is significantly affected by spatial correlation. As noted above, based on the results derived by Barrios et al. (2012), inference would still be valid if treatment is randomly assigned at the cluster (in this case, group) level. However, this is generally a strong assumption in DID applications, so we focus on cases in which treatment may not be randomly assigned.

For $w \in \{0, 1\}$, if we consider a sampling scheme such that we can apply a law of large numbers for $\frac{1}{N_w}\sum_{j \in N_w} \mu_j$ when $N_1, N_0 \to \infty$, then

$$\widehat{W}_j \to_p W_j = (\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j]) + \bar{\epsilon}_{j,\text{post}} - \bar{\epsilon}_{j,\text{pre}}. \tag{8}$$

The potential problem in using the CRVE, defined in equation (4), is that W_j will generally be correlated across j due to the common shocks. This formulation highlights the conditions in which spatially correlated shocks are more likely to generate problems for inference, which will be the case when the variance of $(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j])$ is high relative to the variance of $\bar{\epsilon}_{j,\text{post}} - \bar{\epsilon}_{j,\text{pre}}$. First, note that correlated shocks will be less relevant when the second moment of $(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})$ is small. If λ_t is serially correlated, with the serial correlation decreasing over time, then the shorter the distance between the initial and final periods, the smaller the second moment of $(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})$. The intuition in this case is that the group fixed

effects would absorb most of the relevant spatial correlation if we expect $\bar{\lambda}_{\text{post}}$ to be similar to $\bar{\lambda}_{\text{pre}}$ (that is, $\mathbb{E}[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})^2] \approx 0$).

If we fix the second moment of $(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})$, then the spatially correlated term for a $j \in \mathcal{I}_1$ can be re-written as $(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mathbb{E}[\mu_j | j \in \mathcal{I}_1] - \mathbb{E}[\mu_j]) + (\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mu_j - \mathbb{E}[\mu_j | j \in \mathcal{I}_1])$.[8] The first term reflects that the common shocks can differentially affect, on average, treated and control groups. Therefore, this term would generate less problems for inference when treated and control groups are, in expectation, more similar (which would imply $\mathbb{E}[\mu_j | j \in \mathcal{I}_1] \approx \mathbb{E}[\mu_j]$). In this case, the year fixed effects would absorb most of this variation. The second term, however, highlights that treated and control groups being, in expectation, equally affected by the common shocks is not sufficient so that spatial correlation is innocuous for inference, even both N_1 and N_0 are large. This term would not affect inference if we consider two polar cases. First, if treated groups are more homogeneous, so that $var(\mu_j | j \in \mathcal{I}_1) \approx 0$, then this term would be close to zero and would not generate problems for inference. Alternatively, if μ_j is independent across j, then this term would not generate spatial correlation and would be taken into account by CRVE at the group level.[9] Note that, in this case, there would still be unobserved variables that are spatially correlated. However, what remains from these variables after we control for the fixed effects would be uncorrelated across j.

A potential problem for inference, however, arises when μ_j itself exhibits spatial correlation. The intuition is that, in this case, an average of N_1 observations of μ_j for the treated groups would be less informative than the same average if μ_j were independent across j. Therefore, estimated standard errors that ignore this spatial correlation would be underestimated, which would lead to over-rejection. Importantly, the CRVE allows for spatial correlation in factor loadings within the cluster level. For example, consider a setting with individuals i, at group j and year t. If cluster is at the group level, then μ_{ij} is allowed to be correlated with $\mu_{i'j}$. What is not allowed is that μ_{ij} and $\mu_{i'j'}$ is correlated for $j \neq j'$. If,

[8] The same rationale is valid for a $j \in \mathcal{I}_0$.

[9] In this case, it can be that the distribution $\mu_j | j \in \mathcal{I}_1$ differs from the distribution of $\mu_j | j \in \mathcal{I}_0$, as long as $\mathbb{E}[\mu_j | j \in \mathcal{I}_1] = \mathbb{E}[\mu_j | j \in \mathcal{I}_0]$, so that the first term is equal to zero. Since CRVE is robust to heteroskedasticity, it would take differences in the distribution of μ_j for treated and controls into account.

however, cluster is at the individual level, then μ_{ij} is not allowed to be correlated with $\mu_{i'j}$ for $i \neq i'$.

The results presented in this section highlights the conditions in which spatially correlated shocks coming from a linear factor model structure leads to inference problems when spatial correlation is ignored. Spatially correlated shocks become irrelevant when the average of the pre-treatment common factors is likely to be similar to the average of the post-treatment common factors (that is, $\mathbb{E}[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})^2] \approx 0$). Importantly, this result is valid regardless of the serial correlation of λ_t.[10] In contrast, the averages of factor loadings of treated and control groups being similar is not sufficient for spatially correlated shocks to become irrelevant. It remains true, however, that spatially correlated shocks should lead to a more severe problem when the first moment of the distributions of factor loadings for treated and control groups is different, because in this case the term $(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})(\mathbb{E}[\mu_j | j \in \mathcal{I}_1] - \mathbb{E}[\mu_j])$ would be relevant. Therefore, spatially correlated shocks should be less problematic when the distribution of factor loadings of treated and control groups are more similar, even though we cannot guarantee that such shocks would be innocuous even when the two distributions are identical.

This asymmetry comes from the fact that we are considering inference based on CRVE at the group level, which is the standard alternative when N is large relative to T. If we had a setting with many periods and consider a CRVE at the time level, then the reverse result would hold. A possible alternative in this case, if both N and T are large, could be the use of two-way cluster at the group and time dimensions (see Cameron et al. (2011), Thompson (2011), Davezies et al. (2018), Menzel (2017), and MacKinnon et al. (2019)). While some of these methods report good performance in simulations with few clusters in one dimension, if common factors are serially correlated, then this solution would not take into account the correlation between η_{jt} and $\eta_{j't'}$, for $j \neq j'$ and $t \neq t'$, which would lead to over-rejection.

[10] Of course, the serial correlation of λ_t will affect $\mathbb{E}[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})^2]$. However, the argument here is that, conditional on $\mathbb{E}[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})^2]$, the serial correlation of λ_t does not affect the implications of spatial correlation for inference with CRVE at the group level.

We present a Monte Carlo simulation in Appendix A confirming this intuition.

3 Simulations with Real Datasets

We now test the conclusions from Section 2 in simulations with two real datasets, the American Community Survey (ACS) and the Current Population Survey (CPS). Following the strategy used by Bertrand et al. (2004), we randomly generate placebo interventions, and then evaluate the proportion of simulations in which we would reject the null based on inference ignoring spatial correlation. Note that Bertrand et al. (2004) randomly assigned which states received treatment in their simulations. In light of the results from Barrios et al. (2012), this is likely why CRVE at the state level worked well in their simulations. Here we consider simulations in which treatment may not be randomly assigned at the cluster level.

3.1 Simulations with the ACS

We start considering simulations with the ACS from 2005 to 2017.[11] We select two states and two periods, and then allocate treatment at the Public Use Microdata Area (PUMA) level in the second period. Since it is expected that there are state-level unobserved covariates, the structure of the data is so that there is potentially relevant spatial correlation across PUMAs. We consider two different treatment allocations, one in which PUMAs are randomly assigned treatment independently of their state, and another one in which treatment is assigned at the state level. We also vary the distance in years between the pre- and post-periods, which can be $\delta_{\text{year}} \in \{1, 2, ..., 10\}$. Following Bertrand et al. (2004), we restrict the sample to women between the ages 25 and 50, and consider as outcome variables log wages and employment. In each of these simulations, we estimate the treatment effect using a DID model, and test the null hypothesis of no effect based on standard errors clustered at the PUMA level. Therefore, the inference method allows for arbitrary correlation between

[11] We created our ACS extract using IPUMS (Ruggles et al. (2015)).

individuals in the same PUMA, but imposes the restriction that the error term for individuals in different PUMAs are independent. Since in all cases treatment was randomly assigned, we should expect to reject the null 5% of the time if the inference method is working properly.

The structure of these simulations mimics situations in which we suspect that there may be unobserved variables that are spatially correlated, and we are not able to divide the treatment and control observations in subgroups that are arguably independent. Also, we consider a case in which we do not have a distance measure between groups, or we do not want to make further assumptions about the structure of the errors. In such cases, the only alternative, if we want to allow for unrestricted serial correlation, is to ignore the spatial correlation and rely on the (possibly incorrect) assumption that there are subgroups that are independent, or that treatment is randomly allocated across clusters. In these simulations, we want to study what would happen if we estimate our standard errors allowing for spatial correlation within PUMAs, but ignoring spatial correlation across PUMAs.

In Figure 1, we first present results with randomly allocated treatment across PUMAs for $\delta \in \{1, 2, ..., 10\}$. In this case, based on the results derived by Barrios et al. (2012), the proportion of placebo regressions in which the null is rejected at a 5% significance level test should be around 5%. Rejection rates are close to 5% independently of δ, whether we consider log wages (Figure 1.A) or employment (Figure 1.B) as outcome variables.[12] This is consistent with the fact that treatment was randomly assigned across PUMAs.

We also present in Figure 1 rejection rates for simulations in which treatment was assigned at the state level. In this case, we should expect over-rejection if there is spatial correlation in the error term even after taking into account the state and year fixed effects. When we consider simulations in which pre- and post-treatment periods are consecutive years (that is, $\delta = 1$), there is only mild over-rejection: 6.9% when log wages is used as outcome variable and 7.2% when employment is used as outcome variable. When we increase the distance

[12]We restrict to simulations with at least 20 treated and 20 control PUMAs, because CRVE requires a large number of both treated and control clusters to be reliable, even if we assume clusters are independent (MacKinnon and Webb (2017))

between the pre- and post-treatment periods, however, the over-rejection sharply increases, reaching more than 20% in some cases.

These results are in line with the intuition presented in Section 2 that group fixed effects should capture most of the spatial correlation if the distance between the pre- and post-treatment years is small. However, when this distance is large, then the group fixed effects will capture less of the spatial correlation, implying in more severe over-rejection.

We also consider simulations using the two-way cluster standard errors proposed by Cameron et al. (2011), clustering at both the PUMA and the year levels. In this case, we modify the simulations because two-way cluster does not work well with only one pre-treatment period and one post-treatment period. Therefore, we include in each simulation ten years of data, with the placebo treatment starting after the fifth year. When we consider treatment randomly allocated across PUMAs, rejection rates are 6.5% and 7% when the outcome variable is, respectively, log wages and employment. There is a slight over-rejection, possibly from the fact that there are only ten periods. In contrast, when we consider treatment randomly allocated across states, rejection rates are 23% and 28%. These simulations confirm the intuition presented in Section 2, that two-way cluster procedures may underestimate the standard errors because they fail to take into account correlations between η_{jt} and $\eta_{j't'}$, for $j \neq j'$ and $t \neq t'$. Note that such correlations will appear whenever there are common shocks that are serially correlated. We present a Monte Carlo simulation in Appendix A that confirms this intuition.

3.2 Simulations with the CPS

We now present simulations using the CPS data from 1979 to 2018. We select two years and two age groups. We vary the distance between the pre- and post-treatment periods (δ_{year}), and the distance between the two age groups (δ_{age}), both ranging from 1 to 15. As before, we restrict the sample to women between the ages of 25 and 50, and we consider as outcome variables log wages and employment. Treatment is then randomly allocated in the

post-treatment for one of the age groups. These simulations mimic a setting in which there is a policy change that affects individuals from a specific cohort, so we can use other cohorts as a control group. In these simulations, we treat a pair (state $\times$ age) as a group i, and we estimate the treatment effect using a DID model including time fixed effects and state $\times$ age fixed effects. We test the null hypothesis of no effect based on standard errors clustered at the state level. Therefore, we implicitly assume that the error term for individuals in different states are independent.

In these simulations, we now have measures of proximity both between the pre- and post-periods (δ_{year}), and between the treated and control groups (δ_{age}). Therefore, we are able to validate, in this example, the intuition presented in Section 2 that correlated shocks should be relatively less important when either (i) treated and control groups are more similar, or (ii) the pre-treatment period is close to the post-treatment period.

We present in Figure 2 rejection rates for combinations of $(\delta_{\text{year}}, \delta_{\text{age}})$. Interestingly, independently of the outcome variable, rejection rates are generally close to 5% when either δ_{year} *or* δ_{age} is small. For example, even when $\delta_{\text{year}} = 10$, in which case the simulations from Section 3.1 displayed large over-rejection, rejection rates remain close to 5% when δ_{age} is small. Likewise, rejection rates are still close to 5% when we consider $\delta_{\text{age}} = 10$, as long as δ_{year} is small. When *both* δ_{year} and δ_{age} increase, however, we find significant over-rejection. With $(\delta_{\text{year}}, \delta_{\text{age}}) = (15, 15)$, for example, we find rejection rates of around 37% when we consider log wages as outcome variable, and 22% for employment. Overall, these simulation results are consistent with the results derived for the linear factor model in Section 2, in that spatial correlation only poses important problems for inference when there is both significant differences between the post- and pre-treatment periods (δ_{year} is large) *and* significant differences between the treated and control groups (δ_{age} is large).

4 Recommendations

Drawing inference in DID models with large N and fixed T is impossible without imposing restrictions on the variance-covariance matrix of the error. Methods that allow for unrestricted serial correlation essentially collapse the information from the pre- and post-treatment periods, and rely on restrictions in the spatial correlation. Most commonly, it is assumed that errors are independent across groups. Other alternatives would rely on additional information, such as some sort of information on the distance between different groups, or a large number of periods. In many DID applications, however, such distance measures or a large number of periods are not available. Ignoring spatial correlation is, therefore, the only option in many cases.

The results derived in Section 2, and corroborated in simulations with two important datasets in Section 3 (the ACS and the CPS), provide guidelines on how one should proceed in empirical applications to minimize the relevance of spatial correlation. We show that spatial correlation can lead to severe over-rejection when (i) the second moment of the difference in the pre- and post-treatment averages of the common factors is large, and (ii) factor loadings have very different distributions for the treated and control groups or factor loadings exhibit spatial correlation.

Therefore, researchers in this situation should make sure that *at least one* of these conditions are not satisfied (or, at least, minimized) in their applications. For example, in a setting with more than one pre- and post-treatment periods in which there are arguably relevant unobserved common shocks that can affect treated and control groups differently, a longer time series would imply larger second moment for the difference between the pre- and post-treatment averages of the common factors if such common factors are serially correlated. One possible recommendation in this case is to restrict the sample to a few periods before and a few periods after the treatment. In this case, the group fixed effects would absorb more of these common shocks, making inference assuming independent groups more

reliable.[13] Moreover, the time periods that are not used in the estimation can be used for placebo exercises. If spatial correlation remains a problem even after restricting the sample to a few periods, then one should expect over-rejection in placebo regressions using the same number of periods, but before the treatment started. This is true even if the parallel trends assumption is valid. Therefore, such placebo exercises will not only provide evidence regarding the validity of the parallel trends assumption, but will also provide evidence for the validity of the inference procedure.

Alternatively, if the focus of the empirical exercise is to estimate the long-term impacts of a policy change, then it would not be possible to minimize $\mathbb{E}[(\bar{\lambda}_{\text{post}} - \bar{\lambda}_{\text{pre}})^2]$ by restricting the sample to periods around the policy change. Therefore, the effort should be in the direction of guaranteeing that the treated and the control groups are as similar as possible. While, in this case, spatial correlation in the factor loadings could affect inference even if the distribution of factor loadings is the same for treated and control groups, focusing on treated and control groups that are more similar ensures that a larger portion of the spatial correlation is absorbed by the year fixed effects.

5 Conclusion

We analyze the conditions in which correlated shocks pose relevant challenges for inference in DID models. Considering that the spatial correlation structure follows a linear factor model, we analyze the conditions in which (ignored) spatial correlation leads to significant distortions for inference. We present simulations with real datasets that corroborate the conclusions that spatial correlation is less relevant when either the distance between the pre- and post-treatment years is small or the treated and control groups are very similar. The simulation results suggest that the linear factor model analyzed in this paper provides a good approximation to real datasets like the ACS and the CPS. Finally, we provide recom-

[13] Restricting to periods close to the policy change can arguably make the identification assumption of the DID model more plausible as well. However, here we focus on the inference problem, so we always assume that the identification assumption for the DID model is satisfied.

mendations to minimize the relevance of spatial correlated shocks in DID applications.

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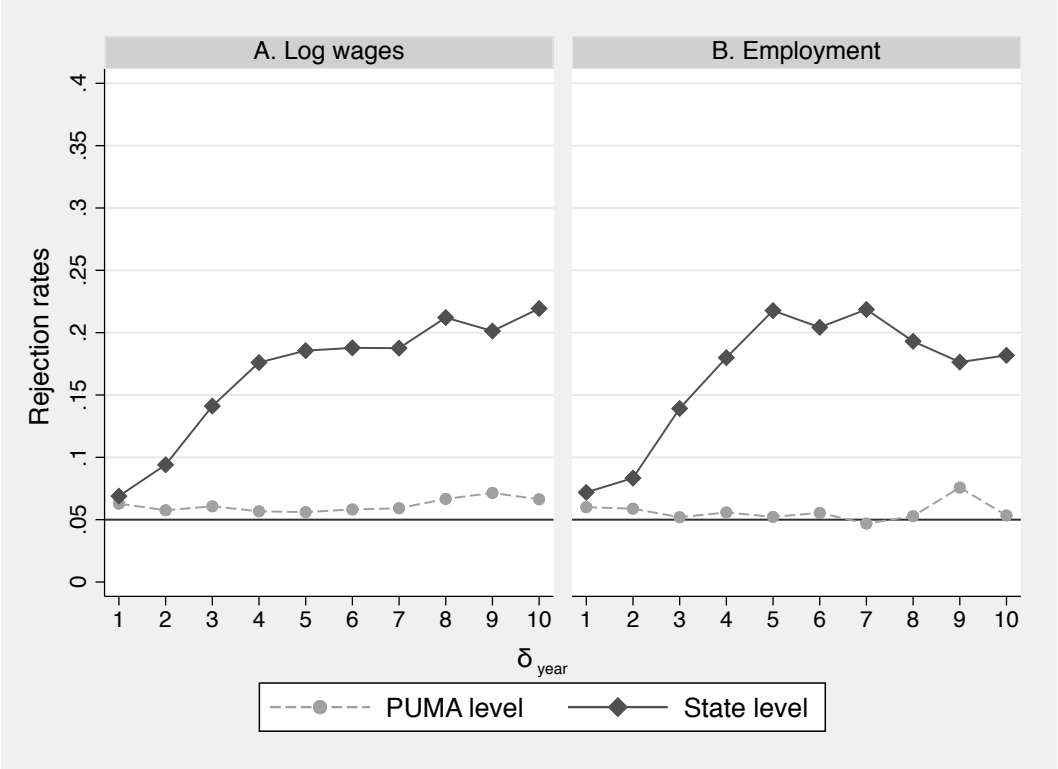
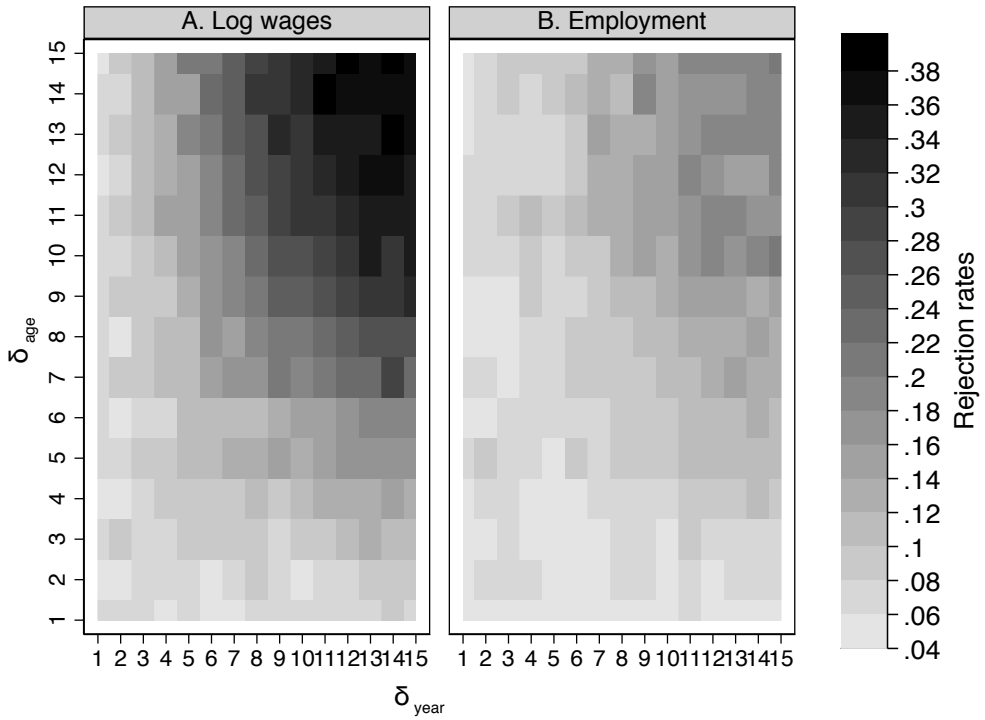
Figure 1: **Simulations with the ACS**



Notes: This figure presents rejection rates for the simulations using ACS data, presented in Section 3.1. Each simulation has two states and two periods. We considered all combination of pairs of states and years. The distance between the pre- and post-treatment periods (δ_{year}) varies from 1 to 10 years. The pre-treatment period ranges from 2005 to 2017-δ_{year}. In the "PUMA level" results, treatment is randomly allocated at the PUMA level, while in the "state level" results, treatment is allocated at the state level. For each simulation, we run a DID regression and test the null hypothesis using standard errors clustered at PUMA level. The outcome variable is log(wages) (subfigure A) and employment status (subfigure B) for women aged between 25 and 50. We consider only simulations with 20 or more treated and control PUMAs.

Figure 2: **Simulations with the CPS**



Notes: This figure presents rejection rates for the simulations using CPS data, presented in Section 3.2. We considered all combination of pairs of years and pairs of ages. The initial time period ranges from 1979 to 2018-δ_{year}. The initial age ranges from 25 to 50-δ_{year}. For each simulation, we run a DID regression and test the null hypothesis using standard errors clustered at the state level. The outcome variable is log(wages) (subfigure A) and employment status (subfigure B) for women with the ages considered in each simulation.

A Monte Carlo Simulations - Two-way Cluster

We present here a small Monte Carlo (MC) simulation to analyze the properties of the two-way cluster in a DID setting. We consider a simple example with 100 groups, half treated and half control, in which $Y_{jt} = \lambda_t^1 + \epsilon_{jt}$ when $j \in \mathcal{T}_1$ and $Y_{jt} = \lambda_t^0 + \epsilon_{jt}$ when $j \in \mathcal{T}_0$. We set $\epsilon_{jt} \sim N(0,1)$, i.i.d. across both j and t. We also set $\mathbb{E}[\lambda_t^w] = 0$ for $w \in \{0,1\}$ and for all t, so the DID estimator is unbiased. However, the λ_t^w generates important spatial correlation that is not absorbed by the time fixed effects.

The λ_t^w follows an AR(ρ) process, with $\rho \in \{0, 0.1, 0.4\}$. We also set $T \in \{2, 10, 100\}$. In all simulations, treatment starts after period $T/2$. Appendix Table 1 present rejection rates based on (i) robust standard errors (with no cluster), (ii) standard errors clustered at group level, and (iii) standard errors clustered at two levels, group and time. As expected, there is a severe over-rejection when we consider inference without clustering, or clustering only at the group level. This happens because this data generating process includes substantial spatial correlation, that is not captured in these variance estimators.

With $T = 2$, using a two-way cluster — at the time and group levels — does not solve the problem. The limitation of the two-way cluster estimator in this case comes from the fact that there is only one post-treatment period and one pre-treatment period. When $\rho = 0$, rejection rates converge to 5% when T increases. When $\rho > 0$, however, there is still over-rejection even when T is large. Moreover, the over-rejection is increasing with ρ.

These results confirm the intuition presented in Section 2, that two-way cluster procedures may underestimate the standard errors, because they fail to take into account correlations between η_{jt} and $\eta_{j't'}$, for $j \neq j'$ and $t \neq t'$. Note that the only case in which such correlation would not appear would be when $\rho = 0$. In this case, we show that two-way cluster would work well when T is large. In contrast, when $\rho > 0$, two-way cluster would still lead to over-rejection even when T is large.

Table 1: **Monte Carlo Simulations - Two-way Cluster**

	No cluster	Cluster at j	Cluster at j and t
	(1)	(2)	(3)
	Panel i: $\rho = 0$		
$T = 2$	0.782	0.682	0.840
$T = 10$	0.760	0.774	0.135
$T = 100$	0.737	0.781	0.049
	Panel ii: $\rho = 0.1$		
$T = 2$	0.753	0.663	0.822
$T = 10$	0.775	0.790	0.147
$T = 100$	0.761	0.803	0.091
	Panel iii: $\rho = 0.4$		
$T = 2$	0.725	0.620	0.804
$T = 10$	0.823	0.845	0.261
$T = 100$	0.839	0.865	0.221

Notes: This table presents rejection rates for the simulations described in Appendix A. Column (1) presents rejection rates based on robust standard errors (with no cluster). Column 2 presents rejection rates based on standard errors clustered at the group level. Column (3) presents rejection rates based on two-way clustered standard errors at the group and time levels.