Model Specification and Spatial Interdependence*

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July 22, 2015

Paper prepared for the 2015 Summer Methods Meeting

*We thank the participants at the 2015 Texas A&M Conference on Innovations in Comparative Political Methodology for their useful feedback on this project. All remaining errors are our own.

Abstract

Researchers now regularly estimate spatial models in applied political science, both to enhance the validity of their "direct" (i.e., non-spatial) covariate effect estimates and to test explicitly spatial theories. While this is a welcome advance over past practices, we worry that much of this first generation of applied spatial research overlooks certain aspects of spatial models. In particular, while different theories imply different spatial-model specifications, statistical tests frequently have power against incorrect alternatives. As a consequence, researchers who fail to discriminate explicitly between the different manifestations of spatial association in their outcomes are likely to erroneously find support for their theoretically preferred spatial process (e.g., diffusion) even where an alternative process instead underlies the association. To help researchers avoid these pitfalls, we elaborate the alternative theoretical processes that give rise to a taxonomy of spatial models, and indicate why, and provide evidence that, these alternative processes are frequently mistaken for one another during conventional hypothesis testing, and suggest a set of strategies for effectively discriminating between various models. We illustrate the utility of this strategy with an application on the relationship between development and democracy.

Cross-sectional, or spatial, interdependence is ubiquitous in the social sciences. In political science, theories indicating that the actions of some units are a function of (i.e., depend upon) those of other units – as they are coerced by, compete with, learn from, and emulate one another – span across the sub-fields and substance of political science.[1] The diffusion of political institutions and policy is well established in American and comparative politics, with units learning from and/or emulating the institutions and instruments of other units. Similarly, the study of political behavior, from voting to violence, is necessarily interdependent as expectations over outcomes is a function of beliefs about the actions of others. The very structure of the global economy indicates the importance of interdependence in the study of comparative and international political economy, evidenced both in deepening economic integration and more frequent policy coordination. More generally, spatial interdependence is present whenever units are affected by the actions, behaviors, and outcomes of other units.

Given the theoretic centrality of spatial interdependence in political science, early work sought to introduce and extend methods for analyzing this dependence directly (Beck, Gleditsch and Beardsley, 2006; Franzese and Hays, 2007). Beyond the classic linear model, statistical methods have been developed for spatial analysis of binary outcomes (Franzese, Hays and Cook, forthcoming), count data (Hays and Franzese, 2009), and durations (Hays and Kachi, 2009; Hays, Schilling and Boehmke, forthcoming). Moreover, researchers have built on the dictum that space is "more than geography," and indicated how the specification of the connectivity matrix itself enables researchers to test a range of political theories (Plümper and Neumayer, 2010). As a result, there has been a proliferation of empirical work in political science which offers theories of, and estimates models testing, spatial interdependence.[2]

While this is a welcome advance over past practices, treating spatial dependence as nuisance or ignoring it altogether, we worry that much of this first generation of applied spatial research does not fully appreciate or is unfamiliar with certain aspects of spatial models. Importantly, distinct spatial model specifications arise from different theoretical explanations of spatial clustering in the outcomes: *i)* endogenous interaction effects (e.g., spillovers in the outcomes), *ii)* exogenous inter-

[1] See Franzese and Hays (2008) for a fuller account of the substantive range of 'spatial' theories advanced in political science.

[2] A trend that is likely to continue grow as these methods become more familiar to researchers and packages facilitating their easy estimation become available in widely used statistical languages.

action effects (e.g., spillovers in the predictors), and *iii)* interactions amongst the residuals (e.g., clustering in the unobservables) (Elhorst, 2010).[3] Problematically, these theoretically distinct statistical models are quite similar, which complicates specification testing (Anselin, 2001; Gibbons and Overman, 2012). Specifically, diagnostic tests have power against incorrect alternatives (testing rejects A in favor of B when, in fact, C is present and not B), making it difficult to statistically distinguish between these various models. To the extent that researchers attach theoretic importance to these specifications and subsequently draw substantively meaningful inferences off these tests, it is important to understand how and the extent to which we can distinguish between these alternatives. Thus, while we can now estimate a variety of spatial models in many different contexts, these ambiguities limit what we can learn from analyses utilizing spatial methods.

To begin to redress these limitations here, we first detail and describe the possible sources of spatial clustering and the econometric models implied when any combination of these sources is present. While a general model that allows for all three sources of spatial clustering is discussed, we show that this model is weakly identified (at best), and cannot, therefore, guide our specification search.[4] Instead, researchers are forced to constrain one of the possible sources of spatial clustering in order to discriminate between the remaining alternatives. While research design or theory should be preferred to justify this constraint, we offer guidance for researchers in situations where these solutions are not present.

In short, we argue that researchers interested in theoretically interpreting spatial parameters should estimate either a spatial Durbin error model (SDEM) or a combined spatial autocorrelation model (SAC), both of which allow us to discriminate between (direct or indirect) spillovers and spatial clustering in unobservables.[5] Importantly, this allows researchers to distinguish those situations where actors are meaningfully influenced by the behavior of one another, as is typically assumed in our theories of interdependence, from those in which they are simply responding to the same unmodeled forces.

[3] Briefly noting the models that these would imply: spatial clustering can manifest due to common unobservables, suggesting an spatial error model (**SEM**), or through exogenous perturbations to the predictors in my neighbor(s), which can influence me directly, motivating a spatially-lagged X (**SLX**) model, or indirectly, by affecting my neighbors outcome and, in turn, my own, as in a spatial auto-regressive (**SAR**) model. Or it might be any combination therein, suggesting one of several more general models.

[4] Note that this precludes a Hendry-like general-to-specific specification search as has been advocated in time series modeling (most recently in political science by (De Boef and Keele, 2008).

[5] Alternatively, we advocate that researchers aiming to minimize bias in the estimates of parameters on non-spatial predictors should estimate a spatial Durbin model (SDM).

Our intention is not to discourage the use of spatial methods, as we feel spatial analysis is necessary and appropriate whenever one has cross-sectional or time-series-cross-sectional observational data. Instead, we are simply advocating researchers exercise greater caution when estimating these models, especially when attempting to articulate and test specific theories of spatial interdependence. Taking 'space' seriously does not simply mean estimating a spatial model, but rather estimating the appropriate spatial model. In the following section, we outline the variety of alternative spatial models, show how easy it is to mistake one for another of these models when drawing inferences, and suggest tests to aid researchers to identify and specify appropriate models for estimation. Subsequently, we evaluate the small-sample performance of these tests under a variety of simulated conditions. Finally, we illustrate our recommended strategy with an application to the relationship between development and democracy.

Specifying Spatial Models

In prior work we have highlighted the substantive/theoretical ubiquity of interdependence across political science. While the emergence of applied spatial research in political science suggests broad agreement on the importance of spatial theories, we believe researchers have potentially been hasty in their application of these methods. Research has quickly turned to articulating and testing specific mechanisms (e.g., emulation vs. learning) and sources (e.g., distance vs. trade) for spatial dependence across a range of issue areas without, we feel, first devoting sufficient attention to the various broader ways in which spatial dependence can manifest in observational data.[6] In short, before discriminating between competing theories of diffusion, researchers must first evidence that there is *any* form of diffusion. To do this, researchers need to be aware of the various possible sources of spatial clustering in their outcomes and adopt models which appropriately nest and test between these competing alternatives. Therefore, we open by discussing the potential sources of spatial heterogeneity, before outlining the spatial econometric models implied by each. [7]

Spatial heterogeneity is present whenever we observe clustering in the outcomes across some set of sample units. By which we mean, when there is non-zero covariance amongst these unit's outcomes:

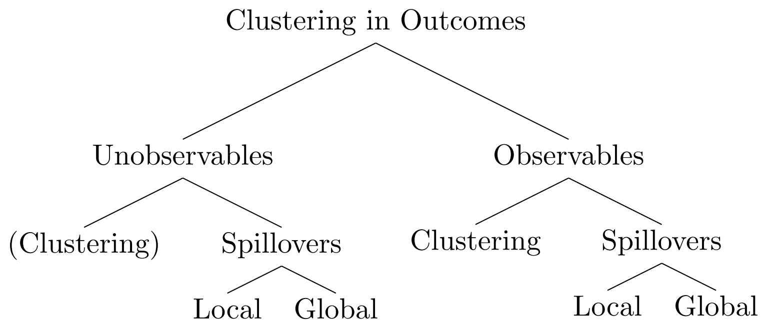
$$\text{cov}(y_i, y_j) = E(y_i y_j) - E(y_i) \times E(y_j) \neq 0 \text{ for } i \neq j \tag{1}$$

That is, whenever variation in the outcome is not randomly distributed across units. This only becomes problematic for non-spatial analysis, however, when the (spatial) distribution of these outcomes cannot be entirely explained by the (spatial) distribution of predictors. In these instances, additional unmodeled factors give rise to the spatial heterogeneity we observe in our outcomes, the failure to account for which potentially threatens the validity of our inferences.

[6] At present, these types of claims are merely our intuitions from reading the literature. In the future we hope to include more systematic discussions on the potential shortcomings of applied spatial research in this respect.

[7] For clarity we confine our attention in this paper to the cross-sectional analysis of continuous data. While many of the themes and topics generalize to a broader set of circumstances, we save peculiarities confronted when dealing with qualitative outcomes and/or panel/time-series-cross-sectional for planned future work.

Figure 1: Manifestations of Spatial Heterogeneity



To elaborate the various manifestations of spatial heterogeneity more fully, consider Figure 1. As we see, clustering in the outcomes arises from spatial effects in the observable and/or unobservable determinants. Specifically, there are two mechanisms which produce spatial heterogeneity: *i)* spatial clustering and/or *ii)* spatial spillovers.[8] As with the outcomes, spatial clustering in the observables (unobservables) occurs when the level, presence, or change of a determinant in one unit is correlated with, but not a function of, the value of that factor in other (spatially proximate) units:

$$y_i = f(x_i, u_i) \text{ and } \operatorname{cov}(x_i, x_j) \neq 0 \text{ for } i \neq j \tag{2a}$$

$$y_i = f(x_i, u_i) \text{ and } \operatorname{cov}(u_i, u_j) \neq 0 \text{ for } i \neq j \tag{2b}$$

Where y is the outcome, x is a predictor, and u is the residual, with subscripts i and j identifying cross-sectional units. Here the predictors and/or residuals are spatially correlated which, in turn, produces in spatial clustering in the outcomes.[9] This does not require, or suggest, interaction between the units, simply that actors possess similar characteristics (ex. natural endowments which span across units) which, when manipulated, cause these unit outcomes to vary concurrently. That

[8] We also refer to these respectively as spatial heterogeneity and spatial dependence.

[9] Generally, this is discussed as the predictor and/or residual being governed by a spatial autoregressive process. However, it may also be that the predictor is a function of spatially dependent factors. The consequences with respect to parameter estimates in the model of y are identical.

is, a common factor in the observables or unobervables results in correlated group effects.[10] For example, policy or technological innovations which change in the costs of inputs or demand (holding supply fixed) impact the revenues of producers of a good even where there is no direct interaction between them.

Alternatively, spatial clustering can arise due to spatial spillovers, when the outcomes of one unit are a function of the outcomes, actions, behaviors of *other* units:

$$y_i = f(x_i, x_j, u_i) \tag{3a}$$

$$y_i = f(x_i, u_i, u_j) \tag{3b}$$

$$y_i = f(x_i, y_j, u_i) = f(x_i, (x_j, u_j), u_i) \tag{3c}$$

This is interdependence and is the spatial effect most commonly assumed by applied researchers. In this case there are spillovers and/or externalities which arise from the observables (Equation 3a), unobservables (Equation 3b), or outcomes (Equation 3c) of other units. Note that here we need not assume that the observables or unobservables are governed by a spatial process, merely that there is cross-unit conditionality where the outcome in i is a function of the observables or unobservables (or both) in unit j. This is the process we believe to be present in our theories of diffusion, strategic decision-making, etc...

While many of our theories suppose interdependence in the outcomes, simultaneity requires that the relation of y_i and y_j operates through the combined spatial effects of the observables (x_j) and unobservables (u_j) Equation 3c. Anselin (2003) discusses that for specification, then, a more fundamental consideration is whether these externalities are global or local (the third dimension of Figure 1 above). That is, whether actors are only affected by the actions of their immediate neighbors, peers, etc..., as assumed by a local process, or, as in Tobler's oft-used expression "everything is related to everything," suggesting a global process. Perhaps more clearly, whether spillovers in the observables and unobservables in my neighbors affect me *directly*, or whether they

[10] More formally, Andrews (2005)'s says common-factor residuals and predictors satisfy the following:

$$u_i = C_g' u_i^*$$
$$x_i = C_g' x_i^*$$

where C_g is a random common (e.g., group) factor with random factor loadings u_i^* & x_i^*. Therefore, if units i and j are each members of group g they are jointly impacted by the respective loading.

affect me *indirectly* through my neighbors outcomes.

Our theoretical beliefs about which combination of these spatial effects produces spatial clustering in the outcomes and how, imply different econometric specifications. Specifically, we have discussed three relevant dimensions which should inform spatial specification: *i)* whether spatial heterogeneity in the outcome is caused by observable or unobservable effects (or both), *ii)* whether these spatial effects arise from clustering or spillovers (or both), and *iii)* if spillovers, whether they are local or global.[11] Table 1 lists the spatial models most commonly discussed in the literature.[12]

Table 1: Spatial Econometric Models

Name	Structural Model	Restrictions
General Nesting Model	$y = \rho Wy + X\beta + WX\theta + u, u = \lambda Wu + \epsilon$	none
Spatial Durbin Error Model	$y = X\beta + WX\theta + u, u = \lambda Wu + \epsilon$	$\rho = 0$
Spatial Autocorrelation Model	$y = \rho Wy + X\beta + u, u = \lambda Wu + \epsilon$	$\theta = 0$
Spatial Durbin Model	$y = \rho Wy + X\beta + WX\theta + \epsilon$	$\lambda = 0$
Spatial Autoregressive	$y = \rho Wy + X\beta + \epsilon$	$\lambda = \theta = 0$
Spatially Lagged X's	$y = X\beta + WX\theta + \epsilon$	$\rho = \lambda = 0$
Spatial Error Model	$y = X\beta + u, u = \lambda Wu + \epsilon$	$\rho = \theta = 0; \lambda = -\rho\beta$
(Spatial) Linear Model	$y = X\beta + \epsilon$	$\rho = \lambda = \theta = 0$

Beginning with the most restrictive of these models, the non-spatial linear regression model assumes that spatial heterogeneity in the outcomes is entirely a function of spatial heterogeneity in the predictors:

$$\mathbf{y} = \mathbf{X}\beta + \boldsymbol{\epsilon} \tag{4}$$

That is, to account for clustering in outcomes, we need simply to include appropriate predictors ($\mathbf{X}$) as regularly done in non-spatial analysis. This is a point we emphasize as it seems to be misunderstood it in the applied literature.[13] Moreover, it underscores the importance of model

[11] This is analogous to (Anselin, 2003)'s two-dimensional taxonomy for externalities.

[12] Note that this is a partial list. In the models we list and discuss here unobservables are always exhibit global spatial autocorrelation, while observables can be either global or local. In future iterations of our paper we plan to fully enumerate the models which arise from different combinations of the dimensions discussed above (as we begin in **??** in the Appendix). For now we simply build upon the models most frequently discussed in the literature. In particular, those advocated by LeSage and Pace (2009) and Elhorst (2010).

[13] For example, Buhaug and Gleditsch (2008) argue that conflicts cluster in space because the characteristics that produce conflict also cluster in space. If correct, this would be captured simply via the inclusion of the relevant country-characteristics. Instead, they estimate a model with spatially-lagged independent variables (e.g., democracy in contiguous countries, etc...), these $WX_{j\neq i}$'s actually test a different argument as we discuss later.

specification more generally when undertaking spatial analysis, as misspecified models – those omitting relevant spatially-clustered predictors – will exhibit spatial dependence in the residuals (and, in turn, spatially lagged dependent variables). As such, better specified models are one obvious solution when confronting spatially clustered residuals.[14]

In estimating these models, researchers assume a spherical error variance-covariance matrix (and, by extension, that $\rho = \lambda = \theta = 0$), that is, that the residuals are not spatially correlated. This can be easily tested through a variety of post-estimation diagnostic tests, including the familiar Moran's I and Lagrange Multiplier tests (Franzese and Hays, 2008). Should these test reject the null, indicating spatial correlation in the residuals, further remedies are needed to avoid inefficiency and possible bias in our parameter estimates. Most applied spatial work in political science engages in this type of exploratory spatial analysis, including these results to justify the use of further spatial methods. However, these tests merely suggest *a* spatial process and are not otherwise helpful for making specification choices from among the broad class of possible spatial models.

Of these models, the most widely discussed have been the spatial error model (**SEM**), the spatial lag model (**SAR**), and, more recently, the spatially-lagged X model (**SLX**). Each assumes that the spatial heterogeneity in the outcomes arises from a single source, constraining the other possibilities to zero. Spatial error models imply that the pattern of spatial dependence is attributable to unmeasured covariates that are orthogonal to the included regressors, resulting in a non-spherical error variance-covariance matrix.[15] Under these conditions, parameter estimates are unbiased but inefficient, while standard error estimates are inaccurate. This can be remedied by accounting for the spatial structure of the residuals, as done in the **SEM**[16]:

$$\mathbf{y} = \mathbf{X}\beta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda \mathbf{W}\mathbf{u} + \boldsymbol{\epsilon} \tag{5}$$

[14]As always, the distribution of our residuals – spatial or otherwise – is entirely a function of the specification of the systematic component of our model.

[15]In the remaining models we will continue to assume that the residuals are orthogonal after the appropriate spatial specification is realized. The possible endogeneity of the predictors is an issue that has not been discussed at length in the spatial econometric literature to date, and typically only with respect to estimation when discussed at all. In related work we have began to explore how such endogeneity further impairs specification searches.

[16]Spatial heterogeneity in the errors can be dealt with in a number of additional ways including the use of robust standard errors (Driscoll and Kraay, 1998).

where $\mathbf{W}$ is an $N \times N$ connectivity matrix with elements w_{ij} indicating the relative connectivity (e.g., relationship) from unit j to unit i and λ indicating the strength of these effects. Using the dimensions given above, this model assumes global spillovers in the unobservables, that is, that the residuals are governed by a spatial autoregressive process.[17] However, this will also be the preferred specification when we believe there is clustering in the unobservables. Unlike observable predictors, we have no means of introducing this heterogeneity into the systematic component of the model directly, but accounting for the structure of the residuals should still provide some insurance against inefficiency resulting from this type of clustering and produce more accurate standard error estimates.[18]

If, instead, researchers believe that there are spillovers in the observables, one of the other single-source spatial models should be estimated in order to: *i)* avoid bias in the non-spatial effects and *ii)* obtain estimates of these spatial (spillover) effects. Where theory suggests these spillovers/externalities are local, the SLX model should be preferred:

$$\mathbf{y} = \mathbf{X}\beta + \mathbf{W}\mathbf{X}\theta + \mathbf{u} \tag{6}$$

Alternatively, where theory indicates these spillovers/externalities are global and in the outcome, the widely-used **SAR** model is called for[19]:

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{u} \tag{7}$$

[17]The local (e.g., moving average) analog to this model would be given as:

$$\mathbf{y} = \mathbf{X}\beta + \mathbf{u} + \gamma\mathbf{W}\mathbf{u}$$

where the residual is decomposed into a spatial and non-spatial component. However, unlike the more common **SEM**, there is not autoregression in the residuals and, therefore, there is no inverse required in the reduced form (as noted by (Anselin, 2003). This model is not widely used in practice, likely because researchers have little information to justify this constraint, instead preferring the more general **SEM** model.

[18]Note that this is not true of panel or time-series-cross-sectional data where we can use spatial fixed effects to account for time-invariant heteorgeneity in the unobservables directly.

[19]In reality, the **SAR** model suggests global spillovers in both the observables and unobservables as we can see from the reduced-form given below.

This will likely be familiar to most readers, as it has quickly become the workhorse model of applied spatial work in political science (and elsewhere). While both **SLX** and **SAR** models allow for spillovers in observables they differ over whether they model these as local or global processes, as discussed above, and whether there are spatial effects in the unobservables. More theoretically, whether we believe there is cause to understand the spillovers of the observables as direct, as is more likely with social aggregates, or indirect, as is more likely with individual decision makers.[20]

We noted above that the similarity of these models creates challenges for diagnostic tests. While this may not be obvious from the structural forms given in Equation 5 - Equation 7, we re-express these to indicate the similarities. Taking the reduced form of u, substituting, and rearranging terms, the **SEM** model becomes:

$$\mathbf{y} = \lambda \mathbf{W}\mathbf{y} + \mathbf{X}\beta - \lambda \mathbf{W}\mathbf{X}\beta + \mathbf{u} \tag{8}$$

The similarities between the **SEM** model and the **SLX** model (given in Equation 6) and the **SAR** model (given in Equation 7) are now readily apparent, as it composed of a spatial lag of the outcomes ($\lambda\mathbf{W}\mathbf{y}$) and spatial lags of the predictors ($\lambda\mathbf{W}\mathbf{X}\beta$). Similarly, taking the reduced form of the **SAR** model in Equation 7 and it's expansion produces:

$$\mathbf{y} = (\mathbf{I} - \rho\mathbf{W})^{-1}(\mathbf{X}\beta + \mathbf{u}) \tag{9a}$$

$$\mathbf{y} = \mathbf{X}\beta + \rho\mathbf{W}\mathbf{X}\beta + \rho^2\mathbf{W^2}\mathbf{X}\beta \ldots + \mathbf{u} + \rho\mathbf{W}\mathbf{u} + \rho^2\mathbf{W^2}\mathbf{u} \ldots \tag{9b}$$

Again the similarities between the **SAR** model and the **SLX** model are now apparent, with the only differences being the higher-order polynomials of the spatial lag of **X** and the spatial error process. As a consequence, spatial spillovers/externalities in the observable predictors will result in a rejection of the null for the spatial effect parameter in *any* of these single-source models.

To ward against this possibility, spatial econometricians have increasingly recommended several

[20] This is not, of course, a strict distinction as individuals can have direct effects as well.

two-source models:

$$\textbf{SDM: } \mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{W}\mathbf{X}\theta + \boldsymbol{\epsilon} \tag{10}$$

$$\textbf{SAC: } \mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\epsilon} \tag{11}$$

$$\textbf{SDEM: } \mathbf{y} = \mathbf{X}\beta + \mathbf{W}\mathbf{X}\theta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\epsilon} \tag{12}$$

And a more general model still, the so-called General Nesting Spatial Model (**GNS**):

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{W}\mathbf{X}\theta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\epsilon} \tag{13}$$

which imposes no constraints on the three spatial parameters (ρ, λ, θ).[21] Given that this model subsumes all the alternatives presented thus far, one could then engage in a Hendry-like general-to-specific specification search (Hendry, 1995). Thereby, avoiding the pitfalls encountered when adopting a specific-to-general model. While this strategy has much to recommend it, and is common place in the time series literature, there are two problems which prevent simply adopting an approach.

First, the **GNS** model is weakly identified. As discussed in Gibbons and Overman (2012), the **GNS** is the analog to Manski (1993)'s well-known linear-in-means neighborhood effects model:

$$y = \underbrace{\rho_1 E[y|a]}_{\text{End. Effects}} + x\beta + \underbrace{E[x'|a]\gamma}_{\text{Exo. Effects}} + v, \text{ where } u = \underbrace{\rho_2 E[u|a]}_{\text{Corr. Errors}} + \epsilon \tag{14a}$$

$$y = x\beta + E[x'|a]\frac{(\rho_1\beta + \gamma)}{1 - \rho_1} + \frac{\rho_1}{1 - \rho_1}E[v|a] + u \tag{14b}$$

This parallel should raise some red flags given the well-known identification problems of the Manski model. As indicated in Equation 14b, it is impossible to separately identify the endogenous and exogenous spatial effects.[22] With spatial econometric methods, however, one does not estimate

[21] We pause again to note that each of these models assumes a global autocorrelation in the residuals.

[22] Instead all that is identified is the total spillover effect, this is Manksi's reflection problem.

"neighborhood" effects. Each unit in a sample is known to be connected to others through **W**, and this matrix almost always provides more information than neighborhood membership. Within a given "neighborhood," there are first, second, and higher order neighbors, for example.[23] As a result, spatial econometric models are *usually* able to use the pre-specification of **W** to achieve identification in most cases.[24] However, in the case of the **GNS** model identification stills proves elusive even after imposing these structural assumptions. After some algebraic manipulation the **GNS** model given in Equation 13 can be re-written as:

$$\mathbf{y} = (\rho + \lambda)\mathbf{W}\mathbf{y} - \rho\lambda\mathbf{W^2}\mathbf{y} + \mathbf{X}\beta + (\theta - \lambda\beta)\mathbf{W}\mathbf{X} - \lambda\theta\mathbf{W^2}\mathbf{X} + \boldsymbol{\epsilon} \tag{15}$$

where the spatial parameters are weakly identified by the second order terms in the polynomial. Another way to see the nature of the identification problem is to consider, as an example, two observationally-equivalent DGPs: case #1) $\rho = \alpha, \lambda = 0, \theta = \delta$ (i.e., no spatial effects in the unobservables) and case #2) $\rho = 0, \lambda = \alpha, \theta = 0, -\lambda\beta = \delta$ (i.e., spatial error autocorrelation). The likelihood surface, in this instance, possesses two global maxima. Though simple substitution would suggest that these imply the same model, the spatial Durbin model, they are theoretically distinct with the former indicating interdependence through the residuals ($\epsilon_i \rightarrow u_i \rightarrow u_j \rightarrow y_j$) and the latter indicating interdependence through the outcomes ($\epsilon_i \rightarrow y_i \rightarrow y_j$). Our inability to distinguish between these alternatives means that the nature of the dependence is poorly identified. As such, a strategy that begins with the estimation of this model offers us little in our specification search. Analogous problems plague specific-to-general searches with test statistics "robust" to the remaining two sources (discussed at greater length below). In short, it is difficult to simultaneously account for all three sources of clustering statistically.

How, then, should researchers interested in undertaking spatial analysis proceed? Broadly there are two strategies one can pursue. The first is to constrain one of the spatial parameters to zero, thereby allowing for identification of the remaining free parameters and estimation of the relevant

[23]This should suggest the importance of **W**, as the degree to which the weights matrix accurately reflects the true spatial relationships among the units is paramount. Both our ability to detect whether spatial dependence is present and identify which source of spatial effects are present depend upon the accuracy of **W**.

[24]In this instance the spatial analog is:

$$\mathbf{y} = \mathbf{x}\beta + \mathbf{W}\mathbf{x}(\beta\boldsymbol{\rho_1} + \gamma) + \boldsymbol{\rho_1}\mathbf{W}\mathbf{x}(\beta\boldsymbol{\rho_1} + \gamma) + \boldsymbol{\rho_1^2}\mathbf{W^2}\mathbf{x}(\beta\boldsymbol{\rho_1} + \gamma) + \ldots + \mathbf{u}$$

two-source model.[25] The second is to add additional structure to the model in the form of unique weights matrices for the observables and unobservables. While possible, this second approach is unappealing to us as a general strategy given that there is no reason to think we would have strong prior information to indicate that unobserved effects are spatially governed in a manner distinct from observed predictors.[26] As such, we focus on evaluating the efficacy of this first strategy, constraining one or more parameters, as a more general approach.

Implicitly, this is the approach advocated by most spatial econometricians in the literature currently. However, few offer clear guidance as to why one should be preferred over the remaining alternatives. To date, researchers have received conflicting advice over which model should be preferred as a general model, with some strongly advocating the **SDM** and others the **SAC**. [27] Moreover, no work with which we are familiar has systematically evaluated the small sample performance of these models when data does not satisfy the constraints assumed by the statistical model.

Instead of simply advocating one model over another, as commonly done, we believe researchers should adopt a more systematic approach to motivating these constraints. First, one could use research design, such as natural experiments, to eliminate one (or more) of the three possible sources. This is the strategy suggested by (Gibbons and Overman, 2012), both to evade the issues which arise from the unidentified **GNS** and avoid models only identified off structure (e.g., spatial econometric models generally). Focusing exclusively on those contexts where natural experiments are available, however, bounds the range of issues that can be studied. As such, we consider

[25] While we do not fully elaborate it here, the intuition, beyond simply being identified, as to why two parameter specification checks work well follows directly from Anselin et al. (1996)'s robust Lagrange Multiplier tests (here given for spatial error):

$$LM^*_\lambda = \frac{\left(\hat{\varepsilon}'\mathbf{W}\hat{\varepsilon}/\hat{\sigma}^2_\varepsilon - \mathbf{\Psi}\hat{\varepsilon}'\mathbf{W}\mathbf{y}/\hat{\sigma}^2_\varepsilon\right)^2}{T\left[1-\mathbf{\Psi}\right]}$$

Which treats ρ – the spatial heterogeneity attributable to the spatial lag of the outcomes – as a nuisance parameter, adjusting for its effect on the likelihood. In effect, removing the portion of $\text{cov}(\hat{\boldsymbol{\varepsilon}}, \boldsymbol{W}\hat{\boldsymbol{\varepsilon}})$ that can be attributable to $\text{cov}(\hat{\boldsymbol{\varepsilon}}, \mathbf{Wy})$. Equivalently we could construct additional pre-specification tests (or simply estimate models) which hold fixed the effect of one alternative while evaluating the second.

[26] Even when these exist, the high degree of correlation between the weights matrices would simply trade an unidentified model for a weakly identified one. The benefits of this with respect to statistical power are unclear, this is an issue we plan to evaluate experimentally in future iterations of the paper.

[27] (Elhorst, 2010, pg. 10) offers a fun account which illuminates this discord: "In his keynote speech at the first World Conference of the Spatial Econometrics Association in 2007, Harry Kelejian advocated [**SAC** models], while James LeSage, in his presidential address at the 54th North American Meeting of the Regional Science Association International in 2007, advocated [**SDM**] models."

approaches where such strategies are not possible.

A natural alternative, in such instances, is to use theory to guide these constraints. Where theory can eliminate one of the possible sources, we should be more confident in our selection of the appropriate two-source model. Even where we do not have strong theory to confidently eliminate one of these sources, all is not lost. Instead, our third alternative, is to use the aim of the research to guide the model selection. That is, where researchers are principally interested in obtaining unbiased estimates of the non-spatial parameters, the spatial Durbin model should be preferred. This should provide the most insurance against possible omitted variable bias by explicitly introducing both forms of observable spillovers into the systematic component of the model. However, where researchers are explicitly interested evaluating spatial theories, we believe one of the other two-source models (**SAC** or **SDEM**) are best. Each frees one parameter to capture spillovers in observables (either ρ or θ) while accounting for spatial effects in the unobservables (λ). To us, distinguishing between spatial spillovers in observables and spatial effects in unobservables is the most significant consideration. Importantly, this will help prevent researchers from drawing erroneous conclusions about diffusion and/or spillovers where none exists, that is, where spatial clustering in the outcomes is determined in whole or part by spatial effects in unobservables. Where such spillovers still find support, we have only lost the ability to statistically and empirically distinguish whether they were truly global or local. A cost which, by comparison, seems less severe.

Using either theory or research focus to guide specification, however, also naturally risks a much more problematic cost: being incorrect. This can occur in three ways with estimation of two-source spatial models. First, the truth is all three spatial effects. Second, the truth is two sources and our statistical model imposes the wrong constraint (e.g., we estimate the wrong model). In either, we risk bias in the estimates of the included spatial and non-spatial parameters, as is always the case with spatially misspecified models. The third way we can be wrong is when the truth is a single source of spatial effects and we estimate the wrong model. That is, the model which does not include the true spatial effect in either its two included spatial parameters.[28] In these instances, we will incorrectly find support for *both* included spatial parameters even though *neither* is the true spatial effect.

[28]We would expect to be fine in the case where we included the true spatial parameter accurately and included an additional irrelevant spatial parameter.

This has been well established for the **SEM** model, which can be re-expressed as a spatial Durbin model (noted above and rexpressed here):

$$\mathbf{y} = \mathbf{X}\beta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\epsilon} \tag{16a}$$

$$\mathbf{y} = \lambda\mathbf{W}\mathbf{y} + \mathbf{X}\beta - \lambda\mathbf{W}\mathbf{X}\beta + \boldsymbol{\epsilon} \tag{16b}$$

In this case, we can test the common factor restriction of (Burridge, 1981), $\theta = -\lambda\beta$, after estimating to SDM to evaluate whether it can be constrained to the SEM. Similarly, we can see that the SAR model can be re-expressed as a higher-order variation of the SDEM:

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{u} \tag{17a}$$

$$\mathbf{y} = \mathbf{X}\beta + \rho\mathbf{W}\mathbf{X}\beta + \rho^2\mathbf{W}^{\mathbf{2}}\mathbf{X}\beta + \ldots + (\mathbf{I} - \rho\mathbf{W})^{-1}\boldsymbol{\epsilon} \tag{17b}$$

That is, the only difference between the SDEM and the SAR model is the higher-order polynomials of **WX** in the latter. [29] Finally, while expressing the relationship between the SLX and the SAC model is not as straight forward, the basic intuition for why a true effect of θ in the SLX model would cause significant findings for both ρ and λ in the SAC model extends the above discussions in that the estimates of each is a function of **WX**:

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\epsilon} \tag{18a}$$

$$\mathbf{y} = (\lambda + \rho)\mathbf{W}\mathbf{y} + \lambda\rho\mathbf{W}^{\mathbf{2}}\mathbf{X} + \mathbf{X}\beta - \lambda\mathbf{W}\mathbf{X}\beta + \boldsymbol{\epsilon} \tag{18b}$$

$$\mathbf{y} = \mathbf{X}\beta + \rho\mathbf{W}\mathbf{X}\beta + \rho^2\mathbf{W}^{\mathbf{2}}\mathbf{X}\beta + \ldots + (\mathbf{I} - \rho\mathbf{W})^{-1}(\mathbf{I} - \lambda\mathbf{W})^{-1}\boldsymbol{\epsilon} \tag{18c}$$

[29] While this does not as easily permit a Burridge type restriction, we could specify a higher-order SDEM model and then perform and F-test on the parameters of these higher-order polynomials. Rejection would indicate that the standard SDEM model is insufficient. To be clear, we would not be able to reject the possibility that the truth is some higher-order SDEM from this analysis. This problem is analogous to that discussed by Beck (1991) in the time-serial context where the AR(1) model can be closely approximated by a higher order MA model. While we have no information to discriminate the two, researchers in these situations should typically prefer the more parsimonious model.

Both this and the SAR-SDEM relation do not allow for a simple common factor restriction test (as in the SEM-SDM case). Therefore, rather than test constraints on parameters, we are currently pursuing tests which allow us to base comparisons on the models. That is, using Vuong (1989) 'closeness' test, a likelihood-ratio test for non-nested models, we can evaluate whether the two models differ significantly in their ability to explain the data. In our context, a failure to reject the null hypothesis would indicate support for the more parsimonious single-source model. (Note: we are still in the process of developing and assessing the performance of such a test.)

Therefore, in the next section we explore the consequences of imposing the wrong constraints when estimating spatial models.

Monte Carlo Analysis

In our simulations, we explore the possibility of detecting interdependence in outcomes and spillovers from covariates in cross-sections of data when there is spatial clustering in both observables and unobservables using the relevant models from Table 1. We define clustering as a common spatial or group fixed effect. Substantively, clustering differs from both interdependence and spillovers in that changes in covariates and disturbances inside one unit do not cause outcomes to change in other units. The DGP is:

$$\mathbf{y} = \rho \mathbf{W} \mathbf{y} + \beta \mathbf{x} + \theta \mathbf{W} \mathbf{x} + \mathbf{u}$$

where $\mathbf{y}$ is an $N \times 1$ vector of outcomes, $\mathbf{x}$ is an $N \times 1$ covariate vector, $\mathbf{u}$ is an $N \times 1$ vector of disturbances, $\mathbf{W}$ is an $N \times N$ spatial weights matrix, ρ is the spatial interdependence parameter, β is the direct effect parameter, and θ is the spatial spillover parameter.

The individual elements of the vectors $\mathbf{x}$ and $\mathbf{u}$ are generated as

$$x_{ig} = \eta_g^x + \varepsilon_{ig}^x \text{ and } u_{ig} = \eta_g^u + \varepsilon_{ig}^u$$

where x_{ig} and u_{ig} refer to the covariate and disturbance for unit i in spatial group g, η_g^x and η_g^u are the common spatial effects, distributed as standard normal variates, and ε_{ig}^x and ε_{ig}^u are the unit-specific components of the covariate and disturbance, which are also distributed as standard normal variates.

The spatial weights matrix identifies intragroup connectivity and takes the form

$$\mathbf{W} = \begin{bmatrix} \mathbf{W}_1 & 0 & \cdots & 0 \\ 0 & \mathbf{W}_2 & \cdots & 0 \\ \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & \cdots & \mathbf{W}_G \end{bmatrix}$$

Thus, the complete weights matrix, has a block diagonal structure for **G** groups, when the units or individuals in the sample are stacked by groups. We set the number of groups (**G**) to 15, the number of members in each group (n_g) to 20, and the degree of intra-group connectivity at 40%. We assume the connectivity weights are uniform and sum to one. That is, the weights are $1/n_c$, where n_c is the number of intra-group connections. This weights matrix is motivated by the fact that we usually do not know the relevant spatial groups. Should North Africa be grouped with Sub-Saharan Africa? Does Pennsylvania belong in the northeast or midwest? We do however observe intragroup relationships such as contiguity.

We evaluate the small sample performance of the **SAR**, **SAC**, **SLX**, **SDEM**, **SDM**, and **GNS** models under four experimental conditions: 1) no spillovers and no interdependence ($\theta = 0, \rho = 0$), 2) spillovers and no interdependence ($\theta = 0.2, \rho = 0$), 3) no spillovers and interdependence ($\theta = 0, \rho = 0.2$), 4) both spillovers and interdependence ($\theta = 0.2, \rho = 0.2$). We set $\beta = 2$ in all of our experiments. Furthermore, clustering in the covariate and disturbances, as generated above, are present in all experiments.

Table 2 provides the ML estimates for the direct covariate effect ($\hat{\beta}$). It is notable that all of the models perform reasonably well across the experiments with the exception of **SAR**. The direct effect is underestimated on average with this model. Clustering in the disturbances strengthens their correlation with the spatial lag, above and beyond the correlation that exists when the structural disturbances are i.i.d. This generates an inflating simultaneity bias in $\hat{\rho}$ that induces an attenuating bias in $\hat{\beta}$. Moreover, estimation using the SAR model performs relatively poor in root mean squared error terms (largely a function of the bias), and the standard error estimates are overconfident.

Table 2: ML Estimates of Direct Covariate Effect ($\hat{\beta}$, $\beta = 2$, $N = 300$, 1,000 Trials)

	(1) $\theta = 0, \rho = 0$	(2) $\theta = 0.2, \rho = 0$	(3) $\theta = 0, \rho = 0.2$	(4) $\theta = 0.2, \rho = 0.2$
SAR				
Bias	-0.18	-0.17	-0.20	-0.18
RMSE	0.21	0.19	0.22	0.20
Overconfidence	1.47	1.42	1.47	1.41
SAC				
Bias	-0.01	-0.01	-0.01	-0.01
RMSE	0.07	0.07	0.07	0.07
Overconfidence	1.05	1.05	1.05	1.06
SLX				
Bias	0.00	0.00	0.01	0.01
RMSE	0.07	0.07	0.08	0.08
Overconfidence	0.95	0.95	0.92	0.92
SDEM				
Bias	0.00	0.00	0.00	0.00
RMSE	0.06	0.06	0.06	0.06
Overconfidence	1.05	1.05	1.03	1.03
SDM				
Bias	0.00	-0.01	-0.03	-0.04
RMSE	0.06	0.06	0.07	0.08
Overconfidence	1.06	1.06	1.05	1.04
GNS				
Bias	0.00	0.00	-0.01	-0.01
RMSE	0.06	0.06	0.06	0.06
Overconfidence	1.03	1.04	1.06	1.07

The results for the spatial interdependence parameter estimates ($\hat{\rho}$) are presented in Table 3. Here we see the inflation bias driven by spatial clustering in the disturbances. The standard error estimates are highly overconfident as well. Across all four experiments, the standard deviation in the sampling distribution for $\hat{\beta}$ is more than double the size of the average estimated standard error. The combination of an inflation bias and overconfident standard errors means the rejection rate is extremely high when the null hypothesis is true. In other words, estimation with SAR produces a high rate of false positive rejections.

Estimation with **SAC** does better than with **SAR** in terms of bias, root mean squared error performance, and standard error accuracy. The improvement stems from the fact that **SAC** accounts for the clustering in the disturbances by allowing them to follow a spatial AR process.

Table 3: ML Estimates of Interdependence ($\hat{\rho}$, $\beta = 2$, $N = 300$, 1,000 Trials)

	(1) $\theta = 0, \rho = 0$	(2) $\theta = 0.2, \rho = 0$	(3) $\theta = 0, \rho = 0.2$	(4) $\theta = 0.2, \rho = 0.2$
SAR				
Bias	0.29	0.32	0.23	0.45
RMSE	0.30	0.33	0.24	0.46
Overconfidence	2.24	2.17	2.25	2.18
False Positives (.10 level)	97.4%	99.2%		
Power (.10 level)			99.9%	99.9%
SAC				
Bias	-0.08	0.01	-0.09	0.20
RMSE	0.12	0.10	0.14	0.22
Overconfidence	1.16	1.21	1.19	1.24
False Positives (.10 level)	28.8%	18.2%		
Power (.10 level)			36.9%	72.6%
SDM				
Bias	0.70	0.70	0.56	0.76
RMSE	0.70	0.70	0.56	0.76
Overconfidence	1.38	1.38	1.33	1.36
False Positives (.10 level)	100%	100%		
Power (.10 level)			100%	100%
GNS				
Bias	0.11	-0.03	-0.30	-0.22
RMSE	0.80	0.79	0.75	0.66
Overconfidence	7.06	6.20	4.75	3.84
False Positives (.10 level)	97.9%	97.5%		
Power (.10 level)			92.2%	89.0%

This is not a perfect representation of the true DGP, but the AR specification is easy to implement when the spatial groups are not known, and there are substantial gains from doing so. The **SAC** provides protection against false positive rejections. The cost is a loss of power, which is large in column (3). However, the rate at which the SAC model correctly rejects the null hypothesis is sensitive to experimental conditions. If we increase the strength of interdependence, for example, the power will improve. Both the **SDM** and **GNS** models perform poorly, producing biased estimates and overconfident standard errors.

Table 4 provides the ML estimates for the spillover parameter ($\hat{\theta}$). Whenever there is no interdependence ($\rho = 0$), estimates from the **SLX** model do well in terms of bias, but not in terms of efficiency. The variance in the sampling distribution is relatively large. Also, the standard errors are highly overconfident. Across the experiments, the standard deviations for the empirical sampling distributions are about 2.5 times large than the average estimated standard error. Because of the overconfident standard errors, the **SLX** model produces an high rate of false positive rejections, even when where is no interdependence. When there is interdependence, omitted variable bias causes the performance of **SLX** to deteriorate. Similar to the **SAC** improvement over **SAR**, estimation with **SDEM** does better than with **SLX** in terms of bias, root mean squared error performance, and standard error accuracy. **SDEM** provides some protection against false positive rejections. The cost for this protection is a loss of power. Again, both the **SDM** and **GNS** models perform poorly, producing biased estimates and overconfident standard errors.

Table 4: ML Estimates of Spillover Effect ($\hat{\theta}$, $\beta = 2$, $N = 300$, 1,000 Trials)

	(1) $\theta = 0, \rho = 0$	(2) $\theta = 0.2, \rho = 0$	(3) $\theta = 0, \rho = 0.2$	(4) $\theta = 0.2, \rho = 0.2$
SLX				
Bias	0.00	0.00	0.46	0.49
RMSE	0.32	0.32	0.60	0.63
Overconfidence	2.41	2.41	2.62	2.62
False Positives (.10 level)	47.8%		74.8%	
Power (.10 level)		56.6%		89.7%
SDEM				
Bias	0.01	0.01	0.41	0.42
RMSE	0.21	0.21	0.46	0.47
Overconfidence	1.12	1.12	1.08	1.08
False Positives (.10 level)	13.6%		70.0%	
Power (.10 level)		31.6%		92.1%
SDM				
Bias	-1.60	-1.70	-1.55	-1.64
RMSE	1.61	1.71	1.56	1.65
Overconfidence	1.29	1.31	1.28	1.32
False Positives (.10 level)	99.9%		99.9%	
Power (.10 level)		99.9%		99.9%
GNS				
Bias	-0.23	0.05	0.58	0.81
RMSE	1.84	1.89	1.80	1.77
Overconfidence	6.16	5.71	4.59	3.89
False Positives (.10 level)	97.6%		97.7%	
Power (.10 level)		98.9%		98.7%

To sum, clustering in unobservables complicates our ability to detect interdependence in outcomes and spillovers from observable covariates in cross-sections of data. When one suspects both interdependence and spillovers, it would seem natural to estimate either the **SDM** or **GNS** models, but this is not advisable. The **SDM** allows for both interdependence and spillovers, but it ignores the clustering in disturbances. This omission generates a simultaneity bias in the estimates for the interdependence parameter (ρ) and induces a bias in the estimates for the spillover parameter (θ). Why not allow for spatial correlation in the disturbances in addition? This is what the **GNS** does. Unfortunately, this model is weakly identified (at best) and does not perform any better than **SDM**. Both models frequently produce statistically significant estimates of interdependence and spillover parameters with the wrong sign!

When one suspects clustering on unobservables, it does not seem advisable to estimate either of these models. Instead estimating either **SAC** or **SDEM** would seem to be a more prudent strategy (and preferable to **SAR** and **SLX** as well). While design should be leveraged to select between these models where possible, often this will not be an option and researchers will instead have to eliminate either interdependence or spillovers on theoretical grounds.[30] This makes it difficult to offer a general prescription, however, the nature of ones data will often be instructive. When the outcomes of interest are social aggregates – such as unemployment rates, crime rates, or the aggregate demand for cigarettes (these are common outcomes in the spatial econometrics literature) – interdependence is non-sensical. The unemployment rate in one locality does not literally cause the unemployment rate in others. On the other hand, when the outcomes are choices made by strategically interdependent actors – as is common in political science – interdependence is far more plausible.

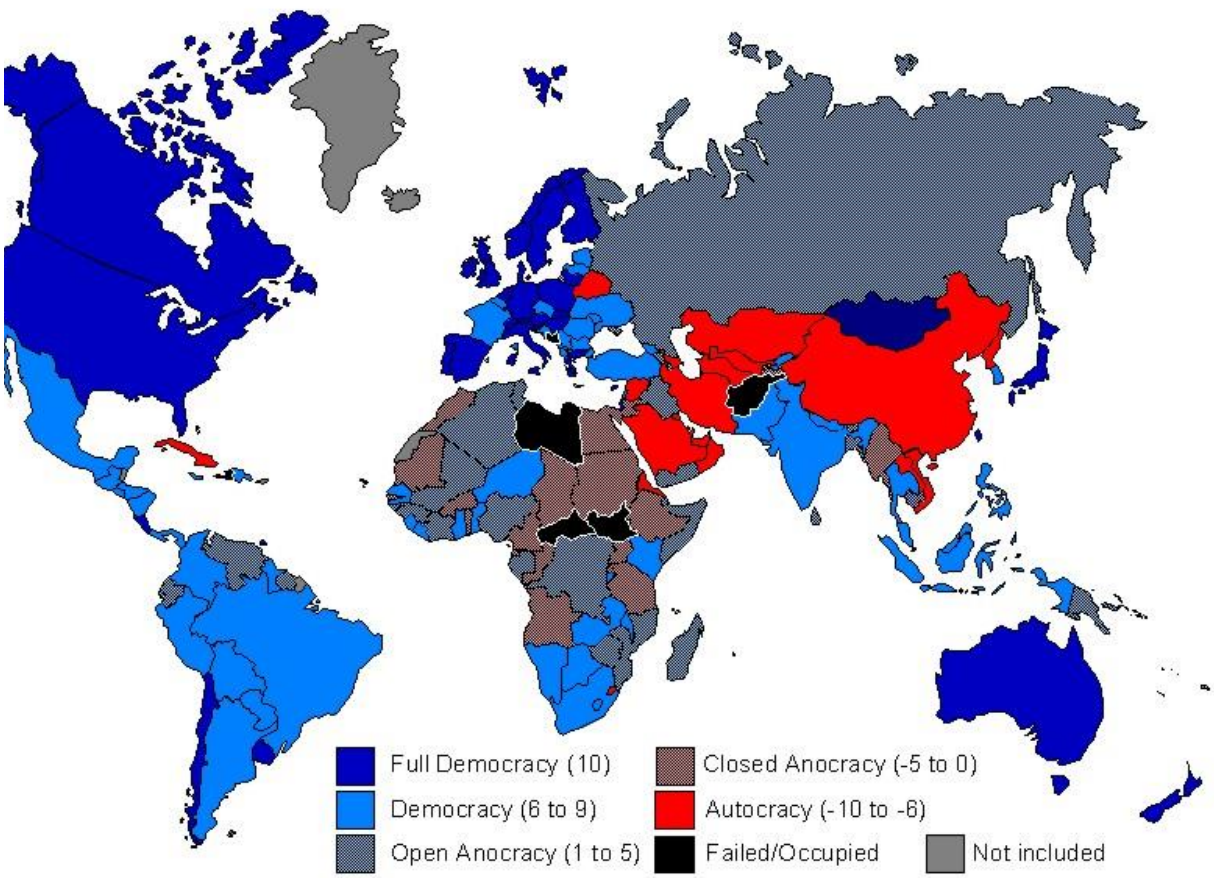
Ultimately, however, researchers are simply deciding whether theory indicates that changes to my neighbors covariates affect me directly (as in **SDEM**) or indirectly through the changes the elicit in my neighbors outcome (as in **SAC**). To us this consideration seems less consequential than determining whether the spatial clustering we observe in the outcomes arises from contagion of either type or merely through the presence of (spatially) common unobservables, which both **SDEM** and **SAC** allow us to do.

[30] As an alternative, one could estimate both **SAC** and **SDEM**. If one rejects $\lambda = 0$ in both models and $\rho = 0$ and $\theta = 0$ in the **SAC** and **SDEM** models respectively, it is likely that all three sources of clustering in the outcome are present. Power concerns make it more difficult to interpret the other combinations of results.

'Diffusion' and Democracy

That democracies are spatially clustered is both visually apparent (see: Figure 2) and widely accepted in comparative politics. Gleditsch and Ward (2006) find that "since 1815, the probability that a randomly chosen country will be a democracy is about 0.75 if the majority of its neighbors are democracies, but only 0.14 if the majority of its neighbors are non-democracies." The reason for this evident clustering in regime types, however, remains unresolved, as theories suggesting interdependence, spillovers, and clustering (in predictors and unobservables), are supported by the literature. Yet, little work has explicitly focused on discriminating between these competing sources. Instead, interdependence frequently goes presumed as the cause of this clustering, with research turning now to focus on how interdependence operates (**?**). As expressed above, we believe that before researchers focus on *how* interdependence, they need first establish *whether* interdependence. Therefore, we briefly review the existing literature on the regional determinants of democratization before empirically discriminating between these possible spatial sources.

Figure 2: Regimes by Type - Polity IV (Figure from Marshall and Gurr 2013)



Clustering in democracies is most commonly ascribed to interdependence, that is, that democracy spreads through 'diffusion' or 'contagion' in a cascading effect. **?** indicate that the belief in such political contagion has guided U.S. foreign policy from Roosevelt through Bush, with President Eisenhower most clearly articulating it in his speech on the "falling domino principle." Early research in political science echoed these sentiments, arguing that democracy comes in 'waves' (**?**), diffusing from one country to the next (**?**). This argument has, ostensibly, found wide empirical support (**????**Gleditsch and Ward, 2006). In each, that countries are more likely to democratize when surrounded by other democracies is taken as evidence of interdependence. As indicated earlier, this neglects other sources of spatial clustering in democracies.

First, the determinants of democracy may be spatially clustered, thereby eliciting clustering in the outcomes. By example, development is often found to be strongly correlated with democracy (**????**, among others). Given that development is itself strongly positively spatially correlated, this would producing clustering in democracy independent of any interaction between states.[31] In addition, development in one country may contribute to democratization in surrounding countries by allowing for the emergence of transnational advocacy networks. **?** evidence how these networks can directly produce liberalization in surrounding countries. Finally, unmodeled historical factors may have facilitated the development of democracy in particular states, and regions, producing the patterns we now observe (**?**). Therefore, to conclude in favor of the diffusionary theory of democracization (e.g., interdependence), one need first rule out the possible alternatives (e.g., clustering in observables, spillovers, clustering in unobservables).

To explore this possibility, we replicate **?** oft-cited study on income and democracy.[32] The dependent variable, *Democracy*, is the Freedom House Political Rights Index, ranging from 0 (the least free) to 7 (the most free).[33] The main independent variable, *GDPpc*, is GDP per capita (in PPP) from the Penn World Tables V6.1 (**?**). Finally, as in **?**, we include a lagged dependent variable in all models. As in the experiments in the last section, we estimate and report the results for the **SAR**, **SAC**, **SLX**, **SDEM**, **SDM**, and **GNS** models.

Table 5 provides the results. We see findings consistent with those from the MC experiments above. In short, a variety of spatial theories could be confirmed from the estimation of these models. **SAR**, **SDM**, and **SAC** each reject the null for ρ, suggesting interdependence. **SLX** and **SDEM** both reject the null for θ, suggesting spillovers. Lastly, **SAC**, **SDEM**, and **GNS** reject the null for λ, suggesting clustering in the unobservables. What, then, should one conclude from this? Two things are worth noting. First, it is likely that there are (at least) two sources of spatial dependence in the data. Second, that failing to account for clustering in the unobservables leads to dramatically biased estimates of either spillovers or interdependence. For example, the parameter estimate for ρ is 1.5 times greater when we fail to account for clustering in the unobservables (i.e., **SAR** vs. **SAC**).

[31] We note that all of the studies mentioned previous do, in fact, account for this mechanism by virtue of including GDP as a covariate, we are simply outlining the various competing sources of spatial heterogeneity.

[32] **?** estimate models with one-, five-, ten-year, and twenty-year panels. For the purposes of our study we confine our attention to one-year panels.

[33] **?** further supplement the index to account for missingness using the **?** data. See that text for additional information.

Table 5: Income and Democracy

	SAR (1)	SLX (2)	SDM (3)	SAC (4)	SDEM (5)	GNS (6)
$GDPpc_{t-1}$	0.064	0.048	0.057	0.062	0.053	0.053
	(0.008)	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)
ρ	0.344	–	0.296	0.220	–	0.065
	(0.053)		(0.071)	(0.080)		(0.130)
λ	–	–	–	0.461	0.554	0.518
				(0.080)	(0.085)	(0.113)
θ	–	0.128	0.035	–	0.094	0.078
		(0.025)	(0.034)		(0.031)	(0.045)
$Democracy_{t-1}$	0.651	0.682	0.654	0.682	0.688	0.687
	(0.024)	(0.023)	(0.024)	(0.025)	(0.024)	(0.025)
$Constant$	-0.511	-1.254	-0.717	-0.447	-1.025	-0.928
	(0.059)	(0.166)	(0.104)	(0.068)	(0.215)	(0.286)
Obs.	945	945	945	945	945	945

This is an admittedly sparse model, as such we add several additional covariates – *Education* and *Population* – and rerun the analysis.[34] Note that now because we have 3 covariates, we also must include 3 spatial lags of X in models testing for possible spillovers. The findings are given in Table 6.[35] The most dramatic difference is that we now fail to reject the null for any of the spillover parameters (the θ's).[36] Additionally, while we still find support for interdependence in the **SAR** model, we fail to reject the null on ρ in any other specification. Only clustering on unobservables appears to receive consistent support across models. Notably, in the **SAC** model we now fail to find support for interdependence (when also accounting for clustering on unobservables). While this analysis is obviously preliminary, it may indicate that greater caution should be exercised before concluding in favor of the diffusion of democratization. We plan to expand on this analysis in future iterations of the paper.

[34] *Education* is the average total years of schooling in the population age (from **?**), and *Population* is the total population in thousands (from **?**).

[35] It is important to note that the inclusion of the additional covariates results in the loss of nearly 30% of the sample. In future iterations of the paper we plan to more adequately account for this missingness.

[36] One could also run a joint test of significance across all 3 θ's, assessing whether there was support for spillovers generally.

Table 6: Income and Democracy

	SAR	SLX	SDM	SAC	SDEM	GNS
	(1)	(2)	(3)	(4)	(5)	(6)
$GDPpc_{t-1}$	0.045	0.048	0.051	0.049	0.047	0.047
	(0.015)	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)
$Educ_{t-1}$	0.016	0.015	0.015	0.016	0.015	0.015
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Pop_{t-1}	-0.001	-0.000	-0.001	-0.000	-0.000	-0.000
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
ρ	0.220	–	-0.057	0.094	–	0.019
	(0.079)		(0.071)	(0.107)		(0.166)
λ	–	–	–	0.376	0.407	0.397
				(0.136)	(0.115)	(0.149)
θ_1	–	-0.030	-0.057	–	-0.016	-0.019
		(0.057)	(0.058)		(0.069)	(0.074)
θ_2	–	0.028	0.016	–	0.021	0.020
		(0.018)	(0.019)		(0.025)	(0.026)
θ_3	–	-0.012	-0.006	–	-0.004	-0.004
		(0.029)	(0.029)		(0.032)	(0.032)
$Democracy_{t-1}$	0.600	0.614	0.600	0.617	0.620	0.620
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.031)
$Constant$	-0.325	0.008	0.077	-0.297	-0.143	-0.129
	(0.114)	(0.500)	(0.496)	(0.119)	(0.600)	(0.609)
Obs.	677	677	677	677	677	677

Conclusion

In general, there are two primary conclusions we hope to leave readers and practitioners with. First, the importance of undertaking appropriate diagnostics to explicitly test the restrictions implied by the one's model. While this will seem obvious to readers more familiar with model specification in other literatures (e.g., time series), these issues have not been as well articulated in the spatial literature which guides political scientists to date.[37] This is especially important in a spatial context where researchers are more likely to attach theoretic importance to these findings and, as such, should exercise greater care when specifying their models. Second, that no model can or should serve as baseline specification which guards against misspecification. While the spatial econometric literature has strongly advocated for the SAR, SLX, and Spatial Durbin models, we present a variety of theoretically plausible and empirically likely conditions where these model will

[37]For example, in their widely cited SAGE 'Green Book' introducing spatial regression methods, Ward and Gleditsch (2008) do not discuss tests which would allow one to discriminate between different spatial models.

cause researchers to draw faulty inferences.

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