Empirical Modeling Strategies for Spatial Interdependence: Omitted-Variable vs. Simultaneity Biases

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Abstract: Scholars recognize that time-series-cross-section data typically correlate across time and space, yet they tend to model temporal dependence directly while addressing spatial interdependence solely as nuisance to be "corrected" (FGLS) or to which to be "robust" (PCSE). We demonstrate that directly modeling spatial interdependence is methodologically superior, offering efficiency gains and generally helping avoid biased estimates even of "non-spatial" effects. We first specify empirical models representing two modern approaches to comparative and international political economy: *(context-conditional) open-economy comparative political-economy* (i.e., *common stimuli, varying responses*) and *international political-economy*, which implies interdependence (plus *closed-economy* and *orthogonal-open-economy* predecessors). Then we evaluate four estimators—*non-spatial OLS*, *spatial OLS*, *spatial 2SLS-IV*, and *spatial ML*—for analyzing such models in spatially interdependent data. Non-spatial OLS suffers from potentially severe omitted-variable bias, tending to inflate estimates of common-stimuli effects especially. Spatial OLS, which specifies interdependence directly *via* spatial lags, dramatically improves estimates but suffers a simultaneity bias, which can be appreciable under strong interdependence. Spatial 2SLS-IV, which instruments for spatial lags of dependent variables with spatial lags of independent variables, yields unbiased and reasonably efficient estimates of both common-stimuli and diffusion effects, *when its conditions hold*: large samples and fully exogenous instruments. A tradeoff thus arises in practice between biased-but-efficient spatial OLS and consistent- (or, at least, less-biased-) but-inefficient spatial 2SLS-IV. Spatial ML produces good estimates of non-spatial effects under all conditions but is computationally demanding and tends to underestimate the strength of interdependence, appreciably so in small-N samples and when the true diffusion-strength is modest. We also explore the standard-error estimates from these four procedures, finding sizable inaccuracies by each estimator under differing conditions, and PCSE's do not necessarily reduce these inaccuracies. By an accuracy-of-reported-standard-errors criterion, 2SLS-IV seems to dominate. Finally, we explore the spatial 2SLS-IV estimator under varying patterns of interdependence and endogeneity, finding that its estimates of diffusion strength suffer only when a condition we call *cross-spatial endogeneity*, wherein dependent variables (y's) in some units cause explanatory variables (x's) in others, prevails.

I. Introduction

Although social scientists now commonly recognize that time-series-cross section data will typically correlate across time and space, they tend to treat temporal and spatial interdependence differently. Most analysts model temporal dynamics directly, often by lags of the dependent variable, but address spatial dependence solely as a nuisance to be "corrected" by feasible generalized-least-squares (FGLS) or to which to be "robust" in variance-covariance estimation by panel-corrected standard-errors (PCSE). In this paper, we demonstrate that directly modeling spatial interdependence is superior, offering efficiency gains and generally helping avoid biased estimates, even regarding the effects of non-spatial domestic factors and exogenous-external stimuli, and we evaluate the relative performance of alternative methodological strategies for modeling spatial interdependence: non-spatial OLS (which ignores the interdependence), spatial OLS (*S-OLS*), spatial 2SLS-IV (*S-2SLS*), and spatial ML (*S-ML*).[1] We also explore the accuracy of the standard errors reported by these four procedures and the performance of the S-2SLS estimator under varying patterns and combinations of interdependence and endogeneity.

In summary, we find that, given interdependence, non-spatial OLS suffers from potentially severe omitted-variable bias that tends to inflate estimates of common-stimuli effects. Spatial OLS, which directly specifies interdependence *via* spatial lags, dramatically improves estimates, even of non-spatial (e.g., domestic and exogenous-external) effects, and especially to the degree diffusion (i.e., interdependence) and domestic and common-stimuli (i.e., non-spatial) alternatives are well-specified. S-OLS does, however, suffer a simultaneity bias, which, while mild at low levels of spatial interdependence, can become appreciable when diffusion is strong. This bias manifests as over-estimation of diffusion effects at the expense of common-shock effects. Spatial 2SLS, which instruments for spatial lags of dependent variables with spatial lags of independent variables, seems to redress the resulting dilemma well, yielding unbiased and reasonably efficient estimates of both common-stimuli and diffusion effects, *when its conditions hold*: large samples and fully exogenous instruments. This raises a familiar (*Bartelsian*) tradeoff between the biased-but-efficient S-OLS and

[1] We borrow the standard terminology of *spatial* dependence, shocks, *etc.*, whose connotation of *geographic* and *spatial-distance* bases for interdependence (spatial proximity, shared borders, *etc.*) originates from the spatial-econometrics literature's development in geographically related sciences. However, the notions of *space* and *spatial* dependence extend transparently and without any change in the statistics or mathematics to encompass alternative bases of "proximity" that may induce interdependence such as, *e.g.*, economic notions of nearer and further competitors, sociological notions of network connectivity, or socio-political notions of shared or nearer or further cultural, religious, linguistic, or political heritages.

the consistent- (or, at least, less-biased-) but-inefficient S-2SLS that generally obtains in practice. Small samples, modest diffusion strength, and imperfect exogeneity of instruments—not uncommon conditions, perhaps—favor the simpler S-OLS estimator in mean-squared-error terms, a comforting consideration, especially given the complexity of instrumental-variable estimation for qualitative and limited dependent-variables. ML, meanwhile, produces good estimates of non-spatial effects under all conditions but seems[2] to underestimate the strength of diffusion/interdependence noticeably, and appreciably so in small-N samples and when the true diffusion-strength is modest. S-ML is also is computationally demanding.[3] We also explore the standard-error estimates produced by these four procedures, finding sizable inaccuracies reported by each estimator under differing conditions; nor do PCSE's necessarily reduce these problems. By an accuracy-of-reported-standard-errors criterion, S-2SLS dominates; i.e., it produces, but at least it also reports, relatively large standard errors. Finally, we explore the S-2SLS estimator under varying patterns of spatial interdependence and endogeneity, finding that its estimates of diffusion strength suffer only when a condition we call *cross-spatial endogeneity*, wherein dependent variables (*y*'s) in some units cause explanatory variables (*x*'s) in others, prevails. Spatial interdependence among the explanatory factors does little harm by itself, and within-unit endogeneity (*y*'s causing the same unit's *x*'s) of course biases estimates of the coefficients on those factors but does not by itself do much harm to the S-2SLS estimates of diffusion strength. Given this combination of strengths and weaknesses (so far) of the extant estimators, we do not (as yet) have a universal recommendation, except to emphasize that accurate and efficient modeling of both spatial interdependence (diffusion) *and plausible non-spatial*

[2] We have just begun exploration of the S-ML estimator and have concerns about the accuracy of certain numeric procedures and approximations applied in the Stata "spatreg" command.

[3] Our 100-iteration Monte Carlo experiment for N=40, T=40 samples takes an estimated 72 hours to complete in Stata running on a 2GHz, 2GB-RAM system.

(domestic and/or exogenous-external) alternatives is first-order to all of these more-sophisticated, but usually[4] second-order, considerations. The degree to which any of the three spatial estimators outperform non-spatial OLS far outweighs the differences between them by any criteria. Conversely, as we show elsewhere, insofar as shared exposure to exogenous-external stimuli is inadequately modeled, estimates of the strength of spatial-diffusion will tend to be exaggerated. That is, spatial models that ignore or inadequately specify non-spatial (domestic or, especially, exogenous-external) factors will tend to over-estimate the importance of spatial interdependence.[5]

We begin this analysis by identifying four theoretical and substantive approaches to comparative and international politics and political economy *(C&IPE)* that motivate distinct empirical models. In closed-economy comparative politics *(CP)* and comparative political-economy *(CPE)*, the focus is on domestic variables, and external stimuli and international diffusion processes are ignored.[6] In open-economy CP/CPE, by contrast, the importance of external stimuli and shocks (e.g., oil prices) for the domestic political-economy is recognized, but the domestic responses to

[4] One condition under which the (potential) simultaneity biases of S-OLS (and the other spatial models) could outweigh the omitted-variable bias of non-spatial OLS would be when *cross-spatial endogeneity* dominated the diffusion (cross-unit interdependence of y). We have but do not yet report some preliminary simulations that show this.

[5] Actually, we have at present just some one-shot illustrations of this, not yet multi-iteration demonstrations.

[6] We refer to processes by which outcomes in some units directly affect outcomes in other units as *diffusion*. Also, we distinguish such diffusion processes, which will induce *spatial correlation*, from *spatially correlated responses to spatially correlated exogenous shocks/stimuli*—or *common* or *exogenous-external shocks*, *stimuli*, or *conditions* for short—which will also induce *spatial correlation*. In other work from across the social sciences, synonyms for *diffusion* include *contagion*, *strategic interdependence, strategic dependence*; and synonyms for *spatial correlation* include *spatial dependence, interdependence.* We have noticed, however, no consistency within or across disciplines in how these terms are used. For example, *contagion* would be synonymous with *diffusion* specifically in much of biometrics whereas it is often synonymous with *spatial correlation* generally in much of econometrics, and it seems equally likely to mean either in sociology.

external stimuli, which may be moderated by domestic variables (context-conditional open-economy CP/CPE: *common stimuli, varying responses*) or unconditioned by domestic variables (orthogonal open-economy CP/CPE), are in either case treated as isolated phenomena. That is, in open-economy CP/CPE, external conditions affect domestic policies and outcomes, but these domestic policies and outcomes do not themselves affect the policies and outcomes of other units and so do not reverberate throughout the global polity or political economy. Finally, international-relations (IR) and international-political-economy (IPE) scholars focus explicitly on spatial linkages and mechanisms of diffusion in the global political economy whereby policies and outcomes in some units directly affect those of other units, perhaps in addition to the possibility that multiple units are exposed to common (or correlated) external stimuli. A country might respond to an exogenous domestic or global political-economic shock by lowering its capital tax-rate, for example, but the magnitude of its response may also depend on how its competitors respond and, conversely, its own response may affect the capital tax-rates that policymakers in other countries choose.

In this paper, we focus on the models of context-conditional open-economy CP/CPE and IR/IPE and methods for estimating such models. We do not consider purely domestic models except to note here that, if external influences of the common stimuli and/or the diffusion sort are important, such closed-economy models will produce inefficient estimates of coefficients for domestic variables even in the best of circumstances and, in the worst, biased and inconsistent estimates.

The central challenge we pose for our estimators is to distinguish *common shocks* from *international diffusion* and to estimate well their relative strength. We evaluate the performance of four estimators—*non-spatial OLS*, *spatial OLS*, *spatial 2SLS*, and *spatial ML*—for analyzing empirical models corresponding to these alternative theoretical and substantive visions from spatially interdependent data. We find that, given diffusion, non-spatial ordinary least-squares (OLS) suffers a potentially severe omitted-variable bias that tends to inflate estimates of common-stimuli effects. That is, ignoring diffusion processes when they are present will lead analysts to exaggerate the importance of external stimuli. Conversely, of course, spatial OLS, which directly specifies diffusion *via* spatial lags, will tend to over-estimate diffusion effects if common stimuli are ignored or inadequately modeled. We find, though, that direct modeling of spatial interdependence even by the relatively simple means of spatial lags in OLS models dramatically improves estimates, even of non-spatial (domestic and exogenous-external) effects, and especially to the degree diffusion and domestic and common-stimuli alternatives are well-specified.

Spatial OLS does, on the other hand, suffer a simultaneity bias, even when domestic and exogenous-external factors are modeled perfectly,[7] although it generally remains small at modest levels of spatial interdependence, being second-order to the more-serious misspecification (i.e., omitted-variable) problems of ignoring diffusion or common shocks when either is present. The simultaneity bias of S-OLS, which manifests as over-estimation of diffusion effects at the expense of common-stimuli effects, can be appreciable when diffusion is strong, however. In other words, if certain simultaneity problems discussed below are insufficiently addressed, modeling diffusion by spatial lags can lead analysts to overestimate the importance of diffusion at the expense of common shocks, especially insofar as common shocks and their effects are inadequately modeled.

A spatial two-stage-least-squares (S-2SLS) estimator, which instruments for spatial lags of the dependent variable with spatial lags of the independent variables, seems to provide an effective resolution to this dilemma, yielding unbiased and reasonably efficient estimates of both common-

[7] This is a critical difference between spatial interdependence and temporal dependence. OLS models of the latter that accurately reflect the true dynamics in the dependent-variable will be unbiased, consistent, and efficient; OLS models of the former using spatial lags will be biased and inconsistent even if the lags accurately reflect the interdependence.

stimuli and diffusion effects, at least under its ideal conditions—namely, that domestic and common-shock explanatory variables are not themselves endogenous to dependent variables within or across units but do strongly predict these dependent variables (i.e., they are perfect instruments). These, along with a large sample, are also precisely the conditions necessary to the good properties of all instrumental-variable (IV) estimators—large samples and fully exogenous and strongly predictive instruments—so the solid performance of S-2SLS in our larger-sample experiments is hardly surprising. However, more usually in practice we expect familiar tradeoffs between biased-but-efficient spatial OLS and consistent- (or, at least, less-biased-) but-inefficient spatial 2SLS to emerge. A tradeoff we can actually observe in our smaller-sample experiments where, despite its bias, spatial OLS matches or outperforms S2SLS in root-mean-squared-error terms. Similarly, while S-ML performs well in mean-squared-error terms fairly generally, it also has general and quite troubling tendencies to underestimate diffusion, quite noticeably so when diffusion is modest and units are *few*, and to understate standard errors of those diffusion-effect estimates, dramatically so when diffusion is modest and units are *many*. Thus, small samples, modest diffusion strength, and imperfect exogeneity of available instruments—relatively common conditions, we suspect—may favor the adequacy of simpler spatial-lag estimators over instrumented or ML ones, a comforting consideration given the complexity of IV techniques for estimation of simultaneous relationships in qualitative or limited dependent-variables and the computational intensiveness of S-ML.

We organize the rest of the paper as follows. Section II discusses the four approaches to comparative and international politics and political economy described briefly above, each of which motivates its own characteristic empirical models. Section III presents a generic spatial lag model—an empirical framework well-suited to testing hypotheses about international diffusion—and outlines various methods for estimating coefficients in such models. Section IV gives some analytical results regarding the properties of spatial and non-spatial OLS in the simplest of circumstances and then details the design of our Monte Carlo experiments to explore the properties of these two and of spatial 2SLS and ML in richer, more-realistic scenarios.[8] Section V presents the Monte Carlo results. In Section VI, we conclude these methodological explorations with summary discussion of our findings and the guidance they offer scholars in evaluating attempts to estimate models that include or should include spatial interdependence. One key consideration, we emphasize, is how well the alternative theoretical and substantive mechanisms of *diffusion* and of *common shocks, varying responses* are reflected in the empirical measurement and model because misspecifications in the one have a statistical tendency to induce underestimation of it and overestimation of the alternative.

II. Comparative Political Economy vs. International Political Economy[9]

One can distinguish four broad visions of comparative and international political economy, *C&IPE,* over its development as a field of inquiry: closed-economy comparative-political-economy, *CPE*, orthogonal open-economy CPE, context-conditional open-economy CPE, and (comparative and) international political economy, *(C&)IPE*, which implies diffusion or interdependence. Each of these broad visions of PE has a characteristic mathematical expression of its empirical implications, and these characteristic empirical specifications clarify the inherent theoretical stance (assumptions) in each regarding the substantive roles of common shocks and diffusion.

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[8] We study linear models only here because the important results are more easily explored and explained in the linear-regression context, but we also believe that the intuitions derived from this study generally extend beyond linear-regression models (although often with additional complications). Preliminary simulation results for duration and binary-outcome models (available upon request) support this claim that the conclusions stressed here extend intuitively, but with additional complications related, perhaps *inter alia*, to the non-separability of first and higher-moment parameters in many of these models.

[9] *Comparative and international politics* could replace *comparative and international political economy* in this heading without any loss of applicability. The issues discussed are perhaps more homogenous and clearer in the political-economy subfields, though, so we conduct the discussion mostly in those terms.

A. Closed-Economy Comparative-Political-Economy:

In closed-polity CP and closed-polity-and-economy CPE, domestic political and economic institutions (*e.g.*, electoral systems and central-bank autonomy), structures (*e.g.*, socioeconomic-cleavage and economic-industrial structures), and conditions (*e.g.*, electoral competitiveness and business cycles) are the paramount explanitors of domestic outcomes. Such domestic-primacy substantive stances imply theoretical and empirical models of this form:

$$\mathbf{y}_{it} = \xi_{it}\boldsymbol{\beta} + \boldsymbol{\varepsilon}_{it} \qquad \text{(A)}$$

where $\boldsymbol{y}_{it}$ are the policies or outcomes to be explained (dependent variables) and ξ_{it} are the *domestic* institutional, structural, and other conditions that explain them (independent variables), each of which may vary across time and/or space. Most early 'quantitative' empirical studies in comparative politics and political economy were of this form,[10] perhaps allowing the stochastic component, $\boldsymbol{\varepsilon}_{it}$, to exhibit some spatial correlation, but treating this correlation as nuisance either to be 'corrected' by Parks procedure (FGLS) or, later, to require an adjustment to standard-errors (PCSE). Examples here include most of the early empirical literature on the political economy of fiscal and monetary policy (*e.g.*, Tufte 1978, Hibbs 1987, and successors), coordinated wage bargaining and corporatism (*e.g.*, Cameron 1984, Lange 1984, Lange and Garrett 1985, and successors), and the early central-bank-independence literature (*e.g.*, Cukierman 1992, Alesina and Summers 1993, and successors).

B. Orthogonal Open-Economy Comparative-Political-Economy:

As economies grew more open and interconnected by international trade and, later, finance through the postwar period, and as perhaps their geopolitical interconnectedness increased also,

[10] Many early 'qualitative' studies also tended to ignore the spatial interdependence of their subject(s), or, at most, to mention the international context as among explanatory factors but generally elaborating little. Moreover, many modern political-economy studies of both 'quantitative' and 'qualitative' varieties continue to ignore the spatial interdependence of their data (see Persson and Tabellini 2003, e.g.).

comparativists and comparative political-economists began to view controls for the effects of global political and economic conditions on domestic policies and outcomes to be more important. At first, however, such global conditions were assumed to affect all domestic units equally and to induce equal responses from each unit to that effect. This implies theoretical/empirical models of this form:

$$\mathbf{y}_{it} = \xi_{it}\boldsymbol{\beta}_0 + \eta_t\boldsymbol{\beta}_1 + \boldsymbol{\varepsilon}_{it} \qquad \text{(B)}$$

where the η_t are global shocks (e.g., the oil crises), felt equally by all of the sample spatial units (each feels an identical η_t), each of whom respond equally (by amount $\boldsymbol{\beta}_1$) thereto. Again, the stochastic component, $\boldsymbol{\varepsilon}_{it}$, may exhibit spatial correlation—i.e., spatial correlation distinct from that induced by exposure to these common shocks—but any such correlation was treated as nuisance either to be 'corrected' by Parks procedure (FGLS) or, later, to require an adjustment to standard-errors (PCSE). Examples of empirical models reflecting such stances (often implicit) include many post-oil-crisis political-economy studies, including later rounds of the above literatures wherein time-period dummies or controls for global economic conditions or the practice of differencing domestic from global conditions[11] began to appear: e.g., Alvarez, Garrett, and Lange (1991) with regard to partisanship and corporatism interactions; Alesina, Roubini, and Cohen (1997) with regard to political and/or partisan cycles; Powell and Whitten (1993) with regard to economic voting.

C. Context-Conditional Open-Economy Comparative-Political-Economy:

Modern approaches to CP and CPE recognize the potentially large effects of external shocks and other conditions abroad on the domestic political economy, tending to emphasize how domestic institutions, structure, and conditions shape the degree and nature of domestic exposure to such

[11] Differencing the dependent variable thusly is identical to controlling for global conditions and forcing their coefficient to be -1. Differencing independent variables so amounts to controlling for global conditions and fixing their coefficient to minus the coefficient on the domestic independent variables.

foreign stimuli and/or moderate the domestic policy and outcome responses to these foreign stimuli. This produces characteristic theoretical and empirical models of the following sort:

$$\mathbf{y}_{it} = \xi_{it}\boldsymbol{\beta}_0 + \eta_t\boldsymbol{\beta}_1 + (\xi_{it} \cdot \eta_t)\boldsymbol{\beta}_3 + \boldsymbol{\varepsilon}_{it} \qquad \text{(C)}$$

where the incidence, impact, and/or effects of global shocks, η_t, on domestic policies and outcomes, $\boldsymbol{y}_{it}$, are conditioned by domestic institutional-structural-contextual factors, ξ_{it}, and so differ across spatial units. Examples here include much of modern CP and CPE, including the contributions to the recent *International Organization* special issue (Bernhard, Broz, and Clark 2002) on the choice of exchange-rate regimes and other monetary institutions. That varying domestic institutions/structures moderate the response of domestic policies and outcomes to globally common shocks, or that they shape how common shocks are felt domestically, are also central arguments in Franzese (2002) and Garrett (1998).[12] Once more, any spatial correlation distinct from that induced by common or correlated responses to *common shocks* would typically be left to FGLS or PCSE "corrections".[13]

D. International Political-Economy (with Diffusion):

$$\mathbf{y}_{it} = \rho\sum_{j\neq i} w_{ij}\mathbf{y}_{j,t} + \xi_{it}\boldsymbol{\beta}_0 + \eta_t\boldsymbol{\beta}_1 + (\xi_{it} \cdot \eta_t)\boldsymbol{\beta}_3 + \boldsymbol{\varepsilon}_{it} \qquad \text{(D)}$$

where $\mathbf{y}_{j,t}$, the outcomes in the other ($j\neq i$) spatial units in some manner (given by ρw_{ij}) directly affect the outcome in spatial unit i. Note for future reference that w_{ij} reflects the degree of connection from j to i, and ρ reflects the impact of the outcomes in the other ($j\neq i$) spatial units, as weighted by w_{ij}, on

[12] In some open-economy CPE empirical models, the common shocks or other controls for conditions abroad employed are actually *dependent-variable* conditions abroad—as, e.g, in Franzese's (2002) transfers, debt, unemployment, and inflation models, and Garrett's (1998) growth model. These, then, are actually models of the next type. Even so, these right-hand-side variables are seen as exogenous-external conditions or merely nuisance controls, and the inherent IPE-and-diffusion aspects receive little discussion and no emphasis.

[13] Some scholars employ spatial lags but treat them solely as nuisance controls, rather than interpreting them (also) as models of diffusion (see previous note), which is analogous to using (temporally) lagged dependent variables as nuisance control for serial correlation and ignoring their implications as models of dynamics.

the outcome in *i*. The rest of the right-hand-side model reflects the domestic political economy and, in the literature, has been as simple as (A) or as complex as (C). Examples of these sorts of models include the recent work of Simmons and Elkins (2004) on the global diffusion of liberalization policies and reforms.[14] Franzese (2003) also estimates such a model in a context where domestic inflation policy/outcomes depend upon inflation rates in other countries, weighted (w_{ij}) in a manner determined by patterns of international monetary exposure and exchange-rate commitments. The theoretical and empirical precision with which the spatial-lag weights reflect the diffusion mechanisms argued to be operating and to which these domestic and exogenous-external controls model plausible alternatives are, of course, critical to distinguishing and evaluating empirically the relative strength of diffusion and "common shocks, varying responses" in determining the outcome.

Given that models of sort (D) subsume those of sorts (A)-(C), one might argue that scholars should always begin with (D) and work downward as their data suggest/allow. However, as we elaborate below, obtaining "good" (unbiased, consistent, efficient) estimates of such models and distinguishing open-economy CPE processes from IPE processes, which entail diffusion, are both less straightforward than they may first appear, even when both processes are modeled perfectly.

III. Spatial-Lag Models

A. Specifying and Estimating Spatial-Lag Models

The spatial-lag model is useful for testing hypotheses about and estimating the strength of international diffusion. The model is written formally as

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \qquad (1).$$

$\mathbf{y}$ is a *NTx1* vector of observations on the dependent variable stacked by unit (i.e., unit 1, time 1 to

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[14] See the contributions to the special issue on diffusion edited by Simmons, Garrett, and Dobbin, currently in review at *International Organization*. Franzese (2002) also estimates simple, unweighted (*i.e.*, constant w_{ij}) versions of models of type (D), but his discussion emphasizes the results' context-conditional CPE aspects.

T, then unit 2, time 1 to T, *etc.* through unit N). ρ is the spatial autoregressive coefficient (i.e., the general strength of diffusion), and $\mathbf{W}$ is an $NTxNT$ block-diagonal spatial-weighting matrix (with elements w_{ij}). Thus, $\mathbf{Wy}$ is the spatial lag. $\mathbf{X}$ is an $NTxK$ matrix of observations on K independent variables, and $\boldsymbol{\beta}$ is a $Kx1$ vector of their coefficients. $\boldsymbol{\varepsilon}$ is a $NTx1$ vector or disturbances, assumed to be independent and identically distributed, conditional on the spatial lags and common shocks. Each TxT block along $\mathbf{W}$'s diagonal refers to the corresponding unit's T observations and so is a matrix of zeros unless observations also exhibit temporal dynamics, which we do not consider.[15] Each of the off-diagonal TxT blocks gives the effect of the column units' T observations on the row units' T observations and so, again, has zero off-diagonal elements unless there are cross-temporal spatial effects—time period t in unit j affects some other time period s in unit i—which we also debar.[16] Considering richer stochastic structures, temporal dynamics, or cross-temporal cross-spatial dependence jointly with the simple spatial interdependence studied here would be highly complex, and well beyond the scope of the present exploration.[17] Most important for our purposes are the diagonal elements of these off-diagonal blocks, which reflect the contemporaneous diffusion effect of the column unit on the row unit.[18] Recall that the w_{ij} elements of $\mathbf{W}$ reflect the degree of

[15] With temporal lags, the block-diagonals would be lower triangular, meaning that their prime diagonal and all entries above it are zero. The diagonal and upper-triangular elements multiply current and future time periods and so are not on the right-hand side of the equation; the lower-triangular elements multiply past observations on $\mathbf{y}$ in that unit and so would describe the temporal dynamics in the usual way.

[16] Once again, the sub-matrices would be lower-triangular if cross-temporal spatial dependence were allowed.

[17] However, in the broader project of which this paper is a first slice, we are exploring some of these issues in other contexts where they may be more important.

[18] If diffusion effects occur with some temporal lag, the non-zero elements of these off-diagonal blocks will shift to the corresponding number of diagonals below the prime. Much of the methodological literature on spatial dependence focuses on cross-sections of data (T = 1), avoiding some of these complications. Anselin (1988) offers a comprehensive treatment of spatial econometrics; Anselin (2001) covers recent developments.

connection from unit j to i—so, unlike a variance-covariance matrix, $\mathbf{W}$ need not be symmetric—and that ρ reflects the impact of the outcomes in the other ($j \neq i$) spatial units, as weighted by w_{ij}, on the outcome in i. Thus, ρ gauges the overall strength of diffusion, whereas the w_{ij} describe the relative magnitudes of the diffusions paths between the sample units.

Generally, the set of w_{ij} are determined by theoretical and substantive argumentation as to which units will have greatest affect on outcomes in which other units; the ρ are the coefficients to be estimated on these spatial lags. For example, one common operationalization of the diffusion potentially induced by international economic competition is some set of weights, w_{ij}, based on the trade or capital-flow shares of countries j in country i's total. The inner product of that vector of weights with the stacked dependent variable $\mathbf{y}$, then gives the weighted sum (or average) of $\mathbf{y}$ in the other countries j that time-period as a right-hand-side variable in the regression. The matrix $\mathbf{Wy}$ just gives the entire set of these vector inner-products—in this case, the trade- or capital-flow-weighted averages—for all countries i. Another common approach, frequently used to specify leader-emulation or cultural-connection diffusion mechanisms, for example, is to consider outcomes from unit or set of units j to diffuse to the outcome in i but not the outcomes from other units. For example, only outcomes from countries with similar religious or political heritage diffuse. This implies the weights are 1 (for sums; $1/N-1$ for averages) or 0, so diffusion either occurs from some j to some i or it does not, but otherwise the math is the same.

Several ways to estimate the coefficients of such models exist. One could estimate $\boldsymbol{\beta}$ by an ordinary-least-squares (OLS) regression that ignores (omits) the spatial interdependence. We call this *non-spatial OLS*. This strategy is the simplest, but its estimates will almost certainly suffer omitted-variable bias and inconsistency, plus inefficiency regardless. A second strategy, also simple to implement, is to estimate ρ and $\boldsymbol{\beta}$ by OLS regression of a model that includes $\mathbf{X}$, the domestic and common-stimuli factors, and $\mathbf{Wy}$, the spatial lag, on the right-hand side. We call this *spatial OLS*, or S-OLS.[19] Of course, one could estimate S-OLS models without domestic or common-stimuli factors, but this would just replace one substantive omission, of IPE-with-diffusion considerations, with another equally, if inversely, damaging omission, of (context-conditional) open-economy CPE considerations. Unfortunately, because $\mathbf{Wy}$ is endogenous (as explained below), S-OLS estimates will suffer simultaneity bias and inconsistency, even if the empirical model reflects both sorts of considerations perfectly. This is a crucial difference from temporal dependence. When temporal dynamics are modeled perfectly by temporally lagged dependent variables (LDV), OLS is BLUE. S-OLS, contrarily, will suffer simultaneity bias even when the spatial-lag structure is perfectly specified. A third strategy is to estimate ρ and $\boldsymbol{\beta}$ by maximum likelihood in a model that specifies the endogeneity of $\mathbf{Wy}$ (Ord 1975). This ML approach can be very difficult to implement in models with any but the simplest forms of spatial dependence, but its parameter estimates would be consistent and asymptotically efficient.[20] A fourth strategy is to instrument for $\mathbf{Wy}$ using $\mathbf{X}$ and $\mathbf{WX}$. This approach—call it "spatial two-stage-least-squares, instrumental-variables" (S-2SLS-IV, or just S-2SLS)—would produce consistent and asymptotically efficient estimates, as all well-specified IV estimates do, provided its necessary conditions are met: namely, that the $\mathbf{X}_j$ are indeed exogenous but related to $\mathbf{Y_i}$.[21] Unfortunately, as we just noted, the two simple estimators, both non-

[19] Some of the literature, including Land and Deane (1991) whom we follow some below, refer to this spatial OLS estimator as the generalized population potentials estimator (GPP) (for reasons that elude us). Doreian et al. (1984) refer to it as the QAD (quick-and-dirty) estimator.

[20] More precisely, as with all ML, these estimates would, if the model is correctly specified, be *BANC*: "best asymptotic-normal and consistent", i.e., most efficient among estimators that asymptotically normally distributed and consistent. See Doreian (1981) for a rigorous, but easy to follow, discussion of Ord's MLE.

[21] This list of estimators is far from exhaustive. For a more complete one, see Kelejian et al. (2003). See Kelejian and Robinson (1993) for a technical treatment of the spatial two-stage least squares estimator.

spatial and spatial OLS, are inconsistent; i.e., their estimates of model parameters do not converge to the true parameter values as sample sizes increase. This does not imply, however, that either of these biases and inconsistencies will necessarily be large, nor certainly that they will be equal, nor even that a consistent estimator like S-2SLS or S-ML will necessarily have smaller mean-squared error, so the relative performance of this S-OLS estimator is critically important to assess. Our experiments suggest that the biases of non-spatial OLS are generally large whereas those for S-OLS, especially with appropriately specified models of domestic and exogenous-external factors, are typically smaller, especially insofar as the overall strength of diffusion, ρ, remains modest, but that spatial OLS can perform adequately in mean-squared error comparison to S-ML or S-2SLS, even given perfect instruments, in small samples with modest diffusion strength.

B. Previous Comparisons of Estimation Strategies for Spatially Interdependent Data

A comprehensive review of the literature comparing estimators for the spatial lag model is beyond the scope of this paper, but we highlight four studies that are particularly important for our own, beginning with Doreian (1981), which offers one of the earliest empirical comparisons of non-spatial OLS, S-OLS, and S-ML. He evaluates these estimators in a replication of Mitchell's (1969) analysis of the Huk Rebellion and in several analyses of vote shares for Democratic presidential candidates in Louisiana parishes. In these and other early comparisons, S-ML, which is known to produce BANC estimates, provides the benchmark-optimal estimates. Doreian finds that the non-spatial OLS coefficient estimates are inflated relative to the S-OLS and S-ML estimates. That is, either the simple S-OLS or the complex S-ML models sufficed to show that non-spatial OLS tends to inflate estimates of non-spatial regressors, and both improved upon those estimates. In our substantive terms, non-spatial OLS tends to find too large effects of domestic conditions on domestic policies and outcomes because it omits the diffusion of conditions abroad. However, Doreian's S-

OLS and S-ML estimates of the magnitudes of diffusion are similar in the vote-share analyses but not in the Huk-rebellion analyses. That is, although either S-OLS or S-ML seemed sufficient to demonstrate the typical biases of non-spatial OLS and to improve estimates of the effects of non-spatial factors, the simpler S-OLS may not always give good estimates of diffusion itself or improve estimates of non-spatial effects as well as does S-ML.

Doreian, Teuter, and Wang (1984) compare the same three estimators using Monte Carlo experiments. In their simulations with spatially dependent units, Doreian et al. find that non-spatial OLS inflates coefficient estimates on non-spatial factors while OLS standard errors underestimate the true sample variability. That is, they also find that non-spatial OLS will tend to over-estimate the effects of non-spatial factors when spatial interdependence is in fact present but now, additionally, that non-spatial OLS standard errors will tend to be too low. That combination implies greater t-ratios (higher numerator and lower denominator) than warranted, and so over-confident conclusions for over-sized effects of non-spatial factors. Thus, analysts using non-spatial OLS are more likely to make Type I inferential errors than their p-values suggest (i.e., they will reject null hypotheses sometimes when they should not). However, these Monte Carlo experiments also showed that, as ρ increases beyond a relatively modest level, around 0.1 in linear-regression models, S-OLS begins to overestimate ρ and underestimate the other coefficients somewhat. Plus, the S-OLS standard errors tend to underestimate true sample variability too. Thus, at least where spatial interdependence of appreciable average magnitude exists, S-OLS tends to incur the converse danger of over-confidently over-estimating diffusion effects while underestimating non-spatial effects. We refer to this phenomenon as the inverse spatial Hurwicz bias and the problematic tradeoff it suggests as the spatial Hurwiczian dilemma. Again, the conclusion was that S-ML is preferred (and perhaps S-2SLS also, we would add), although the much simpler S-OLS may perform adequately in some scenarios.

Land and Deane (1992) offer one of the earliest evaluations of the two-stage least-squares instrumental-variable approach to estimating models of spatial interdependence. They compare two similar S-2SLS estimators[22] with non-spatial OLS and S-ML. They find that both S-2SLS estimators and the S-ML estimator produce similar results that, in turn, differ from non-spatial OLS estimates. They also conclude that the non-spatial OLS estimates are decidedly inferior. Kelejian, Prucha, and Yuzefovich (2003) conduct Monte Carlo experiments that compare the S-2SLS and S-OLS (among others). They use a first-order autoregressive spatial model (in **y**), with first-order autoregressive disturbances as well, to generate their experimental data. In such data, they find the S-2SLS estimator generally outperforms S-OLS by the root mean squared error (RMSE) criterion,[23] although, again, not too notably so under when ρ is small.

For our needs, all of these studies leave one or more critical gaps. Among other things, the earliest studies used actual data and so did not know the true parameter values but could only hold S-ML estimates as optima, which, of course, they might not be, especially in limited samples. Many of the later studies compare only some subset of estimators that we would like to explore: non-spatial OLS, S-OLS, S-2SLS, and S-ML. All but the most recent have studied purely cross-sectional data whereas we are interested in time-series-cross-sectional data.[24] And, most importantly, none of these studies considered directly the main alternative-hypothesis for the source of spatial correlation:

[22] The only difference is whether one instruments for **Wy** at the first stage or for **y** only in the first stage, applying **W** to the instrumented **y** in the next step.

[23] RMSE equals the square root of the variance, v, plus the squared bias, b, or $(b^2+v)^{.5}$, and thus combines bias (consistency) and efficiency considerations.

[24] Although we are ultimately also interested in time-series-cross-section data and methodological issues involved in simultaneous spatial and temporal dependence, our current simulations do not probe the temporal dimension beyond the fact that we have several observations per cross-sectional unit (i.e., the data exhibit no temporal dependence), whereas most of these studies involved just one observation per cross-sectional unit.

common (or correlated) external stimuli. Our Monte Carlo experiments will explore these four alternative estimators in data generated by a true model of type D above, i.e., including both *common stimuli, varying responses* and *diffusion*, but, first, we offer some analytical results delineating the precise biases of non-spatial and spatial OLS estimation in the simplest possible case.

IV. Analytical Results and Experimental Design

A. *Non-spatial OLS & Omitted-Variable Bias v. Spatial OLS & Simultaneity Bias*

In this section, we demonstrate analytically, in the simplest possible case—one domestic factor, *X*; diffusion between the outcomes of two countries, 1 and 2; and conditionally independent and identically distributed (*i.i.d.*) errors, ε—that both non-spatial and spatial OLS estimates will be biased and inconsistent in the presence of diffusion, and we specify those biases insofar as possible.

$$Y_1 = \beta_1 X_1 + \rho_{12} Y_2 + \varepsilon_1 \qquad (2);$$

$$Y_2 = \beta_2 X_2 + \rho_{21} Y_1 + \varepsilon_2 \qquad (3).$$

Substituting country 2's outcome from equation (3) into equation (2), which determines country 1's outcome, gives

$$Y_1 = \beta_1 X_1 + \rho_{12}\left(\beta_2 X_2 + \rho_{21} Y_1 + \varepsilon_2\right) + \varepsilon_1 \qquad (2').$$

Performing the converse substitution gives

$$Y_2 = \beta_2 X_2 + \rho_{21}\left(\beta_1 X_1 + \rho_{12} Y_2 + \varepsilon_1\right) + \varepsilon_2 \qquad (3'),$$

which can be solved for Y_1 to give

$$Y_1 = \frac{\beta_1}{1-\rho_{21}\rho_{12}} X_1 + \frac{\rho_{12}\beta_2}{1-\rho_{21}\rho_{12}} X_2 + \frac{\rho_{12}}{1-\rho_{21}\rho_{12}} \varepsilon_2 + \frac{1}{1-\rho_{21}\rho_{12}} \varepsilon_1 \qquad (2'').$$

The system is symmetric, so the analogous reduced-form equation for Y_2 is

$$Y_2 = \frac{\beta_2}{1-\rho_{21}\rho_{12}} X_2 + \frac{\rho_{21}\beta_1}{1-\rho_{21}\rho_{12}} X_1 + \frac{\rho_{21}}{1-\rho_{21}\rho_{12}} \varepsilon_1 + \frac{1}{1-\rho_{21}\rho_{12}} \varepsilon_2 \qquad (3'').$$

From any of these equations, one can see that S-OLS estimation of (2) and/or (3) will be biased and inconsistent, even if both the domestic and the diffusion aspects of the model were specified for estimation exactly as in these equations.

In each (2) and (3), notice that a right-hand-side regressor, Y_2 and Y_1 respectively, is itself partly determined in the other equation by the dependent variable of the current equation: a textbook illustration of endogeneity. In (2') and (3'), one can see the same issue differently. A right-hand-side variable, Y_1 and Y_2 respectively, is now also the left-hand-side variable, and so obviously simultaneous. Alternatively, recall that Y_1 and Y_2 contain ε_1 and ε_2 respectively, and so, as regressors, they would correlate with that equation's residual. This is the formal assumption on which the unbiasedness and consistency of OLS rests, that $Cov(Z,\varepsilon)=0$ where Z are the regressors. Finally, from (2") and (3"), one can see that Y_1 and Y_2 also contain ε_2 and ε_1 respectively, so Y_1 as a right-hand-side regressor for an equation with Y_2 as dependent variable will indeed have $Cov(Z,\varepsilon)\neq 0$.

Indeed, from (2"), one can see that the covariance of the regressor Y_1 with the residual ε_2 is

$$
\begin{aligned}
&Cov\left(\frac{\beta_1}{1-\rho_{21}\rho_{12}}X_1+\frac{\rho_{12}\beta_2}{1-\rho_{21}\rho_{12}}X_2+\frac{\rho_{12}}{1-\rho_{21}\rho_{12}}\varepsilon_2+\frac{1}{1-\rho_{21}\rho_{12}}\varepsilon_1\ ,\ \varepsilon_2\right)\\
&\quad=Cov\left(\frac{\rho_{12}}{1-\rho_{21}\rho_{12}}\varepsilon_2\ ,\ \varepsilon_2\right) \qquad\qquad (4).\\
&\quad=\left(\frac{\rho_{12}}{1-\rho_{21}\rho_{12}}\right)Var(\varepsilon_2)
\end{aligned}
$$

We can then combine (4) with the following form of the OLS estimates of any set of coefficients $\boldsymbol{\gamma}$

$$\hat{\gamma}=\gamma+\left(\frac{Z'Z}{n}\right)^{-1}\frac{Z'\varepsilon}{n} \qquad (5),$$

the second term of which gives the bias in any OLS coefficient estimates. Note that $Z'\varepsilon/n$ is the covariance of the regressors with the residual, ε_2, which is zero for X_2 (assuming it is indeed exogenous) and given by (4) for Y_1. Thus, the simultaneity bias in ρ_{21} is

$$bias(\hat{\rho}_{21}) = \left(\frac{Z'Z}{n}\right)^{-1}\left(\frac{\rho_{12}}{1-\rho_{12}\rho_{21}}\right)Var(\varepsilon_2) \qquad (6).$$

$Z'Z/n$ is a variance-covariance matrix (of the regressors, X_2 and Y_1); as such, it is positive definite. Therefore, the simultaneity bias in the estimate of ρ_{21} will have the same sign as the true ρ_{12}. Note the subscript reversal! It is intuitive, actually. If, for example, Germany affects the US negatively while the US affects Germany positively, then the simultaneity bias in OLS estimates of the US→Germany diffusion would, by ignoring the dampening feedback from the negative Germany→US diffusion, lead one to underestimate the positive US→Germany diffusion. Conversely, trying to estimate the negative Germany→US diffusion by S-OLS would incur positive simultaneity bias by ignoring the dampening US→Germany feedback. Thus, oppositely signed diffusion induces a simultaneity bias in S-OLS that fosters underestimation of diffusion. Most diffusion mechanisms seem more likely to induce positive (i.e., same-signed) interdependence, however, so the simultaneity bias in S-OLS probably more commonly tends to inflate diffusion estimates.

However, the bias need not be sizable, certainly does not preclude that S-OLS might improve dramatically upon non-spatial OLS, and may be outweighed in mean-squared-error terms even compared to methods that do address the simultaneity, such as S-2SLS, by the generally greater efficiency of OLS than these alternatives. Indeed, in some sample and parameter conditions, this is what we find below, even though our instruments are (unrealistically) perfect. For now, note that the simultaneity bias of S-OLS will not be large unless the function of ρ in the second term of (6) times the "ratio" of the variance of ε_2 to the variance-covariance matrix of X_1 and Y_2 is also large. This means, essentially, that the simultaneity induced overestimation of ρ would not be large unless diffusion itself is relatively strong in truth and the outcomes are relatively highly stochastic, i.e., relatively inexplicable by the exogenous factors of the model (here, X_1). If, for example, one sought to estimate the effect of US inflation-rates on other Bretton-Woods members' inflation-rates (as

Franzese 2003 does) by S-OLS (S-NLS in his case; let's say S-LS here), assuming the US affects other countries but they do not affect it, then ρ_{12} (others on the US) is zero, and no simultaneity bias arises at all in the S-LS estimates of ρ_{21} (US on others). More realistically, diffusion from other developed democracies' inflation rates to the US inflation rate was certainly much smaller than in the other direction under Bretton Woods, and probably small in absolute terms, in which case the simultaneity bias would small too. By this argument, Franzese's slightly-biased-but-highly-efficient S-LS estimates would compare favorably to possible consistent-but-inefficient S-2SLS alternatives, especially if the latter's exogeneity assumptions did not hold perfectly so that its estimates were only more-consistent/less-unbiased than those of S-LS. The comparison to non-spatial LS, moreover, would not be worth making.

In sum, we just showed that S-OLS suffers a simultaneity bias in the estimation of diffusion effects, but we cannot tell from this whether these biases are appreciable in common circumstances, much less how they compare to problems with alternatives like non-spatial OLS, S-2SLS, or S-ML. Accordingly, we will explore in the next section more complex and realistic scenarios through simulations than we can solve analytically.

First, though, we can also show that the simultaneity bias in estimating ρ—inflating diffusion effects in the case of mutual-reinforcement (same-signed) mechanisms—induces an oppositely signed bias in the estimate of β—dampening the effects of domestic and external stimuli in that more-common case. We find this easiest to demonstrate in mean-deviated scalar notation, which gives the standard formula for the OLS estimate of β in mean-deviated X_1 and Y_2 as

$$\hat{\beta}_1 = \frac{\left(\sum X_1 Y_1\right)\left(\sum Y_2^2\right) - \left(\sum Y_2 Y_1\right)\left(\sum X_1 Y_2\right)}{\left(\sum X_1^2\right)\left(\sum Y_2^2\right) - \left(\sum X_1 Y_2\right)^2} \qquad (7).$$

Using equation (2) to substitute for Y_1 gives

$$\hat{\beta}_1 = \frac{\left(\left(\sum X_1\beta_1X_1\right)+\left(\sum X_1\rho_{12}Y_2\right)+\left(\sum X_1\varepsilon_1\right)\right)\left(\sum Y_2^2\right)-\left(\left(\sum Y_2\beta_1X_1\right)+\left(\sum Y_2\rho_{12}Y_2\right)+\left(\sum Y_2\varepsilon_1\right)\right)\left(\sum X_1Y_2\right)}{\left(\sum X_1^2\right)\left(\sum Y_2^2\right)-\left(\sum X_1Y_2\right)^2} \quad (8).$$

Assuming for convenience that X_1 is fixed and has a variance of one, allows us to simplify to

$$\hat{\beta}_1 = \beta_1 + \frac{\left(\sum X_1\varepsilon_1\right)\left(\sum Y_2^2\right)-\left(\sum X_1Y_2\right)\left(\sum Y_2\varepsilon_1\right)}{\left(\sum Y_2^2\right)-\left(\sum X_1Y_2\right)^2} \quad (9).$$

The first product in the numerator is zero by the assumption that X_1 is exogenous. Using equation (3") to substitute for the variance and covariance terms in the rest of the equation gives

$$\hat{\beta}_1 = \beta_1 - \frac{\left(\frac{\rho_{21}\beta_1}{1-\rho_{21}\rho_{12}}\right)\left(\frac{\rho_{21}Var(\varepsilon_1)}{1-\rho_{21}\rho_{12}}\right)}{\left(\frac{\beta_2}{1-\rho_{21}\rho_{12}}\right)^2+\left(\frac{\rho_{21}}{1-\rho_{21}\rho_{12}}\right)^2 Var(\varepsilon_1)+\left(\frac{1}{1-\rho_{21}\rho_{12}}\right)^2 Var(\varepsilon_2)} \quad (10),$$

which simplifies to

$$\hat{\beta}_1 = \beta_1 - \frac{\beta_1 Var\left(\varepsilon_1\right)\rho_{21}^2}{\beta_2^2+\rho_{21}^2 Var\left(\varepsilon_1\right)+Var\left(\varepsilon_2\right)} \quad (11).$$

By a parallel series of steps, we can reduce the formula in for the S-OLS estimate of ρ_{12} to

$$\hat{\rho}_{12} = \rho_{12} + \frac{\rho_{21}Var(\varepsilon_1)(1-\rho_{21}\rho_{12})}{\beta_2^2+\rho_{21}^2 Var\left(\varepsilon_1\right)+Var\left(\varepsilon_2\right)} \quad (12).$$

Again, the second terms of (12) and (11) are the simultaneity bias in S-OLS estimates of ρ_{12} and the induced bias in the estimate of β_1 respectively. Note that this term has a negative sign in (11) but a positive sign in (12) and that the denominator terms are identical (and positive). Therefore, comparing the numerators will give us the relative signs of the biases in ρ_{12} and β_1; if the numerators have the same sign, the biases will have opposite sign and *vice versa*. In what we believe to be the more common case of positively reinforcing diffusion (ρ_{21} and ρ_{12} are positive), $\hat{\rho}_{12}$ is positive and will have positive bias and so be inflated while (a) if β_1 is positive, $\hat{\beta}_1$ will have negative bias and so be attenuated whereas (b) if β_1 is negative, $\hat{\beta}_1$ will have positive bias and so be attenuated. The simultaneity bias inflating estimates of ρ (diffusion-strength) thus induce a converse attenuation bias in the estimates of β (domestic or non-spatial effects). The implications of other combinations derive analogously, although keeping signs straight can cause headaches.

The upward bias in β induced by omitting the spatial lag when diffusion actually exists—i.e., the omitted-variable bias in non-spatial OLS—is much easier to demonstrate, given the well-known formula for omitted-variable bias, $\boldsymbol{F\beta}$, where $\boldsymbol{F}$ is the matrix of (true) coefficients obtained by regressing the omitted on the included variables and $\boldsymbol{\beta}$ is the vector of (missing, true) coefficients on the omitted variables. In the case of non-spatial OLS, the omitted variable is the right-hand-side *Y*, and the included variable is the *X*. Consider the case of estimating (2) by non-spatial OLS. The omitted variable is Y_2, whose true coefficient is ρ_{12}. Equation (3") gives the true coefficient of the omitted, Y_2, regressed on the included, X_1, as $\rho_{21}\beta_1/(1- \rho_{21}\rho_{12})$. Thus, the omitted variable bias of estimating (2) and (3) by non-spatial OLS, OVB_2 and OVB_3 respectively, are

$$OVB_2 = \frac{\rho_{12}\rho_{21}\beta_1}{1-\rho_{12}\rho_{21}} \qquad (13),$$

and

$$OVB_3 = \frac{\rho_{12}\rho_{21}\beta_2}{1-\rho_{12}\rho_{21}} \qquad (14).$$

From these analytical results, we can see that both non-spatial OLS and S-OLS are biased and inconsistent, and that the magnitudes of these biases depend heavily upon and tend to increase with the general strength of the diffusion process. Even in this simple case, however, determining which, if either, or both of these bias magnitudes will typically be appreciable, and which might typically be the larger problem, and how either might compare with S-2SLS or S-ML would seem, and probably are, impossible, so we turn now to Monte Carlo simulation of richer, more realistic scenarios.

B. Design of Our Monte Carlo Experiments

We use a reduced form of the spatial-lag model to generate the data for our experiments:[25]

$$\mathbf{y} = (\mathbf{I} - \rho\mathbf{W})^{-1}\mathbf{X}\beta + (\mathbf{I} - \rho\mathbf{W})^{-1}\varepsilon \tag{15}$$

Our $\mathbf{X}$ matrix (of non-diffusion elements) has three parts: $\boldsymbol{\xi}$, $\boldsymbol{\eta}$, and $\boldsymbol{\xi\eta}$. The vector $\boldsymbol{\xi}$ is an NTx1 stack of *i.i.d.* draws from a standard normal distribution. These observations, unique to each spatial unit in each time period, represent purely domestic variables. Analytically, these $\boldsymbol{\xi}$ represent the set of domestic institutions, structures, and conditions that exist in each unit *i* at each time *t*. The vector $\boldsymbol{\eta}$ is an NTx1 stack of T vectors, each Nx1 in size, and each element of which is identical. That is, each of the T vectors has N elements that are all the same, but each of the T vectors can differ from the others. Thus, $\boldsymbol{\eta}$ represents a set of globally common shocks, one occurring in each of the T periods. These shocks are also drawn *i.i.d.* from a standard normal distribution. The interaction term, $\boldsymbol{\xi\eta}$, captures the idea that the effects of common external shocks are mediated by domestic variables. In other words, our domestic model is of the context-conditional-open-CPE sort. Additionally, however, the model will involve diffusion of the sort IPE implies, with average magnitude ρ and with specific connections from unit *j* to unit *i* of magnitudes w_{ij}.

Drawing the data for $\boldsymbol{\xi}$, $\boldsymbol{\eta}$, and $\boldsymbol{\xi\eta}$—i.e., for $\mathbf{X}$—in this manner, and drawing $\boldsymbol{\varepsilon}$ also independently from standard normal distributions, we generate the data for **Y** using two different sets of coefficients, $(\beta_1, \beta_2, \beta_3, \rho)$, and two different spatial weighting matrices, **W**, in (2). For coefficients, we use $(\beta_1, \beta_2, \beta_3, \rho) = (1,1,1,0.1)$ and $(\beta_1, \beta_2, \beta_3, \rho) = (1,1,1,0.5)$. Note that one set of coefficients has smaller ρ than the other, and recall that the spatial weighting matrix determines the relative importance of each unit to each of the others in the pattern of spatial diffusion while ρ

[25] Our model differs from Kelejian et al.'s in that we use spatially orthogonal disturbances.

determines the average strength of diffusion. Thus, the second set of coefficients represents a stronger diffusion process. In some experiments, we also examine the consequences of cross-spatial endogeneity—that is, when some unit(s)'s *y*'s cause *x*'s in other unit(s). Substantively, this might be the case if, say, military-spending outcomes in *i* affect (voters' choices) and so government partisanship in *j*. Tables 1 and 2 explore the relative performance of our estimators in the case of weak diffusion. In Tables 7 and 8, we evaluate the estimators under weak diffusion and weak cross-spatial endogeneity. Tables 3-6 and 9-10 focus on the more interesting cases of relatively strong diffusion and cross-spatial endogeneity. We assume the spatial weights are time-invariant so all the elements along the diagonals of the TxT off-diagonal blocks of $\mathbf{W}$ are the same. That is, only one w_{ij} connecting *j* to *i* persists for all T periods; this connectivity does not change from period to period.

We consider two rules to generate the patterns of interdependence between spatial units. We first set all of these w_{ij} equal to 1/(N-1). In this case, every unit affects every other unit equally, and the appropriate right-hand-side spatial lag for each unit-year to reflect this proposition would be an unweighted average of the dependent variable for the other units in that year. This is also equivalent, up to a scaling factor of 1/(N-1), to an unweighted sum, and basically equivalent, especially for binary outcomes and relatives like durations, to the counts or proportions of the other units with $y=1$ *or* $y=0$ often used in those contexts.[26] We evaluate the relative performance of our estimators under this condition in Tables 1-4 and 6-10. For the second spatial weighting matrix we add a random draw from a uniform distribution with support [–0.1, +0.1] to 1/(N-1). This represents a heterogeneous pattern of diffusion in which the connections between units differ by some amount for each directed dyad from the all-equal one just described. Varying the weighting matrix thusly will enable us to

[26] Franzese (2002) also employed exactly this unweighted-average spatial-lag in his empirical models, but, his arguments being of the context-conditional open-economy CPE sort, he then viewed these spatial lags as nuisance controls and did not emphasize their diffusion implications in theoretical or substantive discussion.

explore the implications of unmodeled or imperfectly modeled heterogeneity in the pattern of diffusion, an obviously key practical consideration for any attempt to model diffusion (Table 5). Researchers usually do not know the true **W** but offer a theoretically and/or substantively inspired guess at it. As described below, we will likewise explore ignoring or imperfectly modeling common shocks, which will enable us to compare non-spatial OLS, which omits diffusion, to S-OLS omitting or imperfectly modeling the *common stimuli, varying responses* alternative hypothesis (Table 6).

We evaluate the non-spatial OLS, S-OLS, S-2SLS, and S-ML estimators, the first two with and without panel-corrected standard-errors (i.e., estimates of the variance-covariance matrix of the coefficient estimates that are "robust to", i.e., consistent in the presence of, spatial correlation). We report results for samples with dimensions N = 5, 40 and T = 20, 40.[27] The small N case is considered in Tables 1, 3, 7, and 9. The other experiments focus on the relatively large N case.

To estimate using S-OLS, S-2SLS, or S-ML, the researcher must first specify a spatial weighting matrix. This is a critical theoretical and empirical step for the scholar of diffusion; as we have already emphasized, distinguishing between and evaluating the relative strength of diffusion and exogenous-external factors (Galton's famous problem) relies in the first order upon the relative precision with which these alternative bases of spatial correlation are specified and how much these alternative specifications actually differ empirically. For all the estimations in our simulations, however, simply setting all non-zero elements of **W** to 1/(N-1) suffices to explore the possible abstract scenarios. That is, the hypothetical researcher estimates an equation with a spatial lag given by the unweighted average of the dependent variable in the other cross-sectional units each period, with or without instrumentation in the S-2SLS or S-OLS and S-ML cases, respectively. Thus, in the first set of experiments—the all-equal diffusion case—the weighting matrix used for estimation will

[27] These results are sufficient to demonstrate our major experimental findings. A much larger set of results, which includes additional sample dimensions and sizes, is available from the authors upon request.

be the true weighting matrix; the researcher will have specified the diffusion process exactly correctly (Tables 1-4, and 6-10). In the other case, the researcher will have specified a weighting matrix comprised of imperfect, although unbiased, estimates of the true spatial weights (Table 5). This imperfection reflects the obvious realism that $\mathbf{W}$ is not observed, so analysts will not be using the true spatial weighting matrix for estimation, but rather some theoretically or conveniently specified approximation (of varying accuracy) to it. Note that the specification errors are random (unbiased) in our experiment. This is perhaps the more interesting case to explore since the consequences of systematic error are relatively straightforward.[28]

Likewise, we use our simulations to explore the ramifications, primarily for estimates of diffusion, of imperfectly specifying the exogenous-external stimuli that may also affect the outcome in spatially correlated fashion. We do this by varying the hypothetical researcher's estimation model from specifying the common shock perfectly to getting it half right. First, we actually generate $\boldsymbol{\eta}_t$ as the sum of independent draws from two normal distributions, each with mean 0 and variance ½, which results in $\boldsymbol{\eta}_t$ being independently standard-normally distributed as stated. Then, we consider two estimation models: including all of $\boldsymbol{\eta}_t$ in the model (Tables 1-5 and 7-10) and including only one of its components (Table 6). This enables us to explore the relative magnitudes of the improvements offered by S-OLS models of diffusion over non-spatial OLS as the hypothetical diffusion-scholar's controls for the main alternative, common shocks, improve.

The results for all of our Monte Carlo experiments are based on at least 100 simulations (1000 for the key ones reported).[29] We report the mean coefficient estimates, the mean conventional standard-error estimates, the mean PCSE estimates, the actual standard deviation of the coefficient

[28] For example, if analysts systematically exclude (i.e., attach zero weight to) important units in the spatial weighting matrix, it will obviously bias against finding diffusion effects.

[29] Because S-ML is computationally intensive, our evaluations of its properties are always based on 100 trials.

estimates, and the root mean-squared error (RMSE) of the coefficient estimates. Comparing the mean of the coefficient estimates to the true value of that parameter in that experiment gives the bias. We chose coefficients of 1 (and ρ of .1 or .5) so the percentage bias may be seen directly. By comparing the mean of the estimated standard errors to the actual standard deviation of coefficient estimates, we can observe the potential over-confidence of conventional standard errors and whether and how well PCSE's may redress any such over-confidence. Finally, the RMSE's, being the square root of the sum of the bias-squared and the actual variance of the coefficient estimates, offer summary evaluation combining both bias/consistency and efficiency considerations.

V. Simulation Results

We start by comparing the two simple but inconsistent estimators: non-spatial and spatial OLS. Is it ever reasonable to use these estimators? How large are their respective biases? Tables 1 and 2 give the results for $\rho=0.1$ and $w_{ij}=1/(N-1)$. In Table 1, N is 5; in Table 2, N is a much larger 40; both tables report results for T=20 and T=40. In these experiments, the spatial weights are correctly specified and the diffusion is relatively weak. Therefore, we expect both the omitted-variable and the simultaneity biases to be small such that non-spatial OLS performs somewhat but not terribly poorly and spatial OLS offers some improvement over that.

Table 1. Comparing Estimators (N=5, $\rho = 0.1$, w_{ij} = (1/N-1), 1,000 trials)

		OLS			S-OLS			S-2SLS			ML (100 Trials)		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	.998	.104	.103	.993	.105	.105	.992	.106	.106	0.972	0.121	0.124
	$s.e.(\beta_1)$	.105	.012		.106	.012		.107	.014		0.107	0.012	
	$pcse(\beta_1)$	.103	.012		.103	.013							
	β_2	1.112	.124	.167	1.027	.215	.216	1.003	.236	.235	1.046	0.219	0.222
	$s.e.(\beta_2)$	.108	.020		.175	.037		.2127	.081		0.181	0.036	
T=20	$pcse(\beta_2)$	.111	.026		.163	.042							
	β_3	1.005	.115	.115	1.002	.117	.117	1.001	.119	.119	0.977	0.139	0.14
	$s.e.(\beta_3)$	.111	.024		.111	.024		.113	.026		0.115	0.024	
	$pcse(\beta_3)$	.108	.025		.108	.025							
	ρ				.078	.154	.155	.097	.177	.177	0.074	0.141	0.143
	$s.e.(\rho)$				.119	.024		.158	.068		0.074	0.006	
	$pcse(\rho)$				.112	.028							
	β_1	1.008	.072	.072	1.004	.071	.071	1.003	.072	.071	1.003	0.072	0.072
	$s.e.(\beta_1)$	.073	.005		.072	.005		.073	.005		0.071	0.006	
	$pcse(\beta_1)$	.072	.005		.072	.005							
	β_2	1.112	.081	.139	.991	.125	.125	1.001	.139	.139	1.025	0.120	0.122
	$s.e.(\beta_2)$	.074	.009		.116	.016		.135	.026		0.103	0.012	
T=40	$pcse(\beta_2)$	.079	.013		.110	.018							
	β_3	1.002	.075	.075	1.000	.075	.075	.999	.075	.075	0.995	0.086	0.085
	$s.e.(\beta_3)$	.075	.011		.075	.011		.075	.011		0.073	0.011	
	$pcse(\beta_3)$	.0738	.011		.074	.011							
	ρ				.108	.092	.093	.099	.103	.103	0.072	0.066	0.072
	$s.e.(\rho)$				.079	.011		.100	.023		0.049	0.003	
	$pcse(\rho)$				.076	.012							

In these experiments, the omitted variable bias in the non-spatial OLS estimates manifests primarily in β_2, the coefficient on the common shock.[30] In Table 1, OLS overestimates the (so-called) direct effect of these common shocks by 0.112 on average (i.e., by about 11%). All of the other parameter estimates are very close to their true values. The average S-OLS estimates of β_2 are indeed much better. The size of the biases ranges from -0.009 to +0.027 (or about 3.6%). Note, however, that the sampling variability of the S-OLS estimator for β_2 is large (due to multicolinearity) relative to non-spatial OLS. This makes non-spatial OLS preferable on RMSE grounds; however, this result only holds for very small samples and weak diffusion.

S-OLS overestimates β_2 in one of the experiments and underestimates it in the other. The biases in the S-OLS estimates of β_2 and ρ are also negatively related; i.e., if β_2, the effect of common shocks, is overestimated, then ρ, the strength of diffusion, is underestimated, and *vice versa*. This robust finding, which is more pronounced when diffusion is strong, underscores the difficulty of isolating the effects of common external shocks from diffusion using the non-spatial and S-OLS estimators; the negative relationship is intuitive and consistent with Doreian et al.'s (1984) results, and with our analytic demonstration for the positive true β_2 and ρ case above. The S-2SLS estimator performs the best in terms of bias but does not improve on S-OLS (or OLS) in RMSE terms. Perhaps surprisingly, S-ML does not outperform S-OLS in either bias or efficiency terms. Increasing N from 5 to 40 reduces the variability of all four estimators' sampling distributions (Table 2). This is most noticeable for β_1 and β_3, the coefficients on the domestic variable and the interaction of it with the common shock, both of which are estimated much more precisely with a large N.

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[30] We suspect that, but have not yet explored whether, the omitted-variable biases concentrate so heavily, almost exclusively, in the coefficient on the common shock, and are almost absent from the estimates of the domestic and domestic-factor-moderated responses to common shocks, because our pattern of diffusion weights is uniform and unrelated to those domestic factors.

Table 2. Comparing Estimators (N=40, $\rho = 0.1$, w_{ij} = (1/N-1), 1,000 trials)

		OLS			S-OLS			S-2SLS			ML (100 Trials)		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.000	.036	.036	.999	.036	.036	.999	.036	.036	1.001	0.037	0.037
	s.e.(β_1)	.036	.002		.037	.002		.037	.002		0.036	0.002	
	pcse(β_1)	.036	.002		.036	.002							
	β_2	1.112	.041	.119	1.049	.209	.215	.994	.211	.211	1.042	0.172	0.176
	s.e.(β_2)	.038	.007		.152	.035		.199	.083		0.127	0.026	
T=20	*pcse*(β_2)	.039	.010		.144	.046							
	β_3	1.000	.038	.038	.999	.038	.038	1.000	.038	.038	0.999	0.032	0.032
	s.e.(β_3)	.038	.007		.038	.007		.038	.007		0.039	0.007	
	pcse(β_3)	.038	.007		.037	.007							
	ρ				.055	.186	.191	.105	.188	.188	0.062	0.143	0.147
	s.e.(ρ)				.133	.030		.175	.074		0.025	0.001	
	pcse(ρ)				.126	.040							
	β_1	1.001	0.026	0.026	1.001	0.026	0.026	1.001	0.026	0.026			
	s.e.(β_1)	0.025	0.001		0.025	0.001		0.025	0.001				
	pcse(β_1)	0.025	0.001		0.025	0.001							
	β_2	1.112	0.03	0.116	0.999	0.119	0.119	1.003	0.136	0.136			
	s.e.(β_2)	0.026	0.003		0.101	0.016		0.126	0.032				
T=40	*pcse*(β_2)	0.028	0.005		0.096	0.02							
	β_3	1	0.026	0.026	1	0.026	0.026	1	0.026	0.026			
	s.e.(β_3)	0.026	0.003		0.026	0.003		0.026	0.003				
	pcse(β_3)	0.026	0.003		0.026	0.003							
	ρ				0.101	0.105	0.105	0.098	0.12	0.12			
	s.e.(ρ)				0.087	0.014		0.111	0.028				
	pcse(ρ)				0.083	0.017							

When T is small, the mean reported standard errors seem to underestimate the true variability in the S-OLS estimator.[31] This is particularly true for the standard errors on β_2 and ρ, nor do panel-corrected standard-errors (PCSE's) seem to provide better estimates. (However, with non-spatial OLS, PCSE's do provide better standard error estimates for β_2.)[32] When N=40 and T=20 (Table 2), the mean reported standard error for β_2 underestimates the standard deviation of the coefficient estimates by 27% (0.152 vs. 0.209). The mean reported standard error for ρ understates the true sampling variability by 28% (0.133 vs. 0.186). These numbers drop to 15% and 17% respectively when T is increased to 40, but the problem worsens as N increases. When N=5 and T=20 the mean reported standard errors for β_2 and ρ underestimate the observed sample variability by 19% and 23% respectively. The S-ML estimator also underestimates the standard errors for β_2 and ρ. When N=40 and T=20, the mean estimated standard-error for β_2 understates true sampling variability by 26% (0.127 vs. 0.172), and the mean estimated standard-error for ρ understates the true value by 83% (!). As with S-OLS, the degree of overconfidence rises with N, so, when N is small, the same estimates fall below the true values by a lesser 17% and 48%. Viewed from the other dimension, these results imply that the degree of overconfidence declines with T. Thus, samples with larger time dimensions relative to cross-sectional ones seem to aid separating common shocks from diffusion and obtaining

[31] This is also consistent with Doreian et al. (1984).

[32] This is not surprising. As with any White's style standard-error correction, PCSE's are robust to patterns of residual heteroskedasticity and/or contemporaneous correlation that are somehow related to the patterns of variances and covariances of the regressors (the X'X matrix). Since our residuals are *i.i.d.*, when the model is correctly specified (as is the case with S-OLS), we would not expect PCSE or any other robust variance-covariance estimator to differ on average from the OLS variance-covariance estimator except by being slightly less efficient. PCSE's do offer purchase when model misspecification introduces non-sphericity of a pattern related to X'X matrix into the error term. Hence, with non-spatial OLS, the PCSE standard error estimates for β_2 are better on average than the OLS standard errors.

accurate estimates of the standard errors of these distinct effects. The S-2SLS estimator is unbiased but has a relatively large variance (i.e., it is relatively inefficient), particularly with regard to the estimates of β_2 and ρ. Therefore, when diffusion is weak this estimator may be undesirable from an RMSE viewpoint; the limited gains in terms of bias may not be sufficiently large to justify the efficiency loss inherent in using S-2SLS. Perhaps a stronger case could be made for S-2SLS over S-OLS on hypothesis-testing bases because S-OLS tends to overestimate ρ and underestimate its standard error (and S-ML radically underestimates standard errors in many conditions), but, on the other hand, these examples follow the conditions for perfect instruments, which may rarely obtain in practice. Any shortfall in those conditions would weigh back in favor of S-OLS. We therefore give "ties" or "close-calls" to the non-instrumented estimator.

Table 3. Comparing Estimators (N=5, $\rho = 0.5$, w_{ij} = (1/N-1), 1,000 trials)

		OLS			S-OLS			S-2SLS			ML (100 Trials)		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.092	0.145	0.171	0.988	0.106	0.107	0.997	0.107	0.107	1.010	0.116	0.116
	$s.e.(\beta_1)$	0.138	0.019		0.106	0.012		0.108	0.014		0.106	0.012	
	$pcse(\beta_1)$	0.131	0.021		0.104	0.013							
	β_2	1.999	0.289	1.04	0.837	0.178	0.242	0.998	0.253	0.253	1.019	0.175	0.176
	$s.e.(\beta_2)$	0.142	0.027		0.184	0.04		0.228	0.109		0.162	0.030	
T=20	$pcse(\beta_2)$	0.231	0.051		0.148	0.035							
	β_3	1.076	0.173	0.189	0.997	0.117	0.117	1.002	0.117	0.117	0.983	0.117	0.118
	$s.e.(\beta_3)$	0.146	0.032		0.112	0.024		0.115	0.026		0.110	0.022	
	$pcse(\beta_3)$	0.135	0.034		0.111	0.025							
	ρ				0.579	0.076	0.11	0.499	0.108	0.108	0.482	0.064	0.067
	$s.e.(\rho)$				0.073	0.015		0.098	0.049		0.070	0.005	
	$pcse(\rho)$				0.059	0.013							
	β_1	1.101	0.106	0.146	0.981	0.074	0.076	0.996	0.075	0.075	0.996	0.073	0.073
	$s.e.(\beta_1)$	0.097	0.009		0.073	0.006		0.074	0.006		0.072	0.005	
	$pcse(\beta_1)$	0.095	0.01		0.073	0.006							
	β_2	2.001	0.202	1.021	0.826	0.119	0.211	1	0.146	0.146	1.034	0.118	0.122
	$s.e.(\beta_2)$	0.098	0.013		0.121	0.018		0.145	0.031		0.112	0.014	
T=40	$pcse(\beta_2)$	0.166	0.025		0.102	0.016							
	β_3	1.102	0.122	0.159	0.988	0.075	0.076	1.002	0.075	0.075	1.009	0.077	0.077
	$s.e.(\beta_3)$	0.099	0.015		0.075	0.012		0.076	0.012		0.075	0.011	
	$pcse(\beta_3)$	0.096	0.017		0.075	0.012							
	ρ				0.587	0.049	0.1	0.5	0.061	0.061	0.486	0.045	0.046
	$s.e.(\rho)$				0.048	0.007		0.062	0.014		0.050	0.003	
	$pcse(\rho)$				0.041	0.006							

Tables 3 and 4 give the results for a greater overall strength of diffusion, ρ=0.5, and w_{ij}=1/(N-1). With stronger diffusion, we expect the omitted-variable and simultaneity biases to be larger, and a severe positive omitted-variable bias does indeed manifest in all four non-spatial OLS estimates of for β_2. The bias is approximately +1.00 (or +100%!) for all four sample-dimensions. The estimates for β_1 and β_3 are also inflated, although the size of these biases shrinks as N grows. For N=5 and T=20, the biases are +0.092 and +0.076 respectively. These biases drop to +0.011 and +0.007 when N=40. Moreover, the mean reported standard errors once again underestimate the estimator's true sampling variability, for all three coefficients, especially when N and T are small. For N=5 and T=20, the mean reported standard errors underestimate the standard deviation of the coefficient estimates for β_1, β_2, and β_3 by 5%, 51%, and 16%. The PCSE estimate for β_2 cuts the degree of underestimation by more than half to 20%. When N is increased to 40, only the mean standard error for β_2 underestimates the observed sampling variability, but it does so by a large 66%. The PCSE underestimates by a much lesser but still unsatisfactory 22%. Once again, the problems tend to concentrate in the coefficients and standard-error estimates of the un-moderated common-shock effect. When N and T are 40, for example, non-spatial OLS does not underestimate the sampling variability for β_1 or β_3, but it does for β_2 (by 63%).[33]

[33] These results support Doreian et al.'s conclusion that non-spatial OLS produces inflated coefficient estimates and compressed standard errors.

Table 4. Comparing Estimators (N=40, $\rho = 0.5$, w_{ij} = (1/N-1), 1,000 trials)

		OLS			S-OLS			S-2SLS			ML (100 Trials)		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.011	0.038	0.04	0.999	0.036	0.036	0.999	0.036	0.036	1.001	0.042	0.041
	s.e.(β_1)	0.038	0.002		0.036	0.002		0.037	0.003		0.036	0.002	
	pcse(β_1)	0.038	0.003		0.036	0.003							
	β_2	2.004	0.11	1.01	0.861	0.165	0.216	1.008	0.27	0.27	1.054	0.141	0.151
	s.e.(β_2)	0.04	0.007		0.154	0.037		0.208	0.206		0.130	0.022	
T=20	*pcse*(β_2)	0.086	0.02		0.123	0.036							
	β_3	1.007	0.041	0.042	0.998	0.039	0.039	0.998	0.039	0.039	1.001	0.037	0.036
	s.e.(β_3)	0.04	0.007		0.038	0.007		0.038	0.007		0.038	0.006	
	pcse(β_3)	0.039	0.007		0.038	0.007							
	ρ				0.57	0.081	0.107	0.497	0.131	0.131	0.477	0.063	0.067
	s.e.(ρ)				0.074	0.018		0.102	0.097		0.025	0.001	
	pcse(ρ)				0.059	0.018							
	β_1	1.011	0.026	0.029	0.998	0.025	0.025	0.999	0.025	0.025			
	s.e.(β_1)	0.026	0.001		0.025	0.001		0.025	0.001				
	pcse(β_1)	0.026	0.001		0.025	0.001							
	β_2	2	0.073	1.002	0.844	0.1	0.185	1.002	0.135	0.135			
	s.e.(β_2)	0.027	0.003		0.101	0.016		0.127	0.032				
T=40	*pcse*(β_2)	0.061	0.009		0.084	0.015							
	β_3	1.009	0.029	0.031	0.997	0.027	0.027	0.999	0.027	0.027			
	s.e.(β_3)	0.027	0.003		0.026	0.003		0.026	0.003				
	pcse(β_3)	0.027	0.003		0.026	0.003							
	ρ				0.578	0.049	0.092	0.499	0.065	0.065			
	s.e.(ρ)				0.049	0.008		0.062	0.016				
	pcse(ρ)				0.041	0.007							

The average S-OLS estimates, while not perfect, are far better than the non-spatial OLS estimates. Again, biases in the S-OLS estimates of β_2 and ρ are negatively related. Intuitively, the endogenous spatial-lag 'steals explanatory power' from the common shocks variable, much like the tendency for temporal lags to 'steal explanatory power' from trended variables (Achen 2000). Also, as the general strength of diffusion increases, non-spatial OLS seems to perform increasingly poorly in terms of both the bias and the overconfidence of its estimates of non-spatial factors' effects, most especially regarding the size and standard errors of common-shock effects. S-OLS continues to offer considerable improvements in terms of reducing these biases as the general strength of diffusion increases, but its tendency to overestimate the strength of diffusion and underestimate the impact of common shocks also continues with the increased true diffusion-strength.

Not surprisingly, with strong diffusion, and the concomitant increase in the simultaneity bias, S-2SLS instrumental variable estimation (S2SLS-IV) becomes a more attractive alternative to S-OLS. This is particularly true as T increases.

The S-2SLS estimator is also relatively easy (relative to S-ML, that is) to implement. One simply uses the **W** matrix already constructed to generate the spatial lag of **y** to generate the same spatial lags of the **X** variables. These spatially lagged **X** then serve as instruments for the spatial lag of **y**. To elaborate, the endogeneity or simultaneity bias that plagues S-OLS arises because the spatial lag of **y** on the right-hand side of the model is endogenous to, i.e., simultaneous with, the dependent variable, **y**, on the left-hand side. Thus, a regressor, **Wy**, covaries with the true residual, **ε**, violating one of the classical linear regression model assumptions essential to the unbiasedness and consistency of OLS shown in the Gauss-Markov theorem. The easiest way to recognize this simultaneity intuitively is to note that, whereas units *j* affect unit *i*, which is why we place some weighted average of *j*'s outcomes on the right-hand side in the first place, unit *i* also affects (some) unit(s) *j*, and so the spatial lag **Wy** actually contains some part of *i*'s outcome itself. The standard instrumental-variables "solution" to such endogeneity is to find a (some) variable(s), **Z**, can that covaries (covary) with the endogenous regressor but does (do) not covary with the dependent variable (i.e., **ε**) except insofar as they relate to that regressor. Given such a **Z**, the instrumental-variable estimator, $\mathbf{b_{iv}}=(\mathbf{X'Z})^{-1}\mathbf{Z'Y}$, will be consistent and asymptotically efficient. The two-stage least-squares instrumental-variables (2SLS-IV) produces these properties by, first, regressing the set of **X**, including the endogenous regressor(s), on **Z** and the exogenous regressors, and, second, regressing Y on the fitted **X**'s from this first stage. If the instrument(s) **Z** are indeed perfectly exogenous, i.e., their covariance with **ε**, is *exactly* zero, then these IV estimators will enjoy these properties regardless of how strong the covariance of the instrument(s) with the endogenous regressor(s) for which it (they) instrument(s). If not, i.e, if the instruments are to any degree at all non-zero correlated with **ε**, then the instruments are only *quasi-instruments*, in Bartels (1991) terms, and the mean-squared-error costs or benefits of instrumentation will depend on the ratio of the covariance of the instrument(s) with the endogenous regressor(s) relative to the covariance of the instrument(s) with **ε**. In our experiments, to this point, the **X** variables, $\boldsymbol{\xi}$, $\boldsymbol{\eta}$, and $\boldsymbol{\xi\eta}$, are drawn *i.i.d.*, and in particular independently of the draws for **ε**, so our **WX** are perfect instruments by construction. More commonly in practice, however, we expect that researchers will confront right-hand-side **X**-variables that are endogenous to left-hand-side **y**-variables—i.e., the standard endogeneity concern that **y** causes **X** as well as **X** causes **y**. Or, in terms of cross-spatial endogeneity, the *y*'s in one unit partly determine and are partly determined by the *X*'s in other units. If so, then **WX** will offer imperfect, or *quasi*-, instruments at best (intuitively, because *j*'s **X** will also contain some of *i*'s **y**). In principle, researchers should be able to combine the common 2SLS-IV estimation strategy to address the endogeneity of **X** and **y** with the spatial 2SLS-IV estimation strategy suggested here to address such spatial simultaneity. Failing that (e.g., if even imperfectly valid instruments for the common endogeneity problem prove difficult to discover, as they usually do), we expect that the utility of the available **WX**'s as *quasi-instruments* will depend on the relative magnitudes of the intra-**ε** diffusion mechanisms, the intra-**X** diffusion mechanisms, call those magnitudes γ and ρ respectively, the causal mechanisms from **y** to **X**, call those magnitudes α, and the causal mechanisms **X** to **y**, call those magnitudes β. We advise researchers to explain why the **X** used in **WX** have good "Bartels Ratios", which in this case we believe translates to high $\rho\beta/\gamma\alpha$.[34]

When diffusion is strong, spatial maximum likelihood outperforms S-2SLS in RMSE terms, though it does not do better in terms of bias alone. For the most part, when diffusion is strong and N is small, S-ML gives good standard error estimates (Table 3). This is not true when N is large (Table 4). Specifically, S-ML underestimates the true variability of the sampling distribution for ρ by 60%.

[34] Note: the magnitudes cannot be estimated without a model whose identification conditions must assume them. I.e., as Bartels stressed, the magnitudes of parameters that determine the quality of *quasi-instruments* cannot be estimated; we can only offer theoretical arguments about their likely relative magnitudes.

Table 5. Comparing Estimators (N=40, $\rho = 0.5$, w_{ij} = (1/N-1)+U[-.1,+.1], 100 trials)

		OLS			S-OLS			S-2SLS		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.002	0.044	0.044	0.991	0.041	0.042	0.992	0.041	0.042
	$s.e.(\beta_1)$	0.042	0.002		0.04	0.002		0.04	0.002	
	$pcse(\beta_1)$	0.041	0.003		0.04	0.002				
	β_2	1.999	0.168	1.013	0.911	0.185	0.205	1.034	0.255	0.256
	$s.e.(\beta_2)$	0.043	0.007		0.171	0.04		0.229	0.097	
T=20	$pcse(\beta_2)$	0.085	0.021		0.132	0.041				
	β_3	1.003	0.053	0.052	0.995	0.049	0.049	0.995	0.049	0.049
	$s.e.(\beta_3)$	0.043	0.007		0.042	0.007		0.042	0.007	
	$pcse(\beta_3)$	0.042	0.008		0.041	0.007				
	ρ				0.541	0.102	0.109	0.48	0.134	0.135
	$s.e.(\rho)$				0.083	0.02		0.112	0.049	
	$pcse(\rho)$				0.064	0.021				
	β_1	1.016	0.026	0.03	1.002	0.026	0.026	1.004	0.026	0.026
	$s.e.(\beta_1)$	0.029	0.001		0.028	0.001		0.028	0.001	
	$pcse(\beta_1)$	0.029	0.001		0.028	0.001				
	β_2	1.977	0.144	0.987	0.874	0.119	0.173	1.006	0.157	0.156
	$s.e.(\beta_2)$	0.029	0.003		0.111	0.019		0.141	0.041	
T=40	$pcse(\beta2)$	0.061	0.012		0.088	0.018				
	β_3	1.011	0.032	0.033	0.999	0.031	0.031	1	0.031	0.031
	$s.e.(\beta_3)$	0.029	0.003		0.028	0.003		0.028	0.003	
	$pcse(\beta_3)$	0.029	0.003		0.028	0.003				
	ρ				0.556	0.068	0.088	0.489	0.087	0.088
	$s.e.(\rho)$				0.054	0.01		0.07	0.022	
	$pcse(\rho)$				0.043	0.01				

Table 5 reports the results for ρ=0.5 and w_{ij}=1/(N-1)+U[-0.1,+0.1].[35] Again, with this experiment, we are examining the consequences of random specification error in the spatial weighting matrix. Note that the true proportionate variation in the relative strength of cross-unit connections is quite sizable in this example. With N=5, 1/(N-1)=.25, so plus or minus .1 is plus or minus 40%. With N=40, 1/(N-1)≈.025, so plus or minus .1 is plus or minus roughly 400%. Still, given that the true spatial weights are randomly distributed about those used by the analyst in the estimation, we might expect little change in the bias properties of either non-spatial or spatial OLS while the sampling variability for both estimators should increase. Furthermore, since the spatial OLS estimator uses an imperfect (although unbiased) spatial weighting matrix, we might expect these estimates to offer a lesser improvement over non-spatial OLS than it did in the example of Table 4 where the estimator used exactly the right weighting matrix.

Some of these expectations are borne out; others are not. The sampling variability for all three estimators does increase with the introduction of random noise to the spatial weighting matrix. But, surprisingly, the bias in the spatial OLS estimates for β_2 and ρ actually decreases with the introduction of random noise to the spatial weighting matrix. Why does this happen? The most likely explanation is that the random draws add measurement error to the analyst's spatial lag and this leads to an attenuation bias that works in the opposite direction as the simultaneity bias. Another interesting result is that the performance of the S2SLS estimator relative to spatial OLS declines. For example, looking at Table 4, we see that when analysts use the true spatial weighting matrix for estimation and T=40, S2SLS has a better RMSE than spatial OLS (.065 vs. .092). In Table 5, however, this advantage is eliminated. The problem for S2SLS is that when random noise is added to the true weighting matrix it reduces the predictive power of the spatial instruments. In other words, it

[35] We do not have MLE results for the remaining experiments as yet.

creates weak instruments, which are well known to produce flawed estimates (e.g., Bartels 1991).

In Table 6, we make a similar adjustment with respect to the common shock variable η. The true common shock is generated by two independent random draws. However, we estimate our models using only one of the two components. Thus, we are measuring only half of the common shock. As before, this measurement error is random and once again, in the case of spatial OLS estimation, we see an attenuation bias. This time the attenuation bias reinforces the simultaneity bias and the overall bias in the spatial OLS estimator increases. Thus, as expected, inadequate modeling of exogenous external-stimuli mechanisms attenuates estimates of common-shock effects and inflates estimates of diffusion effects. This motivates our most-central advice regarding the primacy of effectively modeling the alternative domestic, exogenous-external, and spatial-interdependence causal mechanisms to distinguishing between them and evaluating their relative weight empirically.

Table 6. Comparing Estimators (N=40, $\rho = 0.5$, w_{ij} = (1/N-1), η = $\eta_1 + \eta_2$, 100 trials)

		OLS			S-OLS			S-2SLS		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.004	0.175	0.174	0.991	0.166	0.165	0.996	0.167	0.166
	$s.e.(\beta_1)$	0.065	0.009		0.044	0.003		0.047	0.006	
	$pcse(\beta_1)$	0.07	0.012		0.055	0.006				
	β_2	2.001	0.468	1.104	0.113	0.102	0.892	0.488	0.623	0.804
	$s.e.(\beta_2)$	0.099	0.023		0.092	0.014		0.265	0.229	
T=20	$pcse(\beta_2)$	0.472	0.127		0.079	0.024				
	β_3	1.023	0.249	0.249	1.017	0.244	0.243	1.021	0.243	0.242
	$s.e.(\beta_3)$	0.1	0.023		0.067	0.013		0.071	0.015	
	$pcse(\beta_3)$	0.103	0.027		0.081	0.017				
	ρ				0.95	0.034	0.451	0.759	0.295	0.391
	$s.e.(\rho)$				0.032	0.005		0.129	0.114	
	$pcse(\rho)$				0.027	0.009				
	β_1	1.007	0.136	0.136	0.982	0.126	0.127	0.987	0.129	0.129
	$s.e.(\beta_1)$	0.046	0.004		0.031	0.001		0.037	0.03	
	$pcse(\beta_1)$	0.049	0.005		0.035	0.002				
	β_2	2.028	0.358	1.088	0.114	0.082	0.89	0.844	1.457	1.458
	$s.e.(\beta_2)$	0.067	0.01		0.063	0.008		0.412	1.759	
T=40	$pcse(\beta2)$	0.321	0.055		0.057	0.014				
	β_3	1.031	0.194	0.196	1.012	0.19	0.189	1.017	0.193	0.193
	$s.e.(\beta_3)$	0.067	0.01		0.045	0.006		0.054	0.04	
	$pcse(\beta_3)$	0.069	0.012		0.05	0.007				
	ρ				0.946	0.023	0.446	0.575	0.861	0.86
	$s.e.(\rho)$				0.021	0.002		0.222	1.088	
	$pcse(\rho)$				0.02	0.004				

In Tables 7-10, we consider the consequences of cross-spatial endogeneity for OLS, S-OLS, and S-2SLS. To create this endogeneity, we simultaneously draw our X's and ε's from a multivariate normal distribution with correlation matrix C. We set the strength of the cross-spatial endogeneity equal to the overall strength of diffusion by giving the appropriate elements of C (c_{ij}) a value of ρw_{ij} (i.e., ρ/(N-1)). The weak diffusion and weak cross-spatial endogeneity results for our small and large N experiments are presented in Tables 7 and 8 respectively. These results show relatively little difference from the experiments with no cross-spatial endogeneity (compare with Tables 1 and 2). As one might expect, the RMSE's increase for each of the estimators, but the same basic conclusions hold. For analysts who want to model diffusion, spatial OLS is the preferred estimator.

However, when diffusion is strong we know the potential gains from using S2SLS begin to grow. With perfect instruments and a large T, S2SLS outperforms S-OLS in RMSE terms (Tables 3 and 4; T=40). This is *not* true when there is strong cross-spatial endogeneity. The performance of both S-OLS and S2SLS deteriorate under these circumstances, but the decline is so much greater for the latter estimator that the advantages of using it—including its relative unbiasedness and the accuracy of standard error estimates—completely disappear. When N=5 and T=40 the S2SLS RMSE for β_2 more than doubles and for ρ it quadruples. At this sample size, the S-OLS RMSE for β_2 and ρ increase by 79% and 111% respectively. With N=40 and T=40, cross-spatial endogeneity has similar consequences. Again, S2SLS shows no real advantage over S-OLS.

Table 7. Comparing Estimators (N=5, ρ = 0.1, w_{ij} = (1/N-1), c_{ij} = .025, 100 trials)

		OLS			S-OLS			S-2SLS		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.028	0.124	0.126	1.022	0.123	0.124	1.022	0.122	0.123
	$s.e.(\beta_1)$	0.106	0.012		0.106	0.012		0.107	0.012	
	$pcse(\beta_1)$	0.104	0.012		0.104	0.012				
	β_2	1.125	0.135	0.183	1.017	0.220	0.219	0.962	0.219	0.221
	$s.e.(\beta_2)$	0.108	0.021		0.172	0.039		0.205	0.069	
T=20	$pcse(\beta_2)$	0.111	0.027		0.156	0.041				
	β_3	0.994	0.110	0.109	0.994	0.111	0.111	0.994	0.110	0.109
	$s.e.(\beta_3)$	0.110	0.024		0.111	0.025		0.113	0.026	
	$pcse(\beta_3)$	0.108	0.025		0.109	0.025				
	ρ				0.100	0.133	0.132	0.145	0.162	0.167
	$s.e.(\rho)$				0.118	0.024		0.154	0.055	
	$pcse(\rho)$				0.108	0.028				
	β_1	1.040	0.074	0.084	1.034	0.073	0.081	1.033	0.074	0.081
	$s.e.(\beta_1)$	0.074	0.006		0.073	0.006		0.073	0.006	
	$pcse(\beta_1)$	0.072	0.006		0.072	0.006				
	β_2	1.109	0.089	0.14	0.957	0.119	0.126	0.938	0.123	0.137
	$s.e.(\beta_2)$	0.075	0.009		0.114	0.015		0.130	0.024	
T=40	$pcse(\beta_2)$	0.081	0.012		0.105	0.016				
	β_3	1.001	0.081	0.081	1.001	0.080	0.08	1.001	0.080	0.08
	$s.e.(\beta_3)$	0.076	0.011		0.076	0.010		0.076	0.011	
	$pcse(\beta_3)$	0.075	0.010		0.074	0.011				
	ρ				0.138	0.077	0.086	0.155	0.083	0.099
	$s.e.(\rho)$				0.078	0.010		0.096	0.019	
	$pcse(\rho)$				0.072	0.011				

Table 8. Comparing Estimators (N=40, $\rho = 0.1$, w_{ij} = (1/N-1), c_{ij} = .003, 100 trials)

		OLS			S-OLS			S-2SLS		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.011	0.037	0.032	1.010	0.037	0.038	1.010	0.037	0.038
	$s.e.(\beta_1)$	0.037	0.002		0.037	0.002		0.037	0.002	
	$pcse(\beta_1)$	0.036	0.002		0.037	0.002				
	β_2	1.112	0.041	0.118	0.979	0.176	0.176	0.921	0.150	0.169
	$s.e.(\beta_2)$	0.038	0.007		0.142	0.030		0.177	0.059	
T=20	$pcse(\beta_2)$	0.041	0.010		0.129	0.038				
	β_3	1.001	0.047	0.045	1.001	0.047	0.046	1.001	0.046	0.046
	$s.e.(\beta_3)$	0.038	0.007		0.038	0.007		0.038	0.007	
	$pcse(\beta_3)$	0.038	0.007		0.038	0.007				
	ρ				0.118	0.156	0.156	0.170	0.130	0.147
	$s.e.(\rho)$				0.123	0.026		0.155	0.053	
	$pcse(\rho)$				0.112	0.033				
	β_1	1.003	0.027	0.027	1.002	0.027	0.027	1.002	0.027	0.027
	$s.e.(\beta_1)$	0.025	0.001		0.025	0.001		0.025	0.001	
	$pcse(\beta_1)$	0.025	0.001		0.025	0.001				
	β_2	1.112	0.033	0.117	0.967	0.118	0.122	0.947	0.117	0.128
	$s.e.(\beta_2)$	0.026	0.003		0.097	0.014		0.118	0.025	
T=40	$pcse(\beta_2)$	0.028	0.004		0.089	0.018				
	β_3	1.000	0.026	0.026	1.000	0.025	0.025	1.000	0.025	0.025
	$s.e.(\beta_3)$	0.026	0.003		0.026	0.003		0.026	0.003	
	$pcse(\beta_3)$	0.026	0.003		0.026	0.003				
	ρ				0.131	0.100	0.104	0.149	0.099	0.111
	$s.e.(\rho)$				0.084	0.012		0.103	0.022	
	$pcse(\rho)$				0.077	0.015				

Table 9. Comparing Estimators (N=5, ρ = 0.5, w_{ij} = (1/N-1), c_{ij} = .125, 100 trials)

		OLS			S-OLS			S-2SLS		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.314	0.167	0.355	1.045	0.113	0.121	1.038	0.115	0.121
	$s.e.(\beta_1)$	0.151	0.017		0.105	0.012		0.106	0.012	
	$pcse(\beta_1)$	0.143	0.023		0.107	0.013				
	β_2	1.971	0.312	1.019	0.724	0.136	0.307	0.690	0.138	0.339
	$s.e.(\beta_2)$	0.156	0.033		0.161	0.036		0.173	0.047	
T=20	$pcse(\beta_2)$	0.260	0.060		0.105	0.025				
	β_3	1.061	0.160	0.17	0.965	0.128	0.132	0.963	0.129	0.133
	$s.e.(\beta_3)$	0.164	0.040		0.111	0.025		0.111	0.025	
	$pcse(\beta_3)$	0.152	0.041		0.111	0.024				
	ρ				0.634	0.047	0.142	0.652	0.054	0.161
	$s.e.(\rho)$				0.061	0.013		0.068	0.018	
	$pcse(\rho)$				0.042	0.009				
	β_1	1.331	0.111	0.349	1.063	0.075	0.098	1.060	0.076	0.097
	$s.e.(\beta_1)$	0.105	0.007		0.072	0.005		0.072	0.005	
	$pcse(\beta_1)$	0.102	0.009		0.074	0.006				
	β_2	2.061	0.199	1.079	0.756	0.092	0.261	0.737	0.090	0.277
	$s.e.(\beta_2)$	0.108	0.011		0.111	0.016		0.117	0.019	
T=40	$pcse(\beta_2)$	0.184	0.021		0.076	0.011				
	β_3	1.103	0.146	0.179	0.982	0.081	0.082	0.981	0.081	0.083
	$s.e.(\beta_3)$	0.109	0.013		0.073	0.010		0.073	0.010	
	$pcse(\beta_3)$	0.106	0.016		0.073	0.010				
	ρ				0.624	0.035	0.129	0.633	0.036	0.138
	$s.e.(\rho)$				0.040	0.006		0.044	0.008	
	$pcse(\rho)$				0.029	0.004				

Table 10. Comparing Estimators (N=40, ρ = 0.5, w_{ij} = (1/N-1), c_{ij} = .013, 100 trials)

		OLS			S-OLS			S-2SLS		
		Mean	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*	*Mean*	*Std. Dev.*	*RMSE*
	β_1	1.030	0.045	0.054	1.004	0.038	0.038	1.004	0.038	0.038
	$s.e.(\beta_1)$	0.039	0.002		0.037	0.002		0.037	0.002	
	$pcse(\beta_1)$	0.038	0.002		0.036	0.002				
	β_2	2.000	0.116	1.007	0.773	0.114	0.254	0.748	0.117	0.278
	$s.e.(\beta_2)$	0.040	0.007		0.140	0.033		0.160	0.050	
T=20	$pcse(\beta_2)$	0.096	0.024		0.091	0.026				
	β_3	1.002	0.043	0.042	0.994	0.039	0.039	0.994	0.039	0.039
	$s.e.(\beta_3)$	0.041	0.007		0.038	0.006		0.038	0.006	
	$pcse(\beta_3)$	0.040	0.007		0.038	0.007				
	ρ				0.614	0.055	0.127	0.627	0.057	0.138
	$s.e.(\rho)$				0.067	0.015		0.077	0.023	
	$pcse(\rho)$				0.044	0.012				
	β_1	1.036	0.025	0.044	1.007	0.022	0.023	1.007	0.022	0.023
	$s.e.(\beta_1)$	0.027	0.001		0.025	0.001		0.026	0.001	
	$pcse(\beta_1)$	0.027	0.001		0.025	0.001				
	β_2	2.011	0.075	1.014	0.757	0.078	0.255	0.766	0.080	0.247
	$s.e.(\beta_2)$	0.028	0.003		0.088	0.013		0.098	0.019	
T=40	$pcse(\beta_2)$	0.071	0.011		0.060	0.010				
	β_3	1.003	0.031	0.031	0.990	0.026	0.028	0.990	0.026	0.028
	$s.e.(\beta_3)$	0.028	0.003		0.026	0.003		0.026	0.003	
	$pcse(\beta_3)$	0.028	0.003		0.026	0.003				
	ρ				0.623	0.038	0.129	0.619	0.038	0.125
	$s.e.(\rho)$				0.042	0.006		0.047	0.009	
	$pcse(\rho)$				0.029	0.005				

VI. Conclusion

Our experimental results support and extend existing studies that evaluate estimators for spatial-lag models. We found (a) that non-spatial OLS, which ignores diffusion processes, performs poorly, producing biased, inefficient, and over-confident estimates (although PCSE reduces this last problem somewhat), especially of common-shock effects and especially when the degree of spatial diffusion is high, (b) that S-OLS reduces these biases of non-spatial OLS dramatically, especially insofar as both the diffusion mechanisms and the domestic and exogenous-external effects are well-specified, but also tends to under-estimate standard errors (and PCSE may not help) and to inflate the spatial lag's coefficient at the expense of other explanatory variables, especially of common shocks and especially when the degree of diffusion is high, (c) that S-2SLS—*under the ideal conditions for its instrumentation assumptions: large samples and perfectly exogenous instruments*—improves further upon S-OLS, producing unbiased estimates of both common shocks and diffusion, with reasonable efficiency and accurate estimates of uncertainty. However, in smaller samples, and/or with imperfect instruments—which is to say: in practice—a familiar tradeoff between biased-but-efficient S-OLS and consistent- (or, at least, less-biased-) but-inefficient S-2SLS obtains. Imperfect exogeneity of instruments and, as our experiments demonstrate, smaller samples and modest diffusion strengths—conditions that we expect are common—tend to favor the simpler S-OLS estimator in mean-squared-error comparisons and at least to improve its bias comparisons. (d) S-ML, meanwhile, estimates non-spatial (domestic and common-shock) effects well in both bias and efficiency terms, and generally performs better or as well as the alternatives in RMSE terms in estimating ρ also. However, (1) the RMSE advantages of S-ML seem negligible in small-N, small-ρ samples; (2) S-ML generally underestimates ρ, considerably so at smaller ρ; and (3) it seems to report reasonably accurate standard errors for its estimates of ρ *only* when N is small and ρ is large and to report (sometimes wildly) overconfident standard errors in the other contexts explored.

Excepting these less uniformly glowing results for S-ML, our findings generally resonate with those of previous studies where they overlap. Our use of a non-diffusion baseline model that explicitly reflects a modern, context-conditional, open-economy, comparative-political-economy approach, and the finding there that the problems of S-OLS and, much more so, non-spatial OLS concentrate precisely in this area of distinguishing common-shocks from diffusion is one substantively important way in which our results extend previous ones. Another is that previous work studied cross-sections of data almost exclusively, whereas our simulations evaluated non-spatial OLS, S-OLS, S-2SLS, S-ML using panels of data. Varying the sizes of T, of N, and N/T, across our experiments, we found, e.g., that the S-OLS standard errors improve as T increases. We believe that our analytic results regarding the inversely related biases in estimating these two sorts of effects are also new.

These results have important implications for the study of politics and society in a global age, and perhaps especially in comparative and international politics and political economy. Analysts who ignore diffusion processes when they are present will tend to exaggerate the importance of domestic variables and external shocks (and their interaction), privileging context-conditional open-economy kinds of political-economic explanations. Conversely, analysts who model international diffusion without adequately specifying domestic, common-shock, and/or common-shock-varying-response alternatives will obviously suffer the opposite specification-error biases, and privilege their diffusion arguments. Empirically distinguishing and evaluating the relative strength of these important theoretical and substantive mechanisms can be extremely difficult and rests, in the first instance, on the accurate specification of the alternatives. Accordingly, the first-order consideration for scholars interested in evaluating empirical results regarding the relative strength of international-diffusion, common-shock, and domestic-factor effects[36] must be the absolute and relative precision with which these alternative causal mechanisms are specified in the empirical model. An obvious point, perhaps, but well worth emphasizing here: credit accrues to estimates of each type of effect in proportion to how well its causal mechanism is specified, both on its own terms and, perhaps less obviously, in terms relative to alternatives. The logic is essentially that of omitted-variables: insofar as the (relatively) mis-specified causal mechanisms are important *and* (i.e., literally, *multiplied by*) insofar as they correlate with the included mechanisms, the analysis will misleadingly favor the (better) specified causal mechanism.

To clarify the importance of modeling interdependence, consider three sorts of empirical observations that researchers sometimes conclude imply diffusion: significant regional or other group dummies, spatially correlated observations or a spatial or clustering pattern in the variance-covariance of residuals, or an S-shaped accumulation of the share of a sample or population that adopts some action.

Convenient expedients like regional, cultural, organizational, linguistic group-membership dummies illustrate well the difficulties of distinguishing these alternatives. If EU membership, e.g., correlates, controlling for the model's other cofactors, with, say, foreign-aid, is this because values-based and/or utilitarian arguments for aid circulate among EU-member elites to diffuse among its members, or because EU institutions provide forums for policymaking elites in member countries to learn from each other the ethical or practical benefits of aid, or because EU membership lowers the cost of responding to some exogenous external shock like a famine, or because some domestic conditions that favor joining the EU also favor foreign aid? These are two different diffusion arguments, one common-shock-varying-response argument, and one domestic-factors argument, and

[36] The abstract generalization of these three types of effects would be *spatial-interdependence*, *exogenous-external*, and *locally-independent* effects.

a dummy for EU membership is incapable of distinguishing between them on its own. However, insofar as the researcher can model these alternative mechanisms more precisely than by the membership indicator, she should do so (of course). Also or alternatively, insofar as she can connect the time-invariant conditional-mean-shifting nature of the dummy's effect to one mechanism, substantively, theoretically, and empirically distinctly from specifications of alternative effect types, then the reader may attribute the coefficient on the dummy to this linked mechanism.

Likewise, approaches to modeling spatial interdependence that emphasize estimates of the variance-covariance structure of residuals are inherently limited and potentially misleading as means of distinguishing diffusion from alternative explanations for such correlation. In the first place, a finding that observations or residuals in some set of countries correlate or cluster in some manner can as easily support a common-exposure as a diffusion argument for the same reasons a dummy alone cannot distinguish these alternatives. In the second place, residual variance-covariance approaches tend to require strong multivariate normality and separability assumptions for tractability and, relegating all spatial correlation to a variance-covariance matrix, logically cannot reflect any asymmetry in the pattern of diffusion that may exist. If we estimate the variance-covariance or clustering of residuals and interpret that as diffusion, then we must conclude *a priori* that, in a model of tax competition estimated in a sample of OECD countries for example, taxes in Luxembourg affects the US as much as the US affects Luxembourg. In the third place, finally, insofar as the diffusion *cum* spatial interdependence lies at the center of the theoretical or substantive issues under exploration, their relegation to the stochastic properties of residuals rather obfuscates the point.

Finally, some refer to an S-shaped accumulation in a plot of the share of a population that has adopted some particular action over time as the characteristic pattern of diffusion: first one unit adopts some practice, then the idea or knowledge of its success diffuses to a few others, from which it diffuses more densely to many others, until enough of the population has adopted the course of action that most of the diffusion paths lead to units that have already adopted and the pace of adoption tapers toward everyone having adopted. That's certainly one story consistent with a sigmoidal accumulation of adopters, but many, many cumulative functions are sigmoidal or roughly sigmoidal. One very important one is the cumulative normal, for example. Suppose, then, that each unit would have to pay some cost (say, to overcome some domestic political opposition) to adopt some course of action. The central limit theorem gives every reason, absent some other theory, to suppose these costs are normally distributed across units. Now suppose globalization or some other exogenous trend that dampens the opposition to or enhances the benefit from adoption of this course of action is commonly experienced by the sample units. Then, cumulative share having adopted would follow the cumulative normal, which is S-shaped. Again, the ability to distinguish these alternative explanations for the adoption path relies in the first order on modeling both accurately, and observational or relatively non-parametric expedients like this one or the preceding two are wholly inadequate to that task.

As we have also shown, on the other hand, (perhaps second-order) simultaneity problems inhere in directly modeling spatial interdependence. Assuming positively-reinforcing diffusion effects, these simultaneity biases induce overestimation of diffusion, at the expense of the common shocks especially. Moreover, these simultaneity biases remain even if the diffusion and common-shocks processes are modeled perfectly and estimated by S-OLS, and they are accompanied by over-confident standard-error estimates (which PCSE may not redress). However, these biases are not necessarily large, tending smaller insofar as the strength of diffusion remains modest[37] and the

[37] This may actually suggest, interestingly, that one might have more confidence in an S-OLS finding of a small but significantly estimated diffusion effect than of a larger one or, at least, of an equally significant larger one.

dependent variable is less stochastic (i.e., better explicated by exogenous explanitors; e.g., excluding the spatial lag for one). Furthermore, instrumentation alternatives to simple spatial-lag estimators, although they do tend to eliminate the bias insofar as their exogeneity assumptions hold, do not necessarily perform better in mean-squared-error comparisons, which combine bias and efficiency considerations. Thus, empirical researchers face a familiar tradeoff between more-efficient-but-biased simple estimators and less-efficient-but-unbiased (or less-biased; strictly: consistent) instrumental-variables estimators. Smaller samples, lower diffusion strength, and imperfect exogeneity of the potential instruments favor the simpler spatial-lag models in this comparison. S-ML performs well in MSE terms, but its weakest comparison with the alternatives lies precisely in the area of estimating diffusion strength (ρ) and providing accurate estimates of the sampling variability of those estimates, and in particular in distinguishing those estimates from common-shock estimates. Therefore, no single estimator has yet emerged as optimal under all, or even most, plausible parameter and sample conditions, and simple S-OLS type approaches can perform as or nearly well as more-sophisticated estimators under a fairly wide range of reasonable sample-sizes and parameter levels. That simple spatial-lag models can, under some not-uncommon conditions, perform so well in root-mean-squared-error terms relative to more-complicated instrumentation or ML alternatives, and, indeed, at least reasonably adequately in absolute bias terms (its least-favorable metric) is, as we have noted, a very comforting consideration to scholars of diffusion, especially for those estimating models other than linear-regression given the complexity of instrumental-variable estimation for qualitative and limited dependent-variables. A reader's primary considerations in this regard when evaluating these attempts to estimate diffusion will be (1) the power and precision with which the alternative causal mechanisms are specified, which is similar to our "first-order" advice, but here is because greater explanatory power for the exogenous factors of the model tends to mitigate the simultaneity bias and, (2) the strength of overall diffusion and, in particular, if these are different, the magnitude of the diffusion from the units represented in observations on the dependent variable (their share of those observations) to units represented in observations on the spatial lag(s) (as weighted) because the simultaneity biases are smaller while such diffusion remains modest.

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