39

Model Specification and Spatial Interdependence*

Scott J. Cook, Jude C. Hays, and Robert Franzese

INTRODUCTION

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., contagion or endogenous global spillovers) even where an alternative process instead underlies the association (e.g., diffusion or exogenous local spillovers). To help researchers avoid these pitfalls, we (1) elaborate the alternative theoretical processes that give rise to a taxonomy of spatial models, (2) indicate why and provide evidence that these alternative processes are frequently mistaken for one another during conventional hypothesis testing, and (3) suggest a set of strategies for effectively discriminating between the seven alternative spatial-lag models (with one, two, or all three of spatially lagged errors, spatially lagged independent variables, and/or spatially lagged dependent variable).

Cross-sectional, or spatial, interdependence is ubiquitous in the social sciences. Theories indicating that the actions of/outcomes in 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, for example.[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, political behavior, from voting to violence, is necessarily interdependent as expectations over outcomes are 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 prevalent policy coordination or competition. The very field name International Relations, meanwhile, centrally implicates interdependence in that area of study. More generally still, 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 and international relations, early work sought to introduce and extend methods for analyzing this dependence directly (Beck et al., 2006; Franzese and Hays, 2007). Beyond the classic linear model, statistical methods have been developed for spatial analysis of binary outcomes (Franzese et al., 2016; Wilhelm and de Matos, 2013), count data (Hays and Franzese, 2017), durations (Hays and Kachi, 2009; Hays et al., 2015), and endogenous predictors (Betz et al., 2020). 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 (Neumayer and Plümper, 2016; Plümper and Neumayer, 2010). As a result, there has been a proliferation of empirical work in political science, which offers theories, estimates models, and conducts tests of spatial interdependence.[2]

While this is a welcome advance over past practices – treating spatial dependence as a 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 interaction effects (e.g., spillovers in the predictors), and/or *iii)* interactions or clustering in the residuals (Elhorst, 2010).[3] Problematically, these theoretically distinct statistical models are quite similar and so produce similar patterns in empirical data, 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 causes the rejection, not B), making it difficult to statistically distinguish between these various models. To the extent that researchers attach theoretic importance to these different model specifications, which they should, and subsequently draw substantively meaningful inferences off these diagnostic tests, it is important to understand how and the extent to which these tests can distinguish between these alternatives. Thus, while we can now estimate a variety of spatial models in many different contexts, these ambiguities, left unaddressed, 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 that are 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 based on structural assumptions and therefore can provide only a precarious guide to our specification search. This precludes a Hendry-like general-to-specific specification search, as has been advocated in time-series modeling (in political science by De Boef and Keele, 2008). Instead, researchers generally must constrain one of the possible sources of spatial clustering in order to discriminate effectively between the remaining alternatives. While research design or theory should be the preferred bases on which to justify this constraint, we offer guidance for researchers in situations where these solutions are not available.

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.[4] Instead, we simply advocate that 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 of these models for another when drawing inferences, and suggest tests to aid researchers in identifying and specifying appropriate models for estimation. Subsequently, we evaluate the small-sample performance of these tests under a variety of simulated conditions.

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, some research may have too 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 first devoting sufficient attention to the various broader ways in which spatial dependence can manifest in observational data. Before discriminating between competing theories of the bases of diffusion, researchers must first evidence that there is some form of diffusion. Researchers need to be aware of the various possible sources of spatial correlation in their outcomes and adopt models that appropriately nest and test between these competing alternatives. Therefore, we open by discussing the potential sources of spatially correlated outcomes, before outlining the spatial-econometric models implied by each.[5]

According to Anselin (2010), spatial heterogeneity is the uneven distribution of a trait, event, or relationship across a region. Therefore, it is present whenever we observe spatial clustering in the outcomes across some set of sample units. By which we mean that when there is non-zero covariance among these units' outcomes:

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

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

To elaborate the various manifestations of spatial association more fully, consider Figure 39.1. As we see, correlation in the outcomes arises from spatial (inter)dependence in the observable and/or unobservable inputs.[6] Broadly, there are two mechanisms that produce spatially correlated outcomes: *i)* spatial clustering and/or *ii)* spatial spillovers or interactions. As with the outcomes, spatial clustering in the observables (unobservables) occurs when the level, presence, or change of an observed (unobserved) determinant in one unit is correlated with but not a function of (not caused by) the value of that factor in other (spatially proximate) units:

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

$$y_i = f(x_i, \varepsilon_i) \text{ and } \text{cov}(\varepsilon_i, \varepsilon_j) \neq 0 \text{ for } i \neq j \qquad (2b)$$

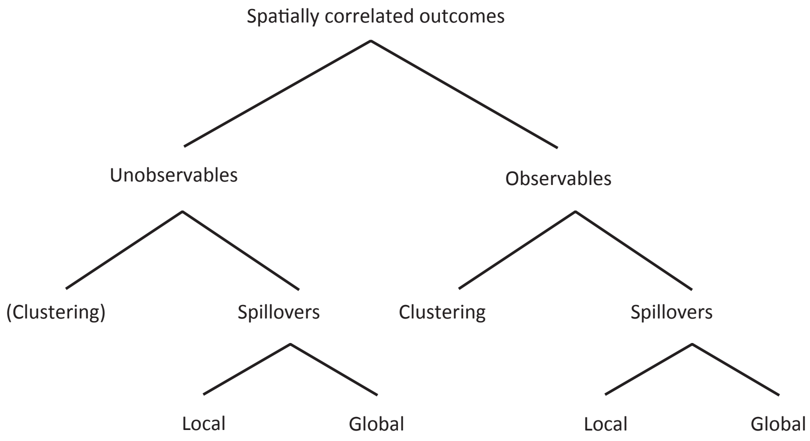

Figure 39.1 Manifestations of spatial association

where y is the outcome, x is a predictor, and ε is the unobserved error, with subscripts i and j identifying cross-sectional units. Here, the predictors and/or errors are spatially clustered, which, in turn, produces spatial clustering in the outcomes.[7] This does not require or suggest interaction between the units, simply that proximate actors possess similar characteristics (e.g., natural endowments that span across units) that, when manipulated, cause these unit outcomes to vary concurrently. That is, a common factor in the observables or unobervables results in correlated group effects.[8] For example, policy or technological innovations that change in the costs of inputs or demand (holding supply fixed) impact the revenues of all producers of a good, even where there is no direct interaction between them.

Alternatively, spatially correlated outcomes can arise due to spatial spillovers, when the outcomes of one unit are a function of (are caused by) the outcomes, actions, and behaviors of *other* units:

$$y_i = f(x_i, x_j, \varepsilon_i) \quad (3a)$$

$$y_i = f(x_i, \varepsilon_i, \varepsilon_j) \quad (3b)$$

$$y_i = f(x_i, y_j, \varepsilon_i) = f(x_i, (x_j, \varepsilon_j), \varepsilon_i) \quad (3c)$$

These we label *interdependence*, which seems to be the spatial process most commonly assumed by contemporary applied researchers. In this case, there are spillovers and/or externalities that 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 (are spatially correlated) – although they certainly may be – merely that there is cross-unit dependence, where the outcome in i is a function of the observables and/or unobservables in unit j. Theories of diffusion or contagion, or of strategic decision-making, for example, would generally imply such interdependent processes.

While many of our theories suppose interdependence in the outcomes, this necessarily implies that the relation of y_i and y_j operates through the combined spatial effects of the observables (x_j) and unobservables (ε_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 39.1), i.e., whether actors only affect 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 in which actors affect proximate actors, who in turn affect their proximate actors, and so on. Perhaps more clearly, the distinction is between whether spillovers in the observables (**X**) and unobservables (ε) in my neighbors affect me *directly*, or they affect me *indirectly* through my neighbors' outcomes (y_j).[9]

Our theoretical propositions about which combination of these spatial effects produces spatial clustering in the outcomes 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 factors (or both), *ii)* whether these spatial effects arise from clustering or spillovers (or both), and *iii)* if spillovers, whether these spillovers are local or global.[10] Table 39.1 lists the spatial models most commonly discussed in the literature.[11]

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

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \quad (4)$$

That is, to account for the spatial correlation in outcomes, we need simply to include appropriate predictors (**X**), as regularly done in non-spatial analysis. We emphasize this simple point as it seems to be misunderstood in some applied literature.[12] Moreover, it underscores the importance of model 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, give power to spatially lagged (in)dependent variables). As such, a better specified model is one obvious solution when confronting spatially clustered residuals.[13]

In estimating these models, researchers assume a spherical error variance–covariance matrix (and, by extension, that $\rho = \lambda = 0$; $\boldsymbol{\theta} = \mathbf{0}$), i.e., 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 tests 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 to justify the use of further spatial methods. However, these tests merely suggest *a* spatial process and generally are not very 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 any spatial correlation in the outcomes arises from a single source, exogenous observables, unobservables, or

Table 39.1 Common spatial econometric models

Name	*Structural model*	*Restrictions*
General nesting model	$\mathbf{y} = \rho\mathbf{Wy} + \mathbf{X}\boldsymbol{\beta} + \mathbf{WX}\boldsymbol{\theta} + \mathbf{u}, \mathbf{u} = \lambda\mathbf{W}u + \boldsymbol{\varepsilon}$	None
Spatial Durbin error model	$\mathbf{y} = X\boldsymbol{\beta} + \mathbf{WX}\boldsymbol{\theta} + \mathbf{u}, \mathbf{u} = \lambda\mathbf{Wu} + \boldsymbol{\varepsilon}$	$\rho = 0$
Spatial autocorrelation model	$\mathbf{y} = \rho\mathbf{Wy} + \mathbf{X}\boldsymbol{\beta} + \mathbf{u}, \mathbf{u} = \lambda\mathbf{Wu} + \boldsymbol{\varepsilon}$	$\boldsymbol{\theta} = \mathbf{0}$
Spatial Durbin model	$\mathbf{y} = \rho\mathbf{Wy} + \mathbf{X}\boldsymbol{\beta} + \mathbf{WX}\boldsymbol{\theta} + \boldsymbol{\varepsilon}$	$\lambda = 0$
Spatial autoregressive	$\mathbf{y} = \rho\mathbf{Wy} + \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon}$	$\lambda = 0$; $\boldsymbol{\theta} = \mathbf{0}$
Spatially lagged Xs	$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{WX}\boldsymbol{\theta} + \boldsymbol{\varepsilon}$	$\rho = \lambda = 0$
Spatial error model	$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{u}, \mathbf{u} = \lambda\mathbf{Wu} + \boldsymbol{\varepsilon}$	$\rho = 0$; $\boldsymbol{\theta} = \mathbf{0}$
(Spatial) Linear model	$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon}$	$\rho = \lambda = 0$; $\theta = \mathbf{0}$

outcomes, restricting the other possibilities to zero. SEMs 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.[14] Under these conditions, parameter estimates are unbiased but inefficient (and standard errors are incorrectly estimated). Efficient parameter estimates and correct standard errors can be obtained by accounting for the spatial structure of the residuals, as done in the **SEM**:

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\varepsilon} \qquad (5)$$

where **W** is an $N \times N$ connectivity matrix with elements w_{ij} indicating the (pre-specified, exogenous) relative connectivity (i.e., relationship) from unit j to unit i and λ indicating the strength of the spatial interdependence along this pre-specified pattern of connections.[15] In the terms of Table 39.1, this model assumes global spillovers in the unobservables, i.e., that the residuals are governed by a spatial autoregressive process.[16] This will also be the preferred specification when we believe there is clustering in the unobservables. Unlike with observable predictors, we have no means of introducing this heterogeneity into the systematic component of the model directly and must assume that these unobserved components are orthogonal to the observed ones (and so no bias issue), 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.[17]

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

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{W}\mathbf{X}\boldsymbol{\theta} + \boldsymbol{\varepsilon} \qquad (6)$$

Alternatively, where theory indicates these spillovers/externalities are global and in the outcome, the widely used **SAR** model is called for:[18]

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \qquad (7)$$

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 earlier, and whether there are spatial effects in the unobservables. More theoretically, they also differ over whether we believe there is cause to understand the spillovers of the observables as direct, $\mathbf{x}_j \Rightarrow y_i$, as is more likely with social aggregates, like GDP for example, or indirect, $(\mathbf{x}_{j,}\ \varepsilon_j) \Rightarrow y_j \Rightarrow y_i$, as is more likely with strategic independence among decision makers like as in public policies, for example.

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

$$\mathbf{y} = \lambda\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} - \lambda\mathbf{W}\mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \qquad (8)$$

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 is 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 its expansion produces

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

$$\begin{aligned}\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \rho\mathbf{W}\mathbf{X}\boldsymbol{\beta} + \rho^2\mathbf{W}^2\mathbf{X}\boldsymbol{\beta}\ldots \\ +\boldsymbol{\varepsilon} + \rho\mathbf{W}\boldsymbol{\varepsilon} + \rho^2\mathbf{W}^2\boldsymbol{\varepsilon}\ldots\end{aligned} \qquad (9b)$$

Again, the similarities between the **SAR** and **SLX** models 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, unmodeled spatial spillovers/externalities in the observable predictors, in the unobservables, or in the outcomes will result in a rejection of the zero null for the spatial-effect parameter in *any* of these single-source models.

To ward against this possibility, spatial econometricians have increasingly recommended the two-source models:

$$\textbf{SDM}: \mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{W}\mathbf{X}\theta + \varepsilon \quad (10)$$

$$\textbf{SAC}: \mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \varepsilon \quad (11)$$

$$\textbf{SDEM}: \mathbf{y} = \mathbf{X}\beta + \mathbf{W}\mathbf{X}\theta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \varepsilon \quad (12)$$

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

$$\textbf{GNS}: \mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\beta + \mathbf{W}\mathbf{X}\theta + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \varepsilon \quad (13)$$

which imposes no constraints on the three spatial parameters (ρ, λ, θ).[19] Given that this model subsumes all the alternatives presented thus far, one might think to engage in a Hendry-like general-to-specific specification search (Hendry, 1995), thereby avoiding the pitfalls encountered when adopting a specific-to-general approach. While this strategy has much to recommend it and is commonplace in the time-series literature, there are two problems that prevent adopting the general-to-specific approach in the spatial context.

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

$$y = \underbrace{\rho_1 E[y \mid a]}_{\text{Endog. Effects}} + x\beta + \underbrace{E[x' \mid a]\gamma}_{\text{Exog. Effects}} + v, \text{ where } u = \underbrace{\rho_2 E[u \mid a]}_{\text{Corr. Errors}} + \varepsilon \quad (14a)$$

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

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 in this model.[20] With spatial econometric methods, however, one does not simply estimate one sum 'neighborhood' effect: 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. For example, within a given 'neighborhood', there are first-, second-, and higher-order neighbors.[21] As a result, spatial-econometric models are *usually* able to use the pre-specification of **W** to achieve identification in most cases.[22]

Focusing on an example with a single predictor, some algebraic manipulation of the **GNS** model in Equation 13 allows it to be re-written as

$$\mathbf{y} = (\rho + \lambda)\mathbf{W}\mathbf{y} - \rho\lambda\mathbf{W}^2\mathbf{y} + \mathbf{x}\beta + (\theta - \lambda\beta)\mathbf{W}\mathbf{x} - \lambda\theta\mathbf{W}^2\mathbf{x} + \varepsilon \quad (15a)$$

$$\mathbf{y} = q_1\mathbf{W}\mathbf{y} + q_2\mathbf{W}^2\mathbf{y} + \mathbf{x}q_3 + q_4\mathbf{W}\mathbf{x} + q_5\mathbf{W}^2\mathbf{x} + \varepsilon \quad (15b)$$

where the spatial parameters are weakly identified by the second-order terms in the polynomial. The reduced form of the **GNS** provides five parameters from which we can recover the four structural parameters.

Substituting q_1 into q_2 and q_4 into q_5 gives a set of quadratic relationships for λ:

$$\lambda^2 = q_2 + \lambda q_1$$
$$\lambda^2 = \frac{-(q_5 + \lambda q_4)}{\beta}$$

These equations provide a unique solution for λ and, in turn, the other parameters:

$$\lambda = \frac{-(\beta q_2 + q_5)}{\beta q_1 + q_4}$$
$$\rho = q_1 + \frac{(\beta q_2 + q_5)}{\beta q_1 + q_4}$$
$$\theta = q_4 - \frac{\beta(\beta q_2 + q_5)}{\beta q_1 + q_4}.$$

The problem is that these parameters are identified solely by the structural assumptions of the functional form implied by autoregression and the pre-specified **W**, and the performance of the **GNS** model deteriorates rapidly as these assumptions become more appreciably incorrect, as we show later in our Monte Carlo experiments.

How, then, should researchers who are 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 firmer identification of the remaining free parameters and more robust estimation of the relevant two-source model.[23] 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 seems unappealing to us as a general strategy, given that we can think of no reason why we would generically expect to have strong prior information to indicate that unobserved effects are spatially governed in a manner distinct from observed predictors.[24] Accordingly, we focus on evaluating the efficacy of the first strategy, constraining one or more parameters, as a more generally applicable approach.

Implicitly, this is the approach currently advocated by most spatial econometricians, who have increasingly recommended one or another of the two-source models. However, 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**. Elhorst (2010: 10) offers a fun account that highlights 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’. Moreover, most of the work systematically exploring the small-sample performance of these models generally has done so with data-generating processes that satisfy the constraints assumed by the statistical model.

Instead of simply advocating one model over another, as is 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 that arise from the unidentified **GNS** and avoid models only identified off-structure (e.g., spatial econometric models, generally).[25] Focusing exclusively on those contexts where natural experiments are available, however, bounds the range of issues that can be studied. As such, we consider approaches where such strategies are not possible.[26]

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, we suggest a third alternative: 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*,[27] 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 in 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 exist, i.e., 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 that, by comparison, seems less severe.

Using either theory or research focus to guide specification, however, also naturally risks a much more problematic cost: estimating the incorrect model (and so, generally, calculating incorrect effect estimates). This can occur in four ways with the estimation of two-source spatial models: (1) the truth is all three spatial effects; (2) the truth is two sources but our statistical model imposes the wrong constraint, yielding the wrong two-source model; (3) the truth is a single-source model; (4) the truth is a non-spatial model. In either of the first two, we risk bias in the estimates of the included spatial and non-spatial parameters, as is always the case with spatially misspecified models. Thirdly, if the truth is a single source among our included two, the estimation should reveal that. If our two-source model does not include the true single source, the combination of estimated coefficients on the included should produce that omitted third, but we would incorrectly find support for *both* included spatial parameters being non-zero, even though the truth is that only the omitted third is non-zero (the fourth, non-spatial case should be unproblematic, as the estimation would return to zero for the spatial parameters.)

The possibility that an omitted single-source process would be reproduced through the combination of two-source parameter estimates has been well established for the **SEM** model, which can be re-expressed as a spatial Durbin model (noted above and re-expressed here):

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{u}, \text{ where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\varepsilon} \quad (17a)$$

$$\mathbf{y} = \lambda\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} - \lambda\mathbf{W}\mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \quad (17b)$$

In this case, we can test the common-factor restriction of Burridge (1981), $\boldsymbol{\theta} = -\lambda\boldsymbol{\beta}$, after estimating an 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:

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \quad (18a)$$

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \rho\mathbf{W}\mathbf{X}\boldsymbol{\beta} + \rho^2\mathbf{W}^2\mathbf{X}\boldsymbol{\beta} + \ldots + (\mathbf{I} - \rho\mathbf{W})^{-1}\boldsymbol{\varepsilon} \quad (18b)$$

Thus, the only difference between the SDEM and the SAR model is the higher-order polynomials of **WX** in the latter.[28] Finally, while expressing the relationship between the SLX and the SAC model is not as straightforward, 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 parallels the above discussions in that the estimates of each is a function of **WX**:

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} + \mathbf{u},\quad \text{where } \mathbf{u} = \lambda\mathbf{W}\mathbf{u} + \boldsymbol{\varepsilon} \tag{19a}$$

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \rho\mathbf{W}\mathbf{X}\boldsymbol{\beta} + \rho^2\mathbf{W}^2\mathbf{X}\boldsymbol{\beta} + \ldots + (\mathbf{I} - \rho\mathbf{W})^{-1}(\mathbf{I} - \lambda\mathbf{W})^{-1}\boldsymbol{\varepsilon} \tag{19b}$$

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 testing constraints on parameters, one could use tests that compare the performance of non-nested models. For example, the 'closeness' test in Vuong (1989) can evaluate whether the two models differ significantly in their ability to explain the data. In this context, a failure to reject the null hypothesis would indicate support for the more parsimonious single-source model. We do not explore this approach at length here, but it may warrant further consideration in subsequent work.

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 39.1. 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 simulation DGP is

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

where **y** is an $N \times 1$ vector of outcomes, **x** is an $N \times 1$ covariate vector, **u** is an $N \times 1$ vector of disturbances, **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 **x** and **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 (clustering), 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 intragroup 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 intragroup 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 ($\theta = 0$, $\rho = 0.2$), and (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 in the disturbances, as generated above, are present in all experiments.

Table 39.2 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 independent and identically distributed (i.i.d.) This generates an inflating simultaneity bias in $\hat{\rho}$, which 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 (we should note, however, that SAR's underestimation of β tends to be in some partial way compensated by its overestimation of ρ in

Table 39.2 ML estimates of covariate-coefficient estimate ($\hat{\beta}$, $\beta = 2$, N = 300, 1,000 trials)

	(1)	*(2)*	*(3)*	*(4)*
	$\theta = 0$, $\rho = 0$	*$\theta = 0.2$, $\rho = 0$*	*$\theta = 0$, $\rho = 0.2$*	*$\theta = 0.2$, $\rho = 0.2$*
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

terms of yielding an estimated total effect $(\mathbf{I}-\hat{\rho}\mathbf{W})^{-1}\hat{\beta}$ closer to the true value, two.

The results for the spatial interdependence-parameter estimates ($\hat{\rho}$) are presented in Table 39.3. Here, we see the inflation bias (in SAR and SDM, especially) driven by the unmodeled spatial clustering in the disturbances. The standard error estimates are highly overconfident as well. In SAR, across all four experiments, the standard deviation in the sampling distribution for $\hat{\rho}$ 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 when there is unmodeled clustering (this is Galton's problem).

Estimation with **SAC** does better than with **SAR** or **SDM** 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 (unmodeled/unobserved factors) by allowing them to follow a spatial AR process. 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

Table 39.3 ML estimates of interdependence ($\hat{\rho}$, $\beta = 2$, N = 300, 1,000 trials)

	(1)	(2)	(3)	(4)
	$\theta = 0, \rho = 0$	$\theta = 0.2, \rho = 0$	$\theta = 0, \rho = 0.2$	$\theta = 0.2, \rho = 0.2$
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 (0.10 level)	97.4%	99.2%		
Power (0.10 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 (0.10 level)	28.8%	18.2%		
Power (0.10 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 (0.10 level)	100%	100%		
Power (0.10 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 (0.10 level)	97.9%	97.5%		
Power (0.10 level)			92.2%	89.0%

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 39.4 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 a 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 further. 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.

To sum, clustering in unobservables – for example, unobserved or unmodeled clustered factors – complicates our ability to detect interdependence in outcomes and spillovers

Table 39.4 ML estimates of spillover effect ($\hat{\theta}$, $\beta = 2$, N = 300, 1,000 trials)

	(1)	(2)	(3)	(4)
	$\theta = 0, \rho = 0$	$\theta = 0.2, \rho = 0$	$\theta = 0, \rho = 0.2$	$\theta = 0.2, \rho = 0.2$
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 (0.10 level)	47.8%		74.8%	
Power (0.10 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 (0.10 level)	13.6%		70.0%	
Power (0.10 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 (0.10 level)	99.9%		99.9%	
Power (0.10 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 (0.10 level)	97.6%		97.7%	
Power (0.10 level)		98.9%		98.7%

from observable covariates in cross-sections of data. When one suspects both interdependence and exogenous spillovers, it would seem natural to estimate either the **SDM** or **GNS** models, but this is not advisable under these conditions. The **SDM** allows for both interdependence and spillovers, but it ignores the clustering in disturbances. This omission generates a bias in the estimates for the interdependence parameter (ρ) and the spillover parameter (θ). Why not also allow for spatial correlation in the disturbances? This is what the **GNS** does. Unfortunately, this model is only identified from strong structural assumptions (functional form and **W**), and, in this case, the structural assumption about the disturbances is incorrect, so failure of these other strong assumptions has severe ramifications. Therefore, **GNS** tends not to 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 the SDM or GNS 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 (plus spatial error dependence) on theoretical grounds.[29] This makes it difficult to offer a general prescription; however the nature of one's 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) – outcome contagion makes little sense. The unemployment rate in one locality does not literally cause the unemployment rate in another; rather, economic conditions cluster spatially and economic conditions in *j* cause unemployment in *i* (exogenous spillovers). 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: tax rates in *i* do likely respond to tax rates in *j*.

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 they elicit in my neighbors' outcome (as in **SAC**). To us, this consideration seems less consequential (though not inconsequential because **Wy** implies multipliers whereas **WX** does not) than determining whether the spatial clustering we observe in the outcomes arises from spillovers of either exogenous (**WX**) or endogenous (**Wy**) type or merely through the presence of (spatially) common unobservables, which both **SDEM** and **SAC** better enable us to do.

CONCLUSION

In general, there are two primary conclusions with which we hope to leave readers and practitioners. The first conclusion is the importance of undertaking appropriate diagnostics to explicitly test the restrictions implied by one's model, considering the different sources of spatial association, exogenous and endogenous, observed and unobserved. 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 that guides political scientists to date. This is especially important in spatial-analytic contexts, where researchers are more likely to attach theoretic importance to these findings and, as such, should exercise greater care when specifying their models. The second is that no single model can or should serve universally as baseline specification that guards against misspecification. While some in the spatial-econometric literature have advocated strongly for the **SAR**, **SLX**, or spatial Durbin models, we present a variety of theoretically plausible and empirically likely conditions where each of these models will cause researchers to draw faulty inferences. We have argued and presented support for the case that the heretofore relatively neglected **SAC** or **SDE** models will have broader utility as prudent defaults; but even those, we would acknowledge, can perform poorly under some plausible conditions.

For interested readers, we expand on the discussion presented here in Cook et al. (forthcoming, b) in several ways. First, we consider alternative specifications of the weights matrix, beyond the block-group structure examined here. Second, we increase the true effect size of the spatial lag of the predictor. Here, we have used a common coefficient size for each of the processes, however this implies a larger total effect size for the spatial lag of the outcome than the spatial lag of the predictor. Third, we consider not just coefficient estimates and hypothesis tests but substantive effects as well – i.e., the derivatives $d\mathbf{y}d\mathbf{x}$ (see Franzese, Chapter 31, this *Handbook*) – comparing the efficacy of the various models in capturing pre-spatial, post-spatial, and total effects. Fourth, we consider extensions to TSCS data, where temporal dynamics may also impair spatial-model specification. Finally, we illustrate our approach to model selection with an empirical example (democracy and income) to aid applied researchers.[30]

ACKNOWLEDGEMENTS

We thank the participants at the 2015 Texas A&M Conference on Innovations in Comparative Political Methodology, the 2015 Summer Methods Meeting of the Society for Political Methodology, the 2016 Meeting of the American Political Science Association, and the 2018 Workshop Modeling Spatial and Network Interdependence in International Relations for their useful feedback on this project. Any remaining errors are ours alone.

Notes

1 See Franzese and Hays (2008) for a fuller account of the substantive range of 'spatial' theories advanced in political science. In addition, Cook et al. (2019) discuss the application of spatial-econometric models to research in public administration and public management.

2 This trend is likely to continue growing as these methods become more familiar to researchers and packages facilitating their easy estimation become available in widely used statistical software languages, such as the 'spdep' package in *R* and the 'sp' suite in *Stata 15*.

3 Briefly noting the models that these would imply: spatial clustering can manifest due to unobserved factors common to proximate units, suggesting the 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 thereby my own outcome, as in a spatial autoregressive (**SAR**) model. Or it might be any combination thereof, suggesting one of several models that are more general.

4 We suspect this is often true in experimental data on human subjects as well.

5 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 address in other work.

6 Although most of the literature uses the terms *observables* and *unobservables*, the actual issue is whether these factors are *observ*<u>ed</u> and included in the model's systematic component or *unobserv*<u>ed</u> and left in the unmodeled residual component. We will continue to follow convention throughout, but the reader is encouraged to understand *(un) observeds* for all references to *(un)observables*.

7 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 correlated (but not autoregressive) factors. The consequences with respect to parameter estimates in the model of *y* are identical.

8 More formally, Andrews (2005) states common-factor residuals and predictors satisfy the following:

$$u_i = \mathbf{C}_g' \mathbf{u}_i^*$$
$$x_i = \mathbf{C}_g' \mathbf{x}_i^*$$

where $\mathbf{C}_g$ is a random common (e.g., group) factor with random factor loadings $\mathbf{u}_i^*$ & $\mathbf{x}_i^*$.

Therefore, if units *i* and *j* are each members of group *g*, they are jointly impacted by the respective loading.

9 The distinction also closely parallels that between moving-average (MA) and autoregressive (AR) processes in time-series contexts. Roughly, spatial-lag **X** (and spatial-MA error) processes are local, MA-like, and spatial-lag *y* (and spatial-AR error) processes are global, AR-like (in the errors only, not the outcomes, as in the SAR-error case).

10 This is analogous to Anselin's (2003) two-dimensional taxonomy for externalities.

11 Note that this is a partial list. All of the models presented here assume parameter constancy, first-order spatial dependence (when present), and global (and not local) spatial autocorrelation in the unobservables (when present). As noted, these are the most common alternatives in the literature and importantly include those advocated by LeSage and Pace (2009) and Elhorst (2010).

12 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), these **WX**s actually relate to a different argument as we discuss later.

13 As always, the distribution of our residuals – spatial or otherwise – is entirely dependent on the specification of the systematic component of our model.

14 In the remaining models, we will continue to assume that the residuals are orthogonal after the appropriate spatial specification is set. The possible endogeneity of the predictors present further complications as discussed in Betz et al. (2020).

15 Under spatial dependence in orthogonal residuals, standard errors can also be consistently estimated, leaving the parameter-inefficiency unaddressed, by using appropriately designed robust standard errors (Driscoll and Kraay, 1998).

16 The local (i.e., moving average) analog to this model would be given as

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

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 perhaps greater generality of the **SEM** model.

17 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 heterogeneity in the unobservables directly. An example of this can be found in Cook et al. (2019).

18 In actuality, the **SAR** model suggests global spillovers in both the observables and unobservables as we can see from the reduced form given below.

19 We note again that each of these models assumes a global autocorrelation in **y** and/or $\boldsymbol{\varepsilon}$ and that only first-order processes are considered.

20 Instead, all that is identified is the total spillover effect; this is *Manksi's reflection problem*.

21 This should suggest the importance of **W** given that 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 to identify which source of spatial effects are present depend upon the accuracy of **W**.

22 In this instance, the spatial analog is

$$\mathbf{y} = \mathbf{X}\beta + \mathbf{WX}(\beta\rho_1 + \gamma) + \rho_1\mathbf{WX}(\beta\rho_1 + \gamma) + \rho_1^2\mathbf{W}^2\mathbf{X}(\beta\rho_1 + \gamma) + \ldots + \varepsilon$$

23 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.'s (1996) robust Lagrange Multiplier tests (here given for spatial error):

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

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 $\text{cov}(\hat{\boldsymbol{\varepsilon}}, \mathbf{W}\hat{\boldsymbol{\varepsilon}})$ that can be attributable to $\text{cov}(\hat{\boldsymbol{\varepsilon}}, \mathbf{Wy})$. Equivalently, we could construct additional pre-specification tests (or simply estimate models) that hold fixed the effect of one alternative while evaluating the second.

24 Even when these exist, the likely high degree of correlation between the weights matrices would likely leave a still weakly identified model.

25 See also Egami (2018) for a strategy attempting nonparametric causal-inference tests of spatial spillovers in observational data.

26 Egami's (2018) approach requires no temporally simultaneous interdependence and is designed to test for, but not estimate (see Franzese, Chapter 31, this *Handbook*), spatial effects. We are inter-

ested in spatial-effect estimation in other contexts.

27 Unbiased estimation of isolated parameters is sufficient for testing purposes, for effect or response estimation; however, generally one needs more (see Franzese, Chapter 31, this *Handbook*).

28 While this does not as easily permit a Burridge-type restriction, we could specify a higher-order SDEM model and then perform an F-test of zero coefficients on 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 between those two, researchers in these situations should typically prefer the more parsimonious SAR model.

29 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 possible results.

30 Interested readers can also find an empirical application in Cook et al. (2015), an earlier version of this paper.

REFERENCES

Andrews, D. W. (2005), 'Cross-section regression with common shocks', *Econometrica* 73(5), 1551–1585.

Anselin, L. (2001), Spatial econometrics, *in* B. Baltagi (ed.) *A Companion to Theoretical Econometrics*. Oxford: Blackwell, pp. 310–330.

Anselin, L. (2003), 'Spatial externalities, spatial multipliers, and spatial econometrics', *International regional science review* 26(2), 153–166.

Anselin, L. (2010), 'Thirty years of spatial econometrics', *Papers in regional science* 89(1), 3–25.

Anselin, L., Bera, A. K., Florax, R. and Yoon, M. J. (1996), 'Simple diagnostic tests for spatial dependence', *Regional science and urban economics* 26(1), 77–104.

Beck, N. (1991), 'Comparing dynamic specifications: The case of presidential approval', *Political analysis* 3(1), 51–87.

Beck, N., Gleditsch, K. S. and Beardsley, K. (2006), 'Space is more than geography: Using spatial econometrics in the study of political economy', *International studies quarterly* 50(1), 27–44.

Betz, T., Cook, S. J. and Hollenbach, F. M. (2020), 'Spatial interdependence and instrumental variable models', *Political science research and methods* 1–16, doi:10.1017/psrm.2018.61.

Buhaug, H. and Gleditsch, K. S. (2008), 'Contagion or confusion? Why conflicts cluster in space1', *International studies quarterly* 52(2), 215–233.

Burridge, P. (1981), 'Testing for a common factor in a spatial autoregression model', *Environment and planning A* 13(7), 795–800.

Cook, S., An, S.-H. and Favero, N. (2019), 'Beyond policy diffusion: Spatial econometric models of public administration'. *Journal of public administration research and theory,* 29(4): 591–608.

Cook, S. J., Hays, J. C. and Franzese, R. J. (2015), 'Model Specification and Spatial Interdependence'. http://www.sas.rochester.edu/psc/polmeth/papers/Cook_Hays_Franzese.pdf (Accessed on 19 November, 2019).

Cook, S. J., Hays, J. C. and Franzese, R. J. (forthcoming, b), *Empirical Analysis of Spatial Interdependence*. Cambridge: Cambridge University Press.

De Boef, S. and Keele, L. (2008), 'Taking time seriously', *American Journal of Political Science* 52(1), 184–200.

Driscoll, J. C. and Kraay, A. C. (1998), 'Consistent covariance matrix estimation with spatially dependent panel data', *Review of economics and statistics* 80(4), 549–560.

Egami, N. (2018), 'Identification of causal diffusion effects using stationary causal directed acyclic graphs', *arXiv preprint*. arXiv:1810.07858.

Elhorst, J. P. (2010), 'Applied spatial econometrics: raising the bar', *Spatial economic analysis* 5(1), 9–28.

Franzese, R., Hays, J. and Cook, S. (2016), 'Spatial- and spatiotemporal-autoregressive probit models of interdependent binary outcomes', *Political science research and methods* 4(1), 151–173.

Franzese, R. J. and Hays, J. C. (2007), 'Spatial econometric models of cross-sectional interdependence in political science panel and time-series-cross-section data', *Political analysis* 15(2), 140–164.

Franzese, R. J. and Hays, J. C. (2008), Empirical methods of spatial interdependence, in Janet M. Box-Steffensmeier, Henry E. Brady, and David Collier (eds.) *Oxford Handbook of Political Methodology*. Oxford: Oxford University Press, pp. 570–604.

Gibbons, S. and Overman, H. G. (2012), 'Mostly pointless spatial econometrics?', *Journal of regional science* 52(2), 172–191.

Hays, J. C. and Franzese, R. J. (2017), A comparison of the small-sample properties of several estimators for spatial-lag count-models, *in* R. J. Franzese (ed.) *Advances in Political Methodology*. Cheltenham: Elgar Research Collections, Edward Elgar, pp. 180–207.

Hays, Jude C. and Kachi, Aya. (2015) Interdependent duration models in political science. In Franzese, R. J. (ed.) *Quantitative Research in Political Science (Vol. 5)*. Thousand Oaks (CA): Sage, pp. 33–62.

Hays, J. C., Schilling, E. U. and Boehmke, F. J. (2015), 'Accounting for right censoring in interdependent duration analysis', *Political analysis* 23(3), 400–414.

Hendry, D. F. (1995), *Dynamic Econometrics.* Oxford:Oxford University Press.

LeSage, J. and Pace, R. K. (2009), *Introduction to Spatial Econometrics*. Boca Raton (FL): CRC press.

Manski, C. F. (1993), 'Identification of endogenous social effects: The reflection problem', *The review of economic studies* 60(3), 531–542.

Neumayer, E. and Plümper, T. (2016), 'W', *Political science research and methods* 4(1), 175–193.

Plümper, T. and Neumayer, E. (2010), 'Model specification in the analysis of spatial dependence', *European journal of political research* 49(3), 418–442.

Vuong, Q. H. (1989), 'Likelihood ratio tests for model selection and non-nested hypotheses', *Econometrica*, 57(2): 307–333.

Wilhelm, S. and de Matos, M. G. (2013), 'Estimating spatial probit models in r.', *The R journal* 5(1): 130–143.