Spatial Econometric Modeling, with Application to Employment Spillovers and Active-Labor-Market Policies in the European Union

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ABSTRACT: European integration implies strategic (and non-strategic) interdependence in domestic policymaking. We evaluate four empirical estimators—non-spatial OLS, spatial OLS, spatial 2SLS-IV, and spatial ML—applied to such policies in a generic comparative and international political economy context with domestic factors, exogenous external factors, and interdependence. Non-spatial OLS, not surprisingly, suffers from potentially severe omitted-variable bias, tending to inflate estimates of exogenous-external effects especially. Spatial OLS, which directly specifies the interdependence via spatial lags of the dependent variable, dramatically improves estimates but also suffers some simultaneity bias, appreciable under strong interdependence. Spatial 2SLS-IV, which instruments for dependent-variable's spatial-lags with independent-variables' spatial-lags, yields unbiased and efficient estimates, *when its conditions hold*: large samples and fully exogenous instruments. Practical tradeoffs thus arise between biased-but-efficient spatial OLS and consistent- (or less-biased-) but-inefficient (in practical samples) 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, especially when it is modest. We also find sizable inaccuracies in the standard-error estimates of each estimator under differing conditions, and PCSE 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 its interdependence-strength estimates to suffer simultaneity biases only if *cross-spatial endogeneity* exists: i.e., dependent variables in some units cause explanatory variables in others. We conclude with estimation of active-labor-market policy-spending under European integration, which, given employment spillovers, suffer a free-rider problem and so should be under-provided by domestic policymakers in proportion to how much its neighbors/partners spend on such policies. The econometric results reveal surprisingly strong negative feedbacks of this sort in domestic active-labor-market policymaking, highlighting the importance of modeling directly the spatial interdependence implied by European integration, and suggesting a strong case for supranational (EU) policymaking in this area.

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I. INTRODUCTION:

This paper studies European integration and domestic policymaking, emphasizing the implied strategic (and non-strategic) interdependence among European countries' domestic policies and the resultant spatial interdependence of policy data. In fact, spatial interdependence is substantively central to the very concept of integration in general and is perhaps no small part of the goal of European-Union integration specifically; theoretically and empirical-methodologically, however, it raises several interesting challenges.

Theoretically and substantively, we consider how (un)employment spillovers across border regions due to the strong interconnectedness of regional labor markets (Overman and Puga 2002) yield incentives for each country to free-ride on its neighbors'/partners' employment-enhancing policies or, equivalently but perhaps more intuitively, how employment-enhancing policies entail cross-border positive externalities that induce domestic under-investment in them. This suggests that as neighbor/partner-countries' employment-enhancing public policies increase, domestic polities would tend to reduce their own, *ceteris paribus*. We find strong evidence for a surprisingly large degree of such negative feedback in national active-labor-market-policy spending *per* unemployed worker, suggesting a strong case for supranational (EU) policymaking in this area.

Empirical-methodologically, we evaluate four strategies, two simple and two sophisticated, for modeling empirically such interdependence (also proposing en route an alternative of intermediate sophistication). While recognizing that time-series-cross-section data typically correlate across time and space, empirical scholars have tended to model temporal dependence directly while addressing spatial interdependence solely as nuisance to be "corrected" (FGLS) or to which merely to render standard-error estimation "robust" (PCSE). As we demonstrate, however, directly modeling spatial interdependence is methodologically superior, offering efficiency gains and generally helping avoid biased estimates even of "non-spatial" effects.

We begin this econometric demonstration and consideration by specifying generic empirical models representing modern approaches to studying domestic policymaking in the context of rising integration: *(context-conditional) open-economy comparative political-economy* (i.e., correlated exogenous, external shocks, varying domestic responses) and *(interdependent) international political-economy*, plus *closed-economy* and *orthogonal open-economy* predecessors. Then we evaluate econometrically four estimators—non-spatial OLS, spatial OLS, spatial 2SLS-IV, spatial ML[1]—for analyzing such models in spatially interdependent data generated by the most general version of these generic models, which would seem to be implied by European integration, for example.

In this context, we find analytic and simulation (Monte Carlo) results indicating that:

(1) Non-spatial OLS suffers from potentially severe omitted-variable bias, tending to inflate estimates of exogenous-external effects especially.

(2) Spatial OLS, which specifies interdependence directly via spatial lags, dramatically improves estimates but suffers a simultaneity bias, which can be appreciable under strong interdependence.

(3) 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 exogenous-external and interdependence effects, *when its conditions hold: large samples and fully exogenous instruments.* It exhibits no such advantages when its conditions do not hold. A tradeoff thus arises in practice between biased-but-efficient spatial OLS and consistent- (or less-biased-) but-inefficient spatial 2SLS-IV.

[1] OLS≡Ordinary Least Squares; 2SLS-IV≡instrumental-variables by two-stage-least-squares; ML≡maximum likelihood.

(4) 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 interdependence-strength is modest.
(5) Furthermore, the standard-error estimates from all four procedures yield sizable inaccuracies under differing conditions, which PCSE's do not necessarily reduce. By an accuracy-of-reported-standard-errors criterion, 2SLS-IV seems to dominate. S-ML reports especially suspect standard errors.
(6) Mis-specification in either the pattern of interdependence or in the exogenous-external conditions harms all the estimators' performances, but notably worsens S-2SLS-IV performance more than S-OLS.
(7) Finally, we also explore the spatial 2SLS-IV estimator under varying patterns of interdependence and endogeneity, finding that its estimates of interdependence strength suffer simultaneity biases only when a condition we call cross-spatial endogeneity, wherein dependent variables in some units cause explanatory variables in others, prevails.

All of these spatial-lag econometric models require that the researcher pre-specify a *connectivity matrix*, **W**, whose elements, w_{ij}, reflect the effect of country *j*'s outcome on country *i*'s outcome. The econometric exercise is to estimate a coefficient on the spatial lag of the dependent variable, **Wy**, which gives the overall strength of interdependence following this spatial pattern. In geography, where spatial econometrics first emerged and developed, these w_{ij} necessarily reflect geographic notions of contiguity or proximity; in many other substantive applications, and in most political-economy ones, the relevant notion of connectivity would be something other than simple geometric distance: e.g., trade, financial, currency, or other international-economic connections, ethno-linguistic-cultural proximity, resource or product complementarity or substitutability, common international-treaty or -organization membership, government-partisanship proximity, etc. (See, e.g., Simmons and Elkins XXXX; Beck and Gleditsch, forthcoming.) In such cases, determination of **W** becomes more central, debatable, and consequential. Ideally for the researcher theoretically and substantively interested in interdependence, **W** would derive from the theoretical expected connectivities between nations, and the empirical testing of this pattern against alternatives, as well as of interdependence explanations against correlated exogenous-external or domestic-factor alternatives, becomes paramount. As we show, however, even for the researcher not centrally interested in spatial interdependence, obtaining good estimates even of non-spatial effects will require some attention to empirical modeling of such connectivity if it exists. For these researchers and for those substantively interested but seeking an inductive empirical alternative to theoretical pre-specification of **W**, we propose a strategy of pre-estimating partial correlations from supplementary data related to the theoretically expected connectivity. We conclude with an empirical application of this technique to domestic policymaking under European integration, specifically to active-labor-market (ALM) policy (spending), which highlights the importance of modeling interdependence directly using methodologies that recognize and address the implied simultaneity. In this context, the theoretically expected externality that induces the national under-investment and thereby spatial interdependence in ALM among EU member-countries is regional integration of labor markets across borders (Overman and Puga 2002). Accordingly, our inductive approach pre-estimates partial correlations of EU member-country unemployment outcomes, using those partial correlations as the w_{ij} in subsequent estimation of the spatial-lag ALM-spending model.

II. Approaches to Comparative and International Political Economy:[2]

We begin by identifying three approaches to comparative and international political economy (C&IPE) that motivate distinct empirical models. In closed-economy comparative political-economy (CPE), the focus is on domestic variables and external shocks and international interdependence

[2] One could substitute comparative and international politics for comparative and international political economy without any loss of applicability in all of the following discussion. The issues discussed are perhaps more homogenous and clearer in the political-economy subfields, though, so we conduct the discussion in those terms.

processes are ignored.[3] In open-economy CPE, by contrast, the importance of *exogenous* external conditions/shocks (e.g., oil prices) for the domestic political-economy is recognized; but the domestic policy or outcome responses to these foreign shocks, which responses may be either moderated by domestic variables (context-conditional open-economy CPE) or unconditioned by domestic variables (orthogonal open-economy CPE), are treated as isolated phenomena. That is, in open-economy CPE, exogenous 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 political economy (IPE) focuses explicitly on spatial linkages and mechanisms of interdependence in the global political economy whereby policies and outcomes in some units directly affect the policies and outcomes of other units, perhaps in addition to the possibility that multiple units are exposed to common (or correlated) exogenous-external shocks. A country might respond to an exogenous domestic or global political or economic shock by lowering its capital tax-rate, for example, but the magnitude of its response may depend on how its competitors respond and, conversely, its own response may affect the capital tax-rates that policymakers in other countries choose. If these responses are competitive, the initiating country will likely lower its capital taxes by more than it would have in the absence of tax competition.

In this paper, we focus on the models of context-conditional open-economy CPE and IPE and methods for estimating such models. We do not consider purely domestic models except to note that,

[3] Following recent practice in political science, we refer to processes by which the outcomes in some units directly affect the outcomes in other units as *interdependence*. We distinguish such interdependence processes, which will induce *spatial correlation*, from *spatially correlated responses (outcomes) to spatially correlated exogenous shocks*, or *common shocks* for short, which will also induce *spatial correlation*. For us, synonyms for *interdependence* 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 *interdependence* 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.

if external influences are important, these models will, even in the best of circumstances, produce inefficient estimates of the coefficients for domestic variables and, in the worst, biased and inconsistent estimates. The central problem we consider here is the difficulty distinguishing *common shocks* from *international interdependence*. On the one hand, ignoring interdependence processes when they are present will lead analysts to exaggerate the importance of external shocks. On the other hand, if certain endogeneity problems discussed below are insufficiently addressed, modeling interdependence with spatial lags can lead analysts to overestimate the importance of interdependence at the expense of common shocks, especially insofar as such common shocks are inadequately modeled. A spatial two-stage-least-squares estimator seems to provide an effective resolution to this dilemma, at least under the circumstances so far considered: namely, that domestic explanatory variables are not themselves endogenous to dependent variables and are spatially correlated but are not themselves subject to a interdependence process (*i.e.*, they correlate by the *common shocks* mechanism).

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

A. Closed-Economy Comparative-Political-Economy:

In 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:

$$\mathbf{y}_{it} = \xi_{it}\boldsymbol{\beta} + \boldsymbol{\varepsilon}_{it} \tag{1}$$

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,[4] 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, merely 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, comparative political-economists began to consider controlling for the effects of global political and economic conditions on domestic policies and outcomes to be more important. At first, however,

[4] 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 2004, e.g.).

such global conditions were assumed to hit all domestic units equally and to induce equal responses from each domestic unit to that impact. This implies theoretical/empirical models of the following form:

$$\mathbf{y}_{it} = \xi_{it}\boldsymbol{\beta}_0 + \eta_t\boldsymbol{\beta}_1 + \boldsymbol{\varepsilon}_{it} \qquad \textbf{(2)}$$

where the η_t are the exogenous 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, merely to require a standard-error adjustment (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 began to appear: e.g., Alvarez, Garrett, and Lange (1991) on partisanship and corporatism interactions; Alesina, Roubini, and Cohen (1997) on political and/or partisan cycles; Powell and Whitten (1993) on economic voting.

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

Modern approaches to CPE continue to recognize the potentially large effects of global shocks and other conditions abroad on the domestic political economy, but tend now to emphasize also how domestic institutions, structure, and conditions shape the degree and nature of domestic exposure to such shocks/conditions and moderate the domestic policy and outcome responses to these differently felt foreign stimuli. This produces characteristic theoretical and empirical models of the following sort:

$$\mathbf{y}_{it} = \xi_{it}\boldsymbol{\beta}_0 + \eta_t\boldsymbol{\beta}_1 + (\xi_{it}\cdot\eta_t)\boldsymbol{\beta}_3 + \boldsymbol{\varepsilon}_{it} \qquad \textbf{(3)}$$

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 CPE, including all of the contributions to the recent *International Organization* special issue (Bernhard, Broz, and Clark 2002) on the choice of exchange-rate regimes (an international, or, at least, foreign-policy, institution) and other monetary institutions. Once more, any spatial correlation distinct from that induced by common or correlated responses to globally *common shocks* would be left to FGLS or PCSE "corrections".[5]

D. (Comparative &) International Political Economy:

International political economy is distinct from comparative political economy, if distinct at all, in the emphasis the former places on international *relations* within political economy. That is, whereas open-economy comparative political-economy recognizes a role for international conditions, these global conditions remain exogenous to domestic policies and outcomes. International factors may be part of CPE explanations, but they are not typically central to the analysis. IPE analyses, contrarily, directly stress the international relations, i.e. the interdependence of policies and outcomes across nations, implying that, in general, policies in outcomes in countries *i* and *j* affect each other:

$$\mathbf{y}_{it} = \rho \sum_{j \neq i} w_{ij} \mathbf{y}_{j,t} + \xi_{it} \boldsymbol{\beta}_0 + \eta_t \boldsymbol{\beta}_1 + (\xi_{it} \cdot \eta_t) \boldsymbol{\beta}_3 + \boldsymbol{\varepsilon}_{it} \qquad \textbf{(4)}$$

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 the outcome in *i*. The rest of the right-hand-side model reflects the domestic political economy and,

[5] Such arguments that varying domestic institutions and structure condition the response of policies and outcomes to globally common shocks are also central to Franzese (2002). However, the estimated empirical models in that book are of type **(4)**, even though the resulting IPE and interdependence aspects receive little emphasis.

in the literature, has been as simple as **(1)** or as complex as **(3)**. Examples of these sorts of models include the recent work of Simmons and Elkins (2004) on the global interdependence of liberalization policies and reforms.[6] 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.

III. ESTIMATING C&IPE MODELS WITH INTERDEPENDENCE:

Obviously, the empirical example to be considered here, that of positive-externality-induced negative interdependence of ALM policies across EU member-countries, is also a model of type **(4)**. Surely, domestic political-economic factors, notably government partisanship and labor organization, will contribute strongly to an explanation of variation in ALM policies. Likely, exogenous-external conditions, like technology- and trade-induced labor-market structural trends, and possibly also their interactions with domestic factors, will contribute too. The central substantive interest in this case, though, is the potential interdependence: namely, underinvestment by national policymakers in these ALM policies given the positive-externality in their effects.

More generally, given that models of sort **(4)** subsume those of sorts **(1)**-**(3)**, one might argue that scholars should perhaps always begin with **(4)** and reduce downward as their data suggest/allow. Econometrically, however, as we demonstrate below, obtaining "good" (unbiased, consistent, efficient) estimates of coefficients and standard errors in such C&IPE models and, in particular, distinguishing open-economy CPE processes from IPE (interdependence) processes are not straightforward tasks. The first and foremost considerations are the relative and absolute theoretical and empirical precisions of the alternative open-economy CPE and IPE-interdependence parts of the model, i.e., the interdependence parts and the common, correlated, or domestic-context-conditional

[6] Franzese (2002) also estimates simple, unweighted (*i.e.*, constant w_{ij}) versions of models of type **(4)**, but the discussion there emphasizes the context-conditional CPE aspects of such models.

responses to common, correlated, or domestic-context-conditioned exogenous-external factors (henceforth: *domestic* and *exogenous-external*) parts. To elaborate: the relative and absolute accuracy and power with which the spatial-lag weights, w_{ij}, reflect and gain leverage upon the actual interdependence mechanisms operating and with which the domestic and exogenous-external parts of the model reflect and gain leverage upon the alternatives critically affect the attempt to distinguish and evaluate their relative strength empirically because the two mechanisms produce similar effects so that inadequacies or omissions in the specification of the one tend, quite intuitively, to induce overestimation of the importance of the other. Secondarily, even if the domestic, exogenous-external, and interdependence mechanisms are modeled perfectly, the spatial lags in this model will be endogenous (i.e., they will covary with the residuals), so estimates of ρ will suffer simultaneity biases. Moreover, as with the primary omitted-variable or (relative) misspecification biases, these secondary simultaneity biases in estimating the strength of interdependence induce biases in the opposite direction in estimating the strength of open-economy CPE mechanisms.

A. Spatial-Lagged-Dependent-Variable Models:

One way to write models to test hypotheses about and estimate the strength of international interdependence is the spatial-lag model (*spatial-lagged-dependent-variable, S-LDV*), written formally as

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \qquad \textbf{(5)}$$

where $\mathbf{y}$ is a $NTx1$ vector of observations on the dependent variable stacked by unit; ρ is the spatial autoregressive coefficient; $\mathbf{W}$ is an $NTxNT$ block diagonal spatial weighting matrix, $\mathbf{Wy}$ is the spatial lag, $\mathbf{X}$ is a stacked $NTxK$ matrix of values on K independent variables, $\boldsymbol{\beta}$ is a $Kx1$ vector of coefficients on those $\mathbf{X}$, and $\boldsymbol{\varepsilon}$ is a $NTx1$ vector of disturbances. Each TxT block along $\mathbf{W}$'s diagonal is a matrix of zeros (unless $\mathbf{y}$ also correlates temporally, in which case only the prime diagonal is zero and the off-diagonals of these *TxT* blocks on the diagonal are non-zero and reflect these temporal correlations); each of the off-diagonal *TxT* blocks has zero off-diagonal elements (unless **y** also correlates spatially-and-cross-temporally) but non-zero diagonal elements (reflecting the contemporaneous spatial correlation in **y**).[7] Recall that the w_{ij} elements of $\mathbf{W}$ reflect the degree of connection from unit *j* to *i*—so, importantly, $\mathbf{W}$ need not be symmetric—and 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 interdependence, whereas the w_{ij} describe the relative magnitudes of the interdependences paths between the sample units. Typically, the set of w_{ij} are determined by theoretical and substantive argumentation as to which countries will have greatest affect on outcomes in which other countries; ρ is the coefficient on these pre-specified spatial-lag weights, reflecting the general strength of interdependence by these pre-specified **W** paths. In political economy, for example, a common operationalization of interdependence 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 **y** then gives as a right-hand-side variable in the regression the weighted sum (or average) of **y** in the other countries *j* that time-period. The matrix **Wy** 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 mechanisms, for example, is to consider outcomes from some country or set of countries *j* to diffuse to the outcome in *i* but not the outcomes from other countries. This implies the weights are *1* or *1/(N-1)* (for sums or averages) for some *ij* and 0 for others, so interdependence either occurs from some *j* to some *i* (e.g., if they use a common language) or it does not (if they do not), but otherwise

[7] Much of the methodological literature on spatial dependence focuses on cross-sections of data ($T=1$). For a comprehensive treatment of spatial econometrics, see Anselin 1988, and for new developments, see Anselin 2001.

the mathematics are the same.

One could estimate the coefficients in **(5)** several ways. One could simply drop $\rho\mathbf{Wy}$ (i.e., ignore interdependence) and estimate $\boldsymbol{\beta}$ by ordinary least-squares regression. Call this *non-spatial OLS* (OLS). This strategy, perhaps with standard-error corrections (PCSE), is certainly simplest but will typically suffer omitted variable biases and/or inefficiency. A second strategy, almost as simple to implement, is to estimate ρ and $\boldsymbol{\beta}$ by an OLS regression including both $\mathbf{Wy}$ and $\mathbf{X}$ on the right-hand side. Call this *spatial OLS* (S-OLS).[8] Unfortunately, because $\mathbf{Wy}$ is endogenous (as explained below), S-OLS suffers simultaneity bias. A third strategy is maximum-likelihood (ML) estimation of ρ and $\boldsymbol{\beta}$ in a model that specifies the joint likelihood of $\mathbf{y}$, fully reflecting spatial interdependence (Ord 1975). *Spatial ML* (S-ML) estimation is computationally intense and can be difficult to implement in models with more than the simplest forms of spatial dependence, but its parameter estimates would be consistent and asymptotically efficient.[9] A fourth strategy is to instrument for $\mathbf{Wy}$ using $\mathbf{X}$ and $\mathbf{WX}$. This approach, call it *spatial instrumental-variables by two-stage least-squares* (S-2SLS-IV), would also produce consistent and asymptotically efficient estimates, like all appropriately specified 2SLS-IV estimates do, provided its necessary conditions are met: namely, that the $\mathbf{X}$ are indeed exogenous but related to $\mathbf{Y}$.[10]

The two simplest estimators are, as just noted, inconsistent—their estimates of model parameters do not converge to the true parameter values as sample sizes increase—due to omitted-variable (or misspecification) bias for *non-spatial OLS* and simultaneity bias for *spatial OLS*, This does not imply, however, that either of these biases (and inconsistencies and inefficiencies) will

[8] Others (e.g., Land and Deane 1991) call this the *generalized population potentials* estimator (GPP)
[9] More precisely, as with all MLE, such estimates would, if the model is correctly specified, be *BANC*: "best asymptotic-normal and consistent", i.e., most efficient among estimators that are consistent and asymptotically normally distributed.
[10] 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.

necessarily be large, nor certainly that they will be equal, nor even that a consistent estimator like S2SLS-IV or S-ML will necessarily have smaller mean-squared error, so the relative performance of these estimators in typical samples reflecting the generic C&IPE context given in **(4)** will be especially important to assess. Our experiments suggest that the biases of non-spatial OLS generally are large whereas those for spatial OLS, especially with appropriately specified models of domestic and exogenous-external factors, are typically smaller, especially insofar as the overall strength of interdependence, ρ, remains modest. Spatial OLS can, therefore, perform adequately in mean-squared error comparison to S-2SLS-IV and S-ML under some conditions, even given perfect instruments, in small samples with modest interdependence strength. On the other hand, with appreciable interdependence strength, using one of the consistent estimators becomes more important, and there, once again, we find the simpler S-2SLS to perform remarkably well in comparison to the more technically challenging S-ML estimator.

In C&IPE, spatial-lag models have been used in one of two ways. Some scholars treat the spatial lags as substantively interesting regressors (e.g., Simmons, Dobbin, and Garrett 2004), employing theoretically informed spatial weights (e.g., Basinger and Hallerberg 2004, Simmons and Elkins 2004) to generate their spatial lags. For example, one could operationalization standard tax-competition arguments using 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 w_{ij} with the stacked dependent variable **y**, then gives the weighted sum (or average) of **y** in the other countries *j* that time-period as a right-hand-side variable. Others employ spatial lags but treat them solely as nuisance controls, using simple, arbitrary spatial weights like *1/(N-1)* (e.g., Franzese 2002, Hays 2003). This adds unweighted averages of **y** in the other countries *j* that time-period as a nuisance-control regressor. Accordingly, our econometric exploration will also want to compare the performance of such relatively *ad hoc*, nuisance-control strategies to more theoretical specified alternatives. Our findings suggest that specification/measurement error in **W**, which any arbitrary nuisance control essentially admits and accepts, can induce sizable biases and inefficiencies, although the damage is not so great if the specification/measurement error in the w_{ij} is uncorrelated and, in particular, orthogonal to the domestic and exogenous-external components of the model. On the other hand, our results also illustrate that nuisance controls which yield unweighted averages or sums of $\mathbf{y_j}$ as regressors render distinction of common exogenous-external shocks from interdependence extremely difficult empirically. Accordingly, we will suggest also an inductive strategy intermediate between full theoretical pre-specification and fully arbitrary simplifications: namely, pre-estimate spatial partial-correlations from theoretically related auxiliary data.

B. Spatial-Lagged-Error Models:

Before proceeding to econometric evaluation of estimators for the S-LDV model, consider a related model in which spatial interdependence resides only in the stochastic (disturbance) terms of our models. In the "nuisance" approach to spatial dependence now standard in C&IPE, OLS-PCSE, e.g., one is assuming that spatial dependence in the true data generating process is limited entirely to the error term, $\boldsymbol{\varepsilon}$, and completely uncorrelated with the systematic component (i.e., with $\mathbf{X}\boldsymbol{\beta}$ and anything else in the model). Regarding tax competition, e.g., this would imply that only unexpected changes in other countries tax policies, *and not expected ones,* would cause governments to reform their own. This seems unlikely, but perhaps capital flows are more sensitive to unexpected tax policy changes. In any case, this would give what the spatial econometrics literature calls a *spatial error* model:

$$\begin{aligned}\mathbf{y} &= \mathbf{XB} + \boldsymbol{\varepsilon} \\ \boldsymbol{\varepsilon} &= \rho\mathbf{W}\boldsymbol{\varepsilon} + \mathbf{u}\end{aligned} \quad \textbf{(6)}$$

As before, **y** is an *NTx1* vector of dependent-variable observations, stacked by unit (i.e., unit 1, time

1 to *T*, then unit 2, time 1 to *T*, *etc.* through unit *N*), and **W** is an *NTxNT* block-diagonal spatial-weighting matrix (with elements w_{ij}). Thus, $\rho\mathbf{W\varepsilon}$ is an *NTx1* matrix of spatially correlated disturbances. The *NTx1* vector **u** contains the white-noise *i.i.d.* shocks in this spatially correlated disturbance series. Otherwise, model interpretation remains as in **(5)**.[11]

As with any non-spherical error-structure, and assuming $Cov(\mathbf{X},\boldsymbol{\varepsilon})=0$, OLS coefficient estimates in this model will be unbiased and consistent, although inefficient. OLS standard-error estimates will also be inefficient and generally biased and inconsistent (downward) as well. The feasible generalized least squares (FGLS) estimator, developed for this context by Parks (1967), is efficient, but very few political scientists use Parks FGLS since the publication of Beck and Katz (1995). Beck and Katz argued, and demonstrated with Monte Carlo simulations, that Parks' method underestimates the true variance of the FGLS estimator's sampling distribution when *T* insufficiently exceeds *N*. Moreover, they argued the efficiency gains are modest in sample sizes typical in political science research. Thus, small efficiency gains come at a high cost in terms of overconfidence (i.e., significantly underestimating true standard errors), so their recommendation, which is now widely followed, was to report OLS coefficient estimates with their robust (i.e., consistent) panel corrected standard errors (PCSE).

One problem with both Parks-FGLS and OLS-PCSE is that, with spatial dependence relegated to the error term of the model, analysts tend to forget or ignore it. That is, analysts have tended to conclude on the basis of small or insignificant coefficients on international variables (but absent spatial lags) that interdependence is weak, even though their FGLS estimates or differences between

[11] Notice that the diagonal elements of the off-diagonal *TxT* blocks in **W** still reflect the contemporaneous effect of the column unit on the row unit; i.e., the w_{ij} still reflect the degree of connection from unit *j* to *i*. Thus, again, *and unlike a variance-covariance matrix,* **W** need not be symmetric. One of the limitations of empirical models that, unlike spatial-lag models, rely on error or dependent-variable covariances to model spatial-interdependence is this symmetry they impose. Interdependence certainly need not be symmetric; in PE, e.g., Luxembourg→U.S. = U.S.→ Luxembourg is rarely plausible.

OLS and PCSE standard-errors may actually suggest spatial error-correlation. To help debar this oversight, and perhaps to gain some FGLS efficiency without the standard-error overconfidence noted by Beck and Katz, we propose a simple spatial-error OLS (S-E-OLS) estimator that uses the intended model and spatial interdependence in its estimated residuals to create a spatial lag that, for each unit in the dataset, is a weighted average of the contemporaneous OLS residuals from the other units. That is, the strategy is just the spatial analogue to the familiar Cochrane-Orcutt procedure for OLS estimation of temporally lagged-residual models of serial correlation. More specifically, our S-E-OLS approach, which is only appropriate when the conditions in **(6)** hold (including the substantively unlikely one that only unexpected movements in foreign policies or outcomes affect domestic ones), follows four steps:

1) Estimate the regression of $\mathbf{y}$ on $\mathbf{X}$ using non-spatial OLS
2) Use the OLS residuals from step one to estimate $\hat{\boldsymbol{\varepsilon}} = \boldsymbol{\lambda}\hat{\boldsymbol{\varepsilon}} + \mathbf{u}$ again using OLS
3) Implement S-OLS by regressing $\mathbf{y}$ on $\mathbf{X}$ and $\hat{\boldsymbol{\lambda}}\hat{\boldsymbol{\varepsilon}}$
4) Estimate panel corrected standard errors (Beck and Katz 1995)

Using the experimental design from Beck and Katz (1995), Franzese and Hays (2005) show this estimator matches much of the efficiency gains of Parks-FGLS in small samples without the problem of overconfidence, in part because S-E-OLS can be combined with PCSE estimates. The main results from their study are provided in Appendix I (Tables A1 and A2). To summarize here, our experiments suggest (a) that S-E-OLS outperforms non-spatial OLS on efficiency grounds when the strength of contemporaneous correlation, $|\rho|$, exceeds 0.25 and T>2N, and (b) that S-E-OLS with PCSE performs as well as non-spatial OLS with PCSE in standard-error accuracy terms. Perhaps more importantly, though, the direct estimation of ρ in S-E-OLS+PCSE as opposed to in OLS+PCSE prevents the researcher from forgetting the interdependence that s/he has banished to the error term exclusively. However, as noted above, the spatial-lag-error model will frequently make little sense substantively; for example, in the present empirical example, expected and unexpected ALM spending by one's neighbors/partners would presumably create equal positive externalities and so induce the same under-investment *cum* negative interdependence. The spatial-lag *(of the dependent variable)* model will be more appropriate.

C. Analytical Results for OLS and S-OLS: Omitted-Variable and Simultaneity Biases

In this section, we demonstrate analytically, in the simplest possible case—one domestic factor, *X*; interdependence 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 interdependence, and we specify those biases insofar as possible.

$$Y_1 = \beta_1 X_1 + \rho_{12} Y_2 + \varepsilon_1 \quad \textbf{(7)}$$
$$Y_2 = \beta_2 X_2 + \rho_{21} Y_1 + \varepsilon_2 \quad \textbf{(8)}$$

Substituting country 2's outcome from equation **(8)** into equation **(7)**, which determines country 1's outcome, gives

$$Y_1 = \beta_1 X_1 + \rho_{12}\left(\beta_2 X_2 + \rho_{21} Y_1 + \varepsilon_2\right) + \varepsilon_1 \quad \textbf{(7')}.$$

Performing the converse substitution gives

$$Y_2 = \beta_2 X_2 + \rho_{21}\left(\beta_1 X_1 + \rho_{12} Y_2 + \varepsilon_1\right) + \varepsilon_2 \quad \textbf{(8')},$$

which can be solved for Y_1 to give

$$Y_1 = \frac{\beta_1}{1-\rho_{21}\rho_{12}} X_1 + \frac{\rho_{12}\beta_2}{1-\rho_{21}\rho_{12}} X_2 + \frac{\rho_{12}}{1-\rho_{21}\rho_{12}} \varepsilon_2 + \frac{1}{1-\rho_{21}\rho_{12}} \varepsilon_1 \quad \textbf{(7'')}.$$

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

$$Y_2 = \frac{\beta_2}{1-\rho_{21}\rho_{12}} X_2 + \frac{\rho_{21}\beta_1}{1-\rho_{21}\rho_{12}} X_1 + \frac{\rho_{21}}{1-\rho_{21}\rho_{12}} \varepsilon_1 + \frac{1}{1-\rho_{21}\rho_{12}} \varepsilon_2 \quad \textbf{(8'')}.$$

From any of these equations, one can see that S-OLS estimation of **(7)** and/or **(8)** will be biased and inconsistent, even if both the domestic and the interdependence aspects of the empirical model were specified for estimation exactly as in these equations. In each **(7)** and **(8)**, notice that a right-hand-side regressor, Y_2 and Y_1 respectively, is itself partly determined in the other equation by the left-hand-side dependent variable of the current equation: textbook illustration of endogeneity. In **(7')** and **(8')**, one sees 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 covariance of regressors Z with true residuals ε is 0. Finally, in **(7")** and **(8")**, one sees that Y_1 and Y_2 also contain ε_2 and ε_1 respectively, so Y_1 as a regressor for an equation with Y_2 as dependent variable indeed has $Cov(Z,\varepsilon)\neq 0$.

Indeed, from **(8")**, one sees the covariance of the regressor Y_1 with the residual ε_2 is specifically

$$\begin{aligned} &Cov\left(\frac{\beta_1}{1-\rho_{21}\rho_{12}}X_1+\frac{\rho_{12}\beta_2}{1-\rho_{21}\rho_{12}}X_2+\frac{\rho_{12}}{1-\rho_{21}\rho_{12}}\varepsilon_2+\frac{1}{1-\rho_{21}\rho_{12}}\varepsilon_1\ ,\ \varepsilon_2\right)\\ &=Cov\left(\frac{\rho_{12}}{1-\rho_{21}\rho_{12}}\varepsilon_2\ ,\ \varepsilon_2\right)\\ &=\left(\frac{\rho_{12}}{1-\rho_{21}\rho_{12}}\right)Var(\varepsilon_2)\end{aligned} \quad \textbf{(9).}$$

Now combine **(9)** with the following form of the OLS estimates of any set of coefficients $\boldsymbol{\gamma}$

$$\hat{\gamma}=\gamma+\left(\frac{Z'Z}{n}\right)^{-1}\frac{Z'\varepsilon}{n} \qquad \textbf{(10)}$$

the second term of which gives the simultaneity 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

$$bias(\hat{\rho}_{21}) = \left(\frac{Z'Z}{n}\right)^{-1}\left(\frac{\rho_{12}}{1-\rho_{12}\rho_{21}}\right)Var(\varepsilon_2) \quad \textbf{(11)}$$

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}.[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 interdependence would, by ignoring the dampening feedback from the negative Germany→US interdependence, lead one to underestimate the positive US→Germany interdependence. Conversely, trying to estimate the negative Germany→US interdependence by spatial OLS would incur positive simultaneity bias by ignoring the dampening US→Germany feedback. Thus, oppositely signed interdependence induces a simultaneity bias in spatial OLS that favors underestimation of interdependence. More commonly, though, as the introduction notes, the various interdependence mechanisms seem likely to induce positive (i.e., same-signed) interdependence, so the simultaneity bias in spatial OLS tends to inflate interdependence estimates.

However, the bias need not be sizable, certainly does not preclude that S-OLS might improve dramatically upon non-spatial OLS, which, as we demonstrate below, suffers omitted-variable biases instead, and may be outweighed in mean-squared-error terms even compared to methods that do address the simultaneity, such as S-2SLS-IV, by the generally greater efficiency of OLS than these alternatives. Indeed, in some conditions, this is what we find in our simulations below, even though our instruments are (unrealistically) perfect. For now, note that the simultaneity bias of spatial OLS will not be large unless the function of ρ in the second term of **(11)** 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

[12] Note that we assume throughout that $|\rho|<1$, as $|\rho|\geq1$ implies spatial unit-roots, which are highly problematic statistically and, thankfully, highly improbable substantively. This implies that $|\rho_{12}\rho_{21}|<1$ also.

simultaneity induced overestimation of ρ would not be large unless interdependence 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). That is, generally, if interdependence is a strong *and* domestic and exogenous-external factors account for little systematic variation, then simultaneity biases will be sizable.

In sum, we just showed that spatial OLS suffers a simultaneity bias in the estimation of interdependence 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 or S-2SLS-IV or S-ML. Accordingly, we will explore later some 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 interdependence effects in the case of mutual-reinforcement (same-signed) mechanisms—induces an oppositely signed bias in the estimate of β—dampening the estimated effects of domestic and exogenous-external stimuli. 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

$$\hat{\beta}_1 = \frac{\left(\sum X_1Y_1\right)\left(\sum Y_2^2\right)-\left(\sum Y_2Y_1\right)\left(\sum X_1Y_2\right)}{\left(\sum X_1^2\right)\left(\sum Y_2^2\right)-\left(\sum X_1Y_2\right)^2} \quad \textbf{(12)}.$$

Using equation **(7)** to substitute for Y_1 gives

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

(13).

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

$$\hat{\beta}_1 = \beta_1 + \frac{\left(\sum X_1\varepsilon_1\right)\left(\sum Y_2^2\right)-\left(\sum X_1Y_2\right)\left(\sum Y_2\varepsilon_1\right)}{\left(\sum Y_2^2\right)-\left(\sum X_1Y_2\right)^2} \quad \textbf{(14)}.$$

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 denominator gives

$$\hat{\beta}_1 = \beta_1 - \frac{\left(\frac{\rho_{21}\beta_1}{1-\rho_{21}\rho_{12}}\right)\left(\frac{\rho_{21}Var(\varepsilon_1)}{1-\rho_{21}\rho_{12}}\right)}{\left(\frac{\beta_2}{1-\rho_{21}\rho_{12}}\right)^2 + \left(\frac{\rho_{21}}{1-\rho_{21}\rho_{12}}\right)^2 Var(\varepsilon_1) + \left(\frac{1}{1-\rho_{21}\rho_{12}}\right)^2 Var(\varepsilon_2)} \qquad \textbf{(15)},$$

which simplifies to

$$\hat{\beta}_1 = \beta_1 - \frac{\beta_1 Var(\varepsilon_1)\rho_{21}^2}{\beta_2^2 + \rho_{21}^2 Var(\varepsilon_1) + Var(\varepsilon_2)} \qquad \textbf{(16)}.$$

By a parallel series of steps, we can reduce the formula in mean-deviated scalar-notation for the spatial OLS estimate of ρ_{12} to

$$\hat{\rho}_{12} = \rho_{12} + \frac{\rho_{21} Var(\varepsilon_1)(1-\rho_{21}\rho_{12})}{\beta_2^2 + \rho_{21}^2 Var(\varepsilon_1) + Var(\varepsilon_2)} \qquad \textbf{(17)}.$$

The second terms of **(17)** and **(16)** are the simultaneity bias in spatial 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 **(16)** but a positive sign in **(17)** 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 most common case of positively reinforcing interdependence (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. In a case like that of the empirical example to be considered later, (externality-induced) reinforcing negative-feedback interdependence, $\hat{\rho}_{12}$ is negative and will have negative 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. In either case, then, *reinforcing-feedback* interdependence inflates S-OLS estimates of the strength of interdependence and induces thereby attenuation biases deflating estimates of the strength of domestic or exogenous-external factors. The implications of countervailing-feedback interdependence derive analogously (usually dampened estimates of interdependence; inflated estimates of domestic or exogenous-external effects, although keeping track of signs can cause headaches).

The upward bias in β induced by omitting the spatial lag when interdependence actually exists—i.e., the bias in non-spatial OLS is much easier to demonstrate, being a simple case of omitted-variable bias, the formula for which is well-known to be $\boldsymbol{F\beta}$ 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

$$OVB_2 = \frac{\rho_{12}\rho_{21}\beta_1}{1-\rho_{12}\rho_{21}} \qquad \textbf{(18)},$$

and

$$OVB_3 = \frac{\rho_{12}\rho_{21}\beta_2}{1-\rho_{12}\rho_{21}} \qquad \textbf{(19)}.$$

In the case of reinforcing-feedback interdependence, these imply inflation bias, whereas with counter-vailing feedback (which we believe substantively less common), these will be attenuation bias, in the estimates of domestic or exogenous-external effects.

In summary, in the simplest possible case (one domestic or exogenous-external factor; two countries; and conditionally *i.i.d.* errors) *OLS* estimates of **(7)** or **(8)** suffer simultaneity bias, and *OLS* estimates of **(7)** or **(8)** omitting the spatial lag will suffer omitted-variable bias. *OLS* estimates of interdependence from country *j* to *i* will have bias of the same sign as the interdependence from *i* to *j*. If "feedback" from *j* to *i* and *i* to *j* reinforce (both positive or both negative), then *OLS* estimates of interdependence will be inflated. If feedback is dampening (i.e., opposite signed ρ_{ij} and ρ_{ji}), which is probably less likely in most substantive contexts, *OLS* estimates of ρ will be attenuated. Moreover, this bias in the estimated strength of interdependence, ρ, induces an attenuation bias in the estimate of β, the effect of X (i.e., domestic and/or exogenous-external factors). Thus, typically, *OLS* estimates of *C&IPE* models will tend to over-estimate the importance of interdependence (e.g., tax competition, ALMP externalities) and underestimate that of domestic, exogenous-external, and domestic-context-conditional exogenous-external mechanisms (i.e., open-economy *CPE* mechanisms). On the other hand, *OLS* estimates of *CPE* models that ignore interdependence, i.e., that omit spatial lags, suffer the converse omitted-variable biases. Again, with reinforcing feedback (same-signed ρ_{12} and ρ_{21}), these are inflation biases; with dampening feedback, these are attenuation biases. Thus, in the positive-feedback case that we suspect more common, *OLS* estimates of *CPE* models that ignore *interdependence* will tend to over-estimate the power of domestic, exogenous-external, and/or domestic-context-conditional exogenous-external explanations. Finally, these conclusions hold as a matter of degree also: insofar as *interdependence* is inadequately specified, absolutely and relatively to the alternative CPE argument specification, the latter will tend to be overestimated and the former underestimated, and *vice versa*. For example, when spatial lags are generated with arbitrary weights, as often done when they are included as nuisance controls, the coefficient estimates on these lags will likely be biased downwards.

Furthermore, the omitted-variable biases in **(18)** and **(19)** assume X_1 and X_2 independent. If domestic (or exogenous-external) factors correlate spatially, which is (highly) likely, the biases are larger. For example, if the *X*'s represent fully common exogenous-external shocks (i.e., $X_1 = X_2$; e.g., time-dummy fixed-effects), the OLS estimate for β_1 will be

$$\hat{\beta}_1 = \beta_1 + \frac{\rho_{21}\beta_2 + \rho_{12}\rho_{21}\beta_1}{1-\rho_{12}\rho_{21}} \quad \textbf{(20),}$$

and analysts will greatly overestimate the importance of (common) exogenous-external shocks, if they omit the spatial lag. Conversely, omitting common-shock variables will lead to analysts to overestimate spatial-lag coefficients greatly. Assuming X_1 and X_2 are equal, the OLS estimate for ρ_{12} will be

$$\hat{\rho}_{12} = \rho_{12} + \frac{\beta_1\beta_2 + \rho_{21}\beta_1^2}{(1-\rho_{12}\rho_{21})(\beta_2^2 + \beta_1^2\rho_{21}^2 + \rho_{21}^2 \operatorname{var}(\varepsilon_1) + \operatorname{var}(\varepsilon_2))} \quad \textbf{(21).}$$

These simultaneity biases will typically be inflating (reinforcing feedback), but time-differencing and/or -lagging the spatial-lag can alter these biases. Appendix II briefly considers such time-lagged and/or differenced spatial lags, elaborating specifically the case of temporally lagged, differenced spatial lags (used in Basinger and Hallerberg 2004, e.g.), in which the simultaneity bias is attenuating.

These analytical results demonstrate that both non-spatial OLS and spatial S-OLS estimates of interdependent processes are biased and inconsistent and that the magnitudes of these biases increase with the general strength of the interdependence process. Typically, OLS, by not modeling interdependence, inflates estimates of domestic or exogenous-external effects while ignoring interdependence entirely. S-OLS, by contrast, typically overestimates the strength of interdependence and dampens estimates of domestic or exogenous-external effects. Researchers interested in gauging the relative strength of interdependence (IPE) effects vs. exogenous-external or domestic (CPE) effects therefore especially need to weigh carefully these specification and estimation-strategy considerations. Fortunately, instrumental-variables (IV) and maximum-likelihood (ML) methods for redressing the simultaneity problems of S-OLS exist. Spatial instrumental-variables by two-stage-least-squares (S-2SLS-IV) may suffer *quasi-instrument* (Bartels 1991) and efficiency problems characteristic of all *IV* estimators. And spatial ML (S-ML), for its part, is computationally demanding even in simple models with few non-spatial regressors and just one spatial lag, with computational and mathematical complexity rising exponentially as models expand. And S-ML, like all ML estimators, has only asymptotic properties assuredly (namely: consistency, asymptotic efficiency, asymptotic normality), so its performance in realistic, limited samples demands further exploration. Moreover, even in this simplest case of one non-spatial regressor, two countries, and one spatial lag, determining analytically whether one, both, or neither of the omitted-variable biases in OLS and simultaneity biases in S-OLS will typically be appreciable, and which might typically be the larger problem, and how either OLS or S-OLS compare with S-2SLS-IV or S-ML seems impossible (at least to us), so we turn now to Monte Carlo simulation to compare these estimators in richer, more realistic scenarios.

(Since the magnitudes of all these concerns depend on the amount of spatial interdependence in the data, researchers might first want some mechanism for testing the presence and gauging the degree of spatial correlation in their data. Appendix III introduces and conducts a simple simulation of the size and power of one such test.)

D. Comparing Estimators under More Realistic and Limited Sample Conditions

1. Design of the Simulation Exercises (Monte Carlo Experiments)

In designing our experiments, our aim is to explore scenarios reflecting common circumstances in modern comparative and international political-economy research (e.g., globalization or European integration and tax competition or, as in the empirical example to be considered later, ALM spending). Accordingly, the true model in our experiments will contain explanitors reflecting domestic factors, exogenous-external conditions, and the interaction of these two (i.e., context-conditional open-economy CPE mechanisms) as well as interdependence (IPE mechanisms). Specifically, the true model that generates the data for our experiments is the reduced-form solution of the spatial-lag model **(5)**,[13]

$$\mathbf{y} = (\mathbf{I} - \rho\mathbf{W})^{-1}\mathbf{X}\beta + (\mathbf{I} - \rho\mathbf{W})^{-1}\varepsilon \qquad \textbf{(22).}$$

with matrix **X** of non-interdependence elements having three components, $\boldsymbol{\xi}$, $\boldsymbol{\eta}$, and $\boldsymbol{\xi\eta}$, respectively reflecting domestic, exogenous-external, and context-conditional exogenous-external factors, as in the generic C&IPE model given in **(4)**. The vector $\boldsymbol{\xi}$ is an *NTx1* stack of *i.i.d.* draws from a standard normal distribution, which, being thus unique to each spatial unit in each time period, represent purely domestic factors (e.g., domestic institutions, structures, and conditions) in each unit *i* at each time *t*. The vector $\boldsymbol{\eta}$ is an *NTx1* stack of *T* vectors, each *Nx1* in size, 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 differs from others. $\boldsymbol{\eta}$ thus represents a set of globally common shocks, one occurring in each of the *T* time-periods, each hitting each cross-sectional unit equally, and each also drawn *i.i.d.* from a standard normal distribution. The interaction term, $\boldsymbol{\xi\eta}$, captures the notion that the effects of the common exogenous-external shocks are mediated by domestic variables. In other words, our domestic model is of the context-conditional open-economy CPE sort. Additionally, however, the model will involve interdependence of the sort IPE implies, with average magnitude ρ and with

[13] Our model differs from that of Kelejian et al. (XXXX), the most similar of other extant simulation studies in spatial econometrics, in that we use spatially orthogonal disturbances. Like those and all others, we assume that the matrix $(\mathbf{I} - \rho\mathbf{W})$ is invertible, which essentially is to debar spatial unit-roots.

specific connections from unit j to unit i of magnitudes w_{ij}.

Drawing the data for **ξ**, **η**, and **ξη**—i.e., for **X**—in this manner, we then generate the data for **y** by **(22)** using two different coefficient vectors, $(\beta_1, \beta_2, \beta_3, \rho)$, and three different spatial-connectivity matrices, **W**. 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 coefficient vector has smaller ρ than the other, and recall that the spatial weights w_{ij} determine the relative importance of each unit to each of the others in the pattern of spatial interdependence given by **W** while ρ determines the average strength of interdependence. Thus, the second coefficient vector represents a stronger interdependence process.

Regarding spatial connectivity, we first assume the spatial weights, w_{ij}, are time-invariant so all the elements along the diagonals of the TxT off-diagonal blocks of **W** are the same. That is, only one w_{ij} connecting j to i persists for all T periods; this connectivity does not change over time. We consider three patterns of interdependence between spatial units, **W**. 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 regressor in each unit-year to reflect this proposition is just an unweighted average of the dependent variable for the other units that year.[14] 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 interdependence 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 explore the implications of unmodeled or

[14] 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 (say bordering units, or units with the same language, or same treaty or organization membership) with $y=1$ *or* $y=0$ often used in those contexts. Franzese (2002), Hays (2003), and others have also employed this unweighted-average spatial-lag in their empirical models, but, their arguments being of context-conditional open-economy CPE sort, these spatial lags are often treated as nuisance controls and their interdependence implications left unexplored in theoretical or substantive discussion.

imperfectly modeled heterogeneity in the pattern of interdependence, an obviously key practical consideration for any attempt to model interdependence (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 the exogenous-external common shocks, which will enable us to compare non-spatial OLS, which omits interdependence, to S-OLS omitting or imperfectly modeling the *context-conditional exogenous-external* alternative hypothesis (Table 6). For the third **W** matrix we added a random draw from a uniform distribution with support [–0.5, +0.5], representing a more heterogeneous pattern of interdependence and facilitating exploration of a greater degree of misspecification of **W**.

To estimate spatial OLS, S-2SLS-IV or S-ML models, the researcher must pre-specify a spatial weighting matrix. This is a critical theoretical and empirical step for the scholar of interdependence; as already emphasized, distinguishing between and evaluating the relative strength of interdependence and exogenous-external factors (Galton's famous problem) relies firstly upon the relative precision with which these alternative sources of spatial correlation are specified and how much empirical power these alternative specifications actually offer. For all the estimations in our simulations, however, simply setting all non-zero elements of **W** to 1/(N-1) suffices to explore the possibilities. I.e., 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 on the right-hand-side, with instrumentation in the S-2SLS-IV case and without in the S-OLS case. S-ML likelihoods specified for empirical estimation likewise reflect this uniform-interdependence pattern. Thus, in the first set of experiments—the homogenous interdependence case—the weighting matrix used for estimation will be the true weighting matrix; the researcher will have specified the interdependence process exactly correctly (Tables 1-4, and 6). In the other cases, the researcher pre-specified weighting matrix contains imperfect, although unbiased, estimates of the true spatial weights (Table 5), the degree of imperfection greater in the case of ±0.5 heterogeneity in weights. We intend this imperfection to reflect the realism that analysts will rarely know the true spatial weighting matrix, but will rather use some theoretically or conveniently specified approximation (of varying accuracy) to it. Our addition of the second and third spatial-weighting matrices allows us to explore the consequences of analysts using the wrong **W**. 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.[15]

Likewise, we wish to explore the ramifications, primarily for estimates of interdependence, 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 previously stated. Then, we consider two estimation models: including all of $\boldsymbol{\eta}_t$ in the model (Tables 1-5) and including only one component of $\boldsymbol{\eta}_t$ (Table 6). This enables us to explore the relative magnitudes of the improvements offered by S-OLS models of interdependence over non-spatial OLS as the hypothetical interdependence-scholar's controls for the main alternative hypothesis, context-conditional exogenous-external effects, improve.

We evaluate the (non-spatial) OLS, (spatial) S-OLS, S-2SLS-IV, 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

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

correlation: PCSE). We report results for samples with dimensions N = 5, 40 and T = 20, 40.[16] The results for all of our Monte Carlo experiments are based on at least 100 simulations (1000 for the key ones reported). We report the mean coefficient estimates, the mean OLS (ML) standard-error estimates, the mean PCSE estimates, the actual standard deviation of the coefficient 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 the experiment gives the bias. We chose coefficients of $\beta=1$ (and $\rho=.1$ or $.5$) so the percentage biases 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 OLS standard errors and whether and how well PCSE may redress any such over-confidence. The RMSE, being the square root of the sum of the bias-squared and the actual variance of the coefficient estimates, offers summary evaluation combining both bias/consistency and efficiency considerations.

2. Simulation Results: OLS vs. S-OLS, Omitted-Variable vs. Simultaneity Biases

We start by comparing the two simple but inconsistent estimators: non-spatial and spatial OLS. Is using either of these estimators ever reasonable? 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 interdependence is relatively weak. Therefore, we expect the omitted-variable and simultaneity biases to be small such that non-spatial OLS performs somewhat but not terribly poorly and spatial OLS offers some improvement over that.

In these experiments, the omitted variable bias in the non-spatial OLS estimates manifests

[16] 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.

primarily in β_2, the coefficient on the common shock.[17] In Table 1, OLS overestimates the (so-called) direct effect of these common shocks by 0.112 on average (i.e., about 11%—recall: true β are 1). All of the other parameter estimates are very close to their true values. Average S-OLS estimates of β_2 are indeed much better. The size of the biases ranges from -0.009 to +0.027 (averaging about ±1.8%). Note, however, that the sampling variability of the S-OLS estimator for β_2 is large (because of multicolinearity) relative to non-spatial OLS. This makes non-spatial OLS preferable on RMSE grounds. This result seems to hold only for very small samples and weak interdependence though.

S-OLS overestimates β_2 in one of the experiments and underestimates it in the other. Notice, too, that the biases in the S-OLS estimates of β_2 and ρ also are negatively related; i.e., when β_2, the effect of common shocks, is overestimated (underestimated), ρ, the strength of interdependence, is underestimated/overestimated. This robust finding, which is more pronounced when interdependence is stronger (Tables 3-4), underscores the difficulty of isolating the effects of common external shocks from interdependence with non-spatial and S-OLS estimators, especially when both are homogenous and so wholly undifferentiated in their pattern of incidence as in these experiments. The negative relationship is consistent with our analytic demonstration for the positive true β_2 and ρ case above. Increasing N from 5 to 40 reduces the variability of all three estimators' sampling distributions (Table 2), most noticeably for β_1, the coefficient on the domestic variable X.

When T is small, the mean reported standard errors seem to underestimate the true variability in the S-OLS estimator.[18] This is particularly true for the standard errors on β_2 and ρ, nor do panel-corrected standard-errors (PCSE) seem to provide better estimates. (However, with non-spatial OLS,

[17] We suspect that, but have not yet explored whether, the omitted-variable biases concentrate so heavily, almost exclusively, in the coefficient on common shock, and are almost absent from the estimates of the domestic and domestic-factor-moderated responses to common shocks because our pattern of interdependence weights is uniform and unrelated to those domestic factors, which in turn are i.i.d. and so exhibit no spatial correlation themselves.

[18] This and previous results are consistent with Doreian et al. (1984) simulation results from a simpler true model.

PCSE do provide better standard error estimates for β_2.)[19] 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 underestimates 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. Thus, samples with larger time dimensions relative to cross-sectional ones seem to aid separating exogenous-external (CPE) effects from interdependence (IPE) and obtaining accurate estimates of the standard errors of these distinct effects.

[19] This is not surprising. PCSE 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 from the OLS variance-covariance estimator except by being slightly less efficient. PCSE 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 estimates for $V(\beta_2)$ are better on average than the OLS standard errors.

Table 1. Comparing Estimators (N=5, ρ = 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	0.998	0.104	0.103	0.993	0.105	0.105	0.992	0.106	0.106	0.972	0.121	0.124
	$s.e.(\beta_1)$	0.105	0.012		0.106	0.012		0.107	0.014		0.107	0.012	
	$pcse(\beta_1)$	0.103	0.012		0.103	0.013							
	β_2	1.112	0.124	0.167	1.027	0.215	0.216	1.003	0.236	0.235	1.046	0.219	0.222
	$s.e.(\beta_2)$	0.108	0.020		0.175	0.037		0.213	0.081		0.181	0.036	
T=20	$pcse(\beta_2)$	0.111	0.026		0.163	0.042							
	β_3	1.005	0.115	0.115	1.002	0.117	0.117	1.001	0.119	0.119	0.977	0.139	0.140
	$s.e.(\beta_3)$	0.111	0.024		0.111	0.024		0.113	0.026		0.115	0.024	
	$pcse(\beta_3)$	0.108	0.025		0.108	0.025							
	ρ				0.078	0.154	0.155	0.097	0.177	0.177	0.074	0.141	0.143
	$s.e.(\rho)$				0.119	0.024		0.158	0.068		0.074	0.006	
	$pcse(\rho)$				0.112	0.028							
	β_1	1.008	0.072	0.072	1.004	0.071	0.071	1.003	0.072	0.071	1.003	0.072	0.072
	$s.e.(\beta_1)$	0.073	0.005		0.072	0.005		0.073	0.005		0.071	0.006	
	$pcse(\beta_1)$	0.072	0.005		0.072	0.005							
	β_2	1.112	0.081	0.139	0.991	0.125	0.125	1.001	0.139	0.139	1.025	0.120	0.122
	$s.e.(\beta_2)$	0.074	0.009		0.116	0.016		0.135	0.026		0.103	0.012	
T=40	$pcse(\beta_2)$	0.079	0.013		0.110	0.018							
	β_3	1.002	0.075	0.075	1.000	0.075	0.075	0.999	0.075	0.075	0.995	0.086	0.085
	$s.e.(\beta_3)$	0.075	0.011		0.075	0.011		0.075	0.011		0.073	0.011	
	$pcse(\beta_3)$	0.074	0.011		0.074	0.011							
	ρ				0.108	0.092	0.093	0.099	0.103	0.103	0.072	0.066	0.072
	$s.e.(\rho)$				0.079	0.011		0.100	0.023		0.049	0.003	
	$pcse(\rho)$				0.076	0.012							

Table 2. Comparing Estimators (N=40, ρ = 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	0.036	0.036	0.999	0.036	0.036	0.999	0.036	0.036	1.001	0.037	0.037
	$s.e.(\beta_1)$	0.036	0.002		0.037	0.002		0.037	0.002		0.036	0.002	
	$pcse(\beta_1)$	0.036	0.002		0.036	0.002							
	β_2	1.112	0.041	0.119	1.049	0.209	0.215	0.994	0.211	0.211	1.042	0.172	0.176
	$s.e.(\beta_2)$	0.038	0.007		0.152	0.035		0.199	0.083		0.127	0.026	
T=20	$pcse(\beta_2)$	0.039	0.010		0.144	0.046							
	β_3	1.000	0.038	0.038	0.999	0.038	0.038	1.000	0.038	0.038	0.999	0.032	0.032
	$s.e.(\beta_3)$	0.038	0.007		0.038	0.007		0.038	0.007		0.039	0.007	
	$pcse(\beta_3)$	0.038	0.007		0.037	0.007							
	ρ				0.055	0.186	0.191	0.105	0.188	0.188	0.062	0.143	0.147
	$s.e.(\rho)$				0.133	0.030		0.175	0.074		0.025	0.001	
	$pcse(\rho)$				0.126	0.040							
	β_1	1.001	0.026	0.026	1.001	0.026	0.026	1.001	0.026	0.026			
	$s.e.(\beta_1)$	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	0.03	0.116	0.999	0.119	0.119	1.003	0.136	0.136			
	$s.e.(\beta_2)$	0.026	0.003		0.101	0.016		0.126	0.032				
T=40	$pcse(\beta_2)$	0.028	0.005		0.096	0.020							
	β_3	1.000	0.026	0.026	1.000	0.026	0.026	1.000	0.026	0.026			
	$s.e.(\beta_3)$	0.026	0.003		0.026	0.003		0.026	0.003				
	$pcse(\beta_3)$	0.026	0.003		0.026	0.003							
	ρ				0.101	0.105	0.105	0.098	0.120	0.120			
	$s.e.(\rho)$				0.087	0.014		0.111	0.028				
	$pcse(\rho)$				0.083	0.017							

Tables 3 and 4 give the results for a greater overall strength of interdependence, ρ=0.5, and w_{ij}=1/(N-1). With stronger interdependence, we expect the omitted-variable bias of OLS and the simultaneity bias of S-OLS to be larger. Severe positive omitted-variable biases do indeed manifest in all four non-spatial OLS estimates of β_2. Non-spatial OLS overestimates common exogenous-external effects, as expected, by approximately +1.00 (or +100%) for all four sample-dimensions. The estimates for β_1 and β_3, domestic and domestic-context-conditional exogenous-external effects, 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. Intuitively, these effects correlate less with the omitted homogenous interdependence process than do the common exogenous-external effects, so the latter absorbs most of the omitted-variable bias and the former fade far more noticeably as sample-size increases afford greater discrimination between their effects and the omitted interdependence. Moreover, the mean reported standard errors for non-spatial OLS 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%. PCSE also underestimates, but only by 22%. These problems in the standard-error estimates likewise tend to concentrate in the estimates of the common exogenous-external effect. When N and T are 40, however, non-spatial OLS does not noticeably underestimate the sampling variability for β_1 or β_3, but it does for β_2 (by 63%).[20]

The simultaneity biases in spatial OLS, for its part, also become appreciable—estimates of

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

around .58 vs. true .5, so +16% overestimation—with greater strength of interdependence. These biases induce a noticeable bias of opposite sign (and approximately equal magnitude -14% to -17%) in the estimated strength of domestic, exogenous-external, and context-conditional-exogenous-external effects, as expected, which, for reasons just elaborated above, this induced bias concentrates in this example of homogenous interdependence in the common exogenous-external part. Moreover, S-OLS reports remarkably more accurate standard-errors with this stronger interdependence. Discernible inaccuracies (about -10%) remain only in the case where N=40>T=20 and, once again, incur (i.e., are noticeable) only for the two least distinguishable effects, β_2 and ρ.

To sum, simple S-OLS estimates, while not perfect, are usually far better than non-spatial OLS estimates, with the latter better only for very small samples with weak interdependence and only in RMSE terms (and, even there, researchers would wholly miss the omitted interdependence effects). Simultaneity biases in S-OLS estimates of ρ (homogenous interdependence effects) and β_2 (common exogenous-external effects) can be appreciable, although generally smaller than the omitted-variable biases in non-spatial OLS, and 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 interdependence 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, conversely, continues to offer considerable improvements in terms of reducing these biases in coefficient and standard-error estimates as the general strength of interdependence increases, although its tendency to overestimate the strength of interdependence and underestimate the impact of common shocks also continues.

Table 3. Comparing Estimators (N=5, ρ = 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	1.040	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	0.110	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	1.000	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	0.100	0.500	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							

Table 4. Comparing Estimators (N=40, ρ = 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
	$s.e.(\beta_1)$	0.038	0.002		0.036	0.002		0.037	0.003		0.036	0.002	
	$pcse(\beta_1)$	0.038	0.003		0.036	0.003							
	β_2	2.004	0.110	1.01	0.861	0.165	0.216	1.008	0.270	0.270	1.054	0.141	0.151
	$s.e.(\beta_2)$	0.04	0.007		0.154	0.037		0.208	0.206		0.130	0.022	
T=20	$pcse(\beta_2)$	0.086	0.020		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
	$s.e.(\beta_3)$	0.040	0.007		0.038	0.007		0.038	0.007		0.038	0.006	
	$pcse(\beta_3)$	0.039	0.007		0.038	0.007							
	ρ				0.570	0.081	0.107	0.497	0.131	0.131	0.477	0.063	0.067
	$s.e.(\rho)$				0.074	0.018		0.102	0.097		0.025	0.001	
	$pcse(\rho)$				0.059	0.018							
	β_1	1.011	0.026	0.029	0.998	0.025	0.025	0.999	0.025	0.025			
	$s.e.(\beta_1)$	0.026	0.001		0.025	0.001		0.025	0.001				
	$pcse(\beta_1)$	0.026	0.001		0.025	0.001							
	β_2	2.000	0.073	1.002	0.844	0.100	0.185	1.002	0.135	0.135			
	$s.e.(\beta_2)$	0.027	0.003		0.101	0.016		0.127	0.032				
T=40	$pcse(\beta_2)$	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			
	$s.e.(\beta_3)$	0.027	0.003		0.026	0.003		0.026	0.003				
	$pcse(\beta_3)$	0.027	0.003		0.026	0.003							
	ρ				0.578	0.049	0.092	0.499	0.065	0.065			
	$s.e.(\rho)$				0.049	0.008		0.062	0.016				
	$pcse(\rho)$				0.041	0.007							

3. Simulation Results: S-2SLS-IV and S-ML vs. OLS and S-OLS

S-OLS does very well to redress the omitted-variable bias in non-spatial OLS, but it suffers simultaneity biases, which, while generally smaller, become problematic in their own right as the strength of interdependence increases. How well do the consistent estimators, S-2SLS-IV and S-ML, which redress both the simultaneity and the omitted-variable biases, perform in these more realistic contexts and limited samples? Returning to Tables 1-4, the S-2SLS-IV estimator seems to produce unbiased estimates of all the coefficients (domestic, exogenous-external, context-conditional, and interdependence effects) across all sample dimensions and strengths of interdependence. Only its average interdependence coefficient estimates deviate from the true coefficient, by just 2-5%, and only when the true strength of independence is weak, with none of the other coefficients noticeably affected under any conditions, and with this coefficient too becoming unbiased as interdependence strengthens. On the other hand, we also see that S-2SLS-IV estimates, especially of ρ and β_2, have larger standard error than S-OLS (or S-ML) across most of these experimental conditions. This reflects the common weakness of IV estimators: inefficiency. Thus, when interdependence is weak, the S-2SLS-IV estimator may be undesirable in RMSE terms even when its exogeneity assumptions hold perfectly. Indeed, we see from these calculations that, when interdependence is weak, the limited gains in bias reduction are insufficient to offset the efficiency loss from S-2SLS-IV. With stronger interdependence, and the concomitant increase in the simultaneity bias, though, S-2SLS-IV becomes a more attractive alternative to S-OLS not only in bias but in RMSE terms also. This is particularly true as T increases, as one would expect with a consistent estimator like IV. Moreover, while S-2SLS-IV standard-errors are truly larger, at least the estimator reports these greater degrees of estimation uncertainty accurately. That is, the reported standard errors are accurately larger, which, with its unbiasedness, makes S-2SLS-IV appealing on unbiased hypothesis-testing grounds (i.e., not rejecting nulls when they are true), although its inefficiency makes it unappealing on testing power grounds (i.e., failing to reject when nulls are false).

Furthermore, the S-2SLS-IV estimator is relatively easy (relative to MLE, e.g.) to implement. One simply uses the **W** matrix already constructed to generate the spatial lag of **y** to generate the same spatial lags of **X**. 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 (Gauss-Markov) assumptions essential to the unbiasedness and consistency of OLS. As shown in our analytical results above, 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**, 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, $\mathbf{b_{iv}}=(\mathbf{X'Z})^{-1}\mathbf{Z'Y}$, is consistent and asymptotically efficient.

The two-stage least-squares instrumental-variables (2SLS-IV) produces these properties by, first, regressing the full 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 instruments **Z** are indeed perfectly exogenous, i.e., their covariance with **ε**, is *exactly* zero, then these IV estimators will be consistent and asymptotically efficient regardless of how strongly the instruments covary with the endogenous regressor(s) for which they instrument. 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 instruments with the endogenous regressor(s) relative to the covariance of the instruments with **ε**. In our experiments, the **X** variables, **ξ**, **η**, and **ξη** 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 somewhat endogenous to left-hand-side **y**-variables—i.e., the standard endogeneity concern that **y** causes **X** as well as **X** causes **y**. If so, and if **X** exhibits spatial correlation also, 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 standard 2SLS-IV estimation strategy to address the endogeneity of **X** and **y** with the S-2SLS-IV estimation strategy just described to address the spatial simultaneity. Failing that (e.g., if even imperfectly valid instruments for the standard 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** interdependence mechanisms, the intra-**X** interdependence 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 consider this conjecture in some simulations below, but we have not yet explored it fully by experimentation or at all analytically, nor have we yet determined the practical details of combining 2SLS-IV for endogeneity of **X** and **y** with S-2SLS-IV for spatial simultaneity of **y**. Therefore, for now, we advise researchers either to employ only strictly exogenous **X** in generating the spatial instruments **WX**, or, if they trust our conjecture, to explain why the **X** used in **WX** have good "Bartels Ratios", which in this case translates to high $\rho\beta/\gamma\alpha$.[21]

[21] Note: the magnitudes cannot be estimated without a model whose identification conditions must assume them. I.e., as Bartels emphasized, the magnitudes of the parameters that determine the quality of *quasi-instruments* cannot be

Summarizing the results for the S-2SLS-IV estimator so far, then, we have found it easy to implement and that, under its ideal conditions of perfectly exogenous instruments at least (see below for *quasi-instrument* conditions), it performs uniformly well in unbiasedness terms, especially as sample sizes increase, and that it accurately reports standard errors, which combine to yield unbiased hypothesis-testing. However, S-2SLS-IV, like all IV estimators, suffers from inefficiency such that, in smaller samples, it may trail other estimators in RMSE terms and its testing power may be low.

S-ML, contrarily, is not so easy to implement. First, similarly to the temporal dependence case, likelihood functions for spatially interdependent data quickly become, to use a technical term, ugly when more than one interdependence pattern, **W**'s, operate. Second, even with just one **W**, the value of the likelihood function that S-ML will maximize, depends on ρ, so the maximization routine is either tremendously computationally intense, or must rely upon an approximation to the determinant of **W**, which may be unstable in some conditions.[22] To see this, start by expressing the simple spatial-lag model with the stochastic component on the left-hand side:

$$\mathbf{y} = \rho\mathbf{WY} + \mathbf{XB} + \boldsymbol{\varepsilon} \Rightarrow \boldsymbol{\varepsilon} = (\mathbf{I} - \rho\mathbf{W})\mathbf{Y} - \mathbf{XB} \equiv \mathbf{AY} - \mathbf{XB} \qquad \textbf{(23).}$$

The likelihood function for the stochastic component, $\boldsymbol{\varepsilon}$, is then the usual linear-normal likelihood:

$$L(\boldsymbol{\varepsilon}) = \left(\frac{1}{\sigma^2 2\pi}\right)^{\frac{NT}{2}} \exp\left(-\frac{\boldsymbol{\varepsilon}'\boldsymbol{\varepsilon}}{2\sigma^2}\right) \qquad \textbf{(24),}$$

which, in this case, will produce a likelihood in terms of **y** as follows:

$$L(\mathbf{y}) = |\mathbf{A}| \left(\frac{1}{\sigma^2 2\pi}\right)^{\frac{NT}{2}} \exp\left(-\frac{1}{2\sigma^2}(\mathbf{AY} - \mathbf{XB})'(\mathbf{AY} - \mathbf{XB})\right) \qquad \textbf{(25).}$$

This still resembles a standard linear-normal likelihood, except that the transformation from $\boldsymbol{\varepsilon}$ to **y**, is

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estimated; we can only offer theoretical arguments about their likely relative magnitudes.

[22] On the other hand, if one can specify and has computer power sufficient to maximize likelihoods for multiple $\mathbf{W_s}$ models, S-ML may enjoy a different sort of practical advantage over S-2SLS-IV in that MLE would not require discovery of differentiated instrumentation for the multiple $\mathbf{W_s}$ to gain distinct leverage on the corresponding ρ_S.

not by the usual factor of one but by |**A**|, which, as claimed and as now seen in **(23)**, depends on ρ. Thus, each time the maximum-likelihood routine recalculates the likelihood with updated estimates of ρ, it would have to recalculate the determinant, |**A**|, for these new values of ρ. Ord's (1975) solution to this challenge was to use the approximation $\mathbf{\Pi}_i\lambda_i$ for |**W**| because $\boldsymbol{\lambda}$ in this approximation does not depend on ρ. Then $|\mathbf{I}- \rho\mathbf{W}| = \mathbf{\Pi}_i(1-\lambda_i)$, which requires the estimation routine only to recalculate a product, rather than a determinant, as it updates. Incidentally, the estimated variance-covariances of parameter estimates follow the usual ML formula (negative the inverse of Hessian of the likelihood function) and so are also functions of |**A**|. The same approximation serves there.

How well, then, does the S-ML estimator, which, like all correctly specified ML estimators will yield BANC (i.e., Best, or minimum-variance, Asymptotic-Normal and Consistent) estimates, perform in experimental simulations intended to reflect common C&IPE contexts and realistically limited samples? Considering first the summary measure, RMSE, we see across Tables 1-4, that S-ML performs comparatively well across sample and interdependence-strength conditions, obtaining smaller RMSE for its ρ estimates than the relatively inefficient S-2SLS-IV estimator in every case so far explored,[23] by 17% in smaller samples to almost 50% in larger samples. It also outperforms the less inefficient S-OLS estimator by this criterion, with RMSE for ρ estimates by about 8% in smaller samples to over 50% in larger ones. Its RMSE performance for the non-spatial effects, however, is less uniformly impressive, generally outperforming the other two estimators in RMSE of β_2 (common exogenous-external) effects from slightly to about 25%, but underperforming relative to their RMSE's β_1 and β_3 (domestic and context-conditional) effects from slightly to about 25%. Moreover, in terms of bias and standard-error accuracy, the complex and computationally intense S-

[23] Given its computational intensity, even with the simplifying approximation, these S-ML experiments took extremely long to run. (Our 100-iteration Monte Carlo experiment for N=40, T=40 samples would take an estimated 72 hours to complete in Stata running on a 2GHz, 2GB-RAM system.) Unfortunately, therefore, we have at this time results for only 100-trial experiments, for only the first few experimental scenarios. The results are not so far sufficiently encouraging to warrant advising S-ML over alternative estimators in most circumstances yet explored.

ML estimator performs yet less impressively. In our smallest samples (*N=5, T=20*), it somewhat underestimates domestic and context-conditional effects (by 1-2.5% or so), and it overestimates the common exogenous-external effects in every case by a noticeable (but not terrible) 2-5.5%. It also underestimates the strength of interdependence noticeably but not terribly (by about 3.5-5.5%) when that strength is a strong $\rho=.5$ and by a whopping 25% to almost 40% when interdependence is a weaker (but certainly non-negligible) $\rho=.1$. More worrisome still are the sometimes glaringly inaccurate standard errors S-ML reports for its ρ estimates. While the reported standard errors on average slightly overstate the true sampling variation of S-ML ρ estimates in the small-*N*, strong-interdependence cases (.07 *vs.* .064 and .05 *vs.* .045 in Table 3), they understate by about 60% in the large-*N*, strong-interdependence case (.025 *vs.* .063 in Table 4) and by about 25%, 50%, and 85% (.049 *vs.* .066, .074 *vs.* .141, and .025 *vs.* .143 in Tables 1-2) in the weaker interdependence cases, with the worst understatements coming, intuitively, in the smaller-*T* cases. Furthermore, scanning across Tables 1-4, S-ML also seems to underestimate standard errors for most of non-spatial effects (β estimates) under most conditions, often quite appreciably so (up to around 25% overconfidence in the most-glaring case, that of β_2 in Table 2, $T=20$).

In sum, S-ML:

(1) seems to estimate non-spatial (domestic, exogenous-external, and context-conditional) effects comparatively well in both bias and efficiency terms, and
(2) generally performs better than or equally well as alternatives in RMSE terms in estimating ρ also, but
(3) its RMSE advantages seem negligible in small-N, small-ρ samples,
(4) it seems generally to underestimate ρ, considerably so at smaller true ρ, and
(5) it seems rarely to report accurate standard errors for its estimates, especially regarding ρ estimates, for which reasonable accuracy obtains only when N is small and ρ is large, and outside of which case it reports (sometimes wildly) overconfident standard errors.

Thus, S-ML estimators have so far proven in our simulations to offer only irregular gains in truly reduced bias relative to S-OLS or in truly enhanced efficiency relative to S-2SLS-IV, depending on sample size and parameter magnitudes. Furthermore, S-ML tends to produce inaccurate assessments of the sampling variance-covariance of their coefficient estimates (standard-errors) and, in fact, wildly over-optimistic standard-error estimates under some experimental conditions. Add to that uninspiring performance its computational intensity, and we see little reason to prioritize further exploration of S-ML at this point.

Therefore, in summary comparison of the two consistent estimators, S-ML and S-2SLS-IV, against the simpler alternative S-OLS, we conclude that small samples, modest interdependence strength, and (as explored experimentally below) imperfect exogeneity of available instruments—not uncommon 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. On the other hand, when interdependence is stronger, one of the consistent estimators must be chosen over S-OLS, which performs poorly as its simultaneity bias grows. In this case, S-ML may have efficiency advantages over S-2SLS-IV, but these advantages come at the high costs of (downward) bias in estimated interdependence-strength and often severe over-confidence in standard-error estimation, plus non-negligibly high computational costs. Here too, then, we find strong argument for the simpler (to us anyway) estimator: S-2SLS-IV.

4. Simulation Results: Exogenous-External or Interdependence Specification Errors

We wish to consider next the implications of researcher misspecification of the pattern of interdependence, **W**, which is crucial because all of the spatial-lag estimators considered here require pre-specification of **W**. Table 5 reports the results for ρ=0.5 and w_{ij}=1/(N-1)+U[-0.1,+0.1]. With this experiment, we are exploring the consequences of *random* specification error in **W**; i.e., importantly, the specification error is orthogonal to all other components of the model. Misspecifications correlated with any part of **X**, **W**, or **ε** would likely have worse consequences than those found here. 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 ±.1 is ±40%. With N=40, 1/(N-1)≈.025, so ±.1 is ±400% roughly. 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 S-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 obtain; others do not. The standard errors for all three estimators[24] do increase with the introduction of random noise to the spatial weighting matrix, but, surprisingly, the biases in S-OLS estimates of β_2 and ρ actually decrease with the introduction of random noise to **W**. We suspect this occurs because the random draws in true **W** effectively add measurement error to the analyst's spatial lag, which (given the orthogonality of these errors) induces (only) attenuation bias that works in the opposite direction as the simultaneity bias. Meanwhile, the performance of the S-2SLS-IV estimator relative to S-OLS declines. For example, in Table 4, we saw that, when analysts use the true spatial weighting matrix for estimation and T=40, S-2SLS-IV has better RMSE than S-OLS (.065 *vs.* .092) under these sample and interdependence-strength conditions. In Table 5, however, the advantage is eliminated. The problem for S-2SLS-IV is that the random noise in **W** matrix reduces the predictive power of the spatial instruments. Weak instruments are well known to

[24] We have abandoned for now exploration of S-ML given its computational demands, which would delay, apparently unwarrantedly given the uninspiring performance found above, accumulation of results for the other, simpler estimators.

worsen IV estimates (see, e.g., Bartels 1991). In other words, as the researcher provides better specification for the pattern of interdependence, her ability to obtain better estimates of the strength of interdependence, absolutely and relative to domestic, exogenous-external, and context-conditional alternatives, improves not only by the enhanced distinction between these mechanisms but also by the enhanced ability of spatial-lag instruments to redress the simultaneity biases in estimating ρ.

In Table 6, we consider the analogous researcher-error with respect to the exogenous-external variable, $\boldsymbol{\eta}$. We generate the true exogenous-external shock, recall, by summing two independent random draws from normal distributions. Here, however, we estimate our models using only one of these two equal components. Thus, the hypothetical researcher specifies the exogenous-external factor, $\boldsymbol{\eta}$, only half correctly; of course, this misspecification will affect the context-conditional component, $\boldsymbol{\xi\eta}$, also. As in Table 5, this misspecification error is independently random and so, crucially, orthogonal to all the other components of the model. Thus, we expect and indeed find an attenuation bias in S-OLS estimates. This time the attenuation biases in β estimates reinforce the simultaneity bias in ρ estimates, so the overall bias in the S-OLS estimator increases. Thus, as expected, inadequate modeling of exogenous external-stimuli mechanisms attenuates estimates of exogenous-external effects[25] and inflates estimates of interdependence 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.

[25] Surprisingly to us, these biases concentrated almost exclusively in β_2, and not noticeably in β_3, estimates. We expect that the independent normality of the specification errors and the domestic conditions that enter the context-conditional component leaves β_3 estimates unaffected *on average*, but notice the very large increase in its true standard error.

Table 5. Comparing Estimators (N=40, ρ = 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	0.480	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	$pcse(\beta_2)$	0.061	0.012		0.088	0.018				
	β_3	1.011	0.032	0.033	0.999	0.031	0.031	1.000	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		0.070	0.022	
	$pcse(\rho)$				0.043	0.01				

Table 6. Comparing Estimators (N=40, $\rho = 0.5$, $w_{ij} = (1/N-1)$, $\eta = \eta_1 + \eta_2$, 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
	$s.e.(\beta_3)$	0.100	0.023		0.067	0.013		0.071	0.015	
	$pcse(\beta_3)$	0.103	0.027		0.081	0.017				
	ρ				0.950	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	0.190	0.189	1.017	0.193	0.193
	$s.e.(\beta_3)$	0.067	0.010		0.045	0.006		0.054	0.040	
	$pcse(\beta_3)$	0.069	0.012		0.050	0.007				
	ρ				0.946	0.023	0.446	0.575	0.861	0.860
	$s.e.(\rho)$				0.021	0.002		0.222	1.088	
	$pcse(\rho)$				0.020	0.004				

4. Simulation Results: Cross-Spatial Endogeneity and Imperfect Instruments

In Tables 7-10, finally, we wish to consider the consequences of *cross-spatial endogeneity*, our term for the condition which would render spatial-lag **X** instruments imperfectly exogenous, for OLS, S-OLS, and S-2SLS-IV. By *cross-spatial endogeneity* we mean direct or indirect causal correlation from y_i to x_j, i.e. from the outcomes in unit *i* to the non-spatial domestic (and context-conditional[26]) regressors in unit *j*, for example, from tax or ALM policies in, say, France, to government partisanship in Germany. That such "diagonal" causal arrows seem at first blush to be implausible substantatively is why we have suggested previously that the spatial interdependent structure of the data themselves suggest plausible instruments. However, cross-spatial endogeneity, i.e., diagonal arrows, may also arise from the combination of horizontal and vertical arrows, i.e., causal correlation of some **X** (e.g., government partisanship) across units *and* endogeneity of **X** to **y** (e.g., government partisanship to tax or ALM policies). Thus, exploration of the consequences of cross-spatial endogeneity for S-2SLS-IV, which is emerging as our preferred alternative to S-OLS when interdependence is strong, in particular and relative to other estimators, becomes important. To generate such cross-spatial endogeneity, we simultaneously draw **X** and **ε** from a multivariate normal distribution with correlation matrix **C**.

We set the strength of the cross-spatial endogeneity equal to the overall strength of interdependence by giving the appropriate elements of **C** (c_{ij}) a value of ρw_{ij} (i.e., $\rho/(N-1)$). The weak interdependence 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 without cross-spatial endogeneity (compare with Tables 1 and 2). As one might expect, the RMSE increase for each of the estimators, but the same basic conclusions

[26] The common exogenous-external factors, even while exogenous, do not provide identification leverage precisely because they are common to all units. The context-conditional components of those effects may provide leverage.

hold. S-OLS remains the preferred estimator when interdependence is weak. However, when interdependence is stronger, we know from above that the potential gains on all criteria from using S-2SLS-IV grow. With perfect instruments and a large *T*, S-2SLS-IV outperforms S-OLS in RMSE terms (Tables 3 and 4; T=40), but this is no longer true when cross-spatial endogeneity is also strong. The performance of both S-OLS and S-2SLS-IV 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—disappear. When N=5 and T=40 the RMSE for the S-2SLS-IV estimate of β_2 more than doubles and for ρ it quadruples. At this sample size, the RMSE for the S-OLS estimate of β_2 and ρ increase by 79% and 111% respectively. With N=40 and T=40, cross-spatial endogeneity has similar consequences. Even if interdependence is strong, therefore, S-2SLS-IV shows no real advantage over S-OLS when its conditions are equally heavily violated. Thus, the relative advantage of S-2SLS-IV over simple S-OLS, like all IV *vs.* OLS comparisons, depends on the combination of how strong are the instruments, how perfectly they purge the endogeneity, and how strong is the endogeneity to begin. We need at this point to provide more simulations across the ranges of those conditions (and likely helpful to plot rather than tabulate them) to understand better when S-OLS and when S-2SLS-IV may be the wiser estimation strategy.

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	0.140	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	0.080	1.001	0.080	0.080
	$s.e.(\beta_3)$	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, ρ = 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, $\rho = 0.5$, $w_{ij} = (1/N\text{-}1)$, $c_{ij} = .125$, 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				

5. Conclusions from Monte Carlo Simulations

Our simulations suggest, first, that the omitted-variable biases of excluding *interdependence* are of greater concern than the simultaneity biases of including them in OLS regressions, under a wide range of plausible substantive conditions. Analysts who ignore interdependence will generally over-estimate domestic, exogenous-external, and context-conditional effects, with this overestimation of course concentrating in those factors most correlated with the omitted interdependence mechanism. Therefore, regarding the substantive application at hand here, researchers unambiguously do better to include the spatial lags needed to specify correctly the strategic interdependence implied by policy-externality arguments than to ignore/omit that implication. However, we also showed that simultaneity biases from estimating OLS regressions that include spatial lags to reflect interdependence can be appreciable, that they tend toward overestimating the strength of interdependence (ρ), and, worse still, that underestimation of the variance-covariance of its estimate (i.e., its standard error) also prevails.[27] Thus, hypothesis tests based on OLS estimations of *correctly specified* models like **(5)** would be biased in favor of finding strong interdependence effects in ALM policies, perhaps greatly so because the relevant *t*-statistics have inflated numerators and deflated denominators.

Fortunately, one can estimate models like **(5)** of such interdependent processes by spatial two-stage-least-squares instrumental-variables, S-2SLS-IV, or by spatial maximum-likelihood, S-ML, to obtain consistent estimates of ρ and of $\boldsymbol{\beta}$. In fact, the former is not difficult to implement[28] because the spatial structure of the data itself suggests potential instruments. Valid instruments must satisfy that their (asymptotic) covariance with the endogenous regressor—here, the spatially-lagged outcomes in the other countries—is non-zero, and preferably large, whereas their (asymptotic) covariance with the residual in that equation is zero. Stated more intuitively: valid instruments must affect the variable for which they instrument, preferably greatly, but must *not* affect the dependent variable except insofar as they affect the variable being instrumented. In the ALM spending-policy context, this means that valid

[27] Furthermore, PCSE's did not seem to help much in this last regard.
[28] The latter is not so much difficult as computationally intensive. These two methods are the ones we explored and most-commonly discussed but are not exhaustive of those potentially capable of returning "good" estimates of $\boldsymbol{\beta}$ and ρ.

instruments must predict the ALM spending of countries with closely connected labor markets but not affect the ALM spending of the domestic country except insofar as they affect those foreign countries' ALM spending. Thus, all of the **X** variables in **(5)**, i.e., the foreign countries' own domestic, exogenous-external, and domestic-context-conditional-external factors,[29] are candidate instruments! One simply uses the spatial lags of **X**, **WX** (i.e., the same **W** already used to generate the spatial lag itself, **Wy**) as instruments for the spatial lag in the first stage of the *2SLS-IV* estimation. Fortunately, too, our Monte Carlo experiments show that such S-2SLS-IV estimates not only produce consistent estimates but also essentially unbiased ones, even at relatively small sample sizes. Moreover, the accompanying S-2SLS-IV standard-error estimates accurately reflected the true sampling variability of the coefficient estimates across all samples-size and parameter conditions explored. This suggests S-2SLS-IV, unlike S-OLS, will produce unbiased hypothesis tests.

Unfortunately, S-2SLS-IV is not very efficient and, indeed, is routinely outperformed in mean-squared-error terms by both simple S-OLS and, usually, by S-ML also. That is, S-2SLS-IV suffers the typical *IV* problem of weak instruments. In other words, S-2SLS-IV estimates have larger standard errors than alternative estimators, often enough larger to more than offset their unbiasedness, but, in their defense, as noted above, at least they honestly report these larger standard errors. Furthermore, as virtually always true, perfectly exogenous instruments cannot be guaranteed. In the S-2SLS-IV context, the problem of *quasi-instruments* (Bartels 1991) will arise in the presence of what we call *cross-spatial endogeneity*. That is, foreign countries' domestic and exogenous-external explanators will not be valid instruments for foreign countries' outcomes if the outcome in the domestic country correlates for some reason with the explanators in the foreign country. In our context, this would mean if ALM policies in one country somehow affected other countries' domestic (or context-conditioning) factors. French ALM affecting German election outcomes, for example, might seem implausible, so, on this basis, the

[29] The exogenous-external factors may seem not to satisfy the intuitive statement of valid instruments because they enter both domestic- and foreign-country tax-policies. However, they enter both exogenously, so, although they do not provide much leverage or power to the instrumentation—they do so only insofar as they are domestic-context conditioned and this context-conditioning correlates (exogenously) across countries—they are nonetheless valid.

proposed spatial instruments may have strong claim to exogeneity.[30] However, cross-spatial endogeneity can also arise without such direct "diagonal causal-arrows" from one country's outcomes to others' explanatory (domestic) factors because, intuitively, combinations of "horizontal" and "vertical" arrows can make "diagonal" ones. That is, if the more-usual sort of endogeneity problems exist, wherein **y** (ALM policies) causes **X** (e.g., domestic industrial-structure), and spatial correlation among the **X**'s exists also (e.g., industrial structure correlates across countries), then the "diagonal" that violates spatial-instrument validity, covariance of $\mathbf{WX}_j$ with $\mathbf{y}_i$, emerges. We found in simulations that, if this no-cross-spatial-endogeneity condition is in fact violated, S-2SLS-IV estimates suffer radically. In sum, therefore, we can believe the instrumentation assumptions necessary for consistency (and asymptotic efficiency[31]) of S-2SLS-IV estimates of the strength of interdependence in the ALM policies of European-Union countries if we believe (a) direct effects from $\mathbf{y}_i$ to $\mathbf{X}_j$ do not exist and (b) the **X** are either spatially uncorrelated or exogenous to **y**.[32]

S-ML estimators, for their part, have so far proven in our simulations to offer either little in reduced bias relative to S-OLS or little in efficiency relative to S-2SLS-IV, depending on sample size and parameter magnitudes. Furthermore, they tend uniformly to underestimate the strength of diffusion, appreciably so when it is in fact moderate, and they tend to produce inaccurate assessments of the sampling variance-covariance of all of their coefficient estimates (standard-errors), especially those for the estimated strength of interdependence, and, in fact, wildly over-optimistic ones for those estimates under some experimental conditions. Add to that uninspiring performance its computational intensity, and we have seen little reason to pursue S-ML estimation actively at this point.[33]

[30] On the other hand, Persson and Tabellini (2000) discuss just such "strategic delegation" as one implication of their tax-competition model. Voters in one country have incentives to support citizen-candidates of greater or lesser capital-labor endowment than themselves precisely because they internalize the effect on their own capital-tax rates of foreign elections.

[31] *Asymptotic efficiency* should not at all be confused with *efficiency*. The former is an extremely weak property, stating only that as sample sizes *approach infinity* estimates *become* the most efficient ones and nothing at all necessarily about the relative or absolute efficiency of the estimates along the path they follow as sample sizes approach infinity. Furthermore, if one had infinite samples, efficiency would be virtually irrelevant.

[32] The latter of (b)'s two parts is of course the usual regressor-exogeneity assumption necessary to the unbiasedness and consistency of all *LS* estimators. However, violation of it alone produces biased and inconsistent estimates of $\boldsymbol{\beta}$, not of ρ (except insofar as bias in the former bias induces bias in the latter, which, by usual induced-bias intuition, only occurs in some dampened proportion to the degree to which a typical single country's domestic **X** correlates with the foreign **y** in its spatial lag, which is not usually very much).

[33] Excepting these less uniformly glowing results for S-ML, our findings generally resonate with those of previous studies where they overlap (Franzese and Hays 2004 reviews). Our use of a baseline model that reflects a modern, context-

Researchers interested in spatial interdependence, which necessarily includes those interested in European integration, thus face a troubling dilemma. They obviously must specify empirical models that reflect the dependence of one country's policies and outcomes on those of their partners and/or competitors; interdependence, after all, is the core of their interests, and the testing for and gauging of it the core of their empirical estimations. In fact, though, even researchers uninterested in interdependence *per se*, but only in CPE or open-economy-CPE questions, must specify empirical models reflecting spatial interdependence, if it exists, to avoid potentially severe omitted-variable biases in their quantities of interest: domestic, exogenous-external, and context-conditional effects. Indeed, one way to phrase our primary conclusion from these econometric explorations would be to emphasize that accurate and powerful specification of the alternative is as critical to scholars solely interested in *C&IPE* from either the *CPE* or *IPE* angle as it is to those interested in *C&IPE* jointly. Any insufficiency in the specification of the one side will tend to bias our conclusions toward the other.[34]

Beyond this, however, i.e., even after we are fully satisfied (or as satisfied as we can be) with the domestic, exogenous-external, context-conditional, and interdependent components of our model, the researcher into substantive contexts like externalities in ALM policy-spending still faces a dilemma in choosing estimators. S-2SLS-IV performs well in terms of coefficient unbiasedness and accuracy of reported standard errors, *if its assumptions hold*, and so should tend to produce unbiased hypothesis tests. However, these tests may be relatively weak (lack power) given that the estimators are inefficient; moreover, the S-2SLS-IV estimates are sufficiently inefficient that one would prefer the simpler S-OLS point estimates on mean-squared-error grounds even if the former's assumptions hold.

One reasonable approach to this dilemma might be to report point estimates of the strength of

conditional, open-economy, comparative-and-international 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. We believe that our analytic results regarding the inversely related biases in estimating these two sorts of effects are also new.

[34] Furthermore, all the estimators had the hardest time separating the effects of common exogenous-external shocks and homogenous interdependence modeled with spatial lags. We strongly suspect that this generalizes to the difficulty of distinguishing between CPE and IPE mechanisms being a direct function in the correlation of their incidence. Interdependence patterns that are functions of labor-market inter-connections, to draw an example from our empirical exploration below, will be most difficult to distinguish from the effects of domestic, exogenous-external, and context-conditional factors that correlate across units in similar patterns to their labor-market connections.

interdependence, ρ, and other model coefficients, $\mathbf{\beta}$, from the smaller mean-squared-error S-OLS, but to report the hypothesis tests with better unbiasedness properties from S-2SLS-IV, being sure to note and acknowledge the latter's lack of power, meaning to avoid drawing conclusions from failures to reject even more so than one always should. However, this approach leaves ambiguous which standard errors to report. Standard errors from S-OLS tend to be "inaccurately too small" and PCSE's will not necessarily help with this particular problem. Standard errors from S-2SLS-IV, conversely, tend to be "accurately too large" and refer to different point estimates besides. Moreover, if interdependence is indeed reasonably strong, as it does seem to be in the ALM policy-spending case, S-OLS will offer sizably biased estimates, even if that bias may be offset by greater efficiency. At this point, the best we can offer is the advice to show readers both and refer them to our Monte Carlo experiments to decide themselves which or which combination of estimates they prefer, as none statistically dominates.

IV. Labor-Market Externalities and Active-Labor-Market Policy-Spending:

A. Substantive Theory:

European integration implies potentially strong strategic (and non-strategic) interdependence among European countries' domestic policies. In fact, spatial interdependence is substantively central to the very concept of integration in general and is perhaps no small part of the goal of European-Union integration specifically. Next, we consider how (un)employment spillovers across border regions due to the strong interconnectedness of regional labor markets (Overman and Puga 2002) yield incentives for each country to free-ride on its neighbors'/partners' employment-enhancing policies or, equivalently but perhaps more intuitively, how employment-enhancing policies entail cross-border positive externalities that induce domestic under-investment in them. This suggests that as neighbor/partner countries' employment-enhancing public policies increase, domestic polities would tend to reduce their own, *ceteris paribus*.

Specifically, the theoretically expected externality that induces the national under-investment and thereby spatial interdependence in ALM among EU member-countries is regional integration of labor markets across borders (Overman and Puga 2002). This regional labor-market integration induces externalities and so interdependence in ALM policy by several sorts of economic connections. Consider the implications of (effective) French ALM spending for Belgium, for example. Effective French ALM policies enhance Belgian workers' abilities to obtain training, in France, and return, more employable, to work in Belgium. Effective French ALM policies also enhance Belgian workers' abilities to find work, in France. Effective French ALM policies also enhance the pool of workers (quantity, quality, and diversity) available to employers along the Belgian border, thereby luring employers to both sides of the border. These and other agglomeration effects all yield positive externalities of (effective) French ALM policies to Belgian workers (and citizens more generally). Of course, Belgians cannot provide political support to French policymakers in response to these spillover effects, so French policymakers ignore these spillover benefits in determining French ALM spending.[35] Accordingly, ALM spending by national policymakers will exhibit negative interdependence, reflecting the positive externalities.

Since all of these interdependence mechanisms operate through labor-market interconnectivity, we suggest an inductive approach to specifying the matrix of ALM-policy interdependence, **W**, that pre-estimates partial correlations of EU member-country unemployment outcomes, using those partial correlations as the w_{ij} reflecting the $i \rightarrow j$ ALM-policy interdependence in subsequent estimation of a spatial-lag ALM-spending model. We find strong evidence for a surprisingly large degree of such negative feedback in national active-labor-market-policy spending *per* unemployed worker, suggesting a strong case for supranational (EU) policymaking in this area.

B. Data and Methods:

Our sample, annual data from 1980-98, includes 11 EU countries: Belgium, Denmark, Finland, France, Germany, Ireland, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom. Our dependent variable is Active Labor Market (ALM) spending per unemployed worker in constant 1995 (PPP) US dollars.[36] ALM policies include spending on labor-market training, youth measures, employment measures for the disabled, subsidized employment, and employment services and

[35] Ignoring the political rewards Belgians participating in French politics, as EU allows, would generate, but, given cross-border political participation and activity remains nonetheless low, these would be something less than what a French citizen receiving the same benefit would give, so the result, qualitatively, is under-provision even so.
[36] This data is not available for Luxembourg, Greece, Italy, and Austria.

administration. The key independent variable in our analysis is the spatial lag of ALM spending. The calculation of this variable is described below. We also include several control variables in our analysis: the temporal lag of ALM spending, the log difference in the number of unemployed workers, real GDP *per capita*, the percentage of cabinet seats held by left parties, Iversen and Cusak's deindustrialization measure, working age population as a percentage of total population, the number of veto points, and union density.

To explore interdependence in ALM policies, we must first construct spatial-lag variables. The first step in creating these lags is to determine appropriate spatial weights for the connectivity matrix, **W**. In theory, we expect ALM spending-policy interdependence to be related to interconnections—direct migration and other cross-border spillovers—in labor markets. We can, therefore, perhaps, estimate the degree of labor-market interdependence for the 11 countries in our sample with monthly changes in unemployment rates. We apply two estimation strategies for this preliminary step, giving alternative spatial-weights matrices, $\mathbf{W_1}$ and $\mathbf{W_2}$. To produce $\mathbf{W_1}$, we estimate 11 separate equations by OLS regression of the change in each country's unemployment rate in month *t*, using monthly data to add empirical leverage in this first step,[37] on the change in its unemployment rate at month *t-1* and the *t-1* changes in the other 10 countries.[38] To create $\mathbf{W_2}$, we estimate the same 11 equations simultaneously using Zellner's SUR estimator. This estimator gains efficiency by using information about the contemporaneous cross-equation correlations in the error terms. If the labor markets in our sample are being hit with common shocks, and we expect they are, this information will help improve our estimates. The specifications are the same as before except we include three temporal lags of the dependent variable on the right-hand-side of each equation.[39]

Both estimation strategies give similar results. We include those for $\mathbf{W_2}$ below.

[37] This extra leverage is especially useful in these preliminary analyses because we have not yet accounted for uncertainty in this pre-estimation of **W** in the standard errors for the parameter estimates of the primary model reported below.
[38] We used monthly data from the OECD's Quarterly Labor Force Statistics for the period 1982-2004.
[39] In other words, for example, the equation for Belgium includes the Belgian change in unemployment at time t-1, t-2, and t-3 on the right-hand-side along with the time t-1 changes in the other countries. There is no (efficiency) advantage to using the SUR estimator over OLS if each equation in the system includes the same right-hand-side variables (e.g., see Greene 1997, 676). The equations would have identical regressors without the addition of these temporal lags.

$\mathbf{W}_2 =$

	BEL	*DNK*	*FIN*	*FRA*	*DEU*	*IRL*	*NLD*	*PRT*	*ESP*	*SWE*	*GBR*
BEL	0	−.038	.027	.094	.002	−.033	−.003	.034	.012	.077	−.002
DNK	.058	0	.006	−.068	.009	.059	−.016	−.038	−.077	−.001	.031
FIN	.151	.124	0	−.205	−.040	.159	−.115	.006	.039	.048	.232
FRA	.136	.028	.016	0	.030	.042	.005	−.031	.048	.040	.044
DEU	.048	.001	.033	.367	0	−.073	−.072	−.022	.110	.082	−.080
IRL	.053	.011	.010	.059	.024	0	−.069	−.064	.009	.027	.091
NLD	.206	.056	.040	−.121	−.047	.078	0	.070	.012	.029	−.068
PRT	.023	−.086	−.028	.072	.038	.024	.104	0	.052	.048	.062
ESP	.235	.025	.057	−.100	−.016	.096	−.001	−.063	0	.020	.078
SWE	−.067	.177	.220	.220	.100	.078	.219	−.053	.113	0	−.186
GBR	.054	−.018	.018	−.066	−.010	.142	−.002	−.054	.011	.044	0

The cells in each row of the spatial weights matrix contain the partial coefficient estimates from our unemployment rate regressions of that row country on the other 10 column countries. For example, the cell from $\mathbf{W}_2$ at the intersection of row 1 and column 2 gives the effect of unemployment rate changes in Denmark at time *t-1* on changes in the unemployment rate in Belgium at time *t*, *holding changes in the other countries constant*. Note that these partial coefficients do not tell us the *full* effect of a shock to unemployment in one country on unemployment in another because indirect effects operate beyond these direct effects. For example, the direct effect of changes in unemployment in Denmark on unemployment in Belgium may be negative, but Denmark also has positive effects on Finland and Sweden, which, in turn, have positive effects on Belgium. To calculate the full effect of these direct and indirect spatial relationships, we would need to compute the spatial multiplier.

To create our spatial lag, we simply multiply the *NxN* spatial weights matrix, $\mathbf{W}_1$ or $\mathbf{W}_2$, by an *NxT* matrix of observations on the dependent variable. We then include this spatial-lag, which will be a weighted sum of the changes in unemployment in the other 10 countries (weighted by the partial coefficients in the $\mathbf{W}_1$ or $\mathbf{W}_2$ matrix) as a right-hand-side variable in our ALM spending regressions. We estimate two regression models, one for each of our spatial lags. All of our regressors are lagged one period to address potential problems of endogeneity except for the log difference in unemployment, which is contemporaneous with the dependent varible.[40] The regressions contain full sets of country and period dummies as a quick and crude, but often effective, set of proxies for the rest of what a fully and

[40] To standardize our dependent variable, we use the ratio of ALM spending to the number of unemployed workers. Thus, our dependent variable is a ratio. We include the log difference in unemployment to control for changes in the denominator.

powerfully specified C&IPE model of ALM-policy might contain. Considering that we domestic ALM spending will depend on foreign policies in proportion to their labor-market connections through a policymaking process that occurs at budgetary paces, we include these spatial lags in the regression models at a one-year temporal lag, as with the other controls (excepting unemployment).

B. Results:

The regression estimates, both S-OLS with PCSE for $\mathbf{W_1}$ and $\mathbf{W_2}$ and S-2SLS-IV for $\mathbf{W_2}$, are reported in Table 1. Since the spatial lag of ALM spending enters the regression model only at a one-year temporal lag, the simultaneity bias in S-OLS does not arise except insofar as the residuals in this model exhibit temporal correlation (as shown in a similar context in Appendix II; see also Beck and Gleditsch XXXX). Indeed, comparing the estimates for S-OLS and S-2SLS-IV with $\mathbf{W_2}$, columns 2 and 3, we see little difference between the two estimates, the latter producing just 1% smaller estimate, well with standard errors. This suggests our simple S-OLS estimates may be trusted in this case. However, the difference between the strength of interdependence estimates using the less-efficient $\mathbf{W_1}$ and more-efficient $\mathbf{W_2}$ are striking. In terms of the analyses above, then, this seems a case where potential specification error in $\mathbf{W}$ is much more consequential than the degree of simultaneity. S-OLS would seem a satisfactory estimator, and the researcher's efforts are best expended toward measuring $\mathbf{W}$ well.

Substantively, as predicted, the coefficients on the spatial lag variables are negative and statistically significant; ALM spending-policy by national policymakers seems to exhibit negative-feedback interdependence, perhaps due to the posited externality. For two countries with positively correlated labor market outcomes, this implies an increase in active labor market spending in one will cause reduced spending in the other. Indeed, the magnitude of these estimates of the strength of the negative feedback seems surprisingly large.

Table 1. ALM Spending in the European Union (1980-1998)

	Model 1 (W1)	Model 2 (W2)	Model 3 (W2)
LN Diff. Unemployment	-0.083***	-0.086***	-0.087***
	(0.007)	(0.007)	(.011)
Temporal Lag	0.892***	0.889***	0.879***
	(0.064)	(0.061)	(0.045)
Spatial Lag	-0.557**	-0.887**	-0.834**
	(0.265)	(0.344)	(0.323)
Real GDP Per Capita	0.933***	0.883***	.914***
	(0.216)	(0.199)	(.275)
Left Party Cabinet Seats (%)	0.005	0.006*	.005
	(0.003)	(0.003)	(.005)
Deindustrialization	0.021	0.012	0.019
	(0.058)	(0.058)	(0.094)
Working Age Population (% of total)	-0.243**	-0.203**	-0.229
	(0.106)	(0.098)	(0.167)
Veto Points	-0.457	-0.433	-0.412
	(0.286)	(0.374)	(0.264)
Union Density	0.065***	0.078***	0.073*
	(0.024)	(0.023)	(0.037)
Estimator	S-OLS	S-OLS	S-2SLS
No. of Observations	163	163	151
No. of Countries	11	11	11
Min Time Periods	11	11	11
Max Time Periods	18	18	17

***Significant at 1%; **Significant at 5%; *Significant at 10%. Parentheses contain panel corrected standard errors in columns 1-2 and ordinary standard errors in column 3. All regression models estimated with fixed unit and period effects. Coefficients not shown.

To help interpret these results more fully, we provide a set of counterfactual predictions in Table 2. These counterfactuals give the full effect on each country's ALM spending of a unit increase ($1000 per unemployed worker) in the spending of the other EU countries and vice versa. So, for example, if the 10 countries in our sample except Sweden all increased their active labor market spending by $1000 per unemployed worker, our model predicts Sweden would decrease its ALM spending by $547. If Sweden decreased its ALM spending by $1000, we would expect the other countries to increase their spending on average by $29. The first calculation reflects the incentive individual countries have to free ride off of the policies of others. These incentives are strongest when good labor market performance abroad spills over to the domestic economy. The second calculation measures for each country the incentives others have to free ride off its ALM spending. These incentives are high when good domestic performance spills over to other countries. Both of these estimates are indeed substantive large. A total foreign investment of $10,000 inducing a $547 reduction in Swedish spending represents a sizable -5.47% feedback. Swedish investment of $1000 inducing the average other country's policymakers to reduce their ALM spending by $29 represents a more-than-half as sizable -2.9% feedback.

Table 2. Counterfactual and Factual Changes in ALM Spending

	Predicted Change in ALM Spending with Unit Increase (rank)		Average ALM Spending in 80's	Average ALM Spending in 90's	Observed Change in ALM Spending
	Effect of Common Foreign on Domestic	Average Effect of Domestic on Foreign			
Sweden	-0.547 (1)	-0.029 (3)	24.749	18.404	-6.346
Finland	-0.360 (2)	-0.026 (4)	6.837	5.763	-1.075
Spain	-0.331 (3)	-0.024 (6)	1.126	1.740	0.615
France	-0.279 (4)	-0.025 (5)	3.129	4.981	1.851
Germany	-0.224 (5)	-0.007 (9)	7.058	8.300	1.242
Netherlands	-0.206 (6)	-0.006 (10)	4.699	8.848	4.149
Portugal	-0.192 (7)	0.015 (11)	1.574	3.712	2.138
Ireland	-0.117 (8)	-0.043 (2)	2.983	5.155	2.173
United Kingdom	-0.086 (9)	-0.013 (8)	2.486	2.262	-0.224
Belgium	-0.077 (10)	-0.065 (1)	6.596	7.902	1.307
Denmark	0.003 (11)	-0.017 (7)	5.346	9.931	4.585
Mean			6.053	7.000	
Std Dev			6.549	4.631	
Coeff. Var.			1.082	0.662	

Comparing these counterfactual predictions with observed changes in active labor market spending is also interesting (see Table 2, last three columns). How well do these spatial relationships account for the spending patterns we observe? If nothing else, the model seems to provide a reasonable explanation for some of the extreme cases. For example, the largest absolute decrease in ALM spending from the 1980's to the 1990's occurred in Sweden. The spatial explanation for this decrease is that Sweden has benefited from the ALM spending of its economic partners, which rose by almost $1000 on average. Moreover, the benefits of its own ALM spending after joining the EU now diffuse throughout the European economy, reducing the incentive domestic politicians have to keep spending high.

Appendix I: Simulation Results for Spatial-Error OLS Estimation

In our experiments, we replicate and extend the simulations in Beck and Katz (1995), stressing the overconfidence and efficiency issues addressed in their Tables 4 and 5 respectively. (See pages 638-9 for a detailed discussion of their experimental design.) Beck and Katz use a version of equations **(6)** to generate their experimental data, which feature disturbances that are both contemporaneously correlated across units and panel heteroscedastic. The contemporaneous correlation or spatial dependence is experimentally manipulated through $\boldsymbol{\lambda}$ while panel heteroscedasticity is created by allowing the variance of $\mathbf{u}$ to be unit specific. In these experiments, *N* is fixed at 16 while *T* is manipulated.

Table 1 reports our simulation results with respect to standard-error estimates and overconfidence. The table entries gauge the accuracy of the standard error estimates (in percentages) by comparing them with the true variability in the coefficient estimates across the 1,000 experimental trials. Percentages above 100 indicate that the standard error estimates understate true variability. The columns in Table 1 labeled OLS and PCSE replicate the findings in Beck and Katz. OLS standard errors underestimate the true variability in the coefficient estimates while panel corrected standard errors report this variability accurately. While we do not report them, the Parks standard errors are very inaccurate in small samples tending to underestimate the true variability of the coefficient estimates.[41] The column labeled S-E-OLS (PSCE) presents a new set of results, which show that PCSE estimates suffer very little from the inclusion of a spatial lag in the model. For example, when both panel heteroscedasticity and contemporaneous correlation are present, which is likely in real data, the difference between PCSE with and without the spatial lag 3%, 5%, and 6% for T's of 20, 30 and 40 respectively. Note that, for the experiments reported in their Table 4, Beck and Katz generate $\mathbf{X}$ and $\boldsymbol{\varepsilon}$ that are contemporaneously correlated and panel heteroscedastic. Since this specification implies spatial dependence in both the systematic and stochastic components of **y**, we use $\hat{\boldsymbol{\lambda}}\mathbf{y}$, not $\hat{\boldsymbol{\lambda}}\hat{\boldsymbol{\varepsilon}}$, as our spatial lag.

[41] See Beck and Katz (1995) Table 2. The Parks standard errors are highly inaccurate when the number of observations per unit (T) is close to or not much greater than the number of units in the sample (N). The degree of overconfidence when N and T = 20 is more than 600% meaning that the true variability in the coefficient estimates is more than six times larger than that suggested by the standard error estimates.

Table 1. Replication and Extension of Beck and Katz (1995, Table 4)

T	Heteroscedasticity	Contemporaneous Correlation	Overconfidence (%) OLS	PCSE	S-E-OLS (PCSE)
10	0	0	101	102	NA
10	0	0.25	136	105	NA
10	0.3	0	114	100	NA
10	0.3	0.25	145	104	NA
20	0	0	102	102	102
20	0	0.5	220	107	115
20	0.3	0	123	106	106
20	0.3	0.5	224	106	109
30	0	0	96	97	97
30	0	0.5	214	102	115
30	0.3	0	115	99	99
30	0.3	0.5	232	108	113
40	0	0	99	99	99
40	0	0.5	214	101	118
40	0.3	0	114	98	98
40	0.3	0.5	227	104	110

Notes: The spatial weighting matrix (W) was estimated using the OLS residuals. This matrix cannot be estimated unless T > N. S-OLS includes a spatial lag of the dependent variable on the right-hand-side (W*Y). These results are based on 1,000 trials. The number of units was fixed at 16 as opposed to 15

In Table 2 we report the efficiency comparisons. Efficiency is measured in percentages relative to non-spatial OLS. We use the same experimental parameters (N, T, and degree of contemporaneous correlation) as Beck and Katz (1995, Table 5). The columns labeled Parks replicate their findings for the relative efficiency of FGLS to OLS. The columns labeled S-E-OLS compare the efficiency of our spatial OLS estimator to OLS. The first thing to note about these results is that S-E-OLS more or less matches the efficiency of Parks FGLS. The relative efficiency of both estimators relative to non-spatial OLS is determined by the degree of contemporaneous correlation in the data and the ratio of N to T in the sample. Our experiments suggest that on efficiency grounds S-E-OLS outperforms non-spatial OLS when the degree of contemporaneous correlation is above 0.25 and T is at least twice as large as N.

Table 2. Replication and Extension of Beck and Katz (1995, Table 5)

Relative Efficiency of Parks and S-OLS to OLS (Over 100% indicates superiority of OLS)
Contemporaneous Correlation of the Errors

N	T	0.0		0.25		0.5		0.75	
		Parks	*S-E-OLS*	*Parks*	*S-E-OLS*	*Parks*	*S-E-OLS*	*Parks*	*S-E-OLS*
10	**10**	102	NA	102	NA	99	NA	97	NA
	20	112	110	104	106	91	95	73	82
	30	110	110	104	102	86	88	66	73
	40	111	110	99	96	82	84	63	68
15	**15**	102	NA	101	NA	99	NA	98	NA
	20	110	111	101	108	94	103	88	99
	30	107	109	104	105	91	95	74	80
	40	112	113	102	103	87	90	67	72

20	**20**	101	NA	100	NA	99	NA	98	NA
	25	105	109	101	107	96	105	88	100
	30	113	113	102	108	93	100	83	96
	40	114	112	101	101	89	92	74	82

Notes: The spatial weighting matrix (W) was estimated using the OLS residuals. This matrix cannot be estimated unless T > N. S-E-OLS includes a spatial lag of the residuals on the right-hand-side (W*E). These results are based on 1,000 trials. Number of units was fixed at 16 as opposed to 15

To sum, we believe the S-E-OLS approach outlined above allows one to capture much of the efficiency benefit of Parks FGLS without paying the cost of inaccurate standard error estimates, and its use as an alternative to OLS-PCSE can be justified on purely econometric grounds in many circumstances. We stress once again that the experimental conditions we created are the most favorable to the "nuisance" approach to spatial dependence, which makes our results all the more remarkable. We show later that OLS-PCSE performs significantly worse than S-OLS once we relax the assumption that all the spatial dependence is in the error term. Most importantly, S-OLS focuses one's attention on the degree of spatial interdependence in the data, which is too easily ignored with OLS-PCSE. We demonstrate this problem in our reanalysis (Franzese and Hays 2005) of the tax regressions in Swank and Steinmo (2002).

<u>Appendix II: Simultaneity Bias in S-OLS with Differenced and/or Temporally Lagged Spatial Lags</u>

Simultaneity biases in spatial OLS estimation of the strength of interdependence will typically be inflating, assuming reinforcing feedback. In the case of dampening feedback, which we suspect to be less common substantively, the simultaneity biases in S-OLS estimates of ρ will typically be attenuating. However, differencing and/or lagging the spatial-lag temporally complicate these biases and their signs. In the case of temporally lagged and differenced spatial lags, which Basinger and Hallerberg (2004) use in their ground-breaking study of the strategic interdependence implied by tax competition, for example, the simultaneity bias is likely attenuating:

$$\Delta Y_{1,t} = \rho_{12}\Delta Y_{2,t-1} + \Delta\varepsilon_{1,t} \qquad \textbf{(A2.1)};$$

$$\Delta Y_{2,t} = \rho_{21}\Delta Y_{1,t-1} + \Delta\varepsilon_{2,t} \qquad \textbf{(A2.2)};$$

$$\hat{\rho}_{12} = \rho_{12} + \frac{\text{cov}[Y_{2,t-1} - Y_{2,t-2}, \varepsilon_{1,t} - \varepsilon_{1,t-1}]}{\text{var}[Y_{2,t-1} - Y_{2,t-2}]} \qquad \textbf{(A2.3)}.$$

In this case, if the error terms are *i.i.d.*, the estimate of ρ_{12} will be unbiased because interdependence occurs only with a time-lag, removing the simultaneity. However, if countries 1 and 2 experience common shocks (i.e., $\text{cov}[\varepsilon_{1,t}, \varepsilon_{2,t}] > 0$), the numerator from the second term on the right-hand-side of **(A2.3)** will be negative, and S-OLS will underestimate the degree of spatial interdependence. We discover such attenuating bias in our reanalysis of Basinger and Hallerberg (2004) (Franzese and Hays 2004). Note that, if ΔY_2 in **(A2.1)** (ΔY_1 in **(A2.2)**) is contemporaneous with the dependent variable and the countries experience common shocks, S-OLS will overestimate the degree of spatial dependence.

Appendix III: Breusch and Pagan's Lagrange-Multiplier Test for Spatial Correlation

Breusch and Pagan's (1980) highly intuitive measure and test statistic of spatial correlation essentially sums the pairwise (*ij*) correlations in the data. Literally, it is the sum of squared pairwise correlations times T, the number of observations per pair (see **(A3.3)**). Under a null assumption of normality and zero spatial correlation, this measure will have chi-squared distribution, and one can conduct statistical testing on that basis. Without assuming normality, the same statistic will have chi-squared distribution only asymptotically, which creates an interest in exploring the small-sample properties of the test. This appendix reports our Monte Carlo experiments to examine the size of the Breusch-Pagan *LM* test in small samples. The size of a statistical test is its probability of a *Type I Error*, *i.e.*, of falsely rejecting a null hypothesis that is, in fact, true.

We conduct experiments of 1000 independent trials, varying sample dimensions, *i.e.*, the number of cross-sectional units, *N*, and time periods, *T*, across experiments. This model generates the samples:

$$y_{i,t} = x_{i,t} + \varepsilon_{i,t} \qquad \textbf{(A3.1).}$$

The indices $i = 1...N$ and $t = 1...T$ identify each sample's *NT* observations. The exogenous variables x and ε are independent draws from a standard normal distribution. Hence, the data exhibit no spatial correlation. After generating each sample, we estimate the following model by *OLS*:

$$y_{i,t} = \beta x_{i,t} + \gamma \bar{y}_{\sim i,t} + \varepsilon_{i,t} \qquad \textbf{(A3.2).}$$

The variable $\bar{y}_{\sim i,t}$ is the average *y*-value for unit *i*'s sample counterparts, $\{j\}$, at time *t*, this being one way some researchers (*e.g.*, Franzese 2002) have attempted to address (some of) the spatial correlation in their *TSCS* data. Breusch and Pagan (1980: 247) calculate their *LM* statistic gauging correlation as:

$$LM = T\sum_{i=2}^{N}\sum_{j=1}^{i-1} r_{ij}^2 \qquad \textbf{(A3.2).}$$

with r_{ij} the ij^{th} residual correlation. As noted, *LM* is distributed asymptotically chi-squared under the null, with *(N·(N-1))/2* degrees of freedom, yielding critical test values from the $\chi^2_{N\cdot(N-1)}$ distribution.

For now, our primary interest is how this *LM* statistic performs under different sample dimensions (*NxT*). We consider several *NxT* dimensions common in political economy research: *T*'s of 20, 30, or 40 (years) by *N*'s of 5, 10, 15, 20, or 30 (countries). We conduct three experiments for each *T* using different *N*'s, giving nine in total: *5x20, 10x20, 15x20, 5x30, 10x30, 20x30, 10x40, 20x40,* and *30x40*.

Table **A3.1** reports the results for each experiment. We focus on the *95th* percentile of the relevant chi-squared distribution as the critical value for comparison. Not surprisingly, the experimental size of the *LM* test for spatial correlation in small samples always exceeds the asymptotic size of the test: *0.05*. In some cases, it more than doubles this 5% size. The *LM* test may help diagnose spatial correlation, but, as always, analysts must use care in interpreting borderline results suggesting correlation, especially in small samples where it rejects appreciably more often than warranted. One result of the experiments is surprising: for a given *T*, smaller *N* does not always produce more-accurate test-sizes (*e.g.*, test size seemed truer at *10x30* than at *5x30*); nor does increasing *T* for a given *N* always yield truer test-size (*e.g.*, test size is truer at *10x30* than at *10x40*). We suspect that this is because asymptotics in both N and in T matter for different aspects of this test. Regarding the lack of initially normally distributed residuals, we must rely on their asymptotic normality which obtains as $T\rightarrow\infty$ (we believe). Regarding the chi-squared distribution of their squares, and, more-to-the-point, of their cross-products under the null, the relevant asymptotics are in *N(N-1)*, the number of such cross-products. This suggests an optimal *NxT* ratio for test accuracy may exist, but, generally, this *LM* statistic performs reasonably well, although *p*-values may understate true probabilities of *Type I Errors* in small samples. However, the test easily reveals strong spatial correlation where it is present, which is the central need suggested by the econometric considerations in the text, although whether this power is accurate (*i.e.*, avoidance of *Type II Errors*) at smaller degrees of spatial correlation and/or in smaller samples remains untested.

Table 4: Size of Breusch-Pagan LM Test under Null Hypothesis of, and Truly, Zero Spatial Correlation

N:	5	10	15	5	10	20	10	20	30
T:	20	20	20	30	30	30	40	40	40
Trials:	1000	1000	1000	1000	1000	1000	1000	1000	1000
Percentiles									
1%	3.68	28.29	82.22	3.12	29.03	158.60	28.79	154.57	381.87
5%	5.04	34.59	91.22	4.70	32.88	168.14	33.04	164.82	399.21
10%	6.20	36.87	94.10	5.87	35.84	174.75	35.59	172.09	409.12
25%	8.19	41.20	100.90	8.27	40.64	184.67	40.86	182.10	424.64
50%	11.23	47.10	110.83	11.04	46.51	196.48	47.26	195.81	446.34
75%	14.60	53.15	121.48	14.19	53.30	211.30	54.23	209.05	466.71
90%	18.20	59.50	130.99	18.23	60.12	224.42	60.64	224.03	487.62
95%	**20.62**	**65.06**	**138.49**	**20.58**	**64.04**	**230.62**	**65.21**	**233.83**	**497.55**
99%	27.96	76.46	150.91	27.62	74.87	248.61	74.79	251.69	521.02
Degrees Freedom:	10	45	105	10	45	190	45	190	435
Chi-Squared (95%):	**18.31**	**61.66**	**129.92**	**18.31**	**61.66**	**223.16**	**61.66**	**223.16**	**484.63**
Size of LM Test:	**0.095**	**0.08**	**0.113**	**0.096**	**0.074**	**0.108**	**0.086**	**0.104**	**0.116**